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by

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DECODING THE MATRIX: UNEARTHING KEY FACTORS SHAPING WELL-BEING

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Acknowledgements

An essay is a long-term – fingers crossed – harmonious achievement, meticulously crafted from the melding of various elements and factors to form a unique masterpiece. The essence of this work lies in the delightful chaos born from the collaborative creativity and inventive spirit of numerous individuals, blending emotions such as pride and humility, despair and joy, and anguish and resilience.

Above all, an essay is a monumental human endeavor, illustrating our ability – and sometimes our inability – to adapt and grow amidst challenges like the COVID-19 pandemic and personal struggles. The chapters of this work stand as a testament to the power of collaboration and the exchange of ideas, revealing the captivating nature of human connections and the intricate interplay of chance and destiny.

"The group thinks, feels, and acts quite differently from the way in which its members would were they isolated."
Emile Durkheim ("*The Rules of Sociological Method*", 1895)

As one chapter closes and gives way to the next, I express my profound gratitude to the exceptional individuals who have shaped my journey and enriched my work. I extend my heartfelt appreciation to my supervisor, Professor Philippe Van Kerm, who knew how to guide me on this path full of pitfalls. The complexities of the administrative process can sometimes be daunting; Philippe made them accessible and digestible – a miracle in itself! Furthermore, Philippe was a pleasant co-author to work with: straight to the point, his ability to perceive the "big picture" has been pivotal.

Philippe showcased exceptional econometric skills, which blended harmoniously with the qualities of the two other members of my thesis supervisory committee: Professor Alain Trannoy and Professor Eugenio Peluso. Through their rigorous questioning, vast experience, and keen taste for theorizing, they helped establish solid theoretical frameworks that elevated empirical explorations.

Throughout my journey, I have been fortunate to meet other brilliant and inspiring personalities who left an indelible mark on my work. The diversity of backgrounds and collective wisdom contributed depth and coherence, while their enthusiasm and spirited discussions fueled my motivation and inspiration.

As I reflect on my PhD journey, I recognize the importance of mental health and well-being, not only as the subject of my thesis but also as a vital aspect of personal growth. The unwavering support and understanding of my family and friends have been essential in overcoming obstacles and nurturing my well-being. They have transformed this experience into a truly social and fulfilling adventure.

"There is no more dreadful punishment than futile and hopeless labor."
Albert Camus ("*The Myth of Sisyphus*", 1942)

Ultimately, this essay is thus more than a scholarly endeavor; it is a testament to the boundless human investment in the pursuit of knowledge. It is a celebration of the passion, enthusiasm, collaboration, and curiosity

that drive us to explore the depths of understanding. This essay symbolizes the essence of the quest for knowledge, where curiosity, perseverance, and collaboration converge to create a work of enduring beauty and intellectual depth. In our tireless pursuit of innovation and discovery, we navigate the twists and turns of research, always seeking new horizons.

Abstract

This PhD thesis traverses an array of research domains, predominantly focusing on deciphering and tackling societal issues through a socioeconomic perspective. Nevertheless, it is essential to regard each chapter as an independent study, as they engage with distinct research questions, necessitating diverse datasets and methodologies.

Chapter 1 – Understanding trends and drivers of urban poverty in American cities. *Published in Empirical Economics.* Urban poverty arises from the uneven distribution of poor populations across neighborhoods of a city. Over a span of four decades, we critically examine the trends and factors driving urban poverty in American cities. Our approach involves the utilization of several urban poverty indices that account for the incidence, distribution, and segregation of poverty across census tracts. Built on solid normative foundations, these indices provide a more detailed understanding than the concentrated poverty index. We analyze tract-level data to determine how demographics, housing, education, employment, and income distribution impact the levels and changes in urban poverty. Through a decomposition analysis, we differentiate between the effects of changes in the distribution of these factors across cities and changes in their correlation with urban poverty. Our findings highlight the significant role of demographics and income distribution in shaping urban poverty, a result that markedly differs when using concentrated poverty indices.

Chapter 2 – Urban poverty and the onset of the Coronavirus pandemic: Evidence from American cities. This study empirically explores the extent to which urban poverty in American cities influenced the propagation of COVID-19 during the pandemic's initial phase and the impact of mobility restriction measures on this dynamic. Leveraging ACS data, along with mobility and confirmed case data, and accounting for potential bias from measurement errors and unobserved confounders, we ascertain that an increase in urban poverty by one standard deviation corresponds to an escalation of 0.55-0.7 COVID-19 cases per 100,000 residents at the county level. This represents roughly a quarter of the COVID-19 incidence reported in the median American city by the end of April 2020. Intriguingly, we observe that stay-at-home orders fail to curb the contagion, instead inadvertently accelerating it in cities where poverty is less uniformly spread across neighborhoods, attributing this to the underlying factors of urban poverty.

Chapter 3 – The pandemic's toll on domestic violence: Investigating the effect of COVID-19 public health measures. This research examines the influence of COVID-19 public health measures on domestic violence, focusing on the evolution of their effects throughout the pandemic. Given the underreporting issues associated with domestic violence due to the health measures, we employ the relative trajectory of domestic violence-related Google searches across 31 countries as a proxy indicator of domestic violence. By integrating this data with the Oxford COVID-19 Government Response Tracker, I can harness the fine-grained timing and intensity of COVID-19 public health interventions across countries. The results show that the measures significantly contribute to the rise in domestic violence, with evidence of impact as early as two weeks post-implementation of lockdowns. While these effects gradually decline over time, successive variations in

the stringency of the measures persist in influencing domestic violence for several months following their initial introduction. The research further uncovers that while economic support policies intensify the effects of these measures, diminishing compliance to the measures by individuals helps to alleviate them.

Chapter 4 – Psychotropic drug consumption during the COVID-19 pandemic in Luxembourg: Excess consumption and socio-demographic profile. This chapter inspects the evolution in psychotropic drug purchases in Luxembourg amid the COVID-19 pandemic, contrasting it with the preceding trends. Leveraging extensive administrative data – incorporating quarterly purchases of psychotropic drugs from all non-hospital pharmacies in Luxembourg between January 2016 and December 2021 – and anonymized individual-level socioeconomic details from the Inspection générale de la sécurité sociale (IGSS), the study allows for a detailed analysis of the characteristics of the population, aged 18 to 79 years, that resided in Luxembourg in February 2020. The study considers factors such as sex, age, household size, household income, and employment status. Although the findings reveal no clear excess purchases of psychotropic drugs following the pandemic's onset, the usage patterns varied by medication type, showing a rise in antidepressant consumption but no marked increase, or even a decrease, in the use of anxiolytics, hypnotics, and sedatives. Additionally, the research surfaces notable disparities in drug consumption patterns across various demographic strata, with the most pronounced relative change seen among younger individuals. The study underscores the need for further investigations into the pandemic's repercussions on mental health across diverse demographic groups, even as it confirms the pandemic's impact on mental health in Luxembourg, primarily evidenced by increased antidepressant use.

Chapter 5 – Emotional barometers: Twitter emojis and emoticons as tools to gauge temperature's effect on mood. Contrary to the prevalent notion that weather significantly influences mood and well-being, empirical findings have shown mixed evidence. This paper makes two key contributions. First, this study introduces a framework for gauging mood on Twitter by leveraging emojis and emoticons, addressing methodological concerns such as omitted variable bias and small sample issues. Second, the paper offers a fresh perspective on the weather-mood nexus using alternative mood data, with a specific focus on temperature. The data consist of geotagged tweets randomly selected among Twitter users in the United States in 2014, representing different types of profiles and capturing various short-term weather variations. Overall, the findings suggest that individuals are nonlinearly affected by weather conditions. This study provides a versatile, cost-effective, language-neutral, and instantaneous approach to analyzing involuntarily disclosed mood indicators on a global scale, offering a significant complement to conventional methods.

Chapter 6 – Commuting time and absenteeism: Evidence from a natural experiment. This research investigates the effect of commuting time on absenteeism using a natural experiment. This relationship is notoriously difficult to assess without exogenous shocks to commuting and with the survey data typically exploited. The study uses detailed administrative data for Luxembourg to measure the impact on work absences of a temporary shock to commuting time caused by large-scale roadworks at the border between Belgium and Luxembourg. The roadworks affected the commuting time of cross-border workers from Belgium, leaving cross-border commuters from France as a natural control group in a difference-in-difference setup. The findings reveal a positive – but quantitatively relatively small – effect of commuting time on absenteeism, driven mainly by increased absences due to reported illness or family reasons. Male workers appear to respond more than female workers to the shock in commuting time.

Co-author statement

This dissertation includes both single-author papers and collaborative work. The chapters I exclusively authored are:

- **Chapter 3 – The pandemic's toll on domestic violence: Investigating the effect of COVID-19 public health measures.**
- **Chapter 5 – Emotional barometers: Twitter emojis and emoticons as tools to gauge temperature's effect on mood.**

The remaining chapters resulted from collaborative efforts. For transparency and clarity, I provide a detailed account of my specific contributions to each co-authored paper:

- **Chapter 1 – Understanding trends and drivers of urban poverty in American cities**, co-authored with Francesco Andreoli (University of Verona), Mauro Mussini (Università degli Studi di Milano-Bicocca), and Vincenzo Prete (University of Verona) Under the supervision of Principal Investigator Francesco Andreoli, Vincenzo Prete and I first handled data cleaning and preparation. We then performed most of the econometric analysis and participated actively in writing. Mauro Mussini and Francesco Andreoli significantly contributed to developing the research question and framework. Collectively, we each contributed to various aspects of the paper, culminating in the final comprehensive work.
- **Chapter 2 – Urban poverty and the onset of the Coronavirus pandemic: Evidence from American cities**, co-authored with Francesco Andreoli (University of Verona), Mauro Mussini (Università degli Studi di Milano-Bicocca), and Vincenzo Prete (University of Verona) Given the interconnected nature of the two studies, my contributions to this paper are similar to those in Chapter 1.
- **Chapter 4 – Psychotropic drug consumption during the COVID-19 pandemic in Luxembourg: Excess consumption and socio-demographic profile**, co-authored with Philippe Van Kerm (University of Luxembourg) and Tom Rausch (Ministry of Health, Luxembourg). This research project, part of the "Santé pour tous" initiative in collaboration with the Ministry of Health (Luxembourg), was overseen by Principal Investigator Philippe Van Kerm. Philippe and I shared a mutual understanding regarding the research question. While Tom Rausch provided valuable insights during the preparatory stage, the main manuscript resulted from the combined efforts of Philippe and me. Following my initial draft, we jointly developed the subsequent analysis, and distinguishing individual contributions to the resulting paper would be challenging due to our close collaboration.
- **Chapter 6 – Commuting time and absenteeism: Evidence from a natural experiment**, co-authored with Philippe Van Kerm (University of Luxembourg). Initiated by Philippe Van Kerm's research question, this project evolved into a fruitful collaboration. Following my initial draft, we engaged significantly in the subsequent econometric analysis and manuscript preparation, resulting in a solid research paper reflecting our joint efforts.

Dedication to my Grandfather: Infinite Echoes

To Albert,

Your extraordinary penchant for logic, your tireless quest for knowledge, and your ardent drive to voice your insights were unparalleled. In your distinctive manner, you embraced the world. As a trailblazer in computer science, you played a pivotal role in connecting Luxembourg's premier institutions to the rest of the world, particularly when Luxembourg was emerging from the aftermath of a turbulent industrial past. Your mastery in weaving narratives with zeros and ones led you to distant horizons, inspiring and kindling a thirst for learning in all who encountered you.

In myriad ways, you sculpted my core. You were versed not just in the intricate language of machines but also attuned to the delicate heartbeats of humanity. You inspired me to explore a vast array of subjects, ensuring that, thanks to your expertise, knowledge was always mere electric impulses away. Endowing me with this unwavering essence, you unveiled countless doors and opportunities. Through the intricate maze of circuits and wires, you perceived not just tasks but boundless possibilities; not just data but dreams. Your insatiable curiosity inspired me to embark on this dissertation, driving me to shed light on hidden facets of our world and deepen my understanding.

And yet, as I draft this dissertation centered on well-being and mental health, profound sadness and still misunderstanding accompany my words. Behind the glow of computer screens lay shadows too. For you, there were instances when the world's complexities, perhaps, became overwhelming. Moments when, despite your unmatched ability to unravel complexities, words failed to capture the profound depths of your feelings. Times where you were missing vital programming. Like a system under duress, an internal surge ultimately broke the flow. It reminds me that even the most robust circuits can be extremely vulnerable.

This deficiency echoes within me, both personally and professionally, symbolizing a society abundant in splendor but marred with concealed pains seeking solace. I aspire to contribute to the comprehension of these hidden scars.

In homage to your legacy, I dedicate this dissertation, as a testament to my unwavering commitment to relentlessly unearth the truth and dismantle stigmas surrounding mental health. I yearn for this dedication to touch numerous lives, particularly those of the elderly and misunderstood. You instilled my insatiable thirst for knowledge, a thirst I commit to channeling toward a brighter tomorrow. You found your calling at the crossroads of technology and humanity; it is here that I find mine.

Though I lament that you will never read these sincere words, I find comfort in the certainty that, had things gone differently, you would have been among its earliest readers.

To the visionary who introduced me to the magic of machines, this dissertation stands as my humble ode. Albert, my gratitude for your gifts is boundless – as is my love.

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General introduction

General introduction

Navigating the multifaceted landscape of well-being

Well-being is an intricate and multifaceted concept. It integrates both physical (Kekäläinen et al., 2020) and psychological dimensions (Sun et al., 2018). Influenced by various factors, well-being has garnered increasing attention in academic literature, with over 50,000 publications annually (Tov et al., 2020). These studies underscore the critical role of social and familial relationships (Saltzman et al., 2020), socioeconomic conditions (Scoppa & Ponzo, 2008), environmental quality (Dadfar et al., 2018; Van Dyck et al., 2015; White et al., 2013), and access to mental health services (Slade et al., 2014) in shaping well-being outcomes. Moreover, findings further draw attention to the detrimental effects of local poverty on mental health (Mitchell & LaGory, 2002), and the significant impact of public health crises such as the COVID-19 pandemic on mental health (Tsamakis et al., 2021), which contributes to the broader concept of well-being (Tang et al., 2019). As well-being is integral to individual and societal prosperity (e.g., see Diener, 2006), ongoing research endeavors to comprehend and address these influencing factors.

This thesis highlights several direct and indirect factors influencing well-being and its proximal mediators. These elements were chosen based on their relevance to well-being and significance within contemporary societal transformations and emerging challenges. Each Chapter addresses diverse facets of well-being, contributing to an enhanced understanding of how social and economic factors shape well-being outcomes. Advanced quantitative methods applied to diverse, large-scale datasets facilitate this systematic exploration, illuminating insights across a multitude of populations, timelines, and locations.

Precisely, the thesis unfolds as follows: Chapter 1 investigates the trends and drivers of urban poverty in American cities, illuminating the determinants of urban poverty. Chapter 2 delves into the intricate link between urban poverty and the spread of the COVID-19 pandemic, highlighting its ramifications for vulnerable populations. Chapter 3 explores the impact of COVID-19 public health measures on domestic violence, offering a nuanced understanding of the pandemic's societal consequences. Chapter 4 analyses patterns in psychotropic drug purchases during the pandemic in Luxembourg, informing our understanding of mental health patterns and the socio-demographic profile of users. Chapter 5 introduces a framework for gauging mood on Twitter by leveraging emojis and emoticons and examines the emotional influence of weather conditions on individuals. Lastly, utilizing a natural experiment framework, Chapter 6 investigates the relationship between commuting time and absenteeism.

The subsequent sections of this introduction offer a short overview of each focal area. Specifically, they unpack (1) the role and impact of urban poverty on individual development and opportunities, (2) the widespread effects of the COVID-19 pandemic, including stress, violence, and changes in psychotropic drug purchases, and (3) the influence of transient phenomena such as weather conditions and commuting time. These explorations offer valuable insights into the factors shaping individuals' daily experiences and overall well-being.

Urban poverty: A persistent challenge

Urban poverty casts a profound and enduring impact on individuals' life trajectories. Its repercussions are not confined to the personal sphere but permeate the broader community, leading to "neighborhood effects" symptoms. These effects, sorted into three primary categories – sociocultural consequences, health-related outcomes, and barriers to opportunities – underpin the far-reaching impacts of urban poverty.

The sociocultural consequences of urban poverty include the prevalence of racial segregation (Quillian, 2012), high rates of violence (Cunradi et al., 2000), and a marked decrease in social cohesion and support (Ceballo & McLoyd, 2002). These factors amplify the sense of social isolation experienced by low-income, minority residents (Wang et al., 2018), further complicating their access to resources essential for collective efficacy.

The health-related outcomes linked to poverty-stricken neighborhoods encompass notably increased allostatic load, a biological indication of chronic stress, and inflammation (Schulz et al., 2012). These areas also frequently report heightened rates of crime and violence (Cunradi et al., 2000), pervasive drug use (Boardman et al., 2001; Nandi et al., 2010), and escalated environmental pollution (Ard & Smiley, 2022). All these factors collectively compound their residents' health challenges, casting medium- and long-term consequences (Ludwig et al., 2013; Ludwig et al., 2011; Thierry, 2020). Ludwig et al. (2012) even point toward direct adverse effects on well-being.

The barriers to opportunities form another significant outcome of urban poverty. A key downside of residing in neighborhoods characterized by high poverty concentration is the resultant alienation from middle-class environments, curtailing access to robust labor markets, quality education, and other essential public amenities (Jargowsky, 2013). Additionally, the hindrance to relocation to better neighborhoods perpetuates the cycle of poverty and intergenerational inequality (Alvarado & Cooperstock, 2021).

The effects of urban poverty are especially pronounced on children. Children living in high-poverty neighborhoods tend to display poorer cognitive development, reflected in lower math and reading test scores (Pearman, 2019; Sharkey & Elwert, 2011; Vinopal & Morrissey, 2020; Wolf et al., 2017) and diminished verbal abilities (Sampson et al., 2008), relative to their counterparts in neighborhoods with lower poverty concentrations. The long-term effects of prolonged exposure to high-poverty neighborhoods during childhood are stark, leading to lower economic opportunities for future generations, such as lower college attendance, reduced earnings, and a propensity for single-parenthood in adulthood (Chetty & Hendren, 2018; Chetty et al., 2016; Conley & Topa, 2002). Addressing the issue of urban poverty is therefore crucial for enhancing the well-being and prospects of those living in disadvantaged neighborhoods and promoting a more equitable and prosperous society.

The concentrated poverty index, introduced by Wilson (1987) and Jargowsky and Bane (1991), quantifies the level of poverty concentration within a city by indicating the share of the poor population living in neighborhoods with a poverty incidence greater than or equal to a specified threshold (e.g., 20% for high-poverty neighborhoods and 40% for extreme-poverty neighborhoods). However, despite its insights, the index possesses inherent limitations and fails to fulfill some desirable properties. To overcome some of these shortcomings, Andreoli et al. (2021) have axiomatically derived a family of urban poverty indices that provide a more holistic assessment of urban poverty. These indices account for the relationship between the incidence of poverty in a city, the distribution of poverty across high-poverty neighborhoods, and the extent of segregation of poor and non-poor residents between high- and low-poverty neighborhoods.

Considering the profound implications of urban poverty on well-being and associated factors, it is essential to explore its underpinning aspects. Chapter 1 of this thesis examines the trends and drivers of concentrated poverty across American metropolitan areas, building upon previous studies that have identified residential segregation as the primary driver of spatial concentration of poverty in American urban areas (Massey et al., 1991) and investigated its variations and changes over time (Iceland & Hernandez, 2017; Quillian, 2012). By probing into the drivers of urban poverty and its evolution, particularly emphasizing discrepancies between drivers of the family of urban poverty indices and the concentrated poverty index, this Chapter provides new insights into the factors shaping urban poverty, ultimately informing policy decisions and interventions.

The COVID-19 pandemic: Multifaceted repercussions

The COVID-19 pandemic has significantly impacted the well-being of individuals worldwide, touching several aspects of human life (Filindassi et al., 2022). This section examines several issues brought forth by the pandemic, explicitly emphasizing its implications on urban poverty, domestic violence, and mental health – three fundamental dimensions that shape individual and community well-being (Filindassi et al., 2022).

Interplay between urban poverty and COVID-19 The COVID-19 pandemic has exposed and exacerbated the deep-rooted socioeconomic disparities within urban environments, with lower-income residents bearing a disproportionate brunt. This vulnerability is primarily ascribed to systemic inequalities, which manifest as unequal living conditions, increased occupational hazards, and adverse environmental contexts (Cole et al., 2021). These factors translate into higher infection and mortality rates, shedding light on the critical intersection of health outcomes and social determinants (Cole et al., 2021).

A closer examination reveals a constellation of factors, such as poverty, unemployment, the prevalence of low-skilled jobs, crowded housing, and limited vehicle access combined with a strong reliance on public transportation for longer commutes, significantly contributing to increased COVID-19 diagnoses and mortality rates in these populations (e.g., see Benitez et al., 2020; Desmet and Wacziarg, 2022; Eichenbaum et al., 2021; Jay et al., 2020; Khazanchi et al., 2020; Ruiz-Euler et al., 2020). Moreover, research suggests a correlation between the proportion of Black and Hispanic residents in an area and the number of confirmed COVID-19 cases, thereby drawing attention to the inequitable burden of the pandemic on minority populations (Benitez et al., 2020). Collectively, these dynamics enhance the risk of lower-income individuals to the double blow of economic and health-related consequences of the pandemic.

In light of these findings, Chapter 2 delves into the intricate relationship between urban poverty and the incidence of COVID-19. Expanding on the initial analysis of urban poverty determinants in Chapter 1, it further explores its relationship with new COVID-19 cases in American counties during the early stages of the pandemic. Notably, the discussion highlights the pivotal repercussions of urban poverty on public health, emphasizing the need for continued research and targeted interventions.

Ripple effects on domestic violence and mental health The COVID-19 pandemic has further intensified pre-existing social issues, with stay-at-home orders leading to an increased risk of domestic violence (Gama et al., 2020; Henke & Hsu, 2022). Chapter 3 scrutinizes this complex relationship, focusing primarily on the impact of public health measures on domestic violence across 31 countries. This analysis underscores the challenges victims encounter, particularly individuals in abusive relationships. The lockdown has negated their employment-based escape routes, making it considerably more challenging and perilous to extricate themselves from abusive situations due to the amplified costs and risks associated with relocation (Henke & Hsu, 2022).

In addition to escalating domestic violence, the pandemic has incited a mental health crisis, inducing and amplifying stress, anxiety, depression, and other psychosocial consequences (Sher, 2020). The burden is particularly heavy on women grappling with augmented employment and domestic responsibilities (Parikh & Patel, 2023). There is a significant correlation between chronic physical and mental health issues and domestic violence, with survivors often reporting persistent, deleterious effects (Ramsay et al., 2012).

The economic consequences of the pandemic further exacerbate these challenges. Many workers have experienced income reduction or complete loss, with approximately 35% of US workers transitioning to remote work during the pandemic's initial stages (Brynjolfsson et al., 2020; Coibion et al., 2020; Dingel & Neiman, 2020; Kogan et al., 2020). The confluence of these factors – economic instability, health uncertainties, increased emotional stress, and potential increases in substance abuse – have profoundly impacted individuals' psychological well-being (Galea et al., 2020; Pfefferbaum & North, 2020), contributing to the rise in domestic violence (Aizer, 2010; Aizer & Dal Bo, 2009; Anderberg et al., 2016; Card & Dahl, 2011).

Chapter 4 expands upon these findings, probing the pandemic's influence on psychotropic drug purchases and providing a novel perspective on mental health effects. By examining changes in psychotropic drug purchases in Luxembourg during the COVID-19 pandemic compared to trends observed before 2020, the Chapter aims to highlight the pandemic's extensive impact on mental health and expose disparities in drug consumption across various demographic subgroups. This holistic approach to studying the pandemic's impacts provides valuable insights for policymakers and stakeholders, facilitating the development of effective strategies to alleviate the comprehensive mental health and well-being repercussions of COVID-19.

Transient influences

This section delves into the effects of two pervasive factors – weather conditions and commuting time – on the daily lives and overall well-being of individuals. Both factors have significantly influenced various facets of mental and physical health and daily decision-making processes.

Impact of weather conditions The profound impact of weather conditions on both physical and mental health is well established, influencing a range of transient and long-term aspects of individuals' lives (Anderson, 1989, 2001; Baron & Bell, 1976; Connolly, 2013; Lin et al., 2008; Maes et al., 1994; Ponjoan et al., 2017; Tsutsui, 2013). Notably, variations in weather have been correlated with fluctuations in depression prevalence (Huibers et al., 2010), with repercussions on emotional states, bio-tone, work capacity, and concentration levels (Spasova, 2012). These changes can precipitate various physical and mental health challenges (Ponjoan et al., 2017).

In addition to health outcomes, weather significantly shapes daily behaviors and decision-making processes (Cunningham, 1979; R. E. Lucas & Lawless, 2013; Persinger, 1980; Simonsohn, 2009; Watson, 2000; Zong et al., 2017). This influence permeates various aspects of daily life, from personal engagement in activities (R. E. Lucas & Lawless, 2013) to clothing choices, from prioritization processes in college admissions (Simonsohn, 2009) to tipping behaviors in the service industry (Cunningham, 1979).

While there is an extensive body of research on the effects of weather on physical health and daily behaviors, studies examining its impact on mood and well-being have yielded mixed results and lack consensus (Barnston, 1988; Connolly, 2013; Cunningham, 1979; Goldstein, 1972; Howarth & Hoffman, 1984; Keller et al., 2005; Parrott & Sabini, 1990; Persinger, 1975; Rind, 1996; Rind & Strohmetz, 2001; Schwarz & Clore, 1983). Some research emphasizes the feeble or transient impact of weather on mood and judgment (Cunningham, 1979; Goldstein, 1972; Howarth & Hoffman, 1984; Parrott & Sabini, 1990; Persinger, 1975), while others propose more enduring effects (Connolly, 2013; Goldstein, 1972; Keller et al., 2005).

To address this gap in the literature and the increasing urgency posed by climate change, Chapter 5 explores the effects of temperature on mood, utilizing a novel methodology of mood assessment via Twitter data (Novak et al., 2015). This approach, using emojis and emoticons as proxies for mood in tweets, mitigates methodological issues such as omitted variable bias and small sample sizes (Algaba et al., 2020; Barrington-Leigh, 2008; Baylis et al., 2018; Campbell, 2012; Denissen et al., 2008; Denny & Spirling, 2018; Hannak et al., 2012; Howarth & Hoffman, 1984; Keller et al., 2005; Li et al., 2014; Liu, 2012; Park et al., 2013; Preti, 1998; Sanders & Brizzolara, 1982; Tsutsui, 2013; Watson, 2000). This innovative strategy provides a more comprehensive examination of the relationship between weather and mood and illuminates potential shifts that could disproportionately affect specific population groups (Albouy et al., 2016; Hsiang, 2016; Lobell & Burke, 2008; Sinha et al., 2018).

Impact of commuting time A second factor shaping the lived experiences of individuals is commuting time. Commuting duration and frequency have seen a steady increase in many Western countries, with over 20% of European workers dedicating more than an hour and a half to commuting each day (Gimenez-Nadal & Molina, 2016; Kirby & LeSage, 2009; McKenzie & Rapino, 2011).

Chapter 6 examines the influence of commuting time on work absenteeism. On the one hand, longer commutes can widen the job opportunity horizon and optimize the worker-job match (Goerke & Lorenz, 2017). In parallel, an expanded search radius for housing could facilitate more informed decisions regarding residential locations (Goerke & Lorenz, 2017).

On the other hand, lengthy commutes come with several negatives. Often regarded as one of the least pleasurable daily activities (Kahneman et al., 2004), long commutes can reduce the time available for physical activities and be a source of daily stress (Choi et al., 2013; Kahneman et al., 2006; J. L. Lucas & Heady, 2002; Stutzer & Frey, 2008). Environmental repercussions are another concern, with the increase in air pollution as a direct consequence of lengthy commutes. The physical (Evans et al., 2002; Hansson et al., 2011; Novaco et al., 1990; Roberts et al., 2011) and mental (Dickerson et al., 2014; Friman et al., 2017; Gatersleben & Uzzell, 2007) health impacts of commuting, in turn, may lead to increased absenteeism and diminished worker productivity (Grinza & Rycx, 2020; Oswald et al., 2015).

To illuminate these intricacies, Chapter 6 meticulously dissects the relationship between commuting time and work absenteeism, leveraging a unique natural experiment that temporarily disrupted commuting times for Belgian cross-border workers in Luxembourg. The Chapter unveils intriguing findings, particularly the increased absenteeism tied to extended commuting times, suggesting a consequential work-life balance recovery effect. Furthermore, the study probes into the differential impact of disrupted commutes on diverse subsets of workers, such as those with various commute lengths, employment contract types, and gender. This comprehensive examination enriches our understanding of commuting's impact, providing valuable insights to inform future policy decisions to promote sustainable urban living and healthier work-life integration.

Dissertation outline

This thesis delves into a multifaceted examination of urban poverty's social and economic drivers, the ramifications of the COVID-19 pandemic, and the influence of various external factors on individual well-being. Consisting of six distinct but interconnected chapters, the investigations cover: (1) the trends and drivers of urban poverty in American cities, (2) the association between urban poverty and the COVID-19 pandemic, (3) the impact of COVID-19 public health measures on domestic violence, (4) the patterns of psychotropic drug purchases during the pandemic in Luxembourg, (5) the emotional toll of weather conditions on individuals through a novel approach, and (6) the connection between commuting time and work absenteeism, explored via a natural experiment. By tackling these diverse topics, this thesis offers valuable insights and broadens our comprehension of the intricate dynamics that shape individual and societal well-being.

Chapter 1 – Understanding trends and drivers of urban poverty in American cities.

Published in Empirical Economics.

Chapter 1 examines the trends and drivers of urban poverty across American cities over the past 40 years. The study employs a family of urban poverty indices that offer a comprehensive measure of poverty by accounting for its incidence, distribution, and segregation across census tracts. This approach overcomes some drawbacks of the widely-used concentrated poverty index.

The study uses tract-level data to assess the impact of demographics, housing, education, employment, and income distribution on levels and changes in urban poverty. Demographics and income distribution emerge as essential factors in explaining urban poverty patterns, while the effects differ significantly when using concentrated poverty indices. A comparative analysis is conducted further to understand the drivers of poverty concentration and urban poverty, examining the partial effects of poverty factors and considering gentrification as an additional factor.

This Chapter also performs an Oaxaca-Blinder decomposition to analyze the contribution of changes in the distribution of explanatory variables on urban poverty. The results reveal substantial heterogeneity in urban poverty patterns across American metropolitan statistical areas (MSAs). The spatial component of urban poverty is particularly significant in large MSAs where the clustering of high-poverty census tracts is a concern. Overall, Chapter 1 provides valuable insights into the complex factors shaping urban poverty in American cities and enhances understanding of the nuances of different poverty indices.

Chapter 2 – Urban poverty and the onset of the Coronavirus pandemic: Evidence from American cities. Chapter 2 delves into the influence of urban poverty in American cities on the early spread of COVID-19 and the role of mobility restriction policies in mitigating or amplifying this effect. The research employs an econometric strategy to assess the effect of an exogenous change in urban poverty on the COVID-19 spread in American urban counties between February and April 2020. The time frame enables an evaluation of the pandemic's evolution before, during, and right after the introduction of travel bans and mobility restrictions. The study combines ACS data with mobility and COVID-19 confirmed cases data, and utilizes an instrumental variable approach to tackle biases from measurement errors and endogeneity. The findings reveal that a one standard deviation increase in urban poverty correlates with a rise of 0.55 to 0.7 cases of COVID-19 per 100,000 residents at the county level, which equates to a 10% increase in the average county-level incidence of new COVID-19 cases in high-incidence counties.

Additionally, the research investigates the interplay between urban poverty and lockdown policies in a dynamic fixed-effect model. Stay-at-home orders are not only found to be ineffective in reducing the incidence of COVID-19 at the county level, but may also contribute to a faster spread of the virus in cities with less distributed poverty across neighborhoods. This is attributed to factors associated with urban poverty, such as low homeownership rates and household overcrowding, which influence the pace of the pandemic's evolution. Consequently, the effectiveness of mobility restrictions is contingent upon the socioeconomic context in which they are implemented, as low-income individuals face greater constraints in complying with stay-at-home mandates.

Chapter 3 – The Pandemic's toll on domestic violence: Investigating the effect of COVID-19 public health measures. Chapter 3 investigates the influence of COVID-19 public health measures on domestic violence and examines the evolving nature of this effect over time. By utilizing Google search data related to domestic violence in 31 countries as a proxy of domestic violence, the study employs the Oxford COVID-19 Government Response Tracker to assess the impact of the timing and intensity of public health measures across countries. The findings indicate a significant increase in domestic violence as early as two weeks after implementing containment measures. While the effects recede gradually, fluctuations in the stringency of the measures persist in affecting domestic violence-related Google searches, even months following the initial introduction of public health policies.

Furthermore, the study tentatively explores the role of factors such as economic support policies and individual compliance in altering or exacerbating the effect of public health measures on domestic violence. The results suggest that, while individual compliance with measures may decline over time, potentially reducing their impact, economic support policies may inadvertently exacerbate domestic violence. This highlights the intricate interplay between public health measures and domestic violence, emphasizing the necessity for establishing safeguards to protect and shelter victims during crises that restrict external contacts while remaining in line with ongoing health circumstances. Overall, this study underscores the value of alternative data sources, such as Google search data, in providing insights into social phenomena during crises characterized by isolation measures and conventional data scarcity.

Chapter 4 – Psychotropic drug consumption during the COVID-19 pandemic in Luxembourg: Excess consumption and socio-demographic profile. Chapter 4 examines the evolution of psychotropic drug purchases in Luxembourg during the COVID-19 pandemic, contrasting them with pre-2020 trends. The study aims to evaluate the pandemic's mental health repercussions and identify asymmetries in medication purchases across different population subgroups. Using large-scale administrative data on quarterly psychotropic drug purchases from January 2016 to December 2021, the study uncovers no evident acceleration or overuse of psychotropic medications following the onset of the pandemic. However, differences emerge between drug classes: Purchases of antidepressants increase, while anxiolytics, hypnotics, and sedatives exhibit no significant increase or even a decrease in purchases.

The study reveals disparities in the evolution of psychotropic medication purchases by age, gender, household size and composition, employment status, and income. Younger individuals display the most substantial relative change in purchases, whereas older individuals experience minimal fluctuations. The analysis suggests that the pandemic has influenced mental health in Luxembourg, marked by an upswing in antidepressant purchases but no substantial increase in the purchases of anxiolytics, hypnotics, and sedatives. The study highlights the need for further research on the impact of the pandemic on mental health across diverse population subgroups.

Chapter 5 – Emotional barometers: Twitter emojis and emoticons as tools to gauge temperature's effect on mood. Chapter 5 explores the intricate dynamics between weather conditions, with an emphasis on temperature, and human moods, deploying a novel methodological strategy. This approach capitalizes on using emojis and emoticons within Twitter content as indirect mood indicators. This technique mitigates perennial concerns such as omitted variable bias and the challenges associated with small sample sizes while presenting an expansive, diverse sample that can capture real-time mood shifts. Analyzing a randomly selected array of geotagged tweets from Twitter users in the United States during 2014, the study captures the impact of various short-term weather variations. The evidence collected lends credence to the widely-held notion that weather conditions, particularly temperature changes, significantly affect individuals' moods. Typically, these impacts are negative, with dramatic temperature shifts triggering substantial mood swings.

Although the study provides valuable insights, it also acknowledges a few inherent limitations, such as the dependent variable being an imperfect – noisy – proxy for mood, reduced variation in response indicators due to binary response variables, and the potential underrepresentation of specific demographic groups, like the elderly, who may be more vulnerable to extreme weather conditions. Nevertheless, by analyzing daily fluctuations, the study brings a novel lens to the complex interplay between weather conditions and well-being. In addition, it puts forth a cost-effective, adaptable, and easily replicable framework for future examinations of mood indicators, particularly in countries where Twitter usage is widespread. This pioneering approach not only enhances the understanding of the mood-weather relationship but also provides a valuable tool for researchers investigating related areas.

Chapter 6 – Commuting time and absenteeism: Evidence from a natural experiment. Chapter 6 delves into the relationship between commuting time and work absenteeism, using a natural experiment and administrative data from Luxembourg that focuses on Belgian and French cross-border workers from 2015 to 2019. By exploiting roadworks as exogenous, short-lived variations in commuting, the study investigates the impact of a disruption in commuting time between September 2018 and April 2019 on absenteeism among Belgian cross-border workers compared to their unaffected French counterparts. The results reveal a positive relationship between commuting time and absenteeism, primarily due to increased sickness and family-related absences, suggesting a potential work-life balance recovery effect.

The study suggests that men are more affected by commuting time disruptions than women, indicating the possible influence of family responsibilities and existing flexible work arrangements on women's absenteeism behavior. Moreover, the research shows that workers commuting more than 40 km are most affected, while the distance to work alone plays a minor role in determining absenteeism. In addition, permanent contracts mitigate the effect of disruptions on absenteeism, while hourly wage and company size are positively associated with absenteeism.

Finally, the study includes a detailed analysis of different types of absences. Despite the adverse effects of commuting time on absenteeism, the absence of change in injury-related absences and the concentration of the notable rise in short-duration absences suggest that a work-life balance recovery channel is more likely to explain the observed increase in absences than a genuine health impact channel. Overall, the results are supported by multiple robustness checks, making this study a crucial contribution to understanding the relationship between commuting time and workplace absenteeism, particularly in light of the challenges posed by traffic and congestion in numerous urban areas.

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Understanding trends and drivers of urban poverty in American cities

Chapter 1

Understanding trends and drivers of urban poverty in American cities

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1.1 Introduction

The extent of concentration of poor people in some neighborhoods of a city is found to have medium- and long-term adverse effects on health outcomes (Ludwig et al., 2013; Ludwig et al., 2011), job opportunities (Conley & Topa, 2002) and well-being (Ludwig et al., 2012) for those exposed to it. The level of poverty concentration in a city is measured by the concentrated poverty index (P. A. Jargowsky & Bane, 1991; Wilson, 1987). This index indicates the share of the poor population in a city who lives in neighborhoods with a poverty incidence greater than or equal to a certain threshold (e.g., 20% is used for identifying high-poverty neighborhoods, whereas 40% is set for identifying extreme-poverty neighborhoods). Several studies examine the trends and drivers of concentrated poverty across American metropolitan areas. D. S. Massey et al. (1991) finds that residential segregation is the main driver of spatial concentration of poverty in American urban areas. Quillian (2012) investigates changes in concentrated poverty in American metro areas by developing a model incorporating variations in residential segregation. Iceland and Hernandez (2017) analyze the trends in concentrated poverty in American metropolitan areas over the 1980-2014 period and identify the variation in the segregation of poor people as a key driver of changes in concentrated poverty.

Albeit widely used in the empirical literature, the concentrated poverty index does not fulfill some desirable properties. Andreoli et al. (2021) have axiomatically derived a family of urban poverty indices which overcome some drawbacks of the concentrated poverty index. In particular, these indices produce evaluations of urban poverty that take into account (and make explicit the relation between) aspects of the incidence of poverty in the city, the distribution of poverty across high-poverty neighborhoods, and the extent of segregation of poor and non-poor residents between high- and low-poverty neighborhoods.

This paper studies the drivers of levels and changes in urban poverty across American metropolitan statistical areas (MSAs) between 1980 and 2014. To do so, we consider measuring urban poverty with selected indices belonging to the family characterized in Andreoli et al. (2021). Two indices within this family are the adjusted concentrated poverty index and the urban poverty index, calculated by setting a poverty incidence threshold for identifying high-poverty (or extreme-poverty) neighborhoods. A further index belonging to this family of urban poverty measures is the Gini index of inequality in neighborhood poverty incidence, which is obtained by considering all neighborhoods in a city, including medium- and low-poverty neighborhoods (i.e., setting a tolerance level to poverty incidence equal to 0% instead of 20% or 40%). Since the Gini index can be broken down into a neighborhood and a non-neighborhood component (Rey & Smith, 2013), the degree of spatial clustering of poverty across neighborhoods can be assessed directly within the urban poverty measurement framework. Furthermore, the change in the Gini index can be split into different components measuring the convergence and re-ranking of neighborhoods in terms of poverty incidence (Andreoli et al., 2021), adding information to the analysis of trends in urban poverty.

The aforementioned indices constitute the measurement apparatus we use to assess urban poverty for a panel of MSAs over the 1980-2014 period, built by exploiting rich data from the Census and the American Community Survey (hereafter, ACS). We produce a comparative analysis of the potential drivers of concentrated poverty and urban poverty. The partial effects of poverty drivers are obtained from pooled OLS regressions of the various indices of urban poverty concentration on a set of explanatory variables controlling for fixed effects for year, state, and region. We also examine the roles of the same explanatory variables in driving the changes in urban poverty concentration by running OLS regressions on the pooled period-to-period changes in the indices of concentrated poverty and urban poverty. A covariate describing gentrification at the census tract level is added in the regression analysis, as gentrification may cause changes in the composition of the population living in historically high-poverty neighborhoods of a city (Christafore & Leguizamon, 2019).

We further analyze the contribution of changes in the distribution of explanatory variables on urban poverty, net of the effect of changes in the correlation between these variables and urban poverty. To do so, we resort to an Oaxaca-Blinder decomposition (Blinder, 1973; Oaxaca, 1973), which separates the difference between the estimated average levels of urban poverty concentration in 1980 and 2014 into three different components. We find that American MSAs display substantial heterogeneity in urban poverty patterns and that both the re-ranking and convergence components of urban poverty changes are substantial across MSAs. The spatial component of urban poverty is negligible for most MSAs but very significant in the largest MSAs, where the clustering of high-poverty census tracts seems to be an issue. Demographic variables, income distribution, and the distribution of housing values within an MSA are major drivers of urban poverty concentration in American MSAs over the period considered.

The remainder of the paper is organized as follows: Section 1.2 outlines relevant literature on the consequences and drivers of neighborhood poverty and concentrated poverty, highlights critics of the latter measure, and presents the urban poverty index. Section 1.3 describes data and the covariates entered in regressions. Results are shown and discussed in Section 1.4. Section 1.5 concludes.

1.2 Urban poverty and concentrated poverty

1.2.1 Poverty in the city: Relevant literature

The empirical literature has brought evidence that growing up or living in poor neighborhoods is detrimental to a broad set of individuals' lifetime outcomes. From a short-term perspective, place matters for the cognitive development of children, with those living in high-poverty neighborhoods tending to have worse performances in mathematics and reading test scores (Pearman, 2019; Sharkey & Elwert, 2011; Vinopal & Morrissey, 2020; Wolf et al., 2017) and lower verbal abilities (Sampson et al., 2008) than their peers from other neighborhoods. When the focus is on the long-term outcomes, Chetty et al. (2016) and Chetty and Hendren (2018) show that more prolonged exposure to high-poverty neighborhoods during childhood has causal negative consequences on the economic opportunities of future generations, as it reduces college attendance and earnings and increases single parenthood rates, whereas Conley and Topa (2002) and Ludwig et al. (2012) find adverse effects on job opportunities and well-being, respectively. The estimated effects can be explained by the fact that residents in poor neighborhoods tend to be more isolated from the middle-class environment, having a bad connection to the labor market and access to low-quality schools and other public amenities (P. A. Jargowsky, 2013; Wang et al., 2018). In addition, there is also evidence that living in poor places is associated with poor health outcomes (Ludwig et al., 2013; Ludwig et al., 2011; Thierry, 2020) and increased drug use (Boardman et al., 2001; Nandi et al., 2010). Residents in poor neighborhoods are exposed to stressful circumstances which have a negative impact on some biomarkers of biological aging (Lei et al., 2018; Smith et al., 2017; Thierry, 2020) as well as to more environmental pollution due to the high density of industrial facilities, which reduces air quality (Ard & Smiley, 2022). Besides the deleterious impact on this wide range of individual outcomes, poor places act as a barrier reducing the likelihood of moving towards better neighborhoods and hence towards more opportunities for residents and their offspring (Alvarado & Cooperstock, 2021; Huang et al., 2021), contributing to the perpetuation of poverty and inequality of opportunities across generations.

The literature suggests that cities that differ in how poverty concentrates across neighborhoods may also vary regarding long-run well-being. Thus, assessing the distribution of poverty *within* a city is crucial for meaningful poverty comparisons *across* cities. To be able to perform meaningful comparisons, two fundamental issues need to be addressed: i) identify high- or extreme-poor neighborhoods and aggregate information about the distribution of poverty in those places, and ii) uncover the drivers and determinants of the implied level of urban poverty.

Comparisons of poverty distribution across cities can be made by aggregating evaluations of the distribution of poverty across neighborhoods of a city through an index, which is a function mapping information (specific to a city and period) about the distribution of poor and non-poor people across the city neighborhoods into a number, regarded to as the level of urban poverty of that city. When we make use of the term "city", we refer to a specific MSA, as defined by the American Census Bureau, observed in a given year. Each city is partitioned into

n non-overlapping *census tracts*, the smallest available statistical units for which a broad set of characteristics are observable from available census data. It is standard to use census tracts partition to define neighborhoods. For every census tract $i \in \{1, \ldots, n\}$, we observe the demographic size of the tract, denoted by $N_i \in \mathbb{R}_+$, with $N = \sum_{i=1}^{n} N_i$ being the overall population in the MSA, and the size of the group of individuals that are poor and reside therein, denoted by P_i, with $P = \sum_{i=1}^{n} P_i$ being the total number of poor in the MSA.

We represent a city by the corresponding *urban poverty configuration*, denoted by $\mathcal{A}$, which is a collection of counts of poor and non-poor individuals distributed across tracts of the MSA, so that $\mathcal{A} = \{P_i^{\mathcal{A}}, N_i^{\mathcal{A}}\}_{i=1}^{n}$. In what follows, we only use superscripts to indicate a specific urban poverty configuration when disambiguation is needed. The analysis of urban poverty that we make is conditional exclusively on the distribution of poor and non-poor individuals in space provided by an urban poverty configuration. The ratios $\frac{P_i}{N_i}$ and $\frac{P}{N}$ measure the incidence of poverty in tract i in the MSA, respectively. In this paper, the poor are always exogenously identified (for instance, by a federal poverty line for equivalent household income), and the focus is on how poor and non-poor individuals are distributed across census tracts. An urban poverty line $\zeta \in [0, 1)$ can be used to identify tracts where poverty is over-concentrated. A tract i is a high-poverty tract when $\frac{P_i}{N_i} \geq \zeta$. According to the Census Bureau, for instance, $\zeta = 0.2$ and $\zeta = 0.4$ identify high-poverty and extreme-poverty census tracts (ghettos), respectively, i.e., places where poor individuals count for above 20% or 40% of the resident populations. For a given urban poverty line ζ, there are $z \geq 1$ tracts where poverty is highly concentrated and $n - z \geq 0$ where poverty is not concentrated. Assume that tracts are ordered by decreasing magnitude of poverty incidence, so that $\frac{P_i}{N_i} \geq \frac{P_{i+1}}{N_{i+1}}$, we can hence denote $\overline{P}_z = \sum_{i=1}^{z} P_i$ and $\overline{N}_z = \sum_{i=1}^{z} N_i$.

The most widely used measure for urban poverty analysis is the *concentrated poverty* index (hereafter, CP) (Iceland & Hernandez, 2017; P. A. Jargowsky & Bane, 1991; Wilson, 1987). It measures the proportion of poor people who live in high-poverty census tracts as identified by urban poverty line ζ. Formally:

$$CP(\mathcal{A}, \zeta) = \frac{\overline{P}_z}{P}.$$

The index produces an aggregate evaluation of poverty that is meant to correlate with the incidence of the "double burden" of poverty, arising from the fact that poverty tends to concentrate in neighborhoods where poverty incidence is high, thus producing the external effects highlighted above. Understanding how concentrated poverty evolves over time, and its determinants, is a fundamental concern from a policymaker's perspective.

Several studies analyzing recent trends of concentrated poverty across MSAs document that concentrated poverty displays substantial heterogeneity across cities and over time, reflecting the socio-economic transformation in the US over the last decades. More specifically, after a growth in the share of the poor living in high-poverty neighborhoods occurred during the 1980s, the next decade was characterized by a sharp decline in concentrated poverty. However, this trend again reversed in the 2000s, when the Great Recession completely wiped the progress of the previous decade (P. Jargowsky, 2015; P. A. Jargowsky, 1997; Kneebone, 2014; Kneebone et al., 2011), the trend being similar in rural and metro areas (Thiede et al., 2018). Various factors have been identified as drivers behind these trends. D. S. Massey et al. (1991), D. Massey and Denton (1993), and D. S. Massey et al. (1994) find that residential segregation is the primary driver of spatial concentration of poverty in American urban areas. On the same line, Quillian (2012) investigates changes in concentrated poverty in American metro areas by developing a model incorporating variations in residential segregation, yielding similar effects. Racial segregation is found to explain only a small share of variations in trends of concentrated poverty across American cities (Iceland & Hernandez, 2017), whereas income segregation[1] and the extent of segregation of the poor is found to be positively and robustly associated with concentrated poverty (Bischoff & Reardon, 2014; Dwyer, 2012; Wilson, 1987). Overall, the worsening of the economic circumstances, exacerbated by the Great Recession, substantially contributed to increased poverty, one of the drivers of the recent re-emergence of concentrated poverty (Iceland & Hernandez, 2017).

Poverty incidence and the segregation of the poor are important dimensions of urban poverty that are not addressed by (but are correlated with) the concentrated poverty index. Andreoli et al. (2021) have highlighted some additional drawbacks of the concentrated poverty index, which have to do with the normative justification of the index. In particular, counterexamples can be constructed in which a movement of poor people from a low-poverty neighborhood towards a high-poverty neighborhood, i.e., a shift that unambiguously increases poverty concentration, can, in fact, reduce concentrated poverty as measured by CP. Andreoli et al. (2021) characterize a *urban poverty* index consistent with the implications of such transfer. We describe the index and its decomposition properties in the following section. The empirical analysis highlights trends and drivers of levels and changes in the urban poverty index across MSAs between 1980 and 2014.

1.2.2 Urban poverty measurement

The *urban poverty* index (hereafter, UP) combines aspects of incidence and distribution of poverty, and it is defined as follows (Andreoli et al., 2021):

$$UP(\mathcal{A};\zeta) := \beta\left(\frac{\overline{P}_z - \zeta\overline{N}_z}{P}\right) + \gamma\left(\frac{\overline{N}_z}{N}\right)\left(\frac{\overline{P}_z}{P}\right)G(\mathcal{A},\zeta) + \gamma\left(\frac{N-\overline{N}_z}{N}\right)\left(\frac{\overline{P}_z - \zeta\overline{N}_z}{P}\right), \tag{1.1}$$

where $\zeta \in [0,1)$, $\beta,\gamma \geq 0$ and $z \geq 1$. The index value is bounded below by zero, since when $z = 0$, $UP(\mathcal{A},\zeta) = 0$. The level of urban poverty measured by UP depends on its parametrization. The parameters β and γ have a normative interpretation. The parameter γ is the weight of the distributional component of urban poverty, which compounds information about the distribution of poverty across high-poverty neighborhoods $i = 1,\dots,z$ measured by the Gini index $G(\mathcal{A},\zeta)$, alongside information about the distribution of poverty across high- and low-poverty incidence neighborhoods. Instead, the parameter β is the weight of poverty incidence. A convenient weighting scheme assigns equal weight to both components, implying $\beta = \gamma = \frac{1}{2}$. Conversely, by setting $\gamma = 0$ and $\beta = 1$, we identify a specific measure of urban poverty, denoted the *adjusted concentrated poverty* index CP^* (hereafter CP^*). It is defined as follows:

$$CP^*(\mathcal{A},\zeta) := \frac{\overline{P}_z - \zeta\overline{N}_z}{P} = CP(\mathcal{A},\zeta) - \zeta\left(\frac{\overline{N}_z}{P}\right). \tag{1.2}$$

[1] Income segregation refers to the extent at which different income groups (poor, middle class, rich, for instance) are under- or over-represented in some neighborhoods compared to the city as whole. Measures of income segregation are conceptually different from concentrated poverty measures.

The index provides an adjustment of CP by a counterfactual level of poverty concentration in high-poverty tracts that can be tolerated according to the normative view expressed by the urban poverty line ζ.

Lastly, the UP index depends on the threshold ζ. An interesting case is when $\zeta = 0$, indicating that distributional concerns about poverty are extended to all neighborhoods of the city. By setting $\gamma = 1$ and $\beta = 0$, and noticing that when $\zeta = 0$ then $z = n$ and $\overline{P}_z = P$, it can be shown that the relevant urban poverty index converges to the Gini index of poverty incidence at the tract level, which can be written as

$$UP\,(\mathcal{A}, 0) = G(\mathcal{A}) := \frac{1}{2P/N} \sum_{i=1}^{n} \sum_{j=1}^{n} \left[\left(\frac{N_i N_j}{N^2} \right) \left| \frac{P_i}{N_i} - \frac{P_j}{N_j} \right| \right]. \tag{1.3}$$

The index has interesting decomposition properties useful for analyzing urban poverty distribution. As shown in Rey and Smith (2013), the Gini index can be additively decomposed into a neighborhood and a non-neighborhood component. This spatial decomposition of the Gini index relies on a binary spatial weights matrix that quantifies the spatial relationship between any two census tracts i and j within an MSA. More specifically, the ij-th element of the binary spatial weights matrix is equal to 1 if tracts i and j are considered close according to a given criterion,[2] and to 0 otherwise. Once a binary spatial weights matrix has been created, the Gini index can be split into a neighborhood component, which measures inequality among census tracts that are in spatial proximity, and a non-neighborhood component measuring inequality among non-neighboring census tracts. MSAs characterized by a positive spatial autocorrelation in poverty incidence tend to display little heterogeneity among neighboring tracts, and hence a neighborhood component that is relatively small compared to the non-neighborhood component of urban poverty. Conversely, a negative spatial autocorrelation in poverty incidence implies a large heterogeneity in neighboring tracts, hence a higher level of the neighborhood component (relative to the non-neighborhood component).

A further advantage of the index in equation 1.3 is that the year-to-year change in urban poverty from $\mathcal{A}_t$ to $\mathcal{A}_{t+1}$ can be broken down into components measuring different contributions. The quantity $\Delta G = G(\mathcal{A}_{t+1}) - G(\mathcal{A}_t)$ measures the change in urban poverty from time t to $t+1$. However, some aspects relevant to assessing the change in urban poverty may be neglected just by observing the variation in the index. Changes in the distribution of poor and non-poor people across census tracts may modify not only the relative difference in poverty incidence between tracts i and j in equation 1.3 but also the relative weight of that difference, which depends on tract population sizes.[3] To separate the roles of changes in tract population sizes and changes in tract poverty incidences when assessing urban poverty variations, Andreoli et al. (2021) broke down the change in urban poverty into three components, $\Delta G := W + R + D$. In the decomposition, W is the demographic component measuring the change in urban poverty due to changes in tract population sizes, while tract poverty incidences are kept fixed at time $t+1$. A positive (negative) value of W means that the relative weights of more unequal pairs of

[2]There are various criteria to establish whether two spatial units are close or not; among them, a main distinction can be made between the contiguity-based criteria (e.g., two spatial units are close if they share a common border) and the distance-based ones (e.g., two spatial units are close if the distance between their centroids is less than or equal to a chosen distance).

[3]The relative weight of the difference in poverty incidence for the pair of tracts i and j in t, $N_i^{\mathcal{A}_t} N_j^{\mathcal{A}_t} / \left(N^{\mathcal{A}_t}\right)^2$, may differ from that in $t+1$, $N_i^{\mathcal{A}_{t+1}} N_j^{\mathcal{A}_{t+1}} / \left(N^{\mathcal{A}_{t+1}}\right)^2$, for effect of changes in the relative distribution of population across census tracts.

tracts have increased (decreased) from t to $t+1$. Component D measures the change in urban poverty owing to the changes in relative disparities between tract poverty incidences from t to $t+1$, while the ranking of tracts by poverty incidence is kept fixed at time t. A negative (positive) value of D indicates that the relative disparities in poverty incidence between initially high-poverty and low-poverty tracts have become smaller (greater) in $t+1$. As D measures the extent to which the poverty incidences of initially high-poverty and low-poverty tracts have changed disproportionately, this component can be seen as a measure of convergence ($D < 0$) or divergence ($D > 0$) in poverty incidence across tracts.[4] When tract poverty incidences vary disproportionately, a re-ranking of tracts can occur as the poverty incidence of an initially lower-poverty tract can become greater than that of an initially higher-poverty tract. Component R measures the change in urban poverty due to the re-ranking of tracts from t to $t+1$. Since the re-ranking component is always non-negative ($R \geq 0$) (Jenkins & Van Kerm, 2016), the re-ranking component contrasts the effect of convergence ($D < 0$) whereas reinforces the divergence effect ($D > 0$) (O'Neill & Van Kerm, 2008).

Exploiting the linear aggregation features of the Gini index, Andreoli et al. (2021) showed that the three-term decomposition of the change in urban poverty can be further decomposed to separate the contribution of a change in poverty incidence at the MSA level, which otherwise remains incorporated in D.[5] component D can be factored as the product of two terms, $D = C \cdot E$. Component E is a measure of convergence (or divergence) in poverty incidence between tracts, just like component D, with the difference that E is calculated by assuming that the poverty incidence of the whole MSA is unchanged from t to $t+1$. Component E is, therefore, a "pure" measure of convergence in poverty incidence across tracts. C is a scaling factor, expressed as a function of the relative variation in the poverty incidence of the whole MSA from t to $t+1$.[6] A value of C smaller (greater) than 1 indicates that overall poverty incidence has increased (decreased) over time. The decomposition of the change in urban poverty becomes

$$\Delta G := W + R + C \cdot E. \tag{1.4}$$

As shown in Andreoli et al. (2021), ΔG and each of its components, except C being a scaling factor, can be split into a neighborhood and a non-neighborhood component:

$$\Delta G_N + \Delta G_{nN} := W_N + W_{nN} + R_N + R_{nN} + C \cdot (E_N + E_{nN}). \tag{1.5}$$

In equation 1.5, ΔG_N is the change in the neighborhood component of the index, and is given by the sum of the neighborhood components of W, R and E (scaled by C), $\Delta G_N := W_N + R_N + C \cdot E_N$; the same holds for the change in the non-neighborhood component of the index, $\Delta G_{nN} := W_{nN} + R_{nN} + C \cdot E_{nN}$.

[4] The interpretation of D is consistent with the approach suggested by O'Neill and Van Kerm (2008) to examine income convergence across countries. O'Neill and Van Kerm (2008) broke down the change in the Gini index, obtaining a two-term decomposition where a component assesses to what extent the incomes of poorer countries, initially at the bottom of the distribution, have grown proportionally more than those of richer countries at the top of the initial distribution. Therefore, such a component is considered a measure of β-convergence in income across countries (O'Neill & Van Kerm, 2008).

[5] The overall poverty incidence, P/N, changes also when all tract poverty incidences vary in the same proportion, while both D and R are equal to 0 in that case.

[6] c being the relative variation in overall poverty incidence, C is equal to $1/(1+c)$ and ranges between 0 and $+\infty$.

In the rest of the paper, we study the metro-level drivers of the indices $CP(.,\zeta)$, $CP^{*}(.,\zeta)$, $UP(.,\zeta)$, by setting the urban poverty threshold $\zeta = 0.2$ or $\zeta = 0.4$, and of G, corresponding to the case in which $\zeta = 0$.

1.3 Data

We use data produced by the US Census Bureau. Data for 1980, 1990, and 2000 are from the decennial census Summary Tape File 3A. Due to anonymization issues, the STF 3A data are given as statistical tables representative of the census tract level. After 2000, the STF 3A files were replaced with survey-based estimates of the income tables from the ACS, which has run annually since 2001 on representative samples of the US resident population. We focus on three waves of the 5-year module of ACS (estimates based on about 2% of the resident population): 2006-2010, 2010-2014, and 2012-2016. We interpret estimates from the ACS modules as representative for the mid-interval year, i.e., 2008, 2012, and 2014, respectively. These years roughly correspond to the onset, the striking, and the early aftermath of the Great Recession period (Jenkins et al., 2013; Thompson & Smeeding, 2013).

The census and ACS consistently report information about poverty incidence at the census tract level. Poverty incidence is measured by the number of individuals in families with total income below the poverty threshold, which varies by family size, number of children, and age of the family householder or unrelated individual. Poverty status is determined for all families (each family member is assigned the same status). Poverty status is also determined for persons not in families, except for inmates of institutions, members of the Armed Forces living in barracks, college students living in dormitories, and unrelated individuals under 15 years of age.[7] The census reports poverty counts at the census tract level for various poverty thresholds. This paper considers poor households with income below the 100% federal poverty line.

Poverty counts are estimated separately for each census tract in America. Following Andreoli and Peluso (2017), we consider the 2016 Census Bureau definition of MSA to aggregate census tracts into cities. The number and geographic size of the census tracts vary substantially over time within the same MSA. Some census tracts increase in population and are split into smaller tracts. Some other tracts may be consolidated to account for demographic shifts. While raw data allow us to estimate urban poverty at the MSA level, they cannot be used to perform the decomposition exercise because the neighborhood definition is not constant over time. We resort to the Longitudinal Tract Data Base (LTDB), which provides crosswalk files to create estimates of census tables based on the 2010 tract boundaries for any tract-level data that are available for prior years as well as in ACS following years (Logan et al., 2014). These files use re-weighting methods to assign the population in each census and ACS year to the exact census tract boundary defined in the 2010 census. In this way, we can construct a

[7] Both Census 1990 and 2000 and ACS determine a family poverty threshold by multiplying the base-year poverty thresholds (1982) by the average of the monthly inflation factors for the 12 months preceding the data collection. The poverty thresholds in 1982, by size of family and number of related children under 18 years, can be found on the Census Bureau website: *https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-thresholds.html*. For a four persons household with two underage children, the 1982 threshold is \$9,783. Using the inflation factor of 2.35795 gives a poverty threshold for this family in 2013 of \$23,067. If the disposable household income is below this threshold, all four household members are recorded as poor in the census tract of residence and included in the 2014 wave of ACS.

balanced longitudinal database of census tracts for 395 MSAs (those with at least 10 census tracts according to the 2010 census) for the years 1980, 1990, 2000, 2008, 2012, and 2014. We calculate poverty incidence in each census tract/year and then construct measures of urban poverty and concentrated poverty in high (i.e., where poverty incidence is above 20% of the resident population) and extreme (i.e., where poverty incidence is above 40% of the resident population) poverty tracts.

The incidence of poverty at the MSA level is always below 16% on average in the sample under consideration. More than 93% of these MSAs display at least one census tract with a poverty incidence greater than 20%. The average number of census tracts by MSA that display more than 20% (40%) poverty incidence has more than doubled over 35 years, from 21.6 (5.4) in 1980 to 45.2 (10.8) in 2014. The balanced panel allows us to further decompose changes in urban poverty in its underlying components and to study convergence/divergence in urban poverty incidence at the tract level. Census tracts are also geolocalized, implying that measures of the proximity of these tracts can be further produced to separate the neighborhood and non-neighborhood components of urban poverty.

1.3.1 Poverty

In this study, the dependent variables are the measures of concentrated poverty (CP), urban poverty (CP^*, UP, and G), and the components of the change in the urban poverty Gini index (G). These measures are constructed from the observed poverty incidence P_i/N_i in census tract i, which is calculated by using the count of equivalent individuals that are poor in the tract relative to the demographic size of the tract. When calculating CP and UP, the reference urban poverty thresholds provided by the Census Bureau are used to define high-poverty tracts (if the local poverty incidence is greater than 20%) and extreme-poverty tracts (if the local poverty incidence is greater than 40%). The Gini index of tract poverty incidence is obtained by setting the poverty threshold equal to $\zeta = 0$, implying that all census tracts of an MSA are considered when measuring urban poverty in the MSA. Table 1.1 provides descriptive statistics (mean, 25th percentile, and 75th percentile) of the distributions of the changes in CP, UP, and G in five sub-periods of the 1980-2014 period. We also report the components of the change in G in Table 1.1. While ΔCP, ΔCP^*, ΔUP, and ΔG are overall measures of the change in urban poverty concentration, the various components of ΔG (C, D, E, R and W) measure the effects of specific distributional changes in the distribution of poverty within an MSA.

We also consider a measure of convergence alternative to D and E. Such a measure is obtained by regressing the year-to-year log-change in poverty incidence P_i/N_i of each tract i of a given MSA on the log-level of tract poverty incidence in the initial period, getting a year-MSA-specific measure of β-convergence. β-convergence occurs when the partial correlation between the log-change in tract poverty incidence and the initial log-level of tract poverty incidence is negative (Barro & Sala-i-Martin, 1992). We estimate the partial correlation coefficient via OLS for each MSA and each year-to-year change. The descriptive statistics for the distribution of the partial correlation coefficients estimated for the 395 MSAs in each sub-period are reported in the last row of Table 1.1, denoted by β(log-log).[8] These descriptive statistics are negative for each sub-period, suggesting that poverty

[8] β here indicates the type of convergence and should not be confused with parameter β in equation 1.1.

Variable	Statistics	1980-1990	1990-2000	2000-2008	2008-2012	2012-2014
	mean	.0958	-.0474	.104	.0491	-.0153
$\Delta CP(., 0.2)$	pc(25)	.0089	-.1	.0305	-.0028	-.0487
	pc(75)	.1722	.0105	.1697	.0997	.0123
	mean	.0578	-.0393	.0461	.021	-.0118
$\Delta CP(., 0.4)$	pc(25)	0	-.0736	0	-.0013	-.0325
	pc(75)	.0954	0	.0841	.0535	.0055
	mean	.0498	-.0259	0	.0664	-.0104
$\Delta CP^*(., 0.2)$	pc(25)	.0134	-.0537	0	.0326	-.0234
	pc(75)	.0833	-.0003	0	.096	.0032
	mean	.013	-.0082	.01	.0032	-.002
$\Delta CP^*(., 0.4)$	pc(25)	0	-.0122	0	-.0015	-.0062
	pc(75)	.0191	0	.0163	.009	.0014
	mean	.0447	-.0226	0	.0571	-.0087
$\Delta UP(., 0.2)$	pc(25)	.0126	-.0467	0	.0286	-.0198
	pc(75)	.0753	0	0	.0831	.0029
	mean	.0124	-.0078	.0096	.003	-.0019
$\Delta UP(., 0.4)$	pc(25)	0	-.0121	0	-.0014	-.0059
	pc(75)	.0188	0	.016	.0088	.0014
	mean	.0314	-.0038	.0153	-.016	-.0016
$\Delta G(.)$	pc(25)	.0026	-.0258	-.0109	-.0337	-.0135
	pc(75)	.0592	.015	.0401	.0009	.0097
	mean	.0151	.0015	.0121	-.0077	-.0001
$\Delta G(.)$ (not-nbh)	pc(25)	-.0042	-.0097	-.001	-.0181	-.0054
	pc(75)	.0305	.0106	.0247	.0011	.0051
	mean	.0163	-.0053	.0032	-.0083	-.0014
$\Delta G(.)$ (nbh)	pc(25)	-.0029	-.0206	-.0122	-.0164	-.0076
	pc(75)	.0355	.0069	.0169	.002	.0048
	mean	.0041	.0008	.003	.0006	.0004
W	pc(25)	-.0043	-.005	-.0018	-.0015	-.0007
	pc(75)	.0138	.0062	.0069	.0028	.0016
	mean	.0525	.0462	.0664	.0556	.0277
R	pc(25)	.0357	.0318	.0463	.0391	.0192
	pc(75)	.0628	.0567	.0808	.0663	.0325
	mean	-.0328	-.0507	-.0682	-.082	-.0291
E	pc(25)	-.0612	-.0733	-.1017	-.107	-.0411
	pc(75)	.0046	-.0275	-.034	-.0562	-.0157
	mean	.8869	1.044	.8257	.8923	1.0289
C	pc(25)	.7709	.9462	.7387	.8437	.9955
	pc(75)	1.0091	1.1325	.9054	.9308	1.0556
	mean	-.0252	-.0508	-.054	-.0722	-.0297
$D = C \cdot E$	pc(25)	-.0527	-.0727	-.0808	-.0925	-.0419
	pc(75)	.0042	-.028	-.0276	-.0515	-.016
	mean	-.1306	-.1935	-.1694	-.2666	-.1228
β (log-log)	pc(25)	-.2477	-.2679	-.2492	-.3386	-.1712
	pc(75)	-.0247	-.1094	-.0802	-.1892	-.067

Table 1.1: Summary statistics of changes in urban poverty concentration, all 395 American MSAs

incidence grew less in census tracts where poverty was already highly concentrated in the initial period.

1.3.2 Covariates

Explanatory variables are also drawn from STF3A Census files and from ACS. We identify two categories of covariates. For non-monetary characteristics, Census and ACS report information about the number of individuals reporting one specific attribute and living in a given census tract. We aggregate information at the MSA level and then standardize population counts by the appropriate population so that all variables can be interpreted as population shares ranging between 0 and 1. For monetary variables, the Census and ACS report information about the total aggregate value in current dollars of that variable at the census tract level. We aggregate measures at the MSA level and compute per capita or per census tract values. Monetary variables always appear in logs after being actualized at the 2010 prices by using the CPI seasonally adjusted estimates for all US urban consumers (obtained from the Bureau of Labor Statistics).

Census and ACS data come in tabulations by census tract level. We extrapolate information from these tables and aggregate it at the level of the MSA to produce relevant control variables. We construct a dataset of census tract characteristics for 395 MSAs (those with at least 10 census tracts according to the 2010 census) for the years considered in this study. The sample of MSAs we consider is grouped by region: Northeast (12.66%), Midwest (27.34%), South (39.75%), and West (20.25%). Table 1.2 reports unweighted means and standard deviations of the variables observed in the sample of MSAs that we use to perform regression analysis.

The covariates can be grouped into five dimensions: demographics, housing, education, employment, and distributive aspects. Demographics (A) includes the total size of the population (expressed in log) and its composition in terms of both racial/ethnic, age, and origin groups (*Foreign* captures the proportion of non-US citizens and *Moved from outside of state* the proportion of those who declared to have moved from another US State to the MSA in previous years), which are expressed in terms of shares with respect to the entire population of the MSA.

The second group of control variables gathers the MSAs' housing characteristics (B). We consider the shares of new and old houses aged less than 10 years (*New Houses (10 less yrs old)*) or more than 20 years (*Old houses (20 plus yrs old)*) respectively. These variables capture the aggregate quality of the MSA housing market. We further distinguish houses according to the occupant subject by considering the share of houses that are rented (*Rented*) or vacant (*Vacant*) to the total number of houses. The variable *Owner occupied* refers to the share of houses occupied by the owner. The tenure status of the houses is a strong predictor of housing opportunities for low-income, renting households. Lastly, we include variables for the value of owner-occupied houses and for the value of rents that are averaged across households (*Avg. value house (ln)* and *Avg. rent (ln)*). We also consider the distributions of owner-occupied housing values and rents across neighborhoods. This information allows us to distinguish the situations in which low-rent/low-value houses are equally represented across all neighborhoods of the city (in which case the median rent by census tracts would coincide across census tracts) from the situations where the rents/values are highly heterogeneous across neighborhoods (in which case we would expect significant variance in median values and rents by census tracts, with some census tracts being more affordable than others).

