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Faculty Foreword

Dear Reader,

What a pleasure to see the bright minds of the Vancouver School of Economics (VSE) produce this journal. As you will see in reading it, the creativity and diligence of the students who wrote the five incredible papers in these pages is matched by the initiative, hard work and organizational skills of the IONA Journal editorial team.

Having taught econometrics to many of you, I couldn't be more proud to see the use you've made of those skills. These papers should also serve as inspiration to future cohorts – you too can write like this! None of the authors featured in this issue came to the VSE knowing how to write an economics paper. They each got there the way you will get there: spending long hours cursing at unhelpful Stata error messages until you realise you forgot to close a loop.

Any reader who comes to this journal thinking that economists spend their time measuring GDP or predicting inflation will, I hope, be pleased to discover that what we study is both much broader and more fun. You will find papers on mental health, oil pipelines, malaria and how elite mountain climbers evaluate risk.

Akash Uppal studies the effect of Rohingya refugees on the mental health of the communities that host them. These host communities are made up of people who are themselves poor but perhaps not as poor or traumatized as the refugees fleeing violence. Before reading it ask yourself – what effect would you expect?

Avah Hawkins, Jingyi Li, Candace Sykes, and Kelsey Wong study a question closer to home and relevant for all of you reading this: in Canada, does getting a university education affect your mental health? Maybe don't answer this while you're stressed and anxious about an exam.

Bora Hosal has a paper on a deeply important and timely topic: oil pipelines connecting Canada and the United States. Addressing climate change, and dealing with the energy crises that are sure to happen as we transition, requires making difficult decisions. As economists, we hope to help politicians and voters make these decisions with all the necessary information.

Lorena Edah studies one of the great tragedies of the COVID-19 pandemic: reduced effort to prevent and treat other common diseases worldwide. Malaria doesn't just sound scary – it's one of the top killers of children worldwide. Lorena studies how the strain on the healthcare system in Burkina Faso affected cases and deaths from malaria.

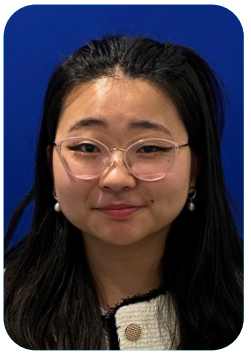
Sarah Wappel uses a niche setting to say something important and widely applicable. We make decisions about risk throughout our lives. Should I buy bitcoin? (Please no!) Should I get on a Boeing 737 Max? Should I pick a safe-but-boring career or make it big on TikTok? It's usually hard to study what specific factors go into these decisions. Sarah looks at Everest climbers to understand how a higher cost for the permit needed to climb affects the life-and-death decisions climbers make on the mountain.

These short summaries don't do the papers justice. Please read them and reach out to the authors with your thoughts – they'd love to hear from you.

To those of you I've had the pleasure to teach, it's been a privilege getting to know you. I hope you feel that the time you've spent in the stern but loving arms of the VSE will serve you well in the path ahead. May the ideas you've come across in your time with us lead you to an intellectually fulfilling career. If not, try napping in a very large dog bed – you might come up with the next big idea.

Munir Squires
Vancouver School of Economics, Assistant Professor
University of British Columbia

Student Foreword



Dear Reader,

On behalf of the Vancouver School of Economics Undergraduate Society (VSEUS), I sincerely congratulate the IONA Journal team for another successful year – it is the innovation and action like that of the team that enables students to challenge concepts explored in the classroom and apply them real-world scenarios. In this edition, we celebrate the thoughts explored and ideas generated by our student scholars following the turbulence of COVID-19.

I hope that this edition of the IONA Journal encourages you to think deeply and critically about a topic or tool in economics. I hope it challenges you to consider a concept that you once overlooked and think critically about how you may be able to leverage it to help solve a problem the world is facing today. Indeed, scholars who are published in the IONA Journal have gone off to make some incredible contributions to the world today, and tomorrow. VSEUS is committed to continue supporting IONA Journal's growth and the VSE student community's research pursuits. Together, VSEUS hopes that the student academic experience at the VSE will continue to strengthen. Cheers to our innovative and accomplished students whose research we celebrate today. Enjoy!

Sincerely,

Erin Song
President
Vancouver School of Economics Undergraduate Society

Letter from the Director of Operations

Dear Reader,

A large part of what makes economics so meaningful to me is the tremendous opportunity it presents for community building and knowledge sharing. In such a broad discipline, any individual can contribute to niches so unique and fascinating that it consistently inspires me to be a more curious, open-minded learner. I like to think that this sentiment is shared among my peers in IONA and in UBC's economics community at large. The papers we've curated and edited for you this year represent just a tiny fraction of the many contributions our community has made to the scholarly conversation. I'm proud to be a part of this community—one that was able to foster the ability of these undergraduate students to speak amazing stories in their niche.

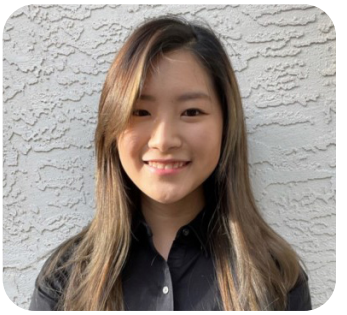
Flip or scroll on and you'll find pages of dense texts, figures, and tables, representing hundreds of hours of research and meticulous analysis. I hope you can picture our authors' long nights, the hoops and struggles they went through, and the good number of 'Eureka!'s that must have come along the way. I hope you'll also notice the influence and touch of our diligent and hardworking Editorial Board and Faculty Review Committee, without whom this volume would not have been possible. My sincerest admiration will always go to Annie and Rachel, who have both gone above and beyond for the IONA Journal of Economics over the years.

Thank you, dear reader, for participating in this community—I hope you can walk away having gained a deeper appreciation for the art and science of economics and the community of great young minds at work behind it. I hope it inspires you to find your voice, and tell amazing stories one day in your own space.

Very truly yours,

Peter Jiang
Director of Operations, Volume VII

Letter from the Editors-in-Chief



Dear Reader,

For many of us at UBC's Vancouver School of Economics (VSE), economics can tell a story. Indeed, these stories may be numerical ones that explore significant changes – and we also see how VSE students use economics to tell innovative stories about novel relationships and potential effects. The IONA Journal showcases these outstanding stories of research papers written by undergraduate student authors, carefully curated and enhanced by the IONA Journal Editorial Board of undergraduate students and the Faculty Review Committee of professors, who dedicate their time and unique insights to determine which economics stories may be the most unique, innovative, and impactful.

What makes a good story? Is it when a paper's research is relevant to the reader, such as Volume VII's papers exploring the oil industry or the effect of university education on mental health? Perhaps good stories include plot twists – a new econometric tool being applied to identify new relationships, or a new situation being explored with our econometric resources. Volume VII includes these types of good stories too: published authors explore the situations of refugees, pandemics and epidemics, all the way up to Mount Everest. After over half a decade since its inception, the IONA Journal of

Economics remains committed to showcasing the forefront of undergraduate research in the discipline of economics.

The IONA Journal of Economics reflects the efforts of our outstanding published authors, our Editorial Board, operations staff, professors of the Faculty Review Committee, and amazing stakeholders. More broadly, this initiative highlights the vivacious academic community at the VSE – a promise of diversity and rigor inherent in undergraduate economics research.

As you explore the stories of each research paper in Volume VII, we thank you for engaging with the ideas and scholarly discourse of the economics community at UBC. This initiative only exists because of the outstanding efforts of the UBC economics community – including you. Dear reader, you are one of the most important parts of the IONA Journal's story, which we have been honoured to continue sharing in Volume VII and beyond.

With much gratitude,

Rachel Lee & Annie Chang
Editors-in-Chief
IONA Journal of Economics Volume VII

Contributors

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Annie Chang & Rachel Lee

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Bora Hosal
Jingyi Li
Candace Sykes
Akash Uppal
Sarah Wappel
Kelsey Wong

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I

PAPERS

How to Host: Evidence from the World's Largest Refugee Camp

Akash Uppal

ECON 494

ABSTRACT

I document the experience of refugees and estimate the effects of a refugee influx on the host community. In August 2017, a military crackdown on Rohingya by Myanmar's armed forces resulted in over 700,000 refugees fleeing to camps in neighbouring Bangladesh. By exploiting quasi-random, geo-spatial variation in host community interaction with refugees, I find that high spillover host community respondents experience disproportionately higher exposure to trauma compared to low spillover households. That being said, they report lower depressive symptoms and reduced symptoms of trauma, supporting a social relativism theory of reference dependence as it relates to the self-assessment of subjective health outcomes in this setting. These results suggest that base group adjustments in the provision of counselling and support services may play an important part in treatment for trauma-exposed individuals.

Keywords: mental health, reference dependence, refugee

JEL Codes: I15, I31, J15

1. INTRODUCTION:

Reference dependence has been widely studied in behavioural economics. The idea, born out of careful observation of the way agents make decisions and evaluate outcomes, is both appealing and intuitive: outcomes are not experienced on an absolute scale, but rather evaluated relative to a certain point of reference. This conception of evaluating prospects was introduced by Kahneman and Tversky (1979), who formalized prospect theory and shed light on the mechanisms that drove deviations from the expected behaviour of rational agents. Understanding how these deviations affect mental health is central to being able to provide psychological support services and improve general well-being (Phillips, 1967; Lu & Shih, 1997).

These services hold value for refugee populations, who often experience stress and trauma (Stein, 1981; Fazel & Stein, 2002). As of mid-2020, over 80 million people have been displaced worldwide because of conflict, persecution, human rights violations, and violence (UNHCR, 2020). Refugees living abroad make up over 25 million of this total, making the current levels of forcible displacement worldwide the highest on record. The world's fourth largest refugee population comes from Myanmar, where over a half of those forcibly displaced are children (UNICEF, 2020). The scale of this issue will only increase over time – by 2050, there will be an estimated one billion forcibly displaced people from climate change alone (IOM, 2014). Evaluating how these refugees are impacted by the rapid deterioration in their circumstances, along with how their influx may affect host communities in neighbouring countries, is critical to designing policies that promote the health and safety of refugees, along with the design of assistance schemes and government programs.

Despite receiving increased attention in recent years (Taylor et al., 2016; Uri & Mona, 2016; Alloush, 2017; Dustmann, 2017; Evans, 2017), literature in this field suffers from four key limitations. First, most studies focus on refugees living in developed nations with a greater capacity to support refugee populations than developing nations. Second, most studies focus on immigrants rather than refugees, whose decisions to leave their home countries may have been driven by fear from an imminent threat, rather than motivated by a search for enhanced opportunities. Third, the sparse literature that deals with refugees in the context of developing nations tends to focus on economic outcomes, rather than examining health or mental health specifically. Fourth, most studies only offer qualitative evidence without an empirical approach or rely upon simulations and projections, generally due to a lack of primary data availability.

This paper presents a real-world estimate of the effects of an unexpected refugee influx on host community mental health. Not only does focusing on this setting bridge gaps in the literature, but by utilizing quasi-exogenous geo-spatial variation in host household distance to refugee camps, this paper helps to better understand impacts on the host community. I further control for a series of characteristics that may differ between host households living close and far from the refugee camps to approach a causal estimate of the effect of this refugee influx on the mental health of host communities. In doing so, I provide evidence of social reference dependence in the self-evaluation of mental health. Furthermore, I can rule out material improvements explaining these differential impacts, as it relates to the domain of healthcare access.

My analysis focuses on the Rohingya refugee camps in the southern tip of Bangladesh, which have grown to become the largest refugee camps in the world. Following military crackdowns and violence by armed forces in the majority Buddhist Myanmar, the Rohingya were forced to flee to the Cox's Bazar region of Bangladesh. During the period from August to December of 2017, approximately 750,000 Rohingya left Rakhine State, Myanmar, crossing the border by foot or raft. Mental health is particularly important in this context, given that many refugees were exposed to traumatic events such as abductions, rapes and murders. Video recordings of these atrocities further documented mutilations of dead bodies and the burning of entire homes and villages. Satellite imagery confirmed the destruction of at least 392 villages, constituting 40 percent of all northern Rakhine settlements, with 80 percent of burned villages being destroyed within the first three weeks of Myanmar's "clearance operations" (Hussam, 2021). Although sympathetic, host populations in Bangladesh did not always welcome refugee populations, who were believed to increase crime, usurp employment opportunities, and displace health and aid resources.

Several research papers dispute the veracity of these beliefs (Alexander Betts et al., 2014; Macchiavello, 2003; Milner, 2016; Sanghi, Onder & Vermuru, 2016; Rubin, 2017). As such, this paper finds its focus not on the refugees themselves, but rather on the host communities in which they seek refuge. In particular, this paper focuses on whether differential mental health outcomes exist for host countries of varying distances from the refugee camps. To do this, I make use of the Cox's Bazar Panel Survey (CBPS), which is a comprehensive, large-sample survey that includes both refugee and host households. This cross-sectional dataset was released on March 4th, 2021 with surveys conducted from March to August of

2019. Not only does the survey ask questions about various outcomes prior to refugee displacement, but it offers insights into the income, consumption, labour market, safety, and mental health of surveyed households – half of which are in refugee camps and half of which are in host communities.

Preliminary analysis of the CBPS presented by Davis, Lopez-Pena, and Mobarak (2021) finds that in Bangladesh, most of the negative effects of the refugees are concentrated on the refugees themselves. Not only did refugees experience major losses of productive assets resulting from the dissolution of their property rights, but there appears to be no evidence of dissaving by host households. Building on this preliminary work, this paper provides supplementary evidence that although the perception of crime for those living near the camps is higher than for those living far from the camps, the incidence of crime is relatively consistent between these areas (Appendix A), supporting the notion that stereotypes about refugees perpetrating conflict persist for communities living adjacent to the camps. Yet, host employment and earnings did not see a reduction following displacement; in fact, they saw a modest increase (David, Lopez-Pena & Mobarak, 2021). Further, refugees earned much lower post-displacement, despite no large reduction in productive capacity or desire for employment. Overall, this suggests that a hypothesis of taste-based discrimination against refugees may take precedence over a narrative of statistical discrimination, resulting in worse labour market outcomes for refugees without a negative effect on average for those in the host community.

As it relates to the mental health of host communities, I find that although exposure to trauma was elevated for those living close to the camps, this did not translate to greater self-reported symptoms of trauma or symptoms of depression. In fact, those living in high spillover zones actually exhibited statistically significant reductions in symptoms of depression and symptoms of trauma. These findings support a social relativism theory of reference dependence, in which people who live closer to the refugee camps view their own outcomes as relative to the refugees and thereby report better mental health. Given that these high spillover areas also saw less improvements in healthcare access relative to before the refugees arrived, a variable that is positively associated with overall health, these findings may be understating the true magnitude of the effect. These findings are robust to a set of pre-crisis demographic controls, along with pre-crisis income and occupation controls. As such, in offering therapy and counselling to improve mental health, changing the base group against whom people compare themselves could be a low-cost intervention to facilitate self-reported improvements in mental health.

The rest of this paper proceeds as follows: Section 2 reviews the relevant literature and provides additional context for the research setting; Section 3 describes the survey design and how the data fits into a broader empirical strategy; Section 4 presents the hypothesis and results; Section 5 discusses these findings; and Section 6 concludes.

2. LITERATURE REVIEW:

2.1 Reference Dependence

When reference dependence first gained prominence in Kahneman and Tversky (1979), it was framed in the context of decision-making under uncertainty and how individuals think in terms of expected utility relative to a reference point. This reference point is often characterized by comparisons against the status quo, from which deviations are categorized as either a gain or loss in value. This understanding of reference dependence has been evaluated in a range of settings. For example, in a paper linking a rise in family violence to upset losses in professional football, Card and Dahl (2011) construct a model of violence risk that is based around this "gain-loss" utility of game outcomes pinned around a rationally expected reference outcome for the game. When controlling for pregame point spread and the size of the local viewing audience, they find that upset losses increase the rate of at-home male-on-female violence by 10 percent. When looking at upset wins or losses in close games with uncertain outcomes, they find small and insignificant effects. This paper thereby supports an asymmetry in the gain-loss utility function, while also not finding evidence for reference point updating based on the halftime score.

Although the family violence and football paper was written three decades later, it provides evidence for Kahneman and Tversky's (1979) prospect theory, in which people underweight outcomes that are "merely probable in comparison with outcomes that are obtained with certainty," violating expected utility theory. In line with this framework, loss aversion (the tendency to prefer avoiding losses over acquiring equivalent gains) and the endowment effect (the tendency to value an owned object higher than its market value) were also demonstrated (Kahneman & Tversky, 1979; Kahneman et al., 1991). Samuelson and Zeckhauser (1988) further introduced a status quo bias, in which agents prefer not to deviate from the reference point (particularly when more choices are included in choice sets). Reference dependence in the context of comparisons with the status quo has been demonstrated in relation to retirement decisions, revealed brand allegiance, and organ donation defaults (Behaghel &

Blau, 2012; Hardie et al., 1993; Johnson & Goldstein, 2003).

That being said, the reference point need not be the status quo – consider comparisons made against past states, expectations regarding future states, or social comparisons. Expectations, which can be based on one's own experience, can impact the behavioural biases exhibited. For example, a field experiment by List (2003) demonstrated an endowment effect for inexperienced sports card collectors, but a near-zero effect for experienced sports card collectors whose expectation is to part with items they have secured. Yet, these expectations can also be heavily impacted by social comparisons against the performance of others. In a study focusing on Indian manufacturing workers, Breza, Kaur and Shamdasani (2018) demonstrate how relative-pay concerns can have potentially broad labour market implications. By randomizing whether co-workers within production units receive the same daily wage or differential wages in relation to their baseline productivity ranks, they show that pay inequality reduces output by 0.45 standard deviations, attendance by 18 percentage points, and reduces co-workers' ability to cooperate in their own self-interest. Crucially, they demonstrate how these effects only persist when coworkers' productivity is difficult to observe, such that the differential pay cannot be explained by discernible productivity differences. As such, this study presents evidence in which the reference point is not one's own base state, but a social comparison against coworkers of similar productivity. In an earlier paper on the effect of peer salaries on job satisfaction, Card et al. (2012) reveal information on peers' salaries to a subset of employees at the University of California. They find an asymmetric response to the information treatment, whereby workers with salaries below the median for their occupation and pay unit report lower job satisfaction and an increase in the likelihood of searching for a new job. However, workers above the median report no higher satisfaction or change in job search intentions, suggesting a nonlinear relationship between relative pay comparisons and job satisfaction.

This paper focuses on this latter conception of reference dependence as being relative to others. Notably, in this setting, host country respondents would be demonstrating reference dependence against a group that they cannot become – Rohingya refugees. Not only has reference dependence not yet been applied to the refugee context, but evidence in this setting could reveal that individuals need not evaluate their outcomes to people they could plausibly become; rather, social comparisons with those in one's immediate environment may be enough to shift subjective outcomes. If this is the case, then information treatments may be one of the most cost-effective ways to improve self-reported mental health outcomes.

2.2 *Migration and Impacts on the Host Community*

For decades, economists have been studying the effects of immigration on host countries, finding mixed results. Dozens of studies have demonstrated that immigrants can have important benefits to host economies, ranging from contributions to the labour force, increasing innovation through the filing of patents, and contributing minor positive net fiscal effects over the medium- to long-run (Card, 1990, Stephan & Levin, 2001; Saiz, 2003; Saiz, 2007; Gonzalez & Ortega, 2013). However, there are distribution concerns when certain segments of the host population may be adversely affected. In particular, employment and wage displacement may occur for less-educated natives or earlier immigration cohorts (Borjas et al., 1996; Borjas, 2009). How these effects manifest can depend on institutional differences across countries, with welfare dependency persisting over time in certain contexts (Kahn & MacGarvie, 2016; Kerr & Kerr, 2011). However, while immigrants choose to leave their home countries in search of opportunity, refugees' departures are driven by forcible displacement and they often experience severe trauma as they flee ethnic persecution or political violence. As such, refugees meaningfully differ from immigrants, particularly as it relates to mental health outcomes.

The importance of studying refugees specifically cannot be understated. There are over 25 million refugees in the world today (UNHCR, 2020), with roughly 85 percent living in developing countries (UNHCR, 2016). Although these are the settings in which host communities might have the lowest capacity to support migrant populations, generally facing high population density and being relatively lower-income, the vast majority of the migration literature focuses on labour and wage outcomes for those in developed nations. The reason for this is that data related to refugees and their host communities in developing countries is either not collected or only available for small, non-representative samples of the population. Nonetheless, some research on refugees has been conducted.

For example, Bjanesoy (2019) shows that while host respondents in Norway may view refugees as deserving of help when they first arrive in the host country, these views grow less sympathetic over time. Bowes et al. (2009) suggest that media and political stereotypes may vilify refugees and portray the forcibly displaced as "burdensome competitors for economic benefit of some sort or another." This distaste for refugees may be amplified in developing countries where resources to assist the forcibly displaced may be limited. This could have negative impacts on the mental and overall health of the host community (Gebrehiwet et al., 2020).

Understanding how refugees impact host communities and

vice versa is central to crafting inclusive and constructive policies that foster long-term solutions. Acknowledging the psychological and physical effects of these interactions on members of the host community is a worthwhile first step in this direction.

2.3 Rohingya Refugee Context

I study this social relativism theory of reference dependence in the Rohingya refugee camps near the southern tip of Bangladesh. Between August and December of 2017, approximately 750,000 Rohingya fled a genocidal campaign by armed troops in Rakhine State, Myanmar. These refugee camps constitute the largest refugee settlement in the world, imposing an economic and psychosocial burden upon local communities in Bangladesh, who faced one of the highest population densities prior to August 2017. Increases in deforestation, inflation, and competition over scarce opportunities have been attributed to the refugee influx and are viewed as adversely affecting the livelihood of host community members (Khatun & Kamruzzaman, 2018; Tay et al., 2019). In addition, while assistance from international humanitarian organizations has certainly bolstered support for refugee populations, it has been unable to match the need for these critical services. Beyond experiencing the destruction of intergenerational assets, disruptions to employment contracts, and an inability to secure basic foodstuffs, many refugees have experienced severe psychological trauma associated with the violence perpetrated against them. In documentation collected by the United Nations International Fact-Finding Mission on Myanmar, eyewitness accounts describe how military operations were intended to "instill immediate terror, with people woken by intense rapid weapons fire, explosions, or the shouts and screams of villagers. Structures were set ablaze and Tatmadaw soldiers fired their guns indiscriminately into houses and fields, and at villagers" (Hussam, 2021). Exposure to such severe trauma would be expected to adversely affect mental health and well-being, potentially contributing to poverty traps.

However, it is important to study not only the experience of refugees, but also the experience of the host communities. In a large-scale survey exploring the perception of Rohingya refugees by the host community, participants were found to be feeling unsafe (85 percent) about the Rohingya residing nearby, reluctant (85 percent) to allow Rohingya children to attend the same school are their children, and prohibitive (48 percent) towards allowing Rohingya to access local facilities (Xchange Foundation, 2018). Host community members in

the Ukhiya region of the Cox's Bazar district in Bangladesh, which is home to the majority of Rohingya refugees, now make up only 24 percent of the total population in Ukhiya (ACAPS/NPM Analysis Hub, 2018). Interaction between these groups is common, in some cases occurring daily, particularly among households living closer to the refugee camps. Anecdotally, reports indicate small-scale conflict involving the collection of firewood, distribution of support, and spreading of rumors (Strategic Executive Group, 2019).

Given these experiences and negative perceptions toward refugees, we might expect that host households that interact with these refugees at a greater frequency would exhibit worse mental health and symptoms of trauma outcomes, in addition to exposure to traumatic situations involving refugee interactions. This can be described as the 'refugee aversion' channel (Staicu & Cutov, 2010). That being said, it is possible that host households with greater interaction with the refugees utilize refugees as a base group when evaluating their subjective outcomes (symptoms of depression and symptoms of trauma), resulting in better outcomes in both of these categories when compared to households with reduced interaction with the refugees. This outcome might hold despite households having a higher exposure to trauma, which is a function of hearing about, or witnessing traumatic events, in addition to whether these events are experienced. In other words, exposure to trauma is largely an objective outcome and thus less susceptible to deviations driven by reference-dependence. This countervailing channel is understood to be the 'social relativism' channel, suggesting reference-dependent outcome evaluation (Kahneman & Tversky, 1979). To understand which of these channels dominates, we turn to the data.

3. DATA:

3.1 Sample

My data comes from the first wave of the Cox's Bazar Panel Survey (CBPS). This is a comprehensive, large-sample survey that tracks both refugee and host households. The dataset offers insights into the income, consumption, labour market, crime, and mental health outcomes of those affected by the refugee influx beginning in August 2017. Although it will eventually become a panel dataset, it is currently cross-sectional.

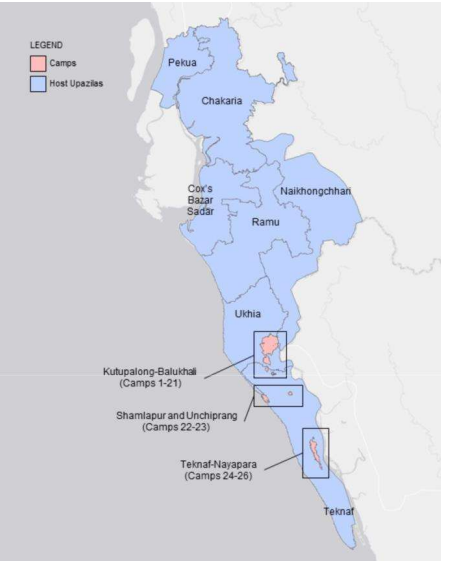
This detailed survey collects data from 5,020 households in six upazilas in Cox's Bazar District: Chakaria, Cox's Bazar Sadar, Ramu, Teknaf, Ukhia, and Pekua; along with one upazila in Bandarban District hosting a significant refugee population. The survey consists of two sections: (1) a household interview conducted with one

adult household member, which covers the household roster, food security, consumption, assistance, assets, household income, and anthropometrics of one randomly selected child under five; and (2) an adult interview conducted with two randomly-selected adults covering labour market outcomes, migration history, crime and conflict, and health. This results in a rich dataset featuring 9,386 adult interviews for 5,020 households with 25,421 households members. Half of these households live in one of the 27 internationally-recognized refugee camps and half live in one of the surrounding districts near the camps. Interviews were conducted between March and August 2019 by officials from Innovations for Poverty Action (IPA), Yale University, The World Bank, and Gender and Adolescence: Global Evidence (GAGE).

3.1.1 Camp Sample

Figure 1 displays a map of the host upazilas with the camps identified, from the CBPS data documentation. As it relates to sample selection, a full household listing was done in all selected primary sampling units (PSUs) in the sample.

Figure 1. Map of Host Upazilas and Refugee Camps



23,233 households were listed across 32 camps in the Ukhia and Teknaf upazilas in the Cox's Bazar district. Sample camp blocks with less than 400 households were fully listed, and camp blocks with more than 400 households were broken into sub-blocks of roughly

100 households using drone imagery and geographical boundaries. These sub-blocks were randomly ranked and sub-blocks were listed by rank until 400 households had been listed. Some of the field teams faced mass refusals, particularly in Kutupalong RC and Nayapara RC blocks, resulting from a general distrust of outsiders and the NGO community. Nevertheless, most camps had only a 0.5 percent refusal rate.

In selecting the sampling frame, the Needs and Population Monitoring Round 12 (NPM12) data from the International Organization for Migration was used. NPM12 divided all camps into "majhee" blocks, each of which represents a block in the Rohingya camps governed by local leaders called "majhees." 200 blocks were randomly selected using a probability proportional to camp size. A full listing was carried out in each selected camp block, apart from 8 blocks where respondents refused en masse. Within each listed camp block, 13 households were randomly selected, resulting in 2,600 camp households in total. 5 households were additionally randomly selected as replacements.

Figures 2, 3, and 4 display a map of the sampled camps with the camps identified, according to the CBPS data documentation.

Figure 2. Camps 1 to 21 Map (Kutupalong-Balukhali)

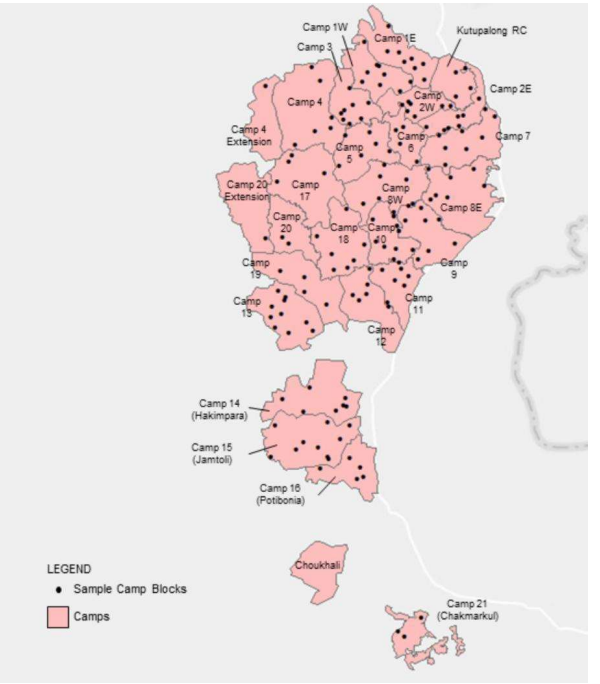
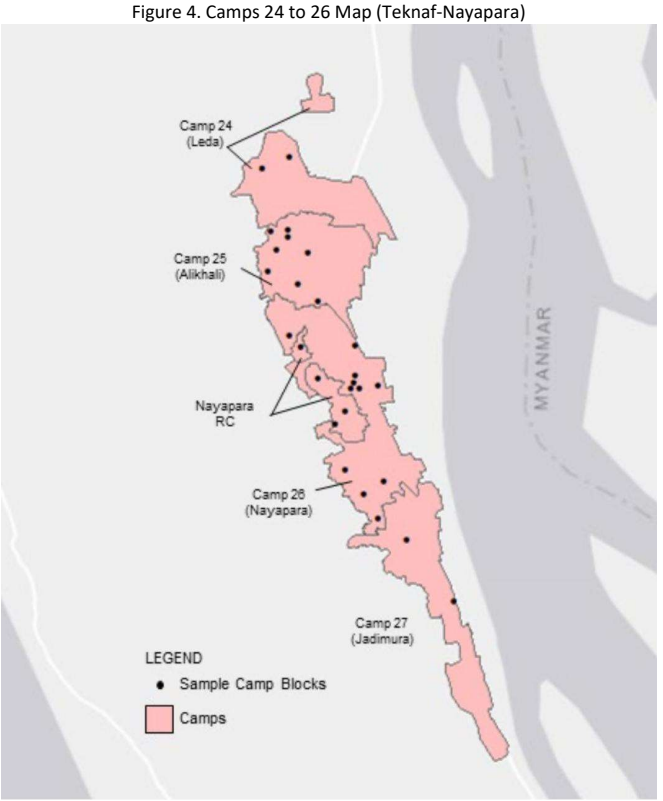


Figure 3. Camps 21 to 22 Map (Shamlapur-Unhciprang)

Figure 4. Camps 24 to 26 Map (Teknaf-Nayapara)



3.1.2 Host Sample

Regarding host households, 22,122 households were listed across 57 host mauza samples, which are the lowest administrative unit in the Government of Bangladesh among the 7 upazilas surveyed. Similar to the surveying of larger camp blocks, host blocks with more than 400 households were segmented into sub-blocks of roughly 100

households, using a combination of Google Earth drone imagery and Open Street Maps. This resulted in each of the 57 unique sample mauzas being divided into 100-household segments based on the 2011 Census population. 3 segments were randomly drawn from each sample and fully listed up to 100-150 households per segment.

For this study's selection of households to include in the sample, mauzas within the 7 upazilas are split into high spillover and low spillover host mauzas, based on whether they are within a 3-hour walking distance (or 15km distance) from the camps. A combination of road network maps from Open Street Maps and insights from locals is used to identify map areas within Cox's Bazar district that range from 2-hour to 5-hour walking distance from the camps. This is the reason for including seven mauzas from regions of the Bandarban district bordering Ukhia into the sampling frame, given their proximity to the campsites.

Figures 5 and 6 display a map of the sampled host segments and the mauzas within each upazila, respectively.

Figure 5. Host Segments Map

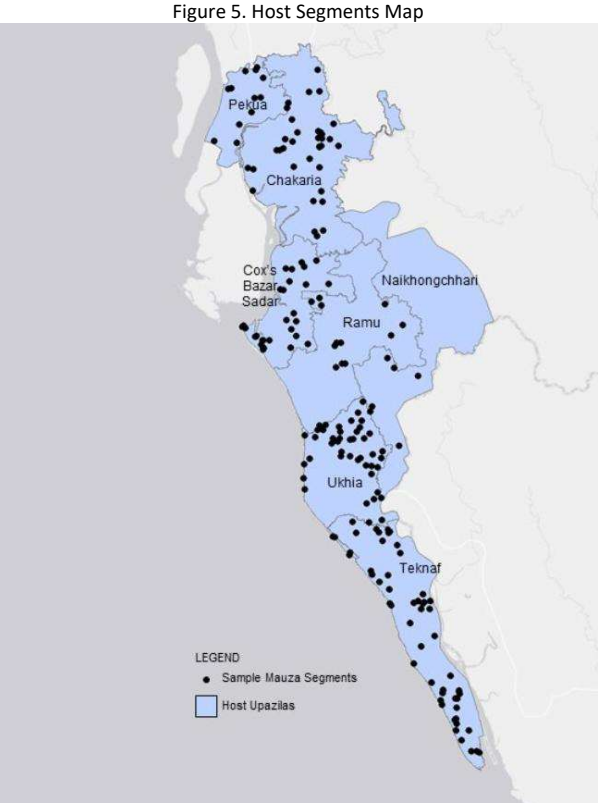


Figure 6. Mauza Reference Map

From the sample frame of 286 mauzas, obtained using data from the 2011 Bangladesh Population Census, 66 mauzas are randomly drawn using probability proportional to population size. 57 unique mauzas are identified, due to repeats for mauzas with a large population size. Each mauza is divided into segments of roughly

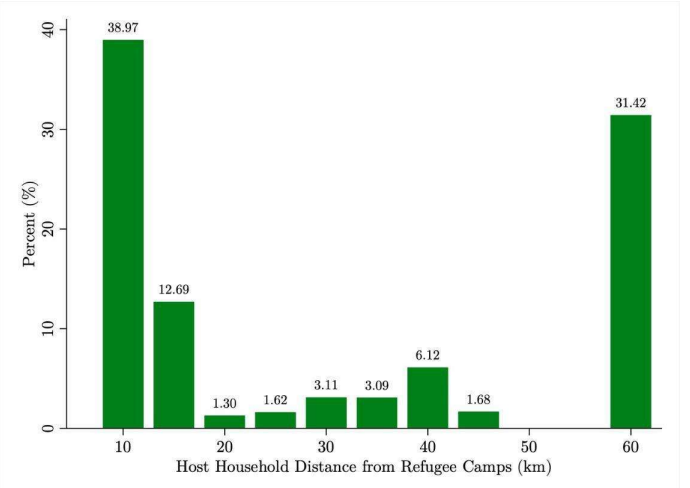
100 to 150 households based on reported census populations and 3 segments were randomly selected from the sample. This results in a total of 195 host segments being listed. Then, from each of the listed segments, 13 households were randomly selected to be surveyed, resulting in a total of 2,535 host households. 5 households were additionally randomly selected as replacements.

3.2 Scope of Analysis

In examining whether the refugee aversion channel dominates the social relativism channel, I focus on host country households. Then, I compare individuals that live close to the refugee camps, defined as being within a three-hour walking distance to the camps, to individuals that live far from the refugee camps. The former is sometimes referred to as high-spillover zones and the latter as low-spillover zones. Equivalently, the cut-off point for a low spillover versus high spillover household is whether they live closer or farther than 15 kilometres from the refugee camp. Having this cut-off not only constitutes a reasonable distance for which to make comparisons, given that most refugees use walking as their primary source of transportation, but it also splits host households into two groups of roughly equivalent size. This cut-off for distance also aligns with how those in charge of the Cox's Bazar Panel Survey defined high-spillover and low-spillover distance when constructing the sampling frame.

We can see the more detailed breakdown for this distance variable in Figure 7, which plots the percent of households within each binned distance.

Figure 7. Distance of Host Country Households from Refugee Camps



Given that data for the distance variable is binned to the nearest 5km in the raw data, the histogram produced was plotted in terms of discrete values that the distance variable took on. Ultimately, host households are split into two groups, yielding the group breakdown presented in Table 1.

Table 1. Breakdown of Groups

Respondent Category	Frequency	Percent (%)
Low Spillover (>15km away from camps)	2,456	24.85
High Spillover (15km away from camps)	2,597	26.27
Refugee Camps	4,832	48.88
Total	9,885	100

Notes: Those in high spillover zones live within a 3-hour walking distance to the refugee camps. Distance was calculated using a combination of road network maps from Open Street Maps and local insights.

Prior to conducting the analysis, it is unclear whether distance defined in this manner is exogenous, such that households living in high-spillover zones are like those in low-spillover zones along all relevant demographic characteristics. Fortunately, the CBPS contains a series of individual characteristics and we can calculate the average values for these characteristics, comparing between groups and testing whether the difference is statistically significant. Table 2 presents this comparison.

Table 2. Comparison of High Spillover and Low Spillover Individuals

	(1) Mean of low spillover	(2) Mean of high spillover	(3) Difference	(4) p-value
Host country sample (low spillover N = 2,300, high spillover N = 2,458)				
Age	33.34	32.48	-0.86**	0.04
Age Squared	1324.93	1267.20	-57.73*	0.01
Female	0.57	0.58	0.01	0.70
Born Bangladesh	1.00	0.98	-0.02***	0.00
Born Myanmar	0.00	0.02	0.02***	0.00
Born Other	0.00	0.00	-0.00	0.91
Religion Islam	0.93	0.93	-0.01	0.44
Religion Hinduism	0.05	0.01	-0.03***	0.00
Religion Buddhism	0.02	0.06	0.04***	0.00
Ethnicity Rohingya	0.00	0.03	0.02***	0.00
Marital Status Married	0.71	0.69	-0.02	0.14
Marital Status Never Married	0.23	0.23	0.00	0.95
Marital Status Widowed	0.05	0.07	0.02***	0.01
Marital Status Divorced	0.01	0.01	-0.00	0.48
Marital Status Separated	0.01	0.01	0.00	0.24
Can Read	0.66	0.55	-0.11***	0.00
Pre-Crisis Ann. Income	86665.16	37831.02	-48834.14	0.19
Pre-Crisis Ann. Log Income	3.43	4.12	0.69***	0.00
Pre-Crisis Employed	0.384	0.464	0.08***	0.00
Host country sample (low spillover N = 881, high spillover N = 1,140)				
Pre-Crisis Agriculture	0.30	0.41	0.11***	0.00
Pre-Crisis Worked for Wages	0.49	0.37	-0.12***	0.00

Notes: Columns (1) and (2) represent means for low spillover and high spillover individuals in the host country sample, respectively. Column (3) computes the difference calculated as (2)-(1), with stars to represent statistical significance based on the p-value reported in column (4). Significance level is denoted by *** p < 0:01, ** p < 0:05, * p < 0:1. As it relates to variable definitions, age is defined to be a continuous variable no smaller than 12 and no larger than 100. Age squared is calculated by taking the second power of the age variable. Pre-crisis annual income is also defined to be a continuous variable defined in terms of Bangladeshi Taka. Values of one are added to pre-crisis annual income when log linearizing the data, so as to preserve rank while not generating excess missing observations. This variable of Pre-crisis annual log income takes on values no smaller than 0 and no larger than 20. All other variables are transformed into dummy variables, taking on a value of one if the indicator applies for a given observation and zero otherwise.

Based on Table 2, those in high spillover and low spillover zones exhibit similar characteristics along a variety of demographic indicators, including age, gender, country of birth, religion,

ethnicity, and marital status. That is, although the difference may be statistically significant for these categories, they do not seem to be meaningful differences that demonstrate systematic differences that are likely to explain differential mental health outcomes between the groups. That being said, if we look at an indicator for literacy, we can observe that while 66 percent of those in low spillover zones can read, only 55 percent of those in high spillover zones can read. This seems to suggest that those living closer to the refugee camps may be of a lower socio-economic status, which is confirmed when looking at their pre-crisis annual income: those living close to the refugee camps earned 37,831 Bangladeshi Taka on average (equivalent to roughly 450 USD) from July 2016 to July 2017, while those living far from the refugee camps earned an average of 87,000 Bangladeshi Taka (over 1,000 USD). Although this difference in income is large, it does not appear to be statistically significant. Notably, these income disparities exist despite an increased share of employment from those in high spillover zones, with 8 percentage point higher employment prior to the crisis when compared to those in low spillover zones. Compositionally, this suggests that the types of jobs being worked by those closer to the refugee camps are lower-paid; in fact, they are 11 percentage points more likely to have worked an agricultural job compared to those in low spillover zones. This is further supported by examining the "Pre-Crisis Worked for Wages" indicator, whereby those in high spillover areas are 12 percentage points less likely to have worked a job for wages paid by a company or the government, instead working on their account or in a business enterprise. Earnings for these other types of work are included when calculating pre-crisis annual income.

Although the groups have similarities among baseline demographic characteristics, some differences between high spillover and low spillover zones exist which may directly affect mental health outcomes, such as the type of employment pre-crisis and whether the respondent was employed. As such, we will control for as many of these differences as possible to ensure that differences in mental health outcomes between high spillover and low spillover zones can be attributed to the refugee influx, rather than potentially omitted variables. Since all of the variables in Table 2 were determined prior to the crisis and are therefore unaffected by the August 2017 refugee influx, we can control for these characteristics when conducting inter-group comparisons without introducing bias into the regression model.

3.3 *Main Outcome Variables*

For my central analysis, I compare three sets of mental health outcomes: (1) symptoms of depression; (2) exposure to trauma; and (3) symptoms of trauma. Each of these outcomes is calculated as an equally-weighted linear sum of component indicators. In composing these indexes, I utilize the approach of Kling, Liebman, and Katz (2007) whereby variables are transformed into z-scores. These z-scores are calculated by subtracting the control group mean (those who live far from the camps) and dividing by the control group standard deviation. As such, each component of the index has mean 0 and standard deviation 1 for the control group. There are nine indicators for symptoms of depression, twelve indicators for exposure to trauma, and sixteen indicators for symptoms of trauma. Each indicator takes on values between zero and three, after converting categorical responses to ordinal values. As such, adding up indicators for each outcome variable yields potential individual ranges of 27, 36, and 48, for symptoms of depression, exposure to trauma, and symptoms of trauma, respectively. In all cases, higher values correspond to worse mental health.

To measure symptoms of depression, the nine-item Patient Health Questionnaire (PHQ-9) is used, outlined in Table 3. This is a standardized screening tool that assesses mental and emotional health disorders. If an individual scores between a 0 and 4, they are diagnosed with minimal depression; 5 to 9 is mild depression; 10 to 14 is moderate depression; 15 to 19 is moderately severe depression; and 20 to 27 is severe depression. Physicians are instructed that scores greater than 14 warrant treatment for depression, including the prescription of antidepressants and referrals for psychotherapy or counselling.

Table 3. Symptoms of Depression Items in Patient Health Questionnaire (PHQ-9)

Over the LAST TWO WEEKS, how often have you been bothered by the following problems?
1. Little interest or pleasure in doing things
2. Feeling down, depressed, or hopeless
3. Trouble falling or staying asleep, or sleeping too much
4. Feeling tired or having little energy
5. Poor appetite or overeating
6. Feeling bad about yourself, that you are a failure or have let yourself or family down
7. Trouble concentrating on things, such as watching television
8. Moving or speaking so slowly that other people could have noticed. Or the opposite | being so fidgety or restless that you have been moving around a lot more than usual
9. Thoughts that you would be better off dead or of hurting yourself in some way

Notes: Respondents could indicate "not at all" encoded as 0, "sometimes" encoded as 1, "more than half of the days" encoded as 2, and "nearly every day" encoded as 3.

To measure trauma, an adapted version of the Harvard Trauma Questionnaire is used. The first section asks whether a respondent has heard about, witnessed, or experienced a series of traumatic events; the second section asks about the extent of symptoms associated with any traumatic experiences for the respondent. The second section is only asked if the respondent reports at least one traumatic event in the previous section. Tables 4 and 5 provide an outline of these two sections of the Harvard Trauma Questionnaire.

Table 4. Exposure to Trauma Items in Harvard Trauma Questionnaire

Please indicate whether you have experienced, witnessed, or heard any of the followed events.
1. Imprisonment
2. Serious injury
3. Combat situation
4. Rape or sexual abuse
5. Forced isolation from others
6. Being close to death
7. Forced separation from family members
8. Murder of family or friend
9. Unnatural death of family or friend
10. Murder of stranger or strangers
11. Lost or kidnapped
12. Torture

Notes: Respondents could indicate "not" encoded as 0, "heard about it" encoded as 1, "witnessed" encoded as 2, and "experienced" encoded as 3. In answering these questions, respondents were given the following instructions: "We would like to ask you questions about your past history and present symptoms. This information will be used to help us provide you with better medical care. However, you may find some questions upsetting. If so, please feel free not to answer. This will certainly not affect your treatment. The answer to the questions will be kept confidential."

Table 5. Symptoms of Trauma Items in Harvard Trauma Questionnaire

The following are symptoms that people sometimes have after experiencing hurtful or terrifying events in their lives. Please listen to each one carefully and decide how much the symptoms bothered you in the PAST WEEK.
1. Recurrent thoughts or memories of the most hurtful or terrifying events
2. Recurrent nightmares
3. Feeling detached or withdrawn from people
4. Unable to feel emotions
5. Feeling irritable or having outbursts of anger
6. Not wanting to interact with others outside the household
7. Feeling as if you don't have a future
8. Having difficulty dealing with new situations
9. Troubled by physical problem(s)
10. Feeling unable to make daily plans
11. Feeling that people do not understand what happened to you
12. Feeling others are hostile to you
13. Feeling that you have no one to rely upon
14. Feeling no trust in others
15. Feeling powerless to help others
16. Spending time thinking why these events happened to you

Notes: Respondents could indicate "not at all" encoded as 0, "a little" encoded as 1, "quite a bit" encoded as 2, and "extremely" encoded as 3.

Crucially, while the symptoms of depression and symptoms of trauma indicators are measured using a scale that is increasing in symptom frequency, the exposure to trauma indicators use a scale that is increasing in severity of trauma exposure. This is inherent in the structure of this question in the Harvard Trauma Questionnaire, wherein people are asked whether they have "heard of," "witnessed," or "experienced" each one of the twelve indicators. As such, the objective evaluation of this variable is in contrast to the subjective evaluation of symptom frequency, meaning that it should not be reference dependent. Moreover, although those living close to the refugee camps may not have experienced the traumas that refugees experience, they might be more likely to have heard about them or witnessed them as a result of interactions with the Rohingya refugees living in nearby camps.

3.4 Estimation Strategy

To compare these sets of mental health outcomes, I run the following regressions:

$$SymptomsOfDepression_i = \alpha_1 + \beta_1 CloseToCamp_i + X_i + \epsilon_i$$
$$ExposureToTrauma_i = \alpha_2 + \beta_2 CloseToCamp_i + X_i + \epsilon_i$$
$$SymptomsOfTrauma_i = \alpha_3 + \beta_3 CloseToCamp_i + X_i + \epsilon_i$$

where the dependent variables denote mental health outcomes of interest for individual i, $CloseToCamp_i$ is an indicator for whether the host country respondent i lives close to the nearest refugee camp, X_i is a vector of control variables, and ε_i is an error term for individual i.

The parameters of interest are β_1, β_2, and β_3. As robustness checks, I run a series of specifications that adjust the set of controls used in each regression. This includes controlling for baseline demographic characteristics, a squared term for age to account for any potential non-linear relationship with the mental health outcomes, pre-crisis controls for income and type of occupational work, along with a log-linearized income variable that helps account for any potential non-linear relationships.

Additionaly, I would expect to find bigger differences between low spillover and high spillover zones for indicators in which refugees are a more salient or relevant reference group. As such, I will plot the sub-indicators and evaluate whether the effects on the index are driven by indicators that most relate to the reference dependence channel. Then, I will examine overall health outcomes to explore alternative channels for any demonstrated effects arising through differential healthcare improvements between the groups of host households. In doing so, the following regression specification will be run:

$$PerceptionOfHealth_i = \alpha_4 + \beta_4 CloseToCamp_i + ActualHealth_i + ChangeHealthcare_i + X_i + \epsilon_i$$

where the dependent variable denotes self-reported overall health for individual i, $CloseToCamp_i$ is an indicator for whether the host country respondent i lives close to the nearest refugee camp, $ActualHealth_i$ is an illness avoidance indicator proxying for the actual health of individual i, $ChangeHealthcare_i$ is an ordinal valued variable assessing whether healthcare access has worsened, stayed the same, or improved relative to before July 2017, X_i is a vector of control variables, and ϵ_i is an error term for individual i.

The parameter of interest in this regression is β_4. I will examine whether the results are robust to the same set of controls as was used in the mental health analysis. Furthermore, non-parametric results will be shown to account for variability in the construction of the distance variable.

4. EMPIRICAL RESULTS

We now turn to the main analysis of the effects of the refugee influx. In doing so, I focus on the subsample of host country respondents.

4.1 Mental Health Findings

We begin in Table 6 by estimating a series of linear regression models that quantify the relationship between living close to the refugee camps (i.e. in high spillover areas) when compared to living far from the refugee camps (i.e. in low spillover areas). In addition, Table 7 shows the coefficients on covariates, along with calculating z-scores in an alternate fashion, whereby rather than subtracting the control group mean and dividing by the control group standard deviation, the sample mean and standard deviation are used. For the purposes of interpretation, Table 6 is discussed in depth.

Focusing on Panel A, we can see that living close to the refugee camps is associated with a 0.14 standard deviation increase in exposure to trauma. Yet, those living in these high spillover zones report a 0.14 standard deviation reduction in symptoms of trauma, relative to those in the control group. This higher self-reported mental health for those in the control group is also reflected in the symptoms of depression coefficient, whereby those living close to the refugee camps report a 0.07 standard deviation reduction in symptoms of depression. While the former results are statistically significant at the 1 percent level, the latter holds true at the 5 percent level.

Table 6. Effect of Distance to Refugee Camps on Host Respondent Mental Health

	Panel A			Panel B			Panel C			Panel D		
	(1) Symptoms of Depression (Std. Dev.)	(2) Exposure to Trauma (Std. Dev.)	(3) Symptoms of Trauma (Std. Dev.)	(4) Symptoms of Depression (Std. Dev.)	(5) Exposure to Trauma (Std. Dev.)	(6) Symptoms of Trauma (Std. Dev.)	(7) Symptoms of Depression (Std. Dev.)	(8) Exposure to Trauma (Std. Dev.)	(9) Symptoms of Trauma (Std. Dev.)	(10) Symptoms of Depression (Std. Dev.)	(11) Exposure to Trauma (Std. Dev.)	(12) Symptoms of Trauma (Std. Dev.)
Live Close to Refugee Camp	-0.07**	0.14***	-0.14***	-0.08***	0.14***	-0.17***	-0.08***	0.13***	-0.17***	-0.04	0.20***	-0.13***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Constant	-0.00	0.00	-0.00	-0.58***	-0.28***	-0.34***	-0.59***	-0.28***	-0.34***	-0.48**	-0.33*	-0.34*
	(0.02)	(0.02)	(0.02)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.20)	(0.18)	(0.18)
Individual Demographic Controls?	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Income Controls?	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Occupation Controls?	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Observations	4,758	4,758	4,758	4,745	4,745	4,745	4,745	4,745	4,745	2,015	2,015	2,015

Notes: Dependent variables are transformed into z-scores by subtracting the control group mean and dividing by the control group standard deviation. The independent variable is a dummy which takes on a value of 1 if the respondent lives no farther than 15 kilometers (a 3-hours walking distance) away from the refugee camps and 0 otherwise. Individual demographic controls include age, gender, country of birth, religion, ethnicity, marital status, and reading ability. Pre-crisis income controls include annual total pre-crisis income and annual log total pre-crisis income. Pre-crisis occupation controls include an indicator for pre-crisis employment in agriculture and pre-crisis paid work. Robust standard errors in parentheses. Significance levels denotes by *** p < 0:01, ** p < 0:05, * p < 0:1.

Table 7. Effect of Distance to Refugee Camps on Host Respondent Mental Health [Extended]

	Panel A			Panel B			Panel C			Panel D		
	(1) Symptoms of Depression (Std. Dev.)	(2) Exposure to Trauma (Std. Dev.)	(3) Symptoms of Trauma (Std. Dev.)	(4) Symptoms of Depression (Std. Dev.)	(5) Exposure to Trauma (Std. Dev.)	(6) Symptoms of Trauma (Std. Dev.)	(7) Symptoms of Depression (Std. Dev.)	(8) Exposure to Trauma (Std. Dev.)	(9) Symptoms of Trauma (Std. Dev.)	(10) Symptoms of Depression (Std. Dev.)	(11) Exposure to Trauma (Std. Dev.)	(12) Symptoms of Trauma (Std. Dev.)
Live Close to Refugee Camp	-0.07**	0.15***	-0.15***	-0.09***	0.15***	-0.18***	-0.08***	0.14***	-0.18***	-0.04	0.22***	-0.14***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)
Age				0.02***	0.02***	0.02***	0.02***	0.02***	0.02***	0.01	0.02**	0.01
				(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Age Squared				-0.00	-0.00***	-0.00***	-0.00	-0.00***	-0.00**	0.00	-0.00**	-0.00
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female				0.18***	-0.26***	0.05	0.16***	-0.21***	0.06*	0.32***	-0.15***	0.22***
				(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)
Born, Myanmar				-0.21	-0.09	-0.29	-0.21	-0.09	-0.29	-0.35	-0.27	-0.46*
				(0.18)	(0.22)	(0.22)	(0.18)	(0.22)	(0.22)	(0.22)	(0.30)	(0.26)
Born, Other				-0.32	-0.46**	-0.25	-0.32	-0.46**	-0.25	0.06	-0.83**	0.13
				(0.27)	(0.23)	(0.23)	(0.27)	(0.23)	(0.23)	(0.56)	(0.39)	(0.19)
Religion, Hinduism				-0.00	-0.11	0.04	0.00	-0.11	0.04	-0.12	-0.11	-0.11
				(0.08)	(0.09)	(0.09)	(0.08)	(0.09)	(0.09)	(0.12)	(0.13)	(0.13)
Religion, Buddhism				-0.08	-0.09	-0.02	-0.08	-0.10	-0.02	-0.15	-0.26***	-0.12
				(0.06)	(0.06)	(0.08)	(0.06)	(0.06)	(0.08)	(0.10)	(0.10)	(0.13)
Ethnicity				0.16	0.12	0.30	0.15	0.12	0.30	0.28	0.27	0.35
				(0.16)	(0.18)	(0.21)	(0.16)	(0.17)	(0.21)	(0.20)	(0.24)	(0.27)
Marital Status, Married				0.12**	0.11**	0.14***	0.13***	0.09*	0.13***	0.03	-0.03	0.04
				(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.09)	(0.09)	(0.09)
Marital Status, Widowed				0.47***	0.21**	0.46***	0.48***	0.18**	0.45***	0.46***	-0.03	0.48***
				(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.15)	(0.15)	(0.15)
Marital Status, Divorced				0.88***	0.82***	1.20***	0.90***	0.79***	1.19***	0.55*	0.55**	1.13***
				(0.20)	(0.17)	(0.19)	(0.20)	(0.17)	(0.19)	(0.33)	(0.25)	(0.29)
Marital Status, Separated				0.68***	0.52***	0.85***	0.71***	0.46***	0.83***	0.63***	0.23	0.90***
				(0.16)	(0.16)	(0.17)	(0.16)	(0.17)	(0.17)	(0.18)	(0.21)	(0.22)
Can Read				-0.21***	-0.04	-0.25***	-0.21***	-0.04	-0.25***	-0.15***	-0.05	-0.19***
				(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.05)	(0.05)
Annual Total Income, Pre-Crisis							-0.00	-0.00	-0.00***	-0.00	-0.00	-0.00***
							(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Annual Log Total Income, Pre-Crisis							-0.00	0.01***	0.00	-0.00	0.01*	0.00
							(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Employed in Agriculture, Pre-Crisis										-0.09*	-0.04	-0.05
										(0.05)	(0.05)	(0.05)
Employed in Wage Work, Pre-Crisis										0.07	0.04	0.05
										(0.05)	(0.05)	(0.05)
Constant	0.04*	-0.08***	0.08***	-0.57***	-0.38***	-0.29***	-0.58***	-0.37***	-0.29***	-0.47**	-0.43**	-0.29
	(0.02)	(0.02)	(0.02)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.21)	(0.20)	(0.20)
Individual Demographic Controls?	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Income Controls?	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Occupation Controls?	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Observations	4,758	4,758	4,758	4,745	4,745	4,745	4,745	4,745	4,745	2,015	2,015	2,015

Notes: Dependent variables are transformed into z-scores by subtracting the sample mean and dividing by the sample standard deviation. The independent variable is a dummy which takes on a value of 1 if the respondent lives no farther than 15 kilometers (a 3-hours walking distance) away from the refugee camps and 0 otherwise. Individual demographic controls include age, gender, country of birth, religion, ethnicity, marital status, and reading ability. Pre-crisis income controls include annual total pre-crisis income and annual log total pre-crisis income. Pre-crisis occupation controls include an indicator for pre-crisis employment in agriculture and pre-crisis paid work. Robust standard errors in parentheses. Significance levels denotes by *** p < 0:01, ** p < 0:05, * p < 0:1.

When including a series of individual-level demographic controls (Panel B), the exposure to trauma coefficient remains relatively consistent, whilst the symptoms of trauma and symptoms of depression coefficients become more negative. When examining the covariates found in Table 7, we find negative coefficients on an indicator for literacy (namely, the ability to read), as it relates to symptoms of trauma and symptoms of depression, but not exposure to trauma. Intuitively, this makes sense given that the channel between literacy and exposure to trauma is unclear. In addition, we know from Table 2 that those living close to the refugee camp have reduced literacy on average. As such, part of the reduction in the coefficients of interest in columns (4) and (6) can be explained by the inclusion of a control for literacy. In addition, we find that our coefficient on symptoms of depression has increased in statistical significance, from the 5 percent level in column (1) to the 1 percent level in column (4).

Panel C presents the results for similar regression specifications, which now include controls for individual income prior to the refugee influx in August of 2017. I control for both total annual income from July 2016 to July 2017 and a log-linearized variable capturing total annual income during the same period. To preserve rank-order and avoid missing values for individuals with no reported annual income in the pre-crisis period, I add one prior to performing this transformation. Ultimately, including these controls does not largely alter the coefficients of interest from the previous specifications. There continues to be a positive association between living close to a refugee camp and exposure to trauma, but a negative association with symptoms of depression and symptoms of trauma, statistically significant at the 1 percent level.

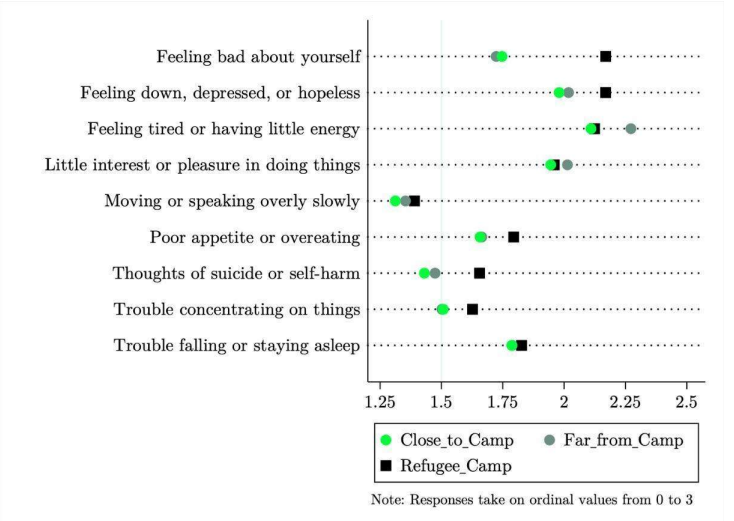
Finally, I include an indicator for whether the individual worked in the agricultural sector prior to the refugee influx, along with an indicator for whether the individual worked for wages prior to the refugee influx, presenting these results in Panel D. In column (11), we can see an increase in the coefficient of interest to now reflect a 0.20 standard deviation increase in exposure to trauma associated with living close to the camps, relative to living far from the camps. By contrast, the coefficients in column (10) and (12) are attenuated, with the symptoms of depression coefficient no longer maintaining statistical significance, even at the 10 percent level. When examining the coefficients on these covariates in Table 7, we can see that employment in agriculture has a negative association with symptoms of depression, whereas being employed in wage work has a positive association with symptoms of depression. The former result is statistically significant at the 10 percent level. We should also recall the results from Table 2 that demonstrate that those living close to

the camps were more likely to have been agricultural labourers and less likely to have worked for wages provided by a company or the government. Taken together, these seem to explain the attenuation in the coefficient in column (10).

When examining the coefficient of interest, we can see that despite experiencing greater trauma, individuals living closer to the refugee camps have lower self-reported levels of depression and life dissatisfaction (Panel A). These results are statistically significant at the 1 percent level and robust to the addition of individual demographic controls (Panel B) and pre-crisis income controls (Panel C). If we also include pre-crisis occupation controls (Panel D), similar results hold for exposure to trauma and life dissatisfaction, although the coefficient of interest on the symptoms of depression outcome goes from being statistically significant at the 1 percent level to being statistically significant at the 10 percent level. However, this last specification only includes observations of individuals in the host country who were employed during the year leading up to July 2017.

Additionally, to gain a richer appreciation for what sub-indicators may be driving the effects in each of these three categories of mental health, I produce a series of dot plots seen in Figures 8, Figure 9 and Figure 10 below.

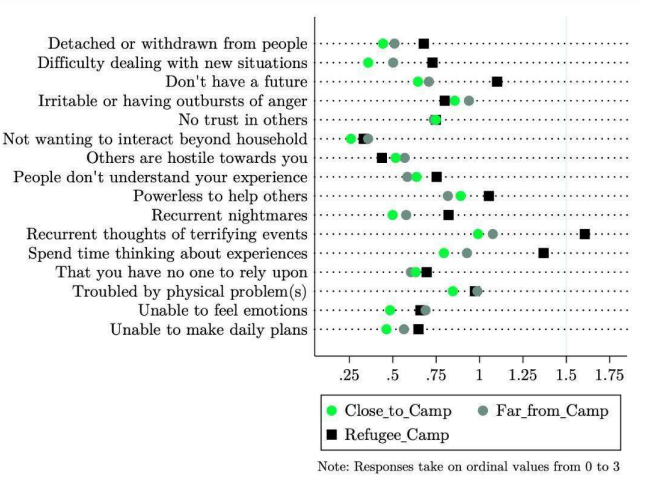
Figure 8. Mean Symptoms of Depression Score



Beginning with the symptoms of depression indicators, we can see that the light green and dark green circles are close for most measures, although the former is generally lower on the scale from 0 to 3. Notable exceptions include “feeling bad about yourself" and

"trouble concentrating on things," which may not elicit comparisons to the refugees when compared to items such as “poor appetite or overeating" or “thoughts of suicide or self-harm." In fact, the largest gap between the two points is related to the indicator of “feeling tired or having little energy," which may readily invoke comparisons with the refugees when self-evaluating outcomes. Crucially, it is unclear whether these judgements are explicit or implicit, but neither of the two hypotheses necessitate an explicit comparison. In other words, it might be that people living close to the camps, and interacting with the refugees at a greater frequency, are comparing their outcomes to those of the refugees without even consciously knowing that they are doing so.

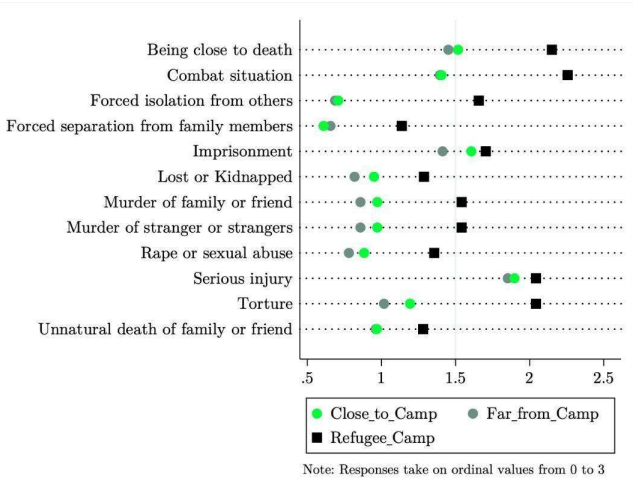
Figure 9. Mean Symptoms of Trauma Score



Note: Responses take on ordinal values from 0 to 3

Now we turn to the symptoms of trauma indicators. Households living close to the refugee camps report lower instances of being “unable to feel emotions," have less “difficulty dealing with new situations," and do not experience “little interest or pleasure in doing things" relative to low spillover households. They also spend less time “thinking why these events happened [to them]," are less prone to “outbursts of anger" or irritability, and are less “troubled by physical problem(s)" than their low spillover counterparts. That is not to say that these households are in an improved position to act upon their purported greater empathy for others – they report greater incidences of feeling “powerless to help others," despite having more “energy" than low spillover households. This feeling of powerlessness may be related to the outsized share of refugees in relation to natives in districts which host refugees.