	1980		1990		2000		2008		2012		2014	
	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
$CP(.,0.2)$	0.32	0.20	0.41	0.20	0.37	0.20	0.47	0.17	0.52	0.17	0.50	0.17
$CP(.,0.4)$	0.06	0.10	0.12	0.14	0.08	0.11	0.13	0.12	0.15	0.13	0.14	0.13
$CP^*(.,0.2)$	0.10	0.09	0.15	0.10	0.12	0.09	0.12	0.09	0.19	0.09	0.18	0.09
$CP^*(.,0.4)$	0.01	0.02	0.02	0.03	0.01	0.03	0.02	0.03	0.03	0.03	0.03	0.03
$UP(.,0.2)$	0.09	0.07	0.14	0.09	0.11	0.08	0.11	0.08	0.17	0.07	0.16	0.08
$UP(.,0.4)$	0.01	0.02	0.02	0.03	0.01	0.02	0.02	0.03	0.03	0.03	0.02	0.03
$G(.)$	0.33	0.09	0.36	0.09	0.35	0.09	0.37	0.08	0.35	0.07	0.35	0.07
$G(.)$ (nbh)	0.16	0.08	0.18	0.09	0.18	0.09	0.18	0.09	0.17	0.09	0.17	0.09
$G(.)$ (no-nbh)	0.16	0.09	0.18	0.10	0.18	0.10	0.19	0.10	0.18	0.10	0.18	0.10
A) % Black	0.09	0.10	0.10	0.11	0.10	0.11	0.11	0.11	0.11	0.11	0.11	0.11
A) % Hispanic	0.05	0.10	0.06	0.12	0.09	0.14	0.12	0.15	0.13	0.16	0.13	0.16
A) % Asian	0.03	0.07	0.02	0.06	0.02	0.05	0.03	0.05	0.03	0.05	0.03	0.05
A) % White	0.82	0.16	0.82	0.16	0.77	0.17	0.74	0.18	0.72	0.19	0.72	0.19
A) Population size (ln)	12.43	1.05	12.30	1.13	12.40	1.14	12.47	1.16	12.50	1.17	12.51	1.18
A) % 65 plus yrs old	0.11	0.03	0.13	0.03	0.13	0.03	0.14	0.03	0.15	0.03	0.16	0.03
A) % 25-64 yrs old	0.48	0.03	0.50	0.03	0.51	0.03	0.54	0.03	0.53	0.03	0.53	0.02
A) % Foreign	0.04	0.04	0.04	0.04	0.06	0.06	0.05	0.04	0.05	0.04	0.04	0.04
A) Moved from outside of state	0.11	0.07	0.10	0.05	0.43	0.06	0.03	0.02	0.03	0.01	0.03	0.01
B) % New Houses (10 less yrs old)	0.14	0.05	0.21	0.09	0.18	0.07	0.13	0.06	0.16	0.07	0.17	0.07
B) % Old houses (20 plus yrs old)	0.77	0.06	0.56	0.13	0.67	0.12	0.73	0.10	0.70	0.11	0.69	0.11
B) % Owner occupied	0.72	0.06	0.70	0.06	0.71	0.06	0.71	0.06	0.68	0.06	0.67	0.06
B) % Vacant	0.03	0.02	0.10	0.06	0.09	0.05	0.12	0.05	0.12	0.06	0.12	0.06
B) % Rented	0.32	0.06	0.33	0.06	0.32	0.06	0.32	0.06	0.34	0.06	0.34	0.06
B) Avg. value house (ln)	4.26	0.40	11.05	0.38	11.30	0.36	11.03	0.49	10.94	0.46	10.94	0.50
B) Median value house by CT (ln)	10.10	0.40	11.68	0.40	11.84	0.38	12.80	0.49	12.67	0.42	12.67	0.43
B) p25% value house by CT (ln)	9.40	0.54	11.35	0.40	11.53	0.38	11.73	0.49	11.62	0.43	11.61	0.45
B) S.d. value house by CT (ln)	9.66	0.43	10.74	0.56	10.81	0.56	11.03	0.58	10.98	0.54	11.00	0.58
B) Avg. rent (ln)	17.36	1.27	17.84	1.28	17.79	1.31	17.97	1.33	18.05	1.33	18.06	1.34
B) Median rent by CT (ln)	8.79	0.16	6.56	0.22	6.41	0.27	6.48	0.29	6.53	0.28	6.53	0.28
B) p25% rent by CT (ln)	8.61	0.17	6.39	0.22	6.22	0.26	6.29	0.28	6.35	0.27	6.35	0.27
B) S.d. rent by CT (ln)	7.35	0.39	5.19	0.47	5.01	0.52	5.11	0.50	5.12	0.49	5.12	0.50
C) College Town	0.10	0.31	0.11	0.31	0.10	0.31	0.10	0.31	0.10	0.31	0.10	0.31
C) Student Town	0.05	0.22	0.05	0.22	0.05	0.22	0.05	0.22	0.05	0.22	0.05	0.22
C) % less than high school	10.49	3.63	6.38	2.70	99.35	2.22	3.73	2.17	3.46	2.11	3.28	2.04
C) % with high school	0.30	0.04	0.32	0.04	0.02	0.01	0.27	0.05	0.21	0.05	0.21	0.05
C) % with college	9.15	3.18	15.37	4.13	41.70	5.69	31.64	5.63	38.28	5.63	39.23	5.67
C) % enrollment (any)	0.23	0.02	0.27	0.04	0.15	0.02	0.14	0.02	0.14	0.02	0.13	0.02
D) % Managerial position	0.22	0.04	0.24	0.04	0.31	0.05	0.32	0.05	0.33	0.05	0.34	0.06
D) % Timework	34.33	5.00	34.53	4.29	32.84	4.97	33.24	5.16	32.12	5.24	32.32	5.29
E) Avg hh income (ln)	10.39	0.33	11.03	0.16	11.14	0.16	11.04	0.18	11.03	0.18	11.03	0.19
E) Median hh income by CT (ln)	10.75	0.15	10.82	0.19	10.90	0.18	10.88	0.18	10.86	0.19	10.85	0.19
E) p25% hh income by CT (ln)	10.51	0.18	10.53	0.22	10.64	0.19	10.59	0.20	10.56	0.21	10.55	0.21
E) S.d. hh income by CT (ln)	9.57	0.34	9.78	0.35	9.76	0.37	9.81	0.33	9.80	0.34	9.78	0.34
E) % of poor	0.12	0.04	0.13	0.05	0.12	0.04	0.15	0.04	0.16	0.04	0.16	0.04
E) Dissimilarity poor	0.27	0.08	0.31	0.08	0.30	0.08	0.32	0.07	0.31	0.07	0.31	0.07
E) Dissimilarity whiteblack	0.59	0.13	0.56	0.12	0.52	0.12	0.53	0.10	0.52	0.10	0.51	0.10
E) Dissimilarity black	0.54	0.13	0.50	0.12	0.46	0.12	0.46	0.11	0.44	0.11	0.44	0.10
E) Dissimilarity hispanic	0.34	0.10	0.35	0.10	0.33	0.10	0.35	0.10	0.33	0.10	0.32	0.09
E) Dissimilarity asian	0.42	0.11	0.47	0.11	0.43	0.09	0.48	0.10	0.47	0.10	0.46	0.10
E) Dissimilarity white	0.08	0.07	0.09	0.07	0.10	0.08	0.11	0.08	0.12	0.08	0.12	0.08
N. of CT	107.88	288.90	125.77	318.49	135.38	332.47	152.15	357.96	152.23	358.31	152.19	358.29
N. of CT with 20% of poor	21.60	66.25	30.89	75.88	29.37	86.32	39.04	89.18	46.93	106.39	45.25	103.88
N. of CT with 40% of poor	5.43	22.78	10.90	30.42	6.64	22.97	9.52	21.14	12.07	27.12	10.86	24.62
Observations	395		367		395		395		395		395	

Table 1.2: Descriptive statistics of regressors

Starting from the observation of the median value/rent at the census tract level, we aggregate distributional features of median housing values/rents across census tracts into median (*Median value house by CT (ln)* and *Median rent by CT (ln)*), first quartile of the housing value and rent distribution (*p25% value house by CT (ln)* and *p25% rent by CT (ln)*) and dispersion (*S.d. value house by CT (ln)* and *S.d. rent by CT (ln)*). All values are expressed in log terms.

The third group of covariates we examine reports information about education and human capital (C). We separately consider three dimensions of education. First, we consider the proportion of the resident population aged 25 or above in a given MSA that has low education (*Less than high school*), some qualification at high school level (*With high school*) and tertiary education or above (*With college*). These variables are meant to measure the human capital composition of an MSA, which reflects both historical trends and residential choices of low and high-educated people based on specific characteristics of the labor market and the supply of services and amenities produced at the census tract and MSA level. Second, we consider the share of the population that is actually enrolled in any form of education (*Enrollment (any)*) as a measure of the demand for consumption of education services in the MSA. Third, we introduce indicators for whether the MSA is a college or student town. The former (*College Town*) identifies MSAs where most selective American colleges are located. The selectivity level is measured according to the college tier description used by the Department of Education (DOE) IPEDS database. We consider as college towns those MSAs hosting colleges of tier levels equal to 1 or 2, which are associated respectively with Ivy League colleges plus Stanford, Chicago, Duke, and MIT alongside other elite schools (both public and private) with Barron's 2009 selectivity index of 1. The second indicator (*Student Town*) identifies the top 20 MSA with the highest number of students enrolled in any college. The number of students refers to the number of IPEDS enrollment (full-time and part-time) in the fall 2013 semester.[9]

The employment structure (D) of the MSA is described by the share of workers occupied with managerial positions (*Managerial Position*) and by the share of workers less than half an hour away from the workplace (*Timework*). Both shares are computed with respect to the total population.

Lastly, to take into account the distributive aspects (E) of income, poverty, and ethnicity within MSA, we control for average household income in the city (*Avg hh income (ln)*) as an objective measure of well-being. The income distribution across census tracts signals the tracts' quality and affordability. We use measures in the census and ACS about median income in the census tract and compute measures of the distribution of incomes across census tracts considering the median affluence of the tracts (*Median hh income by CT (ln)*), the household income for poorest 25% of the census tracts (*p25% hh income by CT (ln)*) and a measure of dispersion of income across census tracts (*S.d. hh income by CT (ln)*). We also consider information about the poverty incidence in the MSA as a whole (*Fraction of poor*) and the way poor and non-poor people (according to the 100% federal poverty line) are unevenly represented across the census tracts (*Dissimilarity poor*).[10] Finally, we measure the ethnic dimension of segregation across the MSA census tracts by using standard measures of segregation (dissimilarity

[9]For a detailed description of the variables used to construct our indicators, see Chetty et al. (2017) and Table 6 and Table 10 at `https://opportunityinsights.org/data/`.

[10]The unevenness dimension is captured by the dissimilarity index, measuring the proportion of poor individuals that should move to restore proportionality across the MSA tracts (about 30% on average across all MSAs), see Andreoli and Zoli (2014).

index) for the Whites, Blacks, Hispanics, and Asians with respect to the overall population, as well as traditional measures of black and white segregation (*Dissimilarity white-black*).

	1980-1990		1990–2000		2000–2008		2008–2012		2012–2014	
	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
F) % gentrified CT (BSM def.)	0.30	0.04	0.31	0.02	0.28	0.06	0.26	0.09	0.27	0.07
F) % stable CT (BSM def.)	0.35	0.04	0.35	0.02	0.36	0.07	0.37	0.09	0.37	0.08
F) % declining CT (BSM def.)	0.35	0.05	0.34	0.02	0.36	0.07	0.37	0.08	0.36	0.05
Observations	391		392		394		394		393	

Table 1.3: Descriptive statistics of gentrification

Note: Gentrification is defined according to Baum-Snow and Marion (2009). Gentrified census tracts are those located in the top tercile of the distribution of housing value appreciation between year t and $t+1$. Census tracts in the middle and bottom terciles are considered stable and declining, respectively.

A related aspect to the quality and affordability of tracts is gentrification, which appears as both the cause and the consequence of the level of urban poverty. Gentrification, induced by the inflow of young, middle-class cohorts into the most affordable and historically high-poverty tracts in inner cities, displaces poor residents towards traditionally middle-class and low-poverty neighborhoods which become more mixed. As a consequence of such a displacement, most non-gentrifying tracts tend to attract disproportionately more poor than other tracts (rising poverty concentration). However, the redistribution of poverty from inner cities towards more marginal neighborhoods makes the distribution of poverty more widespread across neighborhoods. In this paper, we follow Baum-Snow and Marion (2009) and consider the housing value variations at the census tract level to measure gentrification.[11] More specifically, for each census tract, we compute the variation in the housing average value between years t and $t+1$ and consider the distribution of these variations at the MSA level. Then, in each period and in each MSA, gentrified census tracts are those located in the top tercile of the distribution of housing value appreciation. Tracts in the middle and bottom terciles are defined as stable and declining tracts, respectively. Table 1.3 reports the gentrification indicators for the 1980-2014 period.

1.4 Results

1.4.1 Trends in urban poverty

In the last decades, there has been evidence of convergence in poverty across American MSA neighborhoods, with poverty growing everywhere in cities after the Great Recession, but less so in high-poverty neighborhoods. Poverty has been increasingly concentrated in historically middle-class, low-poverty neighborhoods. The set of measures of urban poverty described in previous sections can capture some relevant features of this secular trend. More specifically, we consider the trends in CP and UP for urban poverty lines at 20% (i.e., $\zeta = 0.2$) and 40% (i.e., $\zeta = 0.4$), and the trends in CP^* and G, which are obtained in correspondence of a specific choice of the parameters β, γ and ζ in the general formula of UP (i.e., $\beta = 1$ and $\gamma = 0$ for CP^*, and $\beta = 0$, $\gamma = 1$ and

[11] See Christafore and Leguizamon (2019) for alternative definitions of gentrification.

$\zeta = 0$ for G). We then calculate the correlations between the year-to-year changes in these measures and the MSA-specific measure of β-convergence in poverty incidence across tracts (see table 1.1).

	1980-1990 (1)	1990-2000 (2)	2000-2008 (3)	2008-2012 (4)	2012-2014 (5)
$\Delta CP(.,0.2)$	0.130**	0.164**	0.151**	0.112**	0.172**
$\Delta CP(.,0.4)$	0.163**	0.034	0.266**	0.101**	0.150**
$\Delta CP^{*}(.,0.2)$	0.207**	0.097*	0.000	0.209**	0.320**
$\Delta CP^{*}(.,0.4)$	0.160**	0.026	0.257**	0.142**	0.139**
$\Delta UP(.,0.2)$	0.225**	0.112**	0.000	0.186**	0.325**
$\Delta UP(.,0.4)$	0.170**	0.033	0.259**	0.141**	0.137**
$\Delta G(.)$	0.576**	0.543**	0.437**	0.483**	0.638**
$\Delta G(.)$ (nbh)	0.441**	0.355**	0.381**	0.400**	0.571**
$\Delta G(.)$ (no-nbh)	0.387**	0.463**	0.298**	0.414**	0.509**
R	-0.317**	-0.362**	-0.408**	-0.507**	-0.416**
E	0.680**	0.675**	0.681**	0.700**	0.751**
$C \cdot E$	0.722**	0.704**	0.723**	0.725**	0.750**
MSA	395	395	395	395	395

Table 1.4: Correlations of β-convergence (log-log specification) and measures of urban poverty concentration, by year-to-year changes

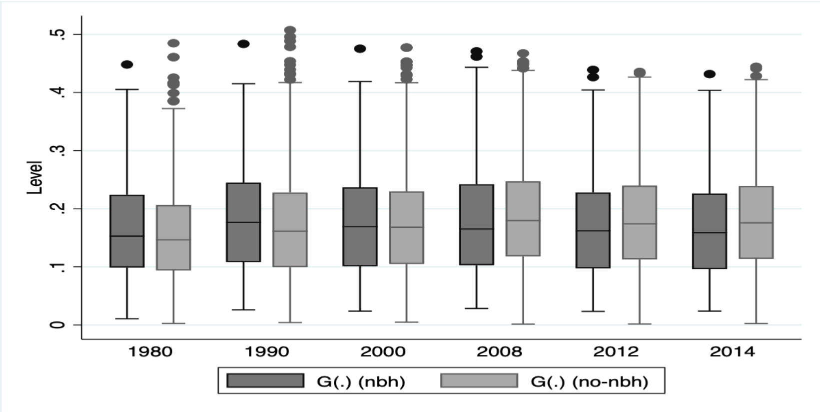
Note: Significance levels: * = 10%, ** = 5%, and *** = 1%.

Table 1.4 shows that correlations are all statistically significant in the last two sub-periods considered, suggesting that all measures of urban poverty agree on the convergence of tracts within MSAs regarding poverty incidence. However, these correlations are different in terms of magnitude, with mild levels for the general measures of urban poverty concentration (CP, UP, and CP^{*}) and higher levels for the components of the change in Gini index, especially E and consequently $D = C \cdot E$. Component E is highly and positively correlated with β-convergence estimates. This indicates that MSAs where poverty growth is clustered in low-poverty tracts on average (higher levels of β-convergence) also display relatively low urban poverty. Component R is negatively and significantly related to the extent of β-convergence. MSAs where poverty growth is clustered in low-poverty tracts also display major changes in the map of poverty, with poverty growing proportionally much less, or even decreasing, in high-poverty tracts compared to the growth observed in low-poverty ones. This combination of changes induces a substantial re-ranking of tracts.

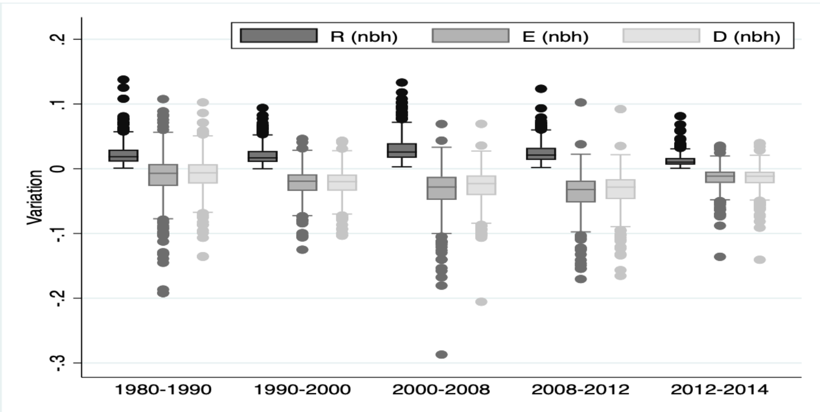
An advantage of using the Gini index to measure urban poverty is that each component of the change in urban poverty can be split into neighborhood and non-neighborhood components.[12] Figure 1.1 shows the box plots of the distributions of the spatial components of ΔG, R, D, and E. From figure 1.1, we see that convergence in tract poverty incidence occurs both among neighboring census tracts and among non-neighboring census tracts, as the

[12] A spatial weights matrix representing the spatial relationships between census tracts in an MSA is needed to obtain the spatial decomposition. We specify a binary spatial weights matrix, the ij-th element of which equals 1 if tracts i and j are neighboring and 0 otherwise. A distance-based criterion establishes whether two tracts are neighboring (Andreoli et al., 2021). More specifically, two tracts are considered close if the distance between their centroids is less than or equal to a cut-off distance, which is set equal to the minimum distance for which every tract in an MSA has at least one neighbor.

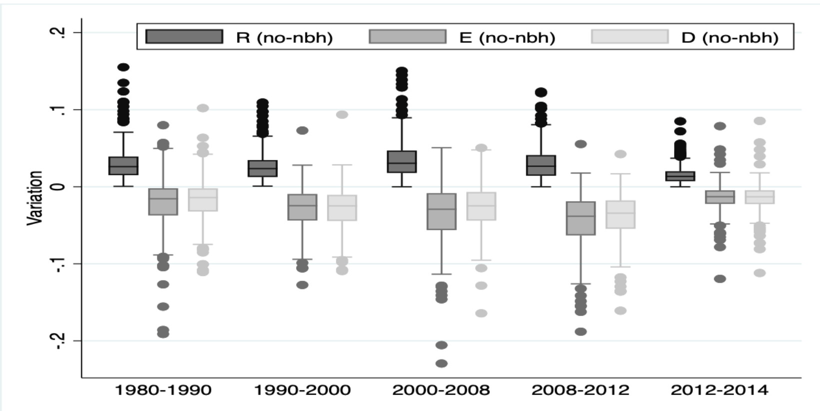
Figure 1.1: Neighborhood and non-neighborhood components of urban poverty levels and year-to-year changes



(a) G levels components



(b) Neighborhood components of inequality changes (ΔG_N)



(c) Non-neighborhood components of inequality changes (ΔG_{nN})

Note: Inequality changes and its components (R, E and $D = C \cdot E$), 1980-2014. Data for 395 MSAs.

values of E_N and E_{nN} are mainly negative. However, the re-ranking effect mitigates the convergence process among neighboring and non-neighboring census tracts.

1.4.2 Drivers of concentrated poverty and urban poverty: Levels

We produce comparative evidence about potential drivers of concentrated poverty and urban poverty and study the partial associations of these variables with the levels and changes in urban poverty across American MSAs. Table 1.5 reports estimates of the effects of the relevant drivers. Values of these indicators are obtained for each MSA and year. The partial correlations of urban poverty with the relevant drivers are obtained from pooled OLS regressions controlling for year, state, and region fixed effects and including a binary indicator for the Great Recession (years 2008 to 2012).

Regression results show that the demographic composition in terms of origin group is correlated with CP^* and UP calculated for both high (20%) and extreme (40%) poverty thresholds. The population distribution by age is correlated with CP^* and UP when the focus is on extreme poverty. Lower levels of urban poverty are associated with a larger share of the population aged 25 or above. This may be explained by the fact that people aged 25-64 are more likely to be employed, while those aged 65 or above are mainly retired. The education and employment composition, strongly associated with opportunities offered by the labor market, correlate with CP, CP^*, and UP for both of the poverty thresholds we consider. MSAs with higher poverty concentration are characterized by lower shares of a highly educated population and living near the workplace. MSAs with higher shares of workers holding a managerial position tend to have higher levels of urban poverty, except when $CP\,(., 0.2)$ is considered.

The distribution of income across census tracts, as well as the features of the housing market, have important implications for $UP(., 0.2)$, $CP^*(., 0.2)$, and G. This evidence can be reconciled with the implications of neighborhood affordability on the geography of poverty. MSAs with a higher median income across census tracts (holding average household income as fixed) display more income mix at the tract level and less inequality across tracts (as the median converges to the average, held fixed). This pattern of income sorting may indicate more widespread access to urban amenities and localized public goods and hence lower incentives for high and low-income families to sort unevenly across census tracts.

All estimated models agree that the poverty incidence in the whole MSA and the degree of dissimilarity in the distribution of poor within the MSA are important drivers of urban poverty. While the dissimilarity in the distribution of poor is strongly positively correlated with every measure of poverty concentration, poverty incidence positively impacts all measures except G. This is a consequence of the fact that the index G focuses on the inequality in poverty incidence across tracts, and such inequality is less emphasized in MSAs where a large fraction of the population is poor. While the impacts of dissimilarity in the distribution of poor and poverty incidence in the MSA on urban poverty are not unexpected, they may be relevant to anti-poverty policymaking when the effects of the remaining explanatory variables are controlled for. Regression results suggest that the tendency of poor people to distribute unevenly across census tracts in an MSA is influenced only to a small extent by the explanatory variables that can be controlled, like the employment composition or the quality of the housing market.

	CP(.,0.2) (1)		CP(.,0.4) (2)		CP*(.,0.2) (3)		CP*(.,0.4) (4)		UP(.,0.2) (5)		UP(.,0.4) (6)		G(.) (7)	
A) % Black	-0.062	(0.12)	0.120	(0.08)	-0.004	(0.06)	0.007	(0.02)	-0.045	(0.05)	0.007	(0.02)	0.026	(0.04)
A) % Hispanic	0.038	(0.11)	-0.060	(0.09)	-0.061	(0.04)	-0.044**	(0.02)	-0.091**	(0.03)	-0.041**	(0.02)	0.001	(0.03)
A) % Asian	-0.057	(0.15)	0.030	(0.09)	-0.146**	(0.06)	0.015	(0.03)	-0.154**	(0.05)	0.014	(0.03)	-0.025	(0.04)
A) % White	-0.094	(0.14)	0.039	(0.09)	-0.068	(0.06)	-0.017	(0.03)	-0.076	(0.05)	-0.014	(0.02)	-0.035	(0.04)
A) Population size (ln)	-0.018	(0.04)	0.017	(0.03)	-0.001	(0.01)	-0.012	(0.01)	-0.003	(0.01)	-0.011	(0.01)	0.020**	(0.01)
A) % 65 plus yrs old	0.387**	(0.16)	-0.025	(0.17)	0.010	(0.10)	-0.144**	(0.05)	0.001	(0.09)	-0.132**	(0.05)	0.053	(0.04)
A) % 25-64 yrs old	0.399**	(0.16)	-0.269	(0.19)	-0.149	(0.09)	-0.203**	(0.04)	-0.129	(0.08)	-0.191**	(0.04)	0.034	(0.04)
A) % Foreign	-0.027	(0.11)	-0.105	(0.10)	-0.028	(0.06)	0.014	(0.03)	-0.039	(0.05)	0.010	(0.03)	-0.068**	(0.03)
A) Moved from outside of state	0.170**	(0.07)	0.021	(0.06)	0.035	(0.03)	-0.008	(0.02)	0.048	(0.03)	-0.005	(0.02)	0.014	(0.02)
B) % New Houses (10 less yrs old)	-0.090	(0.09)	0.089	(0.07)	0.039	(0.04)	0.004	(0.02)	0.020	(0.03)	0.002	(0.02)	-0.049**	(0.02)
B) % Old houses (20 plus yrs old)	0.039	(0.06)	-0.116**	(0.05)	-0.012	(0.03)	-0.027	(0.02)	-0.008	(0.02)	-0.026	(0.02)	-0.025**	(0.01)
B) % Owner occupied	-0.114	(0.22)	0.343	(0.25)	0.155*	(0.08)	0.002	(0.07)	0.100	(0.07)	-0.002	(0.06)	0.080*	(0.04)
B) % Vacant	-0.047	(0.08)	-0.035	(0.06)	-0.005	(0.02)	-0.005	(0.01)	-0.007	(0.02)	-0.005	(0.01)	-0.014	(0.02)
B) % Rented	0.016	(0.19)	0.307	(0.27)	0.201**	(0.08)	0.010	(0.07)	0.180**	(0.07)	0.011	(0.07)	0.050	(0.05)
B) Avg. value house (ln)	-0.001	(0.02)	-0.003	(0.01)	0.001	(0.01)	-0.001	(0.00)	-0.001	(0.00)	-0.001	(0.00)	0.007**	(0.00)
B) Median value house by CT (ln)	-0.015	(0.02)	0.023*	(0.01)	0.006	(0.01)	0.004	(0.00)	0.003	(0.01)	0.004	(0.00)	0.010**	(0.00)
B) p25% value house by CT (ln)	-0.008	(0.01)	-0.008	(0.02)	-0.001	(0.01)	0.001	(0.00)	-0.002	(0.00)	0.001	(0.00)	-0.007	(0.00)
B) S.d. value house by CT (ln)	-0.005	(0.01)	0.005	(0.01)	0.003	(0.00)	0.004	(0.00)	0.003	(0.00)	0.004	(0.00)	-0.005**	(0.00)
B) Avg. rent (ln)	0.022	(0.04)	-0.025	(0.03)	-0.002	(0.01)	0.009	(0.01)	0.001	(0.01)	0.008	(0.01)	-0.012	(0.01)
B) Median rent by CT (ln)	-0.018	(0.07)	0.147**	(0.05)	0.038	(0.03)	0.017	(0.01)	0.033	(0.02)	0.016	(0.01)	0.005	(0.03)
B) p25% rent by CT (ln)	-0.009	(0.03)	-0.075**	(0.03)	-0.025	(0.02)	-0.011	(0.01)	-0.024	(0.02)	-0.011	(0.01)	-0.008	(0.02)
B) S.d. rent by CT (ln)	0.004	(0.01)	-0.016*	(0.01)	-0.006	(0.00)	-0.004	(0.00)	-0.007	(0.00)	-0.004*	(0.00)	0.000	(0.00)
C) College Town	0.002	(0.01)	0.005	(0.01)	0.003	(0.00)	0.001	(0.00)	0.002	(0.00)	0.001	(0.00)	-0.003*	(0.00)
C) Student Town	0.003	(0.01)	-0.003	(0.01)	0.002	(0.01)	-0.002	(0.00)	0.002	(0.01)	-0.002	(0.00)	-0.007	(0.00)
C) % less than high school	-0.003*	(0.00)	-0.000	(0.00)	-0.001	(0.00)	0.001	(0.00)	-0.001	(0.00)	0.001	(0.00)	-0.000	(0.00)
C) % with high school	-0.359**	(0.18)	0.109	(0.18)	-0.076	(0.07)	0.076*	(0.04)	-0.084	(0.06)	0.065	(0.04)	-0.029	(0.03)
C) % with college	-0.002*	(0.00)	-0.000	(0.00)	-0.000	(0.00)	0.000*	(0.00)	-0.000	(0.00)	0.000*	(0.00)	-0.000*	(0.00)
C) % enrollment (any)	0.167	(0.12)	0.210**	(0.09)	0.060	(0.04)	-0.050*	(0.03)	0.030	(0.03)	-0.050*	(0.03)	0.036	(0.03)
D) % Managerial position	-0.255**	(0.10)	0.280**	(0.07)	0.097**	(0.04)	0.124**	(0.02)	0.078**	(0.04)	0.116**	(0.02)	0.034	(0.03)
D) % Timework	-0.002*	(0.00)	0.001*	(0.00)	-0.000	(0.00)	0.000*	(0.00)	-0.000	(0.00)	0.000*	(0.00)	0.000*	(0.00)
E) Avg hh income (ln)	0.007	(0.03)	0.004	(0.03)	-0.005	(0.01)	-0.011	(0.01)	-0.003	(0.01)	-0.010	(0.01)	0.012*	(0.01)
E) Median hh income by CT (ln)	-0.078	(0.07)	-0.017	(0.07)	-0.066**	(0.03)	-0.006	(0.02)	-0.087**	(0.02)	-0.009	(0.02)	-0.066**	(0.02)
E) p25% hh income by CT (ln)	0.066**	(0.03)	0.008	(0.03)	0.022	(0.02)	0.015	(0.01)	0.028**	(0.01)	0.016	(0.01)	0.045**	(0.01)
E) S.d. hh income by CT (ln)	0.026*	(0.02)	-0.011	(0.01)	0.007	(0.01)	-0.000	(0.00)	0.012**	(0.01)	0.000	(0.00)	0.025**	(0.00)
E) % of poor	3.268**	(0.20)	2.083**	(0.21)	1.464**	(0.07)	0.456**	(0.03)	1.067**	(0.06)	0.411**	(0.03)	-0.369**	(0.04)
E) Dissimilarity poor	1.296**	(0.05)	0.825**	(0.08)	0.654**	(0.02)	0.172**	(0.02)	0.625**	(0.02)	0.167**	(0.02)	1.014**	(0.01)
E) Dissimilarity whiteblack	0.102	(0.08)	-0.090	(0.09)	-0.015	(0.05)	-0.019	(0.03)	0.008	(0.05)	-0.017	(0.03)	-0.024	(0.03)
E) Dissimilarity black	-0.141*	(0.08)	0.162	(0.10)	0.044	(0.05)	0.040	(0.03)	0.007	(0.04)	0.036	(0.03)	0.027	(0.03)
E) Dissimilarity hispanic	0.061	(0.04)	-0.021	(0.03)	-0.001	(0.01)	-0.012	(0.01)	-0.001	(0.01)	-0.012	(0.01)	0.007	(0.01)
E) Dissimilarity asian	0.013	(0.03)	0.040	(0.03)	0.007	(0.01)	0.021**	(0.01)	0.012	(0.01)	0.021**	(0.01)	0.008	(0.01)
E) Dissimilarity white	-0.320**	(0.14)	0.110	(0.11)	0.042	(0.06)	-0.013	(0.04)	0.033	(0.05)	-0.010	(0.03)	-0.071*	(0.04)
Great Recession	0.020	(0.18)	0.110	(0.12)	0.006	(0.05)	-0.013	(0.03)	0.016	(0.05)	-0.013	(0.03)	-0.048	(0.04)
State Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Regional Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Year Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Observations	2,342		2,342		2,342		2,342		2,342		2,342		2,342	
MSA	395		395		395		395		395		395		395	
N. of years	6		6		6		6		6		6		6	
R-squared	0.898		0.770		0.928		0.711		0.917		0.709		0.969	
Root MSE	0.065		0.062		0.026		0.016		0.024		0.015		0.015	

Table 1.5: Drivers of concentrated poverty and urban poverty levels, pooled sample

Note: pooled OLS regression controlling for years fixed effects and Great Recession (2008-2012) fixed effects. All models control for state and regional fixed effects (Regions: Northeast, Midwest, South, West). Standard errors, clustered at the state level, are in parenthesis. Significance levels: * = 10% and ** = 5%.

	CP(.,0.2) (1)		CP(.,0.4) (2)		CP*(.,0.2) (3)		CP*(.,0.4) (4)		UP(.,0.2) (5)		UP(.,0.4) (6)		G(.) (7)	
A) % Black	0.021	(0.10)	0.095	(0.07)	-0.001	(0.06)	0.005	(0.02)	-0.040	(0.05)	0.005	(0.02)	0.029	(0.04)
A) % Hispanic	0.102	(0.08)	-0.091	(0.07)	-0.076*	(0.04)	-0.046**	(0.02)	-0.100**	(0.04)	-0.042**	(0.02)	-0.002	(0.04)
A) % Asian	0.047	(0.15)	-0.043	(0.10)	-0.126**	(0.06)	0.014	(0.04)	-0.131**	(0.05)	0.016	(0.04)	-0.027	(0.04)
A) % White	0.019	(0.12)	-0.033	(0.09)	-0.079	(0.06)	-0.023	(0.03)	-0.077	(0.05)	-0.018	(0.03)	-0.038	(0.04)
A) Population size (ln)	-0.011	(0.04)	0.007	(0.03)	-0.004	(0.01)	-0.016	(0.01)	-0.006	(0.01)	-0.015	(0.01)	0.020**	(0.01)
A) % 65 plus yrs old	0.321*	(0.17)	0.074	(0.17)	0.028	(0.09)	-0.139**	(0.05)	0.011	(0.09)	-0.129**	(0.05)	0.070*	(0.04)
A) % 25-64 yrs old	0.305*	(0.17)	-0.159	(0.18)	-0.167*	(0.10)	-0.187**	(0.04)	-0.147*	(0.08)	-0.176**	(0.04)	0.043	(0.05)
A) % Foreign	-0.029	(0.12)	-0.103	(0.11)	-0.000	(0.05)	0.017	(0.04)	-0.020	(0.05)	0.013	(0.03)	-0.078**	(0.03)
A) Moved from outside of state	0.164**	(0.07)	0.053	(0.06)	0.037	(0.03)	0.002	(0.02)	0.051*	(0.03)	0.004	(0.01)	0.019	(0.02)
B) % New Houses (10 less yrs old)	-0.135	(0.08)	0.079	(0.07)	0.013	(0.03)	0.001	(0.02)	-0.006	(0.03)	-0.001	(0.02)	-0.053**	(0.02)
B) % Old houses (20 plus yrs old)	0.025	(0.06)	-0.133**	(0.05)	-0.021	(0.03)	-0.030	(0.02)	-0.018	(0.02)	-0.029	(0.02)	-0.028**	(0.01)
B) % Owner occupied	-0.107	(0.22)	0.406	(0.25)	0.192**	(0.09)	0.010	(0.07)	0.136*	(0.07)	0.007	(0.06)	0.098**	(0.04)
B) % Vacant	-0.047	(0.08)	-0.031	(0.06)	0.000	(0.02)	-0.005	(0.01)	-0.004	(0.02)	-0.006	(0.01)	-0.015	(0.02)
B) % Rented	-0.013	(0.19)	0.385	(0.29)	0.217**	(0.09)	0.015	(0.08)	0.193**	(0.08)	0.015	(0.07)	0.057	(0.05)
B) Avg. value house (ln)	0.001	(0.02)	-0.005	(0.01)	0.001	(0.01)	-0.002	(0.00)	-0.001	(0.00)	-0.002	(0.00)	0.009**	(0.00)
B) Median value house by CT (ln)	-0.011	(0.02)	0.023	(0.01)	0.004	(0.01)	0.004	(0.00)	0.002	(0.01)	0.004	(0.00)	0.012**	(0.00)
B) p25% value house by CT (ln)	-0.009	(0.01)	-0.006	(0.02)	-0.000	(0.01)	0.002	(0.00)	-0.001	(0.01)	0.001	(0.00)	-0.007	(0.00)
B) S.d. value house by CT (ln)	-0.006	(0.01)	0.004	(0.01)	0.002	(0.00)	0.003	(0.00)	0.002	(0.00)	0.003	(0.00)	-0.006**	(0.00)
B) Avg. rent (ln)	0.017	(0.04)	-0.017	(0.03)	0.001	(0.01)	0.014	(0.01)	0.004	(0.01)	0.013	(0.01)	-0.012	(0.01)
B) Median rent by CT (ln)	0.014	(0.07)	0.100**	(0.05)	0.040	(0.03)	0.004	(0.01)	0.038	(0.02)	0.005	(0.01)	0.006	(0.03)
B) p25% rent by CT (ln)	-0.014	(0.03)	-0.063*	(0.04)	-0.024	(0.02)	-0.008	(0.01)	-0.025	(0.02)	-0.008	(0.01)	-0.009	(0.02)
B) S.d. rent by CT (ln)	0.001	(0.01)	-0.011	(0.01)	-0.007	(0.00)	-0.003	(0.00)	-0.008*	(0.00)	-0.003	(0.00)	0.000	(0.00)
C) College Town	0.001	(0.01)	0.007	(0.01)	0.002	(0.00)	0.001	(0.00)	0.001	(0.00)	0.001	(0.00)	-0.003*	(0.00)
C) Student Town	0.005	(0.01)	0.001	(0.01)	0.003	(0.01)	-0.001	(0.01)	0.003	(0.01)	-0.001	(0.00)	-0.005	(0.01)
C) % less than high school	-0.001	(0.00)	-0.001	(0.00)	-0.001	(0.00)	0.001	(0.00)	-0.001	(0.00)	0.000	(0.00)	-0.000	(0.00)
C) % with high school	-0.217	(0.17)	-0.062	(0.12)	-0.061	(0.07)	0.049	(0.04)	-0.063	(0.06)	0.041	(0.04)	-0.037	(0.03)
C) % with college	-0.001	(0.00)	-0.001	(0.00)	-0.000	(0.00)	0.000*	(0.00)	-0.000	(0.00)	0.000	(0.00)	-0.000*	(0.00)
C) % enrollment (any)	0.183	(0.12)	0.208**	(0.09)	0.059	(0.04)	-0.051*	(0.03)	0.031	(0.03)	-0.051*	(0.03)	0.041	(0.03)
D) % Managerial position	-0.244**	(0.10)	0.260**	(0.08)	0.114**	(0.04)	0.117**	(0.02)	0.091**	(0.04)	0.109**	(0.02)	0.032	(0.03)
D) % Timework	-0.002	(0.00)	0.001	(0.00)	0.000	(0.00)	0.000	(0.00)	-0.000	(0.00)	0.000	(0.00)	0.000*	(0.00)
E) Avg hh income (ln)	0.012	(0.04)	-0.009	(0.03)	-0.009	(0.01)	-0.016	(0.01)	-0.007	(0.01)	-0.015	(0.01)	0.009	(0.01)
E) Median hh income by CT (ln)	-0.088	(0.07)	-0.010	(0.07)	-0.072**	(0.03)	-0.004	(0.02)	-0.092**	(0.03)	-0.008	(0.02)	-0.068**	(0.02)
E) p25% hh income by CT (ln)	0.070**	(0.03)	-0.001	(0.04)	0.028*	(0.02)	0.015	(0.01)	0.033**	(0.01)	0.016	(0.01)	0.046**	(0.01)
E) S.d. hh income by CT (ln)	0.027	(0.02)	-0.013	(0.02)	0.008	(0.01)	-0.001	(0.00)	0.013**	(0.01)	-0.000	(0.00)	0.026**	(0.00)
E) % of poor	3.287**	(0.20)	1.995**	(0.20)	1.463**	(0.07)	0.451**	(0.04)	1.073**	(0.06)	0.406**	(0.03)	-0.363**	(0.04)
E) Dissimilarity poor	1.294**	(0.06)	0.847**	(0.08)	0.652**	(0.02)	0.178**	(0.02)	0.623**	(0.03)	0.172**	(0.02)	1.014**	(0.01)
E) Dissimilarity whiteblack	0.066	(0.08)	-0.099	(0.09)	-0.023	(0.06)	-0.015	(0.03)	-0.000	(0.05)	-0.013	(0.03)	-0.029	(0.03)
E) Dissimilarity black	-0.130	(0.08)	0.191*	(0.10)	0.055	(0.05)	0.039	(0.03)	0.016	(0.05)	0.034	(0.03)	0.030	(0.03)
E) Dissimilarity hispanic	0.060	(0.04)	-0.020	(0.03)	0.003	(0.01)	-0.011	(0.01)	0.002	(0.01)	-0.011	(0.01)	0.008	(0.01)
E) Dissimilarity asian	0.021	(0.03)	0.029	(0.02)	0.009	(0.01)	0.021**	(0.01)	0.015	(0.01)	0.021**	(0.01)	0.010	(0.01)
E) Dissimilarity white	-0.255*	(0.14)	0.067	(0.13)	0.042	(0.07)	-0.018	(0.04)	0.039	(0.06)	-0.014	(0.04)	-0.071*	(0.04)
State Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Regional Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Year Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Observations	2,202		2,202		2,202		2,202		2,202		2,202		2,202	
MSA	367		367		367		367		367		367		367	
N. of years	6		6		6		6		6		6		6	
R-squared	0.897		0.776		0.928		0.716		0.918		0.713		0.968	
Root MSE	0.065		0.061		0.026		0.016		0.024		0.015		0.015	

Table 1.6: Drivers of concentrated poverty and urban poverty levels, Fixed Effects estimator

Note: Fixed Effects estimators for the balanced panel of MSA (367). All models control for state and regional fixed effects (Regions: Northeast, Midwest, South, West). Standard errors, clustered at the state level, are in parenthesis. Significance levels: * = 10% and ** = 5%.

Table 1.6 reports estimates of the marginal effects of interest based on a Fixed Effects model. The incidence of poverty at the MSA level plays a major role in explaining urban poverty concentration, irrespective of the measure we use. Variables linked to income distribution mainly affect $UP(.,0.2)$ and G but seem uncorrelated with other measures of urban poverty concentration. Overall, the Fixed Effects estimates support the findings of the cross-sectional models.

1.4.3 Drivers of concentrated poverty and urban poverty: Changes

We examine the effects of the covariates considered in section 1.4.2 on the pooled period-to-period changes in concentrated and urban poverty measures. We expand the model by including controls for gentrification at the census tract level. Table 1.7 presents the effects of demographics, education, housing, labor market, and income distribution on the relevant urban poverty changes. Overall, demographics, education, and housing are the most relevant (and significant) drivers of changes in concentrated and urban poverty. An increment in the proportion of non-US citizens increases $CP^*(.,0.2)$ and $UP(.,0.2)$, while the effect is statistically insignificant for the other indicators. Meanwhile, the high mobility of citizens from different US states is associated with a lower change in urban poverty. These findings suggest an asymmetry in the residential choice of natives and immigrants that move into an MSA. Ethnic density and segregation play a pivotal role in the changes in the indices. The segregation of Blacks and Hispanics is positively associated with the variations in the indices. Conversely, this finding does not apply to the segregation of Asians, which is negatively related to changes in $CP(.,0.2)$, $CP^*(.,0.2)$ and $UP(.,0.2)$.

An increment in housing value tends to increase urban and concentrated poverty. Higher housing costs might force poor individuals to concentrate in some areas, creating distinct rich and poor census tracts. Similarly, the greater the average rent at the MSA level, the higher the concentrated and urban poverty indices, while an increase in median rent by CT implies a decrease in the indices. Finally, the population size, the incidence of poverty, and the fact that a significant share of the population has managerial positions reduce concentrated and urban poverty indices.

Andreoli et al. (2021) tested the null hypothesis of spatial independence in the distribution of poverty incidence across census tracts of American MSAs in 1980 and 2014, rejecting the null hypothesis for the large majority of MSAs with a population larger than 300,000 residents for both years. However, positive spatial autocorrelation in poverty incidence across tracts is not informative about its implications regarding the uneven distribution of poor people across the tracts. The Gini index decomposition into neighborhood and non-neighborhood components allows us to separate the contributions of neighboring and non-neighboring tracts to overall urban poverty.

The effects of the drivers of urban poverty on the levels and changes of the two spatial components of the Gini index are in Table 1.8, models (1)-(4). Few covariates have a significant and stable effect on the levels and changes of the neighborhood and non-neighborhood components of the Gini index. The segregation of the poor positively impacts the levels of the Gini index's spatial components. This means that a greater degree of segregation of the poor does not generally yield a greater tendency of tracts with similar levels of poverty incidence to cluster. The incidence of poor people is negatively associated with the neighborhood component, suggesting that high-poverty tracts have a greater tendency to cluster in MSAs with higher poverty incidence levels. Demographic variables,

	Δ CP(.,0.2)		Δ CP(.,0.4)		Δ CP*(.,0.2)		Δ CP*(.,0.4)		Δ UP(.,0.2)		Δ UP(.,0.4)		Δ G(.)	
	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
A) % Black	0.109	(0.11)	0.289**	(0.08)	0.108**	(0.04)	0.047**	(0.02)	0.093**	(0.04)	0.041**	(0.02)	0.016	(0.03)
A) % Hispanic	0.006	(0.09)	0.210**	(0.04)	0.062**	(0.03)	0.033**	(0.01)	0.055**	(0.02)	0.029**	(0.01)	0.018	(0.03)
A) % Asian	0.207**	(0.10)	0.389**	(0.18)	0.299**	(0.09)	0.050	(0.05)	0.231**	(0.07)	0.040	(0.04)	-0.054	(0.09)
A) % White	-0.077	(0.09)	0.185**	(0.06)	0.091*	(0.05)	0.038**	(0.02)	0.077*	(0.04)	0.033**	(0.01)	0.069	(0.05)
A) Population size (ln)	-0.203**	(0.04)	-0.101**	(0.03)	-0.064**	(0.02)	-0.022**	(0.01)	-0.054**	(0.01)	-0.021**	(0.01)	0.002	(0.01)
A) % 65 plus yrs old	-0.189	(0.17)	-0.200	(0.14)	0.052	(0.07)	-0.036	(0.03)	0.046	(0.06)	-0.036	(0.03)	0.156*	(0.08)
A) % 25-64 yrs old	-0.637**	(0.17)	-0.107	(0.24)	0.024	(0.09)	-0.057	(0.04)	0.022	(0.07)	-0.055*	(0.03)	0.036	(0.06)
A) % Foreign	0.136	(0.15)	0.025	(0.09)	0.136**	(0.03)	-0.016	(0.02)	0.117**	(0.03)	-0.014	(0.02)	-0.056	(0.05)
A) Moved from outside of state	-0.263**	(0.13)	-0.079	(0.08)	-0.126**	(0.04)	-0.011	(0.02)	-0.109**	(0.04)	-0.010	(0.02)	-0.070**	(0.03)
B) % New Houses (10 less yrs old)	0.306**	(0.10)	0.057	(0.12)	-0.035	(0.04)	0.003	(0.02)	-0.027	(0.03)	0.005	(0.02)	-0.038	(0.04)
B) % Old houses (20 plus yrs old)	0.080	(0.08)	0.051	(0.08)	-0.079**	(0.03)	0.006	(0.01)	-0.068**	(0.03)	0.007	(0.01)	-0.080**	(0.03)
B) % Owner occupied	-0.049	(0.21)	0.166	(0.23)	-0.026	(0.11)	0.060	(0.04)	-0.009	(0.09)	0.055	(0.04)	0.111	(0.11)
B) % Vacant	-0.037	(0.07)	-0.006	(0.04)	-0.007	(0.02)	-0.006	(0.01)	-0.010	(0.02)	-0.006	(0.01)	-0.008	(0.02)
B) % Rented	0.159	(0.24)	0.224	(0.22)	-0.004	(0.12)	0.077*	(0.04)	-0.002	(0.10)	0.071*	(0.04)	0.252**	(0.11)
B) Avg. value house (ln)	0.038*	(0.02)	0.010	(0.01)	0.006	(0.01)	0.000	(0.00)	0.005	(0.01)	0.000	(0.00)	0.000	(0.01)
B) Median value house by CT (ln)	0.066**	(0.02)	-0.005	(0.02)	0.015*	(0.01)	0.003	(0.00)	0.014*	(0.01)	0.003	(0.00)	0.002	(0.01)
B) p25% value house by CT (ln)	-0.010	(0.02)	-0.005	(0.02)	-0.008	(0.01)	-0.002	(0.00)	-0.007	(0.01)	-0.001	(0.00)	-0.012**	(0.01)
B) S.d. value house by CT (ln)	-0.032**	(0.01)	0.001	(0.01)	-0.008*	(0.00)	-0.000	(0.00)	-0.007*	(0.00)	-0.000	(0.00)	-0.005	(0.00)
B) Avg. rent (ln)	0.201**	(0.04)	0.096**	(0.03)	0.064**	(0.02)	0.021**	(0.01)	0.054**	(0.02)	0.020**	(0.01)	0.006	(0.01)
B) Median rent by CT (ln)	-0.390**	(0.08)	-0.080	(0.07)	-0.100**	(0.03)	-0.006	(0.02)	-0.079**	(0.03)	-0.007	(0.02)	0.008	(0.03)
B) p25% rent by CT (ln)	0.136**	(0.05)	-0.022	(0.05)	0.019	(0.02)	-0.016	(0.01)	0.016	(0.02)	-0.014	(0.01)	0.013	(0.02)
B) S.d. rent by CT (ln)	0.019	(0.02)	0.011	(0.01)	0.004	(0.00)	-0.000	(0.00)	0.004	(0.00)	-0.000	(0.00)	0.004	(0.01)
C) College Town	-0.003	(0.01)	-0.006	(0.01)	-0.003	(0.00)	-0.000	(0.00)	-0.003	(0.00)	-0.000	(0.00)	0.003	(0.00)
C) Student Town	-0.015	(0.01)	-0.006	(0.00)	-0.012**	(0.00)	0.000	(0.00)	-0.009**	(0.00)	0.000	(0.00)	-0.009*	(0.00)
C) % less than high school	0.003	(0.00)	0.001	(0.00)	-0.002**	(0.00)	-0.000	(0.00)	-0.002**	(0.00)	-0.000	(0.00)	-0.001	(0.00)
C) % with high school	0.028	(0.18)	-0.037	(0.14)	-0.087	(0.08)	-0.045	(0.04)	-0.066	(0.06)	-0.042	(0.03)	0.085	(0.06)
C) % with college	0.001	(0.00)	0.001*	(0.00)	-0.001**	(0.00)	-0.000	(0.00)	-0.001*	(0.00)	-0.000	(0.00)	0.000	(0.00)
C) % enrollment (any)	-0.038	(0.14)	-0.278*	(0.16)	-0.070	(0.07)	-0.077*	(0.04)	-0.035	(0.06)	-0.068*	(0.04)	0.195**	(0.04)
D) % Managerial position	-0.109	(0.10)	-0.170	(0.14)	-0.089*	(0.05)	-0.010	(0.02)	-0.074*	(0.04)	-0.008	(0.02)	0.056	(0.04)
D) % Timework	-0.001	(0.00)	-0.000	(0.00)	0.000	(0.00)	-0.000	(0.00)	0.000	(0.00)	-0.000	(0.00)	0.001**	(0.00)
E) Avg hh income (ln)	-0.241**	(0.04)	-0.064*	(0.03)	-0.058**	(0.02)	-0.013*	(0.01)	-0.049**	(0.02)	-0.013*	(0.01)	0.031**	(0.01)
E) Median hh income by CT (ln)	0.217**	(0.10)	0.024	(0.06)	0.032	(0.04)	0.001	(0.01)	0.023	(0.03)	0.001	(0.01)	0.037	(0.03)
E) p25% hh income by CT (ln)	-0.142**	(0.04)	-0.003	(0.03)	-0.014	(0.01)	0.003	(0.01)	-0.010	(0.01)	0.002	(0.01)	-0.040**	(0.01)
E) S.d. hh income by CT (ln)	-0.016	(0.02)	0.011	(0.01)	0.006	(0.01)	0.002	(0.00)	0.006	(0.01)	0.002	(0.00)	0.005	(0.01)
E) % of poor	-1.209**	(0.26)	-0.201	(0.16)	-0.146*	(0.07)	-0.039	(0.03)	-0.103*	(0.06)	-0.038	(0.03)	0.183**	(0.08)
E) Dissimilarity poor	-0.280**	(0.08)	-0.067	(0.05)	-0.034	(0.03)	-0.005	(0.01)	-0.039	(0.03)	-0.006	(0.01)	-0.395**	(0.03)
E) Dissimilarity whiteblack	-0.055	(0.10)	-0.183**	(0.07)	-0.050	(0.04)	-0.033**	(0.02)	-0.042	(0.03)	-0.029**	(0.01)	-0.015	(0.03)
E) Dissimilarity black	0.101	(0.11)	0.183**	(0.07)	0.055	(0.04)	0.032*	(0.02)	0.045	(0.03)	0.028*	(0.01)	0.003	(0.02)
E) Dissimilarity hispanic	0.082**	(0.04)	0.030	(0.02)	0.031**	(0.01)	-0.003	(0.01)	0.029**	(0.01)	-0.002	(0.01)	0.020	(0.02)
E) Dissimilarity asian	-0.097**	(0.05)	-0.067	(0.04)	-0.043**	(0.02)	-0.013	(0.01)	-0.031**	(0.01)	-0.010	(0.01)	0.017	(0.01)
E) Dissimilarity white	0.049	(0.13)	0.043	(0.10)	0.043	(0.05)	0.031	(0.03)	0.027	(0.05)	0.026	(0.02)	0.120*	(0.07)
F) % gentrified CT (BSM def.)	0.041	(0.08)	0.004	(0.04)	-0.024	(0.03)	0.007	(0.01)	-0.025	(0.02)	0.006	(0.01)	-0.061**	(0.02)
Great Recession	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
State Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Regional Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Year Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Observations	1,936		1,936		1,936		1,936		1,936		1,936		1,936	
MSA	395		395		395		395		395		395		395	
N. of years	5		5		5		5		5		5		5	
R-squared	0.376		0.254		0.547		0.255		0.545		0.258		0.413	
Root MSE	0.096		0.073		0.036		0.016		0.031		0.015		0.031	

Table 1.7: Drivers of changes in concentrated poverty and urban poverty

Note: Dependent variables are measured in terms of period-to-period variations (1980-1990, 1990-2000, 2000-2008, 2008-2012, 2012-2014). Pooled OLS regression controlling for years fixed effects and Great Recession (2008-2012) fixed effects. All models control for state and regional fixed effects (Regions: Northeast, Midwest, South, West). Standard errors, clustered at the state level, are in parenthesis. Significance levels: * = 10% and ** = 5%.

like population composition by origin group and total population size, are positively correlated with the change in the non-neighborhood component, suggesting that a more heterogeneous population composition tends to increase disparities in poverty incidence between non-neighboring tracts within larger MSAs. Other controls, including housing and education, never yield significant effects.

Columns (5) and (6) of Table 1.8 investigate the re-ranking and convergence components of the changes in urban poverty based on a pooled regression with year fixed effects. When considering the re-ranking component R (see column (5)), the pooled regression model shows that the incidence and segregation of poverty have a negative impact on R, suggesting that a re-ranking of tracts is less likely to occur in the MSAs with higher levels of incidence and segregation of poverty. Besides, we find that the features of the housing stock correlate with the re-ranking component. While a larger proportion of old dwellings is associated with a lower re-ranking component, the proportions of owner-occupied and vacant houses positively impact that component. Finally, demographic variables do not play a significant role in re-ranking tracts.

Column (6) of Table 1.8 shows the drivers of component D, measuring convergence in tract poverty incidence. Our estimates reveal that the explanatory variables are less informative about the convergence component than the re-ranking one. The pooled regression model shows that the incidence and segregation of poverty, respectively, affect positively and negatively the convergence component. Demographic, housing, education, and labor market variables (except the share of vacant dwellings and commuting time) do not affect the convergence component. Only the average income level and the income dispersion across tracts positively correlate with the convergence component.

1.4.4 Decomposition results

The previous sections illustrate the contributions of different drivers of poverty concentration on the trends in concentrated poverty and urban poverty indices. Yet, whether these effects primarily stem from variations in these drivers across MSAs or from evolving associations between the drivers and poverty concentration over time remains uncertain. To resolve this, we deploy the Oaxaca-Blinder (henceforth, O-B) threefold decomposition technique (Blinder, 1973; Oaxaca, 1973).

The O-B decomposition breaks down the difference between the means of an outcome variable Y calculated for two different groups, denoted as A and B, into three components. To obtain such components, consider the linear model $Y = \mathbf{X}'\boldsymbol{\beta} + \epsilon$ where $\mathbf{X}$ is a set of predictors observed for each group. The O-B decomposition divides the difference in the expected value of Y between the two groups into the sum of three terms (Jann, 2008), so that $E[Y_A] - E[Y_B] = \Delta_E + \Delta_C + \Delta_I$. Component $\Delta_E = \{E[\mathbf{X}_A] - E[\mathbf{X}_B]\}' \boldsymbol{\beta}_B$ measures the contribution of group differences in the means of predictors (the so-called endowments), corresponding to the *endowments effect*. Component $\Delta_C = E[\mathbf{X}_B]' (\boldsymbol{\beta}_A - \boldsymbol{\beta}_B)$ measures the contribution of differences in the coefficients; i.e., the *coefficients effect*. Component $\Delta_I = \{E[\mathbf{X}_A] - E[\mathbf{X}_B]\}' (\boldsymbol{\beta}_A - \boldsymbol{\beta}_B)$ is the *interaction effect* of differences in endowments and coefficients between the two groups. As in the study by Iceland and Hernandez (2017), the two groups compared are the selected 395 MSAs with their endowments in the starting year of the considered period (1980) and the same MSAs with their endowments in the ending year (2014). We use the available measures of

	G(.) (nbh) (1)		G(.) (no-nbh) (2)		Δ G(.) (nbh) (3)		Δ G(.) (no-nbh) (4)		R (5)		D (6)	
A) % Black	-0.026	(0.10)	0.052	(0.09)	-0.024	(0.03)	0.041**	(0.01)	0.010	(0.03)	-0.018	(0.04)
A) % Hispanic	0.069	(0.08)	-0.067	(0.09)	-0.019	(0.03)	0.037**	(0.01)	-0.005	(0.02)	-0.015	(0.03)
A) % Asian	-0.033	(0.14)	0.008	(0.13)	-0.037	(0.04)	-0.018	(0.05)	0.005	(0.04)	-0.073	(0.07)
A) % White	-0.053	(0.11)	0.018	(0.11)	0.016	(0.04)	0.053**	(0.02)	-0.012	(0.03)	0.065	(0.05)
A) Population size (ln)	-0.042	(0.03)	0.062**	(0.03)	-0.022**	(0.01)	0.024**	(0.01)	0.014	(0.01)	-0.008	(0.02)
A) % 65 plus yrs old	0.100	(0.13)	-0.047	(0.12)	-0.000	(0.05)	0.156**	(0.07)	0.052	(0.05)	0.080	(0.08)
A) % 25-64 yrs old	0.250*	(0.13)	-0.216*	(0.12)	-0.023	(0.06)	0.058	(0.05)	-0.005	(0.05)	0.025	(0.07)
A) % Foreign	0.016	(0.13)	-0.084	(0.13)	-0.031	(0.03)	-0.025	(0.03)	-0.003	(0.03)	-0.016	(0.06)
A) Moved from outside of state	-0.128*	(0.07)	0.143**	(0.06)	-0.027	(0.03)	-0.043**	(0.02)	0.015	(0.02)	-0.058*	(0.03)
B) % New Houses (10 less yrs old)	-0.054	(0.07)	0.006	(0.06)	-0.023	(0.02)	-0.015	(0.03)	-0.023	(0.02)	-0.015	(0.05)
B) % Old houses (20 plus yrs old)	-0.072	(0.05)	0.047	(0.05)	-0.043**	(0.02)	-0.037*	(0.02)	-0.037**	(0.02)	-0.036	(0.03)
B) % Owner occupied	0.271	(0.20)	-0.190	(0.20)	0.001	(0.06)	0.111	(0.07)	0.128*	(0.07)	-0.056	(0.13)
B) % Vacant	-0.083	(0.06)	0.069	(0.06)	-0.002	(0.02)	-0.005	(0.02)	0.050**	(0.02)	-0.064**	(0.03)
B) % Rented	0.227	(0.17)	-0.176	(0.17)	0.024	(0.07)	0.228**	(0.07)	0.108	(0.08)	0.091	(0.12)
B) Avg. value house (ln)	0.004	(0.01)	0.003	(0.01)	0.001	(0.00)	-0.001	(0.00)	-0.001	(0.00)	-0.002	(0.01)
B) Median value house by CT (ln)	-0.012	(0.01)	0.022*	(0.01)	0.010*	(0.01)	-0.008	(0.01)	0.006	(0.00)	-0.006	(0.01)
B) p25% value house by CT (ln)	0.011	(0.01)	-0.018	(0.01)	-0.014**	(0.00)	0.001	(0.01)	-0.000	(0.00)	-0.010	(0.01)
B) S.d. value house by CT (ln)	-0.001	(0.01)	-0.004	(0.01)	-0.003	(0.00)	-0.002	(0.00)	-0.000	(0.00)	-0.005	(0.00)
B) Avg. rent (ln)	0.012	(0.03)	-0.024	(0.03)	0.026**	(0.01)	-0.020**	(0.01)	-0.013	(0.01)	0.013	(0.01)
B) Median rent by CT (ln)	0.081	(0.07)	-0.076	(0.06)	-0.017	(0.02)	0.025*	(0.01)	0.015	(0.02)	0.004	(0.03)
B) p25% rent by CT (ln)	-0.082	(0.07)	0.074	(0.06)	0.011	(0.02)	0.003	(0.01)	-0.002	(0.01)	0.016	(0.02)
B) S.d. rent by CT (ln)	0.011	(0.01)	-0.010	(0.01)	0.003	(0.00)	0.001	(0.00)	0.004	(0.00)	-0.003	(0.00)
C) College Town	-0.005	(0.01)	0.002	(0.01)	0.002	(0.00)	0.001	(0.00)	-0.000	(0.00)	0.003	(0.00)
C) Student Town	0.007	(0.02)	-0.014	(0.02)	-0.002	(0.00)	-0.006**	(0.00)	-0.003	(0.00)	-0.006	(0.00)
C) % less than high school	-0.001	(0.00)	0.001	(0.00)	-0.000	(0.00)	-0.000	(0.00)	0.000	(0.00)	-0.000	(0.00)
C) % with high school	-0.075	(0.13)	0.045	(0.12)	0.110**	(0.04)	-0.024	(0.06)	0.017	(0.04)	0.087	(0.06)
C) % with college	-0.001	(0.00)	0.000	(0.00)	0.000*	(0.00)	-0.000	(0.00)	0.000	(0.00)	0.000	(0.00)
C) % enrollment (any)	0.126*	(0.07)	-0.090	(0.06)	0.098**	(0.03)	0.097**	(0.03)	0.114**	(0.03)	0.063	(0.05)
D) % Managerial position	-0.082	(0.12)	0.117	(0.11)	0.023	(0.02)	0.033	(0.03)	-0.005	(0.04)	0.054	(0.05)
D) % Timework	0.005**	(0.00)	-0.004**	(0.00)	0.001**	(0.00)	0.001**	(0.00)	0.000	(0.00)	0.001**	(0.00)
E) Avg hh income (ln)	-0.027	(0.03)	0.040	(0.03)	0.003	(0.01)	0.028**	(0.01)	0.006	(0.01)	0.034**	(0.02)
E) Median hh income by CT (ln)	-0.159**	(0.05)	0.093*	(0.05)	0.026	(0.02)	0.011	(0.02)	0.032*	(0.02)	-0.018	(0.03)
E) p25% hh income by CT (ln)	0.030	(0.03)	0.015	(0.02)	-0.019**	(0.01)	-0.021**	(0.01)	-0.001	(0.01)	-0.028	(0.02)
E) S.d. hh income by CT (ln)	0.052**	(0.01)	-0.026**	(0.01)	0.008*	(0.00)	-0.003	(0.00)	-0.014**	(0.00)	0.021**	(0.01)
E) % of poor	-0.250*	(0.13)	-0.118	(0.12)	0.158**	(0.06)	0.025	(0.04)	-0.115**	(0.04)	0.275**	(0.08)
E) Dissimilarity poor	0.574**	(0.05)	0.440**	(0.05)	-0.236**	(0.02)	-0.159**	(0.02)	-0.113**	(0.01)	-0.263**	(0.03)
E) Dissimilarity whiteblack	0.124	(0.07)	-0.148**	(0.07)	-0.027	(0.02)	0.012	(0.02)	0.003	(0.03)	-0.027	(0.03)
E) Dissimilarity black	-0.120	(0.08)	0.147*	(0.08)	0.008	(0.02)	-0.005	(0.01)	0.015	(0.03)	-0.003	(0.03)
E) Dissimilarity hispanic	-0.026	(0.03)	0.034	(0.03)	0.011	(0.01)	0.009	(0.01)	-0.002	(0.01)	0.015	(0.02)
E) Dissimilarity asian	0.017	(0.03)	-0.009	(0.02)	0.013	(0.01)	0.004	(0.01)	0.002	(0.01)	0.013	(0.01)
E) Dissimilarity white	-0.109	(0.13)	0.038	(0.12)	0.098*	(0.05)	0.022	(0.03)	-0.001	(0.04)	0.141**	(0.06)
Great Recession	0.024	(0.13)	-0.072	(0.13)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
F) % gentrified CT (BSM def.)					-0.026	(0.02)	-0.035**	(0.01)	-0.025	(0.02)	-0.034	(0.03)
State Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes	
Regional Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes	
Year Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes	
Observations	2,342		2,342		1,936		1,936		1,936		1,936	
MSA	395		395		394		394		394		394	
N. of years interval	6		6		5		5		5		5	
R-squared	0.624		0.725		0.329		0.277		0.511		0.361	
Root MSE	0.055		0.053		0.022		0.020		0.019		0.034	

Table 1.8: Drivers of levels and changes in G: Different components

Note: Dependent variables are measured in terms of realizations of urban poverty as measured by G index, both in pooled levels and in pooled period-to-period variations (1980-1990, 1990-2000, 2000-2008, 2008-2012, 2012-2014). Pooled OLS regression controlling for years fixed effects and Great Recession (2008-2012) fixed effects. All models control for state and regional fixed effects (Regions: Northeast, Midwest, South, West). Standard errors, clustered at the state level, are in parenthesis. Significance levels: $^* = 10\%$ and $^{**} = 5\%$.

urban poverty as outcomes. The O-B decomposition is obtained from the perspective of MSAs in 2014, which then plays the role of group B in the above-described formulation of the O-B decomposition. The results of the decomposition are shown in Table 1.9, where also the contribution of each group of explanatory variables (e.g., demographics) to each of the three components (endowments, coefficients, and interaction) is shown.

The endowments effect is the contribution of the differences in the explanatory variables between 1980 and 2014. It expresses the expected difference in the predicted 2014 mean outcome (the urban poverty level) if the distribution of endowments across MSAs were that of 1980. The second component, the coefficients effect, measures the contribution of differences in period-to-period regression coefficients. This component indicates that the explanatory variables also differ in their influence across periods. The third component, the interaction effect, considers that the differences in terms of endowments and coefficients co-occur between 1980 and 2014. Ultimately, this decomposition method allows us to single out the extent to which the MSAs characteristics explain the differences between 1980 and 2014 (the endowment component), while holding as fixed the coefficients component and their interaction with endowments.

We maintained the driver categories from earlier tables but grouped them to easily read the decomposition results. Diverging from prior regression structures, regional and yearly fixed effects are excluded to prevent zero-variance instances, but state fixed effects are retained.[13]

In column (1) of Table 1.9, concentrated poverty at a 20% poverty line is estimated at $CP = 0.317$ in 1980 and $CP = 0.503$ in 2014. These estimates result in a significant difference of -0.186, which is additively split into the contributions of the three components described above. While the contributions of the endowments and interaction terms are positive, the difference due to the coefficients is negative and large, thus driving the sign of the overall difference observed between the two periods. The contribution of endowments, equal to 0.321, suggests that the overall difference could have been even more considerable if the MSAs had been similarly endowed between the two periods.

Each component is further broken down by the contribution of each group of explanatory variables in the following rows of Table 1.9. The detailed decomposition makes it possible to study the contribution of each group of variables to the decomposition. First, it can be observed that the difference due to endowments originates mainly from housing-related variables. However, this difference is not significant. Moreover, each group's sign, magnitude, and significance vary considerably in terms of coefficient and interaction components. The fixed effects (not shown) almost cancel out this effect with an opposite sign. Overall, the coefficients effect is negative, implying that the influence of the explanatory variables on concentrated poverty changed between 1980 and 2014, contributing to increase CP.

The effects estimated in the remaining columns have a similar interpretation. As in column (1), the indicators are systematically higher in 2014 than in 1980, resulting in significant negative differences. Endowments account for most of the difference in $CP(., 0.4)$, $CP^*(., 0.2)$, $UP(., 0.2)$, and $G(.)$. Conversely, the coefficients are the main contributors to the changes in $CP(., 0.2)$, $CP^*(., 0.4)$, and $UP(., 0.4)$. In all cases except $G(.)$, the elements explaining most of the difference are significant. The contributions of the different components are not uniform

[13]We abstain from reporting fixed effects; hence, variables in Table 1.9 do not entirely elucidate the overall coefficient-driven difference.

	CP(.,0.2) (1)		CP(.,0.4) (2)		CP*(.,0.2) (3)		CP*(.,0.4) (4)		UP(.,0.2) (5)		UP(.,0.4) (6)		G(.) (7)	
Prediction 1980	0.317**	(0.01)	0.062**	(0.01)	0.100**	(0.00)	0.010**	(0.00)	0.092**	(0.00)	0.009**	(0.00)	0.326**	(0.00)
Prediction 2014	0.503**	(0.01)	0.136**	(0.01)	0.180**	(0.00)	0.026**	(0.00)	0.163**	(0.00)	0.025**	(0.00)	0.351**	(0.00)
Difference	-0.186**	(0.01)	-0.074**	(0.01)	-0.080**	(0.01)	-0.016**	(0.00)	-0.070**	(0.01)	-0.015**	(0.00)	-0.025**	(0.01)
Endowments	0.321	(0.31)	-0.877**	(0.31)	-0.225**	(0.09)	-0.174**	(0.08)	-0.192**	(0.09)	-0.160**	(0.07)	-0.047	(0.07)
Demographics	-0.103	(0.06)	0.071	(0.06)	0.023	(0.02)	0.018	(0.02)	0.023	(0.02)	0.019	(0.02)	-0.014	(0.01)
Housing	0.308	(0.32)	-0.514	(0.32)	-0.144	(0.09)	-0.108	(0.08)	-0.125	(0.09)	-0.098	(0.08)	-0.031	(0.07)
Education	0.043	(0.11)	-0.096	(0.11)	-0.006	(0.03)	-0.030	(0.03)	-0.016	(0.03)	-0.031	(0.03)	0.024	(0.02)
Employment	0.020	(0.03)	0.011	(0.03)	-0.007	(0.01)	-0.009	(0.01)	-0.004	(0.01)	-0.009	(0.01)	-0.006	(0.01)
Distribution	0.053	(0.07)	-0.350**	(0.07)	-0.093**	(0.02)	-0.045**	(0.02)	-0.069**	(0.02)	-0.041**	(0.02)	-0.020	(0.02)
Coefficients	-1.627**	(0.48)	0.239	(0.42)	0.002	(0.16)	0.196**	(0.08)	0.061	(0.15)	0.193**	(0.08)	-0.045	(0.13)
Demographics	-0.400	(1.38)	1.273	(1.33)	0.610	(0.43)	0.363	(0.31)	0.516	(0.41)	0.333	(0.30)	-0.658**	(0.32)
Housing	-2.064	(1.71)	-1.326	(1.63)	-0.671	(0.54)	-0.381	(0.38)	-0.408	(0.51)	-0.320	(0.36)	-0.265	(0.41)
Education	-0.313	(0.53)	0.296	(0.50)	0.050	(0.17)	0.205*	(0.12)	0.008	(0.16)	0.181	(0.11)	0.045	(0.13)
Employment	0.289*	(0.16)	-0.059	(0.14)	-0.092*	(0.05)	-0.090**	(0.03)	-0.102**	(0.05)	-0.087**	(0.03)	-0.005	(0.04)
Distribution	4.768**	(1.66)	-2.696*	(1.57)	0.264	(0.53)	-0.300	(0.36)	0.000	(0.50)	-0.294	(0.34)	0.137	(0.40)
Interaction	1.120**	(0.57)	0.564	(0.53)	0.144	(0.19)	-0.038	(0.11)	0.060	(0.17)	-0.048	(0.11)	0.066	(0.14)
Demographics	0.098	(0.08)	-0.040	(0.08)	-0.040*	(0.02)	-0.018	(0.02)	-0.041*	(0.02)	-0.019	(0.02)	0.003	(0.02)
Housing	1.405**	(0.60)	0.396	(0.56)	0.245	(0.20)	-0.058	(0.12)	0.150	(0.18)	-0.068	(0.11)	0.135	(0.15)
Education	0.107	(0.17)	-0.107	(0.16)	-0.104*	(0.05)	-0.024	(0.04)	-0.086*	(0.05)	-0.020	(0.03)	-0.059	(0.04)
Employment	-0.091*	(0.05)	0.056	(0.05)	0.037**	(0.02)	0.027**	(0.01)	0.032**	(0.01)	0.025**	(0.01)	0.009	(0.01)
Distribution	-0.398**	(0.09)	0.261**	(0.08)	0.006	(0.03)	0.035*	(0.02)	0.004	(0.03)	0.033*	(0.02)	-0.021	(0.02)
Observations	790		790		790		790		790		790		790	
MSA	395		395		395		395		395		395		395	

Table 1.9: Oaxaca-Blinder decomposition

Note: Oaxaca-Blinder decomposition of the levels in 1980 and 2014 in concentrated poverty and urban poverty. OLS regression controlling for state fixed effects. Standard errors, clustered at the state level, are in parenthesis. Significance levels: * = 10%, ** = 5%, and *** = 1%.

across the various models considered. Endowments' contribution is negative in all columns except for $CP(., 0.2)$. This result indicates that endowments in 2014 are distributed differently from 1980 and amplify the two periods' overall difference. Conversely, the coefficients have a positive counterbalancing effect but are sparsely significant.