Figure 10. Mean Exposure to Trauma Score



When examining the exposure to trauma outcomes in which differences between those living near the camps and those far away are the largest, we find further evidence for the social relativism theory of reference dependence hypothesis. Notably, large gaps between the light green and dark green circles suggest that not only was distance an effective proxy for increased refugee interaction, but that living closer to the refugee camps was associated with increased exposure to trauma, on average. In fact, households closer to the refugee camps report higher incidences of witnessing or experiencing traumas in 11 out of the 12 indicators, with the sole exception of "forced separation from family members." These differences are largest for more extreme incidences of trauma, including torture, rape, murder, and imprisonment. The fact that exposure to trauma is worse on average for households close to refugee camps, combined with the fact that refugees experience all this trauma in a much higher proportion to communities at a distance, suggests that individuals living close to refugee camps may witness or hear about these traumas being perpetrated in association with the Rohingya refugees.

4.2 Overall Health Findings

Finally, we can extend our analysis beyond mental health to also include overall health. This can be seen in Table 7.

For this set of multivariate regressions, the outcome variable of perceived overall health is a subjective, self-assessed measure in which respondents rate their own health on a 5-point scale from 'very bad' to 'very good.' In column (1), we can see that those living close to

Table 7. Effect of Distance to Refugee Camps on Host Respondent Overall Health

	(1) Perceived Overall Health	(2) Perceived Overall Health	(3) Perceived Overall Health	(4) Perceived Overall Health	(5) Perceived Overall Health	(6) Perceived Overall Health
Live Close to Refugee Camp	0.13***	0.14***	0.13***	0.13***	0.11***	0.10**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Actual Overall Health		0.55***	0.44***	0.44***	0.51***	0.52***
		(0.02)	(0.02)	(0.02)	(0.04)	(0.04)
Change Healthcare Access						0.10***
						(0.04)
Constant	3.17***	2.97***	3.54***	3.55***	3.72***	3.49***
	(0.02)	(0.02)	(0.09)	(0.09)	(0.17)	(0.20)
Individual Demographic Controls?	No	No	Yes	Yes	Yes	Yes
Pre-Crisis Income Controls?	No	No	No	Yes	Yes	Yes
Pre-Crisis Occupation Controls?	No	No	No	No	Yes	Yes
Change Healthcare Access Controls?	No	No	No	No	No	Yes
Actual Overall Health Controls?	No	Yes	Yes	Yes	Yes	Yes
Observations	4,758	4,757	4,744	4,744	2,014	1,869

Notes: Dependent variable of 'Perceived Overall Health' is the response to the question "In general, would you say that your health is [...]?" Ordinal values between 1 and 5 are assigned for responses of 'very bad', 'bad', 'regular', 'good' and 'very good', respectively. The independent variable is a dummy which takes on a value of 1 if the respondent lives no farther than 15km (a 3-hour walking distance) away from the refugee camps and 0 otherwise. The dummy variable 'Actual Overall Health' acts as an illness avoidance indicator and records a response of 'yes' as 0 and 'no' as 1 to the question "During the past four weeks, have you suffered from an illness or injury?" The variable 'Change Healthcare Access' records a response of 'worsened' as 1, 'no change' as 2, and 'improved' as 3 to the question "Compared to before July 2017, has your healthcare improved or worsened?" Individual demographic controls include age, gender, country of birth, religion, ethnicity, marital status, and reading ability. Pre-crisis income controls include annual total pre-crisis income and annual log total pre-crisis income. Pre-crisis occupation controls include an indicator for pre-crisis employment in agriculture and pre-crisis paid work. Robust standard errors in parentheses. Significance levels are denoted by *** p < 0:01, ** p < 0:05, * p < 0:1.

the camps report an average 0.13 increase in their perceived overall health on the 5-point scale, relative to those living far from the camps. This translates to a 3.25 percentage point increase and is statistically significant at the 1 percent level. However, it might be that these differences in perceived health are a result of actual differences in the health of those that live close to the camps relative to living far from the camps. As such, I include a control for a proxy of actual overall health – namely, whether the respondent has suffered from an illness or injury over the past four weeks. Presumably, those who are able to avoid illness or injury can be deemed to have better overall health. When adding in this control for actual overall health, the coefficient of interest increases – explained by the fact that those who live close to the camps have a higher mean for the injury avoidance indicator and avoiding injury is positively associated with perceived overall health. In fact, according to the regression specification in column (2), having avoided injury or illness over the past 4 weeks is associated with a 13.75 percentage point increase in perceived overall health, statistically significant at the 1 percent level. Although both the coefficient on living close to the refugee camp and actual overall health are attenuated when adding in a suite of controls, including individual demographic controls, pre-crisis income controls, and pre-crisis occupation controls, both results remain statistically significant at the 1 percent level.

Yet still, it might be that those in high spillover areas saw an improvement in healthcare access, perhaps due to an influx of NGO support to districts near the refugee camps, which could explain these differential effects. As such, I include a variable that captures whether

access to healthcare has worsened, stayed the same, or improved compared to before July 2017. Although this variable is self-reported, rather than collected from hospitals or healthcare facilities, it can certainly provide insight into whether differential healthcare access could explain the results. In column (6), we see that improved healthcare access is associated with higher perceived overall health, as expected. This result is statistically significant at the 1 percent level. While the coefficient of interest is attenuated, it remains statistically significant now at the 5 percent level. Yet still, those households living close to the refugee camps report a 2.5 percentage point increase in perceived overall health, plausibly due to base group adjustments. Notably, even after controlling for a suite of baseline demographic characteristics, including income and occupation controls, along with actual overall health and change in healthcare access, the coefficient of interest only attenuates from 0.13 to 0.10. In our examination of broader health outcomes, we find evidence for the reference-dependence channel.

4.3 Robustness Checks

4.3.1 Non-Parametric Analysis

In addition to extending my analysis beyond mental health to consider overall health, I also display all results in a non-parametric manner. This helps to determine whether variability in the construction of the distance variable could be explaining the results. To do this, I utilize the binscatter package in Stata and produce Figures 11 to 14 below. Note that binned categories of 1, 2, 3 correspond to groupings of households 10km or within, between 15km and 55km inclusive, and 60km or further, respectively.

Figure 11. Non-Parametric Results: Symptoms of Depression

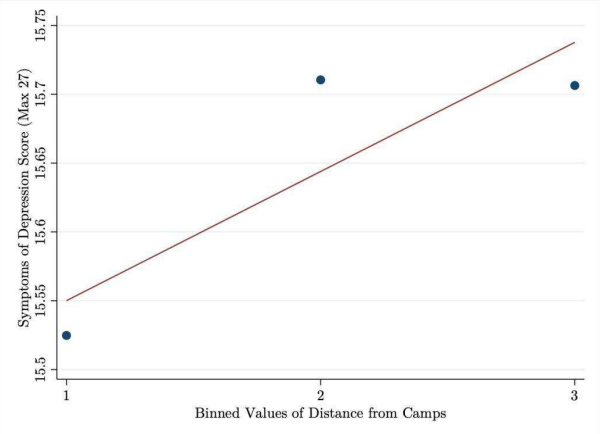


Figure 12. Non-Parametric Results: Exposure to Trauma

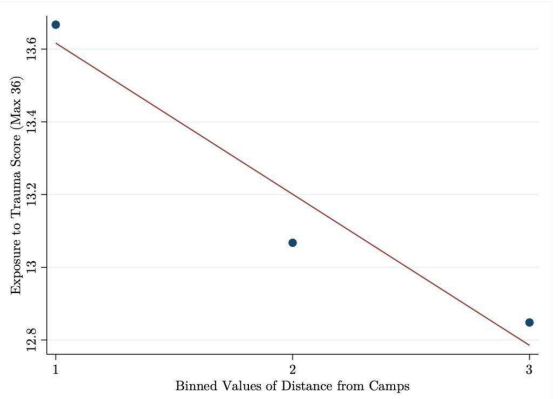


Figure 13. Non-Parametric Results: Symptoms of Trauma

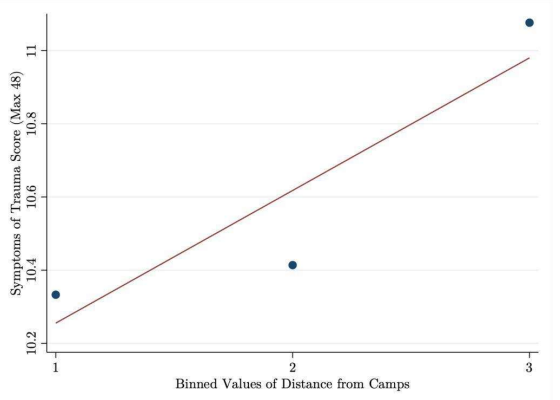
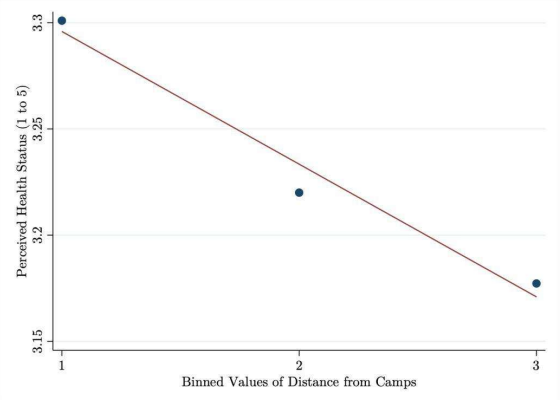


Figure 14. Non-Parametric Results: Perceived Overall Health



In all figures, the relationship between the outcome variable and the distance variable is indicative of a linear relationship. That is, the middle dot is driving up an increase in the outcome variable, suggesting that the earlier results are robust to different constructions of the dependent variable. To comment briefly, there appears to be a larger jump between the first bin and the second bin in Figure 11, suggesting that the 3-hour walking distance cut-off may be a good point to select when sorting host households into high spillover and low spillover households. In addition, Figure 13 is notable in that there is a larger jump between the second and third bins when compared to the first and second bins. Given that this is a subjective, self-reported outcome, and that the more objectively reported Figure 12 shows the opposite effect, we can have confidence in the selection of the cut-off. Finally, the results in Figure 14 seem to suggest that the relationship between distance and perceived health status is largely linear. The fact that the second bin produces a value in between the first and third bins is helpful in suggesting that variability in independent variable construction is not responsible for our findings.

4.3.2 Mental Health and Change in Healthcare Access Control

Although the question of change in healthcare access was asked in association to overall health in the CBPS, we can include this crucial control into the mental health specification to examine whether change in access to healthcare might explain differential mental health outcomes between high spillover and low spillover host respondents. This is a crucial control in so far as it offers an alternative explanation for the main mental health results to the reference dependence hypothesis that seems most likely thus far. This analysis is presented in Table 8.

Examining the regression results, the coefficients remain similar to that of the main analysis. In fact, the coefficients are further from 0, since having an improvement in health-care access has a negative association with symptoms of depression, exposure to trauma, and symptoms of trauma. Since respondents who saw an improvement in healthcare access might have improved resources to overcome their symptoms of depression, exposure to trauma, and symptoms of trauma, we would expect this negative association. As such, the direction of this coefficient is as expected and remains statistically significant at the 1 percent level across all specifications. Particularly in Panel D, the standard errors see a sizeable increase, partly due to the limited sample size when controlling for pre-crisis occupation controls, for which the sample is limited to those who were employed in 2017.

Table 8. Effect of Distance to Refugee Camps on Host Respondent Mental Health (Inc. Control for Change in Healthcare Access)

	Panel A			Panel B			Panel C			Panel D		
	(1) Symptoms of Depression (Std. Dev.)	(2) Exposure to Trauma (Std. Dev.)	(3) Symptoms of Trauma (Std. Dev.)	(4) Symptoms of Depression (Std. Dev.)	(5) Exposure to Trauma (Std. Dev.)	(6) Symptoms of Trauma (Std. Dev.)	(7) Symptoms of Depression (Std. Dev.)	(8) Exposure to Trauma (Std. Dev.)	(9) Symptoms of Trauma (Std. Dev.)	(10) Symptoms of Depression (Std. Dev.)	(11) Exposure to Trauma (Std. Dev.)	(12) Symptoms of Trauma (Std. Dev.)
Live Close to Refugee Camp	-0.09***	0.12***	-0.15***	-0.10***	0.11***	-0.18***	-0.11***	0.11***	-0.18***	-0.06	0.17***	-0.15***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)
Change Healthcare Access	-0.21***	-0.28***	-0.20***	-0.20***	-0.28***	-0.20***	-0.20***	-0.28***	-0.20***	-0.22***	-0.24***	-0.21***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)	(0.04)
Constant	0.57***	0.79***	0.55***	0.01	0.52***	0.25**	0.00	0.53***	0.25**	0.16	0.42*	0.25
	(0.06)	(0.08)	(0.06)	(0.12)	(0.13)	(0.12)	(0.12)	(0.13)	(0.12)	(0.22)	(0.21)	(0.21)
Change Healthcare Access Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Demographic Controls?	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Income Controls?	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Occupation Controls?	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Observations	4,398	4,398	4,398	4,385	4,385	4,385	4,385	4,385	4,385	1,870	1,870	1,870

Notes: Dependent variables are transformed into z-scores by subtracting the control group mean and dividing by the control group standard deviation. The independent variable is a dummy which takes on a value of 1 if the respondent lives no farther than 15 kilometers (a 3-hours walking distance) away from the refugee camps and 0 otherwise. The variable 'Change Healthcare Access' records a response of 'worsened' as 1, 'no change' as 2, and 'improved' as 3 to the question "Compared to before July 2017, has your healthcare improved or worsened?" Individual demographic controls include age, gender, country of birth, religion, ethnicity, marital status, and reading ability. Pre-crisis income controls include annual total pre-crisis income and annual log total pre-crisis income. Pre-crisis occupation controls include an indicator for pre-crisis employment in agriculture and pre-crisis paid work. Robust standard errors in parentheses. Significance levels denotes by *** p < 0:01, ** p < 0:05, * p < 0:1.

Ultimately, this demonstrates that the main results are robust to the addition of a control for the change in healthcare access. This is particularly the case for the exposure to trauma and symptoms of trauma outcome variables, which remain statistically significant at the 1 percent level in all specifications.

4.3.3 Addition of Post-Crisis Employment and Earnings Controls

This robustness check utilizes additional questions related to earnings and employment that were asked to respondents of the CBPS. In understanding whether reference dependence is really at play in this setting, we need to rule out alternative channels by which those living close to camps may have lower symptoms of depression and lower symptoms of trauma. The most apparent of these channels is related to potential differential material impacts associated with the refugee influx. For example, it might be that those living closer to the camps saw a disproportionate increase in their work status compared to those living far from the camps. If work status is negatively associated with symptoms of depression and symptoms of trauma, then our findings might be driven by this omitted variable.

Table 9 helps us understand whether this might be the case, along with presenting the mean and standard deviation for those living in the refugee camps for a comparison. We can immediately notice that employment does not see a substantial differential effect for those in high spillover versus low spillover zones: 38 percent of those in low spillover areas and 46 percent of those in high spillover areas are employed by a business or the government, before and after the refugee influx. While three quarters of those in low spillover areas are still employed at the same place as in 2017, only 65 percent of those in high spillover areas work at the same job. Taken together, this implies that those in high spillover areas were able to find alternative jobs and maintain similar levels of employment when compared to those in low spillover areas. In addition, the average weekly hours worked at the 2019 job is comparable between the groups, but slightly higher for those in low spillover zones at roughly 40 hours versus roughly 36 hours for those in high spillover areas, on average. In addition, both groups saw a comparable decrease in employment in agriculture from 2017 to 2019, perhaps due to displacement of labour from host country residents to the refugees who may have been willing to work for a lower wage. This may be supported by the fact that an average of 11 percent of refugees were employed in Bangladeshi agriculture in 2019.

So far, I have discussed employment outcomes and attributes related to the labour market conditions of host respondents. Turning now to earnings outcomes, I find that both low spillover and high

spillover areas sawa decrease in annual income from 2017 to 2019. By contrast, using a log linearized version of annual income, after adding 1 to all observations to avoid dropping individuals with 0 employment income, we see a different story. When the impact of outliers on the average annual income is suppressed through this transformation, it becomes clear that annual log income increased for both groups. This suggests that on average, incomes for both those close to the camps and those far from the camps increased from 2017 to 2019, despite the negative rhetoric of refugees decreasing earnings by taking the jobs of host country residents.

Table 9. Employment and Earnings Summary Statistics

	Low Spillover Areas		High Spillover Areas		Refugee Camps	
	(1) Mean	(2) Std. Dev.	(3) Mean	(4) Std. Dev.	(5) Mean	(6) Std. Dev.
Work Status in 2017?	0.38	0.49	0.46	0.50	0.48	0.50
Still Employed at 2017 Job?	0.75	0.43	0.65	0.48	0.22	0.42
Work Status in 2019?	0.38	0.49	0.46	0.50	0.21	0.41
Employed in Agriculture 2017?	0.30	0.46	0.41	0.49	0.54	0.50
Employed in Agriculture 2019?	0.26	0.44	0.36	0.48	0.11	0.31
Annual Income 2017?	86,665	1,797,877	37,831	410,603	223,847	979,296
Annual Income 2019?	55,342	619,331	32,259	80,619	7,231	45,601
Annual Log Income 2017?	3.42	5.00	4.12	5.14	4.98	5.84
Annual Log Income 2019?	4.33	5.27	4.92	5.24	2.99	4.36
Average Weekly Hours Worked at Job (2019)?	40.95	24.90	35.94	22.72	30.95	20.50
Job has a Written Contract (2019)?	0.10	0.30	0.12	0.33	0.07	0.25
Interested in Having a Job or Starting a Business?	0.23	0.42	0.28	0.45	0.29	0.45
Searched for a Job or Attempted to Start a Business?	0.19	0.39	0.18	0.39	0.38	0.48

Notes: Columns (1), (3), and (5) represent means for individuals living in low spillover, high spillover, and refugee camp areas, respectively. Columns (2), (4), and (5) present the associated standard deviations for these means. As it relates to variable definitions, work status in 2017 is a self-reported binary employment indicator and work status in 2019 was based on whether or not the individual worked for remuneration for at least one hour during the past 7 days. These variables, along with whether the individual is still employed at their 2017 job, whether the individual was employed in agriculture in 2017 and 2019, whether their job has a written contract in 2019, and whether they are interested in or have started searching for a job or starting a business, are dummy variables with "yes" coded as 1 and "no" coded as 0. The variable of average weekly hours worked at job in 2019 is a self-reported number based on the past 4 weeks and takes on values no smaller than 1 and no larger than 100. Pre- and post-crisis annual income (or annual income in 2017 and 2019) are continuous variables defined in terms of Bangladeshi Taka. Values of one are added to these annual income variables when log linearizing the data, so as to preserve rank while not generating excess missing observations. These variables of pre- and post-crisis log annual income take on values no smaller than 0 and no larger than 20.

While this paper focuses on those in the host community, these descriptive statistics offer insights regarding the refugee experience. Although the employment rate for refugees in the sample was higher than that of host communities, being nearly 50 percent in 2017, this fell dramatically following their forced displacement to an average of 21 percent in 2019. Further, the average weekly hours worked at the 2019 job is 5 percentage points lower than those in high spillover areas and 10 percentage points lower than those in low spillover areas. In other words, even those refugees who were able to find employment in Bangladesh are working fewer hours per week compared to host community residents. In contrast to the findings on host earnings, those in the refugee camps saw both annual income and log annual income fall from 2017 to 2019, as we might expect based on their employment outcomes. Furthermore, the jobs worked by refugees in Bangladesh were on average half as likely to have a written contract as those jobs worked by the host respondents. While we might think that employment discrepancies between host and refugee respondents may be due to a decreased desire or motivation to have a job or start a business, the data suggests otherwise. In reality, 29 percent of those unemployed in the camps are interested in having a job or starting a business compared to 28 percent in high spillover areas and 23 percent in low spillover areas. Furthermore, while only approximately 19 percent of those in the host community have attempted to start a business or searched for a job, nearly 40 percent of those in the refugee camps have done so, on average. These comparisons suggest that refugees face additional hurdles and barriers in being able to secure employment and maintain earnings in Bangladesh, as might have been expected given the negative stereotypes and resulting discrimination directed towards them.

While these results may suggest that labour market outcomes between those in low spillover and high spillover areas have not changed in a differential manner, I nonetheless control for these variables in the regression analysis for both mental health and overall health.

Table 10 presents this analysis for mental health, which includes controls for post-crisis employment and earnings in addition to the control for change in healthcare access. Here, we again find coefficients of similar magnitude, whereby the findings for symptoms of depression and symptoms of trauma are negative, despite a positive coefficient on exposure to trauma. The results on exposure to trauma and symptoms of trauma remain statistically significant at the 1 percent level, even among this subsample that was employed in 2017 and continues to be employed in 2019.

Table 10. Effect of Distance to Refugee Camps on Host Respondent Mental Health (Inc. Controls for Post-Crisis Employment and Earnings)

	Panel A			Panel B		
	(1) Symptoms of Depression (Std. Dev.)	(2) Exposure to Trauma (Std. Dev.)	(3) Symptoms of Trauma (Std. Dev.)	(4) Symptoms of Depression (Std. Dev.)	(5) Exposure to Trauma (Std. Dev.)	(6) Symptoms of Trauma (Std. Dev.)
Live Close to Refugee Camp	-0.06	0.17***	-0.15***	-0.06	0.16***	-0.13***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Constant	0.18	0.42**	0.25	0.18	0.57**	0.26
	(0.22)	(0.21)	(0.21)	(0.24)	(0.23)	(0.23)
Change Healthcare Access Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Individual Demographic Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Income Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Occupation Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Post-Crisis Income Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Post-Crisis Occupation Controls?	No	No	No	Yes	Yes	Yes
Observations	1,870	1,870	1,870	1,758	1,758	1,758

Notes: Dependent variables are transformed into z-scores by subtracting the control group mean and dividing by the control group standard deviation. The independent variable is a dummy which takes on a value of 1 if the respondent lives no farther than 15 kilometers (a 3-hours walking distance) away from the refugee camps and 0 otherwise. Individual demographic controls include age, gender, country of birth, religion, ethnicity, marital status, and reading ability. Pre-crisis income controls include annual total pre-crisis income and annual log total pre-crisis income. Pre-crisis occupation controls include an indicator for pre-crisis employment in agriculture and pre-crisis paid work. Post-crisis controls include the same set of variables, but evaluated in 2018-2019, rather than 2016-2017. Robust standard errors in parentheses. Significance levels are denoted by *** p < 0:01, ** p < 0:05, * p < 0:1.

Table 11. Effect of Distance to Refugee Camps on Host Respondent Overall Health (Inc. Post-Crisis Controls)

	(1) Perceived Overall Health	(2) Perceived Overall Health	(3) Perceived Overall Health	(4) Perceived Overall Health	(5) Perceived Overall Health
Live Close to Refugee Camp	0.10**	0.10**	0.07*	0.07*	0.07
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Actual Overall Health	0.52***	0.52***	0.51***	0.51***	0.51***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Change Healthcare Access	0.10***	0.10***	0.10***	0.10***	0.10***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Constant	3.49***	3.45***	3.56***	3.56***	3.57***
	(0.20)	(0.20)	(0.21)	(0.22)	(0.22)
Individual Demographic Controls?	Yes	Yes	Yes	Yes	Yes
Change Healthcare Access Controls?	Yes	Yes	Yes	Yes	Yes
Actual Overall Health Controls?	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Income Controls?	Yes	Yes	Yes	Yes	Yes
Pre-Crisis Occupation Controls?	Yes	Yes	Yes	Yes	Yes
Post-Crisis Income Controls?	No	Yes	Yes	Yes	Yes
Post-Crisis Occupation Controls?	No	No	Partly	Partly	Yes
Observations	1,869	1,869	1,757	1,757	1,757

Notes: Dependent variable of 'Perceived Overall Health' is the response to the question "In general, would you say that your health is [...]?" Ordinal values between 1 and 5 are assigned for responses of 'very bad', 'bad', 'regular', 'good' and 'very good', respectively. The independent variable is a dummy which takes on a value of 1 if the respondent lives no farther than 15 kilometers (a 3-hours walking distance) away from the refugee camps and 0 otherwise. The dummy variable 'Actual Overall Health' acts as an illness avoidance indicator and records a response of 'yes' as 0 and 'no' as 1 to the question "During the past four weeks, have you suffered from an illness or injury?" The variable 'Change Healthcare Access' records a response of 'worsened' as 1, 'no change' as 2, and 'improved' as 3 to the question "Compared to before July 2017, has your healthcare improved or worsened?" Individual demographic controls include age, gender, country of birth, religion, ethnicity, marital status, and reading ability. Pre-crisis income controls include annual total pre-crisis income and annual log total pre-crisis income. Pre-crisis occupation controls include an indicator for pre-crisis employment in agriculture and pre-crisis paid work. Post-crisis controls include the same set of variables, but evaluated in 2018-2019, rather than 2016-2017. Robust standard errors in parentheses. Significance levels are denoted by *** p < 0:01, ** p < 0:05, * p < 0:1.

Table 11 extends the inclusion of post-crisis employment and earnings controls to analysis regarding overall health. In this case, adding in a set of post-crisis income controls does not change the coefficient compared to the baseline specification presented in column (1). However, by adding in controls for post-crisis occupation, which include an indicator for whether an individual works in agriculture (presented in column 3) and an indicator for whether an individual earns paid wages from a business or government job (presented in column 4), we see that the coefficient of interest is attenuated. Introducing both post-crisis controls simultaneously makes the coefficient statistically significant at the 11 percent level, but not quite the standard 10 percent level. In this analysis, we have also reduced our sample size since we can only consider observations who worked in both the pre-crisis period and the post-crisis period. However, the coefficient remains positive and is on the verge of being statistically significant at the 10 percent level. Considering the sweeping set of controls that have entered the regression equation, it is possible that part of this difference between low and high spillover areas is due to reference-dependent evaluation of the subjective outcome of perceived overall health.

As a final remark, these controls are not a part of my central analysis insofar as they could be argued to be "bad controls:" that is, they might be an outcome of the treatment variable which is a dummy for whether a respondent lives close to the refugee camp. That being said, it is unlikely that many people would have moved in such a way that it would impact their employment opportunities

or profession. As such, the inclusion of these variables can serve as a valuable robustness check that helps to account for differential employment and income trends between places that are near the camps and far from the camps. Since this is an alternative channel that could explain the results, and the findings remain largely robust to the inclusion of these controls, we can gain increased assurance that the social relativism theory of reference dependence might play a role in explaining self-evaluation of subjective outcomes for those near the refugee camps.

5. DISCUSSION

This series of findings support a social relativism theory of reference dependence, in which host country households evaluate their life outcomes in comparison to the lives of those in refugee camps. In doing so, they reflect more favourably on their situation and report better outcomes. To gain a sense of how large these effects are, we can consider the most rigorous specification, outlined in Panel D of Table 6. Under the reference dependence framework, those living close to the camps have a 0.04 standard deviation reduction in symptoms of depression as a result of evaluating their own depressive symptoms in comparison to the nearby refugees. Likewise, individuals in high spillover zones report 0.13 standard deviation lower symptoms of trauma, despite being exposed to 0.20 standard deviation greater trauma. Although the findings regarding symptoms of depression lose statistical significance upon controlling for pre-crisis occupation, the findings on exposure to trauma and symptoms of trauma remain statistically significant at the 1 percent level.

An examination of overall health, in which controls for change in healthcare access and actual overall health are available, reaffirms support for this reference dependence hypothesis. Those living close to refugee camps report 2.5 percentage points greater perceived overall health, statistically significant at the 5 percent level. In addition, the fact that healthcare access saw a greater improvement for low spillover households, when combined with the expected positive association between healthcare access and both mental and overall health, suggests that controlling for these differential impacts would increase our coefficients in the mental health analysis which is indeed the case. Given the presence of other variables (e.g. feelings of patriotism, investments into local communities) that may have seen a greater improvement for low spillover households, and are positively correlated with mental and overall health, our results may understate the true size of the effects.

Considering that these effects may emerge simply from base group adjustments, these are notable results. As such, in providing counselling or support services to host communities impacted by the influx of refugees from neighbouring countries, encouraging these individuals to reflect upon their outcomes in relation to the less-privileged can have a large impact on perceived mental health. Like the intervention posed in Jensen (2010), this is an information intervention with potentially large impacts on the mental health of communities that are relatively low-income. Given the low-cost nature of this type of intervention, along with its simplicity, it could be effective in this context.

That being said, the results of this research are not causal, as differences between control and treatment groups may be explained by: (1) pre-existing differences between the groups prior to August 2017; and/or (2) differential material impacts between groups as a result of the refugee influx. Although I control for a variety of potential confounders, the potential for selection bias is readily acknowledged. Nonetheless, the analysis related to differential overall health impacts between groups, in which I control for actual health and change in healthcare access, suggests a role for the reference dependence channel in this setting. Further research is needed to reaffirm our understanding of the complex nature of mental health in these communities.

This research makes a sizeable contribution to a vast literature on reference dependence by exploring a previously unexamined context. In providing evidence for a social relativism theory of reference dependence, this paper opens up avenues of further research. An obvious extension of this work would be to examine impacts on the refugees themselves, building upon the recent work by Hussam (2021) on the psychosocial impacts of forced idleness. As more data becomes available, instrumental variables or difference-in-differences empirical approaches will become more feasible. At the very least, we must strive to better understand and prepare policy for this underserved population, given that an estimated 1 billion people will be forcibly displaced as a result of climate change by 2050 (IOM, 2014).

6. CONCLUSION

This paper explored the effect of a refugee influx on host community health. In this analysis, two hypotheses were considered: the first posits that distaste for refugees or material harms would lead to worse outcomes with greater interaction; the second suggests that individuals who interact with refugees more often will exhibit better

outcomes with greater interaction, resulting from evaluating their outcomes in relation to the refugee base group. In order to explore these countervailing channels, I utilized quasi-random, geo-spatial variation in distance from refugee camps to proxy for host-refugee interaction in Bangladesh's Rohingya refugee camps. After controlling for a series of potential confounders, I found that individuals living closer to the refugee camps reported greater exposure to trauma but exhibited reduced symptoms of trauma and reduced symptoms of depression. This analysis on mental health is supplemented by an examination of overall health, finding that those living closer to the camps also report better health outcomes after controlling for an indicator of actual health and changes in healthcare access.

Taken together, these findings support a social relativism theory of reference dependence, in which host country households that have greater interaction with refugees view their outcomes relative to those of the refugees, and thereby report improved mental and overall health outcomes relative to households far from the camps. This result has policy implications with regards to how counselling services are provided and provides evidence for the importance of base group adjustments in self-assessments of well-being.

Further research is required to extend these results to a clear causal interpretation and quantify the potential benefits from an information intervention aimed at altering base groups in the provision of counselling services. Given the scale and scope of this issue, this field of research is poised for growth in the years to come.

Is Education Good for Mental Health? A Gender Divide on the Benefits of Higher Education on Adulthood Mental Health in Canada

Avah Hawkins, Jingyi Li, Candace Sykes, & Kelsey Wong

ECON 490

ABSTRACT

The relationship between mental health and education has been studied in recent literature, suggesting that additional years of education have a positive effect on mental health. This paper aims to discuss and determine the extent to which attaining university education benefits the mental well-being of Canadian adults. Using the 2017 version and 31st cycle of the General Social Survey, our dependent variable, mental health, is determined by self-reported measures. With a focus on Canadian individuals ages 25 to 50 years old, we hypothesize that attaining higher levels of post-secondary education, a bachelor's degree or above, is correlated with higher levels of mental health in adulthood. Our economic theory suggests that engaging in post-secondary schooling improves individual social support systems and emotional capacity for navigating setbacks encountered in life. These factors are expected to increase mental resilience and have sustained stabilizing effects on mental health in adulthood. Our research suggests that there exists no statistical significance between university education (attaining a bachelor's degree or above) and adulthood mental health overall. Upon further analysis utilizing a sex-based approach, females are shown to have a statistical significant relationship of 0.0941 points higher mental health, in comparison to their less-educated counterparts, with the attainment of university education, while males show no statistical significant relationship. Thus, this analysis indicates the need for improved focus and prioritization of schooling policy interventions for females.