If we further decompose each component, only a few groups of variables are significant. When looking at the endowments, several variables contribute negatively except demographics, owing to the fact that the group of demographics includes several variables that do not necessarily act in the same direction, creating a sizeable intra-group variability. Moreover, the distribution variables are the sole endowments consistently significant and negative across most concentrated and urban poverty measures except $CP(., 0.2)$, which plays a major role in endowments' contribution.

Conversely, several variables are sparsely significant among the sub-components of the coefficients. The impact of housing on the coefficients is negative but not significant. Employment contributes positively to the difference due to the coefficients for $CP(., 0.2)$ but negatively to the difference due to the other indices' coefficients, while being significant in both cases. A large part of the variation due to the coefficients is indeed attributed to fixed effects. This finding indicates a sizeable unexplained gap due to differences at the state level between 1980 and 2014.

Overall, Table 1.9 shows that the effects of the relevant groups of covariates on urban poverty changes are relatively similar across the urban poverty indices belonging to the family of urban poverty measures. The only exception is the first column, which uses $CP(., 0.2)$ as a dependent variable. We explain these discrepancies in results between $CP(., 0.2)$, often considered the golden rule in the study of urban poverty, and the other indices by the fact that CP disregards the information about the incidence and distribution of poverty across low-poverty neighborhoods.

Conversely, $CP(., 0.4)$ does not show different patterns than UP and CP^*, unlike $CP(., 0.2)$. This pattern is caused by the tolerance level increase from 20% to 40%, therefore addressing a particular subset of census tracts where poverty is highly concentrated. While the group of people included in $CP(., 0.2)$ is relatively large, $CP(., 0.4)$ involves a tiny group of people in extreme poverty. This small group is likely to have specific characteristics that differ significantly from the rest of the population, resulting in endowments substantially determining incidence patterns. Conversely, $CP(., 0.2)$ is likely to include a larger group of people with less specific characteristics, sharing several similarities with non-poor people. Therefore, examining what the rest of the population experiences is relevant to understand urban poverty.

Finally, it is also interesting to compare the results obtained for $CP(., 0.4)$ in column (2) with the results obtained by Iceland and Hernandez (2017). The whole difference is negative over the period, with a strong negative effect of endowments, a positive contribution of coefficients, and a positive interaction term. Our estimated effects for CP largely coincide with those estimated by Iceland and Hernandez (2017), thus validating their analysis.

1.5 Conclusion

The contribution of this article is twofold. First, we compare the effects of the main drivers of urban poverty concentration using alternative indices. Specifically, we contrast the widely-used concentrated poverty index,

CP, with indices from the family of urban poverty measures that Andreoli et al. (2021) axiomatically derived to address limitations of CP. Using pooled regressions, we analyze the influence of various factors on concentrated and urban poverty levels from 1980 to 2014. The regression results highlight that demographic factors, income distribution, and housing variables play a role in urban poverty concentration. However, their impact varies depending on the index and poverty incidence threshold. Notably, the CP index, calculated with a 20% poverty incidence threshold, exhibits distinct behavior compared to other indices, including CP at a 40% threshold. The proportion of poor residents and the segregation of the poor exhibit strong positive correlations with all measures of urban poverty concentration, suggesting these variables' consistent influence across different indices.

Second, we delve into regression results for period-to-period changes in concentrated and urban poverty measures. We identify demographics, education, and housing as primary factors driving these changes. However, these variables are less predictive of the changes than for the actual levels of concentrated and urban poverty. Employing the Oaxaca-Blinder threefold decomposition offers further insights. This method confirms that the chosen index and poverty incidence threshold play a role in explaining urban poverty concentration variations. Although both CP and other indices indicate a rise in poverty concentration from 1980 to 2014, the impacts of endowments, coefficients, and interaction components differ based on the index and threshold. For a 20% threshold, endowments significantly contribute to CP^* and UP, but not CP. This distinction might arise because CP overlooks non-poor populations, while CP^* and UP incorporate broader demographic data. As the threshold rises to 40%, CP aligns more closely with other urban poverty measures. This alignment underscores the influence of the threshold in measuring poverty concentration. The unique behavior of CP at 40% might stem from its focus on a smaller, extremely impoverished group, which inherently has distinct attributes compared to the broader population. These attributes may notably impact urban poverty trends.

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Urban poverty and the onset of the Coronavirus pandemic: Evidence from American cities

Chapter 2

Urban poverty and the onset of the Coronavirus pandemic: Evidence from American cities

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2.1 Introduction

The worldwide outbreak of the Coronavirus pandemic has produced dramatic humanitarian and economic consequences, the effect being stronger in urban areas than elsewhere. An abundant literature has investigated the potential drivers of the pandemic onset. Desmet and Wacziarg (2021) highlight demographics, transportation, and nursing home residents as important drivers. Using zip-code data, Benitez et al. (2020) find a positive correlation between the share of black and Hispanic residents and the number of confirmed cases. The geographic distribution of individuals with heterogeneous characteristics in other dimensions, such as income or poverty, can also drive the pandemic insurgence. Such an aspect has not yet been investigated.

In this paper, we empirically investigate the extent to which the early onset of the Coronavirus pandemic in American cities is driven by the degree of incidence and distribution of poverty across a city's neighborhoods, which we measure by mean of *urban poverty* indices. There are different channels through which urban poverty affects the incidence of new COVID-19 cases in American counties. On the one hand, income determines residential choices and correlates with the degree of the concentration of poverty in certain city areas. On the other hand, income correlates with the intensity and quality of interactions. While wealthy and middle-income residents have the means to limit social contacts under various margins, individuals in poverty are more vulnerable to the pandemic and less capable of insuring against its economic and public health consequences. Poor urban residents are more likely to commute more hours, use public transport, be employed in low-skill jobs, spend more time away from home and live in smaller, overcrowded houses (Eichenbaum et al., 2021; Jay et al., 2020; Lou et al., 2020; Ruiz-Euler et al., 2020). In addition, poor families live in smaller homes shared with more co-residents than the average family, contributing to the spreading of the Coronavirus within families.

We develop an econometric strategy to identify and estimate the effect of an exogenous change in urban poverty on the spread of the Coronavirus pandemic. The main dependent variables measure the daily incidence of cases testing positive for COVID-19 in American urban counties since the onset of the pandemic, corresponding to the period February 2020 to April 29, 2020, i.e., three weeks after implementing all stay-at-home orders across the US. We select this time window to analyze the evolution of the Coronavirus pandemic before, during, and immediately after the introduction of travel bans and mobility restrictions. Focusing on the incept of the pandemic's first wave has clear identification advantages. First, due to the unexpected nature of the first pandemic wave, we observe no significant effects on the mobility choices of urban residents before stay-at-home orders are put in place. Our estimates are hence largely unaffected by behavioral responses (e.g., reduced participation in public and social events) that might have consequences for spreading the pandemic. Data shows that mobility drops only after the introduction of stay-at-home orders. Second, by focusing on the early incidence period, it is reasonable to use available data on the distribution of poverty in cities and the county-specific covariates recorded in the pre-pandemic period to describe the drivers of the pandemic, assuming that the urban social composition observed in February-April 2020 is not dissimilar to what observed in the pre-pandemic period. After the first wave, jobs and workers' relocation has produced substantial effects on the spatial distribution of individuals in a way that affects the incidence and evolution of the pandemic that cannot be measured empirically. Third, once mobility restrictions have been put in place, their effect becomes indistinguishable from the impact of other drivers of the pandemic.

Our treatment variable corresponds to urban poverty measures axiomatically characterized in Andreoli et al. (2021). A measure of urban poverty is an index assigning a number, understood as the level of urban poverty displayed by a city, to the distribution of poor and non-poor residents in the neighborhoods of that city. Our preferred measure of urban poverty, the *UP* index, captures three aspects of the spatial distribution of poverty: the incidence, the distribution of poverty across neighborhoods where poverty is more concentrated , and the extent of segregation of poor and non-poor individuals across high- and low-poverty neighborhoods. The drivers of urban poverty indices, measured across American cities, are described in Andreoli et al. (2022).

For identification, we follow an instrumental variable approach to deal with bias introduced by measurement error in the number of cases testing positive for the Coronavirus as well as with endogeneity due to unobserved confounders of poor's residential location that are correlated with the pandemic outbreak across American counties. Our estimates suggest that increasing urban poverty produces a rise in both the virus incidence and the speed of diffusion, which fades out when opportunities for mobility drastically reduce in response to lockdown enforcement. Our findings provide evidence of a new health gradient attributable to the incidence of poverty in the neighborhood of residence.[14]

[14]Relevant references on the negative effects of poverty concentration at neighborhood level are: Ludwig et al.

We further analyze the interaction between urban poverty and lockdown policies in a dynamic fixed-effect model framework. In the absence of a vaccine, quarantine enforcement and social distancing policies have been regarded as unavoidable policy measures to mitigate the spread of the virus. Evidence about past pandemic events supports this view. For instance, Bootsma and Ferguson (2007) found that strict and timely quarantine enforcement was the most effective short-term measure to prevent the spread of the 1918 influenza pandemic across American cities. About half of the American counties have introduced stay-at-home orders starting March 25, 2020. There is mixed evidence on the effects of such measures. Some results suggest that the introduction of such measures has contributed to mitigating the virus spreading by liming movement and social contact (Anderson et al., 2020; Chernozhukov et al., 2021; Courtemanche et al., 2020; Hsiang et al., 2020; Pei et al., 2020) and improving health outcomes (such as hospitalization of intensive care units usage) during the pandemic (Dave et al., 2020; Dave et al., 2021; Fowler et al., 2020; Sears et al., 2020). Berry et al. (2021) and Agrawal et al. (2021) suggest, however, that the effects of lockdown policies on mobility have been short living, more so among the poor population. The effectiveness of mobility restrictions hence depends on the socio-economic context of the places where these restrictions are imposed. Recent evidence has shown that low-income individuals face greater constraints in complying with stay-at-home orders (Coven & Gupta, 2020; Garnier et al., 2021; Jay et al., 2020; Miller et al., 2020). Moreover, a high incidence of poverty tends to reduce compliance with stay-at-home orders for both U.S. cities (Jung et al., 2021; Lou et al., 2020; Wright et al., 2020) and in developing countries (Bargain & Aminjonov, 2020).

We analyze the relationship between COVID-19 cases, lockdown policies, and urban poverty across American cities. In our regression analysis, we control for county-specific time-invariant drivers of the intensity and speed of the virus spreading related to urban poverty. We find that stay-at-home orders (occurring between 7 to 14 days before the assessment of the virus incidence) do not significantly contribute to reducing the incidence of COVID-19 at the county level. Instead, introducing lockdown policies intensifies the effect of urban poverty on COVID-19 spreading.

The rest of the paper is organized as follows. Section 2.2 introduces the urban poverty measures used in this paper. Section 2.3 describes the data sources and the geographic matching we perform to construct our using sample of 1054 American urban counties. The empirical strategy is set out in Section 2.4, whereas results are discussed in Section 2.5. Section 2.6 concludes.

2.2 Measuring urban poverty

This paper focuses on American cities and the degree of income poverty observed therein. The main treatment variable that we study are *urban poverty* measures and concerns the uneven spatial distribution of poverty across the neighborhoods of a city. A *urban poverty* index maps information about the degree of concentration of poor individuals in areas of the city where poverty is relatively over-represented into a number, understood as the

(2011), Ludwig et al. (2013) for health outcomes, Conley and Topa (2002) for labor market attachments, Ludwig et al. (2012) for individual well-being and Chetty et al. (2016), Chetty and Hendren (2018) for the economic opportunities of future generations.

level of urban poverty displayed by that city. Urban poverty measures capture the fact that, in the presence of urban poverty, the poor are more likely to share the same neighborhood and thus interact locally with other poor residents than the non-poor population, thereby suffering a *double burden* of poverty: not only in areas where poor residents are highly concentrated there is more poverty, but also those living in these places suffer the detrimental consequences of being exposed to urban poverty in terms of opportunities for human and social development.

In this setting, every movement of a poor individual from a high-poverty neighborhood (i.e., where the share of poor residents is above a given acceptance threshold) towards a neighborhood where poverty is less concentrated is always bound to reduce urban poverty. Owing to this principle, alongside technical axioms, Andreoli et al. (2021) characterize a family of urban poverty measures combining information on: i) features of the incidence of poverty in the city, ii) the distribution of poverty across high-poverty neighborhoods, and iii) the extent of segregation of poor and non-poor populations across high-poverty and low-poverty neighborhoods. This section provides the theoretic underpinnings of urban poverty measurement.

We use the American Census Bureau's definition of Metropolitan Statistical areas (MSAs) to identify cities. Each MSA consists of n non-overlapping census tracts, defining neighborhoods. Let $N_i \in \mathbb{R}_+$ denote the population living in census tract $i \in \{1, \dots, n\}$, whereas P_i is the population that is poor living in i. Then, $N = \sum_{i=1}^{n} N_i$ and $P = \sum_{i=1}^{n} P_i$ are, respectively, the overall population and the total number of poor in the MSA. In our model, we represent an MSA by the corresponding *urban poverty configuration* $\mathcal{A} = \{P_i^{\mathcal{A}}, N_i^{\mathcal{A}}\}_{i=1}^{n}$. Let $\zeta \in [0, 1]$ be the urban poverty line used to identify tracts where poverty is more concentrated, that is where the ratio $\frac{P_i}{N_i} \geq \zeta$. Hence, for a given urban poverty line ζ, there are $z \geq 1$ tracts where poverty is highly concentrated. Assuming that tracts are ordered by decreasing magnitude of poverty incidence, so that $\frac{P_i}{N_i} \geq \frac{P_{i+1}}{N_{i+1}}$, then $\overline{P}_z = \sum_{i=1}^{z} P_i$ and $\overline{N}_z = \sum_{i=1}^{z} N_i$ denote the number of poor individuals and the total population residing in census tracts where poverty is highly concentrated, respectively. Following the Census Bureau definition, we set $\zeta = 0.2$ to identify high-poverty census tracts as places where the share of poor individuals is larger than 20%

The *urban poverty* UP index characterized by Andreoli et al. (2021) is a function that maps a configuration $\mathcal{A}$ into a number measuring the level of urban poverty in the MSA, and it is defined as follows:

$$UP(\mathcal{A}, \zeta) := \beta \frac{\overline{P}_z - \zeta \overline{N}_z}{P} \; + \; \gamma \left(\frac{\overline{N}_z}{N} \right) \frac{\overline{P}_z}{P} G(\mathcal{A}, \zeta) \; + \; \gamma \left(\frac{N - \overline{N}_z}{N} \right) \frac{\overline{P}_z - \zeta \overline{N}_z}{P}, \tag{2.1}$$

where $\beta, \gamma \geq 0$ and $z \geq 1$, with $UP(\mathcal{A}, \zeta) = 0$ if $z = 0$. The parameters β and γ have a normative interpretation. The parameter γ represents the weight of the distributional component of urban poverty, which combines information about the distribution of poverty across high-poverty neighborhoods $i = 1, \dots, z$ measured by the Gini index $G(\mathcal{A}, \zeta)$, with information about the composition of poverty in the population. Instead, the parameter β is the weight assigned to poverty incidence. Different parametric specifications allow focusing on different dimensions of urban poverty, prioritizing the incidence of poverty or its distribution within (and across) high-poverty (and low-poverty) neighborhoods. For example, by setting $\gamma = 0$ and $\beta = 1$, equation 2.1 reduces to the *adjusted concentrated poverty* index:

$$CP^*(\mathcal{A}, \zeta) := \frac{\overline{P}_z - \zeta \overline{N}_z}{P} = CP(\mathcal{A}, \zeta) - \zeta \left(\frac{\overline{N}_z}{P} \right), \tag{2.2}$$

which represents a correction of the well-known concentrated poverty index $CP(\mathcal{A}, \zeta) := \overline{P}_z/P$ (Iceland & Hernandez, 2017; Jargowsky & Bane, 1991; Wilson, 1987) that measures the proportion of poor people living in high-poverty census tracts as identified by the urban poverty line ζ. A related index is the *poverty incidence* at the city level, denoted $H(\mathcal{A}) := P/N$.

By setting $\gamma = 1$ and $\beta = 0$, with $\zeta = 0$ to highlight that distributional concerns about poverty involve all city neighborhoods, the relevant urban poverty index in equation (2.1) becomes

$$UP(\mathcal{A}, 0) = G(\mathcal{A}) := \frac{1}{2P/N} \sum_{i=1}^{n} \sum_{j=1}^{n} \frac{N_i\, N_j}{N^2} \left| \frac{P_i}{N_i} - \frac{P_j}{N_j} \right|,$$

which is the Gini index of the distribution of poverty incidence at the tract level.

One exciting feature of this index is that it can be additively decomposed along the spatial dimensions into a *neighborhood* G_N and a *non-neighborhood* G_{nN} component so that $G(\mathcal{A}) = G_N(\mathcal{A}) + G_{nN}(\mathcal{A})$. Overall, the decomposition allows us to keep track of the spatial clustering dimension of urban poverty. The neighborhood component captures features of the unequal distribution of poverty across neighboring census tracts. A small level of G_N (relative to G_{nN}) reflects, on average, a low heterogeneity in poverty incidence across neighboring census tracts, indicating the presence of clustering. A small level of G_{nN} (relative to G_N) signals that Census tracts not in spatial proximity tend to display similar levels of incidence of urban poverty.

2.3 Data

We perform our empirical investigation using a new database of 1,054 urban counties located in 330 MSAs in the United States. We have matched information about Coronavirus spreading and its potential drivers for each county from the Census Bureau database. Counties are the finest available geographic aggregates at which information on the early incidence of cases testing positive for Coronavirus is reported. We have then matched counties to MSAs and attributed to each county the latter estimate of urban poverty measures available for the MSA where the county is located (issued from Andreoli et al., 2022). Table 2.1 reports a descriptive account of our sample, which we discuss in detail below.

2.3.1 Urban poverty

We produce estimates of urban poverty for the largest American MSAs making use of the information on the distribution of urban poverty obtained from the wave 2012-2016 of the 5-years module of the American Community Survey (ACS), reporting information on poverty incidence at the census tract level.[15] Poverty counts in ACS are expressed as the number of individuals in families with total income below the poverty line, based on the number and age of family members. Poverty status is also determined for individuals not in families, provided they are not prison inmates, Armed Forces members living in barracks, college students living in dormitories, and unrelated individuals under 15. We consider "poor" families (and thus all family members) whose income is less than the federal income poverty line given by the Census Bureau.

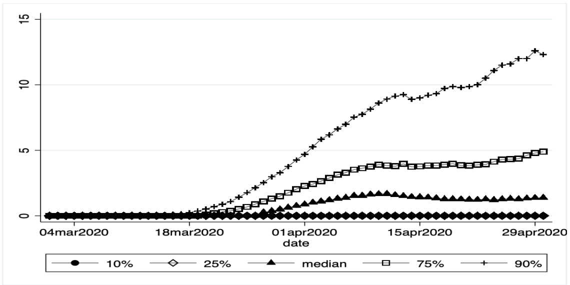
[15] The data have been validated in (Andreoli et al., 2022).

	mean	sd	p25	median	p75
Urban poverty measures					
UP(.,0.2)	0.17	0.06	0.14	0.17	0.20
CP (.,0.2)	0.51	0.13	0.43	0.51	0.58
G	0.39	0.07	0.35	0.40	0.44
G_N	0.16	0.09	0.08	0.14	0.21
G_nN	0.23	0.11	0.15	0.23	0.33
H	0.15	0.04	0.13	0.15	0.17
(A) Demographics					
Population size (ln)	11.48	1.36	10.46	11.52	12.40
Pop. density (ln)	5.22	1.47	4.22	5.14	6.10
(share) Black	0.11	0.14	0.02	0.06	0.15
(share) Hispanic	0.10	0.13	0.03	0.05	0.11
(share) Asian	0.03	0.04	0.01	0.01	0.03
(share) White	0.75	0.19	0.64	0.80	0.91
(share) 65 plus yrs old	0.16	0.04	0.13	0.16	0.18
(B) Housing stock					
(share) Old houses (20 plus yrs old)	0.65	0.14	0.56	0.66	0.74
(share) Owner occupied	0.71	0.09	0.66	0.72	0.78
Median value house by CT (ln)	10.58	1.65	9.40	10.57	12.04
S.d. value house by CT (ln)	10.72	0.81	10.29	10.75	11.20
Median rent by CT (ln)	4.44	1.64	3.24	4.41	5.97
S.d. rent by CT (ln)	4.93	0.75	4.49	5.00	5.45
(C) Education and employment					
% less than high school	3.17	1.89	1.89	2.65	3.99
% with college	38.92	7.14	34.06	38.89	43.65
% Commute < 1/2 hour	28.16	6.87	23.11	27.52	32.77
Median hh income by CT (ln)	8.83	1.55	7.72	8.86	10.29
Avg hh income (ln)	11.06	0.26	10.88	11.05	11.21
S.d. hh income by CT (ln)	9.56	0.63	9.27	9.66	9.98
College Town	0.30	0.46	0.00	0.00	1.00
Student Town	0.15	0.36	0.00	0.00	0.00
(D) Health insurance coverage					
% health insurance among 65+	15.25	3.45	12.83	15.21	17.24
% without health insurance	10.98	4.35	7.77	10.54	13.74
(E) Ethnic dimension of segregation					
Dissimilarity black	0.45	0.10	0.37	0.46	0.53
Dissimilarity hispanic	0.36	0.08	0.31	0.36	0.41
Dissimilarity asian	0.47	0.07	0.42	0.47	0.52
Instrument					
% LIHTC units by MSA	2.14	0.85	1.49	2.02	2.79
Observations	1,054	1,054	1,054	1,054	1,054

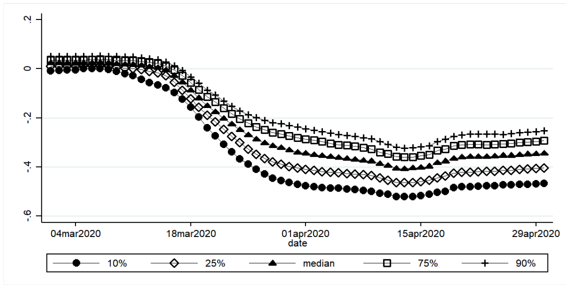
Table 2.1: Descriptive statistics

We group census tracts into cities following the 2016 definition of MSA provided by the Census (see Andreoli and Peluso, 2017). For each census tract, we compute poverty incidence as the number of poor in a given neighborhood normalized by the total number of residents therein. Then, we obtain urban poverty measures for

Figure 2.1: COVID-19 spread and changes in mobility patterns



(a) Daily cases per 100k residents



(b) Daily variation in mobility to workplace

Note: Subfigures (a) and (b) represent the trends by different percentiles in daily COVID-19 cases per 100k residents and mobility, respectively, between March and April 2020. Data on daily COVID-19 cases per 100k residents in panel (a) are from the New York Times COVID-19 repository. Mobility data in panel (b) are from the Economic Tracker, which refers to Google's COVID-19 Community Mobility reports. These reports record percentage changes in time spent in different locations from January 3 - February 5, 2020.

high-poverty neighborhoods, i.e., neighborhoods with a poverty incidence above 20% of the resident population. Table 2.1 provides distributional statistics about urban poverty measures across American MSAs in the pre-pandemic period.

2.3.2 COVID-19 cases

The data on COVID-19 cases are collected from the Economic Tracker, which reports the daily number of cases and deaths at the county, state, and national level, using information from the New York Times COVID-19 repository (for additional details, see Chetty et al., 2020). We use data on the county's daily number of reported cases, normalized by 100k residents and expressed as a seven-day moving average to smooth out daily fluctuations in the number of tests or report delays. Panel (a) of Figure 1 shows trends in the incidence of new cases between March-April 2020. We measure the speed of pandemics in the county as this number changes in the one-week and two-week windows.[16]

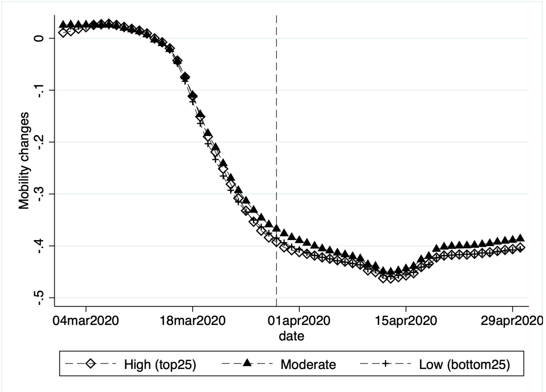
2.3.3 Mobility and stay-at-home orders

We gather mobility data from the Economic Tracker (for a presentation of the dataset, see Chetty et al., 2020), which reports data on daily time spent at various locations (i.e., parks, retail, grocery stores, transit locations, and workplaces) as a percentage change from a baseline period of January 3 to February 5, 2020.

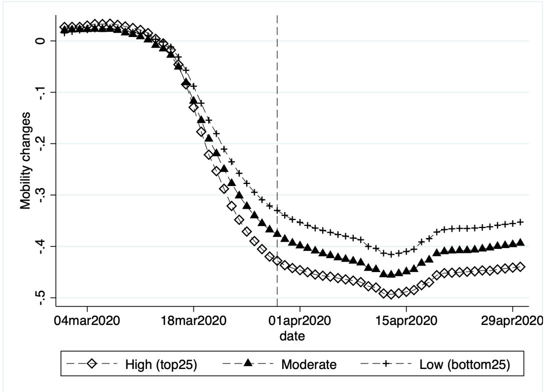
We focus on county-level data on mobility to workplaces, for which urban counties have nearly universal coverage. From panel (b) in Figure 2.1, we note that mobility has dropped since mid-March 2020. Figure 2.2

[16]The Centers for Disease Control and Prevention has produced a restricted-use dataset on county-level health outcomes related to COVID-19 (cases, hospitalization, death), disaggregated by characteristics of the patients. These data cover a limited amount of urban counties, thus reducing the reliability of our identification strategy. For these reasons, our analysis focuses on official aggregate statistics granting universal coverage of urban counties.

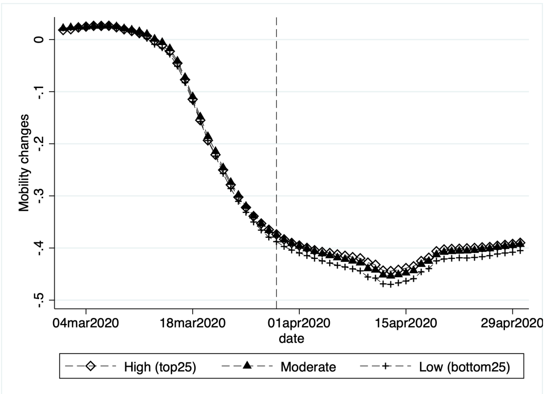
Figure 2.2: Mobility to workplaces



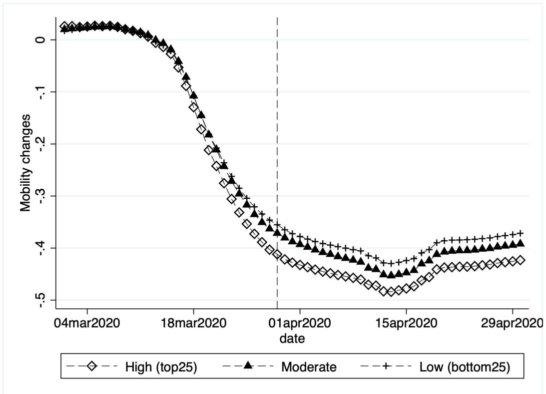
(a) $UP(.,0.2)$



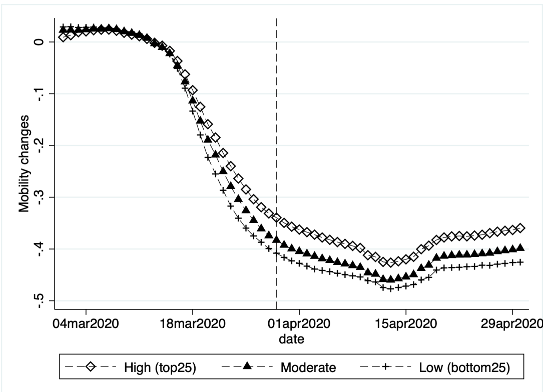
(b) G



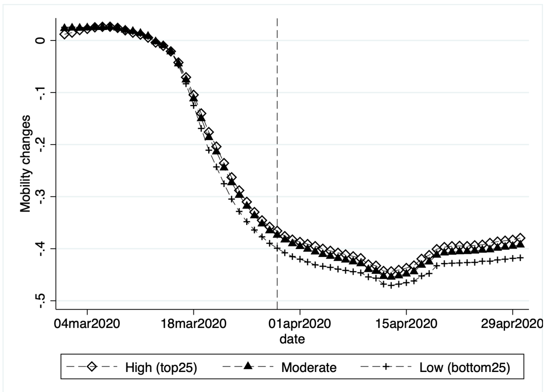
(c) G_N



(d) G_{nN}



(e) H



(f) CP(.,0.2)

Note: Data on mobility are from the Economic Tracker, which refers to Google COVID-19 Community Mobility reports. These reports record the percent changes in the time spent at different locations relative to January 3 - February 5, 2020. Data on poverty indicators are from authors' elaboration of data from ACS.

illustrates the pattern of daily changes in mobility by different levels of urban poverty indicators. More specifically, we group counties into three categories based on the urban poverty level of the MSA to which they belong, i.e., *low* below the 25^{th} percentile, *moderate* between the 25^{th} and 75^{th} percentiles, and *high* above the 75^{th} percentile. Changes in mobility compared to the reference period are shown on the vertical axis, while the horizontal axis reports dates. The vertical dashed line is on March 29, 2020, i.e., the first period considered in our analysis. Looking at urban poverty as measured by the $UP(.,0.2)$ index, we note that the trend is almost the same for the different levels of poverty considered (panel (a)). In contrast, panel (b) shows that mobility declines fastly in counties where the poor are more unevenly distributed. Interestingly, this difference has become evident since March 19, 2020, when the first stay-at-home order was issued. In addition, most of the differences due to the G index are attributable to the non-neighborhood (G_{nN}) component (panels (c) and (d)). Finally, the last two panels show changes in mobility levels by share of poor in the county as measured by the headcount ratio H (panel (e)) and concentrated poverty levels $CP(.,0.2)$ (panel (f)). In both cases, mobility declines less in counties with high levels of these indicators.

2.3.4 Data on covariates

From the wave 2012-2016 of the ACS, we gather additional information on county characteristics, reported in Table 2.1. These variables are measured at the county level and cover the following domains of heterogeneity: demographics, housing, education and employment, health insurance coverage, and ethnic segregation. Demographic data in group A refer to the size of the county's population (in log) and its composition by racial/ethnic and age groups (as shares of the total population). Controls in group B describe the aggregate quality of the housing market (such as the share of old houses aged more than twenty years) and the housing opportunities for low-income families (such as the share of owner-occupied homes). Lastly, we include information on the distributions of owner-occupied housing and rental values, summarized by the median housing/rental values (in log) and dispersion (expressed as standard deviation). Covariates in group C gather information on the human capital composition of the county population. We consider the share of low-educated (less than high-school) and high-educated (with college) residents. The description of the county from the education perspective includes two indicator variables indicating whether the county belongs to a student town (i.e., a top 20 MSA in terms of students enrolled in any college) or a college town (i.e., an MSA hosting selective colleges with tier level equal to 1 or 2).[17] Group C also includes the share of commuters less than a half-hour away from work and variables related to the county's income distribution, such as average household income, county median affluence, and income dispersion. These distributional variables are good proxies for the quality and affordability of the county. Group D examines the health-related dimension of the county, providing information on the proportion of people aged 65 years old who have health insurance and the share of the total population without health insurance. To

[17] Selective colleges are identified according to the college tier description used by the Department of Education's (DOE) IPEDS database. We consider college town cities with colleges with tier levels equal to 1 or 2, corresponding to Ivy League colleges plus Standford, Chicago, Duke, and MIT, alongside other elite private and public schools with Barron's 2009 selectivity index of 1. While the number of students to identify students town refers to the number of IPEDS enrollment in the fall 2013 semester. For a detailed description of the variables used to construct our indicators, see Chetty et al. (2017) and Table 6 and Table 10 at `https://opportunityinsights.org/data/`.

describe the ethnic dimension of segregation across the MSA neighborhoods (E), we use the dissimilarity index for Blacks, Hispanics, and Asians relative to the overall population.

Lastly, we consider additional variables related to the Low Income Housing Tax Credit (LIHTC) program, which we use as instruments for urban poverty. Specifically, we consider the average number and distribution of LIHTC-eligible projects within a county. Our data show that 9% of the census tracts in MSAs are Qualified Census Tracts.[18] Also, 43% of the census tracts are considered metro or central city census tracts. Finally, 26% of LIHTC projects are new constructions, unlike rehabilitation or acquisition projects. The specific characteristic of the LIHTC program may serve as an exogenous source of relocation of high and low-income households that smooth differences in the presence of such groups in low-income census tracts, thus reducing urban poverty. We illustrate the validity of the LIHTC scheme as an instrument for urban poverty in the next section.

2.4 Empirical strategy

2.4.1 Baseline

We endorse different identification strategies to estimate the impact of urban poverty on the occurrence of COVID-19 cases and its interaction with mobility restriction policies. First, we rely on variations in the incidence and distribution of urban poverty across MSAs, controlling for observable characteristics of the cities, for state fixed effects, and reporting estimates for different periods to disentangle the implications of mobility restrictions on actual levels of mobility.

We distinguish three subperiods. The earliest period covers the onset of COVID-19 outbreaks as of March 29, 2020. This date is ten days (roughly corresponding to the average incubation period of the virus) after the first stay-at-home order enacted in California. Therefore, COVID-19 incidence data at this date are likely unaffected by the introduction of early lockdown policies and reflect initial patterns of pandemic evolution in response to local characteristics.

The second period that we consider ends on April 13, 2020, about ten days after the implementation of lockdown restrictions in all US states. We look at the number of new cases as of April 13 and their speed of variation over the one- and two-week windows to test the persistence of urban poverty effects on COVID-19 incidence after enacting lockdown policies.

The third period looks at Coronavirus incidence as of April 29, 2020, after the introduction of mobility restriction orders. We produce separate estimates of our baseline models across these different periods to reduce bias generated by simultaneity in lockdown measures implementation and incidence of COVID-19 at the county level.

The following regression model estimates the effect of interest separately for each period t in which COVID-19 cases are observed:

$$y_{cms}(t) \;=\; \alpha_0 + \beta_1 I_m + \beta_2 M_m + \beta_3 M_m * I_m + \boldsymbol{\beta}_4 \mathbf{X}_c + \beta_s + \varepsilon_{cms}, \tag{2.3}$$

[18]Qualified Census Tracts (QCTs) are tracts with at least 50% of households with an income below 60% of the Area Median Gross Income or with a poverty rate above 25%

where c, m and s denote respectively county, MSA and State. Notice that the spatial organization of the data reflects that MSAs are composed of more than one county, and more MSAs belong to the same state.

The dependent variable $y_{cms}(t)$ measures the incidence of new COVID-19 cases in American MSAs as a weekly moving average at date t (March 29, April 13, and April 29, 2020). We also consider models which focus on the speed of growth of new COVID-19 cases at the county level, measured as the difference in weekly averages of new cases in date t with the new cases registered one week $(t-7)$ or two weeks $(t-14)$ ahead. Our baseline estimates are carried out separately for each date.

The variable I_m denotes an indicator for the level of urban poverty in the MSA m, chosen among those described in Section 2. The level of urban poverty is always normalized by the cross-MSA variability, implying that the effect of interest β_1 continuously measures the implications of a one standard deviation increase in urban poverty on COVID-19 incidence.

The variable M_m captures the average variation in mobility (using the weekly moving average) at the MSA level on a given date relative to January 2020. The larger the value of M_m, the closer mobility observed at a given date to mobility registered in the pre-COVID period. Model (2.3) includes the interaction of mobility with urban poverty in the MSA $(I_m * M_m)$. The associate coefficient (β_3) captures the effect of an exogenous change in urban poverty as a function of changes in mobility due to the introduction of stay-at-home orders issued by state authorities. To address selection on observables, we control for county-specific characteristics $\mathbf{X}_c$, including distributive statistics, and add state fixed effects β_s to account for state-specific features of lockdown policies as well as the local performances of the health care system in terms of COVID-19 spreading testing.

Baseline identification relies on a strong exogeneity assumption, which postulates that within-state variation in urban poverty is unrelated to unobservable drivers of the pandemic that can be related to other variables, such as mobility restrictions enacted in a given county. This assumption may be violated if factors related, for instance, to the quality of health care services provided in one state (which we do not observe and that are related to the quality and quantity of COVID-19 monitoring) are also important drivers of early lockdown policies (often enacted to prevent over-crowding of ICU units). In this situation, bias would negatively affect the estimated impacts of urban poverty. We address these and other concerns with an instrumental variable approach.

2.4.2 Addressing endogeneity

The coefficients of interest in Model (2.3) may be biased because of measurement errors in the dependent variable and unobservable drivers of the pandemic confounding the effect of interest. This may happen if, for instance, the distribution of urban poverty is correlated with the degree of accessibility to some essential services, such as transportation (Glaeser et al., 2008) and healthcare facilities (Eichenbaum et al., 2021; Mercado et al., 2007) which act, in turn, as drivers of the pandemic (Tirachini & Cats, 2020). This problem is likely less relevant in cities with higher median incomes, which tend to exhibit a more heterogeneous income mix at the neighborhood level with less inequality across census tracts (Andreoli et al., 2022) and provide greater access to urban amenities and localized public goods (Eichenbaum et al., 2021). In addition, people living in cities with a higher proportion of poor residents and a higher level of concentrated poverty across census tracts spend more time commuting

(Wang et al., 2018), implying a greater likelihood of contagion spread. Andreoli et al. (2022) also find that urban poverty correlates with individuals who commute to their jobs, are less educated on average, and are less likely to be managers. Finally, weaker social ties between high- and low-income residents, as captured by the segregation component of the urban poverty index, may lead to a more difficult diffusion of social practices and norms that prevent the spread of the virus.

To address endogeneity concerns, we suggest using an instrumental variable approach. An instrument for urban poverty should be correlated with the distribution of poverty in the city but should not be related to the potential sources of endogeneity. We propose using the Low Income Housing Tax Credit Scheme (LIHTC) as a source of identifying information. Established as part of the Tax Reform Act of 1986 to promote the development of affordable rental housing for low-income families, LIHTC has become the largest and most generous federal housing program (Ellen et al., 2016; Ellen et al., 2009). The federal government allocates a LIHTC budget to States yearly based on their demographic size. States assign tax credits to developers who submit projects to build or refurbish low-income rental housing. Projects eligible for the ten-year stream of tax credit must satisfy one of the following two criteria for at least 30 years: i) at least 20% of households that will occupy the units must have income below 50% of the area median income (AMI), or ii) at least 40% of units tenants must have income below 60% of the AMI. Projects in qualified census tracts (QCT) are eligible for a tax credit of at least 30% of the construction cost. QCTs are tracts where at least 50% of households earn an income below 60% of AMI, or the poverty rate is above 25%, provided that less than 20% of the total population of a metropolitan area lives in QCT.

The allocation mechanism of tax credits generates a quasi-random assignment of tax credits (and hence the supply of affordable housing and renovated housing) across census tracts as a function of their eligibility status. Eligibility is randomized according to administrative thresholds based on some relevant neighborhoods' characteristics, inducing a form of spatial discontinuity in the assignment of the LIHTC to projects across otherwise comparable neighborhoods (Baum-Snow & Marion, 2009).

Given its specific characteristics, LIHTC may serve as an exogenous source of relocation of high- and low-income households that smooth differences in the presence of such groups in low-income census tracts, thus reducing urban poverty. Overall, although QCTs receive more projects than other tracts (Baum-Snow & Marion, 2009), existing evidence shows that LIHTC tends to de-concentrate poverty (Ellen et al., 2009; Freedman & McGavock, 2015), albeit with a small effect overall (Ellen et al., 2016). In addition, in the long run, there is an improvement in the neighborhood's conditions which also becomes more attractive to affluent families (Baum-Snow & Marion, 2009; Ellen et al., 2016). Introducing new LIHTC projects raises the proportion of non-poor residents in census tracts where poverty is highly concentrated without displacing poverty from affected neighborhoods. As a result, the LIHTC scheme expansion should result in a more even distribution of poverty across census tracts (Ellen et al., 2016). We exploit quasi-randomization of LIHTC allocation across census tracts to identify exogenous marginal changes in urban poverty. We make use of the number (EZ_m) of housing projects implemented in the last 20 years across MSA m normalized by the total number of houses in m as an instrument for urban poverty indicators I_m, also measured at the MSA level.

Our instrumental variable estimates are obtained in two steps. First, we consider the endogenous variables in the baseline model (2.3), I_m (constant across all counties c comprised in MSA m), and we regress it on available characteristics at the MSA level and the instrument according to the following regression model:

$$I_m \;=\; \gamma_0 + \gamma_1 EZ_m + \boldsymbol{\gamma}_3 \mathbf{X}_c + \gamma_s + v_{mcs}, \tag{2.4}$$

The model, specified at the MSA level, is estimated on county-level data. Identification relies on LIHTC incidence variability across counties in different MSAs. We use estimated coefficients from this first stage to predict values $\widehat{I}_m$ at the MSA level, which we include among the second stage regressors, specified instead at the county level for which we observe the relevant measures of COVID-19 cases:

$$y_{cms}(t) \;=\; \alpha_0^{IV} + \beta_1^{IV} \widehat{I}_m + \boldsymbol{\beta}_2^{IV} \mathbf{X}_c + \beta_s^{IV} + \varepsilon_{cms}^{IV}. \tag{2.5}$$

The coefficient of interest is β_1^{IV}, which reports the effect of a one standard deviation increase in urban poverty induced by the LIHTC scheme on the incidence of cases testing positive for COVID-19 in a reference period t. Our exclusion restriction rests on the fact that the LIHTC affects the distribution of poverty incidence across a city's census tracts without consequences on other dimensions. These are relevant for assessing pandemic incidence but are not captured by the urban poverty indicator. Models (2.3) and (2.5) break down the effects by period (i.e., before, during, and after the introduction of the lockdown policies).

2.4.3 Urban poverty and effectiveness of mobility restrictions

Another aspect of COVID-19 onset that we consider is the interrelation between restriction mobility policies aimed at reducing cases of COVID-19 on the onset of the pandemic and the extent of urban poverty registered in those places where restrictions are implemented. We evaluate whether the effect of mobility restrictions on the pandemic spreading varies along the lines of urban poverty experienced by people subject to those restrictions. We assess such impact empirically, exploiting variation across time and space (counties) of COVID-19 *daily* occurrences from early March 2020 to April 30, 2020. Our preferred specification involves a fixed-effects model, which allows us to investigate the magnitude of stay-at-home orders on COVID-19 spread by differentiating effects based on the incidence of urban poverty in the MSA m to which county c belongs:

$$y_{cmsd} = \delta_0 + \delta_1 \mathbf{1}\{stay{-}at{-}home_c \geq d{-}10\} + \delta_2 \mathbf{1}\{stay{-}at{-}home_c \geq d{-}10\} * P_m + \delta_d + \delta_c + \delta_m + \delta_s + \eta_{cmsd}, \tag{2.6}$$

where y_{cmsd} denotes the *daily* new positive testing cases occurring in county c on day d or the speed of contagion on the same date. The indicator $\mathbf{1}\{stay-at-home_c \geq d-10\}$ takes value one starting ten days after the date of introduction of a stay-at-home order in county c ($stay-at-home_c$) onward, zero otherwise. The ten-day delay in the assignment of lockdown policies allows us to measure changes in pandemic patterns attributable to policy effects. Including time, county, and MSA fixed effects captures time-invariant attributes (such as urban poverty) across counties belonging to different MSAs, whereas state fixed effects account for differences in health care supply across states. The coefficients of interest are δ_1 and δ_2, the latter providing the effect of rising urban poverty on the incidence of COVID-19 conditional on the introduction of lockdown policies. Small values of δ_2

suggest that the effects of urban poverty on COVID-19 spreading depend on attributes of MSA unrelated to policies limiting movements.

We use linear regression methods (OLS and IV) to estimate models (2.3) and (2.5) respectively, using the level or growth of new cases normalized by 100k residents in the county. The propagation speed of COVID-19 is registered as a difference in new cases observed in different data over the period considered in this study. We also use Poisson count models to consider counts of new cases testing positive for COVID-19. We use longitudinal data at a county-day level on the COVID-19 cases and fit fixed-effect models to estimate Model (2.6) coefficients. Standard errors are always robust and clustered at the MSA level, corresponding to the geographic layer of aggregation of the treatments of interest. In the instrumental variable plug-in estimator, we follow the methodology in Murphy and Topel (2002) to correct second-stage standard errors for the uncertainty introduced by first-stage predictions.

2.5 Results

Table 2.2 reports the effect of increasing the index $UP(.; 0.2)$ by one standard deviation on the incidence and speed of COVID-19 at the county level. We discuss different specifications of equation (2.3). Models 1-3 are estimated using OLS on the number of new cases of COVID-19 normalized by 100k residents, whereas models 4-6 are not normalized and are estimated using a non-linear count model (Poisson regression). Models 7-12 provide estimates of the speed of COVID-19 spreading at different dates ($t-7$ and $t-14$). Models differ in the explanatory variables. Model 1 includes only the urban poverty index as the main treatment, whereas model 3 corresponds to the full specification of the equation (2.3). All models control for the explanatory variables A-E described above, as well as for state fixed effects. Estimates are broken down into three periods. During the first period, covering the early incidence of COVID-19 as of March 29, 2020, variations in mobility are assessed as of March 15, 2020, 14 days ahead of the measurement of COVID-19 cases. A similar time frame is used when assessing the delayed effects.

We find that a one standard deviation increase in urban poverty is positively associated with the number of positive Coronavirus case occurrences in the first period, which rises by 0.25 cases per 100k residents in American urban counties. The effect is, however, not significant. Conversely, we find a significant positive impact of urban poverty on the speed of propagation of the Coronavirus pandemic over a week. The magnitude of the effects is 0.27 new cases per 100k residents.

When stay-at-home orders have been heterogeneously introduced across American states, we do not detect significant effects of rising urban poverty on new cases of COVID-19 and the speed of COVID-19 propagation. The negative and large coefficient associated with the variation in mobility reflects the late onset of stay-at-home orders in counties with a low incidence of COVID-19 cases (which, in turn, reflects only small reductions in the extent of mobility to work).

Results suggest that urban poverty rises the speed of new COVID-19 cases at the onset of the pandemic. Before April 29, 2020, a rise in urban poverty was associated with the rising speed of COVID-19 cases, often amplified by the degree of mobility within one or two weeks before the reference date. For instance, a rise in

Dependent variable:	COVID-19 cases						COVID-19 speed new cases					
Measure:	#positives/100k			#positives			#positives/100k					
Time frame:	-	-	-	-	-	-	7 days	7 days	7 days	15 days	15 days	15 days
Method:	OLS			Poisson			OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Early incidence - 29 March												
UP(.,0.2) - SD units	0.25	0.23	0.11	11.15	12.80	8.81	0.27**	0.26**	0.27**	0.27	0.26	0.37*
	(0.20)	(0.21)	(0.25)	(7.90)	(8.27)	(10.25)	(0.12)	(0.13)	(0.13)	(0.19)	(0.20)	(0.20)
Δ Mobility workplaces (15-3)		3.05	17.07		-267.79*	-85.13						
		(4.47)	(11.29)		(147.67)	(179.48)						
UP(.,0.2)*Δ mobility (15 mar)			-4.80			-86.12						
			(3.36)			(83.88)						
Δ Mobility workplaces (8-15 mar)								0.69	11.81			
								(4.64)	(7.43)			
UP(.,0.2)*Δ (avg.) mobility (8-15 mar)									-3.80			
									(2.39)			
Δ Mobility workplaces (29 feb - 15 mar)											4.22	30.36*
											(6.64)	(18.13)
UP(.,0.2)*Δ (avg.) mobility (29 feb-15 mar)												-9.29
												(5.70)
Observations	1,054	1,002	1,002	1,054	1,002	1,002	1,054	1,006	1,006	1,054	1,007	1,007
R-squared	0.543	0.556	0.557				0.445	0.458	0.458	0.532	0.544	0.545
Stay-at-home period - 13 April												
UP(.,0.2) - SD units	0.67	0.51	-1.34	19.27	20.99	11.99	0.14	0.18	2.86**	0.43	0.40	1.41
	(0.52)	(0.50)	(1.95)	(33.45)	(34.07)	(115.29)	(0.18)	(0.18)	(1.09)	(0.36)	(0.37)	(1.40)
Δ Mobility workplace (30-3)		-10.61	2.05		-1,750.10**	-1,701.75**						
		(9.28)	(16.61)		(458.82)	(810.04)						
UP(.,0.2)*Δ mobility (30 mar)			-4.81			-19.51						
			(4.23)			(226.75)						
Δ Mobility workplaces (23-30 mar)								17.44*	-2.79			
								(9.05)	(6.83)			
UP(.,0.2)*Δ (avg.) mobility (23-30 mar)									7.83**			
									(3.08)			
Δ Mobility workplaces (16-30 mar)											0.52	-10.08
											(5.57)	(12.93)
UP(.,0.2)*Δ (avg.) mobility (16-30 mar)												4.00
												(4.43)
Observations	1,054	1,013	1,013	1,054	1,013	1,013	1,054	1,014	1,014	1,054	1,014	1,014
R-squared	0.574	0.587	0.588				0.306	0.337	0.348	0.420	0.433	0.434
Stay-at-home period - 29 April												
UP(.,0.2) - SD units	-0.73	-0.92**	-3.83	0.68	3.03	48.15	-0.76*	-0.74*	-3.79	-1.39**	-1.40**	-2.52
	(0.53)	(0.46)	(3.47)	(48.22)	(49.86)	(216.12)	(0.42)	(0.41)	(2.74)	(0.58)	(0.52)	(3.40)
Δ Mobility workplace (15-4)		3.90	21.77		-3,484.57**	-3,711.04**						
		(22.34)	(37.96)		(778.40)	(1,456.24)						
UP(.,0.2)*Δ mobility (15 apr)			-6.57			89.24						
			(7.03)			(405.73)						
Δ Mobility workplaces (8-15 apr)								13.75	32.54			
								(16.78)	(27.42)			
UP(.,0.2)*Δ (avg.) mobility (8-15 apr)									-7.00			
									(5.57)			
Δ Mobility workplaces (31 mar - 15 apr)											31.74*	38.80
											(18.96)	(34.48)
UP(.,0.2)*Δ (avg.) mobility (31 mar-15 apr)												-2.66
												(7.13)
Observations	1,054	981	981	1,054	981	981	1,054	1,014	1,014	1,054	1,015	1,015
R-squared	0.389	0.496	0.498				0.130	0.142	0.144	0.223	0.263	0.264
Controls A) B) C) D) E)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.2: UP(., 0.2) and COVID-19

Note: Based on author's elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for A), B), C), D), E), and state fixed effects. Estimates are based on urban counties. Robust standard errors are always clustered at the MSA level. Estimated effects have to be interpreted as variation in the number of cases per 100k residents (seven-day moving average) in the county (Models (1)-(3)) or the speed of new cases, i.e., the difference between the daily cases in a one or two weeks window (Models (7)-(12)). Models (4)-(6) report marginal effects at the average. SD stands for standard deviation units. Significance levels: $^{*} = 10\%$ and $^{**} = 5\%$.

1 sd of urban poverty raised the speed of new COVID-19 cases by 2.86 positive cases over 100k residents on April 13, the effect increasing substantially (7.83 cases per 100 residents) in places displaying larger mobility at the end of March 2020. The two dimensions are likely correlated: cities showing a more unequal distribution of poverty across their neighborhoods in the city also display larger patterns of mobility, representing a mechanism reinforcing the effects through which urban poverty contributes to the rise of COVID-19 cases in American MSAs. When restrictions to mobility are widely implemented through the issue of stay-at-home orders (as of April 29), mobility is drastically reduced across all MSAs. The effect of urban poverty on COVID-19 new cases and their speed of growth on this date is now negative and often nonsignificant, owing to the switching off of the mobility channel.

The patterns of the effects of rising poverty incidence in the city, reported in Table 2.5 in the Appendix, mirror those in Table 2.2. Patterns related to concentrated poverty indices, reported in Table 2.6, are also aligned, albeit largely insignificant. Table 2.7 shows that the Gini G urban poverty measure is seldom significantly associated with COVID-19 onset. Table 2.8 reveals a remarkable similarity between the pattern of marginal effects of the rising neighborhood component of the Gini urban poverty index (G_N) on the COVID-19 outbreak and coefficients registered in Table 2.2. Effects related to the non-neighborhood component of urban poverty (G_{nN}), reported in Table 2.9, have opposite signs compared to those related to G_N but similar patterns of significance. Results in Tables 2.8 and 2.9 show that COVID-19 onset is positively associated with the extent of heterogeneity of poverty incidence in neighboring census tracts of American cities.

The effects described so far may be estimated with bias, which we address following an instrumental variable strategy. We use the share of LIHTC units by MSA, computed as the number of housing units supported by the LIHTC scheme normalized by the housing stock in each MSA, as an instrument for urban poverty measures. In Table 2.10 in the Appendix, we report first-stage estimates from the Model (2.4). As expected, an increase in the incidence of LIHTC units has a significant negative impact on several urban poverty measures, the reduction being fostered by the increased homogeneity in poor versus non-poor individuals in the neighborhoods. We find that rising by one percentage point the share of LIHTC census tracts in an MSA reduces $UP(., 0.2)$ by -0.012 sd, reduces G by -0.0053 sd, reduces $CP(., 0.2)$ by -0.0132 sd and reduces H by -0.0051 sd. In contrast, the effect is nonsignificant for the neighborhood and non-neighborhood components of the Gini urban poverty index. The sign and magnitude of the effects are compatible with those in Ellen et al. (2016), where it is shown that LIHTC-supported programs reduce the concentration of poverty by leveling the proportion of poor and non-poor residents in high-poverty neighborhoods.

Second-stage effects are reported in Table 2.3 for each urban poverty index (by rows) and by period (by column) separately. Considering the patterns of significance at the first stage, we analyze the second stage coefficients only for the urban poverty indices with significant coefficients, thereby omitting G_N and G_{nN}. The table features effects on the early incidence of COVID-19 (models 1-4), during the period where stay-at-home orders have been issued heterogeneously across states (models 5-8), and during the post-lock-down settlement (models 9-12). Each set of estimates features both new COVID-19 cases on a given date and measures of speed of progression of COVID-19 infections at 7 and 14 days lag, all normalized by 100k residents and in absolute numbers.

Dependent variable:	COVID-19 early incidence				COVID-19 stay-home period							
Measure:	#Positives/100k			#positives	#Positives/100k			#positives	#Positives/100k			#positives
Time frame:	29 Mar	22-29 Mar	15-29 Mar	29 Mar	13 Apr	6-13 Apr	30 Mar-13 Apr	13 Apr	29 Apr	22-29 Apr	15-29 Apr	29 Apr
Method:	OLS			Poisson	OLS			Poisson	OLS			Poisson
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
UP(.,0.2) - SD units	0.59**	0.55**	0.62**	26.89**	1.59**	-0.08	0.97*	89.50**	-0.54	-1.28**	-1.95**	116.46**
	(0.29)	(0.20)	(0.28)	(10.52)	(0.80)	(0.40)	(0.55)	(42.02)	(0.71)	(0.56)	(0.82)	(59.29)
Observations	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054
R-squared	0.545	0.448	0.534		0.579	0.306	0.425		0.388	0.133	0.224	
G - SD units	-0.50	-0.23	-0.45	29.25**	-0.33	0.44	0.18	61.38	-1.08	-0.65	-1.10	65.57
	(0.33)	(0.22)	(0.31)	(12.54)	(0.48)	(0.30)	(0.30)	(43.59)	(0.96)	(0.73)	(1.01)	(70.62)
Observations	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054
R-squared	0.543	0.443	0.532		0.573	0.307	0.418		0.389	0.128	0.215	
G_nN - SD units	-1.59*	-0.85*	-1.49*	37.72	-0.99	0.57	0.55	229.67**	2.31	2.18	3.00*	435.86**
	(0.84)	(0.49)	(0.79)	(26.76)	(1.34)	(0.90)	(0.87)	(82.49)	(1.88)	(1.59)	(1.75)	(126.52)
Observations	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054
R-squared	0.544	0.444	0.533		0.573	0.306	0.419		0.389	0.129	0.216	
G_N - SD units	-0.01	0.04	0.02	8.33	-0.03	0.37	0.01	-70.10	-2.47*	-1.82*	-2.79**	-178.41**
	(0.47)	(0.32)	(0.45)	(16.99)	(0.73)	(0.52)	(0.46)	(55.75)	(1.34)	(1.00)	(1.37)	(87.54)
Observations	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054
R-squared	0.542	0.443	0.531		0.573	0.306	0.418		0.391	0.131	0.220	
CP(.,0.2) - SD units	0.73**	0.60**	0.74**	19.68*	1.68*	-0.25	0.94	70.56*	-0.65	-1.43**	-2.03**	92.98
	(0.33)	(0.23)	(0.32)	(10.32)	(0.91)	(0.48)	(0.63)	(41.64)	(0.74)	(0.59)	(0.83)	(60.50)
Observations	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054
R-squared	0.545	0.448	0.535		0.578	0.306	0.423		0.388	0.133	0.223	
H - SD units	0.91**	0.74**	0.92**	14.75	1.95*	-0.35	1.03	56.98	-0.57	-1.49**	-2.13**	72.98
	(0.38)	(0.27)	(0.37)	(11.00)	(1.03)	(0.51)	(0.71)	(44.12)	(0.79)	(0.66)	(0.88)	(63.34)
Observations	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054
R-squared	0.547	0.449	0.536		0.579	0.307	0.423		0.388	0.133	0.223	
Controls A) B) C) D) E)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.3: 2nd Stage - Urban poverty measures and COVID-19

Note: Based on authors' elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for state-fixed effects. Estimates are based on urban counties. First-stage estimates are obtained with MSA-level variables. The instrumental variable is the share of LIHTC units to the housing stock in the MSA. Second-stage estimates are obtained with county-level variables. Robust standard errors are clustered at the MSA level. Standard errors are adjusted using Topel and Murphy's SE adjustment in all models, except (4)-(8)-(12). The estimated effect has to be interpreted as variations in the county number of cases per 100k residents expressed as a seven-day moving average (Models (1), (5) and (9)) or the speed of new cases, i.e., the difference between the daily cases in a one or two weeks window (Models (2)-(3), (6)-(7) and (10)-(11)). Models (4)-(8)-(12) report marginal effects at the average. SD stands for standard deviation units. Significance levels: * = 10% and ** = 5%.

We find that an increase of $UP(.,0.2)$ by one sd is significantly associated with an increase of 0.6 cases per 100k residents on March 29, 2020. The effect is relatively high, considering that the weekly average new cases in the median US county (counties ranked by the proportion of cases testing positive for COVID-19) are about 2.5 per 100k residents before April 2020. This effect is attributable to a large change in urban poverty corresponding to one s.d. increase of $UP(.,0.2)$, roughly corresponding to the gap between the bottom and the top quartile municipalities ranked by urban poverty display. Similar effects are also found for H (0.92 cases per 100k residents) and $CP(.,0.2)$ (0.7 cases per 100k residents), considering the number of poor people and their proportion living in high-poverty census tracts, respectively.

The effect on new cases grows when stay-at-home orders are issued (as of April 13, 2020). Conversely, effects on the post-lock-down period (post-April 13) are generally negative and aligned in magnitude with those described in Table 2.2. Overall, results for $UP(.,0.2)$ in Table 2.11 represent our preferred estimates, addressing potential bias arising from measurement error and endogeneity.

Lastly, we analyze the interaction between mobility restriction policies issued through stay-at-home orders

Dependent variable:	Spread of COVID-19											
Measure:	#positives/100k daily cases		Speed- 2 weeks change in new cases		#positives/100k daily cases		Speed - 2 weeks change in new cases		#positives/100k daily cases		Speed - 2 weeks change in new cases	
Time frame:	10 days				7 days				14 days			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
stayhome*UP(.,0.2)	10.68	14.48*	1.76	-7.65	9.76	16.48*	2.95	-3.57	9.43	10.09	-2.53	-14.26
	(7.44)	(8.41)	(6.06)	(7.40)	(6.40)	(8.69)	(5.72)	(5.70)	(7.84)	(7.93)	(5.08)	(8.89)
stayhome		-0.99		2.45		-1.77		1.71		-0.17		3.00
		(1.15)		(1.72)	(6.40)	(8.69)	(5.72)	(5.70)	(7.84)	(7.93)	(5.08)	(8.89)
Observations	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033
R-squared	0.441	0.441	0.136	0.138	0.441	0.441	0.136	0.137	0.440	0.440	0.136	0.139
stayhome*G	6.12	25.83**	3.02	4.18	5.20	25.94**	3.19	6.45**	6.14	25.74**	1.35	1.31
	(4.14)	(12.10)	(2.97)	(2.60)	(3.55)	(12.32)	(2.98)	(2.40)	(4.39)	(11.37)	(2.23)	(4.55)
stayhome		-8.61**		-0.51		-9.06**		-1.43		-8.53**		0.02
		(3.88)		(1.88)		(4.12)		(1.39)		(3.44)		(2.52)
Observations	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033
R-squared	0.442	0.447	0.137	0.137	0.441	0.447	0.137	0.138	0.442	0.447	0.136	0.136
stayhome*G_N	0.63	-6.10	-0.68	-6.57*	0.14	-4.77	-0.26	-5.52**	-0.17	-8.34**	-2.87	-7.86*
	(4.28)	(3.97)	(4.20)	(3.38)	(3.67)	(4.05)	(3.76)	(2.63)	(4.30)	(3.62)	(3.92)	(4.38)
stayhome		2.46		2.15		1.82		1.95		2.89		1.77
		(1.57)		(1.41)		(1.32)		(1.35)		(1.75)		(1.24)
Observations	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033
R-squared	0.438	0.440	0.136	0.138	0.438	0.439	0.136	0.138	0.438	0.441	0.136	0.138
stayhome*G_nN	9.64*	13.34**	5.29*	5.88**	8.31*	12.50**	5.28*	6.00**	10.37*	14.78**	3.82	5.68
	(5.23)	(6.12)	(3.02)	(2.45)	(4.70)	(6.18)	(3.09)	(2.08)	(5.52)	(5.72)	(2.48)	(3.46)
stayhome		-1.64		-0.26		-1.88		-0.32		-1.91		-0.81
		(1.32)		(1.52)		(1.25)		(1.31)		(1.16)		(1.51)
Observations	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033
R-squared	0.444	0.445	0.139	0.139	0.443	0.444	0.139	0.139	0.445	0.446	0.137	0.137
stayhome*CP(.,0.2)	3.03	3.23	1.21	-2.51	2.50	3.81	1.39	-1.36	2.77	1.45	-0.19	-4.96
	(2.29)	(2.97)	(2.25)	(2.85)	(1.86)	(3.02)	(2.11)	(2.26)	(2.45)	(2.67)	(1.88)	(3.48)
stayhome		-0.13		2.40		-0.86		1.79		0.84		3.05
		(1.93)		(1.80)		(1.82)		(1.57)		(2.03)		(1.99)
Observations	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033
R-squared	0.440	0.440	0.136	0.137	0.439	0.439	0.136	0.137	0.439	0.439	0.136	0.138
stayhome*H	4.54	-18.59	2.67	-17.59	2.78	-15.26	2.90	-14.99	3.89	-25.62	-1.25	-22.61
	(6.14)	(17.62)	(8.27)	(13.27)	(4.81)	(18.27)	(7.37)	(9.88)	(6.66)	(16.44)	(7.45)	(16.64)
stayhome		4.31		3.77*		3.38		3.36*		5.42		3.92*
		(3.62)		(2.08)		(3.50)		(1.83)		(3.65)		(2.37)
Observations	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033	65,103	65,103	65,033	65,033
R-squared	0.438	0.441	0.136	0.139	0.438	0.439	0.136	0.138	0.438	0.442	0.136	0.138
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.4: Urban poverty, COVID-19 and mobility restriction policies

Note: Based on authors' elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). Estimates are based on urban counties. Robust standard errors are clustered at the MSA level. Significance levels: * = 10% and ** = 5%.

at the county level and urban poverty. Table 2.4 reports estimates of the relevant coefficients from Model (2.6). The data cover daily new COVID-19 cases over the entire period from February 2020 to April 29, 2020. During this period, mobility restriction policies were gradually introduced in American counties. Models 1-4 look at new cases (1, 2) and the speed of growth of new cases (3, 4), relating variability in COVID-19 incidence to urban poverty and to the introduction of stay-at-home orders 10 days ahead of April 29. Models 5-12 report robustness checks when data on COVID-19 cases are merged with information on stay-at-home orders implemented 7 days (models 5-8) or 14 days (models 9-12) ahead. Models 1-4 provide further evidence of the role of urban poverty in rising COVID-19 incidence, which hampers the scope for stay-at-home orders to reduce the incidence of new

COVID-19 cases onset.

All models' estimates reveal that the introduction of stay-at-home orders has had no significant effect on the surge of COVID-19 cases in American cities. Columns 2, 6, and 10 also reveal that the lockdown policies contributed to the rise in COVID-19 incidence in urban counties that are located in cities characterized by higher levels of urban poverty: for instance, higher urban poverty as measured by $UP(., 0.2)$ rises the number of cases testing positive to COVID-19 by 14.48 cases per 100k residents in places that have introduced a stay-at-home order compared to places that do not, the effect being robust when urban poverty is assessed through the Gini index and its non-neighborhood component or the concentrated poverty index. We do not find evidence that lockdown policies affect the early spreading of COVID-19; instead, lockdown policies seem to have a specific positive gradient on the effect of urban poverty on COVID-19. Such a gradient has two potential explanations. First, the gradient can represent the consequences of the selection issue of lockdown policies along the lines of the degree of poverty in the county. The model specification should consider such an effect insofar that MSA fixed effects are always controlled for. A second potential explanation concerns how lockdown policies complement the drivers of urban poverty. It has been shown in Andreoli et al. (2022) that urban poverty is larger in poorer cities, with lower median rents, with higher non-owners occupancy rates, and where houses are smaller and shared by more occupants. Under these circumstances, policies that foster mobility restrictions can have larger effects on low-income households (on average larger, multi-generational families living in smaller houses) in places where those families are more concentrated. These families have likely higher chances of spreading the contagion. They are less vulnerable vis-à-vis the pandemic effects due, for instance, to reduced house dimensions and co-residency, or there is less widespread knowledge and access to prevention techniques in the place where residents (a problem also related to the extent of poverty concentration in the neighborhood).

2.6 Conclusion

This paper investigates the empirical relationship between urban poverty, encompassing aspects of incidence, distribution, and segregation of the poor population in cities, and the onset of the Coronavirus pandemic across American urban counties. We combine different strategies for identification. First, we exploit variability in Coronavirus spreading across American MSAs, controlling for State-specific levels. Our estimates show that a one standard deviation increase in urban poverty leads to an increment in the number of daily new COVID-19-positive cases and the rate of speed of infection by about 0.6 cases per 100k residents. This magnitude is roughly equivalent to a 10% increase in the average county-level incidence of new COVID-19 cases in high-incidence counties (see Figure 2.1). Furthermore, estimates of county fixed effects reveal that the MSAs' level of urban poverty curtails the effectiveness of mobility restrictions.

The result reveals that the distribution of poverty across the city neighborhood correlates with the insurgence of the pandemic, revealing a new dimension contributing to the double burden of concentrated poverty on people exposed to it. Our results suggest that the most relevant drivers of urban poverty, such as low rate of homeownership and hose overcrowding registered in places where poverty is more concentrated, have had a role in determining the speed at which the pandemic has evolved. From a policy perspective, our results suggest that policies addressing the drivers of urban poverty could lead to unintended spill-over effects in terms of the health outcomes of counties during a pandemic.

By contrast, mobility restrictions policies have mainly been employed to contrast the growth of new cases of COVID-19 at the onset of the pandemic. We exploit the staggered nature of the introduction of stay-at-home orders across American counties in March-April 2020 to assess the role of such policies on COVID-19 spreading. We do not detect evidence of significant effects of mobility restriction policies on COVID-19 cases. We find, nonetheless, a positive gradient of such policies on the impact of urban poverty on COVID-19 new cases registered in the data and on the speed of their growth. Our estimates are consistent with the possibility that urban poverty contributes to the growth of COVID-19 cases through the dynamic of interaction within households, which is fostered by lockdown policies.

Lastly, this paper shows the empirical relevance of the urban poverty UP, characterized by Andreoli et al. (2021), as opposed to alternative and widely adopted measures of concentrated poverty.