1. INTRODUCTION

Why do some individuals report better mental health than others in adulthood? Does sex play a role? As proven by literature, there is a multitude of contributing factors to mental health spanning many different areas of life, including stress levels (Borghouts et al., 2021), sexual orientation (Adams et al., 2013), age of immigration (Leu et al., 2008), family structure (Behere et al., 2017), and many more. While there are numerous variables we can examine as contributing factors to mental health, for the purpose of this research, education is selected as the variable of analysis. We believe that this variable will yield impactful findings, which may help to better inform and encourage impactful policies surrounding mental health. In examining the link between education and mental health, we found little literature on the impacts of post-secondary education, in particular the impact of university-level education, on mental health. Understanding the relationship between post-secondary education and mental health can help to support better mental health in adulthood. If post-secondary education leads to improved mental health outcomes, this may inform methods to encourage the pursuit of higher education, such as through increased university and career counseling within secondary schools, greater funding for higher education institutions, and increased grants for lower-income students.

This paper argues that attaining higher levels of education, specifically, achieving a bachelor's degree or above, improves the mental health of individuals in adulthood. For Canadians, post-secondary education is the first opportunity to exercise freedom of choice/autonomy as to whether or not to continue education (Oreopoulos, 2005), which makes for an interesting area of study: we look at how mental health is impacted by this conscious choice to pursue and complete post-secondary education above this required level. We theorize post-secondary education to be an investment with social, emotional, and financial returns, factors that also affect mental health. The economic theory behind this is as follows.

Firstly, education leads to social benefits. Johnes and Johnes (2004) argue that education generates a 'neighborhood effect', allowing individuals to gain a deeper sense of community alongside knowledge spillovers. We speculate that university classes and events allow for the mixing of motivated and intelligent individuals from various backgrounds; in turn, these opportunities to socialize allow students to foster distinct connections with like-minded individuals and build a support network. As a result, these bonds can act as a dependable resource for encouragement in times of challenge or stress, not only during post-secondary education, but

as individuals later navigate adulthood. Therefore, we theorize that these friendships rooted in shared post-secondary experiences will have a stabilizing effect on mental health and are likely to improve a person's overall mental resilience.

Secondly, from an emotional viewpoint, schooling teaches students about themselves - how they think, learn, and persevere in the face of disappointment. Pursuing a higher education often brings new challenges, including changes in social support structures, financial independence, and new living arrangements (Bolinski et al., 2020). We consider that these testing times may not be futile; in the process, individuals condition their minds against setbacks and improve their confidence through small cumulative victories. Wyatt et al. (2017) argues that social networks, along with learning to maintain a sense of control over one's personal and academic life, may allow students to learn to better handle stress. These competencies then aid in developing one's self-sufficiency and concept of self-worth, and in doing so, prepare people for the real world that lies beyond the bubble of academia. We believe that the resiliency required to accomplish an undergraduate or higher degree can contribute to improved mental health later in life.

Thirdly, in terms of financial returns, education has a positive effect on income known as a "college wage premium" (Walker & Zhu, 2008; Walker & Zhu, 2011). Specifically, women experience on average large and similar rates of return on earnings across various university undergraduate majors, due to facing discrimination in the sub-degree labour market, while men experience the largest returns in earnings from undergraduate education through majoring in Law, Economics, and Management (Walker & Zhu, 2011). Additionally, obtaining post-graduate degrees has high rates of returns in earnings separate from the returns from undergraduate degrees. (Walker & Zhu, 2011). We theorize that greater income provides financial security and stability, thus alleviating a source of stress for individuals, which is a prominent mental health concern (Borghouts et al., 2021)

The incentives for attending university are extensive. Higher education is associated with greater income and increased control over one's working life, as well as lower rates of divorce and unemployment (Chevalier & Feinstein, 2006). As such, we speculate that the investments in friendship, resourcefulness, and personal growth that education brings have substantial and significant returns to future mental health. Receiving a post-secondary education broadens an individual's social horizons and expands their intellectual capacity, allowing for greater career development and relationship-building. Our research seeks to shed light on the potentially protective effects of higher education on mental health, which are largely veiled behind economic benefits.

The paper will proceed as follows: Section I presents an exploration of past literature on the links between mental health and education. Section II expands upon the data utilized in this paper. Section III defines the empirical approach and elaborates on the results. Section IV concludes and discusses possibilities for future research.

2. LITERATURE REVIEW

The relationship between education and mental health is of interest, given its relevance to the general population. Previous studies suggest that education affects mental health by providing economic and socio-economic status rewards, as well as impacting the number of stressors individuals experience and the availability of resources they possess to deal with these stressors. Mental health is thought to be impactful in numerous aspects of life, including physical health, workplace productivity, and overall life satisfaction. The studies we examine collectively take mental health as dependent on years of educational attainment, while accounting for other factors that may influence an individual's well-being, such as income. Prior research has predominantly examined the effects of additional years of elementary schooling on the mental health of adults (Powdthavee et al., 2013), with fewer recent studies investigating the effects of additional time spent in higher education, such as in university programs. We will review the existing literature and interpret these findings in our paper.

A. Compulsory Education

Several health studies attempt to predict the effects of education on mental health by focusing on the implementation of compulsory schooling laws. Jürges et al. (2013) argue that, as determined by self-reported measures of health and biological stress markers, poorer mental and physical health is associated with lower levels of education. By examining the 1947 and 1973 British reforms that increased the age of compulsory schooling by an additional year, the authors established that lower education levels have a negative impact on mental well-being and subsequently increase cardiovascular disease risk. They also found that lower education levels increase fibrinogen blood levels, which are associated with inflammation, leading to an increased risk of cardiovascular disease and various other health risks (Jürges et al., 2013). Economic research has also investigated the effects of education reforms. Specifically, Crespo et al. (2014) use the reforms occurring between 1950 and 1969 that enforced additional years of compulsory schooling to examine these effects.

Their findings showed that for individuals over the age of 50, the probability of developing depression falls by 6.5 percentage points, cognition improves, and memory scores show positive effects in the long run. Taking a somewhat different approach, Powdthavee et al. (2013) collect data from younger cohorts, ages 22 to 65, to determine the effect of compulsory schooling reforms in Australia on mental health and well-being, mediated through income. Positive effects on well-being, as measured through mental health, life satisfaction, and financial satisfaction, are found as a result of an additional year of education.

B. Years of College Education

Past economic research surrounding the links between educational attainment and mental health in adulthood has largely investigated the effects of years spent in primary and secondary schooling (Jürges et al., 2013). Examining the effects of post-secondary education is important to further understand the long-term relationship between education and mental health. McFarland and Wagner (2015) utilize data from monozygotic twins to study the connection between holding a college degree and symptoms of depression. They find, with significance, that individuals who possess college degrees report fewer depressive symptoms relative to those with a high school degree or less, and that having a college degree is inversely associated with and protect against depression. Numerically, this is equivalent to a 0.5 standard deviation decrease in neuroticism—a predictor of depression (McFarland and Wagner, 2015, p. 10).

As well, Amin et al. (2020) examine the association between educational attainment and depressive symptoms. Their article examines the mental health of individuals ages 29 years old in the National Longitudinal Study of Adolescent Health, and 53 years old in the Wisconsin Longitudinal Study. Like the previous findings, the results from the OLS regressions in both datasets imply that college graduation is associated with fewer depressive symptoms. In contrast, a study by Sarmento (2014) that samples students from a higher education institution used data from a questionnaire assessing mental health, anxiety, depression, and risk behaviour. The study finds that, relative to the general population, young students in higher education coming out of high school exhibit better mental health and lower anxiety. However, factors such as higher risk behaviour, unsafe sexual activity, and physical inactivity contribute to depression which is also prevalent among college-educated students.

C. Income and Wages

Various literary sources analyze the relationship between personal income and education; this is due to the general belief that additional years of education are associated with increased future income (Johar and Truong, 2014). We then used this distinction to investigate how wages are indirectly tied to education and mental health. Using adolescent depression as the explanatory variable, with wages and education as the dependent variable, Johar and Truong (2014) find that higher levels of education increase hourly wages. However, depression prevalence in adolescence effectively reduces the number of years spent in education, thus reducing hourly wages later in life (Johar and Truong, 2014).

The effects of educational attainment on mental health may be mediated through income, as suggested by Powdthavee et al. (2013). Their paper focuses specifically on the increased financial returns and improvement in life satisfaction induced by additional compulsory schooling. They find that an extra year of schooling improves mental health; this effect, however, is only statistically significant among males. The authors use changes in compulsory schooling laws that interact with respondents' birth years as an instrument for education. They find that an additional year of education is associated with a 15.5% return rate to personal income for men and a 10.9% return rate for women (Powdthavee et al., 2013, p. 17). Furthermore, income is found to improve subjective well-being in all specifications—life satisfaction, financial satisfaction, and mental health—for men (Powdthavee et al., 2013). The authors find no evidence that additional schooling increases subjective well-being independently of income, suggesting that it is primarily the rise in income that improves reported mental health.

This is also evident in the paper by Andersen and Gunes (2018), which proves that adult education attainment, and thus higher yearly income and earnings, is positively related to good adolescent mental health. Contrary to the findings discussed above, Bracke et al. (2014) find that well-educated individuals are less likely to experience improved mental health in countries with low economic returns to schooling. These results suggest that economic returns to education and financial satisfaction are crucial in shaping the effects of schooling on mental health.

D. Effects on Males Versus Females

The results of additional years of schooling on mental health between men and women vary. In the paper by Brunello et al. (2016), they find that women experience between a 4.0 and 6.4 percentage point decrease in self-reported poor mental and physical health with

more education (that is, their mental health improves), whereas men experience a 4.8 to 5.4 percentage point decrease in poor mental and physical health (Brunello et al., 2016, p. 315) In this study, self-perceived poor health is positively associated with smoking, lack of physical activity, poor diet, and a higher body mass index (BMI). To arrive at this conclusion, 13 European countries provide a multi-country dataset including information on health, education, and health behaviours, sampling men and women over the age of 50. In opposition, Sarmento (2014) finds that higher-educated females are more susceptible to mental health issues, such as depression than their male counterparts.

Lastly, additional literature by Powdthavee et al. (2013) demonstrates that extra years of schooling have a positive and statistically significant effect on life satisfaction amongst males, while for women, this finding lacks statistical significance. Although additional education is found to improve financial satisfaction for both men and women, the authors conclude that the subjective well-being of men is more significantly impacted by compulsory schooling changes. Alternatively, when investigating the association between education and genetic factors, Amin et al. (2020) find that attaining a college education reduces the effects of a genetic predisposition to depression in individuals of older ages. However, the authors do not find significant statistical differences in these effects across genders.

E. Remarks

Education, which allows one to acquire both knowledge and professional skills, has been linked to improved health by teaching individuals to better process information and become more health-conscious (Chevalier & Feinstein, 2006). Existing economic literature has found a positive correlation between education and several components of adult health, including mental health and well-being. We have examined research regarding the association between educational attainment and mental health, as well as how this relationship may be mediated through other factors, such as income and socio-economic status. Subsequently, it has been reinforced that education does have a protective effect on mental health and cognition. The marginal effects of this additional schooling are suggested to be larger for certain subgroups, such as males or those with a lower socioeconomic status in childhood.

Our research contributes to the existing literature by focusing on the effects of attaining a post-secondary education on adult mental health, as opposed to the effects on mental health of additional years of elementary schooling. Specifically, we provide insight on Canadian mental health for adults ages 25 to 50, a demographic for which the

effects of education on mental health have not yet been studied in depth.

3. DATA

This research utilizes the most recent version of the General Social Survey (GSS) dataset, published in 2017. Since 1985, it has been administered approximately every five years by Statistics Canada and focuses on six prominent themes in Canadian society: caregiving, families, time use, social identity, volunteering, and organization. By regularly gathering this data, the well-being and living conditions of Canadian residents is monitored over time.

TABLE 1: SUMMARY STATISTICS

	FEMALE					MALE				
Variable Label	Mean	Std. Dev.	Min	Max	N	Mean	Std. Dev.	Min	Max	N
Mental Health										
Self-reported mental health	3.79	0.96	1	5	4050	3.91	0.98	1	5	3573
Educational Attainment										
Bachelor's degree or above	0.42	0.49	0	1	4014	0.35	0.48	0	1	3538
Individual Attributes										
Income	2.42	1.24	1	6	4082	2.94	1.46	1	6	3602
Overall feelings about life	8.07	1.55	0	10	4039	7.94	1.62	0	10	3560
Marital Status										
Separated	0.04	0.19	0	1	4081	0.03	0.16	0	1	3602
Common-law	0.17	0.37	0	1	4081	0.16	0.37	0	1	3602
Widowed	0.01	0.09	0	1	4081	0.003	0.06	0	1	3602
Divorced	0.05	0.22	0	1	4081	0.03	0.18	0	1	3602
Single	0.25	0.43	0	1	4081	0.30	0.46	0	1	3602
Family Structure										
Intact Family	0.44	0.50	0	1	4082	0.43	0.49	0	1	3602
Stepfamily with a common child	0.03	0.16	0	1	4082	0.02	0.15	0	1	3602
Stepfamily without a common child	0.03	0.17	0	1	4082	0.03	0.17	0	1	3602
Single parent family	0.15	0.36	0	1	4082	0.06	0.25	0	1	3602
Respondent lives without a partner or child	0.17	0.38	0	1	4082	0.28	0.45	0	1	3602
Total number of children the respondent has	1.43	1.29	0	7	4078	1.25	1.33	0	7	3597
Residence Features										
Living in the Atlantic region	0.20	0.40	0	1	4082	0.19	0.40	0	1	3602
Living in Quebec	0.18	0.39	0	1	4082	0.20	0.40	0	1	3602
Living in the Prairie region	0.21	0.41	0	1	4082	0.22	0.41	0	1	3602
Living in British Columbia	0.12	0.32	0	1	4082	0.12	0.32	0	1	3602
Residence ownership	0.80	0.40	0	1	2784	0.82	0.38	0	1	2497

A. Independent and Dependent Variables

The dependent variable being examined is mental health (MENTALHEALTH). The GSS (2020) measures mental health according to a self-reported ranking scheme of 1 being excellent to 5 being poor. For ease of analysis, the GSS (2020) mental health variable is manipulated to have the coding reversed, with 1 now reflecting poor mental health and 5 reflecting excellent mental health. The independent variable is education (HIGHEREDUCATION). Education is constructed as a dummy variable equal to 1 for respondents with a bachelor's degree or higher education (e.g., a master's degree or PhD) and 0 for respondents that have attained schooling less than a bachelor's degree. According to NSC Research Center (2016), on average, it takes students two years to earn an associate degree and more than four years for bachelor's degree earners. The data is then restricted between the ages of 25 and 50 years old, due to the high likelihood that formal education is likely to

be completed by or within this age period, and life cycle events that potentially have strong effects on mental health (positive or negative) are typically encountered during this age period—entering the workforce, experiencing financial uncertainty, finding a significant other, buying a home, etc. Ages above 50 are beyond the parameters noted.

B. Control & Dummy Variables

Numerous control variables are selected for their association with mental health, either positively or negatively, with the intent to eliminate omitted variable bias as best as possible. To isolate the two regressions for male and female, the variable sex (SEX) is utilized and defined as 1 equals male and 2 equals female. Our data set, the GSS (2020), did not account for gender distinctions, so this area of study was not measurable. Our second control is personal annual income (INCOME) which is a categorical variable that measures earnings from less than $25,000 to greater than $125,000 split into six categories of income ranges. As well, a respondent's family income (FAMINCOME) is incorporated and the variable is ranked on a scale from 1 to 6, with the same categories as personal annual income.

Next, there are five dummy variables created from the marital status variable in the GSS (2020), these being common-law (COMMONLAW), otherwise known as a cohabiting relationship, widowed (WIDOWED), separated (SEPARATED), divorced (DIVORCED), and single (SINGLE). To provide a comparison against other marital status variables and to prevent collinearity, married respondents are omitted due to their numerous sample set. The marital status dummy variables are set to equal 1 if the respondent identified that they were in the specified relationship status and 0 in all other cases.

Evidence reveals that family processes in various family types are closely tied with the quality of the parent-child relationship, and mental health is a product of these differences in relation to the quality of family relationships (Voydanoff et al., 2016). The family structure of respondents is then taken into consideration, which includes intact families (INTACTFAMILY), stepfamilies with and without having a common child (CHILDSTEPFAM and NOCHILDSTEPFAM), single parent families (LONEPARENT), and those living without a partner or child (NOPARTNERORCHILD). Each of these family structures is controlled for with the use of dummies, with 1 being

[1] *Roth and Slotwinski attribute this to the lack of high-quality administrative data on relative incomes, particularly for women.*

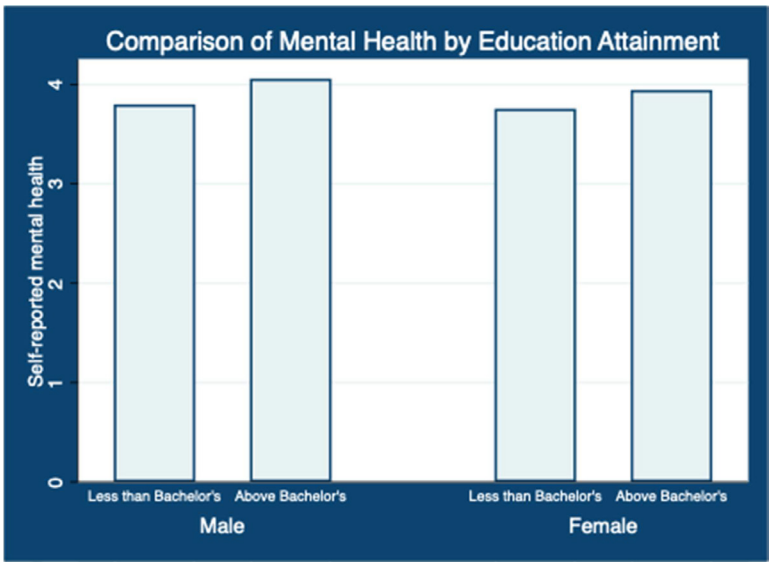
equal to respondents reporting living in that specific arrangement and 0 being equal to respondents reporting otherwise. To avoid collinearity issues and to provide contrast with other family-type dummies, exclusively couple living arrangements are excluded from the analysis. Next, related to family characteristics, the total number of children reported by a respondent is accounted for (TOTALCHILDREN), with numbers of children seven or greater being placed in the same response category.

Lastly, dummies are created for respondents' regions of residence, encompassing British Columbia (BRITISHCOLUMBIA), the Prairie region (PRAIRIE), Quebec (QUEBEC), and the Atlantic region (ATLANTIC). The region dummies are equal to 1 if the respondent lives in the indicated region, and 0 if the respondent lives elsewhere. The Ontario region dummy variable is removed, due to resulting collinearity issues, and acts as a comparison for the other regions. Lastly, whether a respondent is an owner of their dwelling is included as a dummy variable (RESOWNERSHIP), with 1 being equal to yes and 0 being equal to no.

C. Predicted Relationships

The predicted relationship between mental health and education is that with increasing educational attainment self-reported mental health will rise, as shown in FIGURE 1.

FIGURE 1: COMPARISON OF MENTAL HEALTH BY EDUCATION ATTAINMENT



This theory rests on the basis that socially, during university, people build long-lasting friendships, and emotionally, that the university experience is an environment that enhances decision making and problem-solving skills, as well as abilities to deal with hardships. In other words, post-secondary education strengthens a person's mental resilience, with effects likely lasting into adulthood. Figure 1 supports our hypothesis that with increasing education above the bachelor's level, mental health reporting improves.

In terms of the relationships between the controls and mental health, we first predict a positive relationship between income and mental health, as numerous research has already proven. Likewise, we predict that life outlook and mental health will be strongly and positively correlated since it is reasonable to theorize that those who generally maintain a positive mindset will approach life challenges accordingly and will thus experience less strain on their mental health.

Regarding marital status, we predict that those in a marriage or common-law relationship will report better mental health, as these individuals will likely receive consistent and healthy emotional support from their partners. Conversely, we predict that being separated, widowed, or divorced will decrease self-reported mental health, as individuals who report these relationship statuses may experience lingering psychological pain and distress.

In terms of family structure, we predict that intact families provide substantial benefits to mental well-being. Non-traditional, or single-parent families, are believed to have lower resilience to stress, potentially creating strained parent-child relationships; these family structures are also linked to higher rates of poverty and behavioral problems in children (Behere et al., 2017). Single-parent families are further associated with higher risks of mental health issues, including depression and psychological distress, in both children and adults (Behere et al., 2017). Consequently, individuals living in these non-traditional arrangements are expected to report lower mental health, and we expect individuals from intact or traditional family dynamics to report better mental health. Individuals without a partner or children are anticipated to have a positive relationship with their mental health, as they likely face fewer conflicts and their personal welfare is prioritized.

Evidence reveals that Extreme Weather Events (EWEs) (for example heavy rain, cold temperatures, and strong wind) impose health and socio-economic burden on exposed residents. Specially, The psychological impacts on lower education levels and the female gender are pronounced. (Chique et al., 2021). In turn, we predict that individuals residing in British Columbia is highly likely to report

better mental health relative to those residing in Atlantic Canada, Quebec, or the Prairies. It is possible that harsh weather conditions outside of British Columbia and decreased sun exposure may cause a decline in reported mental health. British Columbia's outdoor-centric culture may also elevate one's mood, as well as both physical and mental health. Additionally, Additionally, people invest numerous financial and psychological resources in their real estate. The living condition and loan pressure are intently related to mental health (Evans et al., 2003). Therefore, ownership of a residence is expected to improve mental health, as individuals without a mortgage will likely experience less financial stress and will enjoy a stable living environment.

D. Weaknesses in Data

The dependent variable (MENTALHEALTH) from the GSS (2020) dataset is a self-reported ranking scheme. Atkinson et al. (1997) states that transient, or impermanent, affective factors can influence the validity of measuring subjective well-being. This short-term characteristic applies to mental health and may bias the research results because of individuals' inaccurate reflections of their mental cognition. Moreover, the independent variable (HIGHEREDUCATION) only distinguishes respondents from obtaining below or higher post-secondary education, but fails to layer higher education (e.g. draw a distinction between a master's degree and PhD). Apart from family type and the total number of children in a family, parents' educational attainments are also closely associated with college students' mental well-being (Assari 2018). The GSS (2020) lacks a variable reporting this relationship thus creating a source of weakness in this dataset and preventing a more comprehensive study.

4. RESULTS

We use a multivariate ordinary least squares (OLS) regression model to determine the relationship between higher education and mental health for respondents ages 25 to 50 years old. To prevent omitted variable bias, we control for variables that may also impact mental health and education. We run separate regressions for males and females independent of one another, both before and after post-regression tests are performed and corrected for. Our hypothesis, that higher levels of educational attainment (at or above the bachelor's level) increase a person's mental health in adulthood, is corroborated by our findings, however, the results differ between males and

females. We will first focus on our results concerning females, and then compare these findings to those regarding males. The OLS regression model, including all controls, is written as equation (1):

$$(1)\ MENTALHEALTH_i = \beta_0 + \beta_1 HIGHEREDUCATION_i + \beta_2 INCOME_i + \beta_3 INCOME2_i + \beta_4 FAMINCOME_i + \beta_5 FAMINCOME_i + \beta_6 SEPARATED_i + \beta_7 COMMONLAW_i + \beta_8 WIDOWED_i + \beta_9 DIVORCED_i + \beta_{10} SINGLE_i + \beta_{11} INTACTFAMILY_i + \beta_{12} CHILDSTEPFAM_i + \beta_{13} NOCHILDSTEPFAM_i + \beta_{14} LONEPARENT_i + \beta_{15} NOPARTNERORCHILD_i + \beta_{16} TOTALCHILDREN_i + \beta_{17} TOTALCHILDREN2_i + \beta_{18} ATLANTIC_i + \beta_{19} QUEBEC_i + \beta_{20} PRAIRIE_i + \beta_{21} BRITISHCOLUMBIA_i + \beta_{22} RESOWNERSHIP_i + \varepsilon_i$$

A. Female Regression Results

After running an initial simple linear regression model, we implement several post-regression tests to improve the quality of our findings. We first test for outliers that may be skewing our results. Other than respondents being slightly more likely to report having excellent mental health and having higher numbers of children, there are no clear, distinguishable patterns differentiating the excluded outliers from the mean of our regression.

To determine if the relationship between the independent and explanatory variables are linear, we test for non-linearity, and correct for non-linearity problems in the income, life outlook, and total number of children variables. The variance inflation factors (VIF) for each variable are less than 10, indicating that multicollinearity is not an issue. Finally, the null hypothesis of homoskedastic errors can be rejected, due to the results of a low p-value from the Breusch-Pagan test. Therefore, we conclude that the errors are heteroskedastic and need correcting. Furthermore, sample weights are applied to eliminate oversample effects.

As indicated in Table 2, including the post-regression tests, sample weights, control variables, excluding residuals exceeding 3 in absolute value, and holding everything else constant, we find that post-secondary educated females report their mental health as 0.0941 points higher than less educated females, with significance at the 10% level. These corrections also allow for our R2, or goodness of fit, to improve from 0.479% to 3.18% as the controls are applied by grouping which means that the variation in mental health within this sample is suitably explained by variations in an individual's level of education and the various control variables added. It is worth noticing that the nature of the GSS dataset caused omitted bias for marital status and family structure. In turn, variables related to family structure appear statistically insignificant.

TABLE 2: ADJUSTED FEMALE REGRESSION RESULT

Variable Label	Self-reported mental health				
	(1)	(2)	(3)	(4)	(5)
Educational Attainment					
Bachelor's degree or above	**0.124***** (0.0449)	**0.0979**** (0.0467)	**0.0886*** (0.0465)	**0.0841*** (0.0468)	**0.0941*** (0.0471)
Income Gap					
Income		0.00376 (0.0705)	0.0239 (0.0707)	0.0202 (0.0723)	0.0217 (0.0728)
Squared income		0.00503 (0.0106)	0.00389 (0.0106)	0.00406 (0.0107)	0.00406 (0.0108)
Family income		0.0295 (0.0921)	-0.0391 (0.0920)	-0.0331 (0.0933)	-0.0346 (0.0942)
Squared family income		-0.00328 (0.0111)	-0.00204 (0.0109)	0.00203 (0.0110)	0.00223 (0.0110)
Marital Status					
Separated			**-0.342**** (0.147)	**-0.415**** (0.180)	**-0.410**** (0.180)
Common-law			**0.154***** (0.0550)	**0.169***** (0.0575)	**0.114*** (0.0645)
Widowed			-0.285 (0.230)	-0.365 (0.259)	-0.339 (0.255)
Divorced			**-0.468**** (0.130)	**-0.553***** (0.176)	**-0.578***** (0.179)
Single			-0.0979 (0.0793)	-0.184 (0.131)	-0.226 (0.138)
Family Structure					
Intact family				0.0419 (0.0814)	0.0420 (0.0816)
Stepfamily with a common child				-0.0443 (0.164)	-0.0373 (0.164)
Stepfamily without a common child				-0.0730 (0.134)	-0.0752 (0.135)
Single parent family				0.136 (0.177)	0.137 (0.177)
Respondent lives without a partner or child				0.107 (0.151)	0.120 (0.156)
Total number of children the respondent has				-0.0550 (0.0547)	-0.0659 (0.0548)
Squared total number of children the respondent has				0.0114	0.0127
Residence Features					
Living in the Atlantic region					-0.360 (0.0606)
Living in Quebec					**0.129**** (0.0645)
Living in the Prairie region					0.0147 (0.0591)
Living in British Columbia					-0.0625 (0.0755)
Residence ownership					-0.0369 (0.0628)
Cons	3.818*** (0.0322)	3.722*** (0.180)	3.890*** (0.184)	3.887*** (0.192)	3.922*** (0.195)
N	2730	2730	2730	2730	2730
R^2	0.00479	0.00718	0.0259	0.0277	0.0318
Standard errors in parentheses ="* p<.10	** p<.05	*** p<.01"			

B. Male Regression Results

We run the same post-regression tests on the male regression as we did on the female regression to ensure the quality of our findings. In testing for outliers that may alter our results, we find no direct or identifiable patterns causing extreme estimates far from the mean. To be certain that our assumption of a linear relationship between our independent and predictor variables is correct, we test for non-linearity which is then addressed. Our model controls for non-linearity in the income, life outlook and total number of children variables. Each variable's VIF is lower than 10, implying that multicollinearity is not an issue within our model. To test for heteroskedasticity, we implement the Breusch-Pagan test; a small p-value allows us to reject the null hypothesis of having homoskedastic residuals, leading to the conclusion of heteroskedastic residuals. Finally, sample weights are applied to this model to eliminate oversample effects.

Including our adjustments for multicollinearity, heteroskedasticity, non-linearity, and exclusion of residuals exceeding absolute values of 3, we find in Table 3, holding everything else constant, higher educated males report having mental health 0.0625

points above males with an education level that is lower than a bachelor's degree. This result, however, is not statistically significant. Alternatively, our R2, or goodness of fit, rises from 0.377% to 3.30% as control groupings are added, suggesting that the variation in self-rated mental health in this sample is well-explained by the respondents' level of education, as well as the various other control variables. The omitted bias for marital status and family structure makes variables related to marital status appear statistically insignificant.

TABLE 3: ADJUSTED REGRESSION RESULT FOR MALES

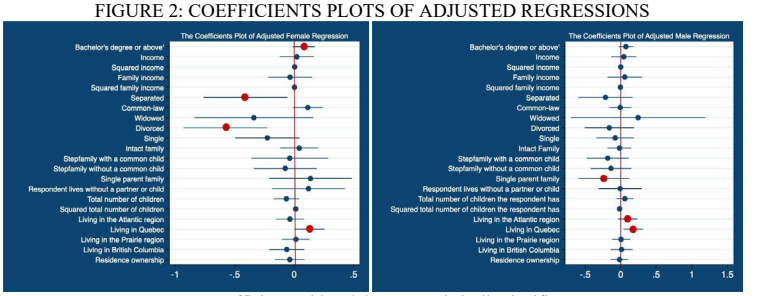
Variable Label	Self-reported mental health				
	(1)	(2)	(3)	(4)	(5)
Educational Attainment					
Bachelor's degree or above	**0.115****	**0.0878****	**0.0923****	0.0659	0.0625
	(0.0475)	(0.0497)	(0.0502)	(0.0506)	(0.0509)
Income Gap					
Income		0.0911	0.0831	0.0796	0.0661
		(0.0858)	(0.0861)	(0.0890)	(0.0889)
Squared income		-0.00774	-0.00716	-0.00647	-0.00421
		(0.0114)	(0.0114)	(0.0116)	(0.0116)
Family income		0.111	0.0640	0.0511	0.0570
		(0.121)	(0.119)	(0.122)	(0.121)
Squared family income		-0.0120	-0.00724	-0.00612	-0.00684
		(0.0143)	(0.0140)	(0.0140)	(0.0139)
Marital Status					
Separated			**-0.297***	-0.197	-0.206
			(0.175)	(0.191)	(0.192)
Common-law			0.0440	0.0911	0.0160
			(0.0379)	(0.0604)	(0.0645)
Widowed			0.170	0.383	0.399
			(0.394)	(0.418)	(0.437)
Divorced			**0.230***	-0.0992	-0.102
			(0.130)	(0.172)	(0.175)
Single			**-0.115***	-0.00265	-0.0305
			(0.0638)	(0.126)	(0.131)
Family Structure					
Intact Family				-0.0120	0.0140
				(0.0822)	(0.0826)
Stepfamily with a common child				-0.193	-0.208
				(0.145)	(0.149)
Stepfamily without a common child				-0.169	-0.170
				(0.146)	(0.145)
Single parent family				-0.293	**-0.318***
				(0.181)	(0.182)
Respondent lives without a partner or child				-0.0507	-0.0562
				(0.149)	(0.154)
Total number of children the respondent has				0.0812	0.0646
				(0.0585)	(0.0590)
Squared total number of children the respondent has				**-0.0211****	**-0.0191***
				(0.0106)	(0.0107)
Residence Features					
Living in the Atlantic region					**0.114***
					(0.0654)
Living in Quebec					**0.175****
					(0.0639)
Living in the Prairie region					-0.00449
					(0.0635)
Living in British Columbia					-0.00124
					(0.0774)
Residence ownership					-0.00560
					(0.0619)
Cons	3.970***	3.559***	3.706***	3.723***	3.729***
	(0.0298)	(0.216)	(0.212)	(0.217)	(0.224)
N	2424	2424	2424	2424	2424
R^2	0.00377	0.0118	0.0172	0.0273	0.0330
Standard errors in parentheses					
="* p<.10	** p<.05	*** p<.01"			

C. Discussion & Comparison

In general, our analysis supports our hypothesis, but with differing results for females and males. The relationship between completing higher education and self-reported mental health is positive and statistically significant for women, though this is not the case for men. Education may provide greater returns to mental health amongst women relative to men because women tend to have fewer resources and opportunities in general; for example, men have historically dominated the workforce in higher-paying jobs and roles,

while women are forced to work harder to achieve similar outcomes (Livingstone et al., 2014). Thus, the pursuit of higher education and moving towards a schooling level that is greater than that of a bachelor's degree have greater returns for women. The insignificance of results among men may be because men with equal or lesser education to women continue to earn higher earnings and wages, and thus, education does not have a comparatively large impact on male mental health.