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Appendix

Dependent variable:	COVID-19 cases						COVID-19 speed new cases					
Measure:	#positives/100k			#positives			#positives/100k					
Time frame:	-	-	-	-	-	-	7 days	7 days	7 days	15 days	15 days	15 days
Method:	OLS			Poisson			OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Early incidence - 29 March												
H - SD units	0.47	0.44	0.38	7.17	7.02	4.73	0.43**	0.42**	0.41**	0.48*	0.46	0.50
	(0.30)	(0.29)	(0.31)	(9.02)	(9.44)	(12.07)	(0.19)	(0.19)	(0.19)	(0.28)	(0.28)	(0.31)
Δ Mobility workplaces (15-3)		3.03	11.51		-248.66*	-89.76						
		(4.51)	(16.57)		(150.66)	(346.02)						
H*Δ mobility (15 mar)	H - SD units		-2.01			-46.30						
			(3.90)			(99.79)						
Δ Mobility workplaces (8-15 mar)								0.87	7.45			
								(4.66)	(10.96)			
H*Δ (avg.) mobility (8-15 mar)									-1.57			
									(2.78)			
Δ Mobility workplaces (29 feb - 15 mar)											4.65	23.01
											(6.70)	(25.35)
H*Δ (avg.) mobility (29 feb-15 mar)												-4.50
												(6.33)
Observations	1,054	1,002	1,002	1,054	1,002	1,002	1,054	1,006	1,006	1,054	1,007	1,007
R-squared	0.544	0.557	0.557				0.446	0.459	0.459	0.533	0.545	0.545
Stay-at-home period - 13 April												
H - SD units	1.02	0.72	2.55	21.06	24.23	66.07	0.07	-0.03	3.19**	0.58	0.42	2.16
	(0.75)	(0.73)	(2.12)	(36.51)	(37.07)	(130.90)	(0.28)	(0.24)	(1.57)	(0.48)	(0.49)	(1.64)
Δ Mobility workplace (30-3)		-11.18	-30.93		-1,758.86**	-2,130.50*						
		(9.53)	(22.24)		(465.33)	(1,201.57)						
H*Δ mobility (30 mar)			4.91			97.94						
			(4.67)			(276.85)						
Δ Mobility workplaces (23-30 mar)								16.78*	-21.64			
								(8.91)	(13.93)			
H*Δ (avg.) mobility (23-30 mar)									9.70**			
									(4.88)			
Δ Mobility workplaces (16-30 mar)											-0.19	-27.70
											(5.57)	(21.62)
H*Δ (avg.) mobility (16-30 mar)												6.93
												(5.06)
Observations	1,054	1,013	1,013	1,054	1,013	1,013	1,054	1,014	1,014	1,054	1,014	1,014
R-squared	0.575	0.587	0.588				0.306	0.336	0.349	0.421	0.432	0.435
Stay-at-home period - 29 April												
H - SD units	-0.38	-0.88*	3.25	15.84	21.48	421.28*	-0.94*	-0.95	-4.50*	-1.29*	-1.69**	-2.56
	(0.80)	(0.51)	(2.48)	(54.19)	(54.76)	(221.56)	(0.56)	(0.59)	(2.44)	(0.76)	(0.65)	(2.44)
Δ Mobility workplace (15-4)		5.04	-32.26		-3,520.83**	-6,508.70**						
		(22.73)	(38.55)		(788.18)	(1,783.51)						
H*Δ mobility (15 apr)			9.48*			838.51*						
			(5.34)			(451.74)						
Δ Mobility workplaces (8-15 apr)								14.50	47.11*			
								(17.02)	(25.40)			
H*Δ (avg.) mobility (8-15 apr)									-8.28*			
									(4.67)			
Δ Mobility workplaces (31 mar - 15 apr)											33.50*	41.80
											(19.49)	(29.98)
H*Δ (avg.) mobility (31 mar-15 apr)												-2.10
												(4.79)
Observations	1,054	981	981	1,054	981	981	1,054	1,014	1,014	1,054	1,015	1,015
R-squared	0.388	0.495	0.497				0.130	0.142	0.145	0.218	0.263	0.263
Controls A) B) C) D) E)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.5: H and COVID-19

Note: Based on author's elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for A), B), C), D), E), and state-fixed effects. Estimates are based on urban counties. Robust standard errors are always clustered at the MSA level. Estimated effects have to be interpreted as variation in the number of cases per 100k residents (seven-day moving average) in the county (Models (1)-(3)) or the speed of new cases, i.e., the difference between the daily cases in a one or two weeks window (Models (7)-(12)). Models (4)-(6) report marginal effects at the average. SD stands for standard deviation units. Significance levels: * = 10% and ** = 5%.

Dependent variable:	COVID-19 cases						COVID-19 speed new cases					
Measure:	#positives/100k			#positives			#positives/100k					
Time frame:	-	-	-	-	-	-	7 days	7 days	7 days	15 days	15 days	15 days
Method:	OLS			Poisson			OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Early incidence - 29 March												
CP(.,0.2) - SD units	0.18	0.14	0.06	6.88	8.36	3.45	0.20*	0.18	0.19	0.20	0.16	0.26
	(0.19)	(0.19)	(0.22)	(7.52)	(7.93)	(9.36)	(0.12)	(0.12)	(0.12)	(0.18)	(0.18)	(0.19)
Δ Mobility workplaces (15-3)		2.91	16.58		-263.19*	60.76						
		(4.46)	(14.09)		(150.39)	(228.00)						
$CP(.,0.2)$*Δ mobility (15 mar)			-3.60			-110.98						
			(3.52)			(84.54)						
Δ Mobility workplaces (8-15 mar)								0.63	11.73			
								(4.63)	(9.64)			
$CP(.,0.2)$*Δ (avg.) mobility (8-15 mar)									-2.95			
									(2.74)			
Δ Mobility workplaces (29 feb - 15 mar)											4.44	33.12
											(6.66)	(22.46)
$CP(.,0.2)$*Δ (avg.) mobility (29 feb-15 mar)												-7.84
												(5.87)
Observations	1,054	1,002	1,002	1,054	1,002	1,002	1,054	1,006	1,006	1,054	1,007	1,007
R-squared	0.543	0.556	0.556				0.444	0.456	0.457	0.532	0.543	0.544
Stay-at-home period - 13 April												
CP(.,0.2) - SD units	0.44	0.25	-0.49	9.16	15.49	-18.76	0.05	-0.03	2.77**	0.32	0.22	1.19
	(0.38)	(0.37)	(1.47)	(29.03)	(30.14)	(105.13)	(0.22)	(0.20)	(1.23)	(0.25)	(0.25)	(1.04)
Δ Mobility workplace (30-3)		-11.99	-5.12		-1,765.56**	-1,507.79						
		(10.00)	(17.52)		(464.83)	(957.99)						
$CP(.,0.2)$*Δ mobility (30 mar)			-1.94			-75.06						
			(3.55)			(220.41)						
Δ Mobility workplaces (23-30 mar)								16.78*	-12.09			
								(8.94)	(8.98)			
$CP(.,0.2)$*Δ (avg.) mobility (23-30 mar)									8.31**			
									(3.87)			
Δ Mobility workplaces (16-30 mar)											-0.59	-14.13
											(5.73)	(14.62)
$CP(.,0.2)$*Δ (avg.) mobility (16-30 mar)												3.88
												(3.79)
Observations	1,054	1,013	1,013	1,054	1,013	1,013	1,054	1,014	1,014	1,054	1,014	1,014
R-squared	0.573	0.586	0.586				0.306	0.336	0.348	0.419	0.431	0.433
Stay-at-home period - 29 April												
CP(.,0.2) - SD units	-0.58	-0.92*	-1.35	0.94	13.61	140.78	-0.81*	-0.84*	-4.69*	-1.05*	-1.26**	-2.22
	(0.63)	(0.55)	(3.06)	(45.64)	(48.03)	(211.63)	(0.44)	(0.45)	(2.50)	(0.58)	(0.53)	(2.78)
Δ Mobility workplace (15-4)		5.33	8.80		-3,520.03**	-4,376.46**						
		(22.68)	(40.70)		(789.16)	(1,731.42)						
$CP(.,0.2)$*Δ mobility (15 apr)			-0.98			254.24						
			(6.01)			(410.60)						
Δ Mobility workplaces (8-15 apr)								14.84	46.27*			
								(16.93)	(27.50)			
$CP(.,0.2)$*Δ (avg.) mobility (8-15 apr)									-8.90*			
									(4.89)			
Δ Mobility workplaces (31 mar - 15 apr)											34.26*	42.30
											(19.40)	(34.43)
$CP(.,0.2)$*Δ (avg.) mobility (31 mar-15 apr)												-2.29
												(5.63)
Observations	1,054	981	981	1,054	981	981	1,054	1,014	1,014	1,054	1,015	1,015
R-squared	0.389	0.496	0.496				0.130	0.142	0.146	0.218	0.260	0.260
Controls A) B) C) D) E)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.6: CP(., 0.2) and COVID-19

Note: Based on author's elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for A), B), C), D), E), and state-fixed effects. Estimates are based on urban counties. Robust standard errors are always clustered at the MSA level. Estimated effects have to be interpreted as variation in the number of cases per 100k residents (seven-day moving average) in the county (Models (1)-(3)) or the speed of new cases, i.e., the difference between the daily cases in a one or two weeks window (Models (7)-(12)). Models (4)-(6) report marginal effects at the average. SD stands for standard deviation units. Significance levels: * = 10% and ** = 5%.

Dependent variable:			COVID-19 cases							COVID-19 speed new cases		
Measure:		#positives/100k			#positives					#positives/100k		
Time frame:	-	-	-	-	-	-	7 days	7 days	7 days	15 days	15 days	15 days
Method:		OLS			Poisson					OLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Early incidence - 29 March												
G - SD units	-0.40	-0.33	-0.31	14.95	19.46*	13.78	-0.19	-0.16	-0.16	-0.36	-0.30	-0.25
	(0.30)	(0.31)	(0.30)	(10.46)	(10.95)	(11.27)	(0.20)	(0.20)	(0.21)	(0.29)	(0.29)	(0.31)
Δ Mobility workplaces (15-3)		2.80	-6.22		-284.72**	1,422.90						
		(4.44)	(31.21)		(143.84)	(917.66)						
G*Δ mobility (15 mar)			1.61			-305.25*						
			(5.46)			(171.50)						
Δ Mobility workplaces (8-15 mar)								0.80	4.34			
								(4.65)	(25.52)			
G*Δ (avg.) mobility (8-15 mar)									-0.63			
									(4.30)			
Δ Mobility workplaces (29 feb - 15 mar)											5.05	26.61
											(6.71)	(45.00)
G*Δ (avg.) mobility (29 feb-15 mar)												-3.84
												(7.55)
Observations	1,054	1,002	1,002	1,054	1,002	1,002	1,054	1,006	1,006	1,054	1,007	1,007
R-squared	0.543	0.556	0.556				0.443	0.456	0.456	0.532	0.544	0.544
Stay-at-home period - 13 April												
G - SD units	-0.08	0.05	-3.12**	28.83	28.85	-56.15	0.53*	0.81**	0.18	0.32	0.50	-0.23
	(0.55)	(0.54)	(1.58)	(52.97)	(54.91)	(95.59)	(0.27)	(0.29)	(0.88)	(0.36)	(0.36)	(0.78)
Δ Mobility workplace (30-3)		-12.26	37.13		-1,730.69**	-597.85						
		(9.96)	(26.02)		(455.19)	(1,564.88)						
G*Δ mobility (30 mar)			-8.96**			-202.58						
			(4.05)			(249.21)						
Δ Mobility workplaces (23-30 mar)								18.68**	29.81			
								(9.38)	(24.02)			
G*Δ (avg.) mobility (23-30 mar)									-2.01			
									(3.10)			
Δ Mobility workplaces (16-30 mar)											0.16	18.23
											(5.80)	(16.72)
G*Δ (avg.) mobility (16-30 mar)												-3.23
												(3.07)
Observations	1,054	1,013	1,013	1,054	1,013	1,013	1,054	1,014	1,014	1,054	1,014	1,014
R-squared	0.573	0.586	0.588				0.308	0.343	0.343	0.419	0.432	0.433
Stay-at-home period - 29 April												
G - SD units	-1.21	-0.99	-9.13**	12.12	13.44	-405.41	-0.46	-0.50	-0.73	-1.32	-0.93	-2.48
	(0.78)	(0.72)	(4.53)	(72.26)	(76.32)	(248.29)	(0.56)	(0.54)	(3.43)	(0.83)	(0.75)	(4.12)
Δ Mobility workplace (15-4)		4.98	114.92*		-3,476.57**	1,730.49						
		(22.48)	(67.54)		(772.28)	(3,412.86)						
G*Δ mobility (15 apr)			-19.44**			-885.99*						
			(9.48)			(517.15)						
Δ Mobility workplaces (8-15 apr)								15.17	18.30			
								(16.68)	(54.26)			
G*Δ (avg.) mobility (8-15 apr)									-0.56			
									(7.36)			
Δ Mobility workplaces (31 mar - 15 apr)											34.49*	56.32
											(19.07)	(66.58)
G*Δ (avg.) mobility (31 mar-15 apr)												-3.92
												(8.99)
Observations	1,054	981	981	1,054	981	981	1,054	1,014	1,014	1,054	1,015	1,015
R-squared	0.390	0.495	0.504				0.127	0.139	0.139	0.218	0.255	0.255
Controls A) B) C) D) E)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.7: G and COVID-19

Note: Based on author's elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for A), B), C), D), E), and state-fixed effects. Estimates are based on urban counties. Robust standard errors are always clustered at the MSA level. Estimated effects have to be interpreted as variation in the number of cases per 100k residents (seven-day moving average) in the county (Models (1)-(3)) or the speed of new cases, i.e., the difference between the daily cases in a one or two weeks window (Models (7)-(12)). Models (4)-(6) report marginal effects at the average. SD stands for standard deviation units. Significance levels: * = 10% and ** = 5%.

Dependent variable:	COVID-19 cases						COVID-19 speed new cases					
Measure:	#positives/100k			#positives			#positives/100k					
Time frame:	-	-	-	-	-	-	7 days	7 days	7 days	15 days	15 days	15 days
Method:	OLS			Poisson			OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Early incidence - 29 March												
G_N - SD units	0.55*	0.58*	0.32	-3.68	-3.15	1.46	0.37*	0.37*	0.39*	0.54*	0.56*	0.87**
	(0.31)	(0.32)	(0.40)	(7.12)	(7.42)	(8.84)	(0.20)	(0.21)	(0.20)	(0.30)	(0.31)	(0.30)
Δ Mobility workplaces (15-3)		3.51	22.44*		-247.57*	-524.75**						
		(4.47)	(13.07)		(149.73)	(247.65)						
G_N*Δ mobility (15 mar)			-10.13			165.92						
			(6.18)			(114.62)						
Δ Mobility workplaces (8-15 mar)								1.24	17.52**			
								(4.73)	(8.14)			
G_N*Δ (avg.) mobility (8-15 mar)									-8.59*			
									(5.05)			
Δ Mobility workplaces (29 feb - 15 mar)											5.58	54.79**
											(6.75)	(22.41)
G_N*Δ (avg.) mobility (29 feb-15 mar)												-26.60**
												(11.35)
Observations	1,054	1,002	1,002	1,054	1,002	1,002	1,054	1,006	1,006	1,054	1,007	1,007
R-squared	0.545	0.558	0.562				0.446	0.459	0.462	0.535	0.546	0.553
Stay-at-home period - 13 April												
G_N - SD units	1.39**	1.41**	1.85	16.51	17.47	233.52**	-0.49	-0.43	0.62	0.74**	0.79**	1.27
	(0.57)	(0.57)	(1.64)	(28.82)	(29.11)	(99.10)	(0.51)	(0.48)	(0.88)	(0.33)	(0.33)	(1.07)
Δ Mobility workplace (30-3)		-10.76	-13.00		-1,743.59**	-2,573.25**						
		(9.69)	(13.73)		(455.48)	(623.76)						
G_N*Δ mobility (30 mar)			1.13			476.42**						
			(3.75)			(202.48)						
Δ Mobility workplaces (23-30 mar)								16.41*	10.29			
								(8.62)	(7.59)			
G_N*Δ (avg.) mobility (23-30 mar)									3.05			
									(2.89)			
Δ Mobility workplaces (16-30 mar)											-0.08	-3.71
											(5.60)	(9.29)
G_N*Δ (avg.) mobility (16-30 mar)												1.86
												(3.31)
Observations	1,054	1,013	1,013	1,054	1,013	1,013	1,054	1,014	1,014	1,054	1,014	1,014
R-squared	0.579	0.593	0.593				0.309	0.339	0.341	0.424	0.437	0.437
Stay-at-home period - 29 April												
G_N - SD units	-0.36	-0.28	3.77	13.11	16.16	416.48**	-0.68*	-0.73*	2.49	-1.50**	-1.38**	1.47
	(0.43)	(0.41)	(2.83)	(44.05)	(45.17)	(208.61)	(0.40)	(0.41)	(2.40)	(0.52)	(0.50)	(2.69)
Δ Mobility workplace (15-4)		5.70	-12.35		-3,488.40**	-4,741.98**						
		(22.91)	(17.65)		(779.34)	(903.77)						
G_N*Δ mobility (15 apr)			8.95			798.29**						
			(6.64)			(403.10)						
Δ Mobility workplaces (8-15 apr)								14.83	0.20			
								(17.02)	(14.32)			
G_N*Δ (avg.) mobility (8-15 apr)									7.27			
									(5.50)			
Δ Mobility workplaces (31 mar - 15 apr)											34.04*	20.56
											(19.58)	(13.37)
G_N*Δ (avg.) mobility (31 mar-15 apr)												6.69
												(6.61)
Observations	1,054	981	981	1,054	981	981	1,054	1,014	1,014	1,054	1,015	1,015
R-squared	0.388	0.493	0.495				0.129	0.141	0.143	0.222	0.261	0.263
Controls A) B) C) D) E)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.8: G_N and COVID-19

Note: Based on author's elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for A), B), C), D), E), and state-fixed effects. Estimates are based on urban counties. Robust standard errors are always clustered at the MSA level. Estimated effects have to be interpreted as variation in the number of cases per 100k residents (seven-day moving average) in the county (Models (1)-(3)) or the speed of new cases, i.e., the difference between the daily cases in a one or two weeks window (Models (7)-(12)). Models (4)-(6) report marginal effects at the average. SD stands for standard deviation units. Significance levels: * = 10% and ** = 5%.

Dependent variable:	COVID-19 cases						COVID-19 speed new cases					
Measure:	#positives/100k			#positives			#positives/100k					
Time frame:	-	-	-	-	-	-	7 days	7 days	7 days	15 days	15 days	15 days
Method:	OLS			Poisson			OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Early incidence - 29 March												
G_nN - SD units	-1.05**	-1.04**	-0.81*	9.17	10.02	3.81	-0.65**	-0.63**	-0.68**	-1.01**	-0.99**	-1.30**
	(0.40)	(0.40)	(0.44)	(7.70)	(8.01)	(9.42)	(0.27)	(0.27)	(0.26)	(0.38)	(0.39)	(0.40)
Δ Mobility workplaces (15-3)		3.55	-17.69		-253.45*	209.81						
		(4.45)	(13.33)		(148.59)	(260.67)						
G_nN*Δ mobility (15 mar)			10.48			-212.03*						
			(6.56)			(118.71)						
Δ Mobility workplaces (8-15 mar)								1.61	-15.25			
								(4.72)	(13.34)			
G_nN*Δ (avg.) mobility (8-15 mar)									8.30			
									(5.08)			
Δ Mobility workplaces (29 feb - 15 mar)											7.03	-37.18*
											(6.87)	(21.46)
G_nN*Δ (avg.) mobility (29 feb-15 mar)												21.58**
												(9.83)
Observations	1,054	1,002	1,002	1,054	1,002	1,002	1,054	1,006	1,006	1,054	1,007	1,007
R-squared	0.549	0.561	0.565				0.448	0.461	0.463	0.538	0.549	0.554
Stay-at-home period - 13 April												
G_nN - SD units	-2.00**	-1.92**	-4.38**	-8.14	-9.32	-205.77**	1.06	1.15	0.25	-0.82**	-0.76**	-1.41
	(0.70)	(0.70)	(1.54)	(37.47)	(38.30)	(86.93)	(0.74)	(0.72)	(1.00)	(0.36)	(0.37)	(0.96)
Δ Mobility workplace (30-3)		-13.17	-1.73		-1,743.95**	-832.57						
		(10.13)	(11.50)		(459.71)	(585.92)						
G_nN*Δ mobility (30 mar)			-6.27*			-431.43**						
			(3.53)			(172.04)						
Δ Mobility workplaces (23-30 mar)								17.58*	22.27			
								(9.25)	(13.92)			
G_nN*Δ (avg.) mobility (23-30 mar)									-2.59			
									(3.53)			
Δ Mobility workplaces (16-30 mar)											-1.44	3.39
											(5.80)	(6.67)
G_nN*Δ (avg.) mobility (16-30 mar)												-2.53
												(3.05)
Observations	1,054	1,013	1,013	1,054	1,013	1,013	1,054	1,014	1,014	1,054	1,014	1,014
R-squared	0.581	0.593	0.595				0.315	0.348	0.349	0.422	0.434	0.435
Stay-at-home period - 29 April												
G_nN - SD units	-0.34	-0.30	-9.04*	-10.00	-12.91	-478.42**	0.63	0.67	-2.46	1.17*	1.28**	-2.51
	(0.50)	(0.42)	(4.99)	(54.21)	(55.29)	(193.00)	(0.48)	(0.49)	(3.81)	(0.62)	(0.58)	(4.61)
Δ Mobility workplace (15-4)		6.15	43.16		-3,487.38**	-1,225.16						
		(23.11)	(38.65)		(780.71)	(1,389.34)						
G_nN*Δ mobility (15 apr)			-19.30*			-927.97**						
			(10.89)			(380.30)						
Δ Mobility workplaces (8-15 apr)								15.68	28.96			
								(17.26)	(29.89)			
G_nN*Δ (avg.) mobility (8-15 apr)									-7.03			
									(8.21)			
Δ Mobility workplaces (31 mar - 15 apr)											35.85*	52.23
											(20.07)	(37.44)
G_nN*Δ (avg.) mobility (31 mar-15 apr)												-8.83
												(10.43)
Observations	1,054	981	981	1,054	981	981	1,054	1,014	1,014	1,054	1,015	1,015
R-squared	0.388	0.493	0.504				0.128	0.140	0.142	0.216	0.257	0.260
Controls A) B) C) D) E)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.9: G_{nN} and COVID-19

Note: Based on author's elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for A), B), C), D), E), and state-fixed effects. Estimates are based on urban counties. Robust standard errors are always clustered at the MSA level. Estimated effects have to be interpreted as variation in the number of cases per 100k residents (seven-day moving average) in the county (Models (1)-(3)) or the speed of new cases, i.e., the difference between the daily cases in a one or two weeks window (Models (7)-(12)). Models (4)-(6) report marginal effects at the average. SD stands for standard deviation units. Significance levels: * = 10% and ** = 5%.

Dependent variable:	UP (.,0.2)	Gini	Gini (neigh)	Gini (non-neigh)	CP (.,0.2)	H
Method:			OLS			
	(1)	(2)	(3)	(4)	(5)	(6)
% LIHTC units by MSA	-0.0126**	-0.0053**	0.0006	-0.0059	-0.0132*	-0.0051**
	(0.0032)	(0.0027)	(0.0060)	(0.0059)	(0.0071)	(0.0014)
A) Population size (ln)	-0.0026	0.0055**	-0.0168**	0.0223**	-0.0021	-0.0038**
	(0.0022)	(0.0016)	(0.0050)	(0.0049)	(0.0047)	(0.0009)
A) 10 larger MSA	-0.0088	-0.0270**	-0.0016	-0.0254	-0.0099	0.0063
	(0.0166)	(0.0113)	(0.0217)	(0.0228)	(0.0352)	(0.0076)
A) 5 larger MSA	-0.0182	0.0014	-0.0019	0.0033	-0.0445	-0.0156**
	(0.0191)	(0.0128)	(0.0213)	(0.0241)	(0.0429)	(0.0071)
A) % Black	0.0901	0.2045	0.2394	-0.0349	0.1995	0.0393
	(0.1048)	(0.1445)	(0.2648)	(0.3297)	(0.2282)	(0.0422)
A) % Hispanic	0.0391	0.1200	0.3095	-0.1895	0.1476	0.0540
	(0.0999)	(0.1446)	(0.2602)	(0.3311)	(0.2074)	(0.0382)
A) % Asian	0.0726	-0.0650	0.0772	-0.1422	0.4310	0.2147**
	(0.1367)	(0.1586)	(0.3279)	(0.3827)	(0.2845)	(0.0590)
A) % White	0.0463	0.1466	0.2677	-0.1211	0.1406	0.0364
	(0.1061)	(0.1448)	(0.2705)	(0.3377)	(0.2235)	(0.0378)
A) % 65 plus yrs old	1.3781	0.5516	7.6016**	-7.0500**	-0.5560	0.2205
	(2.0041)	(1.5464)	(3.5681)	(3.2845)	(4.4516)	(0.7502)
B) % Old houses (20 plus yrs old)	-0.0376	-0.0661**	0.0476	-0.1137	-0.0575	0.0056
	(0.0385)	(0.0256)	(0.0705)	(0.0695)	(0.0822)	(0.0192)
B) % Owner occupied	-0.6344**	-0.0894*	-0.3349**	0.2455*	-1.1771**	-0.2883**
	(0.0791)	(0.0495)	(0.1297)	(0.1277)	(0.1649)	(0.0351)
B) Median value house by CT (ln)	0.0186	-0.0008	0.0123	-0.0131	-0.0016	0.0104
	(0.0116)	(0.0106)	(0.0241)	(0.0226)	(0.0244)	(0.0082)
B) S.d. value house by CT (ln)	-0.0357**	-0.0244**	-0.0298	0.0054	-0.0501*	-0.0093*
	(0.0117)	(0.0102)	(0.0254)	(0.0237)	(0.0283)	(0.0048)
B) Median rent by CT (ln)	-0.0621*	0.0235	-0.0761	0.0996	-0.1384*	-0.0547**
	(0.0351)	(0.0305)	(0.0720)	(0.0680)	(0.0816)	(0.0146)
B) S.d. rent by CT (ln)	-0.0293**	-0.0178*	0.0187	-0.0365*	-0.0450*	-0.0053
	(0.0120)	(0.0092)	(0.0219)	(0.0217)	(0.0244)	(0.0051)
C) % less than high school	0.5371	-0.1583	0.0848	-0.2431	1.1968	0.3896**
	(0.3708)	(0.3033)	(0.7041)	(0.6803)	(0.7956)	(0.1758)
C) % with college	0.1744	-0.0644	0.2119	-0.2763	0.3267	0.1427**
	(0.1241)	(0.0937)	(0.2503)	(0.2378)	(0.2560)	(0.0505)
C) % Commute < 1/2 hour	-0.0091	0.2372**	0.4517**	-0.2145	-0.1420	-0.0656*
	(0.0930)	(0.0715)	(0.1630)	(0.1626)	(0.2030)	(0.0352)
C) Avg hh income (ln)	-0.2385**	0.0256	0.0526	-0.0270	-0.6275**	-0.1640**
	(0.0414)	(0.0261)	(0.0804)	(0.0763)	(0.0955)	(0.0220)
C) S.d. hh income by CT (ln)	0.2310**	0.1968**	0.1263**	0.0705**	0.3723**	0.0557**
	(0.0203)	(0.0167)	(0.0356)	(0.0340)	(0.0493)	(0.0092)
C) College Town	-0.0031	-0.0058	-0.0060	0.0002	0.0081	0.0008
	(0.0061)	(0.0047)	(0.0162)	(0.0154)	(0.0120)	(0.0021)
C) Student Town	0.0004	-0.0046	0.0163	-0.0209	0.0110	0.0042
	(0.0114)	(0.0083)	(0.0220)	(0.0224)	(0.0218)	(0.0051)
D) % health insurance among 65+	-1.4188	-0.4126	-7.7275**	7.3150**	0.1896	-0.3809
	(2.0342)	(1.5731)	(3.6153)	(3.3322)	(4.4831)	(0.7534)
D) % without health insurance	0.1249	-0.1424	0.2457	-0.3881	0.5472	0.2082**
	(0.1587)	(0.1202)	(0.2599)	(0.2473)	(0.3388)	(0.0814)
E) Dissimilarity black	-0.0248	-0.0075	-0.1040	0.0964	-0.0163	-0.0023
	(0.0408)	(0.0299)	(0.0757)	(0.0704)	(0.0879)	(0.0161)
E) Dissimilarity hispanic	0.0999**	0.0698*	0.0010	0.0687	0.2515**	0.0383
	(0.0459)	(0.0373)	(0.0793)	(0.0756)	(0.1000)	(0.0248)
E) Dissimilarity asian	0.1640**	0.0759**	0.2123**	-0.1365*	0.2521**	0.0518**
	(0.0488)	(0.0385)	(0.0831)	(0.0753)	(0.1131)	(0.0196)
Observations	1,068	1,068	1,068	1,068	1,068	1,068
R-squared	0.773	0.879	0.656	0.777	0.790	0.876

Table 2.10: 1st Stage - LIHTC program and urban poverty measures

Note: Based on authors' elaboration of data from ACS data. All models control for state-fixed effects. Robust standard errors are clustered at the MSA level. Estimates are based on counties with MSA-level variables. Significance levels: * = 10% and ** = 5%.

Dependent variable:	COVID-19 early incidence				COVID-19 stay-home period							
Measure:	#Positives/100k			#positives	#Positives/100k			#positives	#Positives/100k			#positives
Time frame:	29 Mar	22-29 Mar	15-29 Mar	29 Mar	13 Apr	6-13 Apr	30 Mar-13 Apr	13 Apr	29 Apr	22-29 Apr	15-29 Apr	29 Apr
Method:	OLS			Poisson	OLS			Poisson	OLS			Poisson
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
UP(.,0.2) - SD units	0.59**	0.55**	0.62**	26.89**	1.59**	-0.08	0.97*	89.50**	-0.54	-1.28**	-1.95**	116.46**
	(0.29)	(0.20)	(0.28)	(10.52)	(0.80)	(0.40)	(0.55)	(42.02)	(0.71)	(0.56)	(0.82)	(59.29)
A) Population size (ln)	0.01	0.15	0.01	138.97**	-0.68	-0.13	-0.67	506.28**	-1.69*	-0.07	-1.01	878.90**
	(0.34)	(0.24)	(0.32)	(9.53)	(0.65)	(0.50)	(0.43)	(32.16)	(0.87)	(0.66)	(0.82)	(56.12)
A) Pop. density (ln)	0.72**	0.28*	0.68**	9.63**	0.89*	-0.32	0.13	42.18**	0.74*	-0.04	-0.09	38.98
	(0.33)	(0.16)	(0.30)	(4.42)	(0.51)	(0.29)	(0.24)	(16.46)	(0.40)	(0.52)	(0.44)	(32.62)
A) 10 larger MSA	-1.90*	-1.40**	-1.82*	-144.17**	-5.48*	2.05	-3.04	-496.07**	1.01	1.55	4.34**	-443.63**
	(1.07)	(0.71)	(1.02)	(22.16)	(3.27)	(1.43)	(2.41)	(80.91)	(1.43)	(1.32)	(2.08)	(123.91)
A) 5 larger MSA	7.13**	4.56**	6.97**	73.59**	11.31**	-1.95	3.38	330.48**	3.49	-1.68*	-6.24**	423.36**
	(3.20)	(2.20)	(3.12)	(23.33)	(5.11)	(1.42)	(2.27)	(71.91)	(3.23)	(0.99)	(2.11)	(110.22)
A) % Black	-3.68	-3.19	-3.87	-450.01**	6.06	5.82	7.58	-1,567.40**	-1.78	-10.30	-5.05	-3,008.65**
	(5.53)	(3.71)	(5.41)	(188.65)	(11.75)	(3.73)	(7.11)	(718.54)	(15.91)	(10.24)	(13.09)	(1,167.57)
A) % Hispanic	0.44	-0.97	0.08	-408.28**	4.35	1.13	1.51	-1,092.72	-4.62	-10.97	-7.13	-2,426.42*
	(4.83)	(2.85)	(4.64)	(195.10)	(10.19)	(4.08)	(6.51)	(836.15)	(17.77)	(10.47)	(13.10)	(1,368.72)
A) % Asian	-8.38	-11.29**	-9.68	-468.86**	-12.69	8.71	-5.72	-2,000.22**	-5.91	-7.26	10.62	-3,888.18**
	(6.33)	(4.71)	(6.09)	(208.87)	(11.79)	(5.96)	(7.34)	(904.91)	(18.68)	(13.57)	(13.77)	(1,439.84)
A) % White	-8.91	-7.69**	-9.20	-700.85**	-12.13	3.08	-4.10	-2,450.59**	-22.37	-15.58	-9.56	-4,458.21**
	(5.73)	(3.82)	(5.73)	(173.45)	(10.42)	(3.24)	(5.48)	(736.52)	(16.87)	(10.29)	(13.45)	(1,202.61)
A) % 65 plus yrs old	-7.56	4.82	-8.77	4,730.17**	53.57	55.77	73.26	3,391.77	55.58	-239.58	1.24	10,390.09
	(51.98)	(40.77)	(53.13)	(2,153.42)	(126.62)	(79.99)	(88.35)	(7,644.00)	(259.59)	(165.43)	(227.38)	(15,327.29)
B) % Old houses (20 plus yrs old)	6.61**	5.00**	6.42**	75.09*	11.31**	-4.10	4.51**	581.16**	4.78	-1.52	-4.63	942.54**
	(2.69)	(2.28)	(2.72)	(38.75)	(4.01)	(2.85)	(1.97)	(152.64)	(4.19)	(3.13)	(3.14)	(312.75)
B) % Owner occupied	-8.65**	-0.34	-6.82*	392.39**	-2.64	-1.70	8.49	1,413.62**	16.72*	17.26**	16.22*	2,215.26**
	(3.10)	(4.21)	(3.55)	(60.13)	(9.75)	(3.96)	(7.00)	(228.44)	(9.36)	(8.10)	(8.89)	(389.30)
B) Median value house by CT (ln)	5.48**	2.42**	5.12**	87.54**	5.47*	-1.38	-0.43	145.43*	3.40	0.52	-1.46	186.08
	(2.53)	(1.09)	(2.38)	(26.79)	(2.85)	(1.07)	(1.14)	(84.00)	(2.88)	(2.33)	(2.85)	(137.79)
B) S.d. value house by CT (ln)	0.28	0.27	0.27	2.49	0.34	0.02	0.26	-8.50	-2.44	-1.93	-2.72	-24.16
	(0.26)	(0.17)	(0.25)	(15.60)	(0.52)	(0.45)	(0.41)	(63.14)	(2.39)	(2.35)	(2.24)	(121.09)
B) Median rent by CT (ln)	-0.32	-0.54	-0.18	303.22**	2.01	-2.22	2.69	737.15**	2.09	-2.25	-0.73	948.14**
	(1.31)	(1.05)	(1.30)	(59.93)	(3.10)	(2.15)	(2.53)	(172.10)	(4.26)	(3.44)	(3.64)	(324.02)
B) S.d. rent by CT (ln)	0.16	0.17	0.16	-71.78**	0.38	0.05	0.15	-15.51	0.72	0.34	0.41	13.87
	(0.29)	(0.20)	(0.28)	(18.56)	(0.55)	(0.37)	(0.40)	(62.41)	(0.70)	(0.57)	(0.64)	(121.27)
C) % less than high school	-0.10	-0.17	-0.12	-8.55	0.11	0.35*	0.23	-30.23	1.63**	0.92**	1.35**	3.99
	(0.15)	(0.13)	(0.15)	(10.24)	(0.35)	(0.20)	(0.24)	(37.22)	(0.71)	(0.45)	(0.64)	(61.19)
C) % with college	-0.22	-0.13	-0.21	-1.16	-0.29*	0.00	-0.06	-9.72	-0.08	0.17	0.18	-17.92
	(0.14)	(0.08)	(0.14)	(2.60)	(0.16)	(0.07)	(0.05)	(8.75)	(0.15)	(0.13)	(0.14)	(14.27)
C) % Commute $< 1/2$ hour	-0.21**	-0.07**	-0.18**	-3.10**	-0.27**	-0.00	-0.04	-4.82	0.10	0.31**	0.35**	-2.42
	(0.06)	(0.03)	(0.05)	(0.92)	(0.07)	(0.04)	(0.08)	(4.08)	(0.17)	(0.13)	(0.16)	(8.16)
C) Median hh income by CT (ln)	-5.23*	-2.02	-5.03	-410.31**	-7.13	3.96*	-1.81	-926.03**	-5.04	1.30	2.19	-1,178.89**
	(3.13)	(1.86)	(3.07)	(73.76)	(4.35)	(2.32)	(2.79)	(177.53)	(3.64)	(3.48)	(3.21)	(305.48)
C) Avg hh income (ln)	8.61**	5.31**	8.25**	140.06**	11.78**	0.38	2.63	463.88**	4.23	-5.03**	-5.53**	742.88**
	(3.10)	(2.13)	(2.96)	(44.21)	(3.28)	(1.58)	(2.09)	(141.41)	(2.90)	(2.44)	(2.60)	(266.04)
C) S.d. hh income by CT (ln)	-0.40	-0.12	-0.35	139.74**	-0.31	-0.00	0.04	203.47*	2.81	1.16	2.81	304.53**
	(0.37)	(0.38)	(0.37)	(51.69)	(0.71)	(0.46)	(0.50)	(103.93)	(2.12)	(2.01)	(2.04)	(142.02)
C) College Town	0.33	0.36	0.32	36.66**	1.10	-1.87	0.73	124.20**	0.48	-0.94	-0.56	118.44
	(0.97)	(0.63)	(0.94)	(11.09)	(1.49)	(1.37)	(0.66)	(44.88)	(1.23)	(0.95)	(1.01)	(78.06)
C) Student Town	0.75	0.79	0.70	83.62**	4.79**	0.90	3.76**	363.29**	3.23**	0.77	-0.53	456.10**
	(0.84)	(0.54)	(0.81)	(22.18)	(2.31)	(0.85)	(1.62)	(85.76)	(1.32)	(0.88)	(1.48)	(127.28)
D) % health insurance among 65+	0.34	0.11	0.34	-43.51**	-0.28	-0.56	-0.77	-23.86	-0.51	2.30	-0.20	-90.25
	(0.48)	(0.39)	(0.49)	(22.10)	(1.24)	(0.80)	(0.90)	(78.53)	(2.64)	(1.67)	(2.31)	(160.63)
D) % without health insurance	-0.14	0.02	-0.12	1.47	-0.08	0.06	0.07	13.02	-0.05	0.03	0.09	19.09
	(0.10)	(0.07)	(0.10)	(1.54)	(0.19)	(0.07)	(0.15)	(8.45)	(0.19)	(0.14)	(0.17)	(17.22)
E) Dissimilarity black	-6.30	-3.16	-5.90	-191.49**	-11.82**	-5.06	-5.32*	-1,239.71**	-6.60	-0.27	3.23	-1,878.75**
	(4.15)	(2.79)	(4.01)	(84.44)	(5.87)	(4.31)	(2.78)	(323.61)	(5.86)	(3.79)	(4.89)	(527.28)
E) Dissimilarity hispanic	-5.32	-3.43	-5.12	7.62	-1.88	8.09**	3.31	424.30	19.05**	9.39	19.28**	1,141.42*
	(3.82)	(2.45)	(3.68)	(98.86)	(6.30)	(3.53)	(3.87)	(408.33)	(7.29)	(5.84)	(7.55)	(635.29)
E) Dissimilarity asian	1.97	-0.40	1.48	-115.43	2.13	3.21	0.43	-115.71	1.05	1.88	0.27	-73.71
	(3.06)	(2.06)	(2.93)	(115.92)	(5.25)	(3.14)	(3.20)	(437.72)	(5.70)	(5.12)	(5.60)	(701.86)
Observations	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054
R-squared	0.545	0.448	0.534		0.579	0.306	0.425		0.388	0.133	0.224	

Table 2.11: 2nd Stage - UP(.,0.2) and COVID-19

Note: Based on authors' elaboration of data from ACS data. COVID-19 cases are from the Economic Tracker, which refers to New York Times COVID-19 repository (data extracted on October 31, 2020). All models control for state-fixed effects. Estimates are based on urban counties. First-stage estimates are obtained with MSA-level variables. Robust standard errors are clustered at the MSA level. Standard errors are adjusted using Topel and Murphy's SE adjustment in all models, except (4)-(8)-(12). The estimated effect has to be interpreted as variations in the county number of cases per 100k residents expressed as a seven-day moving average (Models (1), (5) and (9)) or the speed of new cases, i.e., the difference between the daily cases in a one or two weeks window (Models (2)-(3), (6)-(7) and (10)-(11)). Models (4)-(8)-(12) report marginal effects at the average. SD stands for standard deviation units. Significance levels: * = 10% and ** = 5%.

The pandemic's toll on domestic violence: Investigating the effect of COVID-19 public health measures

Chapter 3

The pandemic's toll on domestic violence: Investigating the effect of COVID-19 public health measures

3.1 Introduction

Being at home does not always mean being at peace and safe. Particular power dynamics persist and are distorted by the abusers within a household, sheltered from the outside world, resulting in many acts of physical, sexual, and psychological violence. Women are the primary victims of these dynamics (World Health Organization, 2012), with one in three to one in four women having at least one day experienced domestic violence (Truman & Morgan, 2014; World Health Organization, 2013).

Domestic violence heavily impacts victims' and their children's decisions and opportunities (Borker, 2021). Victims suffer from physical and psychological injuries, including depression and post-traumatic stress (Truman & Morgan, 2014), sometimes leading to suicide (Devries et al., 2011; World Health Organization, 2013). Victims also experience high social and economic costs (Fearon & Hoeffler, 2014), with 21-60% losing their jobs for causes directly or indirectly related to the abuse they experience at home (Truman & Morgan, 2014). Additionally, victims' children are more neglected or experience abuse (Gage & Silvestre, 2010; Kimber et al., 2018). Domestic violence results in short- and long-term repercussions for victims' children (Aizer, 2011; Fantuzzo et al., 1997; Holt et al., 2008; Kitzmann et al., 2003; Mullender et al., 2002; Sternberg et al., 1993), leading even them to repeat the cycle of violence into adulthood by incubating abusers (Gage & Silvestre, 2010). Children who witness violence are also more likely to become victims of domestic violence and suffer from psychological and behavioral disorders (Holt et al., 2008; Kitzmann et al., 2003; Sternberg et al., 1993).

The COVID-19 pandemic may have further clouded the picture. Public health measures introduced by authorities have promoted social distancing and the reduction of contacts external to households to prevent the spread of the virus. Millions of workers lost their jobs during the health crisis or saw their incomes decline (Coibion et al., 2020), while the demand for new workers also declined during the health crisis (Forsythe et al., 2020). Besides, approximately 35% of workers in the United States began working from home in the initial weeks of the pandemic (Brynjolfsson et al., 2020; Dingel & Neiman, 2020; Kogan et al., 2020), overlapping with the closure of schools in favor of distance learning, simultaneously increasing parental time. These factors coincide with economic and health uncertainty, emotional stress, and alcohol and substance abuse patterns, known to affect people's psychological state (Galea et al., 2020; Pfefferbaum & North, 2020) and to foster domestic violence (Aizer, 2010; Aizer & Dal Bo, 2009; Anderberg et al., 2016; Card & Dahl, 2011).

As a result, lockdowns and associated policies have often forced victims and abusers to confine themselves together for long weeks. In regular times, victims typically wait for the abuser to leave the scene before contacting the police (Campbell, 2020). Victims, trapped by the public health measures, were thus left with few options for escaping their abusive situation during the pandemic (Aizer, 2010; Anderberg et al., 2016; Bhalotra et al., 2019) and had reduced opportunities to seek external help or to get out of the hands of their abusers. Besides, the mere fear of catching the virus and the miscommunication about the services for victims were exploited by the abusers (Leigh et al., 2022), increasing victims' loneliness and lack of support. Victims were consequently cornered alongside their abusers.

However, the existing studies analyzing the impact of the pandemic on domestic violence present mixed outcomes. Some analyses, mainly using hotlines data, show a significant increase in domestic violence at the beginning of the crisis (e.g., Arenas-Arroyo et al., 2020; Leslie and Wilson, 2020; Perez-Vincent et al., 2020; Ravindran and Shah, 2020; Sanga and McCrary, 2020; Silverio-Murillo et al., 2020). The emergency calls from women affected by domestic violence went up by 60% in several EU countries (Mahase, 2020). The increases were primarily transient for the first two to five weeks following the introduction of the first lockdown (Leslie & Wilson, 2020; Piquero et al., 2020). Moreover, increases in calls were mainly driven by couples with no prior history of domestic violence (Bullinger et al., 2020).

In contrast, other studies relying on police data suggest modest decreases or no effect resulting from public health measures (e.g., Bullinger et al., 2020; Campedelli et al., 2021; Miller et al., 2020; Perez-Vincent et al., 2020; Piquero et al., 2021; Ravindran and Shah, 2020; Silverio-Murillo et al., 2020). The data may at least partially explain the discrepancies between studies (Berniell & Facchini, 2021). Numerous domestic violence incidents already remain unknown to authorities in regular times (Aizer, 2011) due to the social desirability issue associated with domestic violence (Podaná et al., 2010). During lockdowns, quantifying domestic violence proves even more delicate because of exacerbated communication difficulties, household bargaining power balances, and fear of contracting the virus (Silverio-Murillo et al., 2020). Police reports, therefore, often illustrate the tip of the iceberg, failing to reveal the vast submerged part of domestic violence that remains hidden. Additionally, countries suffer from a lack of comparable data (Jayachandran, 2015) because authorities' encoding of crime data varies within and between countries.

Based on these observations, this paper aims to (1) estimate the impact of COVID-19 public health measures on domestic violence using Google users' trace data as proxies for domestic violence. Then, the study examines

(2) how the impact of public health measures evolves. Finally, the paper (3) tentatively investigates the role of several factors, such as governmental economic support and individuals' compliance with measures, in mitigating or exacerbating the effect of the measures.

I use weekly domestic violence-related Google searches performed in 31 countries between January 2017 and December 2021 as proxies of domestic violence incidents. I combine these data with the Oxford Covid-19 Government Response Tracker to exploit the fine-grained timing and intensity of COVID-19 public health measures across countries. Then, I perform a linear regression panel model with fixed effects accounting for country specificities and seasonal patterns. I also complement the analysis with an event study model to analyze the patterns of domestic violence-related Google searches following the first introduction of public health measures. As a preview of the findings, Figure 3.1(b) depicts a high correlation between measures and changes in domestic violence. I return to details in the following sections.

The identification strategy relies on the exogeneity of public health measures when including controls. Although the associations do not prove causality, domestic violence should not significantly influence the severity of the public health measures (Aknin et al., 2022), which allows for ruling out most likely the presence of reverse causality. Also, the richness of the fine-grained timing of the data, the inclusion of temporal and geographical fixed effects (controlling in particular for the potential presence of confounders linked to the pandemic), and the battery of robustness checks carried out further support the interpretation of the results. In particular, including relative cases and deaths in the robustness checks does not alter the estimated effect of public health measures.

The digital traces left by users when searching for information on the Internet are a valuable source of information, giving rise to the term "infodemiology" (Eysenbach et al., 2009). Digital traces reflect concerns people experience (Anderberg et al., 2022; Lazer et al., 2009). Many researchers already use Google data for estimating migrations (Alexander et al., 2020; Zagheni & Weber, 2012; Zagheni et al., 2017), employment (Baker & Fradkin, 2017) or biological phenomena (Delpierre & Kelly-Irving, 2018; Ojala et al., 2017; Rampazzo et al., 2018) and the evolution of outbreaks such as influenza (Ginsberg et al., 2009) or salmonella (Brownstein et al., 2009).

Google data are potent predictors of domestic violence incidents (Anderberg et al., 2022). The digital traces limit the underreporting issue due to the phenomenon of social desirability (Ertan et al., 2020) while providing greater anonymity for victims seeking help. Google data also likely attenuate the exacerbated underreporting problem attributable to public health measures disrupting external contact opportunities (Riddell et al., 2022). Google searches data are thus powerful alternatives when obtaining reliable data through traditional sources such as surveys is complex (e.g., see Lippi and Cervellin, 2019; Stephens-Davidowitz, 2014) and suffers from a lack of systematic collection (Anderberg et al., 2022). Köksal et al. (2022) conclude that Google searches on domestic violence are potent tools for tracking the evolution of phenomena in times of crisis, seen as proxies of latent constructs (Baldwin & Mussweiler, 2018; Bento et al., 2020), allowing nowcasting and forecasting policy outcomes (Alexander et al., 2020).

This paper contributes to the burgeoning topic of the impact of COVID-19 on the incidence of domestic violence. This paper differs from most other studies by exploiting the heterogeneity of public health measures across 31 countries. Moreover, the data avoid underreporting issues (Miller et al., 2020; Silverio-Murillo et al.,

2020) and offer comparability across different countries. Finally, the approach allows empirically validating the existing studies by extending the analyzed COVID-19 period and determining how the effect of public health measures evolved over the following months and years.

The results suggest that public health measures increase domestic violence. A tremendous increase in domestic violence occurs two weeks following the first introduction of lockdown measures. The effect then slowly fades over time, suggesting long-lasting effects. Nevertheless, successive variations in the stringency of the measures continue to affect domestic violence more than a year after the first introduction of public health measures. Finally, the effect of the measures seems plausibly exacerbated by economic support policies and partially mitigated by individuals' compliance with these measures. The significant increase in domestic violence is thus an additional cost caused by the pandemic, inducing a "shadow pandemic".

3.2 Economics behind domestic violence

The complexity of the factors influencing domestic violence during the COVID-19 pandemic is undeniable. Multiple channels contribute to the incidence of domestic violence, with some related to structural factors such as cultural, social, or economic environments (Jewkes et al., 2002). Other factors, known as situational factors, lend themselves to the COVID-19 pandemic, including economic shocks (Aizer, 2010; Anderberg et al., 2016; Buller et al., 2018), increased exposure to perpetrators due to confinement measures (Dugan et al., 1999), and mental health issues, such as anxiety, stress, and depression (Card & Dahl, 2011). These situational factors are known to exacerbate the risk of domestic violence.

Economic theories, such as the exposure reduction theory (Dugan et al., 1999) and intrahousehold bargaining power theory (La Mattina, 2017), have been put forward to explain the patterns of domestic violence during the pandemic (e.g., see Henke and Hsu, 2022). As put forth by (Dugan et al., 1999), the exposure reduction theory posits that domestic violence incidents may increase under stringent public health measures due to the increased opportunity for perpetrators to commit the crime. The theory suggests that the longer the perpetrator's exposure to the opportunity to commit a crime, the greater the likelihood of its occurrence. The current pandemic has highlighted this through efforts to curb the spread of the virus, which have led to increased confinement of victims with their abusers, thus providing more opportunities for domestic violence to occur (Chin, 2012; Henke & Hsu, 2022).

Another relevant candidate theory explaining domestic violence patterns during the pandemic is the intrahousehold bargaining power theory (La Mattina, 2017). The abuser derives some utility from committing acts of violence to alleviate frustration (Card & Dahl, 2011). The theory also posits that the abuser is willing to "pay" the victim with intrahousehold transfers in exchange for committing the crime. The level of intrahousehold violence is determined by the abuser's preference for violence and the bargaining power of the victim, who may use the threat of leaving the household as leverage. This theory highlights the crucial role of gender inequality in shaping the intrahousehold dynamics that contribute to domestic violence (Alon et al., 2020; Nguyen et al., 2021).

The evolution of an abuser's preference for committing acts of violence is influenced by various factors, including emotional cues such as job loss caused by crises (Card & Dahl, 2011). The restrictive COVID-19 public health measures have likely affected the underlying factors contributing to the incidence of domestic violence, as described by the exposure reduction theory and the intrahousehold bargaining power theory. On the one hand, the pandemic may have altered the balance of bargaining power within households, notably with abusive partners potentially experiencing temporary job loss and an increase in the importance of intrahousehold daycare work, likely performed by victims. This could result in additional transfers from the abuser to the victim to restore balance within the household. On the other hand, abusers' appetite for violence and victims' exposure to violence may have changed because of the social distancing public measures imposed to limit the spread of the virus. As such, the overall impact of the pandemic on domestic violence remains uncertain.

In addition to the theories above, other contrasting perspectives may help understand domestic violence during the pandemic. The male backlash and instrumental violence theories suggest that increasing victims' intrahousehold bargaining power could paradoxically increase the likelihood of experiencing violence. As the abuser may feel powerless when facing victims' increasing bargaining power, they may attempt to compensate for their relative loss of power by perpetrating violence (Angelucci & Heath, 2020; Chin, 2012). A non-employed abuser may perceive a potential victim's employment as a threat to their control (Alonso-Borrego & Carrasco, 2017; Anderberg et al., 2016; Caetano et al., 2008) and employ violence to re-establish control over the victim and prevent them from leaving the household (Henke & Hsu, 2022).

3.3 Data

The study period for this research covers a span of five years, from January 2017 to December 2021, and includes data from 31 countries. The baseline regressions combine domestic violence-related Google searches as proxies for domestic violence incidents and COVID-19 public health measures. Robustness checks also include GPS-based mobility data and control variables such as COVID-19 weekly new cases. The pandemic hit the 31 countries hardly, and government responses varied in timing and strictness of public health measures, providing a rich set of variations for analysis.

3.3.1 Domestic violence

To gauge actual domestic violence incidents, I use weekly domestic violence-related search data from 31 countries with a significant Internet penetration (World Bank, 2020) between January 2017 and December 2021. The list of countries is provided in Appendix in Table 3.4. These data come from digital traces left by Internet users when they perform domestic violence-related searches on Google. I extract the data with the private Google Trends API. Google Trends is a tool tracking the popularity of a keyword or a topic within a geographical area during a given period. In the present study, the data are extracted by topic, allowing Google to automatically select a wide range of multilingual domestic violence-related searches, including derivatives of keywords and spelling mistakes. The data represent the fraction of query-related searches relative to the overall mass of Google searches. Google's algorithm then reports the normalized frequency of searches related to the topic on a scale from 0 to 100.[19]

Several researchers already consider domestic violence-related searches from Google as proxies of domestic violence incidents (Baldwin & Mussweiler, 2018; Bento et al., 2020). Additionally, domestic violence-related searches closely match helplines or police data patterns in Spain (Berniell & Facchini, 2021), Finland (Koutaniemi & Einiö, 2021), London and Los Angeles (Anderberg et al., 2022), strengthening the promise of this approach for large-scale analyses during disruptive periods. Besides, the assumption does not require that the proxy comprehensively represents actual domestic violence incidents. Instead, the present analysis assumes that the proxy of domestic violence and the actual domestic violence incidents evolve symbiotically.

Google searches data are particularly salient compared to surveys or police-recorded data. Google searches often correspond to victims' first logical step (Qin et al., 2020). In regular times, less than half of domestic violence incidents are reported to the police (Morgan & Oudekerk, 2019). The COVID-19 pandemic may have further exacerbated this underreporting phenomenon. The stringency of public health measures increased victims' exposure to abusers, aggravating difficulties in reporting to authorities or acquaintances outside the household, especially because of the fear of being caught on the phone (Mahapatro et al., 2021). Still, as traditional measures, Google searches may not cover severe forms of domestic violence. Additionally, some domestic violence-related searches may be performed by people interested in the topic but not suffering from domestic violence. This phenomenon was likely exacerbated at the beginning of the pandemic when news alerted the risks associated with lockdowns (e.g., Usher et al., 2020) and is handled in robustness checks. However, Google data still present desirable solid properties, such as being available in real-time and identically collected across countries, overcoming many limitations encountered with survey-based or administrative-based data.

3.3.2 COVID-19 public health measures

I use the Oxford COVID-19 Government Response Tracker (OxCGRT) to estimate the impact of governments' policies. Governments have implemented a wide range of responses to contain the COVID-19 outbreak. The public health measures vary in intensity and timing, given the asymmetric health situations and countries' specificities. Therefore, the OxCGRT project aims to collect daily information on governments' health and related policies in a rigorous, systematic, comparable, and reliable way. Also, the OxCGRT project collects governments' policies on a scale that distinguishes the extent of the different policies.[20]

The Tracker includes 21 indicators, such as school closures, public gatherings bans, travel restrictions, and work-from-home orders. Additionally, several composite indices are included and aggregate policies, considering, for instance, the strictness of public health measures. The following analysis primarily considers two weekly indicators for each country: the Stringency Index and the Social Distancing Composite Indicator. First, the

[19] The zero value does not necessarily indicate that there are no searches for the API query on a specific date. A zero value may also suggest that the number of searches performed on a specific date is low and does not reach a sufficient volume of searches, according to a privacy threshold determined by Google.

[20] It is essential to note that the analysis is at the country level. The policies' effect and implementation may vary depending on different factors within a country (Campedelli et al., 2020). This phenomenon may amplify the discrepancies in domestic violence within countries compared to national averages (e.g., see Peek-Asa et al., 2011).

Stringency Index translates the strictness of the public health measures, mainly restricting individuals' behaviors, into an index between 0 and 100. Second, the Social Distancing Composite Indicator is a constructed binary variable equal to 1 when there are stay-at-home orders, school closures, or workplace closures.

Most studies about the relationship between domestic violence and the pandemic only cover short periods, predominantly focusing on the onset of the pandemic (e.g., Leslie and Wilson, 2020). The studies usually employ the date of the first implementation of lockdowns, not distinguishing the various measures and considering them uniform (Berniell & Facchini, 2021; Leslie & Wilson, 2020). Some other studies use mobility-based data, exploiting the variation in individuals' mobility during the pandemic (Ravindran & Shah, 2020). Still, these data are often unavailable before 2020 (e.g., the Google Mobility indices), making cleaning up seasonal effects tricky.

In contrast, the present approach allows tracking the day-to-day decisions made by policymakers consistently and comparably across countries. In addition, the period is long and extends to December 2021, including several tightenings and loosenings in public health measures. Overall, the OxCGRT data allow considering the richness and variability of the public health measures taken in each country over time.

3.3.3 Additional variables

I use several additional variables as controls. First, I consider variables related to COVID-19 spreading. COVID-19 data are daily data from Our World in Data. In particular, I exploit the smoothed number of new COVID-19 cases and deaths per million. These variables might comprise valuable information on the stress associated with the pandemic, influencing domestic violence. Although correlated with governmental responses to the pandemic and presenting a risk of multicollinearity, these data capture the shape of the pandemic within a country and dissociate the effect of the public health measures from the mere influence of the pandemic.

Second, I also use the Google Mobility indices as alternatives for public health measures from the OxCGRT project in robustness checks. The Google Mobility indices reveal the changes in stay duration in different locations relative to normal (corresponding to the median value between January 3 and February 6, 2020). The places covered by the Google Mobility indices are parks, transit stations, workplaces, residential places, groceries and pharmacies, and places related to retail and recreation. However, I focus on variations in stay duration in the workplace, tracking the possible lockdowns and implying unusual increases in time at home.

Third, I exploit the Economic Support Index provided by the OxCGRT. The index translates measures compensating income losses and alleviating household debts on a scale from 0 to 100. Additionally, I construct two binary variables: Income Support, indicating whether governments replace the income by at least 50% in case of income loss, and Debt/Contract Relief, telling whether public authorities implemented schemes to alleviate household debts on a broad scale.

Finally, as an additional control, I consider the weekly number of tweets containing keywords about domestic violence on Twitter to monitor media attention around the topic.[21] The tweets are extracted with the Twitter API for Academic Research.

[21]The keywords are: "domestic abuse", "domestic violence", "family violence", "violence against women", "femicide", "violence couple", "abusive relationship".

Table 3.3 in the Appendix describes the countries with diverse variables presenting socioeconomics and health facets. The countries show some homogeneity in human development and median age, translating into an average life expectancy of 81 years. Nevertheless, the countries have different healthcare systems, particularly regarding available hospital beds. Also, the countries suggest an inevitable variability in extreme poverty and population density.

Additionally, domestic violence significantly varies within the dataset, with the average value at 34.56. Furthermore, the Google Mobility indices reveal substantial variations for some places like transit stations. The evolution of mobility is presented in Figure 3.2 in the Appendix and suggests significant variations over the different waves of the pandemic. Finally, in Table 3.3 in the Appendix, the public health measures show reasonably large variability. Overall, these factors may influence COVID-19 spreading and the public health measures implemented by the authorities, suggesting the importance of controlling for country-specificities, which might affect the overall crisis management.

3.4 Methodology

The core of the analysis relies on a linear regression panel model with fixed effects. However, the model is first introduced with an event study model and then checked with a difference-in-differences model to test several hypotheses and validate the results through various specifications.

First, the weekly event study model focuses on how domestic violence evolves following the date of the first introduction of COVID-19 public health measures. More precisely, the possibility of long-lasting impacts and their timing are examined through a visual inspection to guide the subsequent analysis. In particular, I determine when and how domestic violence deviates significantly from the reference level after implementing measures, suggesting potential impacts on individuals' behaviors. The date of the first introduction of COVID-19 public health measures is at the country level. Additionally, the event study model considers seasonality and country-specificities by including country, week, and year-fixed effects. The regression equation, based on Leslie and Wilson (2020) and Henke and Hsu (2022), is:

$$DV_{cwy} = \alpha + \sum_{j=2}^{J} \beta_j (Lag\, j)_{cwy} + \sum_{k=1}^{K} \gamma_k (Lead\, k)_{cwy} + \psi_c + \mu_w + \delta_y + \varepsilon_{cwy} \tag{3.1}$$

$Lag\, j$ ($Lead\, k$) is a binary variable indicating that the country c is $j-1$ ($k-1$) weeks before (after) the first introduction of the public health measures at week w in year y. The reference week is the week preceding the first introduction of measures in each country (i.e., $j = 1$). Following Clarke and Tapia-Schythe (2021), $Lag\, J$ and $Lead\, K$ accumulate lags and leads beyond the J and K periods. ψ_c, μ_w, and δ_y are fixed effects controlling for country, week, and year differences, respectively. ε_{cwy} is an unobserved error term.

I then introduce the linear regression panel model with fixed effects. The linear regression panel model is the baseline of the analysis for its voluntary simplicity. The model aims to estimate the impact of COVID-19 public health measures on domestic violence. The regression equation in its simplest form is:

$$DV_{cwy} = \beta Public\,Health\,Measure_{cwy} + \psi_c + \mu_w + \delta_y + \varepsilon_{cwy} \tag{3.2}$$

The outcome DV_{cwy} is the fraction of domestic violence-related searches relative to the overall mass of Google searches in the country c during the week w of year y. ψ_c, μ_w, and δ_y are fixed effects controlling for country, week, and year differences, respectively. ε_{cwy} is an unobserved error term. $Public\,Health\,Measure_{cwy}$ is finally a variable representing the public health policy of the country c during the week w of year y. The results in the following section are robust to several other specifications, including replacing the fixed effects with country-by-week and country-by-year fixed effects. The identification assumption is that COVID-19 public health measures provide exogenous variation when including controls.

From this model, I then derive a slightly modified panel linear regression model with fixed effects to introduce flexibility in the model and study the evolution of the impact over time. The specifications consider notably the interaction of the COVID-19 public health measures with the past cumulated public health measures. The interaction allows considering the evolution of the effect of measures as the pandemic progresses, during which lassitude may emerge.

Finally, following Leslie and Wilson (2020), I employ as robustness checks a difference-in-differences model to quantify the effect of measures on domestic violence. The model compares domestic violence between years affected by COVID-19 public health measures with domestic violence in the preceding years, comparing periods experiencing heterogeneous stringencies of public health measures. This method might better account for seasonal changes in domestic violence. I estimate the following equation:

$$DV_{cwy} = \beta Public\,Health\,Measure\,(Bin)_{cw} \times Year\,2020_y + \psi_c + \mu_w + \delta_y + \varepsilon_{cwy} \tag{3.3}$$

The $Public\,Health\,Measure\,(Bin)_{cw}$ indicator is a binary variable equal to 1 if the observation is subject to public health measures. The regression includes the same set of fixed effects. The idea is to exploit the measures' heterogeneity and timing across geographical areas. The identification hypothesis of the difference-in-differences model depends on the parallel trends assumption. Therefore, the assumption is that domestic violence would have followed similar seasonal trends to previous years in the absence of public health measures.

3.5 Results

Section 3.5.1 analyzes the effect of COVID-19 public health measures on domestic violence and how this effect evolves. Next, Section 3.5.2 explores how several factors, such as compliance with governments' measures and economic support, may play a role in the effect of public health measures on domestic violence. Finally, Section 3.5.3 provides various robustness checks with alternative specifications and control variables.

3.5.1 Effect of COVID-19 public health measures

Figures 3.1(a) and 3.1(b) depict how domestic violence evolves following the date of the first introduction of COVID-19 public health measures. The Figures present the coefficients and confidence intervals of lags and leads of Equation 3.1 while using the intensity of domestic violence-related Google searches as the dependent variable. The models include arbitrarily 50 leads, corresponding to roughly one year of data after the start and two waves, and 30 lags, to show the stability of domestic violence before the date of the first introduction. Extra leads and lags are not included because additional coefficients would lower the precision in estimating coefficients. Also, in Figure 3.1(a), week 0 corresponds to the week of the first introduction of COVID-19 public health measures, resulting in the first increase in the Stringency index during the pandemic. Week 0 is usually a week around February and March 2020 for most countries, corresponding to the first distancing and tracing measures imposed by governments at the very onset of the pandemic, generally preceding the formal introduction of more stringent measures, such as stay-at-home orders. The reference week is the week preceding the introduction of measures.

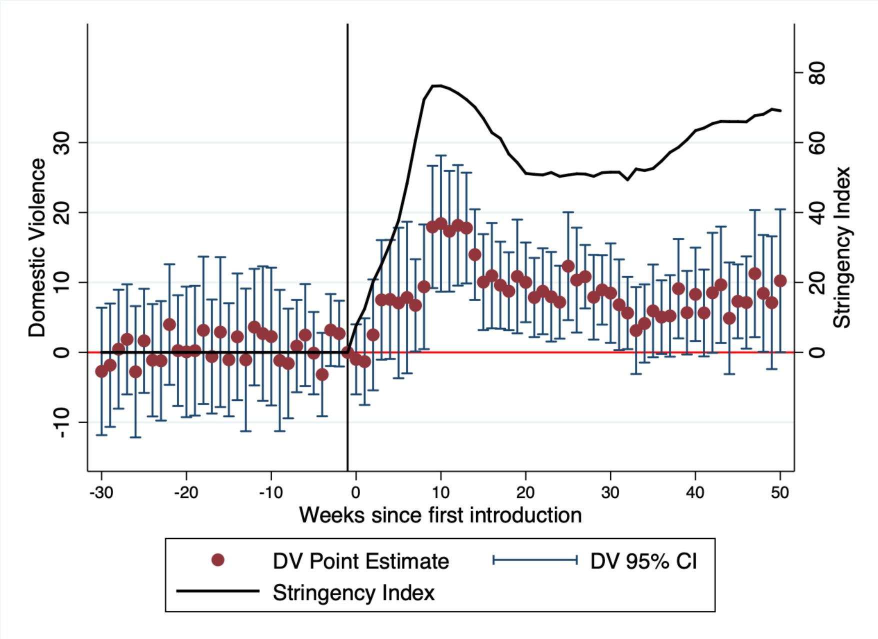
Figure 3.1(a) shows that domestic violence significantly increases from week 9, peaking rapidly between week 10 and week 15. The increase in domestic violence then fades over time and disappears around week 30 before increasing slightly and significantly again. In contrast, Figure 3.1(b) indicates that domestic violence increases significantly as early as week 2 and peaks at week 5. The asymmetry in the timing between Figures 3.1(a) and 3.1(b) may be explained because the Social Distancing Composite Indicator only equals one when, by definition, governments impose relatively severe measures. Conversely, the Stringency Index may increase due to mere public health recommendations, such as keeping a minimum distance between individuals. However, similarly to Figure 3.1(a), the increase in domestic violence-related searches tapers off over time and fades around week 30. Domestic violence then rises again very quickly.

Interestingly, the magnitude of the coefficients estimated in the event study models follows the intensity of COVID-19 public health measures, with one or two weeks of lags. More specifically, domestic violence increases as the Stringency Index increases. Similarly, when the Social Distancing Composite Indicator increases again from week 30, domestic violence increases significantly. This result is a fortiori gripping because the estimated coefficients in Equation 3.1 are only related to COVID-19 public health measures through the date of the first introduction of measures, taking place at the onset of the pandemic. The richness of the following variability of public health measures is thus not considered by the model. Therefore, the graphical correspondence between the estimated coefficients and the public health measures suggests that measures play a role in domestic violence.

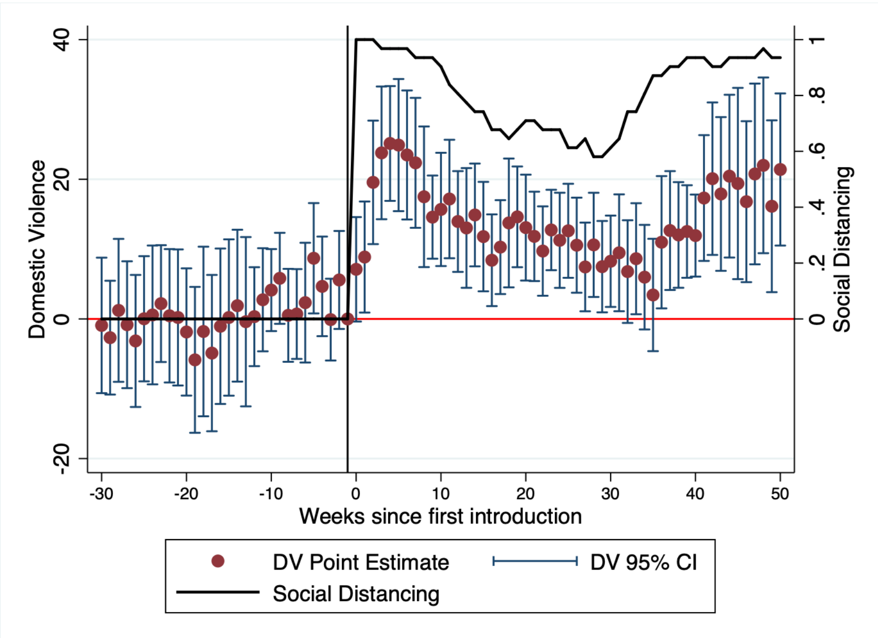
Table 3.1 hence aims to assess whether COVID-19 public health measures impact domestic violence statistically. Columns 1 and 2 of Table 3.1 present the results using the baseline specification in Equation 3.2. $Public\,Health\,Measure_{cwy}$ corresponds to the Stringency Index in Column 1 and the Social Distancing Composite Indicator in Column 2, respectively. The impact of COVID-19 public health measures on domestic violence is positive and significant. An increase in the stringency of the measures boosts domestic violence-related searches on Google, suggesting an increase in the underlying domestic violence experienced within households. The results in Column 2, although based on an alternative independent variable, conclude similarly. These results are robust to using country-by-year and country-by-week fixed effects (Columns 3 and 4).

Next, I test alternative COVID-19 public health measures, such as workplace closing, stay-at-home orders or school closing (Columns 5, 6, and 7, respectively). The combination of these three measures in Columns 8 and 9 suggests that workplace closure mainly influences domestic violence. This outcome corroborates the exposure reduction theory (Dugan et al., 1999) and the male backlash theory if the workplace closure results in substantial

Figure 3.1: Event studies



(a) Stringency Index



(b) Social Distancing Composite Indicator

The x-axis displays the weeks between the week analyzed and the week of the first introduction of considered COVID-19 public health measures. The week of the first introduction of measures in Figure 3.1(a) (Figure 3.1(b), resp.) is equivalent to the week from which the Stringency Index (the Social Distancing Composite Indicator, resp.) increases.

Specification:	Baseline Measures				Alternative Measures				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Stringency Index	0.1208**		0.1145**						
	(0.0226)		(0.0256)						
Social Distancing Composite Indicator		3.8894**		3.9599**					
		(1.0874)		(1.2223)					
School Closing					2.7046**			0.6433	0.9756
					(0.9001)			(0.9097)	(1.1227)
Workplace Closing						4.2784**		3.3476**	3.6047**
						(1.0590)		(1.0788)	(1.3186)
Require Stay at Home							3.2354**	1.6649	-2.9801
							(1.1826)	(1.1290)	(2.0417)
School Closing × Workplace Closing									-1.3639
									(1.6175)
School Closing × Require Stay at Home									3.0013
									(3.3169)
Workplace Closing × Require Stay at Home									3.0041
									(2.1490)
School Closing × Workplace Closing × Require Stay at Home									-0.1943
									(3.3366)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes
Country x Year Fixed Effects	No	No	Yes	Yes	No	No	No	No	No
Country x Week Fixed Effects	No	No	Yes	Yes	No	No	No	No	No
Observations	8,091	8,091	8,091	8,091	8,091	8,091	8,091	8,091	8,091
Countries	31	31	31	31	31	31	31	31	31

Table 3.1: Effect of COVID-19 public health measures on domestic violence

Note: Panel regressions including data between January 1, 2017, and December 31, 2021 in 31 countries with high Internet and Google penetration. Standard errors, clustered at the country level, are in parenthesis. Significance levels: $^{*} = 10\%$, $^{**} = 5\%$, and $^{***} = 1\%$.

income losses for the breadwinner, altering the household bargaining power balance.

Table 3.2 examines then how the effect of COVID-19 public health measures on domestic violence adjusts over time. In particular, I use different specifications to determine whether the positive and significant effect in Table 3.1 fades over time or whether the effect proves to be relatively stable, holding other things, including the Stringency Index, constant. In Columns 1 and 2, the Stringency Index and the Social Distancing Composite Indicator are accumulated over time and added to Equation 3.2, respectively, reflecting the cumulation of measures in the eyes of individuals as the pandemic advances. Nevertheless, the accumulated measures are inconclusive, while the effect of public health measures remains positive and significant.

Subsequently, in Columns 3 and 4, I interact these accumulated measures with the current measures. The results suggest that the accumulation of measures over time increases domestic violence. Hence, while the measures directly affect domestic violence, they also have an effect through their accumulation. Over time, the accumulated measures increase domestic violence significantly, while the interaction of the accumulated measures with the current measures has a negative and significant effect. This negative interaction coefficient points out that the more restrictions individuals have experienced in the past, the more limited the effect of the measures will be on domestic violence.

Columns 5 and 6 of Table 3.2 use a slightly different specification but lead to similar results. Specifically, instead of including a variable representing accumulated measurements over time, I use a variable indicating the number of weeks since the first introduction of public health measures. The emerging findings are that while the number of weeks does not affect domestic violence, the interaction of the number of weeks with the $Public\,Health\,Measures$ has a negative and significant effect. In other words, the same health policy will have a reduced impact in week 30 compared to week 10.

3.5.2 Compliance, economic support, and other factors

This section tentatively explores several potential factors for explaining the decline over time in the effect of public health measures on domestic violence. The first considered rationale is that individuals' compliance with measures may evolve. While the Stringency Index remains high over long periods as governments maintain restrictive public health measures to contain the spread of the virus, individuals may become increasingly disobedient to public health measures as policies persist. Table 3.5 in the Appendix explores this possibility while employing the residential Google Mobility Index as the dependent variable. The Google Mobility Index is interpreted as follows: The higher the residential Google Mobility Index, the longer individuals are at residential locations. The results indicate that the Stringency Index and the Social Distancing Composite Index significantly increase the frequentation in residential places. However, it also appears that the accumulation of the measures has a negative effect on the presence in residential areas (Columns 1-4), suggesting that individuals frequent more other places, potentially transgressing governments' mandates. Similarly, the interaction of governments' measures with the weeks since the first introduction of public health measures suggests a negative effect on the frequentation of residential locations, further corroborating the findings.

Another factor that may moderate the effect of the measures and explain its decline over time is the attrition of

	(1)	(2)	(3)	(4)	(5)	(6)
Stringency Index	0.1228**		0.1722**		0.1879**	
	(0.0229)		(0.0284)		(0.0302)	
Accumulated Stringency Index	-0.0003		0.0014**			
	(0.0005)		(0.0005)			
Social Distancing Composite Indicator		4.0334**		7.2962**		7.8602**
		(1.0894)		(1.3258)		(1.4991)
Accumulated Social Distancing Composite Indicator		-0.0139		0.0819**		
		(0.0354)		(0.0399)		
Stringency Index × Accumulated Stringency Index			-0.0000**			
			(0.0000)			
Social Distancing Composite Indicator × Accumulated Social Distancing Composite Indicator				-0.1259**		
				(0.0356)		
Weeks since first Stringency policy					0.0301	
					(0.0301)	
Stringency Index × Weeks since first Stringency policy					-0.0022**	
					(0.0006)	
Weeks since first Social Distancing policy						0.0159
						(0.0285)
Social Distancing Composite Indicator × Weeks since first Social Distancing policy						-0.1013**
						(0.0304)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,091	8,091	8,091	8,091	8,091	8,091
Countries	31	31	31	31	31	31

Table 3.2: Evolution of the effect of COVID-19 public health measures on domestic violence

Note: Panel regressions including data between January 1, 2017, and December 31, 2021 in 31 countries with high Internet and Google penetration. Standard errors, clustered at the country level, are in parenthesis. Significance levels: * = 10%, ** = 5%, and *** = 1%.

the stock of couples and, in particular, of couples at risk of domestic violence. Although the data in this analysis do not permit empirical testing of this hypothesis, some studies provide evidence supporting this partial explanation. Divorces declined in the early months of the pandemic in several geographic areas (Manning & Payne, 2021). In addition to economic and social constraints, this reduction was partly due to legal and administrative challenges and the additional response time of legal bodies (Goldberg et al., 2021). Nonetheless, the pandemic also led to a significant decline in new marriages (Ghaznavi et al., 2022), resulting from relocation difficulties and reduced ability to meet new people. Consequently, the overall stock of married couples living in the same household may have decreased. Since the Google searches-based measure is a relative measure of domestic violence-related searches to the overall mass of Google searches, the measure may have diminished because the number of couples living together has decreased.

Table 3.6 then explores the interaction effect between COVID-19 public health measures and economic support measures on domestic violence. Columns 1 and 2 consider the binary variable income support equal to 1 when the government replaces 50% or more of lost income. The interaction effect on domestic violence is positive and significant in Column 1, suggesting that the increase in income support may have an unfavorable influence on domestic violence by increasing it, all other things being equal. In contrast, Column 2, using the Social Distancing Composite Indicator instead, does not find any interaction effect. Then, Columns 3 and 4 focus on governments' measures to alleviate household debts by implementing broad debt reliefs and come to similar conclusions as in Columns 1 and 2, respectively. Finally, Columns 5 and 6 use the Economic Support Index, a variable ranging from 0 to 100 provided by the OxCGRT and summarizing the debt relief measures and the income support measure into one index while considering more subtle changes. Again, the effect of the interaction between the Economic Support Index and the Stringency Index is positive and significant, while the interaction with the social distancing composite indicator is inconclusive.

The inconclusive effect of the interaction of economic support with the Social Distancing Composite Indicator may likely be explained to the combination of the binarity of the Social Distancing Composite Indicator and the little variability of the economic support measures. Indeed, the economic support measures were relatively stable over time, especially when stringent measures were applied, resulting in little variability. In contrast, the positive and significant effect of the interaction of economic support measures with the Stringency Index may find its roots in the recent economic theory around domestic violence.

The effect of economic support measures is theoretically ambiguous. The loss of income may involve stress, resulting in domestic violence (Card & Dahl, 2011). However, the COVID-19 public health measures containing the spread of the virus implied gender asymmetries, affecting more women through unemployment proportionally (Albanesi & Kim, 2021). Women's income loss corresponds to a loss of bargaining power. The household bargaining power theory then indicates that women's loss of bargaining power results in increased domestic violence as men gain relative bargaining power in the overall equilibrium (Henke & Hsu, 2022).

In contrast, instrumental violence and male backlash theories suggest alternative responses driven by men feeling that their traditional gender role is threatened (Aizer, 2010). In this context, men aim to achieve dominance through different means, such as economically or violently. Therefore, a proportionally higher reduction in women's bargaining power may instead reduce domestic violence (Guarnieri & Rainer, 2018), as men can reduce their level of violence to achieve dominance. As a result, economic assistance to compensate for economic losses might exacerbate domestic violence as men seek to re-establish dominance through violence.

The resulting balance in domestic violence relies on the gender distribution of economic losses. This finding is in line with studies analyzing the impact of gender unemployment on domestic violence (e.g., Anderberg et al., 2016; Henke and Hsu, 2022). This result is further supported by Bertrand et al. (2015)'s findings regarding the consequences of relative women's income within households, suggesting that households have, on average, an aversion to a situation where the wife earns more than the husband. In such cases, the households are less satisfied with their marriage and more likely to divorce (Bertrand et al., 2015). Economic assistance, helping relatively more women, may thus increase domestic violence, everything else being held constant.

Finally, several other conceivable explanations could be put forward to explain the decline of the effect of the measures over time. For instance, individuals may experience a habituation process, altering their reference point in terms of violence. Besides, the phenomenon may be driven by the very nature of Google searches. Berniell and Facchini (2021) note that the effect tends to extinct quicker when using Google searches than survey-based data. Once victims obtain sufficient information on the Internet, they may be less likely to google it again. In contrast, since domestic violence incidents do not decrease suddenly, survey-based studies may display more persistent effects. These phenomena may imply an under-measurement of domestic violence incidents, suggesting that estimates are lower bound estimates.

3.5.3 Robustness checks

Finally, I test several specifications as robustness checks. First, Figure 3.4 presents the coefficient of interest of Equation 3.2 by excluding each country one by one to check if any country does not drive the overall estimated effect. Results suggest that no country biases the estimated effect in previous tables. Second, Columns 1 and 2 of Table 3.7 add new COVID-19 cases and deaths, respectively, per million to the baseline equation. Although potentially highly correlated with governments' health measures, these variables may reflect the state and concerns about the pandemic. Including these variables does not change the interpretation of the effect of public health measures on domestic violence. Third, Column 3 of Table 3.7 includes the number of tweets about domestic violence and related variations in English, French, and Spanish. This variable allows controlling for the mass media attention surrounding domestic violence that may have biased Google searches (Cervellin et al., 2017), notably when media raised awareness about the impact of the pandemic on domestic violence. Adding the variable to the specification does not change the interpretation of the effect of public health measures. At the same time, the number of tweets proves to affect Google searches too. Fourth, Column 4 of Table 3.7 replaces the dependent variable with a placebo Google search topic which is hypothetically not related to the measures, "pumpkins". The coefficient of measures is not significant, which satisfies the objective of the placebo regression.

Finally, Columns 5 to 8 of Table 3.7 reproduce Equation 3.3. The model follows a Difference-in-Differences-alike (DiD) approach based on Leslie and Wilson (2020). Contrarily to the typical definition, the treatment group comprises observations from the post-2020 period, while the control group is based on the pre-2020 period. The validity of the DiD analysis is motivated in particular by Figure 3.3, displaying parallel trends before the introduction of measures. Overall, the model concludes similarly to the previous regressions.

3.6 Conclusion

This study contrasts in multiple dimensions with existing studies on the effect of the COVID-19 pandemic on domestic violence. First, the analysis focuses specifically on the effect of COVID-19 public health measures by exploiting the variability of the stringency of the governmental measures over time. This approach, which leverages the richness of the specificities of policies, reveals that public health measures increase domestic violence.

Second, the study's timeframe extends to December 2021 and allows observing how the effect of the measures evolves across consecutive waves. In particular, I indicate that the effect of the measures on domestic violence declines over time after a large initial shock. Therefore, policymakers may tailor policies to mitigate the impact of the measures by focusing on the initial shock.

Third, the study exploits weekly domestic violence-related Google searches as proxies for domestic violence incidents. The underlying idea is that Google searches are closely related to actual domestic violence incidents. Although this type of data is not new in research (e.g., see Anderberg et al., 2022), the large number of countries covered, owing to the comparability of the data, expands the geographical scope. Additionally, the data are well suited to the pandemic, during which the underreporting issue associated with traditional domestic violence measures is exacerbated by public health measures that reduce opportunities for external contacts. The results underline the interest of Google users' traces data in times of crisis for studying social phenomena. The data form a bridge overcoming the gap caused by crises involving isolation measures and creating conventional data scarcity.

Fourth, I tentatively explore the role of a few potential factors, such as individuals' compliance with public health measures and economic support policies, in altering or exacerbating the effect of measures on domestic violence. On the one hand, hints suggest that individuals' compliance with measures may decline over time, potentially diminishing the impact of measures. On the other hand, I find that economic support policies might not contribute to decreasing domestic violence. On the contrary, economic support measures are linked to exacerbated domestic violence. COVID-19 public health measures were associated with asymmetries in terms of gender, involving proportionally more female unemployment (Albanesi & Kim, 2021). A more than proportional increase in female unemployment may imply a decrease in female bargaining power within the household. However, the economic support policies that replace lost income at least partially restore bargaining power. The resulting perverse effect might be that domestic violence increases, following the theories of male backlash and male instrumental violence, as men seek to re-establish a form of dominance through violence (e.g., see Anderberg et al., 2016; Henke and Hsu, 2022).

One may identify additional mechanisms lessening or exacerbating the effect of measures on domestic violence. However, more fine-grained data, contrasting with the nature of Google data, would be needed. For instance, Google data do not deliver micro-level insights or gender breakdowns. Therefore, while showing the paramount role of these data in providing first insights, especially during severe crises, the study does not dismiss the value of more conventional data sources, such as police data or data from help hotlines. These sources are excellent complements offering many opportunities to study social phenomena.

The present study highlights the shadow existence of domestic violence and the particular vulnerability of victims during crises limiting external contacts. Policies to date have unintentionally often pointed towards a tradeoff between victims of the COVID-19 pandemic and victims of domestic violence (i.e., the shadow pandemic) to contain the spread of the virus. The analysis emphasizes the importance of establishing safeguards to protect and shelter victims. Further research shall thus focus on designing policies for mitigating the exacerbation of domestic violence in times of crisis that are compatible with the health circumstances.

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Appendix

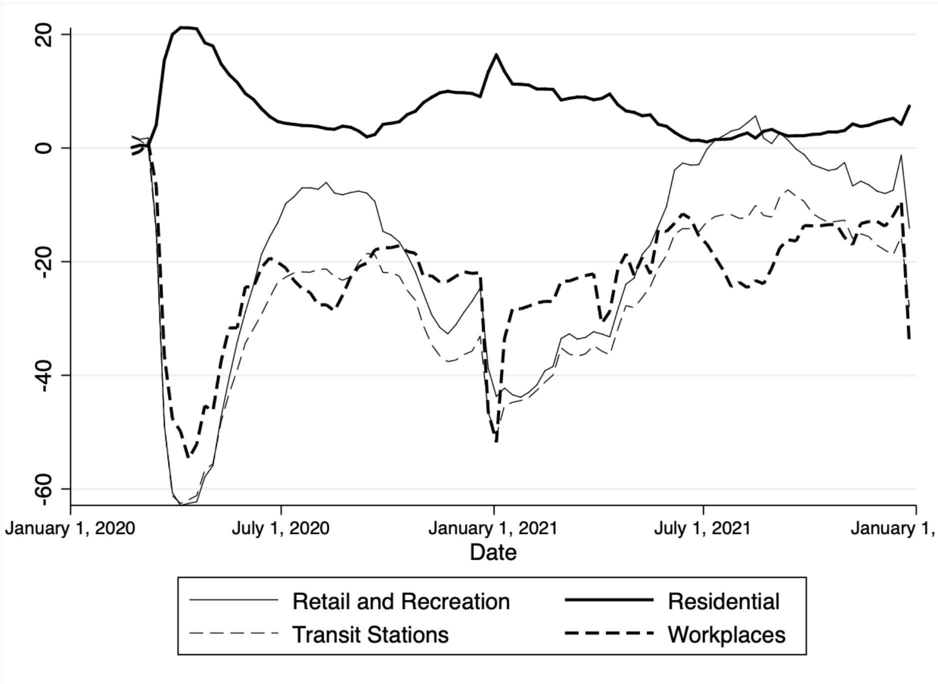
	mean	sd	p25	p50	p75
Domestic violence	34.56	18.72	21.00	33.00	47.00
Stringency Index	21.01	29.37	0.00	0.00	48.15
Social Distancing Composite Indicator	0.29	0.45	0.00	0.00	1.00
Economic Support Index	22.55	34.70	0.00	0.00	50.00
Google Mobility Index					
Mobility - Retail and Recreation	-19.15	22.38	-32.57	-14.43	-2.86
Mobility - Grocery and Pharmacy	2.92	15.60	-5.29	1.86	11.57
Mobility - Transit Stations	-26.68	20.29	-40.00	-27.00	-12.29
Mobility - Workplaces	-22.89	13.81	-29.86	-21.29	-14.14
Mobility - Residential	6.73	6.56	2.14	5.71	10.14
COVID-19 Situation					
New COVID-19 Cases (per million)	64.15	171.28	0.00	0.00	30.00
New COVID-19 Deaths (per million)	0.94	2.68	0.00	0.00	0.33
ICU patients (per million)	24.52	25.99	5.05	14.56	36.41
Hospitalised patients (per million)	154.09	207.26	25.20	71.72	188.47
Weekly ICU admissions (per million)	10.77	11.99	1.96	6.19	16.22
Weekly admissions (per million)	82.82	95.09	16.80	48.03	115.01
New Tests (per thousand)	4.50	7.44	1.10	2.40	4.26
New People vaccinated (per million)	0.19	0.19	0.06	0.12	0.28
Countries Specificities					
Population (in million)	37.16	62.15	6.90	10.37	46.75
Population Density	133.62	120.43	46.86	106.75	205.86
Median Age	41.15	4.29	39.70	41.80	43.40
Aged 65 or Older	17.35	3.64	15.32	18.52	19.72
Aged 70 or Older	11.46	2.73	9.79	11.88	13.27
GDP per capita (in thousand)	39.70	16.13	27.22	38.61	46.68
Extreme Poverty	0.96	1.21	0.20	0.50	1.20
Hospital Beds (per thousand)	4.51	2.18	2.81	3.60	6.62
Life Expectancy	80.63	2.74	78.73	81.91	82.43
Human Development Index	0.90	0.05	0.86	0.92	0.94
Observations	8,091	8,091	8,091	8,091	8,091

Table 3.3: Descriptive statistics

Country Name	Country Code
Australia	AU
Austria	AT
Belarus	BY
Belgium	BE
Bulgaria	BG
Canada	CA
Czechia	CZ
Denmark	DK
Finland	FI
France	FR
Germany	DE
Greece	GR
Hungary	HU
Ireland	IE
Israel	IL
Italy	IT
Luxembourg	LU
Mexico	MX
Netherlands	NL
New Zealand	NZ
Norway	NO
Poland	PL
Portugal	PT
Romania	RO
Slovakia	SK
Spain	ES
Sweden	SE
Switzerland	CH
Turkey	TR
United Kingdom	GB
United States	US

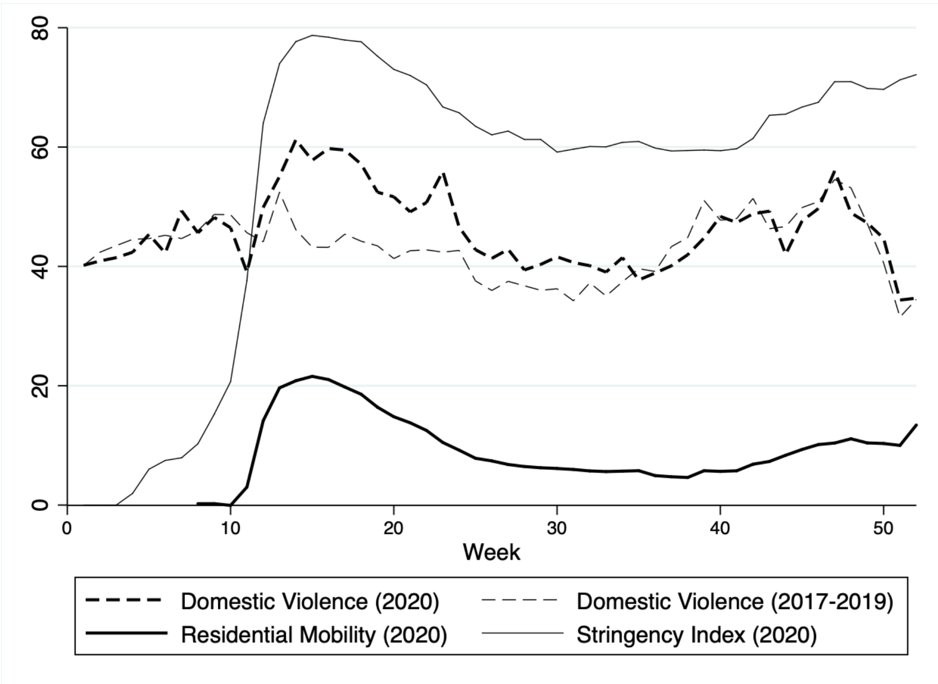
Table 3.4: List of countries
Note: Country codes are ISO 3166-1 alpha 2 codes.

Figure 3.2: Evolution of mobility by places type



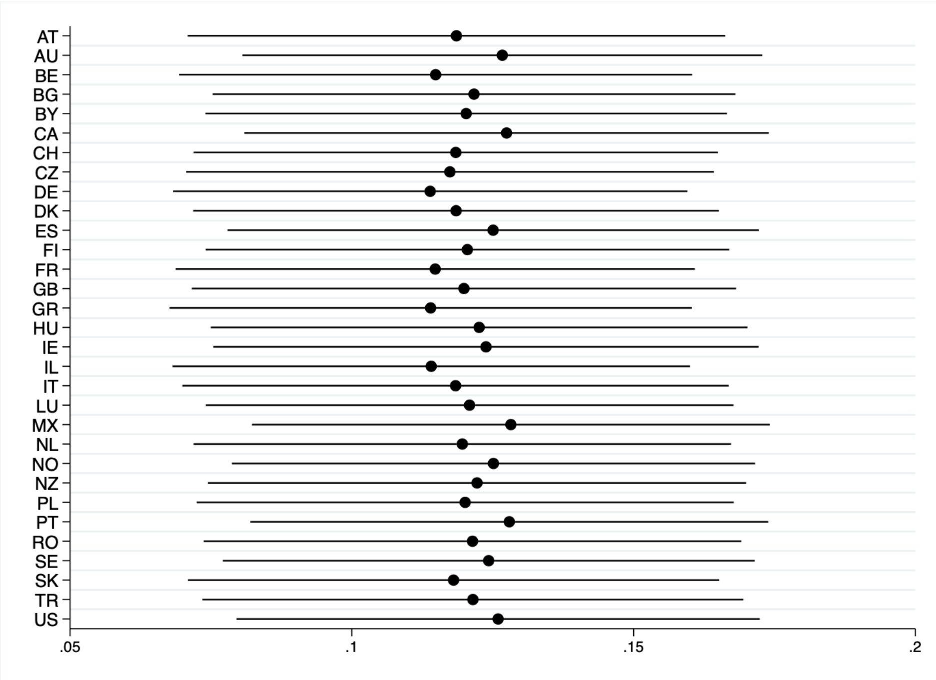
Source: Google Mobility Report

Figure 3.3: Evolution in Google searches, residential mobility & Stringency Index



Source: Google Trends, Google Mobility Report & OxCGRT

Figure 3.4: Effect of COVID-19 public health measures on domestic violence, excluding each country one by one



Note: The coefficient represented on each line is the effect of public health measures on domestic violence as in Table 3.1, excluding data for the country mentioned on the y-axis. Panel regressions include data between January 1st, 2017, and December 31st, 2021, in 31 countries with high Internet and Google penetration. Standard errors, clustered at the country level, are in parenthesis. Significance levels: $^{*}=10\%$, $^{**}=5\%$, and $^{***}=1\%$.

	(1)	(2)	(3)	(4)	(5)	(6)
Stringency Index	0.1844**		0.2788**		0.2922**	
	(0.0134)		(0.0132)		(0.0170)	
Accumulated Stringency Index	-0.0028**		0.0015			
	(0.0008)		(0.0010)			
Social Distancing Composite Indicator		3.9798**		8.2316**		7.5415**
		(0.7791)		(0.9354)		(0.9038)
Accumulated Social Distancing Composite Indicator		-0.1041**		0.0434*		
		(0.0323)		(0.0244)		
Stringency Index × Accumulated Stringency Index			-0.0000**			
			(0.0000)			
Social Distancing Composite Indicator × Accumulated Social Distancing Composite Indicator				-0.1316**		
				(0.0154)		
Weeks since first Stringency policy					4.2702**	
					(0.5543)	
Stringency Index × Weeks since first Stringency policy					-0.0026**	
					(0.0004)	
Weeks since first Social Distancing policy						-2.3854**
						(0.5764)
Social Distancing Composite Indicator × Weeks since first Social Distancing policy						-0.0916**
						(0.0111)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,005	3,005	3,005	3,005	3,005	3,005
Countries	31	31	31	31	31	31

Table 3.5: Evolution of the effect of COVID-19 public health measures on compliance

Note: Panel regressions including data between January 1st, 2017, and December 31st, 2021, in 31 countries with high Internet and Google penetration. Standard errors, clustered at the country level, are in parenthesis. Significance levels: * = 10%, ** = 5%, and *** = 1%.

Economic Measures:	Income Support		Debt Relief		Economic Support Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Stringency Index	0.0673**		0.0918**		0.0427	
	(0.0299)		(0.0296)		(0.0379)	
Income Support	-1.9401	2.7020*				
	(2.3010)	(1.5129)				
Stringency Index × Income Support	0.0890*					
	(0.0505)					
Social Distancing Composite Indicator		1.9322		2.9011**		0.9606
		(1.7231)		(1.3278)		(2.0205)
Social Distancing Composite Indicator × Income Support		1.5784				
		(2.3018)				
Debt/Contract Relief			-4.5530*	-0.1384		
			(2.4761)	(2.0979)		
Stringency Index × Debt/Contract Relief			0.0937*			
			(0.0519)			
Social Distancing Composite Indicator × Debt/Contract Relief				2.2107		
				(2.5412)		
Economic Support Index					-0.0474	0.0264
					(0.0298)	(0.0201)
Stringency Index × Economic Support Index					0.0015**	
					(0.0007)	
Social Distancing Composite Indicator × Economic Support Index						0.0338
						(0.0322)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,091	8,091	8,091	8,091	8,091	8,091
Countries	31	31	31	31	31	31

Table 3.6: Effect of the interaction between COVID-19 public health measures & economic support measures on domestic violence

Note: Panel regressions including data between January 1st, 2017, and December 31st, 2021, in 31 countries with high Internet and Google penetration. Standard errors, clustered at the country level, are in parenthesis. Significance levels: * = 10%, ** = 5%, and *** = 1%.

Specification:	Baseline + Controls			Placebo Topic	Difference-in-Differences			
Treatment Period:					2020		2021	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stringency Index	0.1184**	0.1145**	0.1111**	0.0215				
	(0.0238)	(0.0253)	(0.0225)	(0.0154)				
New COVID-19 Cases (per million)	0.0010							
	(0.0026)							
New COVID-19 Deaths (per million)		0.1054						
		(0.2009)						
Tweets about Domestic Violence			0.0001**					
			(0.0000)					
Post-2020					-1.1352	2.6683**	1.7191	4.2748**
					(0.9728)	(1.0124)	(2.3281)	(1.8933)
Stringency (2020)					0.0049			
					(0.0227)			
Post-2020 × Stringency (2020)					0.1616**			
					(0.0271)			
Social Distancing (2020)						-0.7163		
						(0.8646)		
Post-2020 × Social Distancing (2020)						6.5218**		
						(1.2549)		
Stringency (2021)							0.0089	
							(0.0254)	
Post-2020 × Stringency (2021)							0.0548	
							(0.0390)	
Social Distancing (2021)								1.0577
								(1.1542)
Post-2020 × Social Distancing (2021)								0.6460
								(1.9339)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,088	8,075	8,060	8,091	6,479	6,479	6,479	6,479
Countries	31	31	31	31	31	31	31	31

Table 3.7: Robustness checks: Effect of COVID-19 public health measures on domestic violence

Note: Panel regressions including data between January 1st, 2017, and December 31st, 2021, in 31 countries with high Internet and Google penetration. Standard errors, clustered at the country level, are in parenthesis. Significance levels: * = 10%, ** = 5%, and *** = 1%.

Psychotropic drug consumption during the COVID-19 pandemic in Luxembourg: Excess consumption and socio-demographic profile

Chapter 4

Psychotropic drug consumption during the COVID-19 pandemic in Luxembourg: Excess consumption and socio-demographic profile

with Philippe Van Kerm (University of Luxembourg)
and Tom Rausch (Ministry of Health, Luxembourg)

4.1 Introduction

Governments have taken measures of an unprecedented scale to contain the spread of COVID-19 to protect individuals' health and prevent healthcare systems from being overwhelmed. Sanitary measures included workplace and school closures and stay-at-home orders at the outset of the pandemic and during peaks of infections. While these measures helped contain the spread of the virus, concern has been expressed that sanitary measures affected individuals' emotional states and habits and may have left many individuals feeling disenfranchised in a stressful sanitary and socioeconomic environment (Farina et al., 2021; Ripoll et al., 2021; Wang et al., 2021). This, in combination with the generally anxiogenic context of the COVID-19 pandemic, has raised concerns about possibly severe consequences on mental health. Previous studies have shown that unfavorable socioeconomic contexts with financial or labor market instability can drive psychotropic drug consumption (Nicieza-García et al., 2016; Odriozola-González et al., 2020; Vittadini et al., 2014). The COVID-19 pandemic, with its restrictive sanitary measures, stressful health contexts, and adverse socioeconomic consequences, may thus have created an environment conducive to mental health disorders. Studies have reported an increase in symptoms of depression, addiction, post-traumatic stress, anxiety, and sleep disorders since the onset of the pandemic (e.g., Farina et al., 2021; Qiu et al., 2020; Wang et al., 2021). With mental health being a leading cause of disability in Europe (Barcelo et al., 2016), a possible double impact of the pandemic on both mental and physical health exists and needs to be quantified.

This study uses large-scale administrative data to examine the evolution of psychotropic drug consumption in Luxembourg during the COVID-19 pandemic compared to the trends observed in the few years prior to 2020. The analysis relies on individual-level quarterly purchases of psychotropic drugs made in all (non-hospital) pharmacies in Luxembourg between January 2016 and December 2021. We examine the purchase of three broad classes of psychotropic drugs: anxiolytics (ATC N05B), hypnotics and sedatives (ATC N05C), and antidepressants (ATC N06A).[22] These quarterly data are coupled with anonymized individual-level socioeconomic data from the Inspection générale de la sécurité sociale (IGSS), providing a complete picture of the characteristics of the population aged 18 to 79 years and residing in Luxembourg in February 2020. Whereas most epidemiological studies on mental health rely on self-reports obtained from (large-scale) population surveys (e.g., see Tennant et al., 2007), access to administrative data provides comprehensive coverage of the population and avoids issues of self-reporting bias and selective study participation. Furthermore, data linkage allows us to observe detailed information on individuals and their characteristics. This includes information on age and sex, household size, household income, and employment status of individuals, for example. These data allow better control of the profile of individuals and for a detailed analysis by population subgroups (which are sometimes underrepresented in survey-based studies). An analysis by subgroups is instructive, given the heterogeneity in the exposure of different population groups to the pandemic and sanitary measures.

The main advantage of exploiting administrative data on psychotropic drug purchases is the accuracy and reliability of measurement – we observe all purchases of drugs aimed at treating mental disorders made in pharmacies and do not rely on individual self-reports. The downside is that drug purchases may not fully capture mental health disorders. For mental health issues to be reflected in drug purchases, affected individuals must first seek treatment with healthcare providers. Then, the condition must be sufficiently severe for a health professional to decide on prescribing a psychotropic drug. Finally, the patient must be able to go and purchase the prescribed drug. Our data provide insight into the population that reaches that margin, and how this has evolved since the onset of the pandemic.

In the period under study, we expect changes in psychotropic drug purchases to be driven mainly by the population's evolution of mental health conditions. However, to be able to do so, we are also careful to account for longer-term trends in medication and possible short-term restrictions in access to care during the peaks of the pandemic. Changes in sales of psychotropic medications can come from two sources. First, an increase in the demand for a drug may reflect deteriorating mental health in the population. We set out to assess if the pandemic had such an impact. However, second, an increase in sales of psychotropic medications may also reflect an increase in the supply of healthcare services, changes in protocols and the treatment of mental health over time, and possibly an increase in the use of medications in treatments. To avoid attributing to the pandemic any

[22]Some people may, in a few cases, take these medications for reasons other than mental health, such as sleep disorders or pain management.

longer-term variation in supply, we use data for the quarters pre-pandemic (from 2016 to 2019) to estimate the trends and seasonality in purchases against which we can compare purchases in 2020 and 2021—the pandemic period—to quantify a possible "excess consumption" during the pandemic – our impact of interest.

Some studies have suggested that the pandemic period coincided with an increase in the consumption of psychotropic drugs beyond what was projected vis-à-vis the long-term upward trend. Levaillant et al. (2021) and Benistand et al. (2022) are two studies based on population-wide data for France which found clear increases in psychotropic drug consumption. However, most studies conducted in other countries using large-scale data did not find such clear results. Leong et al. (2022) found increases only in the consumption of antidepressants in the Canadian province of Manitoba. Wolfschlag et al. (2021) found no effect in the Swedish region of Scania. Krupa et al. (2022) found no noticeable change in psychotropic drug purchase in Poland. Uthayakumar et al. (2022) found no effect in Canada. While the absence of an effect is observed in these studies in the long run, most find a sharp but short-lived deviation from a trend at the start of the pandemic in the period of strictest confinement. Similarly, Hirschtritt et al. (2021) in a study of the immediate after the onset of the pandemic in California, detected a decrease in medication compared to the previous year during the first confinement.

Only a few studies have been able to examine these changes by population subgroups. Contrasting results have been reported by age and gender. Leong et al. (2022) found limited excess consumption of psychotropic drugs among elderly people, like Vahia et al. (2020) did, but they found increases in consumption among women. Kuitunen (2022) mostly found decreases in psychotropic drug consumption among Finnish children. No more detailed analysis by population groups based on population-wide data is available to date.

Our analysis reveals no simple story, but exhibits results consistent with previous studies conducted in 2020 and 2021. While we observe a long-term increase in the consumption of psychotropic drugs, we do not observe a clear acceleration or excess consumption across the board after the onset of the pandemic. Some differences emerge, however, by class of medication. We do observe an *increase* in the consumption of antidepressants above and beyond the trends observed prior to the pandemic. Still, we find no large increase—and sometimes a *decrease*—in the consumption of anxiolytics, hypnotics, and sedatives. Although they align with evidence reported for other countries—except in France—as mentioned above, these results are somewhat surprising. We expected stress related to the pandemic to lead to anxiety and stress-related disorders, which would lead themselves to treatment based on hypnotics and sedatives or anxiolytics. We do not observe excess consumption of anxiolytics, and we find some evidence of excess consumption of hypnotics and sedatives in particular population subgroups only. On the contrary, the consumption of antidepressants is generally associated with long-term treatments and was therefore expected to be less likely to show sharp fluctuations in the pandemic period – yet the number of consumers of antidepressants has increased beyond the long-term trends since the beginning of the pandemic in Luxembourg.

Like most other studies, we observe a fall in the purchase of psychotropic drugs in the second quarter of 2020 across all medication classes. We hypothesize that the decrease in pharmacy accessibility (due to stay-at-home orders) explains the decrease in purchases in the period covering the first containment. The decrease in antidepressant consumption is then fully offset in subsequent quarters, showing a higher-than-projected increase in psychotropic drug consumption (both in daily doses sold and in the number of users). This increase in consumption is not observed for anxiolytics and hypnotics and sedatives.

Finally, the analysis suggests asymmetries in the evolution of psychotropic drug consumption according to age, gender, household size and composition, employment status, and income. It is among young people that the relative change in consumption appears to be the largest, while older people do not seem to experience marked changes in consumption – yet the level of psychotropic drug consumption remains much higher among the latter. Evidence of excess consumption is also observed among groups that had initially low use of psychotropic drugs but whose routines were likely affected by sanitary measures.

The rest of this manuscript is structured as follows. In Section 4.2, details about the data and the description of the statistical methodology are presented. Section 4.3 presents the main empirical results, which are discussed and put into perspective with existing international evidence in Section 4.4. Section 4.5 briefly concludes.

4.2 Data and methods

The study explores data on psychotropic drug purchases collected by Luxembourg's social security administration from January 2016 to December 2021. Data from 2016 to 2019 allow us to identify the pre-COVID-19 trends in drug consumption (and the seasonality in purchases) and, therefore, to examine the "excess" consumption following the onset of the pandemic compared to previous trends.

4.2.1 Population and data

Study population Our study population comprises 369,063 Luxembourg residents aged between 18 and 79 at the onset of the pandemic in February 2020 who have continuously been residents and affiliated with Luxembourg's national health insurance throughout 2016–2020.[23] While most residents in Luxembourg are affiliated with the national social security, this selection excludes international civil servants.

Socio-economic characteristics Several socio-economic and demographic characteristics are recorded, namely age and gender, household income, the number of children registered in a household, and country of birth, all measured as of February 2020 (except for household income, for which the most recent available data stems from 2019).[24] The composition of the study population reflects the heterogeneity of the overall resident population. Table 4.1 shows that 47% of individuals in our study population are between 25 and 49 years old, and 33% between 50 and 69. 18 to 24 and 70 to 79 years olds, representing the distribution's tails, account for 11% and 9% of the population, respectively. Furthermore, 29% of the population has at least one child under 15. Additionally, individuals belong to heterogeneous households: 17% live alone, while 56% live with at least two other persons. As Luxembourg is a high immigration country, only half of our study population was born in Luxembourg. Finally, 46% of individuals work as private sector employees, 8% are civil servants, 5% are self-employed, and the rest are inactive. Slightly more than two-thirds of the population has an annual household

[23]A few individuals for whom socio-economic characteristics—notably household income and employment status—could not be found in social security records have, however, been excluded.

[24]See Van Kerm et al. (2022) for details on the data extraction.

income between 25,000 and 60,000 euros, while the remaining share constitutes the lower and upper extremes.

Table 4.1: Characteristics of the study population

	Percentage	N
Gender		
Male	50	185,958
Female	50	183,105
Age Group		
18–24	11	40,989
25–49	47	172,590
50–69	33	123,593
70–79	9	31,891
Number of children (aged <15)		
No child	71	262,817
1 child	15	55,879
2 children	11	39,383
3+ children	3	10,984
Household Size		
1 person	17	61,749
2 persons	27	99,778
3+ persons	56	207,536
Birthplace		
Luxembourg	53	195,206
Belgium/France/Germany	12	42,585
Portugal	15	55,698
Ex-YU	3	10,739
Other EU	7	26,832
Other	10	38,003
Activity Status		
Private employee	46	169,873
Civil servant	8	30,058
Self-employed	5	18,072
Inactive adult	27	99,498
Inactive elderly person	14	51,562
Income Group (Household)		
Less than 25 000 euros	12	42,735
25 to 40 000 euros	34	125,895
40 to 60 000 euros	36	133,271
More than 60 000 euros	18	67,162
Individuals	369,063	369,063

Source: Inspection générale de la sécurité sociale (IGSS)

Psychotropic drug purchases and consumers For each individual in the study population, the study considers quarterly purchases from January 2016 to December 2021 of psychotropic drugs partially or fully reimbursed by the national health insurance (CNS) in non-hospital pharmacies.

We consider three categories of psychotropic drugs: (i) anxiolytics, (ii) hypnotics and sedatives, and (iii) antidepressants. Practically, anxiolytics are all drugs classified as N05B in the Anatomical Therapeutic Chemical (ATC) Classification System. Hypnotics and sedatives are drugs classified as N05C. Antidepressants are drugs classified as N06A. While anxiolytics and sedatives are medications generally aimed at quickly alleviating and soothing mental health symptoms, antidepressants are usually delivered in longer-term treatments of depressive pathologies. The purchase of these psychotropic drugs is strictly subject to obtaining a prescription from a

Table 4.2: Psychotropic drug consumption in the study population

	Average DDDs	Percentage	Average DDDs consumed among consumers
All psychotropics	12.22	10.66	114.71
2016	11.15	9.73	114.56
2017	11.51	10.02	114.85
2018	11.79	10.43	113.04
2019	12.35	10.91	113.25
2020	13.07	11.21	116.60
2021	13.47	11.64	115.74
Anxiolytics (N05B)	2.87	4.54	63.25
2016	2.61	4.17	62.45
2017	2.73	4.36	62.64
2018	2.81	4.49	62.60
2019	2.94	4.64	63.33
2020	3.06	4.73	64.68
2021	3.07	4.82	63.64
Hypnotics and sedatives (N05C)	3.87	3.63	106.85
2016	3.67	3.27	112.18
2017	3.78	3.34	113.29
2018	3.75	3.53	106.19
2019	3.85	3.74	102.88
2020	4.10	3.89	105.56
2021	4.09	3.99	102.67
Antidepressants (N06A)	5.48	5.67	96.63
2016	4.88	5.17	94.22
2017	5.00	5.29	94.43
2018	5.23	5.48	95.50
2019	5.56	5.77	96.42
2020	5.91	5.99	98.66
2021	6.31	6.33	99.69
Individuals	369,063	369,063	369,063

Note: Psychotropic drugs encompass quarterly averages in Defined Daily Doses (DDDs) of psychotropic drugs in selected ATC classes that were purchased in a public pharmacy established in Luxembourg from 2016 to 2021 and (partially) reimbursed by the national health insurance or the accident insurance to affiliates of the Luxembourgish health insurance system. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.

physician.

Quantities of drugs purchased are measured in uniform units of Defined Daily Doses (DDDs), which provide a comparable metric across medication types.[25] They are aggregated for each of the three classes of drugs per quarter. For much of the study, we focus on how psychotropic drug purchase quantities – measured in DDDs – evolved during the pandemic. To assess the prevalence of mental health pathologies, we also examine the evolution of the number of consumers—where a consumer (in quarter q) is any person for whom a purchase is recorded during quarter q, independently of the quantity purchased.

The average consumption of psychotropic drugs per individual by quarter in our study population is 2.87 DDDs of anxiolytics, 3.87 DDDs of hypnotics and sedatives, and 5.48 DDDs of antidepressants; see Table 4.2. Anxiolytics are consumed by 4.54 percent of our study population in an average quarter, hypnotics and sedatives by 3.63 percent and antidepressants by 5.67 percent. All drugs combined, our study population consumes, on average, 12.22 DDDs of psychotropic medications per quarter, and 10.66 percent of individuals (approximately 39,340 people) have at least one drug purchase in an average quarter. In total, more than 4.5 million DDDs of psychotropic medications were consumed each quarter in Luxembourg in the period 2016–2021. Antidepressants constitute almost half of these average daily doses.

Figure 4.1 shows the evolution of psychotropic drug consumption in DDDs since 2016 in our study population. The consumption of all psychotropic drugs seems to increase in the long term. The increase is most pronounced for antidepressants, from less than five DDDs in 2016 to more than six DDDs per individual a quarter by the end of 2021. To a large extent, the increase in consumption is driven by upward trends in the number of consumers. Note that we are examining the evolution of consumption in a fixed study population, so the increase in consumption may be due to the group's aging over time.

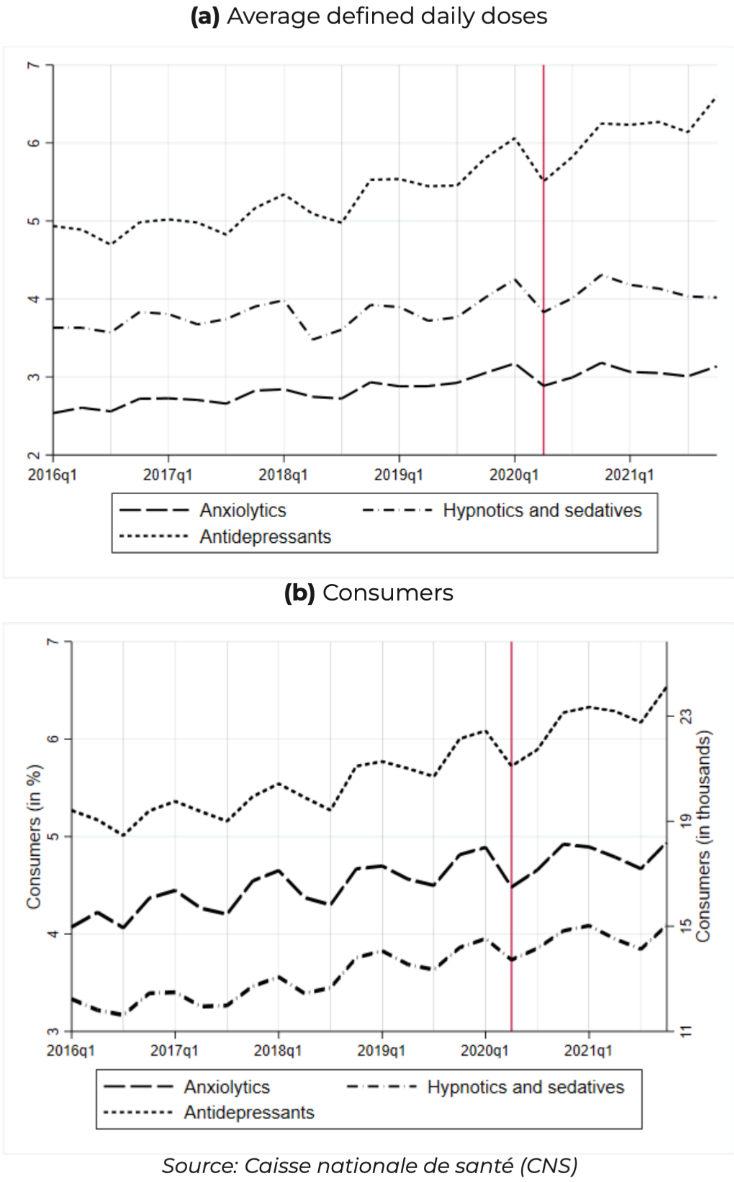
The consumption of psychotropic drugs dropped in the second quarter of 2020 – during which the first confinement mainly occurred. The consumption of hypnotics, sedatives, and anxiolytics did not seem to catch up with the long-term trend in the following quarters. Conversely, while the consumption of antidepressants fell significantly between the first quarter of 2020 and the second quarter of 2020, it rose subsequently to peak at the end of 2021.

Statistics on the consumption of psychotropic drugs by population subgroups before and during the pandemic are provided in Table 4.3. Unsurprisingly, large consumption differences can be observed by age and gender. Consumption of psychotropic drugs is much higher among elderly populations. For all three drug classes, more than 10 percent of the 70–79 are observed to consume psychotropic drugs in any quarter. Women are also fifty percent more likely than men to be consumers of anxiolytics and one hundred percent more likely to be consumers of antidepressants and hypnotics and sedatives. There is also a moderate gradient in consumption by income, with higher consumption among individuals living in a household with lower income.

Comparison of the period 2016Q1–2020Q1 ('Before' the pandemic) to the period 2020Q2–2021Q4 ('During' the pandemic) reveals that the consumption of psychotropic drugs has been on the increase in all groups and for

[25] A Defined Daily Dose is defined by the World Health Organization as "The assumed average maintenance dose per day for a drug used for its main indication in adults" (https://www.who.int/tools/atc-ddd-toolkit/about-ddd). Expressing medication in DDDs allows comparisons and aggregation of doses consumed over time across drug types (within a class) and concentrations.

Figure 4.1: Evolution of psychotropic drugs consumption, 2016–2021, by quarters



Source: Caisse nationale de santé (CNS)

Note: Psychotropic drugs encompass quarterly averages in Defined Daily Doses (DDDs) of psychotropic drugs in selected ATC classes that were purchased in a public pharmacy established in Luxembourg from 2016 to 2021 and (partially) reimbursed by the national health insurance or the accident insurance to affiliates of the Luxembourgish health insurance system. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.

all three classes of drugs. The consumption growth is substantial among the youngest (18–24), among men, and in families with children. To be clear, this growth *may* be due to the emergence of the pandemic, but it may also be due to a long-term trend towards higher medication for mental health or the aging over time of the study population. We estimate the 'excess consumption' that may be attributed to the pandemic in Section 4.3.

4.2.2 Statistical methods

We consider two statistical models to estimate the growth in psychotropic drug consumption that may be attributed to the pandemic.

Estimation of excess consumption First, we document the evolution of psychotropic drug use by the ATC class since 2016 and focus on the distinctive patterns that emerge following the onset of COVID-19. Since a part of the increase observed after February 2020 may reflect a trend that began before the emergence of COVID-19, we control for the long-term trend (and seasonality) and quantify the "excess consumption" above and beyond consumption that would be predicted from pre-pandemic trends. To do so, we fit a regression model considering the cyclical behavior of COVID-19, consisting of waves and peaks.

We estimate the parameters of the following regression equation

$$y_{iq} = \alpha + g(q) + S_q\,\eta + D_q\delta + e_{iq} \tag{4.1}$$

where

- y_{iq} is either the purchases of a particular ATC class of psychotropic drug expressed in DDDs by individual i during quarter q, or a binary indicator equal to 1 if individual i had any drug purchase in quarter q, and 0 otherwise;
- $g(q)$ captures the (pre-COVID-19) trend in psychotropic drug consumption and avoids attributing to COVID-19 part of a long-term 'secular' increase in the consumption of psychotropic drugs. We set

  $$g(q) = \gamma q$$

  (linear trend) but find similar results when

  $$g(q) = \gamma_1 q + \gamma_2 q^2$$

  (quadratic trend). $g(0)$ is normalized to 0 for the last quarter pre-COVID-19 by numbering quarters from the first quarter of 2020 for which $q = 0$ ($q = 1$ for the second quarter of 2020 until $q = 7$ for the fourth quarter of 2021 and $q = -1$ corresponds to the fourth quarter of 2019, down to $q = -16$ for the first quarter of 2016). Note that the function $g(q)$ also captures the effect on drug consumption of the aging of the individuals in the data, alongside a long-term increase in usage;
- S_q is a vector of three seasonal dummies (for the second, third, and fourth quarter of every year);

Table 4.3: Consumption of psychotropic drugs by sociodemographic subgroups

	Consumption (in DDDs)				Consumers (in %)			
	Before	During	Difference	Change (%)	Before	During	Difference	Change (%)
Anxiolytics (N05B)								
All population	2.80	3.05	0.25	9.03	4.44	4.76	0.32	7.12
Male	2.16	2.38	0.22	10.29	3.32	3.61	0.28	8.46
Female	3.44	3.73	0.28	8.22	5.58	5.93	0.35	6.31
18–24	0.21	0.48	0.27	126.79	0.66	0.98	0.31	47.11
25–49	1.36	1.62	0.26	19.13	2.63	2.97	0.34	13.11
50–69	4.50	4.77	0.27	5.99	6.48	6.72	0.24	3.70
70–79	7.28	7.40	0.12	1.66	11.22	11.69	0.47	4.15
No child	3.47	3.73	0.25	7.21	5.19	5.50	0.31	5.99
1 child	1.29	1.60	0.31	24.03	2.86	3.18	0.32	11.25
2 children	0.92	1.11	0.19	21.00	2.31	2.61	0.30	12.98
3+ children	0.92	1.13	0.21	23.11	2.30	2.78	0.48	20.92
Less than 25 000 euros	4.66	4.95	0.28	6.03	5.49	5.99	0.51	9.29
25 to 40 000 euros	3.36	3.65	0.29	8.66	5.14	5.46	0.32	6.15
40 to 60 000 euros	2.45	2.70	0.25	10.26	4.28	4.56	0.28	6.61
More than 60 000 euros	1.24	1.40	0.16	13.20	2.79	3.05	0.26	9.32
Luxembourg	3.51	3.75	0.24	6.87	5.01	5.29	0.28	5.51
Belgium/France/Germany	2.30	2.50	0.20	8.90	3.79	4.11	0.32	8.46
Portugal	2.51	2.78	0.27	10.93	4.91	5.18	0.27	5.44
Ex-YU	2.61	3.10	0.49	18.75	4.88	5.48	0.60	12.28
Other EU	1.46	1.73	0.28	18.94	2.91	3.36	0.45	15.38
Other	1.12	1.37	0.25	22.19	2.52	2.93	0.42	16.60
Hypnotics and sedatives (N05C)								
All population	3.79	4.07	0.28	7.46	3.50	3.93	0.44	12.45
Male	2.63	2.94	0.31	11.88	2.49	2.89	0.39	15.82
Female	4.97	5.22	0.25	5.08	4.52	4.99	0.48	10.56
18–24	0.07	0.16	0.08	110.63	0.15	0.26	0.10	66.81
25–49	1.17	1.43	0.26	21.83	1.20	1.46	0.26	21.56
50–69	6.20	6.61	0.42	6.70	5.61	6.24	0.63	11.31
70–79	13.43	13.61	0.17	1.29	12.04	13.08	1.04	8.68
No child	4.88	5.16	0.28	5.82	4.39	4.89	0.49	11.26
1 child	1.36	1.72	0.36	26.27	1.51	1.82	0.30	20.18
2 children	0.81	0.97	0.16	19.54	1.07	1.32	0.25	23.26
3+ children	0.88	1.20	0.32	36.51	0.96	1.32	0.36	37.67
Less than 25 000 euros	4.17	4.42	0.25	6.07	3.45	3.96	0.51	14.74
25 to 40 000 euros	4.34	4.60	0.25	5.86	3.82	4.26	0.44	11.39
40 to 60 000 euros	3.96	4.30	0.34	8.58	3.68	4.12	0.45	12.11
More than 60 000 euros	2.18	2.42	0.24	11.04	2.57	2.94	0.37	14.43
Luxembourg	4.91	5.20	0.28	5.77	4.27	4.75	0.47	11.10
Belgium/France/Germany	3.08	3.41	0.34	10.95	3.14	3.57	0.42	13.49
Portugal	3.09	3.36	0.27	8.79	3.16	3.59	0.42	13.35
Ex-YU	1.90	2.22	0.32	16.58	1.99	2.31	0.32	16.18
Other EU	2.10	2.35	0.25	11.91	2.15	2.49	0.34	15.75
Other	1.60	1.85	0.25	15.57	1.78	2.15	0.37	20.67
Antidepressants (N06A)								
All population	5.22	6.12	0.90	17.18	5.47	6.17	0.70	12.86
Male	3.53	4.18	0.65	18.28	3.69	4.22	0.54	14.55
Female	6.93	8.08	1.15	16.62	7.27	8.15	0.87	11.99
18–24	0.88	1.59	0.71	81.18	1.07	1.76	0.69	64.75
25–49	3.35	4.28	0.93	27.85	3.59	4.35	0.76	21.04
50–69	8.15	8.90	0.75	9.18	8.36	8.78	0.43	5.09
70–79	9.59	11.11	1.52	15.83	10.05	11.55	1.50	14.97
No child	6.06	6.99	0.92	15.22	6.27	6.96	0.69	11.07
1 child	3.49	4.36	0.87	24.91	3.87	4.60	0.73	18.83
2 children	2.83	3.61	0.78	27.72	3.16	3.93	0.76	24.11
3+ children	2.42	3.24	0.82	33.85	2.72	3.31	0.59	21.64
Less than 25 000 euros	6.29	7.25	0.97	15.41	6.41	7.05	0.63	9.85
25 to 40 000 euros	5.82	6.75	0.94	16.10	6.02	6.73	0.71	11.80
40 to 60 000 euros	5.10	5.96	0.86	16.78	5.38	6.07	0.69	12.89
More than 60 000 euros	3.66	4.52	0.86	23.44	4.00	4.75	0.75	18.88
Luxembourg	5.88	6.82	0.94	15.94	6.00	6.73	0.73	12.17
Belgium/France/Germany	4.29	5.07	0.79	18.38	4.57	5.26	0.69	15.12
Portugal	6.67	7.69	1.03	15.39	7.15	7.84	0.68	9.54
Ex-YU	4.00	4.34	0.34	8.54	4.46	4.63	0.17	3.80
Other EU	3.22	4.18	0.96	29.72	3.47	4.26	0.78	22.60
Other	2.52	3.26	0.74	29.27	2.97	3.68	0.70	23.69
Observations	6,274,071	6,274,071	6,274,071	6,274,071	6,274,071	6,274,071	6,274,071	6,274,071
Individuals	369,063	369,063	369,063	369,063	369,063	369,063	369,063	369,063

Note: Psychotropic drugs encompass quarterly averages in Defined Daily Doses (DDDs) of psychotropic drugs in selected ATC classes that were purchased in a public pharmacy established in Luxembourg from 2016 to 2021 and (partially) reimbursed by the national health insurance or the accident insurance to affiliates of the Luxembourgish health insurance system. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System. The 'Before' column refers to consumption in the period 2016Q1–2020Q1. The 'During' column refers to the period 2020Q2–2021Q4.

– D_q is a vector of seven quarter-year dummies for the seven quarters with $q > 0$, i.e., from the second quarter of 2020 onwards. The coefficients vector δ measures the quarter-specific deviation beyond the pre-COVID-19 trend $g(q)$ and the seasonal effect, a measure that we interpret as an indicator of 'excess consumption';

– e_{iq} is a residual, idiosyncratic term.

Separate models are estimated for each ATC class (N05B, N05C, and N06A). Cluster-robust standard errors account for the correlation of observations over time for the same individuals across each model.[26]

Assessing the impact of the severity of containment measures Secondly, we attempt to capture how much of the evolution of consumption is driven by public health policies and the severity of containment measures. To do so, we examine the association between drug consumption and the evolution of the Stringency Index (hereafter: SI). The SI is a composite index from the Oxford COVID-19 Government Response Tracker (OxCGRT) that combines nine different metrics into a single index to provide insight into the strictness of Government policies. The metrics encompass restrictive measures against individuals, such as closing schools, living and working places, banning gatherings, closing public transport, or even stay-at-home orders. The index ranges from 0 to 100, suggesting significant restrictive measures when the index is close to 100.

We aim to relate the measures of excess consumption to our measures of the severity of COVID-19. To do so, we estimate a regression equation similar to Equation (4.1) in which the Stringency Index replaces the dummies in the COVID-19 period:

$$y_{iq} = \alpha + g(q) + S_q\,\eta + \mu\,Q1_q + \sum_{k=0}^{K} \varrho_k\,\mathrm{SI}_{q-k} + e_{iq} \tag{4.2}$$

where the new variables are as follows

– the dummy variable $Q1_q$ is kept in the regression to capture the specific situation of the early period, it is a dummy variable equal to 1 if $q = 1$ only and μ captures the drop in consumption observed in the second quarter of 2020 that may be due to restrictions in access to medical treatment and pharmacies and may deviate from the linear trend;

– SI_{q-k} is the k-th lag of the Stringency Index at quarter q with $\mathrm{SI}_q = 0$ for all $q < 1$. Models are fitted with $K \in \{0, 1\}$.

With this specification, parameters estimates of ϱ_k capture the extent to which psychotropic drug consumption responded to the stringency of sanitary measures – with ϱ_0 capturing the contemporaneous quarterly association between drug purchases and average stringency, ϱ_1 capturing the association between drug purchases and average stringency in the previous quarter, etc.

Heterogeneity by socio-economic characteristics To assess variations in the evolution of psychotropic drug consumption during the pandemic by socio-economic characteristics, we take advantage of our large

[26] Restricted versions of Equation (4.1) were also estimated for the sake of completeness – excluding the seasonal dummies, excluding the time trend, and combining the pandemic quarters into a simple pre-/post dummy.

datasets and re-estimate each model using subsamples corresponding to each modality of a range of socio-economic characteristics (all characteristics are categorical). This approach provides estimates of 'excess consumption' during the COVID-19 pandemic for population subgroups.

To facilitate reporting, we present estimates of restricted versions of the regression models in which the six coefficients for the quarters from the third quarter of 2020 onwards are summarized in one common coefficient:

$$y_{iq} = \alpha + g(q) + S_q\,\eta + 2020\mathrm{Q2}_q\delta^{\mathrm{Lock}} + \mathrm{Post}_q\delta^{\mathrm{Post}} + e_{iq} \tag{4.3}$$

where all variables are as in Equation (4.1) except that the six dummy variables D_q are replaced by one dummy variable $2020\mathrm{Q2}_q$ equal to one only for the second quarter of 2020 – during most of which the severe lockdown was in place – and by a dummy variable Post_q equal to one for all quarters from 2020Q3 onwards.

With this specification, the ratios $\frac{\delta^{\mathrm{Lock}}}{\alpha}$ and $\frac{\delta^{\mathrm{Post}}}{\alpha}$ can be conveniently interpreted as, respectively, the increase in consumption observed in the second quarter of 2020 expressed relative to the expected consumption level in the first quarter of 2020 (net of long-term trends and seasonal effects), and the average increase in quarterly consumption observed in the quarters from 2020Q3 onwards expressed relative to the expected consumption level in the first quarter of 2020 (again, net of long-term trends and seasonal effects).

4.3 Results

Given their contrasted evolution, we separately present results for the three ATC classes in order of ATC classification – anxiolytics (N05B), hypnotics and sedatives (N05C), and antidepressants (N06A). As the results will show, most of the evidence of excess consumption during the pandemic quarters is for the latter.

4.3.1 Anxiolytics

Table 4.4 presents results about the quantity and incidence of anxiolytic drug consumption. Columns 1–3 report quantities in DDDs, and Columns 3–6 are for the number of quarterly users. Columns 1 and 4 are results for Model 4.1, while Columns 2, 3, 5, and 6 are for Model 4.2. The distinction between the two models including the Stringency Index is the presence of lags in the specification.

Three main results emerge from coefficient estimates of Columns 1 and 4. First, results confirm the upward trend in anxiolytic drug consumption between 2016 and 2021 depicted in Figure 4.1 (0.0300, SE 0.0021). Second, consumption decreased in the second quarter of 2020, coinciding with the first confinement (–0.1477, SE 0.0264). Consumption fell in this second quarter of 2020 by the equivalent of approximately one year of pre-pandemic trend (four times 0.03). Similarly, the percentage of anxiolytic users dropped by about six quarters of the trend (–0.26, SE 0.03), equivalent to about 960 fewer users. Third, and most importantly, the rest of the pandemic period did *not* show any increase in psychotropic drug consumption beyond the long-term trends. The initial decline in consumption appears long-lasting and even amplifies at the end of 2021 (–0.20, SE 0.04). Patterns are similar whether we look at DDDs or the number of consumers.

Columns 2, 3, 5, and 6 show coefficient estimates for Model 4.2 and lead to similar conclusions. Quarters during which the Stringency Index was high have been associated with lower anxiolytics consumption on average. These results are consistent with the analysis by quarter.

Table 4.4: Effects of COVID-19 on Anxiolytics (N05B) consumption

Margin	Consumption (in DDDs)			Consumers (in %)		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	3.0728**	3.0528**	3.0539**	4.8565**	4.8416**	4.8398**
	(0.0425)	(0.0419)	(0.0419)	(0.0299)	(0.0293)	(0.0293)
Trend	0.0300**	0.0281**	0.0282**	0.0389**	0.0372**	0.0370**
	(0.0021)	(0.0019)	(0.0019)	(0.0017)	(0.0016)	(0.0016)
2020Q2	-0.1477**	-0.0445	-0.0947*	-0.2624**	-0.1407**	-0.0580
	(0.0264)	(0.0296)	(0.0504)	(0.0294)	(0.0307)	(0.0663)
2020Q3	-0.0211			0.0029		
	(0.0294)			(0.0304)		
2020Q4	-0.0012			-0.0661**		
	(0.0319)			(0.0317)		
2021Q1	-0.1269**			-0.1238**		
	(0.0312)			(0.0330)		
2021Q2	-0.1042**			-0.1092**		
	(0.0368)			(0.0351)		
2021Q3	-0.1271**			-0.1406**		
	(0.0380)			(0.0355)		
2021Q4	-0.1674**			-0.2017**		
	(0.0418)			(0.0368)		
Stringency Index		-0.0014**	-0.0006		-0.0018**	-0.0031**
		(0.0006)	(0.0008)		(0.0005)	(0.0010)
Stringency Index (L1)			-0.0008			0.0014
			(0.0007)			(0.0010)
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512
Individuals	369,063	369,063	369,063	369,063	369,063	369,063

Note: Regressions include data on psychotropic drugs purchases in Defined Daily Doses (DDDs) between 2016Q1 and 2021Q4 in Luxembourg. Standard errors, clustered at the individual level, are in parenthesis. Significance levels: * = 10%, ** = 5%, and *** = 1%. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.

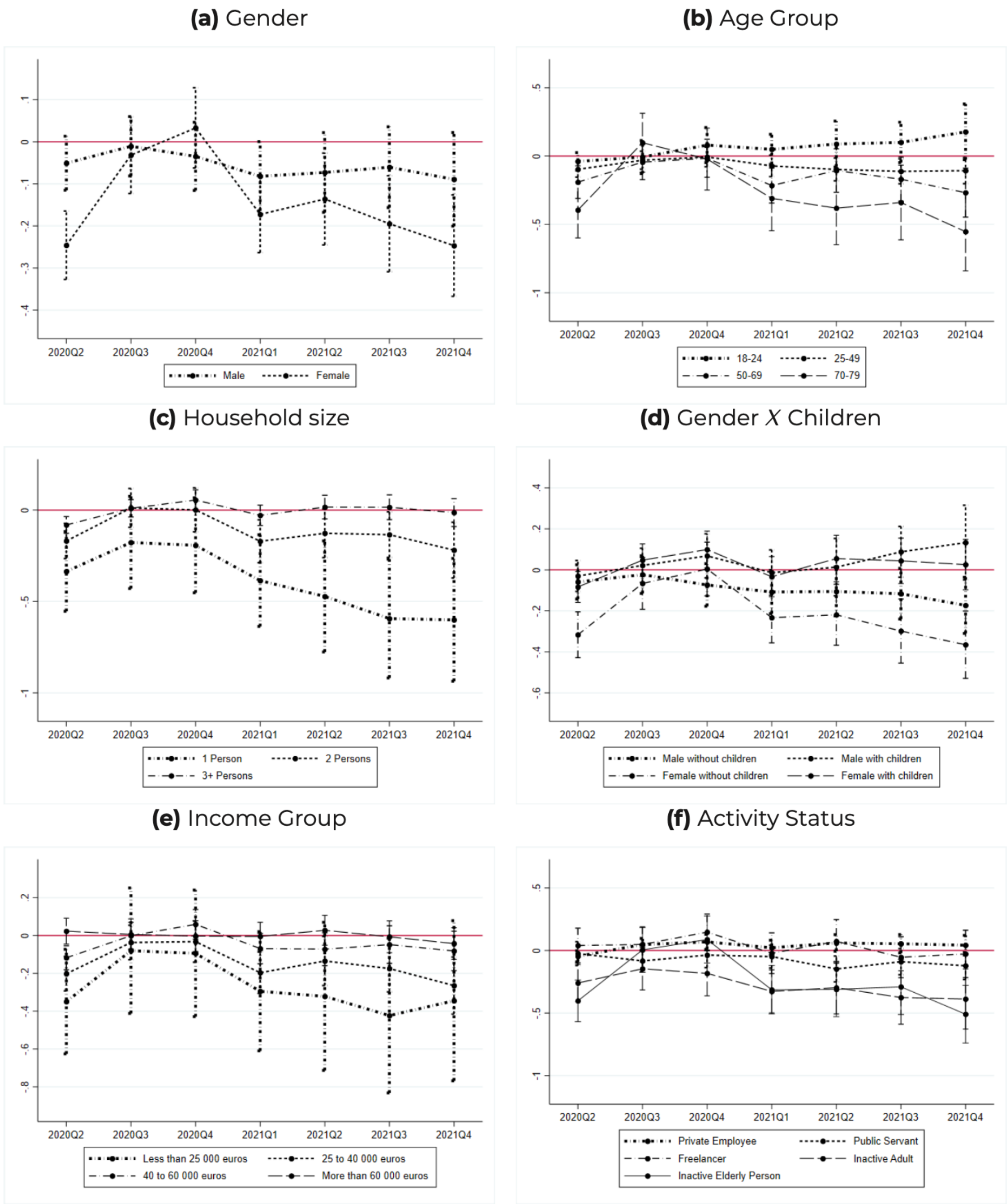
Figure 4.2 shows estimates of the evolution of the deviation of anxiolytics consumption from the trend by population subgroups – estimates of the coefficients δ_q in Equation (4.1). These are obtained here by interacting all variables of the model with socio-demographic variables of interest.

Figure 4.2a shows deviations in anxiolytics consumption according to gender. The decrease occurring in the second quarter of 2020 is more significant for women than men. This difference decreases from the third quarter of 2020 onwards and increases significantly from 2021 onwards.

Figure 4.2b shows the evolution of the deviation of anxiolytics consumption from the trend as a function of age. There is a clear difference according to age, with older people showing a significant negative deviation from the trend. The deviation is negatively related to the age of the individuals, and a gradation of the effect is visible.

Figures 4.2c to 4.2f show the evolution as a function of household size, childcare responsibilities, income, and activity, respectively, this time additionally controlling for the gender and age of individuals. Figure 4.2c shows the evolution of consumption by household size, with single persons showing a more significant decline than other

Figure 4.2: Deviations from trend in Anxiolytics (N05B) consumption during COVID-19 by population subgroups



Note: Regressions are performed for each subpopulation. Figures (c)-(f) include additional controls for age and gender. Regressions include data on psychotropic drug purchases in Defined Daily Doses (DDDs) between 2016Q1 and 2021Q4 in Luxembourg. Standard errors are clustered at the individual level. Lower and upper bounds of 95% confidence intervals are represented. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.

households. Figure 4.2d looks at the effect of the interaction between parenthood and gender on the consumption of anxiolytics. Unsurprisingly, women with children saw their consumption decrease during the pandemic more than men with children. On the other hand, men or women without children did not deviate significantly from the trend during the pandemic.

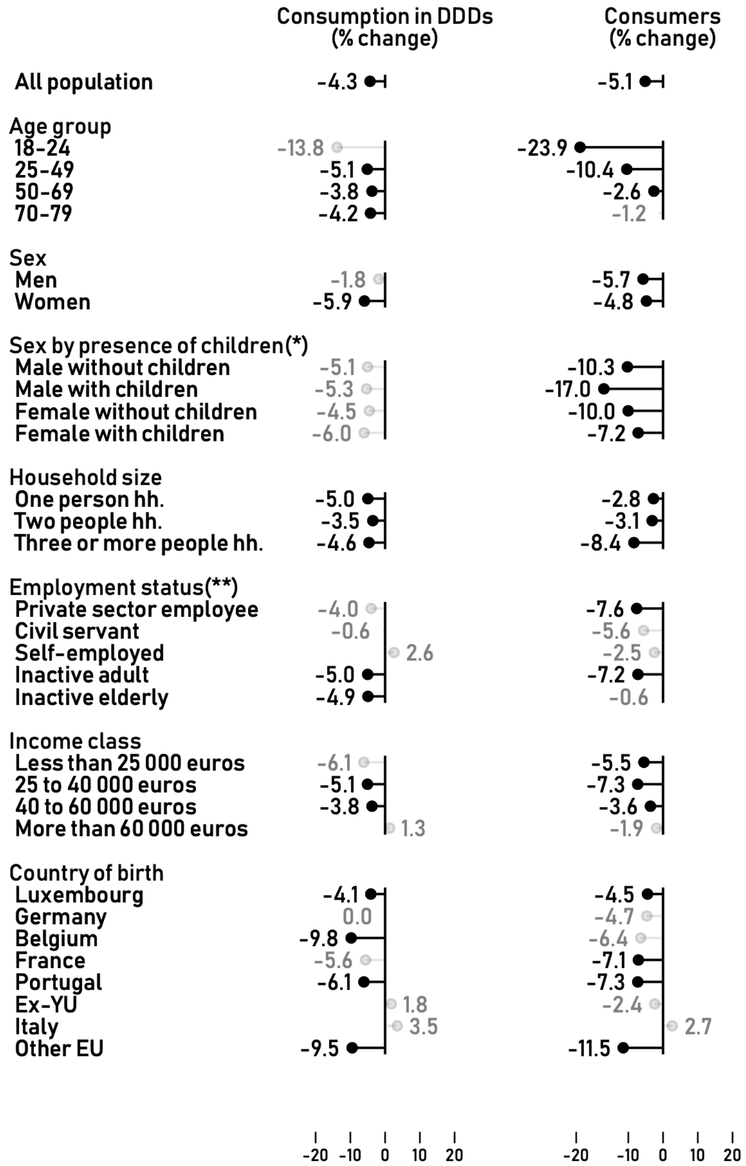
Similarly, income is related to variations in the consumption of anxiolytics; the lower the income, the greater the decline in consumption. Finally, while employed persons in the private or public sector show remarkable similarities in deviation, inactive and elderly persons show more significant deviations and see their consumption of psychotropic drugs decrease compared to the trend.

These detailed patterns are summarized in Figures 4.3 and 4.4 – both in defined daily doses (left) and in the percentage of consumers (right). To facilitate interpretation, since the pre-pandemic level of consumption varies widely across subgroups (cf. Table 4.3), estimates reported in Figures 4.3 and 4.4 express quarterly excess consumption in 2020Q2 or from the third quarter of 2020 as a percentage of expected consumption in the first quarter of 2020 using estimates of the ratios $\frac{\delta^{\text{Lock}}}{\alpha}$ and $\frac{\delta^{\text{Post}}}{\alpha}$ from Equation (4.3). Here estimates are obtained by estimating Equation (4.3) separately for each population subgroup of interest.

The general decline in consumption in 2020Q2 is confirmed in all population groups in Figure 4.3. The decline is more marked in the number of users rather than the overall quantities consumed. Some subgroups (e.g., the 18–24 and 25–49 year-olds) saw more than a ten percent reduction in the percentage of consumers in the second quarter of 2020.

The consumption of anxiolytics remained below the expectations from long-term trends in subsequent quarters. In no subgroups do we observe a statistically significant "excess consumption". (The seemingly large increase of quantities consumed in the 18–24 age group is not statistically significant from zero – the baseline consumption in this group being very low.) In most cases, the excess consumption relative to 2020Q1 is not statistically significantly different from zero – some groups even saw significant declines in anxiolytic consumption (notably, and surprisingly, men, individuals living alone, and the inactive).