FIGURE 2: COEFFICIENTS PLOTS OF ADJUSTED REGRESSIONS



Note: coefficients with red dots are statistically significant

Our results, despite our efforts, may potentially be affected by omitted variable bias. For example, the family structure and marital status that omit in regressions. Also, education may act as a proxy for other unobserved factors, such as family background, genetic background, or personality traits. Additionally, these effects on mental health may be driven in part by the socio-economic and cultural environments in which the respondents grew up, and the ways in which these factors structured how individuals pursue their education. Therefore, further research must be conducted to account for a greater number of explanatory variables that may be correlated with mental health.

Our dataset is also subject to potential limitations. Few respondents self-report mental health as being fair or poor, relative to the large number who report mental health as being good, very good, or excellent. Thus, our analysis could potentially be improved by using a larger and more diverse sample. As well, conducting research that includes surveying respondents multiple times, as opposed to simply once in their lifetime, could provide us with a better understanding of the long-term relationship between educational attainment and mental health.

Our results are significant in that they provide support for higher education as well as policy interventions and are consistent with our prediction that post-secondary education will have a protective effect on mental health. Additional educational attainment, such as that of a bachelor's degree, as our economic theory states, may

generate better social support frameworks, cognitive and emotional regulation, decision-making abilities, and skills and knowledge that support future employment and income opportunities. These factors all support better overall mental health and well-being, which in turn should encourage the implementation of policies early on in life that promotes the benefits of higher education for adulthood mental health, particularly for women.

5. CONCLUSION

Using an OLS regression analysis, our paper provides evidence for educationally derived mental health benefits for Canadian females. From our results, we conclude that for females ages 25 to 50, a higher level of educational attainment is correlated with improved mental well-being. This finding is consistent with our hypothesis, and it also contextualizes the results demonstrated by McFarland and Wagner (2015), Sarmento (2014), and Amin et al. (2020), which state that holding a college degree is associated with fewer depressive symptoms. However, there is no substantial evidence regarding the educational and income effect on male mental health, and the income effect on mental health is also ambiguous.

Focusing on female marital status, we can see that our predicted relationships hold; divorced and separated respondents have relatively worse mental well-being compared to married respondents. It is also surprising to find that, regardless of whether the respondent is male or female, compared to other regions of Canada, those living in Quebec self-report the highest mental health. Moreover, for males, living in Alantic is also a sub-optimal choice. This is inconsistent with our earlier prediction that British Columbian respondents would report the highest mental health of all the residential regions, due to the higher likelihood of having an outdoor-focused lifestyle and moderate year-round temperatures.

These conclusions constitute the theoretical basis for policy implications; higher educational experience is of great value to female mental health. According to Rothon (2007), pupils' performance during secondary or compulsory education is the cut-off point for college admission and entry to higher education. Thus, policy interventions early in childhood are essential, as high-quality compulsory education paves the way for earning a place in higher educational institutions, such as colleges or universities. Alternatively, females report relatively poor mental health when experiencing marital problems. Our research suggests that growth in legal and financial assistance in this area could aid in improving the mental health of Canadian women.

Another conclusion we can take away from these findings is the socio-economic impact of education and mental health. Since additional years of education are proven to improve self-reported mental health in females, this also contributes to greater productivity and participation within the economy. Good mental health is a key predictor of a smooth-functioning and prosperous economy, due to the emotional and cognitive resilience that educated individuals possess (Balogh 2011). Conversely, individuals with poorer mental health contribute to lost productivity, imposing a negative externality on society. Thus, schooling reforms have positive implications for the economy in the context of mental health.

While our findings clearly demonstrate the relationship between education and mental health for females, there is no significant evidence for the association between male mental health and educational attainment. This possibly arises from the limitations of our dataset and the inclusion of too few controls. As well, some aspects of education were left unaccounted for; further research could investigate an individual's choice of undergraduate major or area of study within or above a bachelor's degree, and the effects this may have on mental health. Moreover, university students' mental health may also depend on the characteristics of the institution they attend. Hence, overall university satisfaction and comprehensive university ranking should be taken into consideration. Finally, what makes Quebec outstanding from other regions of Canada—is it the weather, ethnic composition, or other factors? Investigating these relationships will help determine the key variables that ultimately cause fluctuations in self-rated mental health. Ultimately, research in these areas will aid in implementing future educational policies to improve and capitalize on the power that post-secondary education has over mental health in adulthood.

The Effect of Crude Oil Pipelines on Border Effects Between Canada and the United States in the Petroleum Industry

Bora Hosal

ECON 494

ABSTRACT

This study will explore the effects of crude oil pipelines on international petroleum trade volumes between Canada and the United States, and whether border effects are weaker if regions are connected with pipelines. Using data from 2013, this study will contribute to the existing studies on border effects by focusing on petroleum products and pipelines to estimate border effects in the petroleum industry. As pipelines reduce transportation costs, results suggest that the existence of pipelines reduce border effects significantly; however, border effects still remain as a barrier to international trade.

1. INTRODUCTION

Numerous studies have explored gravity theory and border effects, and these studies show that borders between countries introduce a significant trade barrier due to national borders, language and cultural barriers, security barriers, and informational barriers. Border effects are defined as "the asymmetries in trade patterns between regions of different countries that share a national border" (Word Finder, n.d.). While current literature studying general border patterns do exist, they are quite broad. Differentiating from existing studies, this study will only focus on bilateral petroleum trade volumes between two countries – Canada and the United States – which have many similarities in culture, language, and institutions. This study will estimate border effects in the petroleum industry using data from 2013. This study will explore the effects of crude oil pipelines on bilateral petroleum trade volumes between Canada and the United States and explore whether border effects are weaker if regions are connected with pipelines. I hypothesize that the existence of an international crude oil pipeline reduces border effects significantly and increases bilateral trade as it reduces transportation costs.

Existing studies show findings about border effects and international trade while observing a broad range of commodities. It may be interesting to explore whether petroleum products have the same border effects compared to other commodities if excessive infrastructure such as crude oil pipelines exists. Pipelines can cause a significant trade advantage compared to other modes of transportation, thus potentially introducing bias to the findings of existing empirical studies.

Studying the relationship between crude oil pipelines and trade volumes in North America is also a crucial topic as Canada and the United States are strategic trade partners in the petroleum industry and the Canadian economy highly relies on the US oil trade. 98% of Canadian crude oil exports are to the United States, which accounts for 48% of US crude oil imports (Natural Resources Canada, 2020). Currently, there are ongoing controversial political discussions in both Canada and the United States regarding new pipeline proposals, such as the Keystone XL pipeline extension project. This study will provide an additional empirical resource to the ongoing controversial political discussions about the effects of crude oil pipelines on international trade volumes and existing border barriers.

2. LITERATURE REVIEW

Gravity theory was first introduced by Jan Tinbergen in 1962, linking the connection between bilateral trade volumes and distance. There is a variety of literature on gravity theory and border effects in Canada and the United States. One of the pioneering and most influential studies investigating border effects in the context of Canada and the United States was done by McCallum (1995). Using data from 1988, McCallum showed that the U.S.-Canada border reduced cross-border bilateral trade by 22 times compared to trade between Canadian provinces. Anderson and Smith (1999) observed border effects on a more micro-scale and showed that border effects varied across Canadian provinces.

Further investigation of these results explored a constant elasticity of substitution gravity model. Anderson and van Wincoop (2001) showed that McCallum's findings were overestimated and the U.S.-Canada border reduces trade to a factor of 5.2. Feenstra (2002) compared Anderson and van Wincoop's computational method of computing border effects to using country fixed effects and showed that the fixed effects approach gives consistent estimates with the structural approach. These three studies used a log transformation of an OLS regression to estimate border effects.

Using improved data sources, Ishise and Matsuo (2015) re-estimated the border effects using 1993 data both with a fixed effects model and structural model and showed that the actual border effects were lower than what Anderson, van Wincoop, and Feenstra estimated. By comparing data from 1993 and 2007, Ishise and Matsuo also showed that border effects have decreased by approximately 22% through the years but still exist. Using a Poisson Pseudo-Maximum Likelihood estimator, Suvankulov (2015) extended existing studies to a more recent and extensive time period between 2001-2010. In addition to U.S.-Canada trade, the study included Canada's trade with European Union and G-20 countries to estimate border effects.

Differentiating from the existing literature, this study will not suggest a new way to investigate border effects; instead, it will use Feenstra's fixed effects approach as a baseline, and extend it using a Poisson Pseudo-Maximum Likelihood estimator and a more specific dataset. Additionally, there may be a gap in existing research regarding a focus on border effects in relation to petroleum products and pipelines. Unlike analyzing general border patterns, this study is unique as it will only focus on specific types of infrastructure, pipelines, and commodities: petroleum products.

3. DATA SUMMARY

3.1 Data Source and Scope

The primary data set of this research is formed from a combination of interprovincial trade, interstate trade, province-state trade, gross domestic product (GDP) of Canada, GDP of the U.S. GDP, pipeline locations[1], and distance data sets. In this study, an observation is defined to be the total value of petroleum products measured in Canadian dollars traded from a region of origin to a region of destination in 2013. Regions are specified as 8 Canadian provinces[2] and 32 U.S. states[3], adding to a total of 40 regions. Canadian regions include 6 provinces with a connection to a major crude oil pipeline and railroad, and 2 provinces with only a railroad connection. The 32 states include most of those bordering Canada, the top five major oil producing and consuming states, and states connected to a major pipeline with certain exceptions. These exceptions are discussed in the Data Limitations section.

Key variables in the final dataset include region of origin, region of destination, trade volumes measured Canadian dollars, distance between two trading regions measured in kilometers, GDP from the region of origin and region of destination, importer dummy, exporter dummy, and pipeline dummy. Interprovincial pipeline crude oil trade data is from interprovincial and international trade flows of Statistics Canada; interprovincial crude oil trade by rail and truck transportation is from Canadian Freight Analysis Framework of Statistics Canada; province-state crude oil trade is from Canadian International Merchandise Trade Database of Statistics Canada; interstate crude oil trade data is from Freight Analysis Framework of Bureau of Transportation Statistics (BTS) and Federal Highway Administration (FHWA); GDP data for Canadian provinces is from Statistics Canada; GDP data for US states are from the Bureau of Economic Analysis; and midpoint coordinates of regions are obtained from https://www.maps.ie/coordinates.html. The unit of currency of the dataset is Canadian dollars. All monetary values from US based sources are converted into Canadian dollars using 2013's average exchange rate, 1.0302.

3.2 Data Selection

The major limitations of this data set are the limited bilateral trade volumes between some provinces and states. Since not every state trades petroleum products with every province, among the

1. Refer to Appendix 9.2 for list of Pipelines.
2. Refer to Appendix 9.2 for list of Canadian Provinces
3. Refer to Appendix 9.2 for list of US States

1600 observations, 25.94% of the observations have zero bilateral trade volume with each other. In order to increase the number of observations, the time frame of the dataset is chosen to be 2013. Compared to more recent years, 2013 corresponds to a year with more bilateral trade volumes, thus representing a year with a higher number of valid observations. It can be acknowledged that choosing a peak year can possibly bias results; however, it is chosen to make the dataset more reliable. On the other hand, the first three phases of the Keystone Pipeline — one of the major pipelines between Canada and the United States — started its operations before 2013. Thus, this year was also chosen to include this pipeline in the dataset.

3.3 Data Limitations

Due to limited and missing interprovincial marine and air transportation data, only pipeline, rail, and truck transportation of petroleum products will be included in the Canadian interprovincial trade data. However, marine transportation will be included in both interstate and cross-border trade between Canada and United States data, limiting the validity of the dataset. Marine transportation in Canada accounts for a large portion of overseas trade; however, due to a huge separation between Western and Eastern Canada, marine transportation does not account for a significant portion of within-country trade. Therefore, it can be argued that not including within-country marine transportation does not introduce a significant bias to my dataset. I will further test this assumption in my robustness analysis.

Distance between regions is calculated with the great circle distance methodology using the coordinates of the midpoints of every region. This is a popular approach also used by both Anderson and van Wincoop (2001), Feenstra (2002), as well as many other existing studies. One major limitation of this approach is that it does not use the exact coordinates of the corresponding trade locations and does not take into account the actual travel distance.

Exempted states from the dataset are Alaska, Florida, Idaho, Vermont, and Wyoming. Marine transportation is the main form of petroleum transportation in Alaska and Florida; therefore, these states are removed in order not to introduce bias to this study's estimates. Idaho, Vermont, and Wyoming are exempted due to limited bilateral trade volumes with Canadian provinces.

Interstate and interprovince bilateral trade data will only focus on 4 specific commodities which are suitable for both land and pipeline transportation. Classified in the Standard Classification of Transported Goods (SCTG) codes, these commodities are: 16 - Crude

Petroleum, 17 - Gasoline and Aviation Turbine Fuel, 18 - Fuel Oils, and 19- Products of Petroleum Refining n.e.c. and Coal Products.

On the other hand, cross border bilateral trade data are classified with the HS codes: 270900 (petroleum oils and oils, obtained from bituminous minerals, and crude), 271012 (light oils obtained from petroleum bitumen, crude, biodiesel, and waste), and 271019 (oils from bituminous minerals, not containing biodiesel, crude, light, waste containing by weight 70%) which accounts for a significant portion of the SCTG codes 16, 17, 18, 19, but does not cover all which is another limitation of the dataset.

3.4 Descriptive Statistics

Summary Statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
Value (CAD)	1,600	1.52e+09	1.83e+10	0	6.81e+11
Distance (km)	1,600	1762.192	987	0	4669
Origin GDP (CAD)	1,600	3.82e+11	4.49e+11	2.92e+10	2.33e+12
Destination GDP (CAD)	1,600	3.82e+11	4.49e+11	2.92e+10	2.33e+12

The dataset contains 1600 observations. 15.44% of the observations have a pipeline connection[4], 32% of the observations account for cross-border trade[5], and 25.94% of the observations have a bilateral trade value of zero[6]. The mean bilateral trade value between regions is 1.52e+09 with a maximum value of 6.81e+11 and a minimum value of 0 measured in Canadian dollars. The average distance between two regions is 1762 km and the maximum distance between two regions is 4668 km.

The data shows a strong negative correlation between distance and trade volumes. As the distance between two regions increases, the corresponding bilateral trade volume decreases; however, within-country trade tends to be less affected by distance compared to cross border trade. While this relationship is true for both regions connected with a pipeline and regions not connected with a pipeline, the slopes for regions connected by pipelines are less steep, and the corresponding trade volume per distance is higher on average This shows that pipeline-connected regions may be less affected by distance.

4. Refer to Figure 1 in Appendix 9.3 for breakdown of Pipelines
5. Refer to Figure 2 in Appendix 9.3 for breakdown of Type of Trade
6. Refer to Figure 3 in Appendix 9.3 for breakdown of Bilateral Trade

Figure 1
Relationship between Distance and Trade Value by Type of Trade

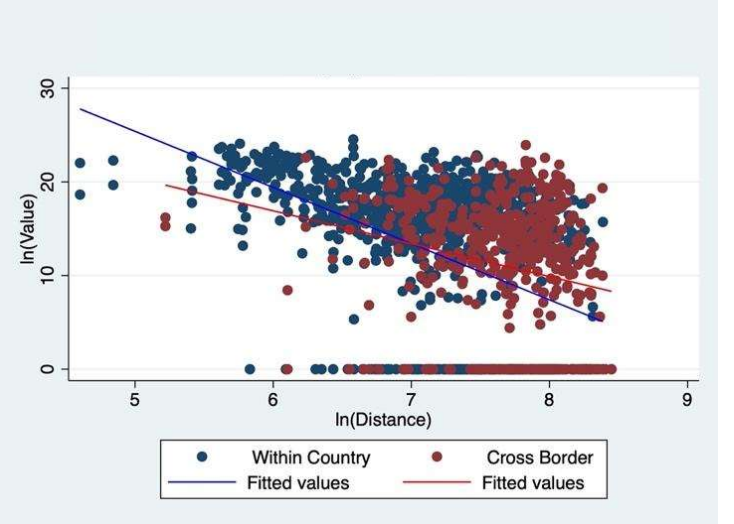


Figure 2
Relationship between Distance and Trade Value by Pipeline

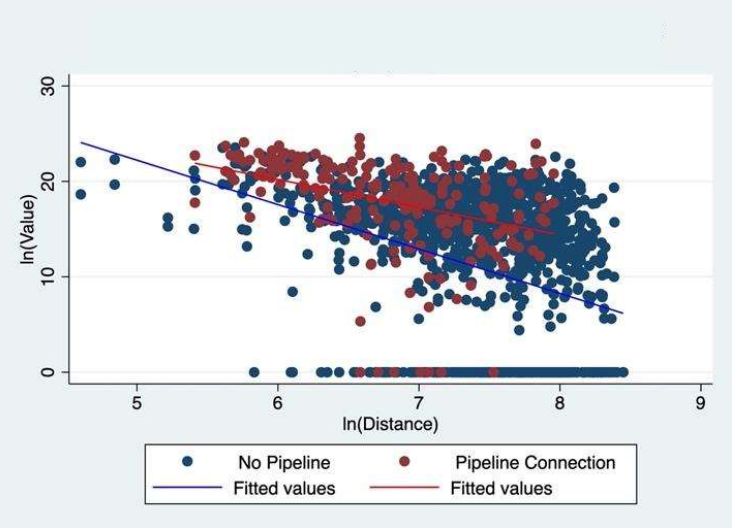


Figure 3
Relationship between Distance and Trade Value by Cross Border Trade

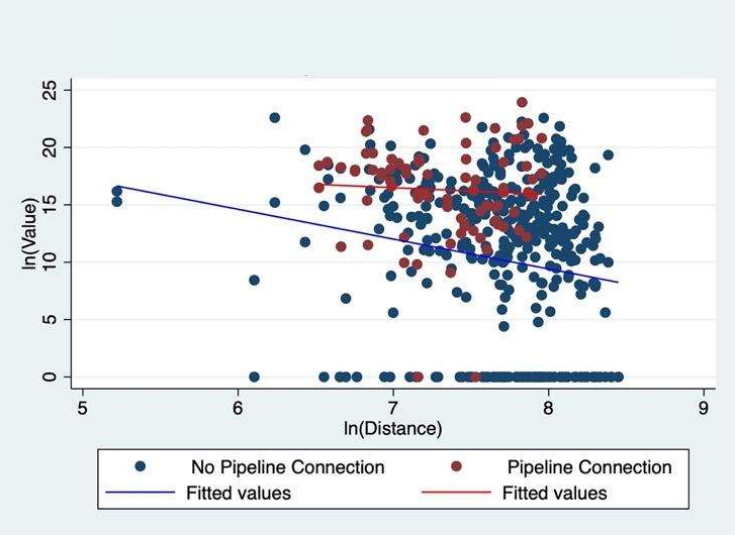
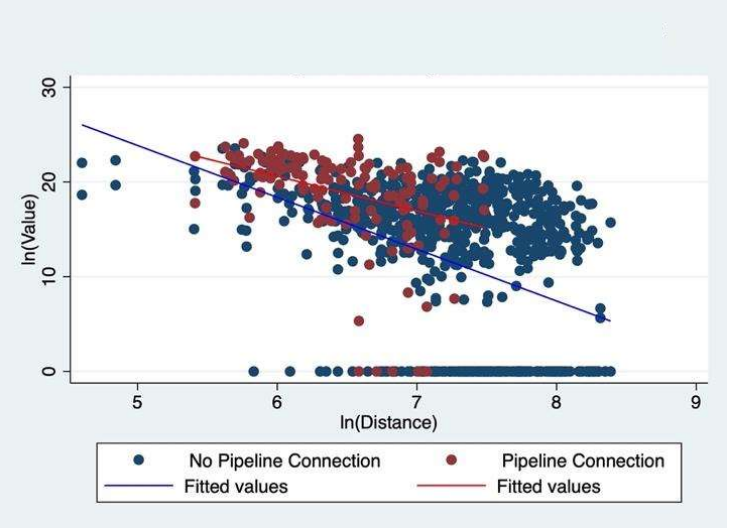


Figure 4
Relationship between Distance and Trade Value by Within Country Trade



The data shows that bilateral petroleum trade volume between pipeline connected regions tend to be higher both in cross border and within country trade. In cross border trade, the average trade volume between pipeline connected regions is significantly higher than other regions which suggests that pipelines reduce border effects. Additionally, very few cross-border pipeline connected regions have zero trade with each other while a significant amount of cross border regions with no pipeline connection have zero trade with each other. On the other hand, the same correlation between pipelines on bilateral trade volumes is also present in within-country trade, but these estimates are less significant compared to cross-border trade.

4. EMPIRICAL STRATEGY

This study will take Feenstra's (2002) fixed effects approach that adds exporter and importer dummies to the regression as a baseline to determine the general border effects in the petroleum industry in 2013. Feenstra showed that the fixed effects approach gives consistent estimates of the average border effect with the structural approach used by Anderson and van Wincoop (2001). The fixed effects approach will be the preferred empirical method to address importer and exporter characteristics for this study.

Taking Feenstra's regression as a baseline, I will first run a log transformation of an ordinary least squares regression to determine the general border effects in the petroleum industry in 2013. When there is no bilateral trade between two regions, the reported trade volume in the dataset is zero. Running a log transformation of an OLS regression drops observations corresponding to zero bilateral trade volume, thus causing selection bias. Since 25.94% of my observations have a bilateral trade value of zero, to prevent observations from getting dropped, I resolve this by adding 1 to all trade values. Feenstra's regression is shown below and the results of the regression are represented in the first 2 columns of Table 1.

$$ln\ ln\left(\frac{x_{ij}}{y_i y_j}\right) = \alpha_1 ln\ distance_{ij} + \alpha_2\ border_{ij} + \alpha_3 export_i + \alpha_4 import_j + (1-\sigma)\varepsilon_{ij}$$

Where:

x_{ij}: Exports from region i to region j

y_i and y_j: GDP of regions i and j respectively

$distance_{ij}$: Distance from region i to j

$border_{ij}$: Dummy variable = 1 for cross border trade and zero otherwise

$export_i$: Exporter Dummy = 1 if region i is the exporter, and zero otherwise

$import_j$: Importer Dummy = 1 if region j is the importer, and zero otherwise

This study will extend Feenstra's regression by adding a pipeline dummy variable to the regression to measure the effect of crude oil pipelines on existing border effects. The pipeline dummy variable will also interact with the border dummy to make the model more reliable and the following regression is formed. The results of this regression are represented in column 3 of Table 1.

$$ln\ ln\left(\frac{x_{ij}}{y_i y_j}\right) = \alpha_1 ln\, distance_{ij} + \alpha_2\, border_{ij} + \alpha_3 import_i + \alpha_4 export_j + \alpha_5 pipeline_{ij} + \alpha_6(\, border_{ij} \times pipeline_{ij}) + (1-\sigma)\varepsilon_{ij}$$

Flowerdew and Aitkin (1982) showed that replacing zero bilateral trade flows with a small positive number creates inaccuracies. As an alternative approach to running an OLS regression, Santos Silva and Tenreyro (2006) showed that running a Poisson Pseudo-Maximum Likelihood (PPML) estimator with fixed effects does not drop observations and prevents sample selection bias. Feenstra's approach will be improved by using a PPML estimator instead of an OLS regression to increase the robustness of the results. I will use Santos Silva and Tenreyro's PPML command because it can be argued that PPML is a better estimator of the causal effect of pipelines on bilateral trade volumes and the following regression is formed. The results of this regression are shown in column 4 of Table 1.

$$x_{ij} = \alpha_1 ln\, y_i + \alpha_2 ln\, y_j + \alpha_3 ln\, distance_{ij} + \alpha_4\, border_{ij} + \alpha_5 import_i + \alpha_6 export_j + \alpha_7 pipeline_{ij} + \alpha_8(\, border_{ij} \times pipeline_{ij}) + (1-\sigma)\varepsilon_{ij}$$

5. EMPIRICAL RESULTS

The first column of Table 1 represents Feenstra's fixed effects regression using 2013 data and petroleum products. All five columns of Table 1 use robust standard errors and all regressions are clustered by distance. Running a log transformation of an ordinary least squares regression drops 415 observations corresponding to zero bilateral trade volumes and the sample size of column 1 is 1185. Using Feenstra's regression, the results show a strong negative relationship between bilateral trade volumes and distance. The border coefficient is 1.442, and taking the exponential of the border coefficient, the average border effect between Canada and the United States using Feenstra's methodology is e1.442=4.229. This suggests that within-country petroleum trade is on average 4.23 times larger than cross border petroleum trade in 2013.

Table 1: 2013 Petroleum Industry Canada – US Border Effects

	(1) OLS	(2) OLS	(3) OLS	(4) PPML	(5) PPML
log GDPi	-0.404	6.981*	6.192*	0.290	0.332
	(1.36)	(2.73)	(2.82)	(0.23)	(0.20)
log GDPj	-1.706	-0.987	-1.777	0.607**	0.593**
	(1.34)	(2.67)	(2.72)	(0.22)	(0.19)
log Distance	-1.275***	-2.304***	-2.230***	-0.607***	-0.509***
	(0.26)	(0.65)	(0.62)	(0.02)	(0.03)
Border	-1.442***	-1.289**	-1.521***	-0.456	-0.315
	(0.26)	(0.41)	(0.43)	(0.29)	(0.27)
Pipeline			0.659		2.099***
			(0.60)		(0.29)
Border x Pipeline			1.179		-0.230
			(0.92)		(0.53)
Constant	79.969	-127.777	-87.058	0.167	-0.610
	(58.90)	(114.51)	(118.61)	(5.51)	(5.22)
Border Effect [a]	4.229	3.639	4.577	1.578	1.370
Pipeline Effect [b]			1.933		8.158
R^2	0.564	0.552	0.555	0.986	0.989
Observations	1185	1600	1600	1600	1600

Notes:
Standard errors in parenthesis
[a] Computed as the exponent of the absolute value of the border variable
[b] Computed as the exponent of the pipeline variable
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Column 2 represents Feenstra's regression with 1 added to all trade values, and the number of valid observations increases from 1185 to 1600. Therefore, it can be argued that regression 2 gives a more accurate estimate. Column 2 suggests a more significant negative correlation between distance and trade volumes. A 1% increase in distance leads to a 2.3% decrease in bilateral trade value. Even though there is still a very significant border effect present, regression 2 suggests a less significant negative relationship between borders and trade volumes. The border effect is reduced from 4.229 to 3.639 and is statistically significant at the 1% level.

The initial effects of crude oil pipelines on existing border effects are shown in column 3. Observations with zero bilateral trade volumes are handled in the same way in this regression. Including all 1600 observations and adding a pipeline dummy does not change the explanatory power of the regression, and R-squared remains in the 0.55-0.56 range. The results contradict my initial hypothesis. With the existence of crude oil pipelines, the absolute value of the border coefficient increases to 1.521 and is statistically significant at the 0.1% level. On the other hand, the pipeline coefficient is 0.659 and both the pipeline coefficient and the interaction term are not statistically

significant, suggesting that pipelines may not have a significant effect on borders. However, it can be argued that this could be caused by the limitations of the dataset, making the results potentially biased.

To further test these results, my final results are shown in column 5 of Table 1 where I ran a Poisson Pseudo-Maximum Likelihood (PPML) estimator with fixed effects instead of an OLS regression to increase the robustness of the results. The sample size remains at 1600 and no observations are dropped. Compared to columns 1-3, the R-squared value in column 4 increases from 0.55 to 0.99. This shows that running a PPML estimator increases the explanatory power and accuracy of the regression, as the data now fits almost perfectly compared to the regression model. With the existence of pipelines, the distance coefficient is reduced from -2.23 to -0.51 but is still statistically significant and suggests a strong negative relationship between distance and bilateral petroleum trade values.

Results suggest that other factors held equal, petroleum trade values between regions connected with a pipeline are 8 times higher than regions not connected with a pipeline, and the pipeline coefficient is statistically significant. Controlling for pipelines, the Canada-U.S. border leads to an increase of within-country trade to a factor of 1.37 times higher than cross border trade. The existence of crude oil pipelines reduces border effects from 1.578 to 1.370; however, contradicting my hypothesis, the pipeline interaction term is not statistically significant, which suggests that pipelines do not have a significant effect on border effects.

In comparison, as shown in column 4 of Table 1, running a PPML estimator without the pipeline variable and interaction term does not change the statistical significance of the border coefficient to a great extent. The absolute value of the border coefficient rises from 0.315 to 0.456, which shows that borders reduce bilateral trade to a factor of 1.578 and the border effect in the petroleum industry in 2013 is relatively lower than general border effects. It can be seen that even though the interaction term is not statistically significant, pipelines do have a visible effect on cross-border trade.

It may be possible that the limitations of my dataset impacted the interaction term to be statistically insignificant and the coefficient may be inaccurate. Among the 1600 observations, only 82 (5.12%) of the observations simultaneously account for both cross border and pipeline trade. There is a strong possibility that the insignificance of the interaction term is caused by the small sample size. The differences and unmatching of the SCTG and HS commodity classification codes in comparing within country and cross-border trade might be another limitation causing the insignificance of the interaction term.

It can be concluded that borders between Canada and the United States still have a negative effect on bilateral trade volumes; however, border effects are less significant in 2013's petroleum trade. Even though the existence of crude oil pipelines reduces border effects and increases petroleum trade volumes significantly, results show that the effect of pipelines on border effects is not statistically significant.

6. ROBUSTNESS

6.1 Landlocked Regions

To test the robustness of my results, I will be using a more specific sample from my dataset. Since marine transportation was not included in the Canadian interprovincial trade data, I will be only using landlocked states to fully eliminate marine transportation from my dataset. Table 2 is constructed using the PPML regression that was used in column 5 of Table 1.

Table 2: Landlocked Regions

	PPML
Border	-0.548
	(0.33)
Pipeline	2.029***
	(0.31)
Border x Pipeline	-0.470
	(0.64)
R^2	0.948
Observations	676

Standard errors in parenthesis
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The sample size decreases from 1600 to 676 and the R-squared decreases from 0.99 to 0.95. Despite these changes, both the magnitude of the new border and pipeline coefficients are very similar and the statistical significance does not change. Using this new sample, the absolute value of the border coefficient increases from 0.315 to 0.548, and the pipeline coefficient drops from 2.099 to 2.029. It can be concluded that even though there are minor changes, results are consistent with my estimates using only landlocked states.

6.2 Economic Size of Regions

To further test the robustness of the results, I divided the sample into two groups based on the GDP of the observed regions. Table 3 is constructed with a smaller dataset representing only the bilateral trade between regions with a GDP greater than 300 billion dollars and Table 4 represents the bilateral trade between regions

with a GDP less than 300 billion dollars. Both Table 3 and 4 are constructed using the same PPML regression used in column 5 of Table 1.

Table 3: Regions with Large Economic Size

	PPML
Border	-0.269
	(0.49)
Pipeline	1.533***
	(0.40)
Border x Pipeline	0.393
	(0.76)
R^2	0.997
Observations	289

Standard errors in parenthesis
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Regions with Small Economic Size

	PPML
Border	-0.315
	(0.53)
Pipeline	2.019***
	(0.54)
Border x Pipeline	-1.366*
	(0.71)
R^2	0.939
Observations	529

Standard errors in parenthesis
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Comparing regions with a relatively larger economic size to my primary sample, it is seen that the absolute value of the border coefficient drops from 0.315 to 0.269 and the pipeline coefficient drops from 2.099 to 1.533. Despite this decrease, the statistical significance of both the border and pipeline coefficient does not change. This shows that on average, bilateral trade between larger economic sized regions are less affected by borders and the border effects are not significant.

On the other hand, observing regions with a relatively smaller economic size, Table 4 shows that while the border and pipeline coefficients are consistent with my estimates, the statistical significance of the interaction term changes. The absolute value of the border and pipeline interaction term increases from 0.230 to 1.366 and is now statistically significant at the 5% level. Observing small economic sized regions, it is seen that pipelines have a significant effect on reducing border effects, but this relationship is not true for larger economic sized regions.

7. CONCLUSION

The relationship between crude oil pipelines and international trade is a crucial topic to study in today's world. Although there are many controversial opinions regarding pipeline proposals both in Canada and the United States, very few empirical studies are available. This study estimates the 2013 petroleum industry border effects between Canada and the United States and explores whether border effects are weaker if regions are connected with crude oil pipelines. To examine the relationship between pipelines and border effects, a province and state level bilateral trade dataset including 8

Canadian Provinces and 32 US States is used.