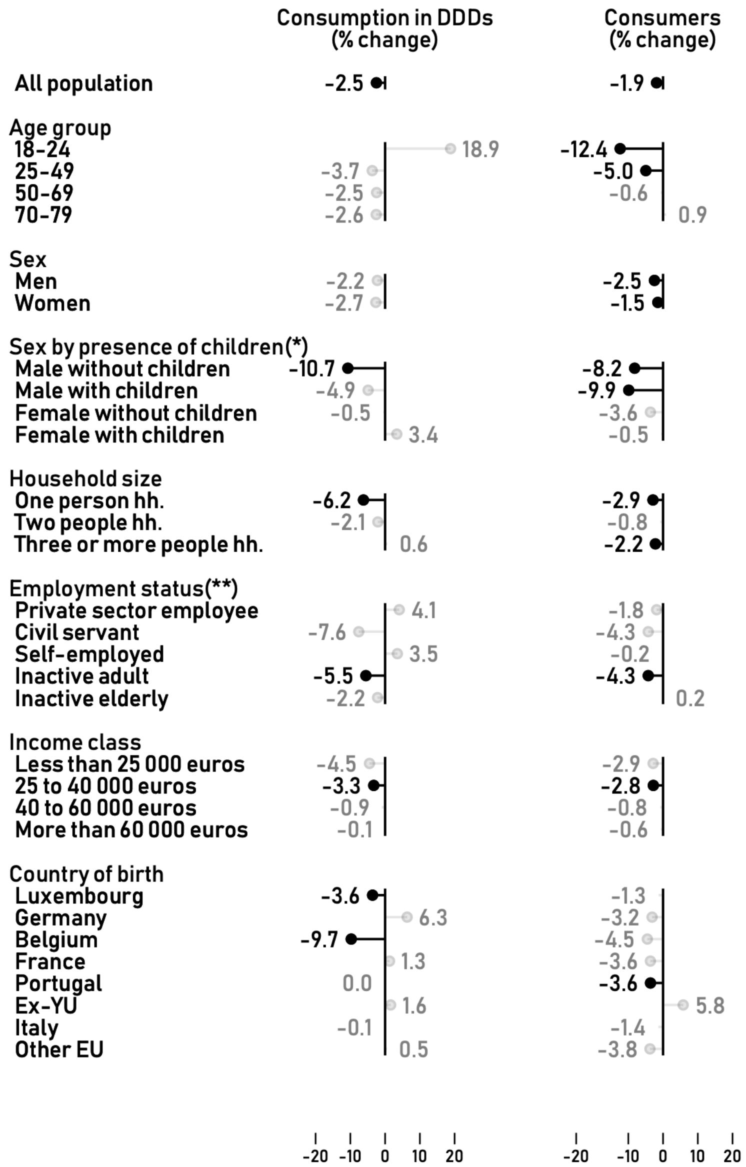
Figure 4.3: Anxiolytics consumption in 2020Q2 by population subgroups



Source: Caisse nationale de santé (CNS)

Note: Psychotropic drugs encompass quarterly averages in Defined Daily Doses (DDDs) of psychotropic drugs in selected ATC classes that were purchased in a public pharmacy established in Luxembourg from 2016 to 2021 and (partially) reimbursed by the national health insurance or the accident insurance to affiliates of the Luxembourgish health insurance system. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System. Estimates shown are 100 times the ratio of the coefficient of excess consumption in quarter 2020Q2 over expected consumption in 2020Q1. Estimates in light grey are not statistically different from zero at a one percent significance level. (*) Subsamples include only individuals aged between 25 and 49. (**) Subsamples include only individuals aged between 18 and 69.

Figure 4.4: Quarterly excess consumption of Anxiolytics in the period 2020Q3–2021Q4 by population subgroups



Source: Caisse nationale de santé (CNS)

Note: Psychotropic drugs encompass quarterly averages in Defined Daily Doses (DDDs) of psychotropic drugs in selected ATC classes that were purchased in a public pharmacy established in Luxembourg from 2016 to 2021 and (partially) reimbursed by the national health insurance or the accident insurance to affiliates of the Luxembourgish health insurance system. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System. Estimates shown are 100 times the ratio of the coefficient of excess consumption per quarter in the period 2020Q3–2021Q4 over expected consumption in 2020Q1. Estimates in light grey are not statistically different from zero at a one percent significance level. (*) Subsamples include only individuals aged between 25 and 49. (**) Subsamples include only individuals aged between 18 and 69.

4.3.2 Hypnotics and sedatives

Table 4.5 shows results regarding the purchases of *hypnotics and sedatives* in the same model as Table 4.4. The doses consumed and the incidence of consumers again exhibit an increasing trend (DDDs: 0.0189, SE 0.0025; Users: 0.04, SE 0.00). The quantities purchased did not drop in the second quarter of 2020, but the number of consumers declined significantly in that quarter (–0.06, SE 0.02).

The consumption trajectory during COVID-19 then seems non-linear and appears to follow an M-shape from mid-2020 to the end of 2021. Consumption rose significantly in the quarters following the second quarter of 2020 but shows a significant decline towards the end of 2021, as well as in the first quarter of 2021. The downward deviation in the last quarter of observations is large – equivalent to 9 quarters of trend, that is going back more than two years in consumption. Similarly, while the number of consumers rose more than expected in the second half of 2020, it significantly declined from 2021 onwards.

The contrasted results from columns 2, 3, 4, and 5 suggest that the M-shape observed for the consumption of hypnotics and sedatives does not appear to be mapped to the evolution of the Stringency Index in a meaningful way.

Table 4.5: Effects of COVID-19 on Hypnotics and Sedatives (N05C) consumption

Margin	Consumption (in DDDs)			Consumers (in %)		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	4.0660**	4.0413**	4.0371**	3.9328**	3.9212**	3.9189**
	(0.0516)	(0.0508)	(0.0508)	(0.0293)	(0.0288)	(0.0289)
Trend	0.0189**	0.0174**	0.0169**	0.0408**	0.0392**	0.0390**
	(0.0025)	(0.0023)	(0.0024)	(0.0015)	(0.0014)	(0.0014)
2020Q2	0.0158	-0.1427**	0.0499	-0.0613**	-0.0444*	0.0589
	(0.0295)	(0.0329)	(0.0610)	(0.0221)	(0.0237)	(0.0481)
2020Q3	0.1505**			0.0660**		
	(0.0320)			(0.0231)		
2020Q4	0.1990**			0.0070		
	(0.0343)			(0.0243)		
2021Q1	0.0386			-0.0174		
	(0.0366)			(0.0258)		
2021Q2	0.2420**			-0.0094		
	(0.0415)			(0.0275)		
2021Q3	0.0966**			-0.1046**		
	(0.0424)			(0.0280)		
2021Q4	-0.1671**			-0.0989**		
	(0.0456)			(0.0294)		
Stringency Index		0.0024**	-0.0007		-0.0001	-0.0017**
		(0.0006)	(0.0010)		(0.0004)	(0.0008)
Stringency Index (L1)			0.0032**			0.0017**
			(0.0009)			(0.0007)
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512
Individuals	369,063	369,063	369,063	369,063	369,063	369,063

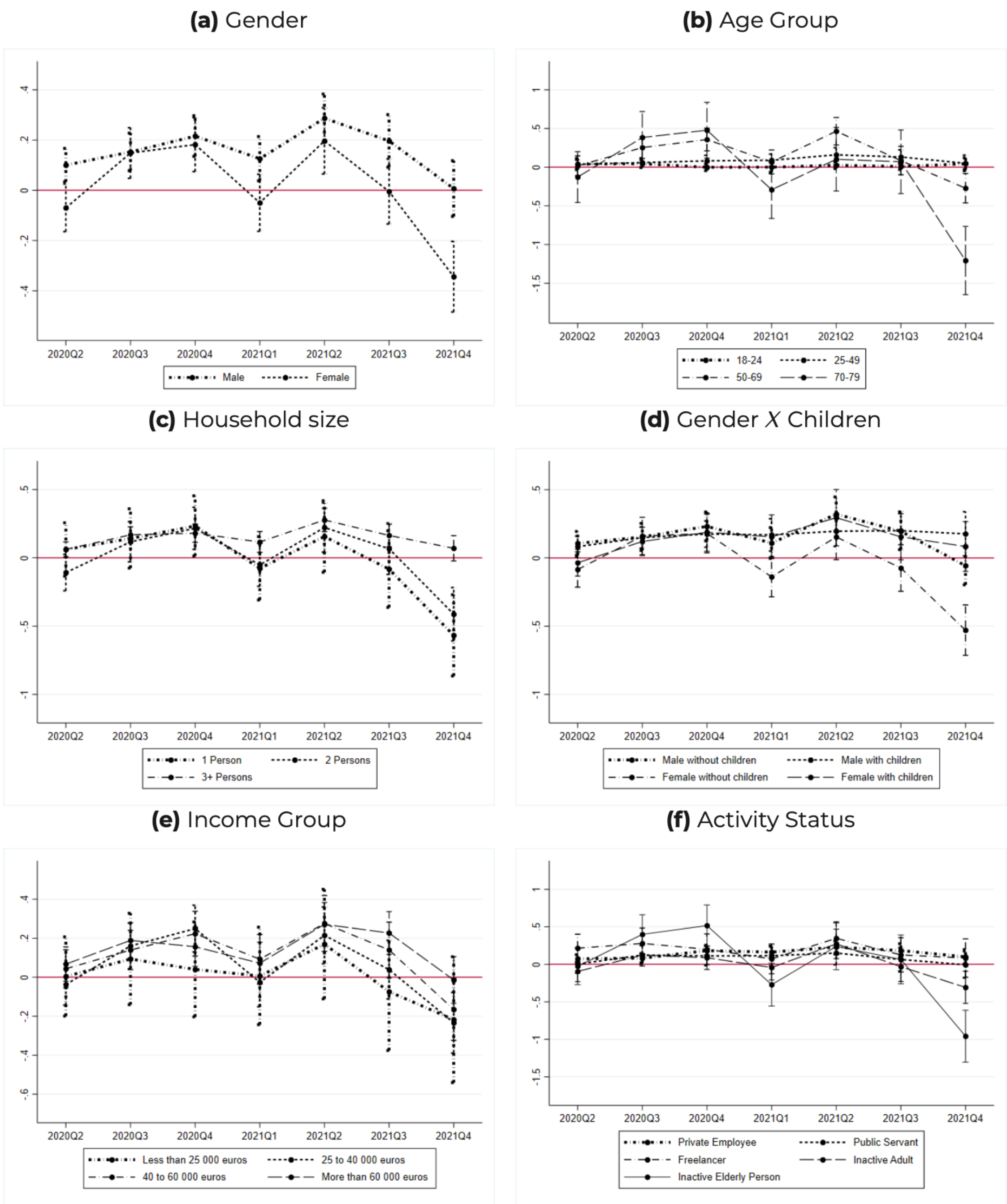
Note: Regressions include data on psychotropic drugs purchases in Defined Daily Doses (DDDs) between 2016Q1 and 2021Q4 in Luxembourg. Standard errors, clustered at the individual level, are in parenthesis. Significance levels: * = 10%, ** = 5%, and *** = 1%. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.

The M-shape evolution is observed within population subgroups too. Figure 4.5 shows the deviations in quarterly consumption of hypnotics and sedatives in selected population subgroups. The consumption of hypnotics and sedatives is generally increasing relative to the trend in quarters 3 and 4 of 2020, while it falls during 2021. This upward deviation in the first or second quarter after the second quarter of 2020 is observed across several figures but quickly declines in 2021Q1 and turns to a negative deviation by the end of 2021.

Figure 4.6 confirms the absence of any systematic change in hypnotics and sedatives consumption in the second quarter of 2020. Summarizing the period from 2020Q3 onwards in just one coefficient, Figure 4.7 indicates an increase in the consumption of hypnotics and sedatives in most subgroups. However, this relative increase is often not statistically different from zero. Interestingly, the overall increase in consumption appears statistically significantly higher in subgroups considered relatively advantaged, e.g., working-age individuals, men, men living with children, private sector employees, and individuals living in high-income households.

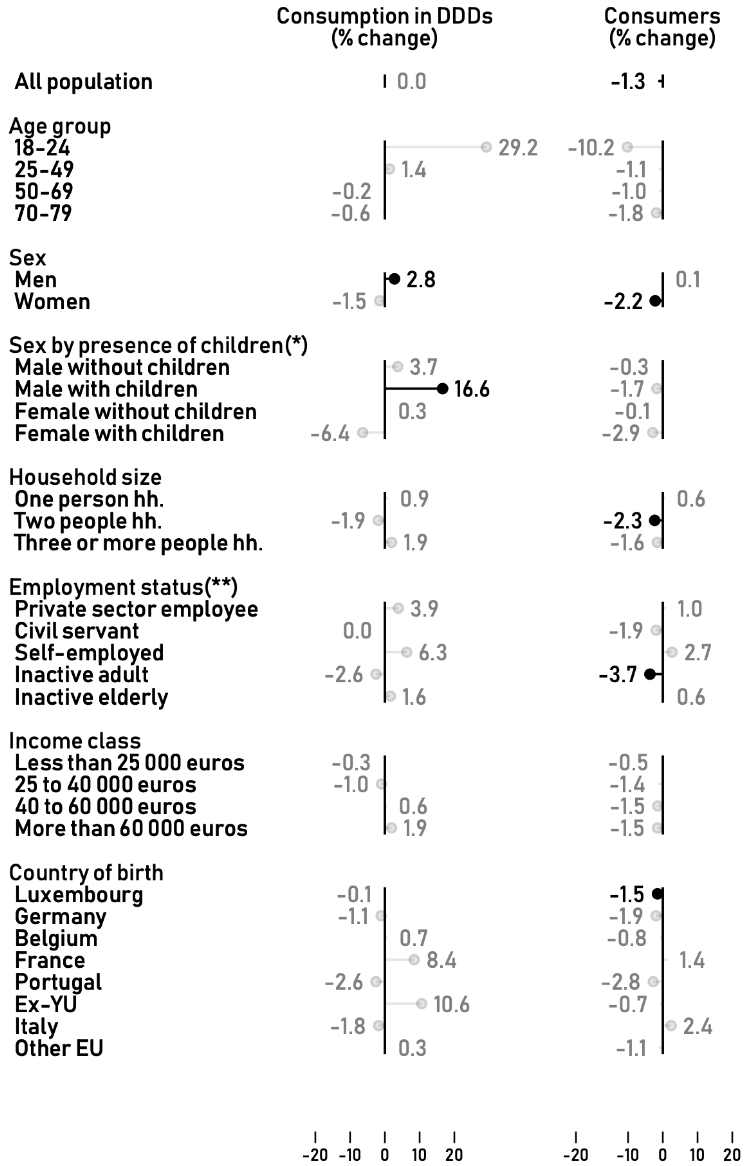
In sum, deviations from a long-term trend are observed in the consumption of hypnotics and sedatives, but purchases appear to have varied across quarters in non-trivial ways. In the pandemic period taken together, the overall excess consumption appears small for most —yet not all—population subgroups.

Figure 4.5: Deviations from trend in Hypnotics and Sedatives (N05C) consumption during COVID-19



Note: Regressions are performed for each subpopulation. Figures (c)-(f) include additional controls for age and gender. Regressions include data on psychotropic drug purchases in Defined Daily Doses (DDDs) between 2016Q1 and 2021Q4 in Luxembourg. Standard errors are clustered at the individual level. Lower and upper bounds of 95% confidence intervals are represented. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.

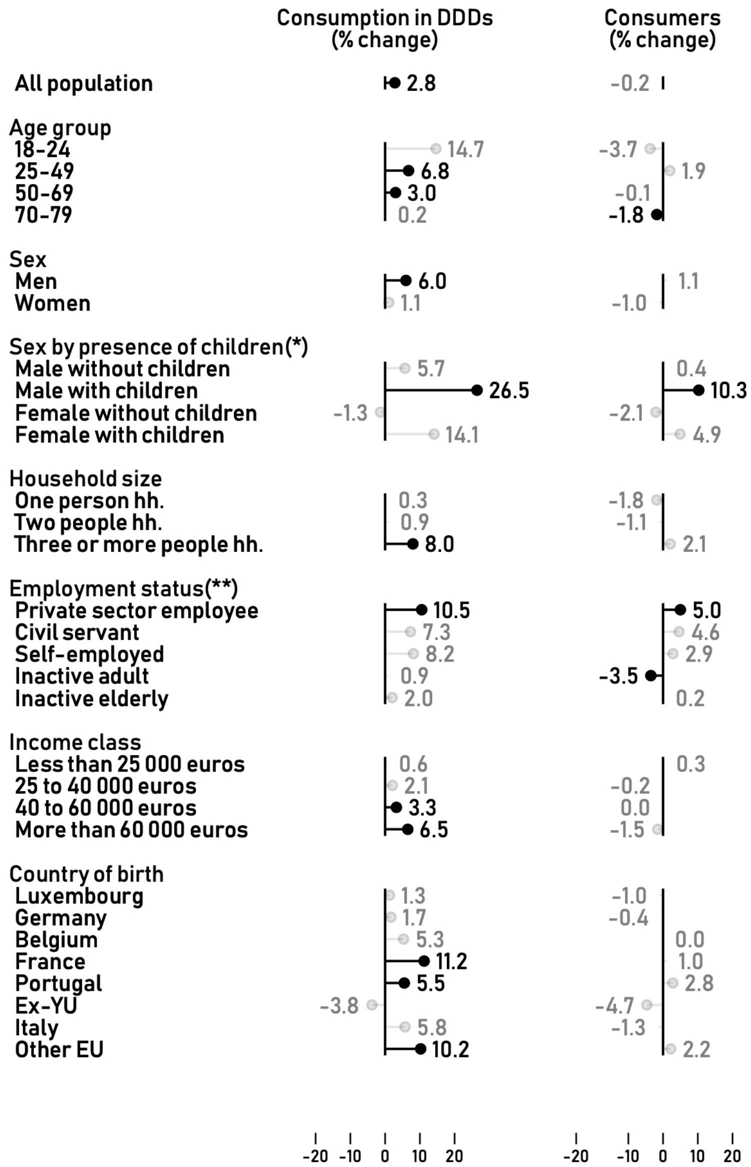
Figure 4.6: Hypnotics and Sedatives consumption in 2020Q2 by population subgroups



Source: Caisse nationale de santé (CNS)

Note: Psychotropic drugs encompass quarterly averages in Defined Daily Doses (DDDs) of psychotropic drugs in selected ATC classes that were purchased in a public pharmacy established in Luxembourg from 2016 to 2021 and (partially) reimbursed by the national health insurance or the accident insurance to affiliates of the Luxembourgish health insurance system. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System. Estimates shown are 100 times the ratio of the coefficient of excess consumption in quarter 2020Q2 over expected consumption in 2020Q1. Estimates in light grey are not statistically different from zero at a one percent significance level. Estimates in light grey are not statistically different from zero at a one percent significance level. (*) Subsamples include only individuals aged between 25 and 49. (**) Subsamples include only individuals aged between 18 and 69.

Figure 4.7: Quarterly excess consumption of Hypnotics and Sedatives in the period 2020Q3–2021Q4 by population subgroups



Source: Caisse nationale de santé (CNS)

Note: Psychotropic drugs encompass quarterly averages in Defined Daily Doses (DDDs) of psychotropic drugs in selected ATC classes that were purchased in a public pharmacy established in Luxembourg from 2016 to 2021 and (partially) reimbursed by the national health insurance or the accident insurance to affiliates of the Luxembourgish health insurance system. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System. Estimates shown are 100 times the ratio of the coefficient of excess consumption per quarter in the period 2020Q3–2021Q4 over expected consumption in 2020Q1. Estimates in light grey are not statistically different from zero at a one percent significance level. (*) Subsamples include only individuals aged between 25 and 49. (**) Subsamples include only individuals aged between 18 and 69.

4.3.3 Antidepressants

Finally, Table 4.6 shows results regarding the analysis of *antidepressants*.

Three main results emerge. First, like for the other two classes of psychotropic drugs, the long-term trend is upward-sloping. Second, antidepressants, like other ATC classes of psychotropic drugs, experienced decreases in consumption and users in the second quarter of 2020. But third, unlike in the preceding two classes, the subsequent quarters are marked by significant increases in the consumption of antidepressants above and beyond expected trends. The third quarter of 2020 exhibits an upward deviation equivalent to about three quarters of growth compared to the long-term quarterly trends. The deviation becomes more pronounced in the fourth quarter of 2020, then weakens at the beginning of 2021 and finally rises again, ending 2021 with an upward deviation of about five quarters of trend.

Correspondingly, we observe a relatively clear positive association between the stringency of sanitary measures and the consumption of antidepressants (after controlling for the particular case of the second quarter of 2022) – the Stringency Index coefficient in Table 4.6.

Table 4.6: Effects of COVID-19 on Antidepressants (N06A) consumption

Margin	Consumption (in DDDs)			Consumers (in %)		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	5.8831**	5.8760**	5.8748**	6.0139**	6.0147**	6.0143**
	(0.0440)	(0.0431)	(0.0431)	(0.0357)	(0.0350)	(0.0351)
Trend	0.0630**	0.0655**	0.0653**	0.0515**	0.0530**	0.0530**
	(0.0024)	(0.0022)	(0.0022)	(0.0020)	(0.0019)	(0.0019)
2020Q2	-0.2220**	-0.5030**	-0.4473**	-0.1717**	-0.3628**	-0.3451**
	(0.0358)	(0.0385)	(0.0776)	(0.0289)	(0.0318)	(0.0616)
2020Q3	0.2049**			0.1154**		
	(0.0370)			(0.0306)		
2020Q4	0.2468**			0.1580**		
	(0.0399)			(0.0326)		
2021Q1	0.0973**			0.1066**		
	(0.0418)			(0.0346)		
2021Q2	0.2866**			0.1861**		
	(0.0460)			(0.0376)		
2021Q3	0.2681**			0.1886**		
	(0.0473)			(0.0387)		
2021Q4	0.3450**			0.2199**		
	(0.0503)			(0.0404)		
Stringency Index		0.0042**	0.0033**		0.0029**	0.0026**
		(0.0006)	(0.0013)		(0.0005)	(0.0010)
Stringency Index (L1)			0.0009			0.0003
			(0.0011)			(0.0009)
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512
Individuals	369,063	369,063	369,063	369,063	369,063	369,063

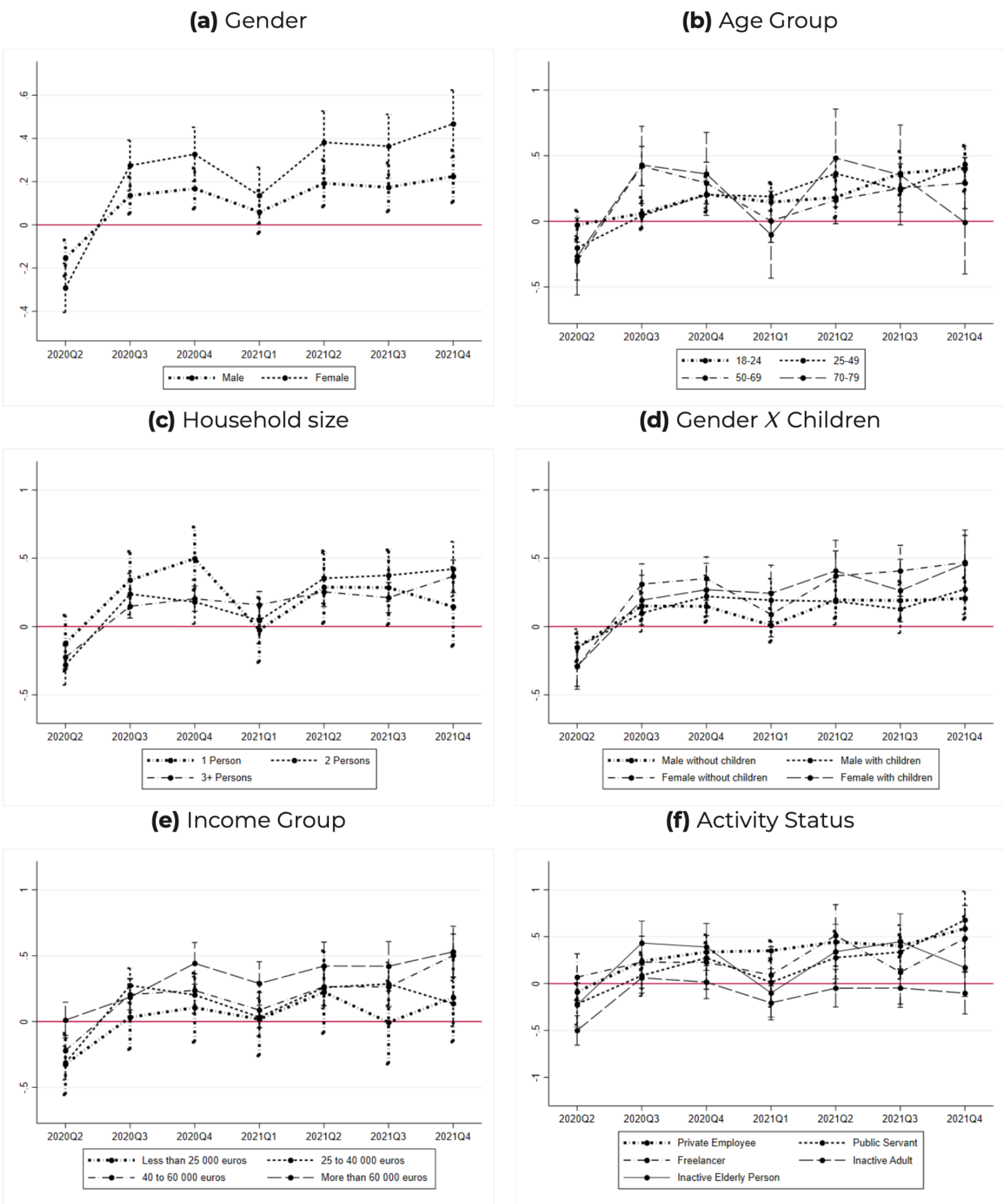
Note: Regressions include data on psychotropic drugs purchases in Defined Daily Doses (DDDs) between 2016Q1 and 2021Q4 in Luxembourg. Standard errors, clustered at the individual level, are in parenthesis. Significance levels: * = 10%, ** = 5%, and *** = 1%. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.

As before, Figure 4.8 shows the evolution of psychotropic drug use for population subgroups. Some results differ from those observed for anxiolytics and hypnotics and sedatives. Antidepressant use increased more significantly for women than men following the second quarter of 2020. Also, younger people seemed to use antidepressants more, although the results are similar in levels for age groups up to 70. In contrast, those over 70 appeared to consume less in some quarters, notably the first and last quarters of 2021. Unlike for anxiolytics, no pattern appears to emerge from Figure 4.8d exploring the relationship between gender and parenthood other than to confirm that women are more affected by the increase in consumption than men, regardless of whether they have a child.

Again, it is useful to examine summary estimates of the relative change in consumption by population subgroups as presented in Figures 4.9 and 4.10. Figure 4.9 illustrates that the drop in consumption in 2020Q2 is clear in all population subgroups. In relative terms, the declines are similar in magnitude to the declines observed in anxiolytics consumption. This initial drop is, however, wholly reversed in subsequent quarters. From 2020Q3 onwards, the average quarterly excess consumption of antidepressants is almost 4 percent of the expected quantity expected in 2020Q1 and 2.5 percent of the number of users.

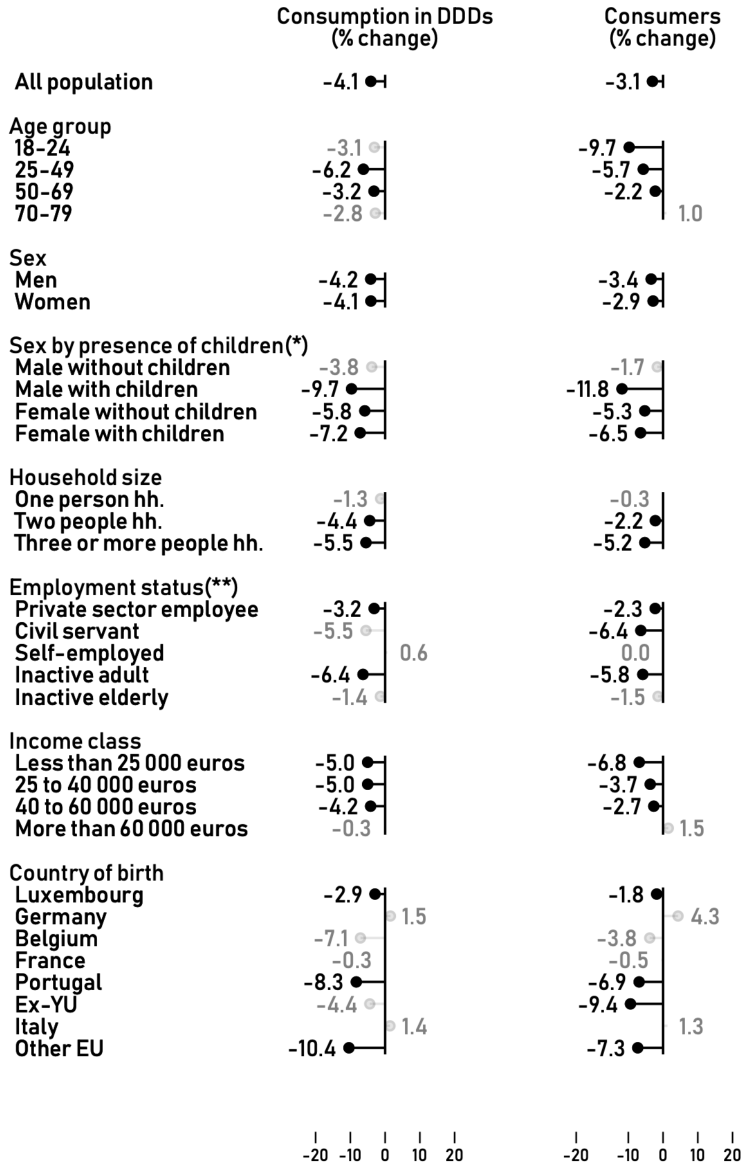
The sharpest excess consumption is observed in the youngest age group 18–24, with excess consumption reaching 14.6 percent of 2020Q1 quantities and 7.2 percent of 2020Q1 users. In relative terms, excess consumption is similar between men and women, but the presence of children appears associated with larger excess consumption among both men and women. Accordingly, individuals in larger households saw larger excess consumption. In line with what is observed for hypnotics and sedatives, relatively privileged groups experienced large excess consumption – higher income households and individuals at work (notably private sector employees) – although their absolute consumption remains lower than the less privileged groups.

Figure 4.8: Deviations from trend in Antidepressants (N06A) consumption during COVID-19



Note: Regressions are performed for each subpopulation. Figures (c)-(f) include additional controls for age and gender. Regressions include data on psychotropic drug purchases in Defined Daily Doses (DDDs) between 2016Q1 and 2021Q4 in Luxembourg. Standard errors are clustered at the individual level. Lower and upper bounds of 95% confidence intervals are represented. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.

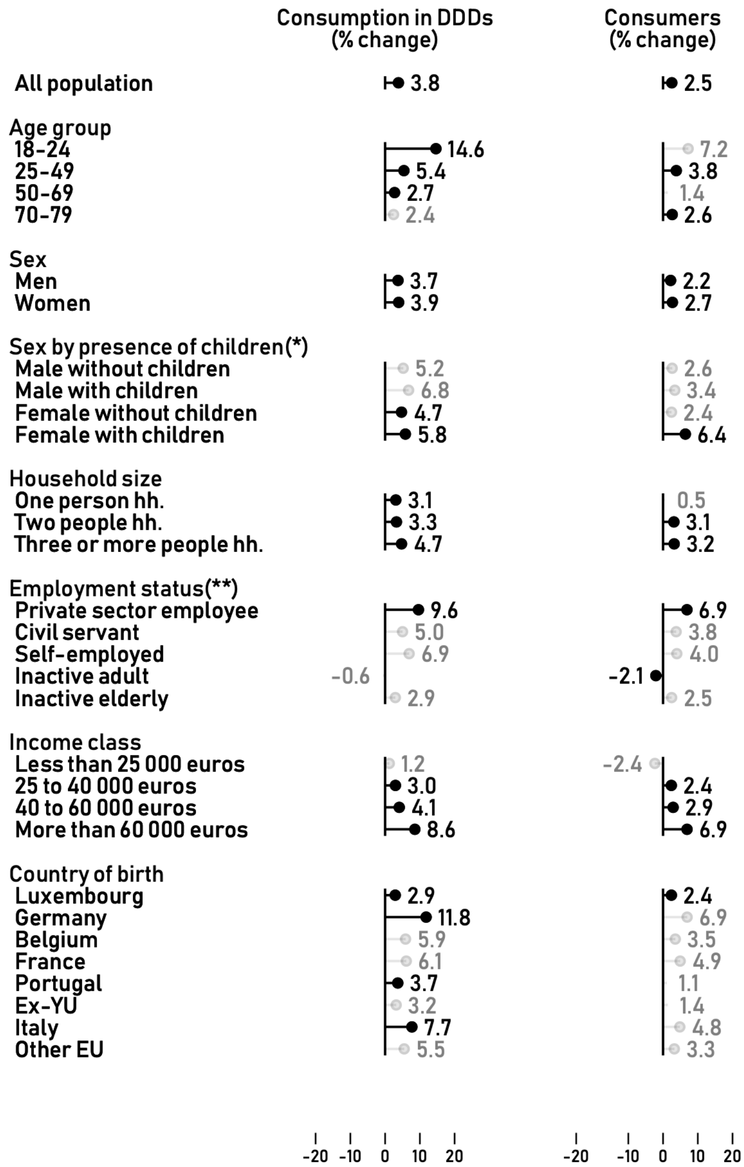
Figure 4.9: Antidepressants consumption in 2020Q2 by population subgroups



Source: Caisse nationale de santé (CNS)

Note: Psychotropic drugs encompass quarterly averages in Defined Daily Doses (DDDs) of psychotropic drugs in selected ATC classes that were purchased in a public pharmacy established in Luxembourg from 2016 to 2021 and (partially) reimbursed by the national health insurance or the accident insurance to affiliates of the Luxembourgish health insurance system. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System. Estimates shown are 100 times the ratio of the coefficient of excess consumption in quarter 2020Q2 over expected consumption in 2020Q1. Estimates in light grey are not statistically different from zero at a one percent significance level. (*) Subsamples include only individuals aged between 25 and 49. (**) Subsamples include only individuals aged between 18 and 69.

Figure 4.10: Quarterly excess consumption of Antidepressants in the period 2020Q3–2021Q4 by population subgroups



Source: Caisse nationale de santé (CNS)

Note: Psychotropic drugs encompass quarterly averages in Defined Daily Doses (DDDs) of psychotropic drugs in selected ATC classes that were purchased in a public pharmacy established in Luxembourg from 2016 to 2021 and (partially) reimbursed by the national health insurance or the accident insurance to affiliates of the Luxembourgish health insurance system. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System. Estimates shown are 100 times the ratio of the coefficient of excess consumption per quarter in the period 2020Q3–2021Q4 over expected consumption in 2020Q1. Estimates in light grey are not statistically different from zero at a one percent significance level. (*) Subsamples include only individuals aged between 25 and 49. (**) Subsamples include only individuals aged between 18 and 69.

4.4 Discussion

The study investigates the evolution of psychotropic medication consumption during the COVID-19 pandemic in Luxembourg. The analysis considers three ATC classes: anxiolytics (N05B), hypnotics and sedatives (N05C), and antidepressants (N06A). The study covers the resident population aged 18 to 79 from 2016 to 2021 by exploiting individual administrative socioeconomic data coupled with psychotropic drug consumption.

To summarize, the results show (i) a common upward trend in psychotropic drug use across all ATC classes that pre-dates the pandemic; (ii) a common decline in consumption and, especially, in the number of consumers across each ATC class in the second quarter of 2020; and (iii) diverging evolutions as the pandemic unfolds across different classes of drugs. The consumption of anxiolytics decreased significantly (relative to the long-term trends) since the second half of 2020. The consumption of hypnotics and sedatives increased moderately, then declined towards the end of 2021. The consumption of antidepressants showed signs of excess consumption beyond long-term trends, reaching a peak in the fourth quarter of 2021 (the last period covered by our data).

Overall, these results align with studies from other countries, which, in the majority, found little impact of the pandemic on psychotropic drug consumption – beyond a short-term shock at the beginning of the pandemic, largely related to supply-side limitations.[27]

The use of psychotropic drugs is linked to individual characteristics, notably gender and age, but also the presence of children, as well as income and employment status (Huybrechts et al., 2012; Lapeyre-Mestre, 2016; Leong et al., 2022). Initial consumption levels varied widely at the onset of the pandemic, with much greater medication observed among the elderly and women, for example. We also observe relatively large differences in estimates of excess consumption during the pandemic for different socio-demographic groups. To a large extent, socio-demographic patterns of excess consumption during the pandemic can be described as a regression to the mean: groups that saw the largest increases in consumption (notably of hypnotics and sedatives and antidepressants) are often groups that had a low initial level of medication. The largest excess consumption is observed among young adults, individuals living with children, employed individuals (and private sector workers in particular), and individuals living in high-income households. These are individuals whose daily routines are most likely to have been directly impacted by sanitary restrictions – notably school and workplace closures, remote work and limitations of social activities. Compared to younger age groups, older individuals (up to 79 years old) appeared to have experienced relatively fewer changes in their psychotropic drug consumption patterns during the pandemic. However, it is worth noting that this study does not extend to the oldest elderly population, particularly those residing in nursing homes, where psychotropic consumption patterns may differ significantly.

The decrease in psychotropic drug purchases across all classes observed in the second quarter of 2020 is likely related to the disruption of the healthcare system during the first wave of COVID-19 and the decline in pharmacy customers. Farina et al. (2021) showed, for example, that the number of pharmacy customers dropped by 18.46% during the first confinement in Italy. Also, the policies introduced by the Government during the first confinement

[27] France appears to be an outlier in this respect with two large-scale studies reporting significant changes in consumption patterns (Benistand et al., 2022; Levaillant et al., 2021). Furthermore, Sanchez et al. (2022) report a significant increase in dispensing of anxiolytics and antidepressants in the French military population during the pandemic period.

limited individuals' circulation, and many private and public healthcare services – deemed non-essential – were disrupted or shut down to contain the spread of the virus. The shortages experienced in the drug industry may have amplified these results. Besides supply-related issues, it is also possible that reducing social interactions at work or school during the confinement may have played a role in reducing anxiety-provoking situations among some patients – and, therefore, temporarily reduced the demand for anxiety-related medication (e.g., see Vignoli et al., 2017).

The consumption of psychotropic drugs followed separate trajectories based on their ATC class from the third quarter of 2020 onwards. This suggests that the direct disruption of healthcare services and supply-related issues faded away, and purchases later reflected the population's mental health, including the consequences of the pandemic on the demand side. Like, e.g., Farina et al. (2021), Levaillant et al. (2021), Benistand et al. (2022), Ferro Uriguen et al. (2022), Leong et al. (2022), we observe an increase in antidepressant purchases from the third quarter of 2020 onwards that is beyond the trends that could be expected from pre-pandemic trends. This result aligns with the hypotheses of Farina et al. (2021) that worsening distressing conditions (as shown by O'Connor and Peroni, 2021) is one driver of this increase (Kunzler et al., 2021; Wu et al., 2021). Leong et al. (2022) also suggest that long-standing COVID-19 cases and depression following infections may drive the upsurge in antidepressants.

The key strength of studies based on extensive administrative records of drug purchases across the entire population is to avoid self-selection issues into the study sample and potential biases resulting from individual self-reporting. Nevertheless, such analysis has limitations too. First, the association between psychotropic drug sales and consumption is only hypothesized. Patients might use up a stock of drugs purchased in previous quarters. On the contrary, purchases may not be consumed in the quarter of purchase but may be stockpiled. For simplicity, we assume that consumption is highly correlated with purchase and that significant purchasing behavioral changes did not occur during the pandemic. Second, despite the fundamental role of psychotropic medications in treating and supporting mental disorders, medication should be viewed as one of many components of mental health status. Additionally, the population diagnosed with mental disorders represents only a fraction of the population with mental disorders. Therefore, the absence of psychotropic medication is not equivalent to good mental health, which goes beyond the absence of diagnosed symptoms. Finally, medical consultation trends within different socio-economic and demographic groups may further skew the findings. Some groups may be more likely to seek medical help for mental health reasons than others, implying that purchases do not necessarily represent the actual spread of mental health issues in the population.

4.5 Conclusion

The analysis of the evolution of psychotropic drug purchases in a large cohort of the Luxembourg resident population indicates a steady increase in the consumption of anxiolytics, hypnotics and sedatives, and antidepressants between 2016 and 2021. This increase is particularly significant for women in absolute terms, although the relative increase is larger for men, who generally consume these drugs in smaller quantities.

While it is tempting to attribute this increase to mental health consequences of the COVID-19 pandemic, our analysis shows that much of the increase observed since the beginning of 2020 is, in fact, the continuation of a trend that pre-existed the pandemic. Evidence of an "excess consumption" in 2020 and 2021 above and beyond what could be expected from the evolution observed from 2016 to 2019 is limited. It primarily concerns the purchase of antidepressants. Interestingly, asymmetries appear in the evolution of purchasing behavior, notably based on gender, age, household composition, and income. Excess consumption appears more common in population groups whose daily routines may have been impacted more by sanitary measures. No robust evidence of a 'COVID-19 effect' is found for the purchase of treatments against anxiety (anxiolytics) and is limited for treating stress or sleep disorders (hypnotics and sedatives).

Nonetheless, some results confirm the significance of the disruption caused by the pandemic on individuals' access to care during periods of severe restrictions and highlight the need to implement appropriate health shields weighing the impact of measures. Under restrictive measures, access to the healthcare system must be guaranteed.

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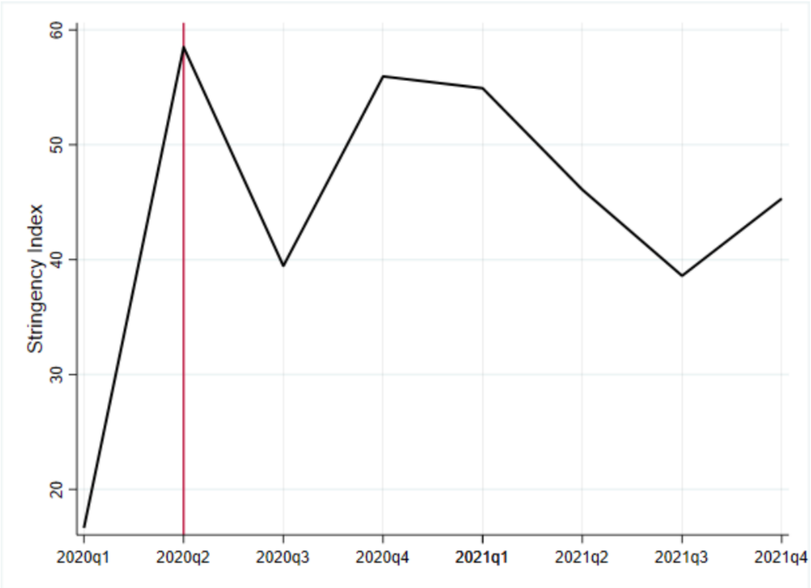
Appendix

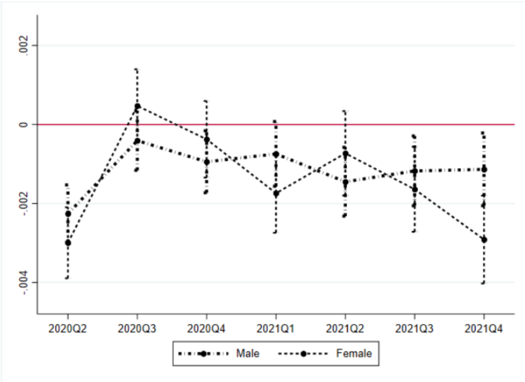
The evolution of the Stringency Index in Luxembourg, 2020–2021

Figure 4.11: Evolution of the Stringency Index, 2020–2021, by quarters

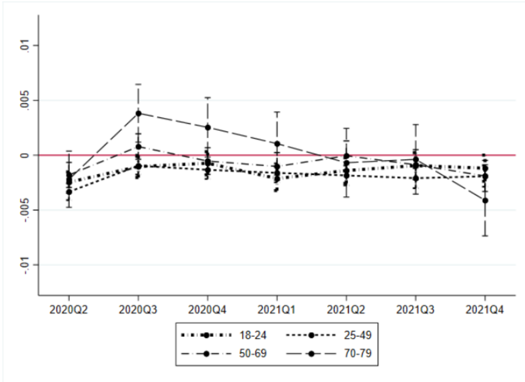


Source: Oxford COVID-19 Government Response Tracker

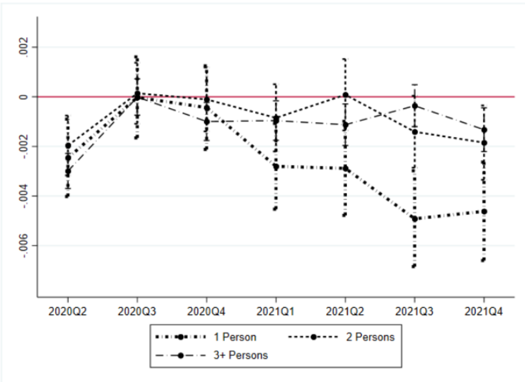
Regression estimates by population subgroups with number of consumers as dependent variable



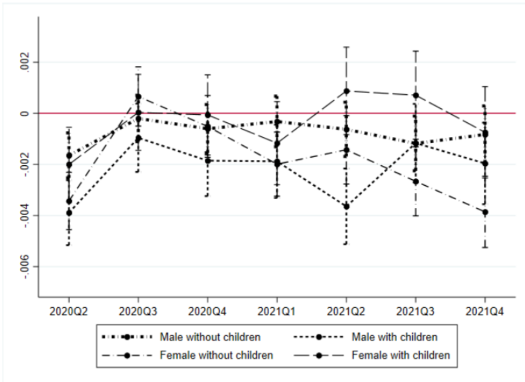
(a) Gender



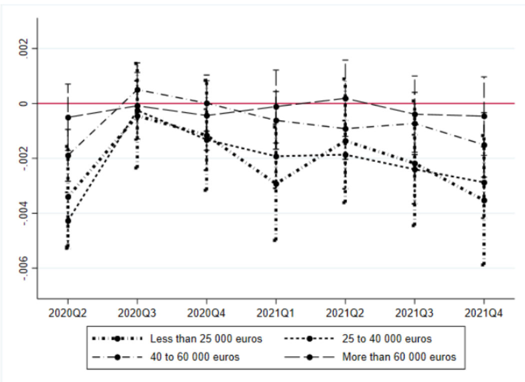
(b) Age Group



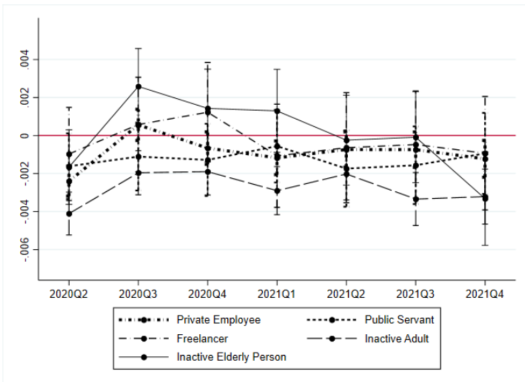
(c) Household size



(d) Gender *X* Children



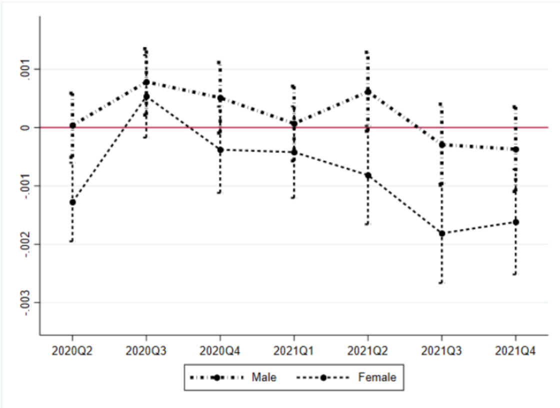
(e) Income Group



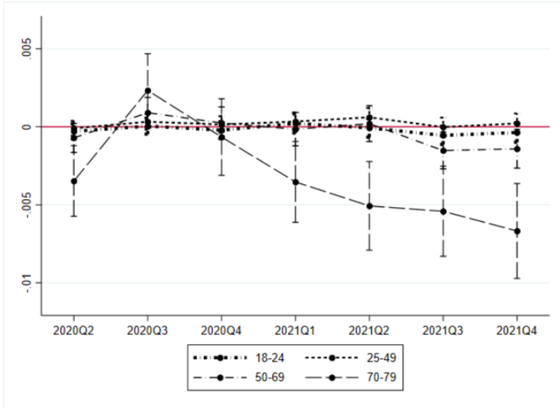
(f) Activity Status

Figure 4.12: Deviations from trend in Anxiolytics (N05B) consumers during COVID-19

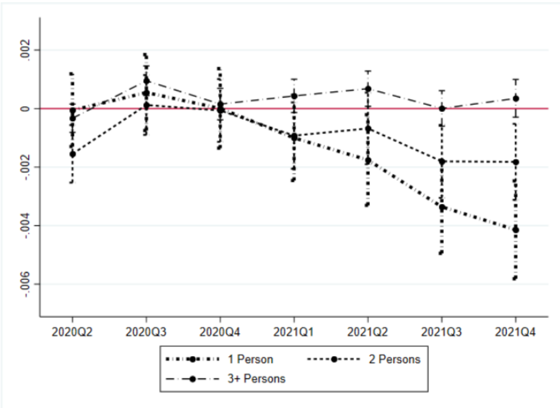
Note: Regressions are performed for each subpopulation. Figures (c)-(f) include additional controls for age and gender. Regressions include data on consumers of psychotropic drug purchases between 2016Q1 and 2021Q4 in Luxembourg. Standard errors are clustered at the individual level. Lower and upper bounds of 95% confidence intervals are represented. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.



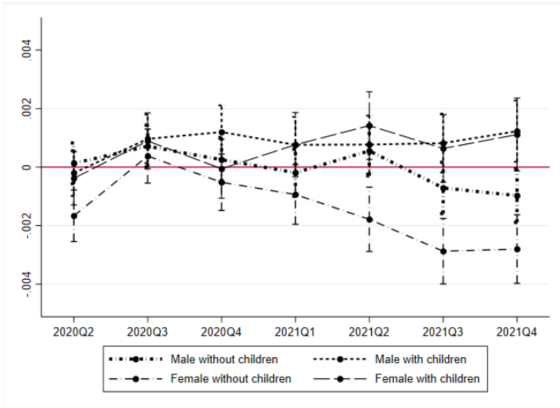
(a) Gender



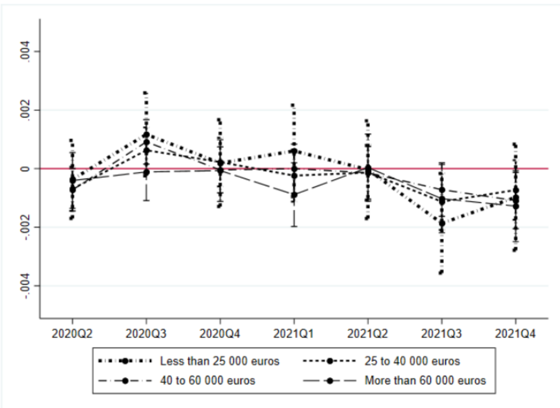
(b) Age Group



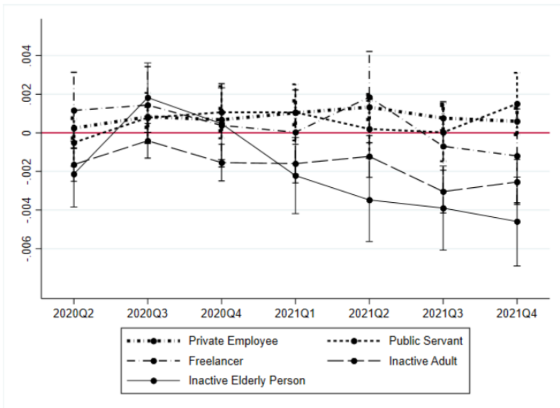
(c) Household size



(d) Gender *X* Children



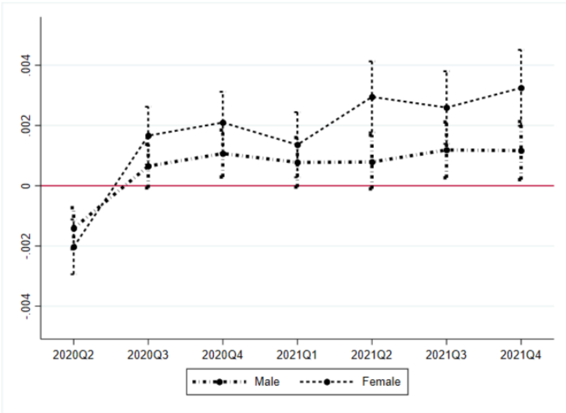
(e) Income Group



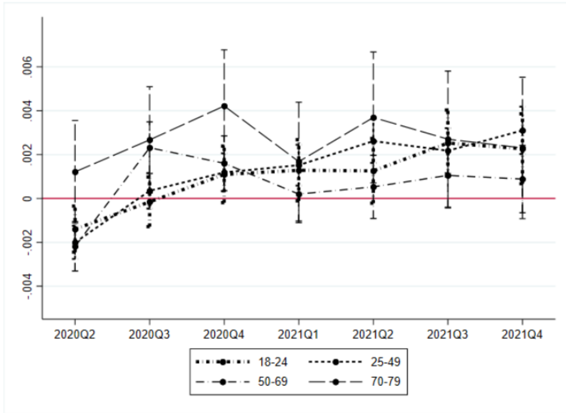
(f) Activity Status

Figure 4.13: Deviations from trend in Hypnotics and Sedatives (N05C) consumers during COVID-19

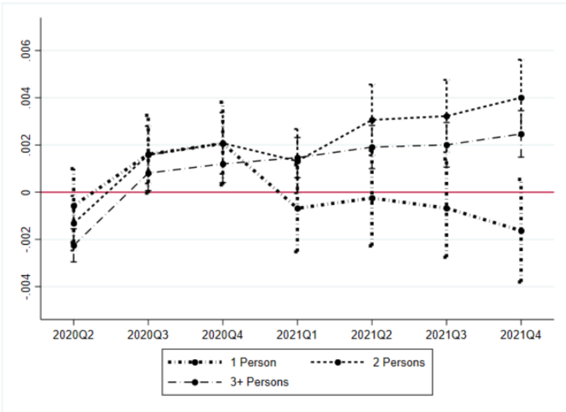
Note: Regressions are performed for each subpopulation. Figures (c)-(f) include additional controls for age and gender. Regressions include data on consumers of psychotropic drug purchases between 2016Q1 and 2021Q4 in Luxembourg. Standard errors are clustered at the individual level. Lower and upper bounds of 95% confidence intervals are represented. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.



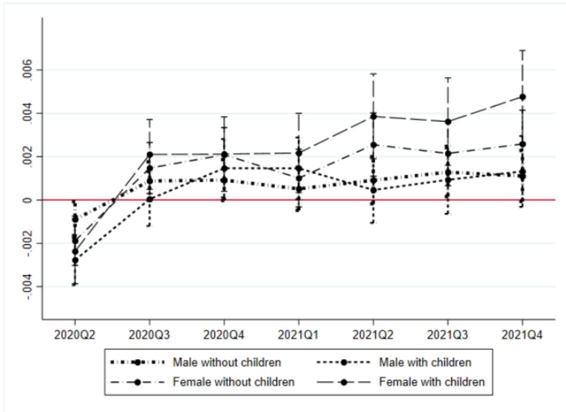
(a) Gender



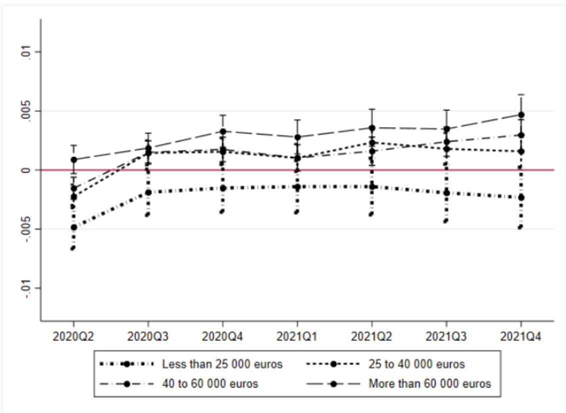
(b) Age Group



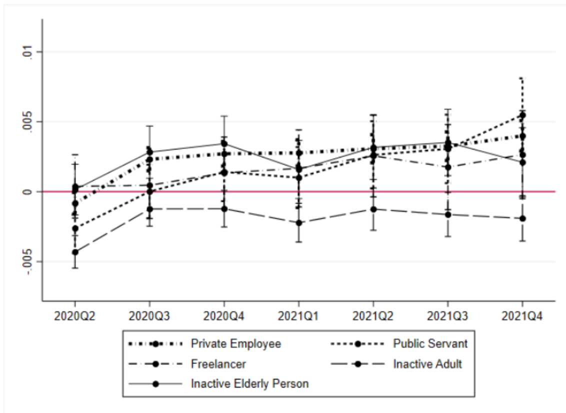
(c) Household size



(d) Gender *X* Children



(e) Income Group



(f) Activity Status

Figure 4.14: Deviations from trend in Antidepressants (N06A) consumers during COVID-19

Note: Regressions are performed for each subpopulation. Figures (c)-(f) include additional controls for age and gender. Regressions include data on consumers of psychotropic drug purchases between 2016Q1 and 2021Q4 in Luxembourg. Standard errors are clustered at the individual level. Lower and upper bounds of 95% confidence intervals are represented. Psychotropic drugs are classified according to the Anatomical Therapeutic Chemical (ATC) Classification System.

Robustness checks: Simplified regression specifications

Tables 4.7, 4.8, and 4.9 in the Appendix perform a battery of robustness checks by implementing slight differences in the Model 4.1. More specifically, Columns 1 and 5 implement Model 4.1 without including seasonal effects, while Columns 2 and 6 do not include any control for the trend. Then, Columns 3 and 7 do not include any bin during the pandemic but consider the impact of the whole pandemic period instead, while Columns 4 and 8 follow the same approach but additionally include a control for the start of the pandemic. Overall, the results are consistent with the previous Tables.

Table 4.7: Robustness checks: Effects of COVID-19 on Anxiolytics (N05B) consumption

Margin	Consumption (in DDDs)				Consumers (in %)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	3.0419**	2.8329**	3.0597**	3.0563**	4.7673**	4.5451**	4.8525**	4.8429**
	(0.0430)	(0.0367)	(0.0417)	(0.0421)	(0.0285)	(0.0259)	(0.0292)	(0.0294)
Trend	0.0308**		0.0289**	0.0286**	0.0405**		0.0383**	0.0376**
	(0.0021)		(0.0019)	(0.0020)	(0.0017)		(0.0016)	(0.0016)
2020Q2	-0.1846**	0.1522**		-0.0560**	-0.3324**	0.1267**		-0.1585**
	(0.0245)	(0.0262)		(0.0261)	(0.0270)	(0.0283)		(0.0285)
2020Q3	-0.1073**	0.2787**			-0.1971**	0.3921**		
	(0.0274)	(0.0269)			(0.0287)	(0.0284)		
2020Q4	0.0486	0.2987**			0.0271	0.3230**		
	(0.0304)	(0.0278)			(0.0305)	(0.0293)		
2021Q1	-0.0995**	0.2328**			-0.0411	0.3432**		
	(0.0323)	(0.0269)			(0.0319)	(0.0292)		
2021Q2	-0.1446**	0.3155**			-0.1857**	0.4356**		
	(0.0359)	(0.0299)			(0.0330)	(0.0302)		
2021Q3	-0.2167**	0.2927**			-0.3471**	0.4043**		
	(0.0364)	(0.0299)			(0.0341)	(0.0297)		
2021Q4	-0.1211**	0.2523**			-0.1151**	0.3431**		
	(0.0406)	(0.0325)			(0.0358)	(0.0308)		
Pandemic Period			-0.0874**	-0.0765**			-0.1214**	-0.0906**
			(0.0260)	(0.0284)			(0.0216)	(0.0231)
Quarter Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512
Individuals	369,063	369,063	369,063	369,063	369,063	369,063	369,063	369,063

Table 4.8: Robustness checks: Effects of COVID-19 on Hypnotics and Sedatives (N05C) consumption

Margin	Consumption (in DDDs)				Consumers (in %)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	3.9628**	3.9147**	4.0517**	4.0448**	3.8367**	3.6063**	3.9266**	3.9240**
	(0.0513)	(0.0467)	(0.0507)	(0.0510)	(0.0285)	(0.0254)	(0.0288)	(0.0289)
Trend	0.0214**		0.0180**	0.0175**	0.0423**		0.0398**	0.0396**
	(0.0025)		(0.0024)	(0.0024)	(0.0015)		(0.0014)	(0.0014)
2020Q2	-0.1523**	0.2048**		-0.1132**	-0.1525**	0.3469**		-0.0425**
	(0.0275)	(0.0324)		(0.0293)	(0.0203)	(0.0221)		(0.0217)
2020Q3	0.0057	0.3396**			-0.0766**	0.4742**		
	(0.0301)	(0.0327)			(0.0218)	(0.0222)		
2020Q4	0.2823**	0.3881**			0.0621**	0.4152**		
	(0.0332)	(0.0339)			(0.0235)	(0.0227)		
2021Q1	0.1317**	0.2655**			0.0729**	0.4724**		
	(0.0364)	(0.0346)			(0.0251)	(0.0232)		
2021Q2	0.0639	0.5068**			-0.1065**	0.5620**		
	(0.0398)	(0.0384)			(0.0260)	(0.0240)		
2021Q3	-0.0583	0.3613**			-0.2531**	0.4669**		
	(0.0412)	(0.0389)			(0.0270)	(0.0236)		
2021Q4	-0.0938**	0.0976**			-0.0496*	0.4726**		
	(0.0447)	(0.0406)			(0.0288)	(0.0244)		
Pandemic Period			0.0917**	0.1137**			-0.0180	-0.0098
			(0.0277)	(0.0303)			(0.0177)	(0.0191)
Quarter Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512
Individuals	369,063	369,063	369,063	369,063	369,063	369,063	369,063	369,063

Table 4.9: Robustness checks: Effects of COVID-19 on Antidepressants (N06A) consumption

Margin	Consumption (in DDDs)				Consumers (in %)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	5.7352**	5.3792**	5.8958**	5.8675**	5.8884**	5.6022**	6.0312**	6.0108**
	(0.0424)	(0.0376)	(0.0430)	(0.0432)	(0.0347)	(0.0308)	(0.0349)	(0.0352)
Trend	0.0644**		0.0661**	0.0639**	0.0526**		0.0538**	0.0522**
	(0.0024)		(0.0023)	(0.0023)	(0.0020)		(0.0019)	(0.0019)
2020Q2	-0.2908**	0.4078**		-0.4665**	-0.2203**	0.3429**		-0.3357**
	(0.0332)	(0.0357)		(0.0353)	(0.0266)	(0.0289)		(0.0289)
2020Q3	-0.0416	0.8348**			-0.1038**	0.6300**		
	(0.0351)	(0.0362)			(0.0291)	(0.0291)		
2020Q4	0.3204**	0.8766**			0.2234**	0.6726**		
	(0.0386)	(0.0375)			(0.0316)	(0.0300)		
2021Q1	0.2394**	0.8531**			0.2275**	0.7241**		
	(0.0409)	(0.0375)			(0.0340)	(0.0304)		
2021Q2	0.2120**	1.1683**			0.1329**	0.9065**		
	(0.0438)	(0.0404)			(0.0358)	(0.0321)		
2021Q3	0.0159	1.1498**			-0.0352	0.9091**		
	(0.0462)	(0.0400)			(0.0377)	(0.0319)		
2021Q4	0.4129**	1.2267**			0.2807**	0.9403**		
	(0.0494)	(0.0422)			(0.0397)	(0.0326)		
Pandemic Period			0.1346**	0.2254**			0.0865**	0.1518**
			(0.0288)	(0.0313)			(0.0244)	(0.0266)
Quarter Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512	8,857,512
Individuals	369,063	369,063	369,063	369,063	369,063	369,063	369,063	369,063

Emotional barometers: Twitter emojis and emoticons as tools to gauge temperature's effect on mood

Chapter 5

Emotional barometers: Twitter emojis and emoticons as tools to gauge temperature's effect on mood

5.1 Introduction

Individuals' physical and mental health are related to weather conditions. Cold days are associated with cardiovascular problems in Catalonia (Ponjoan et al., 2017), while higher temperatures are linked to higher suicide rates (Lin et al., 2008; Maes et al., 1994) and violence (Anderson, 1989, 2001; Baron & Bell, 1976). Additionally, weather influences people's everyday behavior and decisions (Persinger, 1980; Watson, 2000; Zong et al., 2017). People engage in different activities depending on the weather (Lucas & Lawless, 2013), and even dress differently. College admission staff prioritize students' academic performance on cloudy days (Simonsohn, 2009), and restaurant customers tend to give higher tips on sunny, pleasant days (Cunningham, 1979). Finally, weather-related terms permeate many languages as expressions of emotions.[28] For instance, talking about someone as a "little sunshine" has a positive connotation.

However, despite the widespread belief that weather affects people's moods and well-being, empirical research has yielded mixed results, and a consensus is yet to be reached. Most studies on the impact of weather on individuals have focused on its relationship with its correlates, such as health, decisions, and activities. Additionally,

[28]The lyrics of the famous song "Singin' in the Rain" performed by Gene Kelly illustrate this beautifully:

"I'm singing in the rain, just singin' in the rain
What a glorious feeling, I'm happy again
I'm laughing at clouds so dark up above
The sun's in my heart and I'm ready for love "

studies show mixed results. On the one hand, some studies show that weather conditions seem to play a transitory role in people's moods and judgments (Cunningham, 1979; Goldstein, 1972; Howarth & Hoffman, 1984; Parrott & Sabini, 1990; Persinger, 1975). People often report higher happiness and life satisfaction scores (Rind, 1996; Rind & Strohmetz, 2001; Schwarz & Clore, 1983) and better mood (Barnston, 1988; Parrott & Sabini, 1990) on sunny days. On the other hand, some other studies conversely conclude that high temperatures, often accompanied by sunshine, are linked to low potency (Connolly, 2013; Goldstein, 1972), which is akin to a low mood (Keller et al., 2005).

This inquiry into the effects of weather on individuals gains increased relevance against the backdrop of climate change, which is expected to generate asymmetric effects. These effects may trigger disparate weather changes across different regions, shaped by factors such as economic structure (Lobell & Burke, 2008) and local characteristics (Albouy et al., 2016; Sinha et al., 2018). Consequently, certain populations may bear more brunt from long-term climate shifts (Hsiang, 2016). Some individuals may even consider relocation to escape undesirable weather conditions (e.g., see Blomquist et al., 1988; Carleton and Hsiang, 2016; Rehdanz and Maddison, 2005).

Several studies have shown how weather and climate may influence people's well-being and life choices in diverse ways. For instance, Frijters and Van Praag (1998) indicate that a positive change of one degree Celsius would bring Muscovites to the same well-being with 13% less income, suggesting potential benefits of climate change for some populations. Similarly, Ferreira and Moro (2010) confirm that air quality and a warm climate can positively affect people's well-being. Conversely, Sinha et al. (2018), Albouy et al. (2016), and Baylis (2020) reveal that individuals would accept an income reduction ranging from 0.6% to 4% to avert the anticipated climate change outcomes by the end of the century, often manifested as temperature increases. However, Feddersen et al. (2016) provide a different perspective, concluding that climate does not significantly influence people's well-being. This diverse range of findings underscores the complex and multifaceted relationship between weather, climate, and individual well-being.

In light of the above, this study aims to offer a fresh perspective on the weather-mood nexus with alternative mood data, specifically focusing on temperature. The study leverages Twitter data and introduces a new methodology to gauge users' moods via emojis. This approach captures users' moods swiftly and independently of language, allowing for a diverse sample.

The inconsistent results across the existing studies may be attributed to several factors. First, inter-individual differences in reactivity types may play a role (Denissen et al., 2008; Klimstra et al., 2011). Some people look forward to sunny days as spring approaches, while others say grey, rainy weather depresses them (Klimstra et al., 2011). Second, the activities in which people participate may vary (Barrington-Leigh, 2008; Keller et al., 2005), especially during different seasons (Denissen et al., 2008). Additionally, Seasonal Affective Disorder (SAD) has been identified as a mental health condition that may cause non-depressed individuals to experience depressive symptoms during the winter months (Cohen et al., 1992), possibly due to a lack of sunlight (Leppämäki et al., 2003; Oren et al., 1994; Peng et al., 2016; Young et al., 1997). Third, as Rehdanz and Maddison (2005) point out, there may be an ideal comfort zone between the extreme weather conditions. Lower temperatures are usually preferred during the hot months, while higher temperatures are preferred during the cold months. These discrepancies and inconsistencies may explain why some studies have found no or only feeble aggregate effects (e.g., Kämpfer and

Mutz, 2013).

Moreover, many studies present several shortcomings (Howarth & Hoffman, 1984; Preti, 1998; Sanders & Brizzolara, 1982). One of the major criticisms is that the studies are often based on small and specific populations in particular locations over short periods (Tsutsui, 2013).[29] As a result, the findings may be hardly generalizable to larger populations. Additionally, many studies rely on bivariate analysis and do not account for the potential correlation between weather variables such as temperature and sunlight. The omission of a variable may lead to inaccurately attributing an effect to a specific meteorological variable when the effect is due to a combination of variables (Howarth & Hoffman, 1984). Addressing these issues would enhance the internal and external validity of the models (Watson, 2000), making the results more robust and applicable to a broader range of populations and contexts.

This analysis addresses several methodological concerns highlighted in the literature, such as omitted variable bias and limited sample sizes. First, the study leverages randomly selected geotagged Twitter data to achieve substantial daily observations over a year, covering the contiguous United States. This approach provides a more extensive and diverse set of data points than traditional survey-based studies that often suffer from tiny sample sizes (e.g., Barrington-Leigh, 2008; Denissen et al., 2008; Keller et al., 2005). Second, using social network data also avoids the potential biases induced by surveys, such as respondents being in a particular state of mind when answering specific questions at a specific time for one or more reasons (Li et al., 2014). This approach enables the study to capture a wide range of short-term weather variations and includes different types of profiles.

Although several authors have used social network data to obtain large datasets (e.g., Baylis et al., 2018; Hannak et al., 2012; Li et al., 2014; Park et al., 2013), no standard agreement emerges in the literature around the effect of weather conditions on well-being. While some studies suggest that weather conditions are unrelated to well-being or only shallowly related (e.g., Lucas and Lawless, 2013), others find results in line with major survey-based studies (e.g., Li et al., 2014; Park et al., 2013). These existing studies, however, primarily rely on Natural Language Processing (NLP) to analyze textual content and discern sentiments (Algaba et al., 2020; Liu, 2012). This common methodology, while valuable, has potential limitations, including misinterpretation of context, sarcasm, or complex language use.

In contrast, the present study introduces a novel approach focusing on emojis and emoticons as signals conveying real emotions or moods. This innovative method, building a new framework upon the classification of Emoji feelings by Novak et al. (2015), is designed to reduce the ambiguities often present in text-based sentiment analysis. By associating these conveying moods and well-being signals with a broad range of weather variables, this study addresses the potential drivers of discrepancies between social network-based studies, including the frequent omission of the combination of weather variables when investigating possible effects and potentially involving confounding factors (e.g., see Howarth and Hoffman, 1984). Given the nature of microblogging content, language complexity, or sarcasm and negation (e.g., see Denny and Spirling, 2018), this approach provides a unique and potentially more accurate perspective on the relationship between weather and well-being.

This paper proposes an innovative approach for exploring the influence of weather conditions, primarily

[29] Paul Ekman declared at the Being Human conference: "We basically have a science of undergraduates" (Campbell, 2012), referring to the numerous studies focused on students.

temperature, on individuals' moods, harnessing the power of emojis and emoticons within Twitter data to capture mood-related information. The contributions of this study are twofold. First, this study introduces a framework for extracting mood on Twitter through emojis and emoticons, addressing potential pitfalls such as omitted variable bias and issues related to small sample sizes. Second, the analysis illuminates the nuanced ways weather conditions impact people's moods, indicating a non-linear relationship. The approach is flexible, cost-effective, and easily replicable in different geographic regions, making it suitable for future research questions. The proposed method offers a better understanding of well-being changes through its intentional simplicity.

The rest of the paper is organized as follows. Section 5.2 describes the data sources used for weather and mood, and explains the methodology employed to extract mood information. Section 5.3 outlines the Benchmark Model. The results are presented and discussed in Section 5.4. Section 5.5 provides the concluding remarks of this study.

5.2 Data

This section provides a detailed overview of the data sources and manipulations used to derive the weather and mood variables for evaluating the effects of weather conditions on individual well-being. To begin with, subsection 5.2.1 introduces the daily weather variables used in this study. Subsection 5.2.2 introduces how Twitter is used as a source for mood data, comparing it to more conventional data sources such as surveys. This subsection outlines the challenges and limitations associated with social media data. Finally, Subsection 5.2.3 presents the technique to match the weather data with mood data using a regular tessellation of the conterminous United States, followed by inverse distance weighted (IDW) interpolation to derive the weather variables for each polygon centroid.

5.2.1 Weather

This study utilizes daily weather variables, a decision underpinned by their superior reliability and widespread availability compared to hourly data. Derived from measurements taken throughout the day, daily data sidestep the potential skew from transient errors. They also account for lag effects, such as the time it takes to respond to a meteorological event. In contrast, compiling hourly data over a year can strain infrastructural resources for what could amount to marginal gains at best.

The weather data for this study originate from three primary sources: NOAA's Global Historical Climatology Network (GHCN), the Global Surface Summary of the Day (GSOD), and DAYMET. These datasets each offer distinct advantages. The GHCN, managed by NOAA, comprises daily observations from a vast network of over 100,000 surface stations globally, all subject to stringent quality assurance checks. Despite an unbalanced distribution of stations, the GSOD dataset boasts superior coverage in recent years. Lastly, DAYMET offers gridded estimates of daily weather variables across North America, produced at a 1 km x 1 km scale. This model-based product interpolates and extrapolates GHCN weather observations. By integrating these sources, the present study can mitigate individual limitations and leverage their unique strengths, creating a robust, reliable dataset. Further details about these datasets can be found in Appendix Weather.

The datasets utilized here offer comprehensive daily coverage of the entire contiguous United States for 2014. Their combination supports robustness checks and yields a comprehensive depiction of weather conditions—capturing the "complete weather picture" San-Gil et al., 1991, p. 402. Key variables in our analysis include maximum temperature, precipitation, short-wave radiation, relative humidity, visibility, wind speed, and heat index. For a complete list of variables, along with their definitions and sources, please refer to Appendix Weather variables: Definitions, units of measurement, and sources.

5.2.2 Mood

This study extracts mood and sentiment data from the social media Twitter (Algaba et al., 2020; Bollen et al., 2011; Liu, 2012; Tumasjan et al., 2010). Twitter posts, known as "tweets", are short messages limited to 140 characters until November 2017 (A. Rosen, 2017). While tweets can cover any topic, their limited size usually implies that only one topic is covered per tweet. Additionally, tweets' content is more similar to a conversation than an informative treatment of a topic (Pawar et al., 2015). Since tweets are intended as quasi-instantaneous and spontaneous messages to a network, they do not suffer from a volunteer effect (Li et al., 2014), as individuals often indirectly express their emotions through their tweets, even when not intending to disclose their emotional state.

This paper uses a 1% random sample of geotagged tweets with emojis or emoticons as signals conveying real emotions or moods in the conterminous United States in 2014. The sample is extracted through Twitter Streaming API. Twitter's policy prohibits individual tracking, limiting the use of panel data and control of individual fixed effects. Therefore, the data are cross-sectional and do not include demographic data about the user other than the user's country and location. A notable limitation of Twitter data is thus that individuals' preferences for weather or personality traits are not controlled (Denissen et al., 2008; Ferrer-i-Carbonell & Frijters, 2004). However, this limitation is offset by the large number of tweets, which elevates the scope of the analysis to the population level to capture prevalent trends and associated costs.

Twitter released the geolocation feature in 2009 (Stone, 2009). Until 2015, Twitter recorded the precise GPS coordinates of each tweet posted from the Android or iOS app when the function had not been disabled by the user (Drakonakis et al., 2019). However, as of April 2015, Twitter changed its policy by limiting the accuracy of recorded coordinates. In particular, Twitter disabled precise location by default and asked users to opt-in to enable the option. This policy change significantly shrinks the number of tweets for which precise geolocation was provided (Drakonakis et al., 2019). Therefore, this study uses only data containing precise coordinates in 2014, just before the policy switch.

Emoticons are typographical approximations of facial expressions (e.g., :-), :(; or :D), while emojis are images. Most emojis are recognized by the Unicode Consortium, which implies standardized encoding and display across platforms. Emojis are commonly used among internet users to express emotions (Jibril & Abdullah, 2013). Emojis and emoticons have a practical aspect, replacing words with images or typographic sets that replace the body language usually used in face-to-face communication (e.g., see Guibon et al., 2016; Kelly and Watts, 2015; Lebduska, 2014; Miller et al., 2016; Shiha and Ayvaz, 2017). About 30% of the tweets in this study contain at least one emoji or emoticon. Many authors, therefore, use emoticons and emojis as signals of genuine emotions, moods, or paralanguage (e.g., Deriu et al., 2016; Eisner et al., 2016; Felbo et al., 2017; Go et al., 2009; Hannak et al., 2012; Read, 2005; Tang et al., 2014; Wolny, 2016; Wood and Ruder, 2016).

Using emojis and emoticons as signals conveying moods requires hypothesizing that emojis and emoticons reveal the emotions contained in tweets. In Natural Language Processing (NLP), it is commonly assumed that a single document typically expresses one primary emotion or feeling (Algaba et al., 2020). Due to Twitter's character limit, tweets are even more robust as they typically convey a single topic or idea per tweet. Moreover, emoticons often represent users' underlying feelings (Vogel & Janssen, 2009). Berengueres and Castro (2017) show an 84% agreement between emojis users and readers on the meaning of emojis, suggesting that the meaning of emojis is unambiguous. Additionally, Berengueres and Castro (2017) found that daily mood does not significantly influence the proportion of tweets containing emojis, meaning that if there is a noticeable variation in the use of emojis, it is not aggregated but instead reflects the more willing use of specific emojis that express a particular mood.

I use the Emoji Sentiment Ranking, as devised by Novak et al. (2015), to categorize emojis and emoticons into emotions. Novak et al. (2015) created this emojis lexicon with the help of 83 annotators who categorized 1.6 million tweets containing one or more Unicode emojis from 13 different European languages. Novak et al. (2015) observed that emojis act as universal and language-independent indicators of emotions for European languages, making this tool particularly effective for our analysis. The resulting output is a table of 751 emojis with statistics related to the occurrences of positive, neutral, and negative tweets containing each emoji. I build on the Emoji Sentiment Ranking and classify emojis and emoticons as positive (negative) if the positive (negative) tweet occurrences containing the emoji are at least 20% higher than the negative (positive) tweet occurrences containing the emoji and 20% higher than the neutral tweet occurrences containing the emoji.[30] The resulting dependent variable is a binary variable reflecting two states: positive and negative moods. It should be noted that while this Emoji Sentiment Ranking has proven effective for broad mood categorization, it may not capture some of the subtleties of emotion that emojis can convey. Furthermore, this ranking is based on the emotional content of tweets, and hence, it may carry certain biases inherent to the Twitter user population and the nature of Twitter communication. Appendix Mood provides more details about the methodology.

This approach contrasts sharply with most measures used in previous studies to capture the mood, such as self-reported measures administered via computer, diary, or orally (e.g., Keller et al., 2005; Rusting and Larsen, 1998). When individuals explicitly indicate their subjective well-being, they supposedly weigh a broad set of life parameters against their individual preferences (Schimmack et al., 2009). A cognitive iterative process gathers as much information as possible to construct a single value. Significant events, mood, and people's environment then affect the subjective well-being by influencing the information available at the time of exercise (Emmons & Diener, 1985; Lucas & Lawless, 2013; Rusting, 1998). The idea is that the ability of individuals to remember information is biased by the mood of individuals when they answer survey questions (Strack et al., 1991), even using their mood as a yardstick. On this basis, several researchers have analyzed the impact of weather on mood, exploiting the short-term variations of subjective well-being as representing the mood when answering the question. In

[30]More restrictive thresholds were tested and did not yield significant differences (results not shown).

contrast, the present analysis uses indirectly reported emotions and moods[31] through social media data, avoiding any volunteer effect.

5.2.3 Weather and mood matching

To determine local weather for each date across the contiguous United States, I generate a grid of 2,500 squares by subdividing each edge of the bounding box into 50 segments. This guarantees comprehensive coverage of the area under study. Subsequently, inverse distance weighted (IDW) interpolation is applied to approximate the weather conditions around each centroid for each date. The IDW interpolation formula is as follows:

$$\hat{Z}(c_0) = \sum_{i=1}^{N} \delta_i Z(c_i) \tag{5.1}$$

Where $\hat{Z}(c_0)$ is the interpolated value of the tweet at the location c_0, $Z(c_i)$ is the observed value of a station i at the location c_i, δ_i is the weight attached to the value of the station i, and N is the number of stations considered to interpolate the value at the tweet location. The weight δ_i is defined:

$$\delta_i = \frac{\frac{1}{d_{i0}^P}}{\sum_{i=1}^{N} \frac{1}{d_{in}^P}} \tag{5.2}$$

The weights δ_i collectively add up to 1. The IDW function modulates the importance of stations in accordance with their proximity to the interpolated points, with stations closer to these points having a greater influence when adjusting the power parameter p. This parameter p influences how quickly the weights of distant stations diminish. The following analysis employs a power of 2, although testing with a power of 1 revealed no significant alteration in the models' estimates. Further, a search boundary of $300KM$ radius is set around the interpolated point, allocating a zero weight to stations beyond this boundary. This intentional constraint significantly enhances the algorithm's speed without materially affecting the interpolated values.

Upon interpolating weather values for each centroid, an algorithm maps each tweet's coordinates to its corresponding polygon. The centroid's interpolated values are then linked to the related tweet. This ensures that each tweet is associated with a weather value accurately reflecting its location.

5.3 Empirical strategy

The richness of the dataset, which covers a large territory, includes diverse individuals, and spans an extended period, makes it possible to detect even subtle effects. However, numerous influences other than weather contribute to a user's decision-making process regarding emoji selection in a tweet. Consequently, the mood proxy's expected weather impact is anticipated to be marginal. Therefore, the analysis chiefly centers around the coefficients' sign and statistical significance, setting a predetermined alpha level cut-off at .05.

Unobserved factors may be related to mood and weather (Baylis et al., 2018). For example, individuals might be happier in certain cities or during specific months. Furthermore, with its vacation time and extended daylight

[31] Algaba et al. (2020) note that these subjective human terms are used interchangeably in the literature.

hours, the summer season may enhance perceived enjoyment (Barrington-Leigh, 2008). Thus, to counteract the effects of such unobserved phenomena, the analysis incorporates several control variables, including date, time, and location.[32]

Significantly, weather conditions are largely immune to short-term external influences. In the long run, however, one could discern the impacts of climate change or, for instance, climate-based population selection (i.e., individuals opting to live in one area over another). Nonetheless, these factors are unlikely to considerably affect this study's outcomes due to the relatively brief analysis period. As such, the coefficients can be interpreted as causal without necessitating the inclusion of a multitude of variables. Although lacking an experimental design, the outcomes will be attributed to a unidirectional relationship flowing from weather to mood.[33]

A generalized linear model (GLM) is adopted to represent the weather-mood relationship. The model, linked to the response variable through a link function, integrates parameters linearly. Specifically, the Benchmark link function is a probit function, optimally suited for the binary dependent variable.[34] The model can be expressed as follows:

$$E(y) = \Theta(\beta_0 + f(W) + Month + Location + DayWeek + Hour + Festive) \tag{5.3}$$

Where y denotes the dependent variable mood, $f(W)$ is a function that depends on a vector of meteorological variables, and $\Theta()$ is the inverse normal cumulative distribution function. Month, Location, DayWeek, and Hour are fixed effects controlling for each tweet's month, state, day of the week, and hour, respectively. Festive is a dummy variable that takes the value of 1 on festive days (e.g., Valentine's Day or Independence Day) and 0 otherwise. Besides, heteroskedasticity-robust standard errors are used to adjust for the autocorrelation of errors and are clustered by month and county. This model assumes that weather realizations can be seen as random under the given specification.

5.4 Results

The following section unfolds in four distinct parts. First, Subsection 5.4.1 presents the variables in detail and selects a smaller set of variables subset. Subsequently, Subsection 5.4.2 unravels the core findings concerning the weather-mood relationship across several specifications. Then, Subsection 5.4.3, inspired by Levinson (2012), attempts to assign value to weather conditions. Lastly, Subsection 5.4.4 includes a series of alternative specifications.

5.4.1 Descriptive statistics

Several overlapping variables from the three weather datasets are omitted to prevent multicollinearity. In particular, the maximum temperature from the Global Historical Climatology Network (GHCN) is chosen to encapsulate

[32] It is pertinent to note that socioeconomic factors are excluded as their inclusion could potentially neutralize weather condition effects if the non-weather variable is itself affected by weather conditions (Burke et al., 2015).

[33] Mediating processes, such as social activities, sports, attire selection, and other less overt processes, may lie between weather and mood.

[34] All results demonstrate robustness to the application of a multiple linear regression model.

possible mood changes triggered by temperature. The focus on maximum rather than minimum temperature is rooted in the fact that individuals can conveniently shield themselves from cold conditions in Western nations. Moreover, minimum temperatures typically occur during the night when people are primarily indoors, within heated accommodations, while maximum temperatures generally happen during the day, when individuals might be outdoors or in non-air-conditioned settings. Consequently, on any given day, people may be more impacted by the maximum temperatures than the minimum ones.

The precipitations variable comes from the GHCN, which combines data from multiple sources and is known for its desirable properties. For example, the NCEI team conducts upstream quality controls, and the dataset offers wide-ranging geographical coverage, even in relatively thinly populated areas. Additionally, the coefficients estimated using GHCN data align closely with those calculated using the GSOD dataset. Although unused variables are omitted from the primary analysis, several supplementary variables have been explored to affirm the robustness of the results and have given rise to similar conclusions.

Table 5.1 lists the final collection of variables chosen post redundancy elimination. Approximately 75% of tweets with emojis are recognized as positive, with the rest being negative. Also, the maximum temperature spans a considerably broad range, with an average maximum temperature nearing 22 degrees Celsius and a standard deviation of 10 degrees. Moreover, it is noteworthy that the variables represent daily averages of readings collected throughout the day (except for the maximum temperature, which is the highest reading recorded in the day). These daily averages temper the impacts of transient phenomena such as fog or wind gusts.

Table 5.1: Descriptive statistics

	mean	sd	min	max	p25	p50	p75
Score of the Tweet	.75	.43	0	1	0	1	1
Maximum Temperature	21.81	10.18	-26.87	46.03	15.89	24.47	29.36
Precipitations	2.9	6.77	0	227.08	.01	.25	2.5
Snow	1.48	10.41	0	366.11	0	0	0
Relative Humidity	64.45	16.05	4.12	99.96	56.2	66.87	75.56
Dew Point	8.91	10.03	-32.49	26.17	1.96	10.61	17.09
Visibility	15.1	2.6	1	87.85	14.37	15.58	16.04
Wind Speed	5.75	2.99	.04	29.87	3.68	5.26	7.38
Short-Wave Radiation	357.40	109.55	22.4	800	281.6	371.2	444.8
Water Vapor Pressure	1306.61	771.36	40	3560	640	1160	1880

Note: Statistics calculated based on the weather data attributed to each tweet. Significance levels: * = 5%, ** = 1%, and *** = 0.1%.

The correlation matrix presented in Table 5.8 in the Appendix offers additional insights into the interrelations between weather variables. Short-wave radiations correlate positively with maximum temperatures ($\rho = 0.36$), implying that sunnier days are warmer. The dew point temperature strongly correlates with the maximum temperature ($\rho = 0.81$), as does the water vapor pressure ($\rho = 0.72$). Additionally, relative humidity negatively correlates with visibility ($\rho = -0.54$). Overall, these relationships appear intuitive at first sight. However, it is crucial to note that several variables are inherently interrelated, with some offering the possibility to infer or approximate another variable. For instance, the temperature and relative humidity can determine the dew point temperature. Furthermore, some variables in the DAYMET database result from the interaction of different input variables, such as temperatures or precipitations. Therefore, monitoring these potential interrelations is crucial to sidestep multicollinearity issues.

5.4.2 Effects of weather on mood

Table 5.2 exhibits the primary findings. The set of variables used in the model (Column 1) mirrors those in Table 5.1, excluding certain variables that potentially correlate highly with the existing variables, such as snow, dew point temperature, and water vapor pressure. The outcomes align with common intuition: precipitations negatively affect mood; the more it rains, the less content individuals are. Short-wave radiations, a proxy measure of visible light, lead to a statistically significant mood uplift, while maximum temperature suppresses mood. Visibility and wind strength, however, do not significantly impact mood.

Table 5.2: Effects of weather on mood (benchmark)

Daily Timeframe:	0-24		12-24		0-24
	(1)	(2)	(3)	(4)	(5)
Maximum Temperature	-0.0006**	-0.0006**	-0.0008***	-0.0008***	-0.0006**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Precipitations	-0.0004**	-0.0004***	-0.0004*	-0.0003*	-0.0004**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Short-Wave Radiations	0.0001***	0.0001***	0.0001***	0.0001***	0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Relative Humidity	0.0003**	0.0003**	0.0002	0.0002	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Visibility	0.0004		-0.0007		
	(0.0006)		(0.0007)		
Wind Speed	-0.0001		0.0002		
	(0.0005)		(0.0005)		
Short-Wave Radiations (Av. Week)					0.0001*
					(0.0000)
Relative Humidity (Av. Week)					0.0005**
					(0.0002)
States Fixed Effects	Yes	Yes	Yes	Yes	Yes
Months Fixed Effects	Yes	Yes	Yes	Yes	Yes
Days of Week Fixed Effects	Yes	Yes	Yes	Yes	Yes
Hours Fixed Effects	Yes	Yes	Yes	Yes	Yes
Festive Days Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	1,770,871	1,770,871	1,230,925	1,149,494	1,770,871

Note: Robust standard errors are clustered at month-county level. Only coefficients of interest are reported for clarity. Significance levels: * = 10%, ** = 5%, and *** = 1%.

Controls for each state, month, day of the week, hour, and festive day are overwhelmingly significant (*not shown*), indicating mood variations across states and over time. Figure 5.1 in the Appendix showcases mood fluctuation throughout the week, peaking on Sunday, dipping on weekdays, and surprisingly, starting relatively high on Monday. While the effects seem intuitive for six days of the week (Tsutsui, 2013), the effect calculated for Monday seems counterintuitive. Monday is usually seen as depressing, hence the famous "Blue Monday." This could be explained by different communication patterns on Mondays, such as exchanging good wishes for the week ahead, often accompanied by positive emojis.

Column 2 of Table 5.2 omits visibility and wind speed, deemed to have no mood influence, without altering the estimates. Column 3 filters out tweets from the morning hours, postulating people's greater exposure to weather in the afternoon. This leads to a rise in the maximum temperature coefficient while other coefficients remain stable or slightly lose statistical significance. Column 4 uses the same approach as in Column 2 but restricts the daily timeframe to the second part of the day, as in Column 3, finding similar results. Column 5 introduces the average sunshine of the preceding five days and relative humidity, with similar coefficients except for a loss of statistical significance for relative humidity.

Table 5.9 in the Appendix performs several robustness checks. Column 1 employs binary variables for each month-state pair instead of separate controls. Then, in Column 2, I include additional control variables: the day of the week, the hour, and the fact that it is a festive day. Column 3 further adds control variables like the number of followers, the accounts followed by the user, tweets liked, and whether English is the user's language. Lastly, Column 4 applies the same configuration as Column 3, but uses separate binaries for months and states instead of combinations. These tweaks do not significantly affect the estimates.

The role of extremes Table 5.3 proposes an alternate specification, postulating that significant fluctuations in maximum temperature result in mood alterations. Columns 1 and 2 in Table 5.3 incorporate a binary variable set to 1 if the maximum temperature deviates by at least six degrees Celsius from the state's monthly average and 0 otherwise. Results reveal that temperature surges six degrees above the state's monthly average lead to a substantial mood dip (significant at 0.1%), whereas downward fluctuations of the same magnitude result in a milder opposite effect, significant at the 10% level.

Columns 3 and 4 adjust the framework to account for states with lesser climate variability. Specifically, the maximum temperature is assigned binary values, with the top and bottom 10% of the month's maximum temperatures equating to 1 in each respective column and 0 otherwise. The findings largely mirror those of Columns 1 and 2.

Additionally, interpreting the role of relative humidity in earlier models presents a challenge. A 35-degree Celsius climate is more unbearable at 80% humidity than at 40%. The interplay of humidity and temperature substantially impacts human comfort levels. Consequently, Table 5.10 in the Appendix employs the Heat Index, a measure that fuses humidity and temperature.[35] This model is run on a subsample, as the Heat Index formula is undefined for temperatures below 27 degrees or relative humidity below 40%. The results (Column 1) suggest

[35]For a detailed explanation of the Heat Index calculation, visit NOAA's website: https://www.wpc.ncep.noaa.gov/html/heatindex_equation.shtml.

Table 5.3: Effects of weather deviations on mood

Specification:	> 6°C	< 6°C	Top 10%	Bottom 10%
	(1)	(2)	(3)	(4)
Binary: Maximum Temperature > 6°C Monthly State Mean	-0.0147***			
	(0.0043)			
Binary: Maximum Temperature < 6°C Monthly State Mean		0.0079*		
		(0.0043)		
Binary: Maximum Temperature in Monthly State Top 10%			-0.0160***	
			(0.0036)	
Binary: Maximum Temperature in Monthly State Bottom 10%				0.0100***
				(0.0037)
Precipitations	-0.0004***	-0.0004***	-0.0004***	-0.0005***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Short-Wave Radiations	0.0001***	0.0001***	0.0001***	0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Relative Humidity	0.0003**	0.0003**	0.0002**	0.0003**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
States Fixed Effects	Yes	Yes	Yes	Yes
Months Fixed Effects	Yes	Yes	Yes	Yes
Days of Week Fixed Effects	Yes	Yes	Yes	Yes
Hours Fixed Effects	Yes	Yes	Yes	Yes
Festive Days Fixed Effects	Yes	Yes	Yes	Yes
Observations	1,770,871	1,770,871	1,770,871	1,770,871

Note: Robust standard errors are clustered at month-county level. Only coefficients of interest are reported for clarity. Significance levels: * = 10%, ** = 5%, and *** = 1%.

that the perceived heat has a negative impact on individuals, while the statistical significance of precipitations and short-wave radiations drops off. A plausible explanation could be that precipitations correlate with humidity, and radiations with heat.

Temperature non-linearity The association between weather conditions and mood could be non-linear (Baylis, 2020), implying that individuals might have a preferred temperature, with deviations leading to discomfort. For example, the ideal temperature for Americans is reportedly 18.3 degrees Celsius (Albouy et al., 2016).

I restructure the maximum temperature variable into several binary categories to examine this hypothesis.

Figure 5.2 in the Appendix shows the relationship of interest using indicator variables for each 5-degree Celsius interval by indicating the probability of having a good mood depending on the temperature level. The probability of being in a good mood is satisfactorily lower from the interval 30–35. Additionally, the interval above 35 degrees reveals broad standard deviations due to the limited observations compared to the entire sample. These insights align with key studies (Barrington-Leigh, 2008; Baylis et al., 2018; Denissen et al., 2008; Tsutsui, 2013), proposing that in developed regions, temperature influences mood only beyond a specific threshold, and mitigating heat effects could be more intricate than warding off cold temperatures.

To further investigate the non-linear relationship between temperature and mood, I employ a polynomial form of the maximum temperature, encompassing the maximum temperature and its square, shown in Figure 5.3 in the Appendix. As expected, the probability of being in a good mood decreases as the maximum temperature increases. In Figure 5.4, I estimate a restricted cubic spline with 4 knots positioned at the percentiles advocated by Harrell Jr (2001): 5, 35, 65, and 95. Although the relationship does not endorse a statistically significant difference between two maximum temperature levels, the pattern is consistent with the findings of Albouy et al. (2016), suggesting an increase up to roughly 18 degrees and a subsequent decrease. These results may suggest that the temperature's impact turns negative beyond a certain threshold, while remaining relatively constant before reaching this point.

I undertake a similar exploration with the Heat Index. Columns 2 and 3 of Table 5.10 in the Appendix present the estimates employing a squared term and bins, respectively. The relationship manifests as mildly non-linear in Column 2, while Column 3 does not substantiate any particular curvature.

Climate regions Different climatic conditions may prompt diverse reactions to weather variations. For instance, rainfall could be a boon in arid regions grappling with drought, while it might be undesirable in wet regions. Likewise, higher temperatures may be welcomed in typically colder zones. To investigate this, I rely on the climate region definitions provided by NOAA.

The Benchmark model is applied to each climate sub-sample, integrating standard controls and variables for Maximum Temperature, Precipitations, and Short-Wave Radiations. The relative humidity is excluded from the model due to its marginal significance. Additionally, the maximum temperature variable is input linearly instead of via a non-linear relationship, as smaller subsets could be prone to data noise and restricted sample sizes. Moreover, certain bins might be distinctly missing for specific regions.

Table 5.4 displays the results of the regressions. I find that the sign of the effect of maximum temperatures on mood rarely changes across regions. Specifically, the effect remains negative at 10%, 0.1%, and 10% for the Central, Southeast, and East North Central Regions, respectively. This observation aligns with the fact that the Central, East North Central, and Southeast Regions are defined by a hot-summer humid continental climate or a humid subtropical climate as per the Köppen climate classification system. These climates encounter particularly elevated temperatures in the summer coupled with high ambient humidity, which can pose challenges for the human body.

Conversely, individuals in the West North Central region do not exhibit significant temperature impacts. This region comprises several climates with mainly hot semi-arid summers or more Mediterranean ones. The results

Table 5.4: Effects by climate region

Climate Region:	Central (1)	Northeast (2)	Southeast (3)	Southwest (4)	Northwest (5)	Easth North Central (6)	South (7)	West North Central (8)	West (9)
Maximum Temperature	-0.0012**	-0.0006	-0.0027***	0.0003	-0.0008	-0.0014*	-0.0003	0.0031	-0.0007
	(0.0006)	(0.0006)	(0.0007)	(0.0011)	(0.0017)	(0.0008)	(0.0006)	(0.0020)	(0.0009)
Precipitations	-0.0000	0.0001	-0.0007**	-0.0003	-0.0005	-0.0001	-0.0004	0.0017	-0.0008
	(0.0005)	(0.0003)	(0.0003)	(0.0013)	(0.0011)	(0.0007)	(0.0003)	(0.0018)	(0.0006)
Short-Wave Radiations	0.0001***	0.0000	0.0001*	0.0001	0.0001	0.0001**	0.0000	-0.0001	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
States Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Months Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Days of Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hours Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Festive Days Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	273,985	364,136	316,978	77,502	44,343	116,701	339,365	15,962	221,899

Note: Robust standard errors are clustered at month-county level. Only coefficients of interest are reported for clarity. Significance levels: * = 10%, ** = 5%, and *** = 1%.

remain intuitive, as the impact of short-wave radiations remains positive and significant for several regions and inconclusive otherwise. Lastly, precipitation proves detrimental in the Southeast region, while the estimates for other regions remain uncertain. The Southeast region likely finds precipitation unfavorable as it often accompanies thunderstorms and extreme conditions.

5.4.3 Attempt at valuing implied changes

In the prior Section, the dependent variable functioned as a surrogate for mood. Nonetheless, Baylis (2020) proposes a more stringent assumption, equating the sentiments articulated in tweets with "experienced utility." [36] Compared to "decision utility" (Kahneman, 2000; Kahneman & Sugden, 2005), experienced utility is a measure encapsulating moment-specific satisfaction or happiness, swayed by the immediate environment and personal choices (Kahneman, 2000). Levinson (2012) states that employing experienced utility in discrete choice models as a tool for valuing a time-varying local public good is plausible "as long as respondents with higher latent utility are more likely to say they are happier." This proposition entails that sentiments disclosed in tweets can provide a reliable approximation of people's experienced utility or well-being.

Hence, this exploration tentatively assumes that the dependent variable represents a proxy of experienced utility. This hypothesis makes it possible to conjecture about the value of weather conditions by dividing the response of experience utility to weather conditions by the response of experienced utility to income changes. Refer to Section Methodology: Estimating the value of weather conditions for estimation methodology details in the Appendix. Please see Levinson (2012) and Baylis (2020) for comprehensive discussions on these aspects.

Nonetheless, it is crucial to remember that while the sentiment valuation is intriguing, it inherently requires several key assumptions, rendering it a provisional exercise. First, income changes should be exogenously determined in relation to expressed sentiment (Baylis, 2020). Second, expressed sentiment must be a valid proxy for utility – however, there is no conclusive evidence affirming or refuting a link between subjective well-being and

[36] Throughout this discussion, I interchangeably use the terms happiness, subjective well-being, and experienced utility to refer to the dependent variable.

individual utility (e.g., refer to Diener, 2000; Kahneman and Krueger, 2006; MacKerron, 2012).[37] The strong nature of these assumptions necessitates a cautious interpretation of the results.

Building on Section 5.4.2, the Benchmark model is augmented to incorporate an income variable, assuming income is exogenous. As noted by Levinson (2012), the measure of income can bring forth two important concerns. Firstly, a measurement error issue: income might be inaccurately estimated. In this study, due to user-level income data's unavailability, income is proxied using the average household income derived from the geographic area of users' tweets, a broad approximation that may not truly reflect individual Twitter users' income. The geographic areas are census tracts for which I obtain 2010-2014 income estimates from the American Community Survey (ACS). Secondly, and more fundamentally, a concern emerges about the potential endogeneity of income with respect to happiness. The primary assumption in this analysis is that a higher income engenders greater happiness (or, in this context, more positive sentiment expressed in tweets). However, it is also plausible that the correlation is bidirectional: inherently happier or more positive individuals may attain higher incomes. This implies that income is not purely exogenous in our model, but rather a variable affected by the same unobservable factors influencing the expressed sentiment in tweets. These potential concerns stipulate that the results of this section must be interpreted with utmost caution.

The Marginal Rate of Substitution (MRS) between income and temperature in Column 1 of Table 5.5 is equal to \$437. This might suggest that individuals could be willing to forego \$437 annually to avoid a 1-degree Celsius increase.[38] To obtain the willingness-to-pay (WTP) per day, Levinson (2012) recommends dividing the coefficient of the annual income variable by 365. Thus, people might be inclined to pay slightly less than \$1.2 per day to avoid a 1-degree Celsius increase in daily maximum temperatures.

This exploratory approach can be extended to other weather variables. For instance, a similar calculation for precipitations suggests that individuals might be willing to forego approximately \$0.65 to avoid a 1mm increase in daily rainfall. However, daily precipitations may not be the most appropriate measure, as the intuition may suggest that it is more relevant to consider whether it rained or not and the recent weather conditions. Similarly, the same approach can be used to estimate the value of short-wave radiations, which suggests that individuals are willing to reduce their daily income by approximately \$0.5 to achieve an increase of one unit of short-wave radiations.

Column 2 of Table 5.5 considers the notion of diminishing marginal returns to income by using the logarithm of income instead of the income level. Using logarithms requires a slight alteration in the calculation of MRS between income and weather variables by multiplying the ratio by the average household income. According to the Census Bureau, the average income in the sample is \$65,675, which exceeds the average income of \$53,657 for

[37]Another crucial concern is the assumption that the dependent variable, perceived as a utility proxy, can be treated as cardinal and hence measured and compared between individuals. This assumption conflicts with the concept of ordinal utility.

[38]This study utilizes short-term weather conditions to understand the long-term climate phenomenon, analogous to Levinson (2012)'s study of air quality. Although weather conditions and climate operate on different temporal scales, their inherent relationship is of importance. Daily weather patterns contribute to long-term climate trends. Nonetheless, it is crucial to highlight that people can adapt to consistent conditions over time, hence short-term fluctuations might provoke more discernible impacts on sentiments, indicating stronger sentiment responses than steady climatic conditions.

Table 5.5: Valuing weather

Income Specification:	Level	Log	Level	Log
Model	Benchmark		Additional Controls	
	(1)	(2)	(3)	(4)
Tract Income	0.0000014***		0.0000014***	
	(0.0000001)		(0.0000001)	
Log(Tract Income)		0.0824432***		0.0865601***
		(0.0033860)		(0.0035539)
Maximum Temperature	-0.0006130**	-0.0006279**	-0.0008566***	-0.0008728***
	(0.0002505)	(0.0002506)	(0.0002584)	(0.0002585)
Precipitations	-0.0003296**	-0.0003313**	-0.0002535	-0.0002553
	(0.0001599)	(0.0001598)	(0.0001688)	(0.0001687)
Short-Wave Radiations	0.0000524***	0.0000519***	0.0000546***	0.0000541***
	(0.0000142)	(0.0000142)	(0.0000148)	(0.0000148)
Log(Followers)			-0.0626236***	-0.0625607***
			(0.0027966)	(0.0028059)
Log(Following)			0.0687026***	0.0686296***
			(0.0024064)	(0.0024070)
Log(Favourites)			0.0304966***	0.0304398***
			(0.0010051)	(0.0010070)
Binary: English Language			-0.2869785***	-0.2871089***
			(0.0050742)	(0.0050718)
States Fixed Effects	Yes	Yes	Yes	Yes
Months Fixed Effects	Yes	Yes	Yes	Yes
Days of Week Fixed Effects	Yes	Yes	Yes	Yes
Hours Fixed Effects	Yes	Yes	Yes	Yes
Festive Days Fixed Effects	Yes	Yes	Yes	Yes
Source of the Tweet	No	No	Yes	Yes
Observations	1,762,258	1,762,258	1,605,896	1,605,896
WTP for a one degree reduction	$437	$500	$612	$662

Note: Robust standard errors are clustered at month-county level. Only coefficients of interest are reported for clarity. Significance levels: * = 10%, ** = 5%, and *** = 1%.

the US population in 2014. This observation suggests that Twitter users are somewhat more privileged than the average person or tend to tweet from areas where households earn higher incomes.

Following Column 2, the resulting willingness-to-pay to reduce 1 degree Celsius might be $500 per year or $1.37 per day, similar to the estimates in Column 1. Columns 3 and 4 replicate the results in Columns 1 and 2 while controlling for external characteristics of user profiles. The WTPs are somewhat higher but are not significantly different. For instance, Column 4 suggests a willingness to pay $1.81 per day for a one-degree Celsius daily temperature reduction, compared to $1.37 in Column 2.

Again, given the substantial assumptions underpinning this valuation strategy, the results should be interpreted cautiously. The tentative figures from this section should be viewed as a starting point for further research, not definitive findings. This approach to the valuation of weather conditions and their implications on sentiment is exploratory in nature, and more rigorous testing and research are required to substantiate these preliminary observations. The motivation to pursue this research path does not hinge on its lack of weaknesses. Contrarily, the intriguing aspect of this methodology lies in its unique limitations when juxtaposed with conventional valuation methods for public goods. This distinctive viewpoint makes it a fascinating benchmark, amplifying our comprehension of the interplay between sentiment and weather.

5.4.4 Alternative specifications

Column 1 of Table 5.6 tests a subsample of tweets sent between 5 pm and 9 am to check whether individuals are more exposed to the weather during these hours, as they are less likely to be in air-conditioned offices or classrooms. Results show no change in intuition, with individuals negatively affected by higher temperatures.

Table 5.6: Alternative specifications

Daily Timeframe:	5pm - 9am	0-24						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Maximum Temperature	-0.0007**	0.0007	-0.0003	-0.0006**	-0.0001	-0.0007**	-0.0006**	-0.0006**
	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Precipitations	-0.0003	-0.0004**	-0.0004**	-0.0003**	-0.0003**	-0.0003**	-0.0003**	-0.0003**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Short-Wave Radiations	0.0000	0.0000***	0.0001***	0.0001***	0.0000***	0.0001*	0.0000***	0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)
Maximum Temperature × Maximum Temperature		-0.0000***						
		(0.0000)						
Tract Income			0.0000***					
			(0.0000)					
GTrends: Air Conditioner			0.0026*					
			(0.0015)					
Maximum Temperature × GTrends: Air Conditioner			-0.0001*					
			(0.0001)					
Log(Tract Income)				0.0740***				
				(0.0076)				
Maximum Temperature × (Log(Tract Income)- Log($\overline{TractIncome}$))				0.0004				
				(0.0003)				
Maximum Temperature (Av. Week)					0.0006			
					(0.0004)			
Maximum Temperature × Maximum Temperature (Av. Week)					-0.0000**			
					(0.0000)			
Short-Wave Radiations (Av. Week)						0.0001		
						(0.0001)		
Short-Wave Radiations × Short-Wave Radiations (Av. Week)						-0.0000		
						(0.0000)		
Weekend							0.0118*	0.0122
							(0.0066)	(0.0089)
Weekend × Maximum Temperature							-0.0002	
							(0.0002)	
Weekend × Short-Wave Radiations								-0.0000
								(0.0000)
States Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Months Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Days of Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hours Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Festive Days Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,156,604	1,770,871	1,762,258	1,762,258	1,770,871	1,770,871	1,770,871	1,770,871

Note: Robust standard errors are clustered at month-county level. Only coefficients of interest are reported for clarity. Significance levels: $^{*} = 10\%$, $^{**} = 5\%$, and $^{***} = 1\%$.

Column 2 of Table 5.6 incorporates the Benchmark model and the squared temperature. The coefficient of the temperature square is negative and significant, whereas the single temperature coefficient is insignificant. However, the estimates are jointly significant, implying an increased sensitivity to extreme temperatures.

Column 3 of Table 5.6 introduces an interaction term between the maximum temperature and the weekly search intensity by state for the keyword "Air Conditioner" from Google Trends. A high search intensity implies that many users search for this keyword on Google. The interaction term is only significant at 10% and is negative. However, the interaction term and the estimate for temperature are jointly significant at the 1% level. The negativity of the interaction term suggests that high temperatures, combined with many heat protection searches, negatively correlate with individuals' happiness. This finding is reasonable since people may be more likely to look for ways to protect themselves during heat peaks.

Column 4 attempts to examine whether the temperature varies with the user's income proxy by including an interaction term between the temperature and the difference between the income logarithm and the average income logarithm. The interaction term is positive but insignificant, while the temperature coefficient decreases slightly in absolute value. Although the interaction term alone is insignificant, the temperature and interaction estimates are jointly significant. This finding suggests that wealthier individuals might be willing to pay less to avoid a 1-degree increase in temperature. More specifically, individuals in households at the 75th income percentile might be willing to pay \$1.15 per day, while households at the 25th income percentile might be willing to pay \$1.61. These results suggest that higher-income individuals may have better access to resources to help them cope with high temperatures, such as working in air-conditioned environments or owning homes that are better optimized for extreme temperature variations.

Columns 5 to 8 examine additional specifications that consider average weekly weather conditions preceding the day of the tweet or the impact of weekends. While the interaction of the temperature of the day with the temperature of the previous temperature conditions is negative and significant, suggesting that a small accumulation effect may play a role, the other interactions are not significant.

5.5 Conclusion

This research provides a nuanced understanding of the intricate relationship between weather conditions, especially temperature, and individual moods. While the influence of weather on daily life is often deemed significant by many (Watson, 2000), counterpoints suggest negligible or peripheral impacts on well-being (Denissen et al., 2008; Keller et al., 2005; Watson, 2000).

Employing an innovative methodological approach that embraces a variety of meteorological variables and capitalizes on mood indicators indirectly revealed through Twitter data, this study substantiates the widespread belief that weather conditions, notably precipitation and temperature, significantly influence moods—typically negatively. In addition, this analysis tentatively suggests that individuals might be inclined to pay between \$1.2 to \$1.8 to avoid a one-degree Celsius temperature increase.

The research underscores the pivotal role of extreme temperature fluctuations in inducing considerable mood shifts, thereby highlighting the intricate and potentially non-linear characteristics of the temperature-mood relationship. I observe a critical threshold beyond which an increase in temperature exerts negative effects on moods—an observation consistent with prior research implying that mitigating the impacts of heat may pose greater challenges than dealing with the impacts of cold (Denissen et al., 2008; Keller et al., 2005; Watson, 2000).

This research, however, acknowledges certain limitations. Firstly, the dependent variable employed as a mood proxy might be influenced by unknown factors due to its indirect nature (Li et al., 2014). While previous studies demonstrate that emoticons and emojis generally convey individuals' underlying emotions (Kelly & Watts, 2015; Lebduska, 2014; Miller et al., 2016; Shiha & Ayvaz, 2017), the indirect association between these techniques and individuals' explicit moods necessitates further research (Wang et al., 2014). Secondly, the binary nature of the response variable could reduce variation in mood indicators. Thirdly, the demographic representation among Twitter users differs from that of the US population, potentially underrepresenting groups like the elderly who are particularly vulnerable to extreme weather conditions (Baylis et al., 2018). Lastly, similar to survey-based studies, user behavior on social media platforms might change based on weather conditions and social activities, which could skew the results.

Several factors may explain the discrepancies between the belief that weather strongly influences individuals and the magnitude of the results. First, cultural and historical heritage may play a role (Denissen et al., 2008), as people in Western countries once relied on the weather for their food supply, and a hot, dry summer was often associated with famine. Second, individuals tend to form their understanding of phenomena based on personal observations (Bossema et al., 2013). Weather conditions, as is mood, are continually changing elements surrounding people's daily lives. A link can be established between the two variables without necessarily being scientifically supported and amplified by social relationships (Quick, 1997). Finally, the high rate of air conditioning in the United States, with people spending significant amounts of time indoors in air-conditioned environments, may attenuate the effects (Baylis et al., 2018).

Despite these limitations, the methodology offers substantial benefits over conventional methods. It proposes a cost-effective, adaptable, language-neutral, and immediate means of examining mood indicators. This innovative approach could shape policymaking, helping improve individual well-being and alleviate climate change's negative impacts. Future research should unearth the potential mechanisms and pathways underlying the relationship between weather and well-being and incorporate alternative data sources to validate and enhance this study's limitations.

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Appendix

Data

Definitions, Units, and Sources of Weather Variables

Table 5.7: Weather variables: Definitions, units of measurement, and sources

Variable	**Definition**	**Unit of measurement**	**Source**
Maximum Temperature	Daily 2-meter air maximum temperature	Degrees Celsius	GHCN
Precipitations	Daily total precipitation	Millimeters	GHCN
Snow	Daily total snowfall	Millimeters	GHCN
Relative Humidity	Daily average relative humidity	Percent	GHCN
Dew Point	Daily average dew point	Degrees Celsius	GHCN
Visibility	Daily average visibility	Kilometers	GSOD
Wind Speed	Daily average wind speed	Tenths of meters per second	GSOD
Short-Wave Radiations	Radiant energy flux density emitted by the sun	Watts per square meter	DAYMET
Water Vapor Pressure	Daily average of the pressure of water vapor	Pascals	DAYMET
Heat Index	Index combining the air temperature with the main relative humidity to obtain a human-perceived temperature	Degrees Celsius	Author's calculations

Weather

The data sources are the following:

- The first source is the **Global Historical Climatology Network (GHCN)** database managed by NOAA's National Centers for Environmental Information (NCEI). It includes daily observations from more than 100,000 surface stations in over 180 countries. To achieve this, NCEI combines about 30 data sources obtained through personal contact with representatives of various meteorological centers worldwide. Also, the sources are complemented by Cooperative Observer Program (COOP) data. The COOP comprises about 9000 volunteers who take meteorological measurements where people live, work, and spend their leisure time in the United States. The resulting database covers several decades of daily data while offering valuable quality assurance checks for potential irregularities.
- The second source is the **Global Surface Summary of the Day (GSOD)** dataset. The data are derived from the Integrated Surface Hourly (ISH) dataset by the USAF Climatology Center. While the GHCN database has more land stations than the GSOD before the 1980s, the GSOD generally shows better coverage in recent years. However, Stokes (2010) points out that the GSOD is unbalanced by having many stations in specific parts of the world, while the GHCN is more evenly distributed.
- The last data source is **DAYMET**. DAYMET differs from the other two sources because it is a data product derived by a model interpolating and extrapolating weather data from weather observations from the GHCN. The model first creates 2-degree by 2-degree tiles and then processes each tile individually using the interpolation method developed by Thornton et al. (1997). The model reduces its station search radius in areas of rich-station density while extending in poorly covered regions. The resulting DAYMET product comprises gridded estimates of daily weather variables for North America at a scale of 1 kilometer by 1 kilometer.

The first two sources vary mainly in the information and cleaning processes applied. In contrast, the third source offers a product already processed and derived from a model.

Mood

All emojis with at least 50 occurrences referenced in the Emoji Sentiment Ranking are considered. The threshold of 50 occurrences is arbitrarily defined to meet a certain agreement in their meaning and select the first 300 most used emojis. However, other thresholds (25 and 75) are tested, leading to the same conclusions. The formula can be written as follows:

$$E_x = \begin{cases} Positive & \text{if } x_P > 1.2\ x_N \text{ and } x_P > 1.2\ x_O \\ Negative & \text{if } x_N > 1.2\ x_P \text{ and } x_N > 1.2\ x_O \\ Mixed & \text{otherwise} \end{cases} \tag{5.4}$$

where E_x is the emotion attached to the emoji x, and X is the total number of occurrences of tweets with the emoji x. x_P, x_N, and x_O are the occurrences of tweets with the emoji labeled positive, negative, and neutral, respectively.

Methodology: Estimating the value of weather conditions

The task of attributing a value to weather conditions is uniquely challenging, primarily due to the lack of a defined market. Various methodologies have been developed to address this issue, including but not limited to, travel-costs models, hedonic valuation, location choice models, contingent valuation, and subjective well-being-based models (Albouy et al., 2016; Blomquist et al., 1988; Cragg & Kahn, 1997, 1999; Ferreira & Moro, 2010; Levinson, 2012; Rehdanz & Maddison, 2005; Roback, 1982; S. Rosen, 1974; Welsch & Kühling, 2009).

To address some of the shortcomings of existing methods, Levinson (2020) proposes an approach that involves using individual cross-sectional data collected over several months and matching daily averages. This approach efficiently manages regional and temporal effects and mitigates issues like habituation and locational sorting bias. However, there is a risk of overestimation of effects.

In the process of maximizing their utility, individuals consider various factors, such as risk aversion, prices, budget, and social norms (Levinson, 2012). In theory, if we observe the utility or a proxy of it and individuals' current decisions, we could derive the implicit prices of the goods chosen, given certain strong assumptions. For illustrative purposes, let's consider a multiple linear regression model using individual cross-sectional data taken at different times, factoring in the individual's daily weather W, income I, and happiness H. The model, following Levinson (2012), can be expressed as:

$$Happiness = \beta_1 W + \beta_2 I + Characteristics + Time + Location \tag{5.5}$$

Upon completely differentiating the model and setting $dE(y) = 0$ Levinson, 2012, one can solve for the average marginal rate of substitution between the interest variable and income:

$$\frac{\partial I}{\partial W}|_{dHappiness=0} = -\frac{\beta_1}{\beta_2} \tag{5.6}$$

Theoretically, after regressing this model on actual data and dividing the coefficient of interest by the income coefficient, we get the marginal substitution rate between the good of interest and income (Levinson, 2012; Moro et al., 2008). The result is the income required to compensate for a one-unit change in the interest variable. However, this result is based on several critical assumptions (for more details, see the manuscript and Levinson (2012)). Therefore, the results should be interpreted with a great deal of caution.

Additional Material

Tables

Table 5.8: Correlations of selected variables

	Maximum Temperature	Precipitations	Snow	Relative Humidity	Dew Point	Visibility	Wind Speed	Short-Wave Radiations	Water Vapor Pressure
Maximum Temperature	1.00								
Precipitations	-0.01***	1.00							
Snow	-0.28***	0.14***	1.00						
Relative Humidity	-0.13***	0.36***	0.08***	1.00					
Dew Point	0.82***	0.17***	-0.22***	0.43***	1.00				
Visibility	0.20***	-0.27***	-0.19***	-0.55***	-0.10***	1.00			
Wind Speed	-0.15***	0.12***	0.11***	-0.11***	-0.20***	0.03***	1.00		
Short-Wave Radiations	0.36***	-0.24***	-0.12***	-0.49***	0.06***	0.29***	-0.03***	1.00	
Water Vapor Pressure	0.72***	0.09***	-0.18***	0.29***	0.84***	0.01***	-0.22***	0.03***	1.00

Note: Significance levels: * = 5%, ** = 1%, and *** = 0.1%.

Table 5.9: Robustness checks: Effects of weather on mood

	(1)	(2)	(3)	(4)
Maximum Temperature	-0.0012***	-0.0011***	-0.0014***	-0.0008***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Precipitations	-0.0004**	-0.0004**	-0.0003*	-0.0004**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Short-Wave Radiations	0.0001***	0.0001***	0.0001***	0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Relative Humidity	0.0002	0.0002*	0.0002*	0.0003**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Log(Followers)			-0.0667***	-0.0666***
			(0.0028)	(0.0028)
Log(Following)			0.0682***	0.0682***
			(0.0024)	(0.0024)
Log(Favourites)			0.0334***	0.0332***
			(0.0010)	(0.0010)
Binary: English Language			-0.2862***	-0.2862***
			(0.0050)	(0.0050)
States Fixed Effects	No	No	No	Yes
Months Fixed Effects	No	No	No	Yes
Month $\times$ State	Yes	Yes	Yes	No
Days of Week Fixed Effects	No	Yes	Yes	Yes
Hours Fixed Effects	No	Yes	Yes	Yes
Festive Days Fixed Effects	No	Yes	Yes	Yes
Source of the Tweet	No	No	Yes	Yes
Observations	1,770,871	1,770,871	1,613,794	1,613,794

Note: Robust standard errors are clustered at month-county level. Only coefficients of interest are reported for clarity. Significance levels: * = 10%, ** = 5%, and *** = 1%.

Table 5.10: Effect of heat index on mood

	(1)	(2)	(3)	(4)
Heat Index	-0.000969**	-0.013549***		
	(0.0004)	(0.0036)		
Precipitations	-0.000055	-0.000037	-0.000435***	-0.000038
	(0.0003)	(0.0003)	(0.0002)	(0.0003)
Short-Wave Radiations	0.000050*	0.000039	0.000065***	0.000044
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Heat Index × Heat Index		0.000159***		
		(0.0000)		
Maximum Temperature			-0.000673	
			(0.0006)	
Relative Humidity			0.000214	
			(0.0002)	
Maximum Temperature × Relative Humidity			0.000002	
			(0.0000)	
31 <= Heat Index < 35				-0.012522**
				(0.0058)
35 <= Heat Index < 39				-0.017141***
				(0.0064)
39 <= Heat Index < 43				-0.025876***
				(0.0075)
Heat Index >= 43				-0.022755***
				(0.0079)
States Fixed Effects	Yes	Yes	Yes	Yes
Months Fixed Effects	Yes	Yes	Yes	Yes
Days of Week Fixed Effects	Yes	Yes	Yes	Yes
Hours Fixed Effects	Yes	Yes	Yes	Yes
Festive Days Fixed Effects	Yes	Yes	Yes	Yes
Observations	584,714	584,714	1,770,871	584,714

Note: Robust standard errors are clustered at month-county level. Only coefficients of interest are reported for clarity. Significance levels: * = 10%, ** = 5%, and *** = 1%.

Figures

Figure 5.1: Mood by day of the week

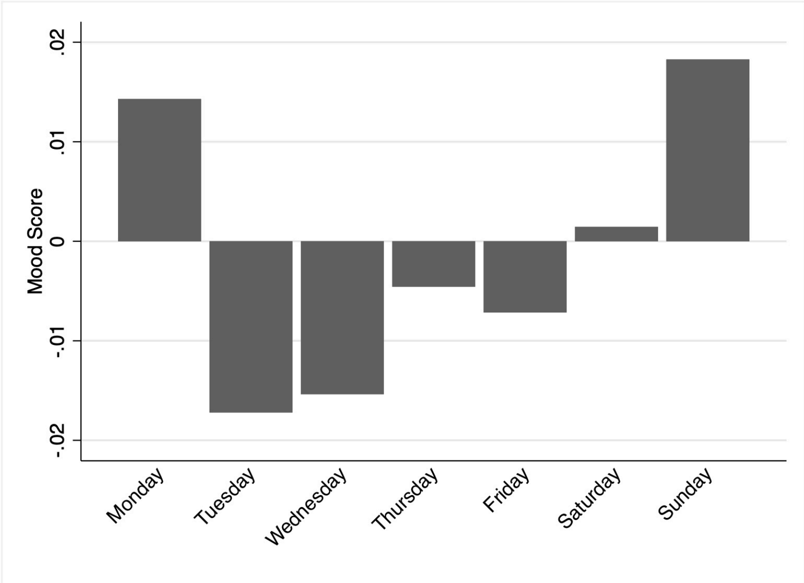


Figure 5.2: Predictions of good mood — Maximum temperature bins

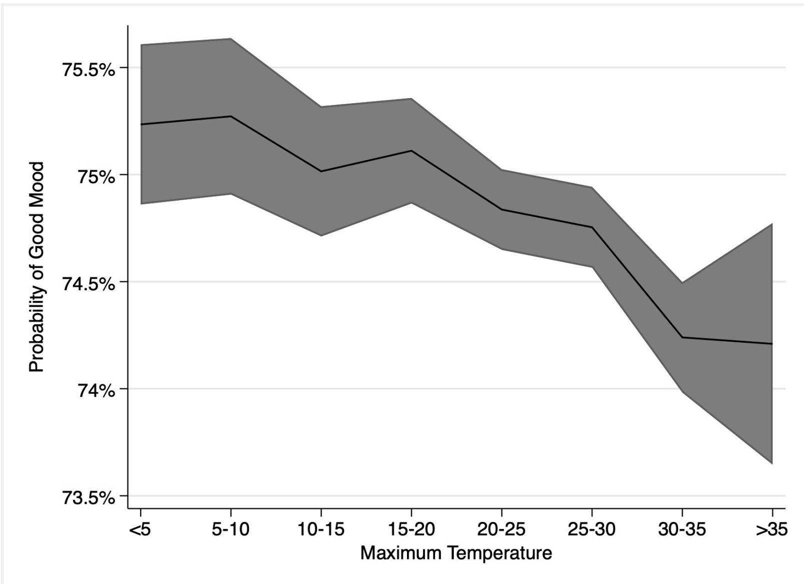


Figure 5.3: Predictions of good mood — Polynomial

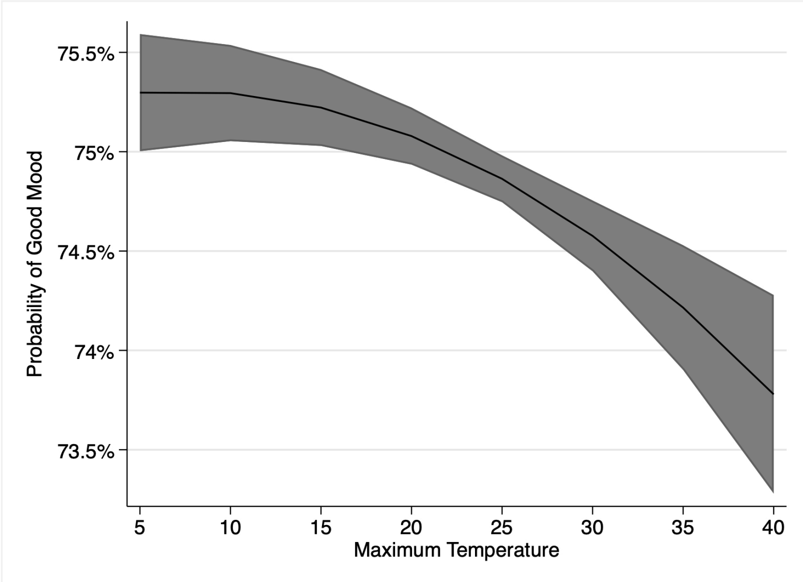
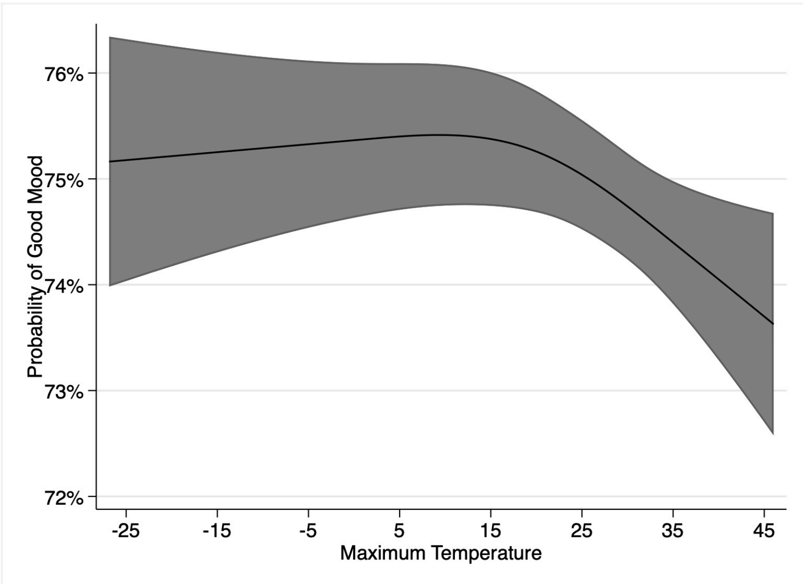


Figure 5.4: Predictions of good mood — Restricted cubic spline



Commuting time and absenteeism: Evidence from a natural experiment

Chapter 6

Commuting time and absenteeism: Evidence from a natural experiment

6.1 Introduction

The time dedicated to traveling to work and the frequency of commuting has increased in recent years in many Western countries (Gimenez-Nadal & Molina, 2016; Kirby & LeSage, 2009; McKenzie & Rapino, 2011). More than 20% of European workers spend more than an hour and a half daily on these trips (Giménez-Nadal et al., 2020).

Commuting can have both positive and negative effects on workers. On the positive side, commuting longer distances can expand the pool of potential job opportunities and increase the chances of finding a better match between workers and job offers (Goerke & Lorenz, 2017). Extending the search radius by commuting longer enables workers to access more housing options when choosing their place of residence (Goerke & Lorenz, 2017). Such matching could potentially positively affect the well-being of individuals and the productivity of firms (Bhat, 2014). However, commuting may also negatively affect individuals. Commuting is one of the least pleasurable activities (Choi et al., 2013; Kahneman et al., 2006; Kahneman et al., 2004). Long commutes reduce available time to engage in physical activities and are a source of daily stress (Gottholmseder et al., 2009; Lucas & Heady, 2002; Novaco et al., 1990; Stutzer & Frey, 2008). The environmental impact of long commutes and exposure to air pollution are also major concerns. Commuting may thus, directly and indirectly, affect individuals' physical health (Evans & Wener, 2006; Evans et al., 2002; Hämmig et al., 2009; Hansson et al., 2011; Künn-Nelen, 2016; Novaco et al., 1990; Roberts et al., 2011) and mental health (Choi et al., 2013; Dickerson et al., 2014; Friman et al., 2017; Gatersleben & Uzzell, 2007; Novaco & Gonzalez, 2009). Such adverse effects on mental and physical health may lead to absenteeism and reduce workers' productivity (Grinza & Rycx, 2020; Oswald et al., 2015).

The relationship between commuting time and employee absenteeism remains relatively understudied in the literature, with few studies moving beyond descriptive associations at the micro-level (Ma & Ye, 2019). Some (mostly descriptive) studies confirm a positive correlation between commuting and absenteeism (Kluger, 1998; Magee et al., 2011), while others find no robust correlation (Künn-Nelen, 2016). Gimenez-Nadal et al. (2022) indicate that a 1% increase in daily commute results in a 0.018% increase in male workers' absenteeism and a 0.027% increase in female workers' absenteeism per year in the US. Most causal studies rely on employer-induced changes in commuting distance due to company relocations and find mixed results. Van Ommeren and Gutiérrez-i-Puigarnau (2011) find that commuting distance increases absences for medical reasons in Germany. Goerke and Lorenz (2017) point towards similar results in Germany again, concluding that only employees with long commutes are 20% more absent than those with no commutes. Hassink and Fernandez (2018) find, in contrast, no effect on monthly absences in the US, except for workers reporting low morale. Ma and Ye (2019) exploit an instrumental variable technique (with commuting instrumented by population density at home or job locations) and find that commuting distance is positively linked to absenteeism in Australia. Finally, Lu et al. (2021) use a natural experiment based on the opening of a subway line affecting commuting in a Chinese city and find no significant change in absenteeism following the opening of the line.

Despite the seemingly evident disutility of the time spent commuting and its potential impact on productivity, empirical evidence of an impact of commuting on absenteeism is limited and mixed. These mixed results can have different explanations. Descriptive studies that do not attempt to control for the simultaneity of location and employment decisions are bound to underestimate the effect of commuting time – workers choosing a longer commute endogenize the disutility of the commute in their decisions. Studies with a design allowing for a plausibly causal interpretation may suffer from low power or measurement error when relying on self-reported survey data (e.g., Gimenez-Nadal et al., 2022; Ma and Ye, 2019). Finally, natural experiments such as the one exploited in Lu et al. (2021) are not ideal as major infrastructures – here, the construction of a subway line – is a permanent and foreseeable shock to commuting time: workers likely anticipate (and therefore endogenize) their future commuting time in their employment and residential location decisions.

Our study exploits another form of natural experiment and better data that provide an improved design for identifying a causal effect of commuting time on absenteeism. We exploit a shock to commuting time induced by major roadworks undertaken on the highway connecting Belgium to Luxembourg in 2018 and 2019. This particular event has at least two attractive characteristics. First, it was relatively large: it significantly affected the commuting time of a large number of workers commuting across the border between the two countries for about seven months. Second, its impact was limited in time. A disruption in commuting time over seven months is unlikely to affect residential location decisions, especially since the roadworks did not lead to any persistent change in commuting time relative to prior levels after completion.

The roadworks affected the commuting time of workers residing in Belgium and working in Luxembourg. This cross-border setting comes with useful features too. Cross-country commuting limits substitution strategies. First, international tax and social security regulations severely constrain remote work possibilities when the worker does not reside in the country of work. Second, the cross-national network of roads and public transport is much more limited than any of the national networks. This limits the possibilities of finding alternative routes to work. Third, the lack of infrastructure cooperation on either side of the border means that the roadworks undertaken on the Belgian side did not lead to any long-run reduction of commuting time (as we explain below).

The setup makes cross-border workers traveling from France toward Luxembourg a natural control group. The transport network structure is analogous – with one main highway connecting France to Luxembourg heading toward Luxembourg City and with limited (often saturated) public transportation alternatives – and the distance from the border to Luxembourg City (where most of the jobs are based) is similar.

Finally, we have access to fine-grained, accurately recorded administrative data on absences from work and data on residential location, individual, and employment characteristics. The analysis uses the recorded absences of all Luxembourg-based private sector workers living in Belgium or France between 2015 and 2019. Employers report absences. Because sickness payments are compensated at 80% from the first day of absence and are fully taken over by the *Caisse Nationale de Santé* from the seventy-seventh day of absence over an 18-month period, employers have an obligation and an incentive to report absences to the social security administration accurately. The absences reported to social security are encoded by type (such as illness, injury, or maternity leave), allowing for fine-grained analysis. Disentangling by cause of absence allows us to get a sense of whether increased absenteeism primarily reflects increased shirking behavior (to avoid the disutility of extended commuting time) or actual adverse health effects of the increased commuting.

In sum, this setup makes for a robust design for assessing any plausibly causal effect of commuting time on absenteeism. Our results show that disruptions to commuting time lead to a significant but quantitatively small increase in absenteeism. However, there seems to be a threshold effect with workers who commute more than 40 kilometers to work responding more strongly to the commuting time shock. Results also highlight significant differences in absenteeism related to gender. Unlike what could be conjectured from usual gender imbalances in family responsibilities, we observe that men are more affected by shocks in commuting time than women. While illness- and family-related absences respond to the commuting shock, we see no change in injury-related absences. This could suggest a predominance of a 'shirking' explanation – rather than a direct health impact – for the increase in absences.

The rest of the paper is structured as follows: Section 6.2 briefly discusses the mechanisms that may link commuting and absenteeism. Section 6.3 presents our methodology, including a description of the natural experiment, data sources, and empirical model. Section 6.4 presents results. Section 6.5 concludes.

6.2 Residential choice, commuting and absenteeism

A simple way to formalize mechanisms linking commuting and absenteeism is through a classic Alonso-Muth-Mills model (Alonso, 2013; Mills, 1967; Muth, 1969; Wheaton, 1974). A worker has preferences over consumption and commuting time represented by an individual utility function $U(C, T)$, where U increases with consumption C and decreases with commuting time T. Employment is located in a single central business district (CBD). Each worker resides around the CBD and travels to the CBD to get to work through a dense radial road network. An agent's residential location away from the CBD determines her commuting time.

In this model, agents choose their residential location l to optimize $U(C, T)$ subject to the constraint $C = W - H$, where W is earnings and H is housing costs. Since U decreases with commuting time T, agents choose, all other things being equal, to live as close as possible to the city center. However, the central business district has a limited housing capacity. The density of the housing market is higher closer to the CBD, which is associated with higher prices per square meter (Brueckner, 1987) to clear the market. So housing costs decrease monotonically with the distance to the CBD. Agents, therefore, make a trade-off between living in a desirable location close to the CBD, which comes with higher housing costs H (and hence lower consumption) but a shorter commute, or living in a less desirable location farther away, leading to longer commutes, which has lower housing density and costs H, but a longer commute T.[39] In equilibrium, the optimal location balances the disutility of commuting with the consumption obtained by lower housing costs (Alonso, 2013; Gutiérrez-i-Puigarnau & van Ommeren, 2010; Mills, 1967; Muth, 1969; Wheaton, 1974; Zenou, 2009).

Commuting time from any location is known and constant in the basic Alonso-Muth-Mills model. In real life, commuting time is, however, stochastic – with variations due to, for instance, incidents, strikes, and weather conditions. In the face of shocks to commuting time, agents are typically unable to re-optimize residential location choices, so the utility is directly affected by such shocks. While it is easy to think of a model in which workers would factor in uncertainty in commuting time when choosing an optimal residential location, stickiness in residential location choices still implies that short-term shocks to commuting affect utility through T. In practice, variations in T that cannot be compensated by adjustments to residential location may lead workers to absenteeism. This may arise through work avoidance behavior ('shirking') with workers calling in sick if significant traffic congestion is expected due to exceptional weather events, roadworks, or strikes (Ross & Zenou, 2008). This may also arise from genuine health shocks caused by a longer commute (Evans & Wener, 2006; Hansson et al., 2011; Künn-Nelen, 2016; Roberts et al., 2011).

Previous studies, such as urban efficiency wage models, have primarily examined the relationship between commuting and absenteeism and productivity through shirking (e.g., Brueckner and Zenou, 2003; Ross and Zenou, 2008; Zenou, 2002, 2009; Zenou and Smith, 1995). These models generally posit that long commutes can have a negative impact on productivity and lead to higher absenteeism as they can take a toll on work effort (Gutiérrez-i-Puigarnau & van Ommeren, 2010). Individuals may choose to shirk at work, depending on the costs associated with doing so (Goerke & Lorenz, 2017; Gutiérrez-i-Puigarnau & van Ommeren, 2010).[40] Some authors suggest that costs of shirking are independent of commuting time (Gutiérrez-i-Puigarnau & van Ommeren, 2010), as workers are not punished differently for shirking when they call in sick for a 20-minute commute versus a 30-minute commute. However, other authors suggest that workers may choose to commute longer in the first place for reasons such as higher wages, better housing, better working conditions (Goerke & Lorenz, 2017; Stutzer & Frey, 2008), or a stronger underlying desire to be involved in their work. As a result, long commuters may disproportionately suffer when punished for shirking, especially when facing a disruption in their commuting time.

[39] This simple model postulates that wages in the CBD are independent of workers location of residence (unlike in, e.g., Ross and Zenou (2008)).

[40] It is worth noting that the efficiency wage theory posits that firms pay higher wages to promote effort and discourage shirking, seen as moral hazard (Shapiro & Stiglitz, 1984). However, monitoring abusive behaviors can be costly and challenging to implement in practice, so employers generally do not pay enough to eliminate workers' shirking entirely (Shapiro & Stiglitz, 1984).

6.3 Data and empirical strategy

Our analysis builds on the idea that, under the following conditions, roadworks can be seen as exogenous events that disrupt commuting time but do not lead to changes in equilibrium locations. First, roadworks must be of sufficient magnitude to impact commuting time. Second, roadworks must be of sufficiently short duration to avoid changes in individuals' structural decisions, such as relocating or changing jobs, between the start and end of the roadworks. Third, roadworks should not result in long-term changes in traffic flows and fluidity. Under these conditions, roadworks can be considered exogenous to employees' decisions and, therefore, a natural experiment through the stochastic variations in commuting time they generate.

6.3.1 Roadworks at the Luxembourg-Belgian border as a Natural Experiment

The theoretical framework outlined in Section 6.2 aligns well with the reality in Luxembourg. Luxembourg is a small and highly urbanized country with a large number of cross-border workers commuting from Belgium, Germany, and France.[41] Most economic activity is centered in Luxembourg City, located at almost equal distances to France, Germany, and Belgium. One-third of Luxembourg's employers are based in Luxembourg City (STATEC, 2021). Luxembourg is characterized by high housing demand and a limited supply, leading to high prices that increase with proximity to Luxembourg City. This has led workers to spread out over large geographical areas, resulting in significant daily flows of commuters beyond the country's borders.

In this context, the natural experiment that we exploit is a road construction project consisting of widening over approximately 10 kilometers of the E411 highway between Arlon and Sterpenich (just before the border between Belgium and Luxembourg) in order to create a carpool lane, as illustrated in Figure 6.1. This project was undertaken as a pilot scheme by the Walloon Region in Belgium to address mobility-related issues. The roadworks took place over seven months, beginning on September 17, 2018, and ending on April 30, 2019. The additional lane was officially opened on May 7, 2019 (Wiessler, 2019).

The E411 highway sees more than 40,000 vehicles cross the border daily, with more than 80% of motorists in Belgium commuting alone in their cars. However, the project has been criticized for its relatively restrictive rules for using the carpool lane, which only permit light vehicles with a minimum of three people and limit the speed to 50 kilometers per hour.[42] Furthermore, the project was carried out without proper consultation with Luxembourg authorities and ended at the border, reducing lanes at the border point and creating a bottleneck. [43]

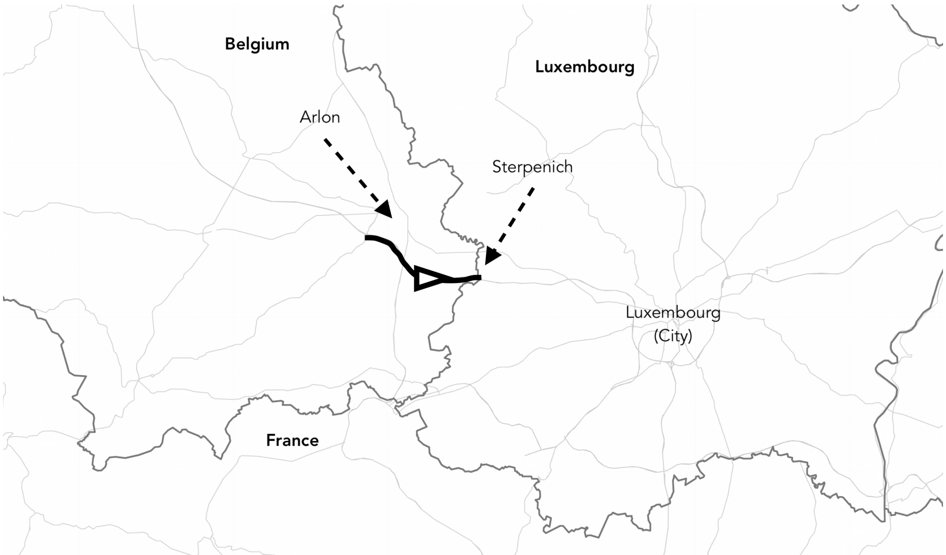
The roadworks resulted in a considerable increase in commuting time. For instance, over 8km of traffic jams were recorded at 6 PM on September 17, 2018 (RTL, 2018). Based on the assumption that the average speed in

[41] STATEC (2019) reports 192,000 workers living outside Luxembourg's borders in 2018.

[42] See https://www.wort.lu/fr/granderegion/la-bande-de-covoiturage-s-avere-etre-un-fiasco-5de4e1a2da2cc1784e351173 (accessed 2023-05-04).