It can be concluded that using petroleum trade data from 2013, borders between Canada and the United States still have a negative effect on bilateral trade volumes. However, border barriers for the petroleum industry are much weaker compared to general border effects. While pipelines increase bilateral trade by 8.2 times, borders reduce cross border trade by 1.4 times compared to within country trade. Pipelines introduce a significant trade advantage by reducing transportation costs, and it is seen that pipelines are estimated to have a very significant positive effect on both within country and cross border petroleum trade. The existence of crude oil pipelines may reduce border effects, but results suggest that the effect of pipelines on border effects was not statistically significant. Testing the robustness of the results, when I ran the regression using only landlocked states, my estimates were consistent. However, when I divided the sample into two subgroups by the GDP of their regions, results showed that pipelines may have a significant effect on reducing border effects in small economic sized regions, but not in larger economic sized regions.

One major limitation of the dataset that could potentially cause the inaccuracies of the border and pipeline interaction term was the relatively small sample size. For future research to provide more reliable results for the causal interpretation of the effect of crude oil pipelines on border effects, the time scope can be extended to a series of years. This will not only increase the sample size but also strengthen the data validity caused by yearly oil price fluctuations. Additionally, to have a better estimate of the petroleum industry border effects in Canada, the geographical scope of the dataset can be extended to Canada's other trading partners for future improvement.

The Impact of COVID-19 on Malaria Infection and Mortality in Burkina Faso

Lorena Edah

ECON 490

ABSTRACT

This paper uses a difference-in-difference method and panel data from 2010 to 2020 to quantitatively assess the impact of the COVID-19 pandemic on district-level malaria outcomes in Burkina Faso. I hypothesize that COVID-19 will have an adverse impact and lead to a resurgence in malaria cases and deaths due to the diversion of resources and an overwhelmed health system. While the results do support the hypothesis and suggest a slight diversion of health resources from malaria, districts affected by COVID-19 saw a 36% more decrease in malaria cases and no average change in malaria deaths, indicating an ambiguous effect. Further research in this area can reveal greater insight on the impact of the COVID-19 pandemic in malaria-endemic countries.

1. INTRODUCTION

Malaria is one of the leading viruses affecting mortality rates and development outcomes in Sub-Saharan African populations. It is a huge health concern, thus a lot of resources have been invested into eradicating malaria and great strides have been made (Kouyaté et al., 2007), particularly between 2000 to 2015. A massive upscale of malaria control interventions, including long-lasting insecticidal nets and effective treatment of clinical malaria cases, led to a 60% reduction in malaria cases and deaths in Sub-Saharan Africa (Wilde et al., 2019). However, despite improvement in malaria prevention, testing, and treatment, malaria remains one of the leading causes of mortality, with more than 1 million annual deaths on the continent (Kouyaté et al., 2007).

To further complicate morality and development outcomes, on March 12th 2020, the World Health Organization declared a pandemic due to the COVID-19 virus which originated in Wuhan, China and soon spread throughout the world at an exponential rate. To stop the spread of COVID-19, national governments and international organizations adopted varying measures including but not limited to lockdown, social distancing, and self-isolation, all of which drastically restricted activities, altering the nature of daily life globally. The health measures taken in Sub-Saharan Africa had to additionally account for less adequate health-systems, resource limitations, and greater morbidity and mortality from other infectious diseases, namely malaria (Assefa, 2021).

As such, while preventing the spread of COVID-19 is a priority, there is reason to be alarmed about the potential adverse impacts to historical efforts to control malaria in

Sub-Saharan Africa. The research question that this paper addresses is: what is the effect of COVID-19 on the number of malaria infections and mortalities in Burkina Faso? In this manner, the treatment variable is defined as the COVID-19 pandemic while the outcome variables observed are the number of malaria infections and mortalities. I hypothesize that COVID-19 will have a causal impact on both malaria cases and deaths, leading to a resurgence in both scenarios due to the diversion of resources from malaria prevention and control to addressing COVID-19 and an overwhelmed health system.

This paper is structured as follows: the next section provides context on the area of study, the outcomes of interest, and the COVID-19 treatment variable as it pertains to the area of study. Following that is a section which highlights the theoretical framework through a literature review of studies on which this research is founded to situate my paper's unique contributions to this

research field. The rest of the paper focuses on the empirical work, including an explanation of the data used, the empirical strategy, and an interpretation of the results. Finally, the paper concludes with a summary of the findings of this research, discusses data limitations, and addresses opportunities for future research.

2. CONTEXTUAL BACKGROUND

2.1 Area of Study: Burkina Faso

Burkina Faso is a landlocked country located in West Africa bordering Mali, Niger, Benin, Côte d'Ivoire, Ghana, and Togo. The importance of understanding the distribution of Burkina Faso's population as a whole is well-depicted (Echenberg et al., 2021). In Burkina Faso, the population is unevenly distributed among the different regions. There are a total of 13 regions and 70 health districts in the country. The eastern and central regions are densely settled and contain about half of the total population. These areas can be characterized as the urban areas of Burkina Faso. The capital, Ouagadougou, is located in the central region and it is the most densely populated area as it is in the center of the country. Aside from Ouagadougou, the principal towns are Bobo Dioulasso, Koudougou, Banfora, Ouahigouya, Pouytenga, and Kaya. In the remaining regions the population is scattered, many living in rural areas in small villages. Moreover, Burkina Faso's yearly population is very young with more than two-fifths of the population under the age of 15 in 2021. This is particularly relevant given that the incidence of malaria is particularly worse for individuals of a young age (Wilde et al., 2021). Finally, the country can be characterized as low-income because about 90% of the population is engaged in subsistence agriculture or raising livestock. These difficult economic conditions are made worse by severe intermittent droughts and political instability due to terrorism, providing insight into why Burkina Faso is severely resource-limited particularly when it comes to healthcare.

2.2 COVID-19 in Burkina Faso

As of December 8th 2021, there have been a total of 16,000 confirmed cases and 286 deaths due to COVID-19 in Burkina Faso (Johns Hopkins, 2021). Compared to other countries such as Canada, the United States, Italy, India, or China, Burkina Faso' COVID-19 case and death numbers seem relatively insignificant, however Burkina Faso has experienced one of the highest death rates in Sub-Saharan Africa. Resources such as the COVID-19 Summary Update on Burkina Faso (2020) depict the evolution of the COVID-19

situation in Burkina Faso. The first COVID-19 case in Burkina Faso was recorded on March 2nd 2020, and at the time only one central teaching hospital (CHR) had the capacity to treat COVID-19 patients. Following the rapidly growing impact of the virus, the government undertook a series of actions to mitigate the impact of the disease. On March 10th 2020, the government established the first COVID-19 treatment center in Ouagadougou and first testing laboratory (RT-PCR) in Bobo Dioulasso. Then, on March 20th, 2020, lockdown measures ensued with President Kabore closing airports and borders and implementing a nationwide curfew. While all 13 regions of Burkina Faso were affected by the pandemic, the capital Ouagadougou remained the hotspot of the outbreak.

Overall, the government used a combination of centralized and decentralized approaches to curb the pandemic. The government established a series of response committees and task forces as part of a multisectoral response, developed an official COVID 19 response plan, allocated funds from national and international partners into a single account, and delegated COVID-19 testing (rapid diagnostic tests) and treatment to district hospitals.

2.3 Malaria in Burkina Faso

Malaria is a virus that occurs in many areas of the world. However, despite malaria's global distribution, approximately 90% of the estimated 300 to 500 million new clinical cases of malaria annually occur in Sub-Saharan Africa, making malaria a major health issue on the African continent (Goodman et al., 2000). The Global Forum for Health Research has provided insight into the impacts of malaria in the Sub-Saharan African region. Malaria is usually caused by the Plasmodium falciparum infection which is particularly devastating to children under the age of 5 and pregnant women. Malaria accounts for the deaths of about 1 million children per year or 25% of all childhood deaths, making it the most important infectious disease in children in Sub-Saharan Africa. Similarly, pregnant women can easily contract the virus because the placenta is a preferential site for parasite development and during pregnancy women have an already weakened immune system. There are severe consequences of malaria for pregnant women such as death, anemia, hypoglycemia, premature delivery, and low birth weight.

In Burkina Faso, malaria is endemic, meaning it is a common phenomenon throughout the country. Moreover, in Burkina Faso, malaria generally coincides with seasonal changes. Many cases occur between June to October due to the rainy season (up to three months in the north, six months in the center, and nine months in the south of the country). Overall, Burkina Faso is among the ten countries

with the highest number of malaria cases and deaths (3% of global cases and 4% of global deaths). 43% of health provider consultations and 22% of deaths in the country are attributed to malaria in Burkina Faso. The go-to treatment in these circumstances for severe malaria in public health facilities is intravenous artesunate or injectable quinine, a chemical that has historically proven to be effective against malaria. Another means for prevention and raising public awareness on malaria is the use of education campaigns, a method that will be observed and discussed in the results section.

3. LITERATURE REVIEW

This paper draws on a broader literature of malaria and development outcomes. Historically, malaria has been prominent on the African continent. However, as Goodman et al. (2000) depict, in 1998 the World Health Organization (WHO) identified malaria as a priority project with the announcement of Roll Back Malaria. The goal of the initiative was to reduce the global malaria burden by focusing on high transmission areas such as Africa, the donor community, and other affected countries. Ever since, various development economists have undertaken research to explore the best measures to help eradicate malaria in Sub-Saharan Africa.

One well-known literature in this domain includes the paper by Cohen and Dupas entitled "Free Distribution Or Cost-Sharing? Evidence From a Randomized Malaria Prevention Experiment" (2010). In this paper, Cohen and Dupas use a field experiment in Kenya in which they randomize the price of anti-malarial insecticide-treated nets (ITN) in prenatal clinics to assess price mechanisms for ensuring efficient usage of bed nets to reduce malaria outcomes in pregnant women. The authors find that the free distribution of anti-malarial insecticide-treated nets has the potential to save a greater number of lives compared to programs that charge a positive price for ITNs. Moreover, the authors point to the large positive externality associated with widespread usage of ITNs which will ultimately outweigh any cost of distributing the bed nets for free. One cannot discuss malaria outcomes without mentioning this very influential paper which is one among many that have tried to find ways to combat this endemic virus that has plagued Sub-Saharan African countries for centuries.

Moving beyond general malaria and development outcome literature, this paper also draws on literature associated with the West African Ebola epidemic as there are revealing commonalities between epidemic and pandemic circumstances; pandemics can be like an exacerbated version of an epidemic. The West African Ebola

epidemic started in the forested rural region of southeastern Guinea and the first case was reported by the WHO on March 23rd 2014 (Centers for Disease Control and Prevention, 2021). This marked the beginning of a rapidly growing disease that later affected Congo, Liberia, and Sierra Leone in addition to Guinea. Silhol et al. (2021) estimated the number of cases and deaths from malaria in Guinea, Liberia, and Sierra Leone from Demographic and Health Surveys data for malaria prevalence and coverage of malaria interventions before the Ebola outbreak. The authors find that the ongoing Ebola epidemic in parts of West Africa largely overwhelmed healthcare systems in 2014, making adequate care for malaria impossible and threatening the gains in malaria control achieved over the past decade. Their results indicate a potential malaria case increase of 45% in Guinea, 88% in Sierra Leone, and 140% in Liberia in 2014 due to the Ebola outbreak. This increase is equivalent to 3.5 million additional untreated cases, with 10,900 additional malaria-attributable deaths.

In a study which uses methodology similar to my paper's affected-unaffected districts method, Plucinski et al. (2015) conducted a cross-sectional survey of public health facilities in Guinea of four selected health prefectures most affected by Ebola virus disease and four randomly selected health prefectures without any reported cases of the disease. The study noted substantial overall reductions in all-cause outpatient visits in all health prefectures. However, in Ebola-affected prefectures, 73 of 98 interviewed community health workers were operational and 35 of 73 were actively treating malaria cases compared with 106 of 112 and 102 of 106, respectively, in Ebola-unaffected prefectures. Here, the reduction in the delivery of malaria care because of the Ebola-virus-disease epidemic threatened malaria control in Guinea. Untreated and inappropriately treated malaria cases led to excess malaria mortality and more fever cases in the community.

Overall, the data on the Ebola epidemic suggests potential adverse effects of the pandemic as outlined in my hypothesis. Very few studies have attempted to capture the impact of the COVID-19 on malaria outcomes, much less in a country such as Burkina Faso. Assefa et al. (2021) use a telephone survey in Burkina Faso, Ethiopia, and Nigeria to assess the effects of the pandemic on healthcare services from the perspectives of healthcare providers and community members. The authors' results indicate that more than half (56%) of essential health services were adversely affected by the COVID-19 pandemic. Furthermore, child health services experienced a higher percentage of interruption (33%) compared to maternal health services (31%). These findings also match my predictions of the negative impact of COVID-19 on malaria outcomes in Burkina Faso.

The comprehensive body of literature suggests that while malaria has been a very extensively researched area in development economics, the occurrence of a global pandemic allows for a gap in the malaria knowledge that calls for serious investigation. Data that quantifies the effect of the pandemic on other health conditions is scarce, especially in low-income countries. Consequently, using Burkina Faso as a case study, this paper uses a difference-in-differences method and panel data from 2010 to 2020 to assess the impact of the COVID-19 pandemic on district-level malaria outcomes. Furthermore, research such as this has immense significance for multiple reasons. First, analyzing the impact of COVID-19 on other factors makes this research relevant given the immense disruption to all daily activities as a result of the COVID-19 pandemic. Second, the findings can be very informative, bringing about important discussions that could shape how people perceive the impact of COVID-19 (and like circumstances) in countries dealing with other viral diseases. Third, in a global context, this research not only affects Burkina Faso or other African countries dealing with similar viral diseases such as malaria, but also external international partners who have direct involvement in African matters in the form of foreign aid or other indirect contributions.

4. DATA

4.1 Collection

4.1.1 Dependent Variables: Malaria Cases and Deaths

The data for malaria cases and deaths is obtained from the annual health statistical summaries from 2010 to 2020, publicly available on Burkina Faso's National Statistics Council website by the Ministry of Health. The annual health statistical summaries are created by aggregating the data in monthly reports from health facilities (public and private) and from hospital care and technical units. With this data source, I have encountered a challenge collecting data for 2019, as that year, health workers organized a massive strike demanding better pay and work circumstances (Garcia, 2019). The health workers thus held back data from the government as leverage and as a result the government was unable to produce an annual health statistical summary for that year. This has important implications for the results I derive in this research as 2019 is the year right before I observe the treatment effect, so my results could over attribute the change in malaria cases and deaths to the occurrence of COVID-19.

4.1.2 Independent Variable: COVID-19 Cases (Treatment)

The data reflecting which districts were and weren't affected by COVID-19 is obtained from a private platform called CORUS (Centre des Opérations de Réponse aux Urgences Sanitaires, with an English translation of Health Emergency Response Operations Center). CORUS is a department under Burkina Faso's National Institute of Public Health created in 2018 to provide leadership in the collective preparation, coordination and management of operations related to health risks and emergencies in the country. Due to the unprecedented nature and lethal impact of COVID-19, CORUS was tasked with assembling all the data related to this virus. I have been granted access to this platform with the help of my father who works in the health domain with various government and non-government partners and his connections. Through this platform, I was able to collect the amount of COVID cases in each district.

4.1.3 Other Variables:

All other variables used in this research analysis are also obtained from the annual health statistical summaries from 2010 to 2020. These variables include malaria cases for pregnant women, malaria cases for children under 5 years old, the number of malaria education campaigns, as well as annual district population. This data is publicly available on Burkina Faso's National Statistics Council website by the Ministry of Health. Similar to the issue with 2019 data collection of the dependent variables, 2019 data is unavailable for these variables.

4.2 Building the Dataset

In spreadsheet format, I manually entered the districts and years for which I had data (2010 to 2020, excluding 2019). Then, in separate columns, I entered annual district data on population, total malaria cases, malaria deaths, total malaria cases for pregnant women, total malaria cases for children under 5 years old, and number of malaria education campaigns. Some of the columns required minor additional manipulations as the data source separated malaria cases into simple and severe, so I added both to get the total numbers used for this analysis. For children under 5 years old, I added the data on children under 1 year old and children aged 1 to 4 to get the total number of malaria cases. Finally, using data from the CORUS platform, I entered the data on COVID-19 cases on the three affected districts and entered a value of zero for the remainder of the unaffected districts.

4.3 Manipulation of the Data

The main manipulation of data in this paper is the use of district per capita amounts of all the outcome variables used in the subsequent regression analysis. The per capita amounts were derived by dividing the number of malaria cases or deaths by the annual district population. I used per capita measures rather than raw numbers to normalize the distribution of the outcome variables, especially given that the noise in the data could be caused by the large discrepancies in population distribution between the treated (urban) and control (rural) groups. The use of natural log would have otherwise been appropriate, but given some zeros in the dataset, per capita measures provided the most valid results. Additionally, each per capita variable was multiplied by 1000 to magnify the point estimates post-regression.

4.4 Unit of Analysis

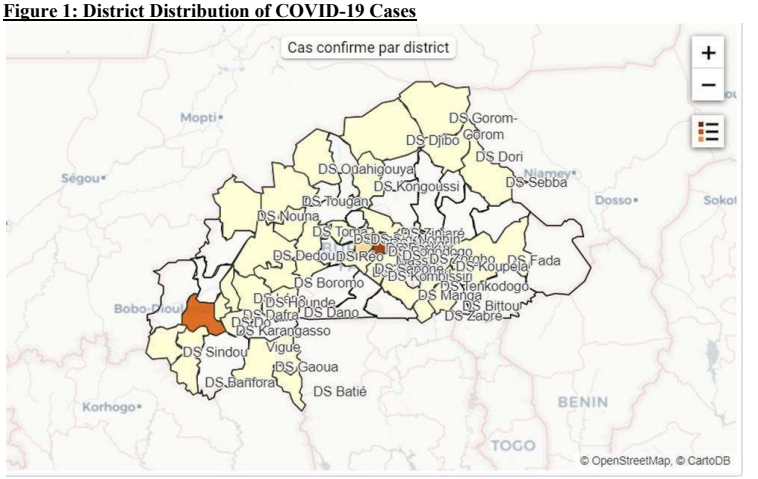
To attempt to attribute a causal impact to COVID-19, this study utilizes district-level units of analysis, consisting of both affected districts which amount to a total of three districts constituting the treatment group, and unaffected districts which comprise the control group. The treatment group includes DS Baskuy, DS Bogodogo, and DS Boulmiougou. The control group consists of DS Dande, DS Bousse, DS Diapaga, DS Boulsa, DS Barsalogho, DS Bogande. These control districts were randomly selected with the only criteria being that they did not have any officially reported COVID-19 cases as per the data.

Affected districts in Burkina Faso were situated in urban areas where the population is denser and allows for higher transmissions of highly infectious viruses such as COVID-19. Unaffected districts were more rurally situated where the population is scattered which allowed for little to-no transmission of the COVID-19 virus. All of this is illustrated by Figure 1 which shows the distribution of COVID-19 cases across the country by district. The brownish-orange areas are the treatment districts, and the yellow areas constitute the districts from which the control group is formed.

An additional consideration regarding the data's validity is the potential for underreporting of individuals who actually contracted COVID-19, given the negative social connotations associated with the virus. In Burkina Faso, discussions of contracting COVID-19 are made to be taboo, and this creates an environment in which individuals fear to acknowledge that they may have potentially contracted the virus and thus refuse to report it. This paper does not focus on these data limitations that are beyond the scope and control of this research.

are made to be taboo, and this creates an environment in which individuals fear to acknowledge that they may have potentially contracted the virus and thus refuse to report it. This paper does not focus on these data limitations that are beyond the scope and control of this research.

Figure 1: District Distribution of COVID-19 Cases



5. ECONOMETRIC FRAMEWORK AND METHODOLOGY

This study adopts a difference-in-differences empirical strategy where the effects of COVID-19 are varied over time (Pre-COVID and Post-COVID) across affected urban (treatment) and unaffected rural (control) districts in Burkina Faso. To carry out this difference-in-differences methodology quantitatively, the following key fixed effects regressions were designed and carried out for each outcome

1) $\text{Total Malaria Cases}_{it} = \beta_0 + \beta_1(\text{District})_i + \beta_2(\text{Post})_t + \beta_3(\text{District x Post})_{it} + \varepsilon_{it}$

2) $\text{Total Malaria Deaths}_{it} = \beta_0 + \beta_1(\text{District})_i + \beta_2(\text{Post})_t + \beta_3(\text{District x Post})_{it} + \varepsilon_{it}$

of interest:

In these fixed effects regressions, "Total Malaria Cases_{it}" and "Total Malaria Deaths_{it}" are my outcomes of interest in district (i) in year (t). "District" is a dummy variable indicating whether the district is part of the treatment group (=**1**) or the control group (=0). "Post" is a dummy variable for time, indicating the time of the intervention and differentiating between pre-pandemic (=0) and post-pandemic (=1) outcomes. District x Post is an interaction dummy variable indicating whether the outcome was observed in the treatment group and if it was observed after the intervention (=1), or any other cases (=0). $\boldsymbol{\varepsilon}_i$ is the residual term capturing all the other factors

influencing the outcomes that are unobserved. In this manner, β_0 represents the average outcome of the control group before the treatment. β_1 represents the difference between the treatment and the control group before the treatment. β_2 represents how much the average outcome of the control group has changed in the post-treatment period. β_3 is the coefficient of interest as it measures how much the average outcome of the treatment group has changed in the period after the treatment, compared to the counterfactual. The counterfactual in this case is the average outcome in the control group in the post-pandemic period assuming the parallel assumption holds. If $\beta_3=0$, the treatment may have no effect. Conversely, if β_3 is positive or negative, the treatment may have either increased or decreased the outcome of interest. Due to my hypothesis, the expectation with these regressions is that the coefficient of interest, β_3, will be positive, indicating an increase in the amount of malaria cases and deaths due to COVID-19.

In addition to the main regressions, the following additional

$$3)\quad \text{Malaria Education Campaign}_{it} = \beta_0 + \beta_1(\text{District})_i + \beta_2(\text{Post})_t + \beta_3(\text{District x Post})_{it} + \varepsilon_{it}$$

regression was also conducted:

This first additional regression estimates the impact of COVID-19 on the amount of malaria education campaigns conducted in COVID-19 urban affected districts. The purpose of this regression is to capture the change in malaria cases and deaths due to the diversion of resources from malaria prevention to COVID mitigation. Given that the main hypothesis in this paper predicts an increase in malaria cases and deaths due to the diversion of resources from malaria prevention to COVID mitigation, the purpose of this regression is to capture that change, if any, due to COVID-19. Here, the diversion of resources is measured by the change in the amount of malaria education campaigns. Again, this regression controls for district (unit) and year (time) fixed effects. I expect that β_3 will be negative, indicating a decrease in the amount of malaria education campaigns due to COVID-19.

Two additional regressions explore impacts for children under 5 years old and for pregnant women, as reflected in the following

$$4)\quad \text{Children} <5_{it} = \beta_0 + \beta_1(\text{District})_i + \beta_2(\text{Post})_t + \beta_3(\text{District x Post})_{it} + \varepsilon_{it}$$

$$5)\quad \text{Pregnant Women}_{it} = \beta_0 + \beta_1(\text{District})_i + \beta_2(\text{Post})_t + \beta_3(\text{District x Post})_{it} + \varepsilon_{it}$$

specifications:

These last two additional regressions estimate the impact of COVID-19 on malaria cases in specific groups of interests, children under 5 years old and pregnant women, that are most vulnerable to contracting and dying from malaria. Similar to the previous

regressions, these regressions control for district (unit) and year (time) fixed effects. Due to my hypothesis, the expectation with these regressions, like the main regressions, is that the coefficient of interest, β_3, will be positive which may indicate an increase in the amount of malaria cases in these specific populations due to COVID-19.

6. DATA ANALYSIS & INTERPRETATION OF RESULTS

Table 1 provides summary statistics by group (treatment or control) for all the variables used in this analysis. There are a total of 90 observations across the entire sample, of which 60 belong to the 6 control districts, and 30 belong to the treatment districts.

Table 1: Descriptive Statistics

Control Districts: DS Boulsa, DS Dande, DS Bousse, DS Diapaga, DS Barsalogho, DS Bogande

Variable	Obs	Mean	Std. Dev.	Min	Max
District	60	5.667	2.832	1	9
Year	60	2014.6	3.065	2010	2020
Population	60	291070.95	109484.83	150679	539953
Malaria Cases	60	4.01	1.191	1.889	6.672
Malaria Deaths	60	.002	.001	0	.005
Malaria Campaigns	60	.024	.016	0	.094
Children <5 Cases	60	2.205	.736	.877	4.169
Pregnant Women Cases	60	.176	.111	.004	.676
COVID-19 Cases	60	0	0	0	0

Treatment Districts: DS Baskuy, DS Bogodogo, DS Boulmiougou

Variable	Obs	Mean	Std. Dev.	Min	Max
District	30	3.667	1.269	2	5
Year	30	2014.6	3.092	2010	2020
Population	30	624919.9	265344.13	211957	1031732
Malaria Cases	30	3.85	.567	2.206	5.191
Malaria Deaths	30	.001	.001	0	.002
Malaria Campaigns	30	.012	.005	.001	.027
Children <5 Cases	30	1.385	.388	.479	2.138
Pregnant Women Cases	30	.27	.461	.012	2.5
COVID-19 Cases	30	38.633	130.651	0	575

We can observe a difference in treated and control group malaria outcome levels. It is also important to note that the average amount of malaria deaths per year in both groups from 2010-2020 did not deviate from zero. This confirms the findings in the contextual background that identify a dramatic drop in malaria deaths due to development interventions after 2000.

Figure 2-1 and 2-2 show the 2010-2020 annual average number of total malaria cases and malaria deaths by control and treatment districts. Here, 2018 marks the end of the pre-policy period and the beginning of the post-policy period due to the lack of data in 2019.

Figure 2-1: Malaria Cases Parallel Trends

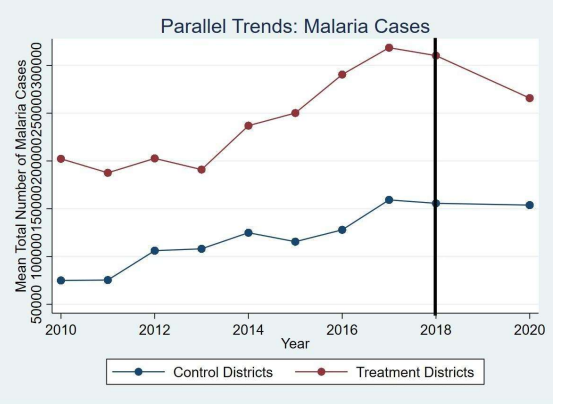
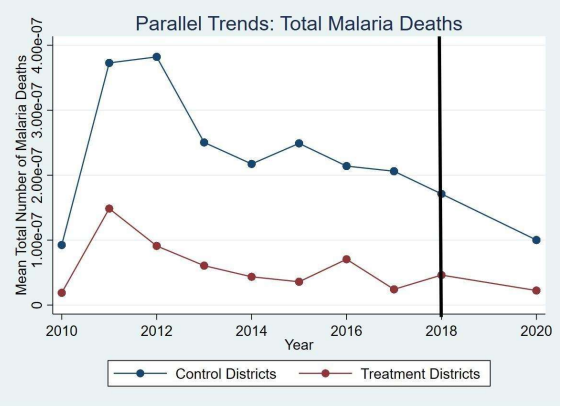


Figure 2-2: Malaria Deaths Parallel Trends



Given the summary characteristics in Table 1, we observe stark average differences in the control and treated levels of each outcome of interest. However, given that this study is premised on a difference-in-differences strategy, the differences between the variable averages in both groups can be minimized if the parallel trend assumption for the outcomes of interest is satisfied. The parallel trends assumption posits that treatment and comparison groups can have different levels of the outcome prior to the start of treatment, but their trends in pre-treatment outcomes should be the same. If this stipulation of the parallel assumptions holds, the impact of COVID-19 on malaria outcomes can be isolated and accurately assessed.

In Figure 2-1 we see that on average, from 2010 to 2018, the parallel trend assumption holds as the total malaria cases in both the control and treated districts trends upward pre-pandemic. After 2018, however, there is a decrease in malaria cases for the treated districts while the control districts remained approximately constant. Conversely, In Figure 2-2 we see that on average, the parallel trend assumption holds between 2011 and 2018 as the total malaria deaths in both the control and treated districts trends downward pre-pandemic. After 2018, however, there is a steeper decline of malaria deaths for the treated districts compared to the decline in the control districts average malaria deaths. In both scenarios, given the parallel trends assumption, we can interpret the average difference in the post pandemic period between treated and control as causal.

Table 2 shows the regression results for COVID-19 cases on the first outcome of interest: malaria cases. Each column represents a different regression that captures the variation in Malaria cases between treatment group and control group when controlling for different fixed effects. For this analysis, we focus our attention on the point estimate in column 4 which corresponds to the β_3 in the first key fixed effects regression discussed in the Econometric Framework and Methodology section.

Table 2: Impact of COVID-19 on Malaria Cases

	(1) Malaria Cases	(2) Malaria Cases	(3) Malaria Cases	(4) Malaria Cases
District x post	-0.807	-0.748	-1.471*	-1.457***
	(0.426)	(0.505)	(0.601)	(0.403)
Constant	3.984***	3.982***	4.006***	4.005***
	(0.110)	(0.0892)	(0.0919)	(0.0586)
N	90	90	90	90
R-sq	0.020	0.410	0.389	0.778
District Fixed-Effect	No	Yes	No	Yes
Year Fixed-Effect	No	No	Yes	Yes

Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Here, contrary to the hypothesis, we can see that COVID-19 affected districts saw a 1.457 percentage point more decrease in malaria cases in the post intervention period compared to the control group. In terms of magnitude, this roughly translates to an approximate 36% greater average decrease in malaria cases calculated using the following formula:

$$\frac{-1.457\ (Point\ Estimate\ on\ District\ x\ Post\ in\ column\ 4)}{4.01\ (Mean\ of\ Malaria\ Cases\ (per\ capita)\ in\ Control\ Districts\ from\ Table\ 1)} \times 100$$

Furthermore, the point estimate, -1.457, is associated with a p<0.0001 which indicates a statistically significant coefficient. In this manner, the p-value is sufficiently small that we can confidently conclude that the results in this regression are not easily explained by chance alone and the null hypothesis can be rejected. The regression in column 4 is also associated with an R-squared value of about 0.778 which indicates that this regression model predicts approximately 78% of all the variability of the response data around its mean. This relatively high R-squared value is mainly driven by the unit and time fixed effects. It is important to note here that the R-squared value is simply an overall measure of the strength of association, and does not reflect the extent to which any particular independent variable is associated with or explains the dependent variable.

Table 3: Impact of COVID-19 on Malaria Deaths

	(1) Malaria Deaths	(2) Malaria Deaths	(3) Malaria Deaths	(4) Malaria Deaths
District x post	-0.00152***	-0.000376	-0.000778	0.00102
	(0.000222)	(0.000627)	(0.000849)	(0.000535)
Constant	0.00174***	0.00170***	0.00172***	0.00166***
	(0.000145)	(0.000111)	(0.000130)	(0.0000778)
N	90	90	90	90
R-sq	0.041	0.475	0.297	0.774
District Fixed-Effect	No	Yes	No	Yes
Year Fixed-Effect	No	No	Yes	Yes

Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Table 3 shows the regression results for COVID-19 cases on the second outcome of interest: malaria deaths. As in the previous table, each column represents a different regression to capture the variation in Malaria cases when controlling for different fixed effects. The point estimate in column 4 corresponds to the β_3 coefficient in the second key fixed effects regression discussed in the Econometric Framework and Methodology section. Here, we can see that the point estimate on District x Post coefficient is 0.00102, even after manipulating the data to magnify the point estimates (multiplying by 1000). This result is not statistically significant nor different from zero which is not surprising given the findings in Figure 2-2. Figure 2-2 illustrates that at a microscopic level the mean for malaria deaths does not deviate from zero, as previously observed in the summary characteristics. Overall, the results show that COVID-19 affected districts have no significant change in malaria deaths post-pandemic.

Table 4 shows the regression results for assessing the impact of COVID-19 on the number of malaria education campaigns. This is the mechanism by which I verify that the hypothesis for this research

investigation holds as malaria education campaigns are one of the resources used to target malaria prevention.