[43] Belgian Minister Philippe Henry even declared in 2021 that the roadworks "look like useless roadworks" (https://www.lessentiel.lu/fr/story/les-motards-utiliseront-la-bande-de-covoiturage-941091627096, accessed 2023-05-04). See also https://paperjam.lu/article/covoiturage-sur-e411-on-retrog (accessed 2023-05-04).

Figure 6.1: Roadworks between Arlon (Belgium) and the Luxembourg border



traffic jams is 20 kilometers per hour, compared to 120 kilometers per hour without congestion, the roadworks added approximately 20 minutes to commuting time. Furthermore, resorting to minor roads as an alternative proved ineffective, as traffic in the villages was heavily saturated, resulting in additional traffic jams and no time-saving advantages. Although public transportation was not directly affected by the roadworks, it is improbable that a significant portion of commuters switched to public transit to avoid roadworks, considering the existing congestion in public transportation systems.[44]

6.3.2 Data

We use administrative microdata from the Luxembourg Microdata Platform on Labour and Social Protection. The platform brings together data extracted from the Common Center for Social Security (CCSS), the Employment Development Agency (ADEM), and the National Health Fund (*Caisse Nationale de Santé*, CNS).

Coverage and worker characteristics

We obtained pseudonymized information on all private-sector, cross-border employees affiliated with the Luxembourg social security system and residing in Belgium and France. The extraction covers the period from 2015 to 2019 and includes information on sociodemographic characteristics, distance from the municipality of residence to the border, job and contract characteristics, and the composition of the firms that employ these workers (see details below).

The population covered and the variables are updated month-by-month from January 2015 to December 2019 to form a panel data structure with a total of 152,249 employees and 5,183,488 person-month observations.

[44]See https://paperjam.lu/article/arlon-met-pression-son-pr-a-vi (accessed 2023-05-04).

Measures of work absences

The extraction contains data provided by the CNS on the number of recorded absence days for each employee in every month of the period covered. Absences are categorized into six distinct types: illness-related, pregnancy-related, injury-related, family-related, maternity-related, and palliative-related. Illness-related absences occur when an employee is unable to work due to sickness, while pregnancy-related absences apply to work exemptions in the framework of a protection scheme exclusively for pregnant or nursing women. Injury-related absences result from work incapacitation due to injuries. Family-related absences refer to leaves granted to a parent when their child is ill and no alternative childcare option is available. Maternity-related absences encompass parental leaves for the birth or adoption of a child, and palliative-related absences involve time off for end-of-life care.

The legal system allows for some degree of flexibility in reporting absences. First, employees incapacitated from work due to illness- or injury-related reasons may be absent for up to two consecutive days without providing a medical certificate but by still notifying the employer from the first day of absence. Nevertheless, since sickness payments are compensated at 80% from the first day of absence (whether medically justified or not), and the *Caisse Nationale de Santé* entirely takes over payments from the seventy-seventh day of absence within an 18-month time frame, employers are both obligated and motivated to accurately report absences to the social security administration. Second, family-related absences, which involve leaves granted to a parent when their child is ill and no alternative childcare is available, may also be prone to misuse. Employers face difficulties verifying the child's illness or the unavailability of alternative childcare options. This leniency in absence reporting could be exploited by employees seeking to take time off without legitimate grounds, particularly in the event of commuting time shocks.

Absences due to illness are the most common, with an average of 0.88 days of absence per employee-month; see Table 6.1. All types combined, the average number of days of absence per month in our data is 1.13 (or 5.18 percent of working days). On average, 16.53 percent of workers claim at least one day of absence per month.

Table 6.1: Absence statistics by type

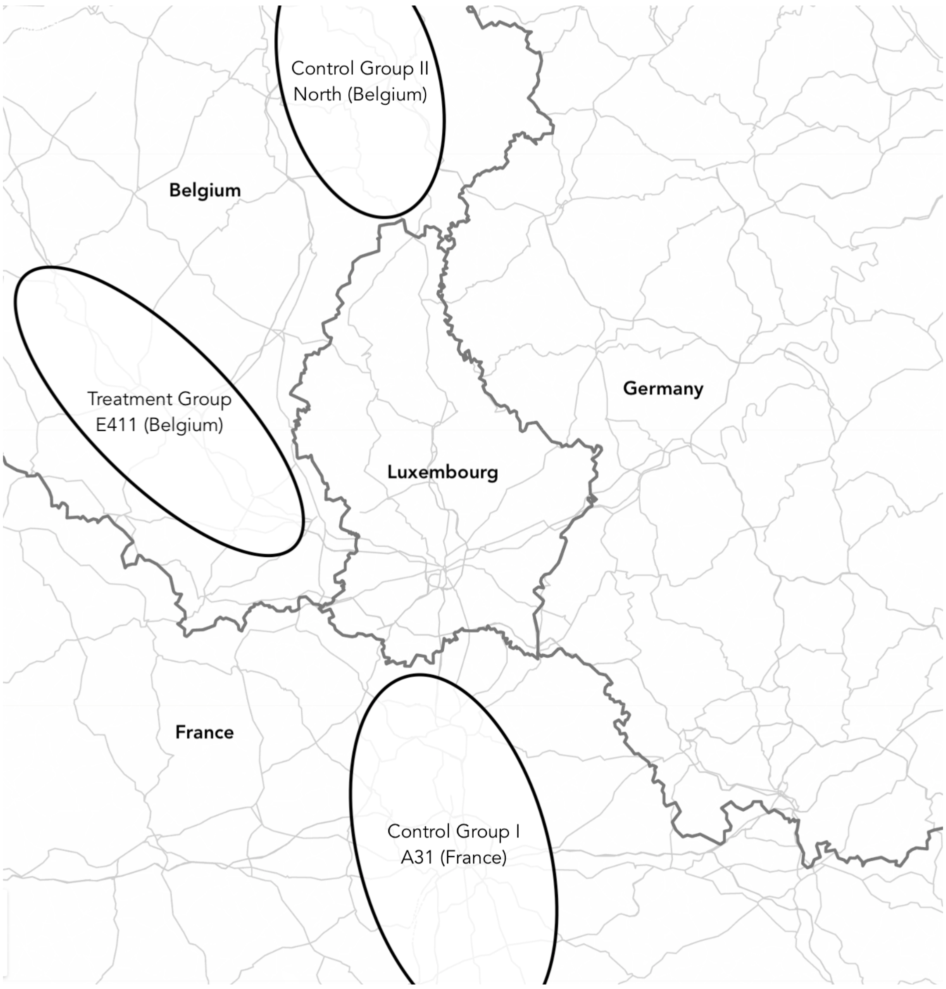
	Average days of absence per month	Share of absent days (in % of working days)	Percentage of employees with at least one day of absence per month
All reasons	1.125	5.185	16.526
By reason			
Illness	0.878	4.048	14.356
Pregnancy	0.180	0.827	0.914
Injury	0.048	0.223	0.442
Family	0.018	0.084	1.152
Maternity	0.000	0.001	0.001
Palliative	0.000	0.001	0.008

Definition of treatment and control groups

We define individuals residing in Belgian municipalities around the E411 highway and upstream from the location of the roadworks in September 2018 as our Treatment Group. We select municipalities ('communes') in close proximity to or intersected by the E411 highway, which was impacted by the roadworks, where few viable alternative routes exist for bypassing the highway and avoiding traffic jams.

We define two control groups that have not been affected by the roadworks and are otherwise similar to treated cases. The first control group is composed of employees residing in France along the A31 highway in September 2018 (Control Group I). The second control group is composed of employees residing in Belgium but in municipalities farther north and away from the E411 roadworks (Control Group II). Figure 6.2 illustrates this construction (details of the geographical areas assigned to the three groups are given in Appendix Groups Composition).

Figure 6.2: Treatment and control groups



The Treatment Group and Control Group I present similar commuting conditions as they both live along major highways (E411 and A31, respectively) and face an entry in point in Luxembourg at a comparable distance from Luxembourg City. Cross-border workers from France share similar characteristics such as language, professional activities, and culture (Pigeron-Piroth & Wille, 2019). Belgium and France also provide the majority of Luxembourg's cross-border labor force. The similarity of the group characteristics is confirmed in our data. Table 6.2 shows that the control and treatment groups are relatively homogeneous in terms of demographic composition.

Use of Control Group I as our preferred benchmark specification is guided by the similarity in the distance of this group to Luxembourg City and the absence of possible contamination. Control Group II comprises residents of municipalities located further north and, therefore, more distant from Luxembourg City. Also, unlike Control Group I, we cannot completely rule out that some of these residents would use the E411 highway as an entry point to Luxembourg (under normal traffic conditions) and would therefore be affected by the roadworks.

Figure 6.3 shows the share of workers absent at least one day in each month between January 2015 and December 2019, for both the Treatment Group and Control Group I. Three observations stand out. First is the strong cyclicality of absences (with peaks in February and March and lows in July). Second is the generally lower absenteeism in the treatment Group (Belgian cross-border workers) than in the control group (French cross-border workers). Third is that this pattern is reversed in the period of the roadworks between September 2018 and April 2019 – months during which absences are higher in the treatment group.

Figure 6.3: Share of Individuals with at least one absence in a month in Treatment and Control Group I

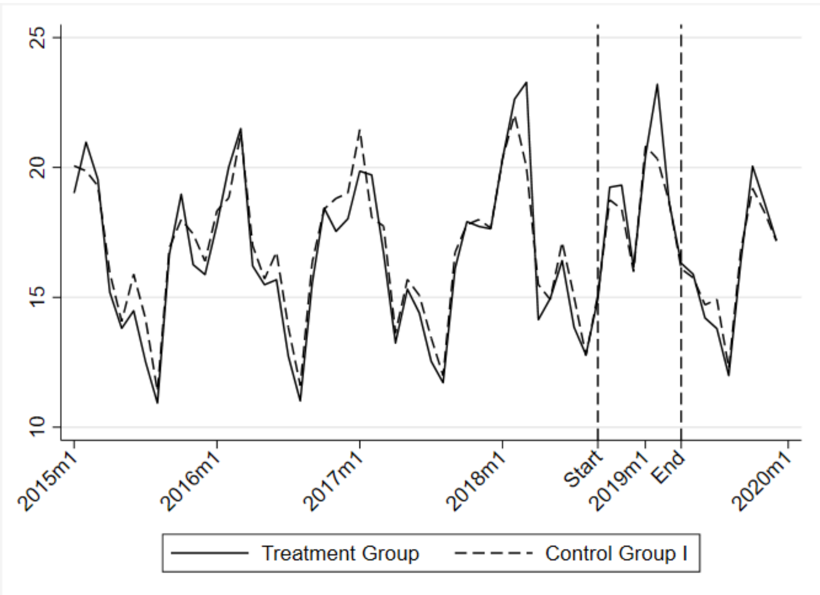


Table 6.2: Descriptive statistics

	ALL	**Treatment Group**	**Control Group I**	**Control Group II**
Contract type				
Permanent contract	0.92	0.95	0.91	0.94
Fixed-term contract	0.04	0.03	0.05	0.04
Temporary contract	0.04	0.01	0.04	0.02
Apprenticeship job	0.00	0.00	0.00	0.00
Blue collar	0.33	0.22	0.34	0.37
Ability of working from home (Bin)	0.54	0.44	0.53	0.64
Monthly total wage ($1000s)	4.07	4.97	3.88	3.95
Hourly total wage	26.33	31.98	25.13	25.84
Number of worked hours	154.71	156.94	154.48	153.74
Enterprise Size				
Less than 5	0.09	0.08	0.08	0.11
6 to 20	0.16	0.17	0.15	0.19
21 to 50	0.14	0.14	0.13	0.17
51 to 200	0.23	0.23	0.22	0.24
More than 200	0.39	0.39	0.42	0.29
Distance to border (Continuous)	21.03	22.22	19.96	23.57
Distance (Bins)				
Less than 15km	0.52	0.50	0.51	0.54
15 to 40km	0.32	0.25	0.42	0.07
More than 40km	0.16	0.26	0.06	0.39
Female	0.38	0.37	0.40	0.34
Age				
Less than 20 years	0.00	0.00	0.00	0.01
20-24 years	0.06	0.05	0.06	0.07
25-29 years	0.14	0.14	0.14	0.14
30-34 years	0.15	0.14	0.16	0.14
35-39 years	0.16	0.16	0.16	0.15
40-44 years	0.16	0.17	0.16	0.15
45-49 years	0.15	0.15	0.15	0.14
50-54 years	0.11	0.12	0.11	0.12
55-59 years	0.06	0.05	0.05	0.06
60 years and more	0.02	0.01	0.01	0.02
Has not Luxembourg citizenship	0.96	0.93	0.98	0.93
Has a child under 19	0.53	0.56	0.52	0.52
N	5,183,231	798,818	3,351,255	1,033,158
Individuals	152,249	21,537	100,129	30,583

Note: Statistics are employee-month averages aggregated over the entire 2015–2019 period.

6.3.3 Empirical model

To examine the effect of the roadworks rigorously, we implement a standard difference-in-differences model with monthly panel data. We primarily focus on absenteeism measured as a binary variable, where 1 indicates that an individual has been absent for at least one day in a given month and 0 otherwise – a measure of the extensive margin of monthly absenteeism. It is noted that individuals who are already frequently absent may be less likely to be affected by the roadworks, as they are likely to commute less.

For tractability, given the size of our dataset, we use a linear regression model as the main specification. The model incorporates fixed effects to control for unobserved heterogeneity at the individual level. The baseline model is thus specified as

$$Y_{it} = \gamma(D_t \times \text{Treatment Zone}_i) + X_{it}\beta + (\text{Year}_t \times \text{Month}_t)\delta + u_i + e_{it} \tag{6.1}$$

where Year_t and Month_t are year and month dummies interacted, u_i is an employee fixed effect, and $D_t = 1$ if t is in the period covered by the road disruption and 0 otherwise. X_{it} incorporates individual (time-varying) characteristics, such as age, parental responsibilities, whether an employee holds a permanent contract, and the employer's size. Treatment Zone_i is equal to 1 for Belgians living around E411 (i.e., the Treatment Group) and 0 for the control groups. Long-term trends and seasonal variations in absenteeism are captured by including monthly and yearly dummy variables (but not their interaction). For heterogeneity analysis, we further include interaction terms between the $(D_t \times \text{Treatment Zone}_i)$ term and some key covariates (gender, age, and the residence's straight distance d_{it} to the border or dummies for different distance categories – see below).

The difference-in-difference specification identifies the causal effect of the disruption in commuting time due to the roadworks under a parallel trends assumption, namely that the difference in absenteeism between the treatment and control groups would have remained constant over time in the absence of treatment. Such condition is plausible in the present context since the time window of the present study is relatively short, spanning over five years with a seven months disruption. It is unlikely that long-term structural changes in location and employment decisions affected the control and treated groups differently. Furthermore, no long-run trend over time in work absenteeism is detected, neither in the control groups over the five years studied nor in the treatment group outside of the treatment period (levels of absenteeism appear to return to their pre-treatment values after the treatment).

6.4 Results

6.4.1 The effect of a commuting time shock on absenteeism

Our baseline results are presented in Column (1) of Table 6.3. The interaction coefficient between the treatment group indicator and the roadworks period dummy shows that the disruption of the E411 highway led to a significant increase of 0.51 percentage points in absenteeism among Belgian commuters relative to their French counterparts. This translates into an approximate increase of 3.1 percent when compared to the benchmark of 16.43 percent on average in the absence of disruptions. Commuting time appears to have a direct, causal impact on work absences. Column (2) reproduces the model by omitting the employee fixed effects, time-varying covariates, and Month × Year dummies, and concludes similarly. The underlying mechanism, however, is uncertain at this point: it could be a result of health hazards associated with increased commuting (Hansson et al., 2011; Künn-Nelen, 2016; Roberts et al., 2011), or it could be due to workers' behavioral responses on the margin of shirking – we return to this in Section 6.4.3 below.

Table 6.3: Baseline estimates of the effect of roadworks on work absences and heterogeneity by commuting distance and gender

Model:	**Baseline**		**Distance**		**Gender**		
Sample:			Full			Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment Group × Roadworks Period	0.51***	0.37**	0.25	-0.07	0.75***	0.73***	0.19
	(0.16)	(0.17)	(0.23)	(0.29)	(0.19)	(0.19)	(0.31)
Treatment Group × Roadworks Period × Between 15 and 40km			0.27				
			(0.40)				
Treatment Group × Roadworks Period × More than 40km			0.99**				
			(0.45)				
Treatment Group × Roadworks Period × Distance to border (continuous)				0.03**			
				(0.01)			
Treatment Group × Roadworks Period × Female					-0.59*		
					(0.36)		
Time-Varying Covariates	Yes	-	Yes	Yes	Yes	Yes	Yes
Month × Year Dummies	Yes	-	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	-	Yes	Yes	Yes	Yes	Yes
F-test			4.57	7.73	8.10		
N	4,121,159	4,135,915	4,121,159	4,121,159	4,121,159	2,508,472	1,612,687
Individuals	121,338	121,666	121,338	121,338	121,338	74,230	47,108

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. The individual, time-varying covariates include: age (bins); the presence of a child under the age of 19 in the household; the individual's employment status in terms of holding a permanent contract; the size of the employer; and the individual's ability to work from home. Significance levels: * = 10%, ** = 5%, and *** = 1%. F-tests are joint tests of equality for the interaction terms between all factors and the base category.

6.4.2 Heterogeneity analysis

The impact of commuting time on absenteeism is further investigated in Columns (3)–(7) of Table 6.3, where we examine the influence of distance and gender on the relationship. The examination of additional factors, including quality of work and professional grade, is presented in Table 6.4.

By commuting distance We first investigate how commuting distance impacts the relationship between commuting time and absenteeism. To do so, we divided the sample into three groups based on commuting distance to the border point: workers who travel less than 15 km, those who travel between 15 and 40 km, and those who travel more than 40km. The regression is conducted on the entire sample, incorporating a double interaction *Treatment Zone × Roadworks Period × Distance (Bins)*, to explore a potential differential impact on absenteeism based on commuting distance.

The commuting time shock mainly affected workers with long commutes. The results in Column (3) show that individuals who commute more than 40 km were 0.99 percentage points more likely to be absent compared to those who commute less than 15 km, with no significant difference between those traveling between 15 and 40 km and those less than 15 km. The analysis is repeated in Column (4) using commuting distance in continuous form, and the double interaction remains significant and positive, indicating a positive relationship between commuting distance and absenteeism when facing a commuting shock.

By gender We then examine the relationship between roadworks disruption and gender and its impact on absenteeism in Column (5) of Table 6.3. Prior research indicates higher absenteeism among women compared to men (Casini et al., 2013; VandenHeuvel & Wooden, 1995; Vistnes, 1997). While we also observe this level difference on average (*coefficients omitted for clarity*), notably among women with children and in childbearing age, we unveil contrasting repercussions of the shock in commuting time. Specifically, our results show that men are more affected by the shock to their commuting time (i.e., the double interaction between being a woman and the disruption is significant at a 10% threshold, with a negative coefficient of –0.59). This divergence becomes even more apparent when analyzing Columns (6) and (7), which provide estimates for gender-specific models. Men significantly increased absenteeism by 0.75 percentage points, while no significant effect of roadworks is observed among women.

Several factors may contribute to this finding, including gender differences in commuting distances, flexibility in work arrangements, and other situational factors. As depicted in Figure 6.5, women often have shorter commutes than men, which could lessen the impact of disruptions on their daily travel by mitigating the exposure to disruptions. A higher prevalence of flexible work arrangements among women could also potentially help respond to disruptions in commuting time (Lachance-Grzela & Bouchard, 2010; Plantenga, 2010). The higher baseline prevalence of work absences among women might also limit the scope for responding to the commuting time shock.

Other factors We show in Table 6.4 the influence of several other factors measured in our data in response to commuting time disruptions. Column (1) considers the interaction with three age groups: under 34, 35 to 54, and 55 and over. The response to the shock is mostly observed among younger and older workers – no statistically significant increase in absenteeism is observed among the 35 to 54 category.

De Cuyper and De Witte (2006) underline the role of the relational psychological contract in explaining asymmetries in organization commitment between permanent employees and others. Quite surprisingly, having a permanent contract reduces the effect of disruption on absenteeism (Column (3)). However, the non-permanent contract category encompasses a range of employment arrangements, including apprenticeships, temporary contracts, and fixed-term contracts, which can vary widely regarding job security and employment prospects. In the specific context of Luxembourg, where a majority of workers (95%) are employed under permanent contracts in the Treatment Zone, this finding highlights the potential importance of job security in mitigating the effects of disruptions on absenteeism.

Conversely, the double interaction with hourly wage is positive (Column (5)). This is in line with the findings of Ma and Ye (2019), Columns (7), (8), and (9) do not provide conclusive evidence on the effect of occupation type (blue vs. white collar jobs), firm size, or industry on absenteeism in response to commuting shocks. The results on blue-collar may come as a surprise since blue-collar workers are expected to have less flexible work arrangements and may face greater occupational hazards associated with increased fatigue due to longer commutes (e.g., Joyce et al., 2010).

To sum up, we find a small but statistically significant increase in the share of workers that report absence from work when exposed to the commuting time shock. The effect appears driven by male, young or old workers, workers with long commutes (greater than 40kms), and workers with higher wages.

6.4.3 Health impacts or shirking?

As mentioned above, the increased absences may be due to genuine health hazards but also to increased shirking temptation by calling in sick. The possibility of taking two days of absence without examination by a health care professional may facilitate some workers declaring themselves absent for non-legitimate reasons.

Table 6.5 analyzes three distinct types of absences. Column (1), using the benchmark model, is compared with the three other columns with variations in the dependent variable. While the primary dependent variable is a binary variable equal to 1 if the worker is absent for any reason at least once during the month, the subsequent columns consider different specific reasons for absenteeism. Columns (2), (3), and (4) consider absences due to illness, injury, and family reasons, respectively. Absences for illness (0.36 percentage points) and family reasons (0.15 percentage points) largely explain the estimated effect of roadworks on absences. Roadworks disruptions do not significantly impact absences due to injury. One plausible interpretation of this observation is that workers may partly falsely report illness or family absences to avoid the discomfort of a longer commute, as these specific absence motives are difficult to monitor if limited to two days. Absences for injury reasons – which are more challenging to claim unduly – do not respond to the commuting time shock, despite the potential occupational risks associated with commuting time fatigue.

Results presented in Table 6.6 further support the idea that the effect of roadworks on absenteeism is concentrated in the first two days of authorized absence. In the models presented in Table 6.6, we consider as dependent variable an indicator variable equal to 1 if the person is absent at least once for one to two days or three days or more over a month, respectively. Roadworks increased the probability of having at least one period of absence of one to two days over the month, while the effect of roadworks is not statistically different from zero for periods of three days or more. (We return to variations in the definition of the dependent variable in Section 6.4.4.)

These results suggest that workers responded to the increase in commuting time by increasing the likelihood of reporting short absences that are not closely monitored.

6.4.4 Sensitivity

Variations in treatment definition and analysis period Our main result of a relatively small but significant increase in absenteeism during the disruption to commuting time is robust to variations in the definition of the treatment and analysis period.

Table 6.4: Heterogeneous effects of roadworks on work absences by age and employment characteristics

	Additional Specifications								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment Group × Roadworks Period	0.89***	0.51**	1.59**	0.51***	0.27	1.29*	0.64	0.52***	2.19
	(0.31)	(0.23)	(0.69)	(0.20)	(0.18)	(0.74)	(0.51)	(0.19)	(2.92)
Treatment Group × Roadworks Period × 35 to 54 years	-0.62*								
	(0.37)								
Treatment Group × Roadworks Period × 55 years and more	0.38								
	(0.71)								
Treatment Group × Roadworks Period × Has a child		0.01							
		(0.32)							
Treatment Group × Roadworks Period × Permanent Contract			-1.17*						
			(0.71)						
Treatment Group × Roadworks Period × Monthly total wage ($1000s)				0.01					
				(0.02)					
Treatment Group × Roadworks Period × Hourly total wage					0.01***				
					(0.00)				
Treatment Group × Roadworks Period × Number of worked hours						-0.00			
						(0.00)			
Treatment Group × Roadworks Period × Enterprise Size: 6 to 20							0.33		
							(0.65)		
Treatment Group × Roadworks Period × Enterprise Size: 21 to 50							0.45		
							(0.68)		
Treatment Group × Roadworks Period × Enterprise Size: 51 to 200							-0.26		
							(0.62)		
Treatment Group × Roadworks Period × Enterprise Size: More than 200							-0.51		
							(0.57)		
Treatment Group × Roadworks Period × Blue collar								0.01	
								(0.40)	
Treatment Group × Roadworks Period × Secondary sector									-1.70
									(2.94)
Treatment Group × Roadworks Period × Tertiary sector									-1.69
									(2.92)
Time-Varying Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	4.72	5.00	5.70	6.57	12.24	6.48	2.81	5.06	3.20
N	4,121,159	4,121,159	4,121,159	4,121,153	4,118,824	4,121,153	4,121,159	4,121,159	4,112,607
Individuals	121,338	121,338	121,338	121,338	121,313	121,338	121,338	121,338	121,153

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. The individual, time-varying covariates include: age (bins); the presence of a child under the age of 19 in the household; the individual's employment status in terms of holding a permanent contract; the size of the employer; and the individual's ability to work from home. Significance levels: $^{*}=10\%$, $^{**}=5\%$, and $^{***}=1\%$. F-tests are joint tests of equality for the interaction terms between all factors and the base category.

Table 6.5: Regressions by absence type

Dependent:	Absence			
	All	Illness	Injury	Family
	(1)	(2)	(3)	(4)
Treatment Group × Roadworks Period	0.51***	0.36**	-0.02	0.15***
	(0.16)	(0.15)	(0.03)	(0.05)
Month × Year Dummies	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes
N	4,121,159	4,121,159	4,121,159	4,121,159
Individuals	121,338	121,338	121,338	121,338

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. The individual, time-varying covariates include: age (bins); the presence of a child under the age of 19 in the household; the individual's employment status in terms of holding a permanent contract; the size of the employer; and the individual's ability to work from home. Significance levels: * = 10%, ** = 5%, and *** = 1%.

Table 6.6: Regressions by absence duration

Specification:	Binary	
	1 or 2 days	3 or more days
	(1)	(2)
Treatment Group × Roadworks Period	0.0051***	0.0016
	(0.0010)	(0.0010)
Time-Varying Covariates	Yes	Yes
Month × Year Dummies	Yes	Yes
Employee Fixed Effects	Yes	Yes
N	4,121,159	4,121,159
Individuals	121,338	121,338

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. The individual, time-varying covariates include: age (bins); the presence of a child under the age of 19 in the household; the individual's employment status in terms of holding a permanent contract; the size of the employer; and the individual's ability to work from home. Significance levels: * = 10%, ** = 5%, and *** = 1%.

Since the roadworks started in mid-September 2018 and finished early in May 2019, our month-by-month analysis does not perfectly match the dates of the start and end of the roadworks. In Column (1) of Table 6.7, we first show estimates in a model where the binary roadworks monthly indicator is replaced with a continuous variable – the number of roadworks days within the month. This change hardly modifies the effect that we observe.

In Column (2) of Table 6.7, the regression model is estimated only with observations spanning January 2015 through April 2019 – the last month fully impacted by the roadworks. Such a specification addresses the possibility of persistence – such as accrued fatigue or modified commuting routines due to the additional lane – lingering beyond the time frame of the roadworks. Column (3) shows estimates where, on the other hand, data from the year 2015 are omitted – this variation discards pre-treatment periods most distant from the treatment period. Column (4) is based on data with the months of February and March excluded. As shown in Figure 6.3, absenteeism has a consistent annual pattern, with a prominent peak occurring during February and March across all observed years. We expect the spikes to be due, at least in part, to difficult driving conditions due to weather and increased traffic due to the holidays during these months.[45] These might introduce a competing disruption to commuting time and therefore call for assessing the robustness of our results to their exclusion from the data. Reassuringly, the estimates of the overall impact of roadworks on absenteeism remain statistically significant and of similar magnitude.

Allowing for the February/March exceptions As an alternative to dropping February and March from the data to handle the February and March exceptions, we have also adapted the model specification by including interaction terms between the treatment group indicator and month dummies. Column (5) reports estimates in a model where we allow, for all months, different calendar month effects in the treatment and control groups. For columns (6) and (7), we only allow such interactions for the months of February and March (jointly or separately). What such an extended specification implies is that the effect of the roadworks is estimated only by *how much bigger than usual* is the Treatment-Control difference in absenteeism gap in each month during the roadworks period – this, therefore, allows for a possible systematic difference across the groups in absenteeism in some months.

Such a specification is motivated by Figure 6.4, which shows the regression coefficients of the Treatment Group indicator interacted with each year-month of the analyzed period – without including the actual treatment period indicator. February and March stand out as two months in which absenteeism often appeared higher in the Treatment Group than in the Control Group, even outside the treatment period. (This is also visible in raw indicators of absenteeism shown in Figure 6.3.) Reasons for this February/March exceptions are unclear, but as mentioned above, they can plausibly be attributed to differences in driving conditions in bad weather. The deviation observed in February 2018 coincides with a cold wave. Belgium registered its lowest temperature on record on February 28, 2018, with the mercury dipping to -18 degrees Celsius in certain localities. This cold wave, commencing on February 18 and finishing on March 4 (Mievies, 2018) possibly led to differential impacts

[45] Please refer to: https://weatherspark.com/y/53907/Average-Weather-in-Luxembourg-Year-Round#Figures-ObservedWeather for further details on weather conditions.

Table 6.7: Sensitivity to variations in treatment period, outcome variable and control group

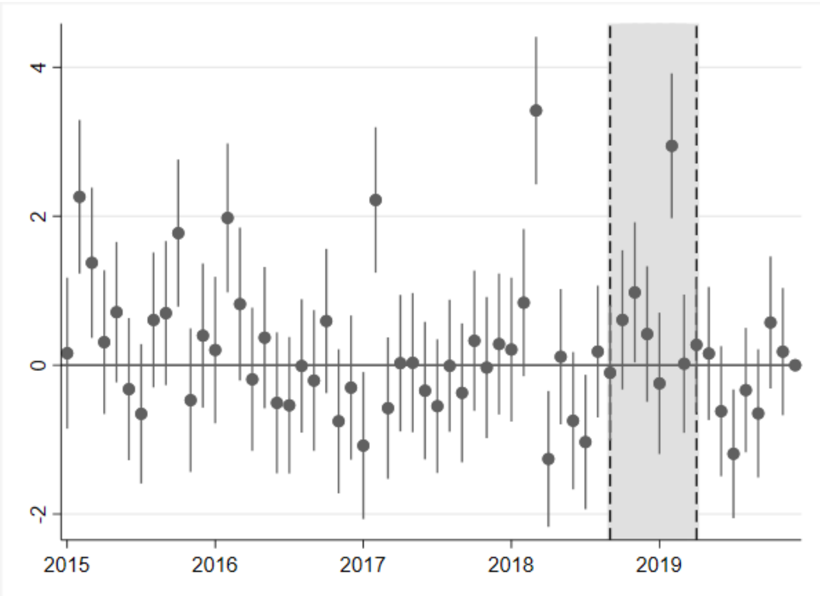
Model:	OLS								Poisson	OLS	Poisson	OLS	Poisson	OLS
Individual characteristics:	**Baseline**										**Less controls**		**More controls**	
Control Group	Control Group I							Control Group II			Control Group I			
Period:	Full	Jan 2015 – Apr 2019	Jan 2016 – Dec 2019	Excluding Feb – Mar					Full					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Roadworks Days	-0.33**													
	(0.15)													
Treatment Group × Roadworks Days	0.36**													
	(0.15)													
Roadworks Period		-0.33	0.31	-1.15***	-1.00***	-1.02***	-1.02***	-0.26	0.33***	0.35***	0.36***	0.40***	0.10***	0.09***
		(0.22)	(0.21)	(0.22)	(0.22)	(0.22)	(0.22)	(0.42)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Treatment Group × Roadworks Period		0.31*	0.56***	0.44**	0.24	0.34**	0.34**	0.39	0.02	0.01	0.01	-0.00	0.02	0.01
		(0.18)	(0.16)	(0.17)	(0.17)	(0.17)	(0.17)	(0.25)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Age														
20-24 years	4.08***	3.80***	4.07***	4.16***	4.08***	4.08***	4.08***	3.30***	0.55***	0.30***	0.65***	0.43***	0.68***	0.44***
	(0.66)	(0.76)	(0.73)	(0.69)	(0.66)	(0.66)	(0.66)	(1.05)	(0.09)	(0.07)	(0.09)	(0.07)	(0.09)	(0.07)
25-29 years	6.72***	6.31***	6.79***	6.87***	6.72***	6.72***	6.72***	6.30***	0.80***	0.48***	0.97***	0.72***	1.09***	0.79***
	(0.70)	(0.80)	(0.78)	(0.73)	(0.70)	(0.70)	(0.70)	(1.13)	(0.10)	(0.08)	(0.09)	(0.08)	(0.10)	(0.08)
30-34 years	7.57***	7.10***	7.61***	7.74***	7.56***	7.56***	7.56***	6.73***	0.80***	0.54***	0.99***	0.79***	1.26***	1.03***
	(0.74)	(0.84)	(0.81)	(0.77)	(0.74)	(0.74)	(0.74)	(1.20)	(0.10)	(0.09)	(0.10)	(0.09)	(0.10)	(0.09)
35-39 years	6.44***	6.14***	6.65***	6.75***	6.43***	6.43***	6.43***	5.89***	0.61***	0.32***	0.79***	0.57***	1.25***	1.01***
	(0.77)	(0.87)	(0.85)	(0.80)	(0.77)	(0.77)	(0.77)	(1.26)	(0.10)	(0.10)	(0.10)	(0.09)	(0.10)	(0.09)
40-44 years	5.02***	4.72***	5.35***	5.40***	5.01***	5.01***	5.01***	5.07***	0.46***	0.15	0.64***	0.38***	1.28***	1.03***
	(0.80)	(0.90)	(0.88)	(0.83)	(0.80)	(0.80)	(0.80)	(1.32)	(0.11)	(0.10)	(0.11)	(0.10)	(0.10)	(0.09)
45-49 years	3.92***	3.74***	4.18***	4.31***	3.92***	3.92***	3.92***	4.27***	0.44***	0.09	0.60***	0.31***	1.45***	1.17***
	(0.83)	(0.93)	(0.92)	(0.86)	(0.83)	(0.83)	(0.83)	(1.38)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.10)
50-54 years	3.78***	3.57***	3.77***	4.19***	3.78***	3.78***	3.78***	4.44***	0.48***	0.14	0.63***	0.33***	1.68***	1.41***
	(0.87)	(0.97)	(0.96)	(0.90)	(0.87)	(0.87)	(0.87)	(1.44)	(0.12)	(0.11)	(0.12)	(0.11)	(0.11)	(0.10)
55-59 years	5.47***	5.19***	5.28***	5.80***	5.46***	5.47***	5.47***	5.75***	0.68***	0.43***	0.82***	0.61***	2.07***	1.91***
	(0.92)	(1.02)	(1.01)	(0.96)	(0.92)	(0.92)	(0.92)	(1.54)	(0.13)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)
60 years and more	8.42***	8.21***	8.13***	8.41***	8.41***	8.42***	8.42***	7.59***	0.96***	0.87***	1.09***	1.01***	2.51***	2.52***
	(1.05)	(1.17)	(1.16)	(1.09)	(1.05)	(1.05)	(1.05)	(1.74)	(0.14)	(0.15)	(0.14)	(0.15)	(0.12)	(0.13)
Has a child under 19	1.59***	1.52***	1.35***	1.48***	1.59***	1.59***	1.59***	2.46***	0.11***	0.32***	0.11***	0.32***	0.16***	0.33***
	(0.17)	(0.19)	(0.20)	(0.18)	(0.17)	(0.17)	(0.17)	(0.29)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Permanent contract	8.16***	7.78***	7.97***	8.08***	8.16***	8.16***	8.16***	7.35***	1.06***	0.92***			0.92***	0.87***
	(0.17)	(0.19)	(0.19)	(0.18)	(0.17)	(0.17)	(0.17)	(0.38)	(0.02)	(0.02)			(0.03)	(0.03)
Ability of working from home (Bin)	1.27***	1.35***	1.43***	1.16***	1.27***	1.27***	1.27***	1.20***	0.23***	0.19***	0.55***	0.55***	0.47	0.04
	(0.21)	(0.23)	(0.23)	(0.21)	(0.21)	(0.21)	(0.21)	(0.42)	(0.03)	(0.03)	(0.03)	(0.03)	(0.54)	(0.48)
Enterprise Size														
6 to 20	2.41***	2.31***	2.42***	2.43***	2.41***	2.41***	2.41***	2.30***	0.22***	0.18***			0.19***	0.17***
	(0.19)	(0.21)	(0.21)	(0.20)	(0.19)	(0.19)	(0.19)	(0.33)	(0.03)	(0.03)			(0.03)	(0.03)
21 to 50	4.11***	4.08***	4.05***	4.05***	4.11***	4.11***	4.11***	4.24***	0.32***	0.28***			0.29***	0.26***
	(0.22)	(0.25)	(0.25)	(0.23)	(0.22)	(0.22)	(0.22)	(0.41)	(0.03)	(0.03)			(0.03)	(0.03)
51 to 200	5.11***	4.96***	5.03***	5.13***	5.11***	5.11***	5.11***	5.12***	0.38***	0.33***			0.33***	0.31***
	(0.23)	(0.26)	(0.26)	(0.24)	(0.23)	(0.23)	(0.23)	(0.43)	(0.03)	(0.03)			(0.04)	(0.03)
More than 200	6.34***	5.96***	6.13***	6.27***	6.34***	6.34***	6.34***	7.40***	0.44***	0.38***			0.38***	0.37***
	(0.26)	(0.28)	(0.29)	(0.27)	(0.26)	(0.26)	(0.26)	(0.49)	(0.04)	(0.04)			(0.04)	(0.04)
Monthly total wage ($1000s)													0.01***	0.01***
													(0.00)	(0.00)
Has not Luxembourgish citizenship													-0.02	0.00
													(0.06)	(0.06)
Distance (Bins)														
Between 15 and 40km													-0.02	-0.01
													(0.03)	(0.04)
More than 40km													0.02	0.04
													(0.06)	(0.07)
Month × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × Treatment Group	-	-	-	-	Yes	-	-	-	-	-	-	-	-	-
February/March × Treatment Group	-	-	-	-	-	Yes	-	-	-	-	-	-	-	-
February × Treatment Group	-	-	-	-	-	-	Yes	-	-	-	-	-	-	-
March × Treatment Group	-	-	-	-	-	-	Yes	-	-	-	-	-	-	-
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	4121159	3513176	3375409	3445372	4121159	4121159	4121159	1182428	3559608	4121159	3570792	4135915	3537707	4093658
Individuals	121338	113498	115469	120614	121338	121338	121338	34503	86601	121338	86837	121666	86084	120433

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. Significance levels: * = 10%, ** = 5%, and *** = 1%.

on the treatment and control groups because of topological differences – the Treatment Group covers an area that generally has a higher altitudinal position relative to Control Group I.

The effect size of the treatment is reduced in these more flexible specifications, but it remains positive and significant if only February and March deviations are adjusted for. In the full specification with allowance for possible Treatment-Control difference in all months – as in Figure 6.4 with the addition of a treatment period interaction term – the coefficient remains positive but loses statistical significance. One could, however, argue that such a model may overfit the data – leaving little scope for identifying the effect of the roadworks.

Figure 6.4: Monthly difference in absenteeism between Treatment Group and Control Group I – Regression-adjusted estimates without treatment period indicator



Note: The plotted point estimates are derived from interaction terms between the treatment group and year-month indicators. The model is specified as follows: $Y_{it} = \gamma(\text{Year}_t \times \text{Month}_t \times \text{Treatment Zone}i) + Xit\beta + (\text{Year}_t \times \text{Month}_t)\delta + u_i + e_{it}$ Standard errors are clustered at the individual level. The timeline is represented on the x-axis, while the y-axis illustrates the magnitude of the coefficients.

Variation in control group The main result is also robust to changing the control group. Column (8) of Table 6.7 shows model estimates when Control Group II – Belgian cross-border workers residing close to the north of Luxembourg – instead of Control Group I – French cross-border workers. However, while the point estimate of the impact of the roadworks remains similar (0.39 against 0.51), its standard error is larger, and the coefficient loses statistical significance. However, for reasons mentioned above regarding proximity to Luxembourg City and potential contamination, we trust Control Group I provides a more appropriate control group.

Variation in the outcome variable Finally, as mentioned above, the choice of outcome variable reveals important. Columns (9)–(14) of Table 6.7 shows estimates of the impact of roadworks on a continuous dependent variable – the number of days of absence in the month – instead of the binary variable denoting at least one day of absence. The model, estimated through both OLS and a Poisson approach, accommodates various specifications with different numbers of control variables.[46]

In all such specifications, our coefficient of interest ceases to be significantly different from zero. As mentioned above, commuting time disruptions predominantly influence absenteeism at the extensive margin: they amplify the probability of an incidence of absence, yet without a statistically noticeable impact on the average number of days absent in the month. This reinforces the observations from Table 6.6 showing that the effect of roadworks on absenteeism appears concentrated within the initial two days of authorized absence.

6.4.5 Placebo analyses

To ascertain the validity of the "roadworks effect", we conducted two types of placebo tests to ensure that no unanticipated effects appear in situations or periods where they logically should not occur. Firstly, the Treatment Group was replaced with Control Group II, which, per our expectations, was either unaffected or only slightly indirectly impacted by the roadworks. Secondly, we estimated the "roadworks effect" during months when no roadworks were causing disruptions.

The estimates derived from these placebo regressions analyses are detailed in Table 6.8. Reassuringly, substituting the Treatment Group with Control Group II does not yield a significant variation in absenteeism during roadworks (Column (1)).

In our next step, adopting the methodology suggested by Roth et al. (2023), we carried out several regression estimations for the second placebo test (Columns (2)-(10)). These models integrated placebo treatment period indicators into the model specification – one for September 2017 to April 2018 and one for September 2016 to April 2017. These periods were tested both separately and concurrently. This technique allowed us to assess the outcomes' concurrent movement pre-disturbance.

Due to reasons previously discussed in Section 6.4.4, we needed to adjust for the increased absenteeism observed in February/March across most years. To this end, we incorporated a single dummy variable for the interaction of either February or March and the Treatment Group indicators (Columns (2)-(4)). Furthermore, we introduced separate dummy variables for both months, each interacting with the Treatment Group indicators (Columns (5)-(7)). In the final set of models (Columns (8)-(10)), we entirely excluded observations from February and March.

Accordingly, the Placebo Period (Sep 2017-Apr 2018) does not yield a significant result, hence successfully passing the placebo test. The Placebo Period (Sep 2016-Apr 2017) indicates a significant negative result in some columns, suggesting a lower bound for our coefficient of interest. However, this significance disappears upon excluding February and March observations (Columns (8)-(10)), thus satisfactorily passing the placebo test in this case.

Throughout all models, our primary coefficient of interest consistently exhibits positive significance. Even though the initial analysis raised some questions regarding the significance of one placebo period, subsequent

[46]See Appendix Poisson Model for details on the Poisson model specification.

models reinforced the confidence in the overall experimental design. Collectively, these placebo analyses enhance the robustness of our principal findings and underline the "roadworks effect" as a credible driver of absenteeism.

6.5 Conclusion

Assessing the causal effect of commuting time on absenteeism requires considering exogenous, short-lived variations in commuting. This is because residential and employment choices are frequently made in tandem by forward-looking, rational workers. A mere cross-sectional exploration of the distance-to-work and absenteeism relationship offers a narrow perspective. Surprisingly, despite the substantial implications of absenteeism – both a health risk and a productivity concern – particularly in urban areas plagued by traffic and congestion, few studies provide plausibly causal estimates on this relationship.

By exploiting roadworks that induced significant yet short-lived disruptions in commuting time for a large population of workers and using accurate population-wide data on work absences, our findings reveal that the disruption on the workers' route increased absenteeism by approximately 0.51 percentage points (or 3.1 percent) – a statistically significant effect, albeit modest in magnitude. While extended commuting times might bear health implications, our data suggests workers might adapt to such disturbances through work avoidance. Interestingly, no medical certificate is required until the third consecutive day of absence. The study highlights that workers commuting more than 40 km are the most affected. Additionally, there are notable gender and age disparities: men and individuals at both ends of the working age spectrum appear more susceptible to commuting shocks.

The findings are generally robust to various robustness checks and placebo analyses. However, the choice of outcome variable reveals pivotal: we find an effect at the intensive margin on the probability of being absent from work, but this does not translate into significant increases at the extensive margin (that is, on the average number of days of absence).

Altogether, our results highlight the importance of considering the toll commuting takes on worker productivity and health, especially in the face of urban congestion and traffic. These results should feed the contemporary debate surrounding the benefits of teleworking possibilities to help workers respond to unforeseen, temporary shocks to commuting time, a topic thrust into the limelight by the COVID-19 pandemic.

Table 6.8: Placebo tests

Treatment:	Control Group II	Treatment Group								
Control:		Control Group I								
Period:		Full						Excluding Feb - Mar		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment × Roadworks Period	0.11	0.37**	0.28*	0.31*	0.37**	0.28*	0.31*	0.44**	0.42**	0.41**
	(0.22)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.18)	(0.17)	(0.18)
Treatment × Placebo Period (Sep 2017–Apr 2018)		0.18		0.11	0.18		0.11	-0.00		-0.04
		(0.17)		(0.17)	(0.17)		(0.17)	(0.18)		(0.18)
Treatment × Placebo Period (Sep 2016–Apr 2017)			-0.37**	-0.35**		-0.37**	-0.35**		-0.22	-0.23
			(0.16)	(0.17)		(0.16)	(0.17)		(0.18)	(0.18)
Time-Varying Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
February/March × Treatment Group	-	Yes	Yes	Yes	-	-	-	-	-	-
February × Treatment Group	-	-	-	-	Yes	Yes	Yes	-	-	-
March × Treatment Group	-	-	-	-	Yes	Yes	Yes	-	-	-
Month × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3 722 813	4 121 159	4 121 159	4 121 159	4 121 159	4 121 159	4 121 159	3 445 372	3 445 372	3 445 372
Individuals	112 195	121 338	121 338	121 338	121 338	121 338	121 338	120 614	120 614	120 614

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. Significance levels: * = 10%, ** = 5%, and *** = 1%.

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Appendix

Poisson Model

Roadworks imply a relatively small shock, and it is expected that individuals will continue to work for long-term financial sustainability. Also absences longer than two days need to be supported by medical examination. Consequently, the marginal effect of taking a day of absence may differ between those who are not typically absent and those who are absent ten days a month.

To explore this, we use the number of days absent in a month as the dependent variable in some robustness checks, employing both a linear regression model (OLS with Fixed Effects) and a Poisson panel model with conditional Fixed Effects. The latter is computationally intensive but well-suited to the dependent variable form.

The outcome, a non-negative integer, represents the number of days of absence (the event) per month worked (the time unit) for each cross-border worker. Such data, far from being normally distributed, could suffer from two potential issues: an abundance of zeros and non-random selection. The Poisson distribution is commonly used for this type of data, known as count data (Hausman et al., 1984).

The Poisson model makes several assumptions, including event independence, a constant lambda arrival rate, and no limit on the number of occurrences. Heterogeneity of observations over time could violate the constant arrival rate assumption, as individuals at the beginning of their contract might be less likely to be absent than those with more seniority. The arrival of events must be independent so that the occurrence of one event does not influence the probability of another. Furthermore, the limited number of days of absence per month, equal to the number of possible work days, could violate the assumption of no limit on occurrences. Finally, the days of absence may vary from month to month depending on the number of days worked.

These factors could create a phenomenon of apparent overdispersion, where the conditional variance of the outcome variable is greater than its conditional expected value. In this case, the standard errors and estimated p-values may be too small. To address these concerns, we use the conditional Fixed Effects Poisson model estimated by maximum likelihood estimation techniques, as proposed by Wooldridge (1999), with robust clustered standard errors. Conditional Fixed Effects eliminate unobserved heterogeneity over time. Moreover, Wooldridge (1999) showed that the Poisson Fixed Effects estimator is robust to all failures of the Poisson model assumptions, except for the failure of the conditional correct mean assumption.[47].

Assuming the response variable Y follows a Poisson distribution, the model can be expressed as

$$\log\left(\mathbf{E}(Y|\mathbf{x})\right) = \alpha + \beta'\mathbf{x} + \log\left(exposure\right) \tag{6.2}$$

where $\alpha \in \mathbb{R}$, $\beta \in \mathbb{R}^n$, and $\mathbf{x}$ is an n-dimensional vector consisting of n independent variables. The model can be rewritten in a more compact form

[47] Although many authors suggest using the Fixed Effects negative binomial approach, we have chosen, following the advice of Wooldridge (1999), not to use it. Indeed, Wooldridge (1999) has shown that the negative binomial approach suffers from several flaws, unlike the Fixed Effects Poisson estimator, which is robust to many hypothesis failures. For a complete discussion of this topic, please refer to his paper.

$$\log\left(\mathbf{E}(Y|\mathbf{x})\right) = \theta'\mathbf{x} + \log\left(exposure\right) \tag{6.3}$$

which implies

$$\log\left(\mathbf{E}(Y|\mathbf{x})\right) - \log\left(exposure\right) = \log\left(\frac{\mathbf{E}(Y|\mathbf{x})}{exposure}\right) = \theta'\mathbf{x} \tag{6.4}$$

If Y_{it} are independent observations with corresponding x_{it} values of the predictor variables, then the coefficients can be estimated by maximum likelihood. To further refine the model, we introduce an exposure variable that accounts for variations in the length of the months studied in terms of working days. In this case, the exposure variable is the number of month working days, which standardizes the dependent variable on the same time scale. As a result, the dependent variable is expressed as a count of absences divided by the number of working days each month ($exposure$).

Groups Composition

Treatment Group

Country	Arrondissement	Commune
Belgium	Arrondissement d'Arlon	Arlon
Belgium	Arrondissement de Neufchâteau	Bertrix
Belgium	Arrondissement de Neufchâteau	Herbeumont
Belgium	Arrondissement de Virton	Chiny
Belgium	Arrondissement de Virton	Etalle
Belgium	Arrondissement de Virton	Habay
Belgium	Arrondissement de Neufchâteau	Libramont-Chevigny
Belgium	Arrondissement de Neufchâteau	Léglise
Belgium	Arrondissement de Neufchâteau	Neufchâteau
Belgium	Arrondissement de Neufchâteau	Saint-Hubert
Belgium	Arrondissement de Virton	Tintigny
Belgium	Arrondissement de Neufchâteau	Daverdisse
Belgium	Arrondissement de Neufchâteau	Libin
Belgium	Arrondissement de Neufchâteau	Wellin
Belgium	Arrondissement de Neufchâteau	Tellin

Control Group I

Country	Arrondissement	Commune
France	Thionville	Rédange
France	Thionville	Algrange
France	Thionville	Knutange
France	Thionville	Aumetz
France	Thionville	Rochonvillers
France	Thionville	Nilvange
France	Thionville	Tressange
France	Thionville	Angevillers
France	Thionville	Lommerange
France	Thionville	Fontoy
France	Thionville	Boulange
France	Thionville	Russange
France	Thionville	Havange
France	Thionville	Ottange
France	Thionville	Neufchef
France	Thionville	Audun-le-Tiche
France	Thionville	Basse-Rentgen
France	Thionville	Zoufftgen
France	Thionville	Rodemack
France	Thionville	Évrange
France	Thionville	Thionville
France	Thionville	Gavisse
France	Thionville	Boust
France	Thionville	Kanfen
France	Thionville	Cattenom
France	Thionville	Breistroff-la-Grande
France	Thionville	Manom
France	Thionville	Mondorff
France	Thionville	Entrange
France	Thionville	Roussy-le-Village
France	Thionville	Koenigsmacker
France	Thionville	Illange
France	Thionville	Hettange-Grande
France	Thionville	Hagen
France	Thionville	Berg-sur-Moselle

France	Thionville	Puttelange-lès-Thionville
France	Thionville	Yutz
France	Thionville	Volmerange-les-Mines
France	Thionville	Fixem
France	Thionville	Escherange
France	Thionville	Beyren-lès-Sierck
France	Thionville	Terville
France	Thionville	Richemont
France	Thionville	Florange
France	Thionville	Fameck
France	Thionville	Mondelange
France	Thionville	Uckange
France	Thionville	Vitry-sur-Orne
France	Thionville	Rosselange
France	Thionville	Hayange
France	Thionville	Moyeuvre-Petite
France	Thionville	Serémange-Erzange
France	Thionville	Moyeuvre-Grande
France	Thionville	Clouange
France	Thionville	Gandrange
France	Thionville	Ranguevaux
France	Thionville	Kemplich
France	Thionville	Bettelainville
France	Thionville	Bertrange
France	Thionville	Stuckange
France	Thionville	Basse-Ham
France	Thionville	Oudrenne
France	Thionville	Valmestroff
France	Thionville	Volstroff
France	Thionville	Budling
France	Thionville	Bousse
France	Thionville	Kédange-sur-Canner
France	Thionville	Guénange
France	Thionville	Distroff
France	Thionville	Hombourg-Budange
France	Thionville	Veckring
France	Thionville	Metzeresche
France	Thionville	Rurange-lès-Thionville

France	Thionville	Luttange
France	Thionville	Elzange
France	Thionville	Buding
France	Thionville	Klang
France	Thionville	Metzervisse
France	Thionville	Kuntzig
France	Thionville	Monneren
France	Thionville	Inglange
France	Thionville	Aboncourt
France	Metz	Metz
France	Metz	Sainte-Ruffine
France	Metz	Coin-sur-Seille
France	Metz	Augny
France	Metz	Vernéville
France	Metz	Lorry-lès-Metz
France	Metz	Lorry-Mardigny
France	Metz	Jussy
France	Metz	Féy
France	Metz	Arry
France	Metz	Châtel-Saint-Germain
France	Metz	Moulins-lès-Metz
France	Metz	Ars-sur-Moselle
France	Metz	Pournoy-la-Chétive
France	Metz	Rozérieulles
France	Metz	Coin-lès-Cuvry
France	Metz	Vionville
France	Metz	Gorze
France	Metz	Gravelotte
France	Metz	Corny-sur-Moselle
France	Metz	Marieulles
France	Metz	Jouy-aux-Arches
France	Metz	Novéant-sur-Moselle
France	Metz	Ancy-sur-Moselle
France	Metz	Lessy
France	Metz	Pouilly
France	Metz	Vaux
France	Metz	Rezonville
France	Metz	Cuvry

France	Metz	Antilly
France	Metz	Hayes
France	Metz	Marsilly
France	Metz	Chesny
France	Metz	Mey
France	Metz	Mécleuves
France	Metz	Servigny-lès-Raville
France	Metz	Chieulles
France	Metz	Sanry-sur-Nied
France	Metz	Raville
France	Metz	Saint-Hubert
France	Metz	Sainte-Barbe
France	Metz	Courcelles-sur-Nied
France	Metz	Saint-Julien-lès-Metz
France	Metz	Vantoux
France	Metz	Courcelles-Chaussy
France	Metz	Montoy-Flanville
France	Metz	Glatigny
France	Metz	Sorbey
France	Metz	Maizery
France	Metz	Ars-Laquenexy
France	Metz	Charleville-sous-Bois
France	Metz	Argancy
France	Metz	Pange
France	Metz	Ogy
France	Metz	Ay-sur-Moselle
France	Metz	Flévy
France	Metz	Jury
France	Metz	Les Étangs
France	Metz	Charly-Oradour
France	Metz	Trémery
France	Verdun	Sorbey
France	Metz	Vry
France	Metz	Silly-sur-Nied
France	Metz	Laquenexy
France	Metz	Servigny-lès-Sainte-Barbe
France	Metz	Sanry-lès-Vigy
France	Metz	Noisseville

France	Metz	Failly
France	Metz	Colligny
France	Metz	Ennery
France	Metz	Nouilly
France	Metz	Coincy
France	Metz	Burtoncourt
France	Metz	Malroy
France	Metz	Vigy
France	Metz	Bazoncourt
France	Metz	Vany
France	Metz	Chailly-lès-Ennery
France	Metz	Maizeroy
France	Metz	Peltre
France	Metz	Retonfey
France	Metz	Saulny
France	Metz	Roncourt
France	Metz	Bronvaux
France	Metz	Amanvillers
France	Metz	Plesnois
France	Metz	Sainte-Marie-aux-Chênes
France	Metz	Norroy-le-Veneur
France	Metz	Marange-Silvange
France	Metz	Fèves
France	Metz	Saint-Privat-la-Montagne
France	Metz	Montois-la-Montagne
France	Metz	Amnéville
France	Metz	Rombas
France	Metz	Pierrevillers
France	Metz	Woippy
France	Metz	Semécourt
France	Metz	Hagondange
France	Metz	Talange
France	Metz	Maizières-lès-Metz
France	Metz	Hauconcourt
France	Metz	La Maxe
France	Metz	Plappeville
France	Metz	Le Ban-Saint-Martin
France	Metz	Longeville-lès-Metz

France	Metz	Scy-Chazelles
France	Metz	Marly
France	Metz	Montigny-lès-Metz

Control Group II

Country	Arrondissement	Commune
Belgium	Arrondissement de Liège	Fléron
Belgium	Arrondissement de Liège	Flémalle
Belgium	Arrondissement de Liège	Visé
Belgium	Arrondissement de Liège	Soumagne
Belgium	Arrondissement de Liège	Oupeye
Belgium	Arrondissement de Liège	Liège
Belgium	Arrondissement de Liège	Awans
Belgium	Arrondissement de Liège	Sprimont
Belgium	Arrondissement de Liège	Grâce-Hollogne
Belgium	Arrondissement de Liège	Beyne-Heusay
Belgium	Arrondissement de Liège	Ans
Belgium	Arrondissement de Liège	Dalhem
Belgium	Arrondissement de Liège	Chaudfontaine
Belgium	Arrondissement de Liège	Seraing
Belgium	Arrondissement de Liège	Juprelle
Belgium	Arrondissement de Liège	Trooz
Belgium	Arrondissement de Liège	Comblain-au-Pont
Belgium	Arrondissement de Liège	Bassenge
Belgium	Arrondissement de Liège	Aywaille
Belgium	Arrondissement de Liège	Esneux
Belgium	Arrondissement de Liège	Blégny
Belgium	Arrondissement de Liège	Neupré
Belgium	Arrondissement de Liège	Herstal
Belgium	Arrondissement de Verviers	Dison
Belgium	Arrondissement de Verviers	Aubel
Belgium	Arrondissement de Verviers	Limbourg
Belgium	Arrondissement de Verviers	Welkenraedt
Belgium	Arrondissement de Verviers	Baelen
Belgium	Arrondissement de Verviers	Thimister-Clermont
Belgium	Arrondissement de Verviers	Jalhay
Belgium	Arrondissement de Verviers	Plombières
Belgium	Arrondissement de Verviers	Herve
Belgium	Arrondissement de Verviers	Lontzen
Belgium	Arrondissement de Verviers	Raeren
Belgium	Arrondissement de Verviers	La Calamine

Belgium	Arrondissement de Verviers	Eupen
Belgium	Arrondissement de Verviers	Spa
Belgium	Arrondissement de Verviers	Olne
Belgium	Arrondissement de Verviers	Trois-Ponts
Belgium	Arrondissement de Verviers	Theux
Belgium	Arrondissement de Verviers	Stavelot
Belgium	Arrondissement de Verviers	Malmedy
Belgium	Arrondissement de Verviers	Stoumont
Belgium	Arrondissement de Verviers	Verviers
Belgium	Arrondissement de Verviers	Waimes
Belgium	Arrondissement de Verviers	Pepinster
Belgium	Arrondissement de Verviers	Lierneux

Figures

Figure 6.5: Distribution of distance to border, by gender

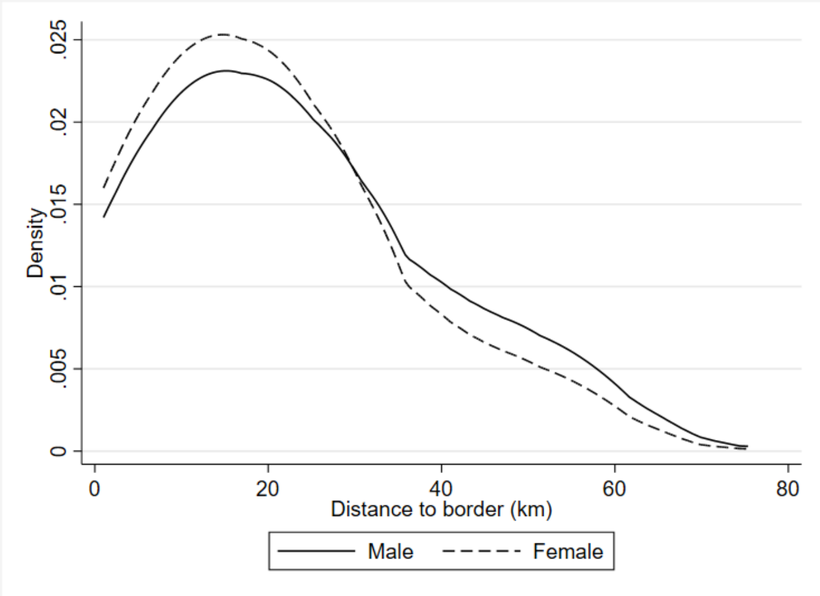


Figure 6.6: Absenteeism by roadworks status, over distances

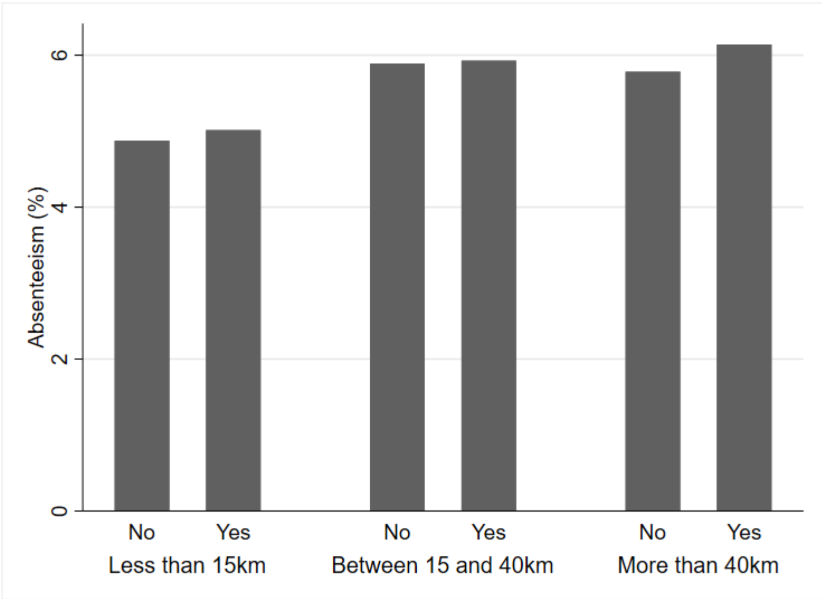
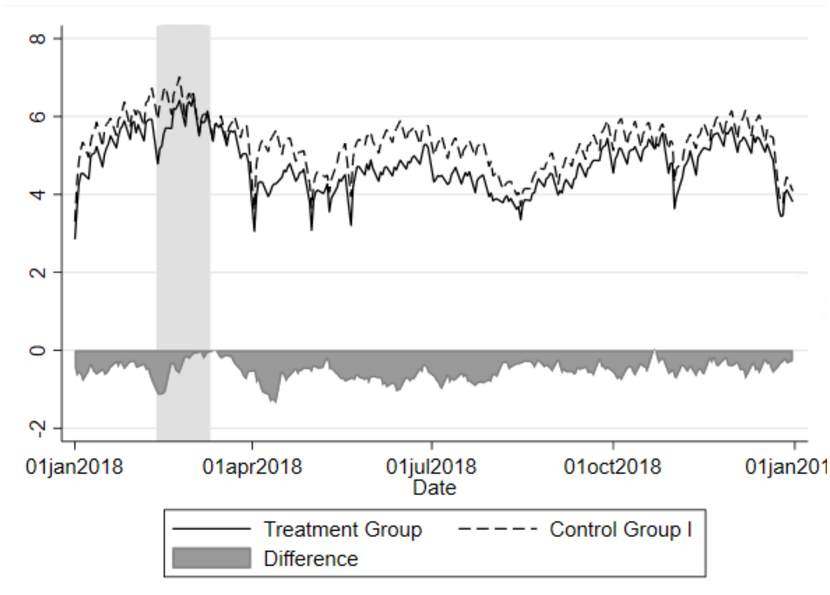


Figure 6.7: Comparison of daily absenteeism rates in 2018 between Treatment and Control Group I



Note: This figure illustrates the 2018 absenteeism rate trajectories for both the Treatment Group and Control Group I. The 'Difference' line is derived by subtracting the absenteeism rate of Control Group I from that of the Treatment Group at each point in time, thereby illustrating the rate disparity between these two groups. A shaded region from February 18th to March 4th, 2018, is emphasized to indicate a period hypothesized to be significantly impacted by weather conditions. The y-axis denotes the absenteeism rate as a proportion, while the x-axis represents the progression of time throughout 2018.

General conclusion

General conclusion

Charting the now and the next

This research delves into the intricate interplay of specific elements shaping both individual and societal well-being. The study hones in on the dynamics of urban poverty, the ripple effects of the COVID-19 pandemic, and tangential factors like commuting time and weather conditions fluctuations.

This exploration aims to demystify a portion of the complex matrix of challenges contemporary individuals face. Key insights derived from this study include:

- **Urban poverty in American cities** Spanning data across four decades and using a family of urban poverty indices rooted in robust normative foundations, Chapter 1 examines the trends and drivers of urban poverty in American cities. The examination reveals substantial heterogeneity in urban poverty patterns across cities, with a spatial component of urban poverty particularly pronounced in larger cities with heightened poverty concentrations. Demographics and income distribution surface as pivotal determinants of urban poverty patterns, suggesting contrasted results compared to concentrated poverty indices. Gentrification also has a subtle yet significant influence.
- **The onset of COVID-19 and urban poverty** Chapter 2 highlights that the early propagation of COVID-19 had a strong association with urban poverty. A one standard deviation increase in urban poverty is associated with a rise of 10% in the average county-level incidence of new COVID-19 cases in high-incidence counties. Intriguingly, the efficacy of mobility restrictions, especially stay-at-home directives, exhibited a complex interrelation with urban poverty. Stay-at-home orders may contribute to a faster spread of the virus in cities where poverty is less distributed across neighborhoods.
- **Rise in domestic violence amidst the pandemic** Chapter 3 highlights that the implementation of public health measures, including stay-at-home orders, was followed by a surge in domestic violence-related Google searches, peaking two weeks post-implementation. While these effects tapered gradually, fluctuations in policy strictness seemed to modulate Google search patterns even months after initial policy rollouts. Additionally, the tentative elucidation of the complex relationship between economic policies, individual compliance, and domestic violence via Google search data suggests that, while individual compliance with measures may decline over time, economic support policies may inadvertently exacerbate domestic violence.

- **Psychotropic drug purchases in Luxembourg** Using large-scale administrative data, Chapter 4 explores Luxembourg's patterns in psychotropic medication purchases during the pandemic and presents a nuanced picture. Purchases of antidepressants swelled, while anxiolytics, hypnotics, and sedatives remained relatively stable or declined. The exploration detected disparities in the patterns of the evolution of psychotropic purchases based on age, gender, household size and composition, employment status, and income. Younger individuals displayed escalated medication purchases, hinting at looming mental health concerns.

- **Weather conditions and their influence on mood** Chapter 5 illuminates the nuanced relationship between transient weather conditions, especially temperature, and mood by leveraging a randomly selected array of geotagged tweets from US Twitter users in 2014. The study leverages emojis and emoticons within Twitter content as indirect mood markers. The results notably highlight a negative impact of temperature on individuals' moods but suggest some non-linearity, hinting towards an ideal of comfort.

- **Commuting time and absenteeism** Chapter 6 analyzes the relationship between commuting time and work absenteeism by leveraging a natural experimental setting in Luxembourg based on a commuting disruption affecting specific cross-border workers. The findings spotlight a positive correlation between commute time and absenteeism. These absences, primarily attributed to sickness and family-related absences, hint at a recuperative balance between professional and personal lives, especially pronounced among the male demographic and long-distance commuters.

These insights shed light on numerous dimensions influencing well-being, providing perspectives for policymakers, scholars, and the general public. For instance, this thesis underlines that urban poverty, while an economic impediment, has ramifications that cascade into health inequalities, suggesting further and profound consequences. Also, the COVID-19 pandemic, initially unfolding as a health exigency, implied deeper psychosocial concerns, as evident through evolving drug purchasing behaviors and escalated domestic violence incidents.

This work resonates with global themes, sculpting current societal narratives. Some overarching ramifications include:

- **Health outcomes in the light of socioeconomic inequalities** The findings on urban poverty and its tie to the COVID-19 pandemic offer an exciting yet alarming reflection of the broader theme wherein socioeconomic divides dramatically sway health outcomes. It raises the question of how socioeconomic inequalities translate into tangible health disparities, a theme that resonates far beyond the confines of American urban cities.

- **Work-life balance in contemporary times** Delving into the repercussions of commuting time on absenteeism catalyzes an expansive dialogue on the evolving challenges of work-life balance in today's dynamic landscape. As urban congestion may intensify in some areas, how will commuting times and evolving professional dynamics recalibrate our occupational ethos and personal realms?

- **Interplay of environment and mood** The investigation into weather-induced mood fluctuations, anchored in Twitter data, accentuates the expansive domain of environmental psychology. It invites a deeper investigation into how our external milieu – from temperature to urban design – influences our psychological and emotional landscapes.

- **Unintended consequences of crisis management** The stark rise in domestic violence following the introduction of stay-at-home orders transcends mere data – It stands as a solemn testament to the far-reaching societal ramifications of global events like pandemics. It prompts us to consider how responses to such crises, while essential for public health, might inadvertently birth adverse societal consequences.

Harnessing digital data

A standout element of this work is the prolific use of digital data in probing well-being and mental health dimensions. With our lives increasingly moving online, digital data may offer a near-real-time reflection of societal nuances. Embracing Google search and Twitter data hints towards the vast possibilities inherent in such digital traces. Digital platforms may be more than just tools; they may act as mirrors reflecting at least a partial view of the societal psyche, for some, in real-time. Each interaction – a click, search, or tweet – may serve as a diagnostic pulse, enabling scholars to grasp the zeitgeist of prevailing moods, concerns, and paradigm shifts.

The COVID-19 pandemic further spotlighted this digital evolution. As people grappled with lockdowns and social isolation, digital platforms saw a surge in unique uses. Some habits may have rapidly transformed from immersive online fitness sessions and mental health symposiums to virtual travel and digital therapy, continuously shaping the digital data spectrum. This enriched the data reservoir and diversified the insights it could offer.

Yet, while the potential of digital data is immense, it is imperative to tread cautiously. Privacy concerns, data security, and the ethical implications of harnessing personal traces for academic purposes remain paramount. Furthermore, disparities in digital engagement and use across demographic divides, such as age brackets or socio-economic tiers, can inadvertently infuse biases in representation. Therefore, while digital data offers an innovative tool, it requires meticulous handling to ensure ethical and accurate empirical insights.

Further research avenues

This research paves the way for numerous future explorations. Potential areas may include:

- **Post-pandemic economic resilience** Understanding how cities and urban populations rebound economically post-pandemic. Which policies and measures may have accelerated economic recovery, and how have they reshaped urban poverty landscapes?
- **Emerging digital mood barometers** As digital platforms undergo metamorphosis, which tools can serve as mood indicators?
- **The future of commuting** With the rise of remote work and changing workplace structures, how might commuting trajectories change in the forthcoming decades? How will such metamorphoses affect absenteeism, occupational contentment, and employee well-being?
- **Urban poverty and technological divide** As technology becomes an integral part of our daily lives, how might the technological divide either deepen or bridge inequalities? Which roles may play e-learning,

telemedicine, and digital job platforms in the context of urban poverty and intergenerational mobility? Furthermore, what is the extent of biases in digital data methodologies prompted by urban poverty?

While this endeavor has revealed pivotal insights, it highlights the necessity of a further multifaceted, interdisciplinary lens. In a world perpetually in flux, this research underscores the exigency to continually refine our investigatory paradigms—allowing us to address present-day quandaries while anticipatorily navigating forthcoming complexities.

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