Table 4: Diversion of Resources from Malaria Prevention to COVID-19 Mitigation

	(1) Malaria Campaign	(2) Malaria Campaign	(3) Malaria Campaign	(4) Malaria Campaign
District x post	-0.0141***	-0.00603	-0.0138	-0.00157
	(0.00279)	(0.00687)	(0.0106)	(0.00835)
Constant	0.0207***	0.0204***	0.0207***	0.0203***
	(0.00160)	(0.00121)	(0.00162)	(0.00121)
N	90	90	90	90
R-sq	0.030	0.479	0.090	0.544
District Fixed-Effect	No	Yes	No	Yes
Year Fixed-Effect	No	No	Yes	Yes

Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Here, in line with my hypothesis, we can see that COVID-19 is estimated to have an adverse impact on the number of malaria education campaigns that occurred post-pandemic. This is indicated by the negative sign on the point estimate that is constant throughout all of the other three regressions as well. While the point estimate of -0.00157 in column 4 is relatively small in terms of magnitude, this roughly translates to treated districts experiencing approximately 6.5% decrease in malaria education campaigns due to COVID-19 compared to the control group which was calculated using the following formula:

$$\frac{-0.00157\ (Point\ Estimate\ on\ District\ x\ Post\ in\ column\ 4)}{0.024\ (Mean\ of\ Malaria\ Cases\ (per\ capita)\ in\ Control\ Districts\ from\ Table\ 1)} \times 100$$

The point estimate in this case is not statistically significant. Thus, it can be hard to rule out that these results are due to chance even with an R-squared of 0.54 which indicates that this regression model may predict approximately 54% of all the variability of the response data around its mean. Once again, the R-squared value in this regression is mainly driven by the unit and time fixed effects.

Tables 5-a and 5-b show the regression results for a more group-specific study. This is the method by which I observe the heterogeneous effect of COVID-19 on different kinds of populations. Based on Table 2, we know that COVID-19 affected districts saw an average 36% more decline in malaria cases. Furthermore, we also know that pregnant women and children under the age of five are the most vulnerable populations to the malaria COVID-19. Tables 5-a and 5-b indicate that this decline in total malaria cases is mostly driven by children under the age of five who saw a 0.624 percentage point decrease in malaria cases whereas pregnant women only saw a 0.194 percentage point decrease in treated districts compared to

the control post-pandemic – the former is statistically significant (p <0.05) while the latter is not. Overall, the results in these tables are consistent with the findings in Table 2 that counter my hypothesis of COVID-19 having an adverse impact on malaria cases.

Table 5-a: Impact of COVID-19 on Children <5 Malaria Cases

	(1) Malaria Children	(2) Malaria Children
District x post	-1.045***	-0.624*
	(0.209)	(0.263)
Constant	1.966***	1.952***
	(0.0790)	(0.0382)
N	90	90
R-sq	0.064	0.823
District Fixed-Effect	No	Yes
Year Fixed-Effect	No	Yes

Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Table 5-b: Impact of COVID-19 on Pregnant Women Malaria Cases

	(1) Malaria Preg. Women	(2) Malaria Preg. Women
District x post	-0.0478	-0.194
	(0.0532)	(0.198)
Constant	0.209***	0.214***
	(0.0309)	(0.0287)
N	90	90
R-sq	0.001	0.293
District Fixed-Effect	No	Yes
Year Fixed-Effect	No	Yes

Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

7. CONCLUSION

7.1 Findings

The results of this study are inconclusive as to the true effect of COVID-19 on district level malaria outcomes in Burkina Faso. Malaria cases have decreased more drastically in post-pandemic treated districts, while malaria deaths saw no average change post-pandemic and were at virtually at zero levels pre-pandemic. Given these diverging results in both outcome variables of interest, it is difficult to make conclusive statement about the impact of COVID-19 on malaria. However, the data and results do holistically

reject my hypothesis which anticipated that COVID-19 would have an adverse impact on malaria outcomes in Burkina Faso. The only regression results consistent with my hypothesis are the results discussed in Table 4 where COVID-19 affected districts are estimated to have seen a small but significant decrease in the number of malaria education campaigns post-pandemic which I interpret as the diversion of resources from malaria to COVID-19.

7.2 Data Limitations & Validity of Results

It is important to acknowledge that this study is limited in data given the previous discussion on missing 2019 data which could have affected the validity of my results.

Furthermore, the data was severely restricted in variables observed as there are many factors to consider when assessing malaria outcomes that were unobserved. It would have been great to run longer regressions with data on school closures due to COVID-19 or health budget allocations to further test my hypothesis. This would have led to more valid conclusions in this study. Overall, this research has clearly shown that conducting a research study in a low-income Sub-Saharan African country can prove to be very challenging given data limitations.

7.3 Opportunity for Future Research

The COVID-19 pandemic is a very recent and ongoing phenomenon, thus data and investigations conducted on its impact are very limited. Oftentimes, research of this nature requires a longer-term study to assess a causal impact which could explain the inconclusive nature of my investigation on top of data limitations. Perhaps longer-term data could be collected in future years, resulting in ample data to better determine the longer-term impact of COVID-19 on health outcomes. Continuing the efforts of this research paper, a subsequent study could involve a fieldwork investigation with health workers in Burkina Faso to obtain their insights on the results of this research and factors they think might explain such an inconclusive outcome. Furthermore, future studies could build off this research and enlarge the scope to include other countries in Sub-Saharan African or even further look at the impact COVID-19 has had on other endemic diseases (HIV, Tuberculosis) in the area. These research endeavors may allow us to move beyond a "one-size fits all" COVID-19 solution for the world and start to unravel the real complexities different countries face as a result of their varying health systems.

II

HONOUR'S THESIS

The Effect of Increased Fixed Costs on High-Risk Decision Making: Evidence from Mount Everest

Sarah Wappel

ECON 499

ABSTRACT

Permits with high royalty costs are the medium through which mountain climbers obtain permission to climb Mount Everest in Nepal. This study examines how increased permit royalties affect risk and decision-making in high altitude mountaineering on Mount Everest. The economic literature has ascertained that changes in fixed costs have wide-ranging effects on behaviour and decision-making. This study applies these effects in the context of mountaineering, where climbers consistently make decisions in high-risk situations. Several estimation strategies are used across various specifications, including both ordinary least squares and zero-inflated negative binomial regressions, as well as a synthetic control. The results suggest that higher fixed costs may be associated with slightly lower risk outcomes and more risk aversion among certain climbers, but that these higher costs are correlated with sizable negative externalities where risk outcomes increase significantly on other peaks in the region.

INTRODUCTION

Basic economics states that demand decreases when price increases, though the literature and various theories, both in economics and other fields, show that the story is far more complex. Price changes produce many effects including composition changes, changes in consumer characteristics, changes in behaviour, and the creation of externalities. One of the basic tenets of rational decision-making in economic theory claims that sunk costs should not affect decision-making. Prospect theory, as developed by Kahneman and Tversky (1979), highlights the issues in applying this theoretical assumption to the real world. Reputation, limited information, as well as time and financial constraints all impact decision-making such that economic agents respond differently than the theory predicts (Zeelenberg and Dijk 1997). Contrary to theory, the literature on sunk costs finds that sunk costs may instead result in risk-seeking behaviour in some situations, and risk-averse behaviour in others, depending on several factors. This study examines how increases in fixed costs impact decision-making in high-altitude mountaineering. More precisely, how do increased permit royalties, or fixed costs, affect risk and decision-making on Mount Everest?

The context of high-altitude mountaineering has been discussed only briefly in economic literature. Previous studies have examined institutional development in the commercial age of mountaineering as well as the difficulty that this context poses for utility theory (Loewenstein 1999; Savage and Torgler 2013). This context also provides an excellent context through which to examine decision-making under uncertainty and risk. Mountain climbers constantly make extremely high-risk, and potentially life-threatening decisions facing uncertainty, whereas the average person rarely makes these decisions. In this sense, mountain climbers, especially experienced ones, can be considered professional high-risk decision-makers.

In 2015, the Nepal Mountaineering Association introduced a new system of mountaineering royalties, or climbing permit fees, eliminating a system where the amount each climber paid depended on team size to a constant rate for all climbers. This change amounted to a $1,000USD increase in permit costs.[1] This study exploits this policy change to determine how this change in costs affected decision-making in this high-risk, high-reward context. Using the number of deaths and serious injuries occurring as a proxy for risk levels, I utilize data from the Himalayan Database ©, a representative database of all mountaineering expeditions occurring

[1] *The Nepal Mountaineering Association issues permits on behalf of the Government of Nepal, which gains the revenues.*

in the Nepalese Himalaya 1905-2020, with a rich set of variables on individual and team characteristics, to explore this context. Using data on the other 8000m peaks in the region, Annapurna I, Cho Oyu, Dhaulagiri I, Lhotse, Kangchenjunga, Makalu, and Manaslu, I use a synthetic control to identify the effect of this policy. This estimation shows that Everest begins trending higher than Synthetic Everest in the post-treatment period. However, when the analysis is narrowed to a finer level using regression analysis, it appears that this trend is driven by a greater number of climbers on the mountain and the route crowding that results from these higher numbers.

After establishing a baseline using ordinary least squares, I use a zero-inflated negative binomial regression model where I find that a $1,000 increase in royalties for a mountaineering permit is associated with 0.01 fewer severe accidents in a 24-hour period during a summit attempt. Further analysis of the data demonstrates that this decrease appears to be associated with a greater number of hired climbers on expedition teams in the post-treatment period. Examination of potential spillover also shows that this policy change created sizable negative externalities wherein both regression analysis frameworks demonstrate a strong, positive, and significant effect on risk on the other peaks in the region.

The sections of this paper proceed as follows: Section II will provide a brief background on relevant information about Everest, the permit system, and an overview of the relevant economic literature. Section III describes the data used in this study, Section IV outlines the estimation strategy, Section V presents and discusses results, and Section VI concludes the paper.

2. BACKGROUND & LITERATURE REVIEW

2.1. Background

For most of Everest's climbing history from the early 20th century onwards, permission to climb was granted by the Nepalese Government to one team, usually representing a nation, each season. Towards the end of the 1980s, professional climbers and guiding companies created a market to guide commercial teams of non-professional climbers on Everest. As more people sought to climb Everest, the Government of Nepal introduced a large-scale mountaineering royalty system for climbing mountains in the Nepalese Himalaya. The number of permits issued increased significantly throughout the 1990s, especially for Mount Everest. (Krakauer 1997) On Everest, the increases stopped on a system, which was in place from 1996-2014, wherein larger teams, with more

than seven members, paid $10,000 USD per climber and smaller teams paid as much as $25,000 (Huey, The Economics of Adventure: On the high cost of Himalayan climbing permits 2001). However, permit fees above $10,000 were rarely paid, as smaller teams would list themselves as one team to take advantage of the discount for larger teams. In 2014, the Government of Nepal Ministry of Culture, Tourism, and Civil Aviation announced changes to the country's long-standing royalty for mountaineering permits (Arnette, New Everest Permit Fee System 2014). From 2015 onwards, all climbers climbing on the main route to the summit, via the South Col and SE Ridge, paid $11,000 USD regardless of team size. Permits are required for all members of the expedition, each at different costs, but this study focuses on those permits granted to climbers attempting to summit Mount Everest.

Permits are the main conduits through which the governments of China and Nepal regulate climbing on Everest. Everest climbing permits limit the time an expedition spends on the mountain to limit the number of climbers going through dangerous climbing aspects on the route, the environmental impacts of climbers, and crowding. Permits also provide the government with high revenues. The Kathmandu Post estimates that in 2019, the Nepalese Government obtained $5.07 million from climbing permit royalties, the vast majority of this number coming from Everest (Prasain 2020).

Climbing Everest is a significant investment of time, money, and effort. Not only do the physical demands of climbing require months of training in advance, but climbing at extremely high altitudes adds another layer of complexity. These high altitudes necessitate, for most individuals, the use of bottled oxygen, especially above 8000 m. Most Everest expeditions may spend up to eight weeks in Nepal or Tibet acclimatizing and preparing for the expedition. This is accomplished by climbing from base camp up to each of the higher camps at greater altitudes, spending time at these camps, and descending back to base camp to prepare their bodies for the summit push where they climb from base camp to the summit and back in one climbing effort over the course of a couple of days. The investment goes far beyond the time spent climbing during the summit push. Climbers invest significant amounts of money to secure the appropriate climbing gear, hiring guides or Sherpa, and various other necessities. Alan Arnette, the world's leading Everest correspondent, estimates that climbers spend at least $30,000 and sometimes over $160,000, with an estimated average of $45,000 on climbing Everest (Arnette, How Much Does it Cost to Climb Mount Everest?- 2021 Edition 2021).

b. Relevant Literature

A number of studies find that in many real-world situations, agents do not discount sunk costs. Arkes and Blumer (1985) define the sunk cost effect as "a greater tendency to continue an endeavour once an investment in money, effort, or time has been made. The prior investment, which is motivating the present decision to continue, does so despite the fact that it objectively does not influence the decision" (Arkes and Blumer 1985). The authors perform several questionnaires to demonstrate the sunk cost effect and find thatmost of the subjects did not respond in a way consistent with discounting sunk costs. Thaler (1980) also develops several anecdotal examples from conversations with other economists and questionnaires which demonstrate the same behaviour wherein sunk costs are not discounted in real-world situations. Thaler, as well as Arkes and Blumer, cite the importance of the model developed by Kahneman and Tversky as a method more readily applicable to analyzing real-world decisions over expected utility theory and rational decision-making. Kahneman and Tversky suggest and later update a model of decision-making as an alternative to expected utility theory called prospect theory (Kahneman and Tversky, Prospect Theory: An Analysis of Decision under Risk 1979; Kahneman and Tversky, Advances in Prospect Theory: Cumulative Representation of Uncertainty 1992). The model is grounded on how people react differently in situations of potential loss versus situations of potential gain. Prospect theory claims that decision-making is based on a decision maker's current position, and in a situation where there is potential gain, individuals are risk-averse and prefer certainty. Alternatively, in situations of potential loss, individuals are risk-seeking in an attempt to avoid potential losses. In both situations, individuals are left with lower expected utilities due to their preferences for certainty or avoiding losses. The value function that Kahneman and Tversky propose is convex for losses and concave for gains. In the context of this study, this suggests that an increase in permit costs may cause climbers to take on additional risks when paying higher fixed costs to avoid losing a larger sum of money in the event they do not reach the summit. Thus, if increased permit prices incentivize climbers to take on more risks, a climber may push for the summit when the conditions do not make this summit push viable. As a result, there would be more adverse outcomes, including deaths, injuries, and abandoned attempts.

Many studies in decision-making are undertaken in lab situations where outcomes result from artificial situations or questionnaires where participants face very little risk. Mountain climbing provides a context where there are real, apparent risks and where decision-

making has clear consequences. These decision-makers, especially lead climbers and guides, consistently make these decisions.

3. DATA

This study utilizes data primarily from the Himalayan Database ©. The database is a representative dataset from all the climbs attempted in the Nepal Himalaya between 1905-2020 and gives detailed information on routes taken, oxygen use, accidents, deaths, successes, dates, team size, and many other variables. I have restricted my sample to the years 2000-2019 and focusing primarily on the Nepal, or south, side of Everest. Each observation represents an individual climber. I also utilize the expedition and member data from the other 8000 m peaks in the database including Kangchenjunga, Lhotse, Makalu, Cho Oyu, Dhaulagiri I, Manaslu, and Annapurna I. Disputed summit claims were dropped. Climbs under abnormal circumstances were excluded including: (1) observations from the Spring 2014 season, following an avalanche in the Khumbu Icefall; (2) observations from the Spring 2015 season as no summits were attempted following a major earthquake; and (3) climbs undertaken in 2020 during the COVID-19 pandemic.

For my dependent variable, risk, I use a proxy, which is the incidence of death and injury. This variable was created from the information given in the Himalayan Database©. I divided each summit attempt into four categories: successful attempts, attempts abandoned, attempts where the climber gets injured or contracts an illness, and attempts where the climber dies.[2] I define a successful summit as reaching the high point (8850 m) and returning to base camp alive and without serious injury. The risk measure, Death and Injury Rate, was created by dividing the number of deaths and accidents by the total number of attempts. I separated risk into five different categories. The first is the risk over 24 hours measured from midnight to midnight for those climbers above the high camp indicating the 24 hour period of this summit push as the date which these climbers reached their high point. The second is the same measure except it includes all climbing during the summit push, including below the high camps. Third, I adjusted the time period to include the entire climbing season, i.e. Spring 2003 or Autumn 2010, above the high camp again. Fourth, the season level again for climbers attempting a summit push above base camp. Fifth, the season level risk for all

[2] *Here, injuries are serious injuries. In the database, injuries are coded with the type, and we find that most often these are exposure/frostbite and many of these outcomes include eventual amputation due to frostbite. Other examples include broken bones, Acute Mountain Sickness (AMS), and other such injuries and illnesses which often require emergency evacuation, which is not always possible.*

climbers regardless of whether they attempted a summit push or not.

The independent variable for this study is the cost of a climbing permit for Mount Everest. This is the only data that was not contained within the Himalayan Database. The main resources for this data were from Alan Arnette through both research on his website and information from guiding companies and individual climber testimony. From 2015 onwards, Nepal offered an alternative pricing system for Nepalese climbers in Nepalese Rupees. I converted this information to USD based on the exchange rate given by Alan Arnette from the date of the announcement of the new fee system.

I utilized data from the entire period of recorded observations in the database to create four different measures for climber experience. I totalled the number of times an individual was listed on any previous 8000 m expedition, then narrowed this to other previous successes on the 8000 m peaks in the database. I repeated this process to generate a sum of a climber's previous attempts and successes on Everest only.[3]

As well, as the Himalayan Database © only includes those 8000 m peaks in the Nepal Himalaya, not the Karakorum or other areas, even the most encompassing experience measure will underestimate most climbers' experience levels. To the best of my knowledge, there is not a comparable, detailed database for those other 8000 m peaks where I could obtain reliable data to improve this variable. Nevertheless, those peaks not contained within the Himalayan Database © are considered more technically difficult than Everest, and more remote, thus making undertaking expeditions more difficult (Arnette, 8000ers 2014). Climbers with previous 8000m experience, if any, are more likely to have undertaken an ascent of Cho Oyu over Nanga Parbat or K2. While these variables will underestimate the high-altitude experience of some climbers, I believe it will still be accurate for most climbers.

To mitigate the risks associated with climbing at extremely high altitudes, most climbers use supplemental oxygen during their climb above 8000 m or for medical purposes when suffering from Acute Mountain Sickness (AMS) or High Altitude Cerebral or Pulmonary Edema(HACE/HAPE).[4] The Himalayan Database © gives detailed information about the nature of each climber's oxygen use. I included this control to account for the variation in risk between

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[3] This is not a perfect measure of experience, as many climbers with no previous high-altitude experience have extensive technical climbing experience, often far beyond that of others climbing Everest. However, as the main route to Everest via the South Col and Southeast ridge does not require this type of technical expertise, the altitude factor may be a better measure of previous experience for this context.

[4] In the "death zone," at an altitude above 8000m, the air contains one-third the amount of oxygen found at sea level. This puts severe stress on the human system and can result in judgement impairments and altitude sickness, among other issues. Climbers can only stay at these altitudes for short periods of time.

those climbers utilizing oxygen and those who do not. While some people are less affected by high altitude, we cannot directly control for an individual climber's physiology.

In a recent article, Carroll, Huey, Salisbury, Wang (2020), have highlighted the effect of age on climbers' success. They found that climbers over the age of 40 have declining rates of success and are more susceptible to death due to illness. (Huey, Carroll, et al. 2020) To control for these effects, I chose to use age as a control variable. The age variable was contained in the database, calculated from the climber's date of birth and the date of the start of the expedition. Observations without ages were dropped.

Information on team size and the number of hired team members on each expedition team were also contained in the database. I utilized this information to create two ratios of the number of climbers to the number of hired members of an expedition. The support of Sherpa, guides, porters, rope-fixers, and various other roles is imperative to undertake a summit bid. The first ratio divides the total climbing members of each expedition team by the total number of hired members of the same expedition team. The second ratio does the same except divided by the number of hired climbers who operate in more than an administrative role at base camp.

Weather and adverse conditions are indicator variables I generated by date from those dates where a climber abandoned their summit attempt, citing bad weather or adverse conditions. Weather often varies widely on different parts of the mountain at any given time, making it difficult to control for this factor, despite its extreme importance. Furthermore, reliable weather data coming from the recently installed National Geographic weather station is only available for 2019 onwards, therefore it is not applicable to the time period of this study.

Crowding on Everest has garnered intense media attention over the past few years, especially in 2019, a season where a higher number of deaths accompanied viral photos of queues along the summit ridges. Crowding is an issue that increases risk, particularly on the highest altitudes of the mountain where routes are much narrower.[5] For those summit dates and climbers climbing above the highest camps, I introduced a same-day summit variable, counting the number of climbers en route to the summit by route and date. From this, I created an indicator variable where the number of people on a given route to the summit exceeds 150 to control for the effects of crowding.

[5] *Ascending and descending climbers must also share the same rope, the few seconds where a climber must unclip from the fixed ropes to alternate positions with another climber are seconds where this climber does not have an important safety tool that could prevent a fatal fall. Crowding also increases wait times and time spent above 8000m, thus increasing the risk of contracting illnesses caused by low oxygenation at high altitudes.*

Table 1: Summary Statistics of Key Variables

	Obs.	Mean	Std. Dev.	Min.	Max.
Deaths & Injuries in 24h	3,378	1.7614	2.2018	0	8
Deaths & Injuries in 24h >7900m	3,123	1.6555	2.0036	0	8
Permit Royalty (1000's USD)	3,796	10.0945	1.4801	0.757	15
Individual Controls		40.1457			
Age	3,796		10.4100	14	81
Oxygen Used	3,796	0.8617	0.3453	0	1
Previous 8000m Attempt	3,796	1.7640	3.4953	0	40
Previous 8000m Success	3,796	0.8172	2.1768	0	22
Previous Everest Attempt	3,796	0.9104	2.2111	0	26
Previous Everest Success	3,796	0.4049	1.5470	0	20
Expedition Composition Controls					
Climbers: Hired	3,778	1.0270	0.5211	0	4
Climbers: Hired Climbers	3,611	1.5219	1.0338	0	8.5
Team Size	3,796	11.4700	8.5809	1	42
Summit Day Controls					
Crowding	3,154	0.1237	0.3294	0	1
Same Day Climbers	3,151	84.9800	61.2169	0	240
Bad Weather	3,796	0.3256	0.4687	0	1
Bad Conditions	3,796	0.0514	0.2208	0	1

To create balanced panel data, I collapsed the data using the means of those predictor variables which would give a good representation of the type of climber and team composition on each peak, specifically the average level of previous 8000m experience and the average ratio of climbers to hired climbers. Specific details of these variables are discussed above. First, I dropped 2015 and 2014 for those other peaks, and I then dropped 2008, as the Beijing Olympics occurred this year. The expedition to take the Olympic torch to the summit of Mount Everest resulted in the cancellation of the Spring climbing season on all peaks in Tibet, including the North side of Everest and Cho Oyu, two of the peaks I expect to be most similar to the Nepal side of Everest. 2001 was also dropped as several peaks in the sample saw no climbing. I then identified the peaks for which climbs were not attempted for a number of years and dropped them from the sample, which balanced the panel. These peaks were Annapurna I, Kangchenjunga, and Manaslu. Alternative specifications, where less of the sample was dropped, are discussed in the appendix. The relevant summary statistics for this panel are presented in Table 2.

Table 2: Summary Statistics for Synth

	Everest-South		Other 8000m Peaks	
	Mean	Std. Dev.	Mean	Std. Dev.
Season Death & Injury Rate (%)	2.2134	1.2206	3.9381	4.7124
Average Previous 8000m Experience	1.7833	0.2893	2.5612	1.6853
Average Hired Ratio	1.7375	0.6051	3.0100	1.6744

4. ESTIMATION STRATEGY

My study utilizes two estimation methods, a synthetic control and regression analysis.

The synthetic control method as developed in Abadie et. al. (2010) exploits the other 8000 m peaks contained in the database to create a counterfactual Everest using weighted outcomes from the other 8000 m peaks contained in:

$$Y'_{EV,t} = \sum_{m \in M} W_m Y_{EV,t} \tag{1}$$

where M is the set of 8000 m peaks, m, contained in the database, $Y_{m,t}$ is the observed rate of death and injury, as a percent, observed on peak m in year t, and W is a 1×m vector $\Sigma_{m \in M}\ W_m = 1$, $W_m \geq 0$ of the weights of each peak used in the synthetic Everest. I utilize the rate of death and injury among climbers as a percentage of total climbers in a season as my main outcome variable here, in place of the count variable used in the regression analysis, to temper the effects of the high number of climbers on Everest, which exceeds the other 8000 m peaks. The composition of the synthetic control is generated using a combination of season and expedition predictors, specifically the average experience of climbers and the average hired ratio in that season. Here each observation is a climbing season at a given peak m, in a year t.

I also utilize regression frameworks to evaluate the effect of a permit increase on risk. My choice dependent variable here is the number of deaths and injuries in 24-hours. Narrowing the estimation in this way allows for the inclusion of controls for factors that affect risk levels. These controls include oxygen use, previous experience in high altitude mountaineering, and age at the individual level. Expedition levels include the ratio of climbers to hired climbers and team size. Summit day controls include indicators of adverse conditions, bad weather, and route crowding. To account for year-to-year changes and other unmeasurable factors including advancements in climbing equipment technology including lighter oxygen apparatuses and other similar improvements, all of which would ease climber's burdens, I include year fixed effects. These fixed effects also absorb the variation in risk from years with shorter weather windows and therefore less days where a summit is possible, and better knowledge of climbing routes as years pass. Where I include seasons other than the main climbing season in the estimation, I include season fixed effects.[6]

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[6] The pre-monsoon, or spring season, is the most popular, seeing 97.5% of attempts on Everest 2000-2019. Autumn is regarded as having a higher risk than the spring season, as the autumn season begins after the monsoon resulting in snowier, more unstable conditions. Winter presents yet more challenges, including more extreme temperatures, shorter daylight hours, and even snowier conditions, all of which increase risk.

As a baseline, I utilize an OLS framework. Since my outcome variable is a count variable, I determined that a different estimation method, such as a Poisson or negative binomial regression, would be more appropriate to analyze this data. Noting as well, as can be seen by the summary statistics, that the variance of my outcome variable exceeds the mean, I use a negative binomial regression framework.

The OLS estimation utilizes the following equation:

$$R_{its} = \beta_0 + \beta_1 P_{its} + \beta_3 X_{its} + \nu_t + \psi_s + \varepsilon_i \quad (2)$$

where P_{its} is the permit royalty that a climber i pays, measured in 1000's of USD[7], R_{its} is the number of deaths and injuries observed during the final 24-hours of the climber's summit attempt, and $X_{(t,i)}$ are the individual, expedition, and summit date controls specified above. Each observation here is an individual climber. ν_t and ψ_s are year and season fixed effects, respectively.

The negative binomial regression is based upon the following distribution, as in (Hilbe 2014):

$$p(r) = P(R = r_i|\mu_i, \alpha) = \frac{\Gamma(r_i + \alpha^{-1})}{\Gamma(r_i + 1)\Gamma(\alpha^{-1})}\left(\frac{1}{1 + \alpha\mu_i}\right)^{\alpha^{-1}}\left(\frac{\alpha\mu_i}{1 + a\mu_i}\right)^{r_i} \quad (3)$$

where μ_i is the conditional mean of R_{its} and α is the dispersion parameter which differentiates a negative binomial from a Poisson distribution. The regression estimates the following equation:

$$\ln(\mu_i) = \beta_0 + \beta_1 P_{its} + \beta_3 X_{its} + \nu_t + \psi_s + \ln(t_i) \quad (4)$$

where each variable is as specified above, and $ln(t_i)$ is a time exposure.

Despite mountain climbing being a high-risk activity, injuries and deaths are rare occurrences. 43% of all observations see 0 deaths in 24hours. My choice specification, therefore, utilizes a zero-inflated negative binomial regression model due to the large number of zeros in the observations. The zeros observations are predicted as follows:

$$P(r_i = j) = \begin{cases} \pi + (1 - \pi)\mathrm{P}(R = r_i) & if\ j = 0 \\ (1 - \pi)P(R = r_i) & if\ j > 0 \end{cases} \quad (5)$$

where π is the logistic link function defined as:

$$\pi = \frac{\lambda}{1 + \lambda} \quad (6)$$

and:

$$\ln(\lambda) = \gamma_0 + \gamma_1 S_{its} + \gamma_2 B_{its} + \ln(t_i) \quad (7)$$

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[6] The pre-monsoon, or spring season, is the most popular, seeing 97.5% of attempts on Everest 2000-2019. Autumn is regarded as having a higher risk than the spring season, as the autumn season begins after the monsoon resulting in snowier, more unstable conditions. Winter presents yet more challenges, including more extreme temperatures, shorter daylight hours, and even snowier conditions, all of which increase risk.

[7] This monotonic transformation was chosen to draw clear comparisons with the 1000USD increase which occurred at the treatment time in the synthetic control/ this is the exact change in permit cost whose variation this study hopes to exploit.

where S_{its} is the number of climbers reaching their high point the same day during their summit bid, and B_{its} is an indicator for bad weather.

The distribution of the relevant variables used in the zero-inflated regressions are presented in the appendix.

5. RESULTS AND DISCUSSION

Here, I use data from the spring climbing season only, though I repeat the same analysis using data from all seasons, and the results are presented in the appendix. Figure 1 presents the trends of the relevant risk variable, the season rate of death and injury, for each peak in the data. The predictor balance for the synthetic control is presented in Table 3 and weights of Synthetic Everest are presented in Table 4.

Table 3: Synthetic Everest Predictor Balance

Predictor	Year	Treated	Synthetic
Average Hired Ratio	2005	1.876475	2.033437
Average Hired Ratio	2009	1.480435	1.540164
Average Hired Ratio	2011	1.185471	1.800944
Average Hired Ratio	2012	1.349298	2.698744
Average Hired Ratio	2013	1.294335	1.39057
Average Previous 8000m Experience	2005	1.452555	1.651099
Average Previous 8000m Experience	2009	1.787645	1.443867
Average Previous 8000m Experience	2011	1.966387	1.709093
Average Previous 8000m Experience	2012	1.673333	2.637846
Average Previous 8000m Experience	2013	1.759259	2.088824
Death & Injury Rate	2000	4.195804	4.901789
Death & Injury Rate	2002	2.419355	3.637751
Death & Injury Rate	2003	4.329004	4.474459
Death & Injury Rate	2005	2.139037	4.471751
Death & Injury Rate	2006	0.4310345	4.196029
Death & Injury Rate	2007	2.027027	3.06433
Death & Injury Rate	2009	0.973236	3.012651
Death & Injury Rate	2010	1.28866	3.1518671
Death & Injury Rate	2011	0.6864989	1.495696
Death & Injury Rate	2012	2.09205	1.664377
Death & Injury Rate	2013	0.6557377	1.474096
Root Mean Squared Prediction Error	1.547861		

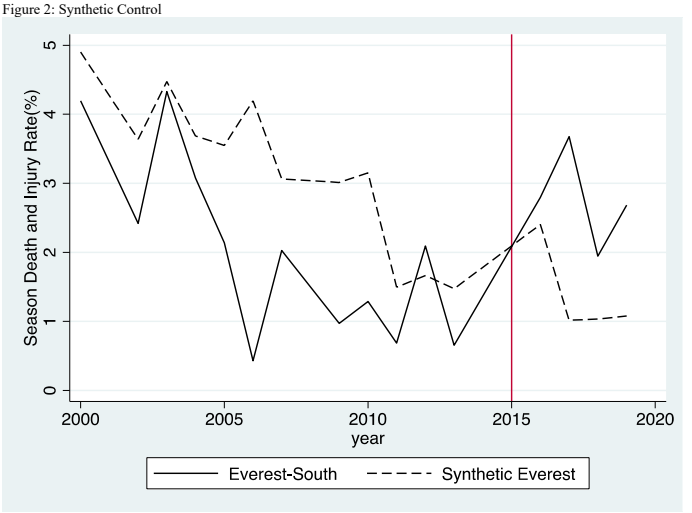
Table 4: Synthetic Everest Weights

Peak	Weight
Everest-North	0.784
Annapurna I	0
Cho Oyu	0.077
Dhaulagiri I	0
Kangchenjunga	0
Lhotse	0.139
Makalu	0
Manaslu	0

As expected, we see most of the weight of Synthetic Everest coming from the Tibet side of Everest, Cho Oyu, and Lhotse.[8] The predictors for Synthetic Everest track the same variables for Everest closely, with a root mean squared prediction error of 1.547861. The rate of death and injury on Everest consistently trends slightly lower than the Synthetic Everest for all years except 2012. The maximum difference between the rate of death and injury on Everest and the Synthetic Everest occurs in 2006, where the difference is just under 4%. Figure 2 shows the graphical results of the synthetic control.

[8] *Lhotse shares its main route to the summit with Everest's main route until about 7900m, or Camp 4. Cho Oyu is considered the "easiest" 8000m climb and attracts a similar profile of climbers as Everest does. (Arnette, Cho Oyu FAQ 2013)*

Figure 2: Synthetic Control



We see that the rate of death and injury in both Everest and the Synthetic Everest follows the same trend in that this rate falls over the course of the pre-treatment period. This rate stays between about 1-2% from 2005 to 2013 for Everest, and slightly higher for the Synthetic Everest. I cannot make any clear statements about the period immediately before and at treatment, as these periods were excluded from the analysis. This figure shows a reversal of the pre-treatment trend wherein Everest-South, the treatment group, begins to consistently trend higher than the Synthetic Everest in the post-treatment period. The counterfactual, Synthetic Everest, sees a slight increase before a drop in the rate of death and injury in the post-treatment period, whereas the death and injury rate on Everest increases in 2016, and remains high for the remainder of the post-treatment period. The results of this estimation suggest that, in the absence of other factors, the increase in permit costs sees risk, or the death and injury rate, increase. However, risk in mountaineering is affected by numerous observable and unobservable variables, many of which this estimation strategy cannot capture. Therefore, I cannot say with certainty whether the relationship between risk and this permit is positive, as the synthetic control estimation suggests.

For this reason, I also utilize the regression framework discussed in the previous section. The results of the OLS estimation are presented in Panel A of Table 5.

The coefficients on the Permit Royalty are small and negative, remaining between -0.02 and -0.015, though becoming smaller as the number of controls increases. These coefficients are insignificant across the various specifications, although this may be due to the large number of observations of risk equalling 0. Interestingly, we

Table 5: Main Regression Results

Dependent Variable: Deaths & Injuries in 24h	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS	-0.0292	-0.0193	-0.0180	-0.0150	-0.0157	-0.0166
Permit Royalty	(0.0255)	(0.0237)	(0.0238)	(0.0238)	(0.0207)	(0.0236)
Crowding		3.985***	3.992***	3.944***	3.207***	3.998***
		(0.143)	(0.143)	(0.145)	(0.130)	(0.143)
R-Squared	0.427	0.622	0.623	0.626	0.603	0.626
Panel B: Zero-Inflated N.B.	-0.00779	-0.0109**	-0.0113**	-0.00990*	-0.00916**	-0.00992*
Permit Royalty	(0.00609)	(0.00533)	(0.00533)	(0.00514)	(0.00422)	(0.00514)
Crowding		0.529***	0.533***	0.527***	0.354***	0.528***
		(0.0427)	(0.0428)	(0.0430)	(0.0367)	(0.0430)
Zero-Predictor:	-0.0540***	-0.0541***	-0.0541***	-0.0597***	-0.0558***	-0.0594***
Same day Summitters	(0.00282)	(0.00285)	(0.00285)	(0.00312)	(0.00305)	(0.00310)
$\chi 2$	18196.8	18607.8	17416.7	16475.0	26885.5	16467.1
Degrees of Freedom	20	21	24	26	26	27
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Weather & Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes	Yes	Yes
Expedition Controls	No	No	No	Yes	Yes	Yes
Season FE	No	No	No	No	No	Yes
Observations	3378	3123	3123	3055	3055	3083

Notes: OLS and negative binomial zero-inflated regressions on Himalayan Database © dataset on climbers and expeditions on Everest's Nepal side 2000-2019. Permit royalties are measured by a factor of 1000's of USD. Weather and Conditions Controls include two indicator variables which take on the value of 1 if climbers abandoned their climbs citing adverse conditions or bad weather in the same 24-hour period. Individual Controls include an indicator variable for oxygen use, taking a value of 1 if oxygen was used during the climb, and controls for age and previous experience on an 8000m peak in the Nepal Himalaya, Everest or otherwise. Expedition Controls include the variable for the number of climbers on their expedition team and the ratio of climbers to hired climbers. Crowding is an indicator variable which takes the value of 1 when there are more than 150 climbers on the same route in the same 24-hour period. The zero-inflated model uses the number of climbers reaching their high point in the same day as a given climber as well as the bad weather indicator variable as zero-predictors. Models (5) and (6) are robustness checks. The dependent variable in model (5) is the number of deaths and injuries observed in 24 hours at the highest altitudes of the mountain above the high camps, or approximately, 7900m.
Robust standard errors are in parenthesis
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

find the coefficients from route crowding are positive, large, and highly significant across the same specifications, remaining stable at around 4. This suggests that on days where there are more than 150 climbers on a given climbing route, there are almost 4 deaths on Everest. These estimates are robust to the inclusion of all climbing seasons, as seen in column (6), though the coefficient decreases slightly when I narrow the estimation to climbing above 7900m only, as seen in column (5).

The results of the zero-inflated negative binomial regression are presented in Panel B of Table 5. The coefficients on the Permit Royalty using this estimation are significant and stable around -0.01 across the introduction of various controls and the inclusion of all climbing seasons. The coefficient falls slightly to -0.009 when the sample is narrowed to climbing above 7900m only, but it remains significant. This suggests that, when accounting for the large number of zeros in the data, a $1000 increase in the Permit Royalty is associated with 0.01 less deaths in 24 hours. The coefficients on the crowding indicator are positive and significant, reducing again when narrowing the sample to climbing above 7900 m, but otherwise remains stable at 0.53, suggesting again that crowding is associated with a higher number of deaths and injuries. This suggests that crowding may drive the increase in the death and injury rate seen in the synthetic control. The full table with both panels and the coefficients on all controls is included as Table I.3 in the appendix.

Taking the results of these two panels together, we see from the positive coefficients on crowding that the increased risk trends observed in the synthetic control estimation above may be driven by increased prevalence of crowding on Everest, but that higher costs of permits appear to have a small, albeit very weak negative relationship

with the number of deaths and injuries. In the data, there are four dates where the number of climbers on the summit route exceeds 150, three of which are in the post-treatment period. These dates are: 19 May 2012, 19 May 2016, and 22 and 23 May 2019. Utilizing the coefficients we see on crowding from the OLS estimates, there should be, on average, four additional injuries and deaths on these days. Prior to 2015, the average number of injuries and deaths per year occurring during summit push climbing is approximately 5.5. Taking this information together, we expect to see around 9 incidences of injury and death in 2016 and 13 in 2019. As crowding exists only as an indicator for numbers more than 150 climbers on the same summit route, I suspect that these estimates may underestimate the actual incidence, especially in 2019 where the number of climbers on the same route exceeded 225 on both 22 and 23 May, which heightens the effect of crowding. The data indicates that the number of deaths and injuries in 2016 and 2019 was 14 and 20 respectively. This suggests that increases in the number of climbers may drive the increase in season death and injury rates seen in the synthetic control.

To explore the relationship between the permit price and risk further, I attempt to establish a mechanism through which permit royalties may affect this risk proxy. Table 6 is a balance table of the pre-and post-treatment period summarizing the mean difference of varying controls, which climbers, guides, and others accept to have an impact on a climber's outcomes.

First, we examine if the profile of the average climber or expedition changed. We see that the age of climbers increased by about a year. Huey et al. (2020) find that those climbers younger than 40 have essentially the same probability of reaching the summit, but that after the age of 40 the probability of achieving the summit declines linearly. While there may be a negative relationship between the physiological reaction to high-altitude and age, the limited change in this variable and away from younger ages suggests that this may not be the mechanism. Though increased age affords a climber more opportunity to gain experience, all four variables capturing high altitude experience remain unchanged between the two periods.

Changes in the choice variables are also noteworthy. The negative difference on bad weather and adverse conditions suggests that there are less climbers climbing when conditions are sub-optimal. We also see that the mean of oxygen use sees a slight but statistically significant increase. Taken together, the change in each of these variables hints that climbers are taking steps to climb when conditions are unfavourable and improving their climbing experience through oxygen use, which may suggest that climbers are becoming more risk-averse. This evidence suggests a potential application to the sunk cost literature, specifically through prospect theory, showing

that perhaps there is more risk-aversion post-treatment, placing climbers on the potential loss side of the value function. However, an evaluation of this context with prospect theory goes beyond the scope of this study. The relevant decreases seen in Table 6, may also indicate better weather forecasting so that climbers can avoid climbing on days where conditions are unfavourable, and maximize their chances of successfully reaching the summit. Increases in oxygen use may also be explained by selection, however, as no-O_2 ascents are generally only undertaken by professional climbers seeking to break records, not by the type of climber Everest attracts in the observed period 2000-2019.

Another potential mechanism can be seen in Table 6, as the ratio of climbers to hired members decreased slightly by about 0.2 fewer climbers per hired member. Noting this compositional change, I add an interaction term, P×HR , where HR is the ratio of climbers to hired climbers, to the OLS and zero-inflated negative binomial regressions.

Table 6: Balance Table of Key Variables for Everest-South 2000-2019

Variable	2000-2014	2015-2019	Difference
Age	39.736	40.971	1.235***
	(10.420)	(10.344)	(0.357)
Oxygen	0.845	0.894	0.049***
	(0.362)	(0.307)	(0.011)
Previous 8000m attempt	1.754	1.784	0.030
	(3.413)	(3.657)	(0.123)
Previous 8000m Success	0.818	0.815	-0.003
	(2.113)	(2.300)	(0.077)
Previous Everest attempt	0.896	0.940	0.045
	(2.252)	(2.127)	(0.075)
Previous Everest success	0.416	0.383	-0.033
	(1.567)	(1.506)	(0.053)
Climbers: Hired Climbers	1.619	1.333	-0.286***
	(1.131)	(0.779)	(0.032)
Climbers: Hired	1.053	0.974	-0.080***
	(0.561)	(0.425)	(0.016)
Team Size	11.397	11.617	0.221
	(8.699)	(8.339)	(0.292)
Same day Summitters	71.525	110.890	39.365***
	(45.484)	(77.229)	(2.556)
Crowding	0.042	0.280	0.238***
	(0.202)	(0.449)	(0.014)
Bad Weather	0.386	0.204	-0.182***
	(0.487)	(0.403)	(0.015)
Bad Conditions	0.075	0.005	-0.070***
	(0.263)	(0.069)	(0.006)
Observations	2,536	1,260	3,796

The results of this estimation, utilizing the same specifications as in Table 5, are presented in Table 7. Columns (1)-(3) in Table 7 demonstrates that, using an OLS estimation, the coefficients on the permit royalty are positive and significant, at the same magnitude of 0.09 as the negative and significant coefficients on the interaction term between permit royalties and the ratio of climbers to hired climbers. This is true across specifications.

Table 7: Main Regressions with Permit X Hired Ratio

Dependent Variable:	OLS			Zero-inflated N.B.		
Deaths & Injuries in 24h	(1)	(2)	(3)	(4)	(5)	(6)
Permit Royalty	0.0916*	0.0916*	0.0921*	0.00296	0.00198	0.00208
	(0.0516)	(0.0523)	(0.0522)	(0.0101)	(0.0103)	(0.0102)
Permit Royalty × Climbers: Hired Climbers	-0.0843**	-0.0840**	-0.0834**	-0.00909	-0.00870	-0.00860
	(0.0378)	(0.0385)	(0.0384)	(0.00584)	(0.00605)	(0.00606)
F-test (p-value):	0.09	0.10	0.13			
	(0.76)	(0.75)	(0.72)			
χ^2-test (p-value): H_0: Permit + Permit Royalty × Climbers: Hired Climbers = 0				1.13	1.34	1.26
				(0.29)	(0.25)	(0.26)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Weather/Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	No	Yes	Yes
Expedition Controls	Yes	No	Yes	Yes	No	Yes
Crowding Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3055	3055	3055	3055	3055	3055

Notes: OLS and negative binomial zero-inflated regressions on Himalayan Database © dataset on climbers and expeditions on Everest's Nepal side 2000-2019. Weather and Conditions Controls include two indicator variables which take on the value of 1 if climbers abandoned their climbs citing adverse conditions or bad weather in the same 24-hour period. Individual Controls include an indicator variable for oxygen use, taking a value of 1 if oxygen was used during the climb, and controls for age and previous experience on an 8000m peak in the Nepal Himalaya, Everest or otherwise. Expedition Controls include the variable for the number of climbers on their expedition team and the ratio of climbers to hired climbers. Crowding is an indicator variable which takes the value of 1 when there are more than 150 climbers on the same route in the same 24-hour period. The zero-inflated model uses the number of climbers reaching their high point in the same day as a given climber as well as the bad weather indicator variable as zero-predictors. The F-test and χ^2-test test the null hypothesis that the permit royalty on the number of deaths and injuries in 24-hours occurs through the effect of this policy on the number of hired climbers on a team.
Robust standard errors are in parenthesis
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

However, the results are less clear in the zero-inflated models, seen in columns (4)-(6) of Table 7. The coefficients on permits as well as the interaction effect are smaller, close to zero, and no longer significant across specifications. Running a χ^2-test to test the null hypothesis that the coefficients sum to zero shows, again, that I cannot reject the same null hypothesis, as the p-value is 0.26. These results are suggestive and indicate that the reduction in risk seen associated with the permit price increase as seen in Table 5 may be driven by a lower ratio of climbers to hired climbers on Everest, rather than the permit royalty itself. Hiring more climbers provides non-hired climbers with better support which in turn may lower risk as a result. This is corroborated in Table I.3 in seeing that the coefficient on the ratio of climbers to hired climbers is generally positive, though only significant in the zero-inflated model, implying that a greater number of climbers to hired climbers increases the incidence of death and injury on Everest. The motivation for hiring more climbers in the face of higher permit costs is less clear, however,

as hiring more support climbers adds to costs. A climber constrained by a budget will not increase their operating costs by hiring more support climbers when their fixed costs have already increased.

Another mechanism through which the reduction in deaths and injuries on Everest may occur is substitution. The increase in the Nepal Mountaineering Royalty for Everest made the other 8000 m peaks in the region relatively cheaper. Everest literature suggests that the demand for climbing these other peaks is inelastic, as many of them do not run commercial expeditions. Nevertheless, climbers with stricter budgets, or professional climbers, who often face strict budgets with more dangerous climbing objectives, such as undertaking new routes or speed ascents, may disperse to other peaks due to this permit increase (Boukreev, 2001). Using the same specification and the full set of controls as in Table 5, including peak and season fixed effects, these results find a large, positive, and significant effect on the number of deaths and injuries in 24 hours on other 8000 m peaks.[9] The coefficients in the OLS estimates suggest that an increase in the permit cost for Everest is associated with 1 additional death in a 24hour period on other peaks. The results from the zero-inflated negative binomial model, presented in Panel B, also demonstrate a sizable, positive relationship between Everest Permit increases and the incidence of death and injury on other 8000m peaks. This model shows that this permit increase is associated with 0.5 more deaths and injuries in 24 hours. All of these estimates are highly significant at the 1% level and consistent across different specifications. These results suggest that this policy change may have created large negative externalities. The reduction of risk on Everest appears small when compared to this observed increase in deaths and injuries on other peaks.

There are several confounders that offer alternative explanations for the effects demonstrated above. Some of these have already been discussed. Most importantly, two undeniable potential confounders which may drive many of the effects seen in the post-treatment period. The south side of Everest saw the spring climbing seasons cancelled for two years in a row due to the Khumbu Icefall Avalanche in 2014 and the Nepal Earthquake in 2015. These events may have motivated a behaviour change where climbers are more cautious and value hired climbers more, as Sherpa were disproportionately affected in the number of deaths and injuries during both these tragedies. On the other hand, those climbers returning to Everest to attempt a summit after a cancelled season may take on more risk to make up for their losses in the previous season. The Nepal Mountaineering

[9] *The main climbing season for other peaks, including Cho Oyu and Manaslu, occurs after the monsoon, in the fall. These peaks see substantially fewer climbers in the spring season.*

Association also honoured climbing permits from the year of the earthquake for a number of years afterwards, and this will create other noise. Evidently, several conflicting explanations exist and cannot be addressed in this study.

This policy may also have created spillovers. Anecdotal evidence points to one or two guiding companies, specifically Alpenglow Expeditions, choosing to climb from the Tibet side during this same time period 2013-2016. However, this change was motivated by the danger posed by the Khumbu Icefall on the Nepal side, compounded by the 2014 avalanche, not due to the change in permit price (Arnette, Everest 2019: Interview with Adrian Ballinger on China's New Rules 2019). Figures 3 and 4 show the number of climbers on each peak included in the database for the spring and fall climbing season respectively.

Figure 3: Spring Season Climbing Numbers by Peak

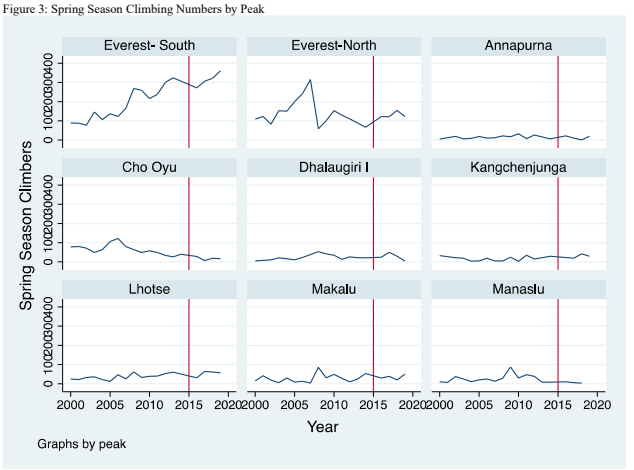
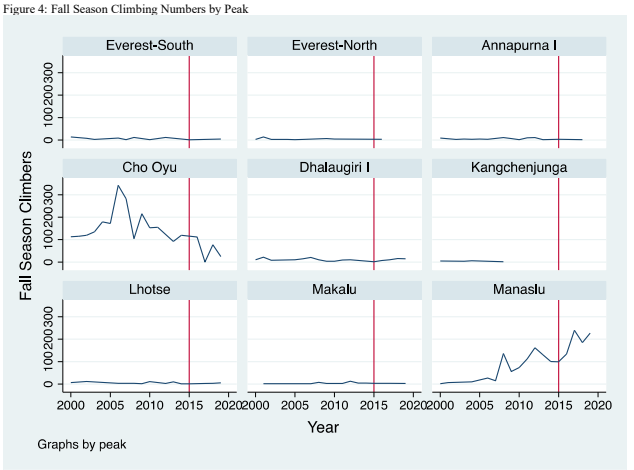


Figure 4: Fall Season Climbing Numbers by Peak



With the exception of Manaslu in the fall season, which already displayed an increasing trend before treatment, we do not observe any apparent increases in the number of climbers on any peaks in any season except for Everest.

Many climbers climb as part of larger commercial expeditions where the decision-making lies solely with the leader or guide. Therefore, an individual climber's decision-making is negligible in the larger scheme of decision-making. These guides, along with their teams, decide the number of hired climbers necessary for their expedition, choose summit timelines, and make other significant decisions. Guides running commercial companies are motivated by getting as many of their clients to the summit, whilst simultaneously minimizing risk, to attract future clients. This explanation may motivate the reduction in climbing in bad weather and adverse conditions observed in Table 6. These motivations are conflicting, however guiding companies emphasize that the summit is not guaranteed, often quoting Ed Viesturs in saying that "Summit is optional, coming down is mandatory," suggesting that risk-aversion may overcome the summit-maximizing effect (Viesturs 2006). Though with the given data, we cannot parse out each guide's individual preferences; however, I expect the presence of this effect would bias estimates towards zero.

Finally, a $1000 increase is small relative to the other high costs of climbing Everest. As discussed in Section II, climbers spend anywhere from $30,000 to $120,000 on expenses to climb Everest, not including other related costs. The $1000 may not be large enough to motivate changes in behaviour, climber profile, or any other variable explored in this study, as this increase is so small relative to these other costs. If this is the case, the results of this study are wholly spurious.

6. CONCLUSION

This study's main objective was to explore the ways that an increase in permit royalties for Mount Everest affected decision-making and risk. Contrary to my expectations, the results of the regression analyses suggest that the implementation of higher permit royalties is associated with a lower incidence of death and injury on Everest. It seems that this effect may stem from a change in team composition, wherein expedition teams not facing strict budgets are hiring more support climbers. As well, it appears that climbers are climbing less often where there are adverse weather or climbing conditions and generally utilizing more oxygen. Each of these mechanisms are consistent with an increase in risk aversion. However, as demonstrated with the synthetic control method, risk

levels on Everest trend higher after 2015, which seems to be related to the upward trend in the number of climbers on Everest during the climbing season and the effects a higher number of climbers has on route crowding. A high number of risk-averse climbers, climbing when conditions are favourable, during short weather windows, to maximize their probability of reaching the summit, has created crowding, which displays a strongly significant and large effect on risk. Furthermore, this policy appears to have created negative externalities on other Himalayan peaks, wherein low-budget climbers flocking to relatively cheaper peaks which then see a higher incidence of deaths and injuries.

While these results are suggestive and face numerous issues regarding causality, this analysis opens opportunities to further examine this context. Specifically, this context provides excellent prospects for analysis in behavioural economics to examine more nuanced decision-making while facing uncertainty. Another path of research could follow the ways in which these effects are different between commercial and professional teams, to determine how beliefs change with different motivations. Finally, the policy suggestion that I would postulate from these conclusions would be to limit the number of permits issued each season to stem the effects of route crowding.

III

REFERENCES AND APPENDICES

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REFERENCES AND APPENDICES

I. PAPERS

II. HONOURS THESIS

How to Host: Evidence from the World's Largest Refugee Camp

Akash Uppal

ECON 494

All progress begins on the shoulders of giants. My sincere appreciation to Siwan Anderson, Matt Lowe, Reshma Hussam, Mushfiq Mobarak, Paula López-Peña, Nadhanael GV, Pulkit Aggarwal, and Carla Colina. For the support of these people, along with my family and friends, I am deeply grateful. I take full responsibility for any shortcomings remaining.

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2. APPENDICES

Appendix A: Crime and Conflict Analysis

Within the Cox's Bazar Panel Survey, respondents were asked questions regarding crime and conflict in their neighbourhoods. Figures 15 presents the average perception of crime and conflict for a series of indicators between those living close to the camps and those living far from the camps. In addition, the average response for those living in the refugee camps is also shown. Figure 16 offers a similar set-up, but instead displays the group averages for various indicators as it relates to the incidence of crime and conflict. Figure 17 shows these results on the same graph, offering a visual comparison between the perception and incidence of the available crime and conflict indicators.

Both variables are coded as dummy variables, being 0 if the respondent answered 'yes' and 1 if the respondent answered 'no' to the question "Is (...) currently an issue in your neighbourhood" for perception of crime and the question "Have you experienced (...) at any time during the past 12 months in this area" for incidence of crime. There were nine crime and conflict indicators for which these questions were asked, including: bribery/corruption; harassment; theft; forced eviction; physical violence/assault; gender-based violence; business disputes; family disputes; and indebtedness.

Since it is not clear if differences in crime rates between the groups are a result of the refugee influx or pre-existing differences in crime rates, in addition to an inability to access data on crime and conflict prior to July 2017, I decided against performing an analysis similar to that of the overall health outcomes. Nonetheless, these results provide important insights on the refugee experience.

This supplementary analysis builds on preliminary work presented by Davis, Lopez-Pena, and Mobarak (2021) in providing evidence that those living close to the camps perceive a set of crime indicators to be commonplace, despite minimal differences in the incidence of crime when compared to those living far from the camps. This fits with the literature related to how refugees are often perceived to be dangerous, perpetuated through negative media coverage and fear mongering by politicians in host countries (UNHCR, 2015).

However, those in the refugee camps have a much lower perception of crime within their communities, along with reduced incidence of crime for a host of indicators including business/family disputes, gender-based violence, harassment, and theft. Further analysis on crime and conflict, along with the role of the media in perpetuating stereotypes on ethnically marginalized populations, is encouraged.

Figure 15. Perception of Crime and Conflict

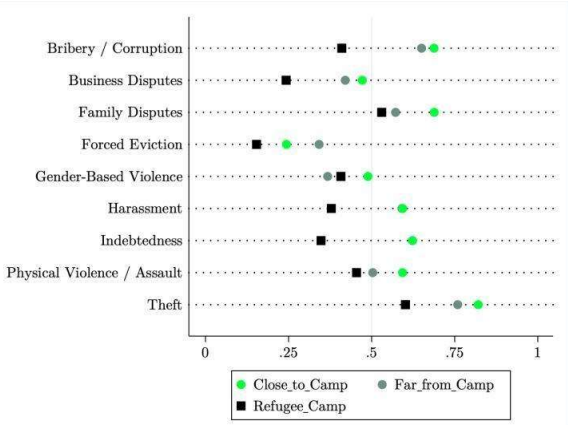


Figure 16. Incidence of Crime and Conflict

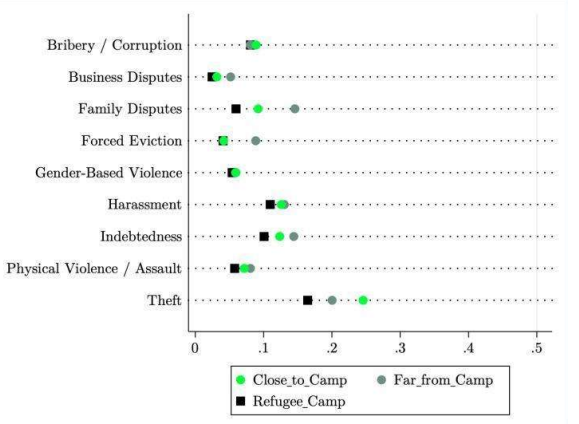
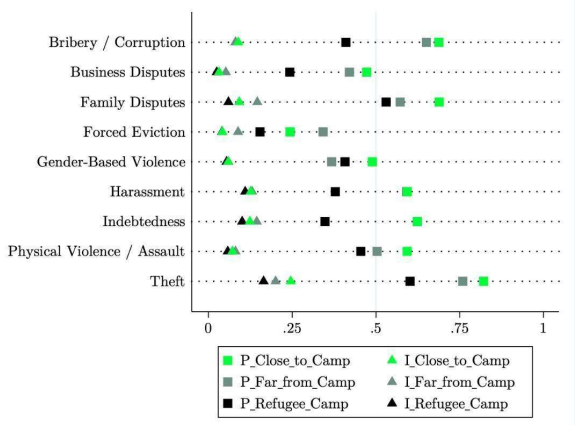


Figure 17. Perception Versus Incidence of Crime and Conflict



Appendix B. Data Files

Please note that .do and .dta files are available from the author upon request. For more information, please reach out to the author at auppal2010@gmail.com.

Is Education Good for Mental Health? A Discussion on the Relationship between Post-Secondary Education and Adulthood Mental Health in Canada

Avah Hawkins, Jingyi Li, Candace Sykes, & Kelsey Wong

ECON490

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The Effect of Crude Oil Pipelines on Border Effects Between Canada and the United States in the Petroleum Industry

Bora Hosal

ECON 494

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2. APPENDIX

A. Data Transformations

By combining pipeline, rail, and truck transportation of the corresponding SCTG codes, interprovincial trade datasets are appended into a single dataset. Individual import and export datasets for every province for the corresponding commodities are appended into a single dataset and a border dummy was added to each observation. Then, by appending interprovince, interstate,

and province-state datasets into a single dataset and collapsing duplicating observations, a total bilateral trade dataset was formed. Canadian and United States GDP datasets are appended into a single dataset and two new GDP datasets corresponding to region of origin and region of destination were created. The distance dataset is formed by calculating the great circle distance between regions using coordinates of the midpoint of every region. Finally, the pipeline dataset was formed introducing a pipeline dummy for regions connected with a pipeline. The final dataset is created by merging the total bilateral trade dataset, GDP datasets, distance dataset, and the pipeline dataset.

B. List of Tables

Table B1. List of US States			
Alabama	Iowa	Mississippi	Oklahoma
Arkansas	Kansas	Missouri	Oregon
California	Kentucky	Montana	Pennsylvania
Colorado	Louisiana	New Hampshire	South Dakota
Connecticut	Maine	New Jersey	Tennessee
Georgia	Massachusetts	New York	Texas
Illinois	Michigan	North Dakota	Washington
Indiana	Minnesota	Ohio	Wisconsin

Table B2. List of Provinces
Alberta
British Columbia
Manitoba
New Brunswick
Nova Scotia
Ontario
Quebec
Saskatchewan

Table B3. List of Pipelines	
Keystone Pipeline	Capline Pipeline
Enbridge Pipelines	Mid-Valley Pipeline
Trans Mountain Pipeline	Wood River Oil Pipeline
Express Pipeline	Bridger Pipeline
Kiantone Pipeline	Portland–Montreal Pipeline

C. Figures

Figure C1.

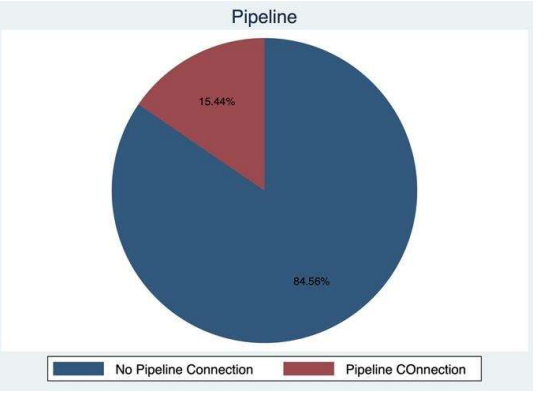


Figure C2.

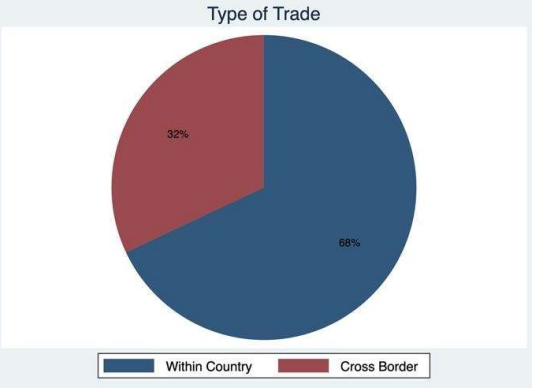
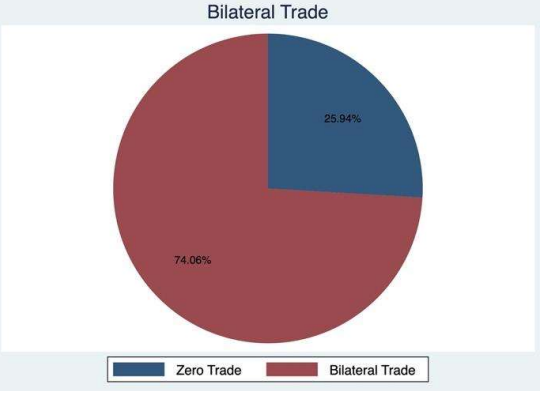


Figure C3.



The Impact of COVID-19 on Malaria Infection and Mortality in Burkina Faso

Lorena Edah

ECON 490

A special thank you to my father, Parfait Edah, for pointing me to Burkina Faso's National Statistics Council website by the Ministry of Health where I found holistic district-level data on malaria infections and mortalities and additionally for helping me secure data on COVID-19 for Burkina Faso by granting me access to the country's private CORUS platform.

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The Effect of Increased Fixed Costs on High-Risk Decision Making: Evidence from Mount Everest

Sarah Wappel

ECON 499

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2. APPENDIX

First, I address an alternative specification for the synthetic control. I chose to limit my sample to the spring climbing season in the main body of the paper, as using all climbing seasons sequentially would have created a highly unbalanced panel and an excess of noise. However, I also note that the main climbing seasons for Cho Oyu and Kangchenjunga occur in the fall, as well as the large number of observations that needed to be dropped in order to utilise a synthetic control estimation, which limit strength of the estimation above. Noting however that 97.5% of climbs attempted on Everest are made during the spring season, I determined that noise created by the inclusion of the fall and winter season, would be small for my treated group. This is confirmed by comparing the treated column in Table 4 and Table A.3 wherein we see that the change in the treatment predictors are small where there was any change at all. Figure A.3 presents the Synthetic Control where all seasons are included using the same predictors as in the main paper. . We see that the Synthetic Everest matches Everest-South more closely in the pre-treatment period, reinforced by the predictors and RMSPE in Table A.3. The weights of this Synthetic Everest are more heavily weighted to Cho Oyu, undoubtedly because the control now includes that peak's main climbing season. We observe the same trends between Everest and the Synthetic Everest in the post-treatment period.

Next I address the concept of permit sharing in the pre-treatment period. In the absence of permit sharing, we do not see the clear change in permit royalties between $10000 and $11000 per climber at the treatment period. Instead permits vary widely between $10000 and $25000, though the 67% of observations pay $10000. The results of the main regressions in the absence of permit sharing are presented in Table A.4. We see a small positive effect of the permit royalty on the number of deaths and injuries in 24 hours in the OLS estimations, but these results are not significant using the full specifications or with the inclusion of the fall season or restricting the sample to climbing above the high camps only. The same coefficients in zero-inflated negative binomial regressions are insignificant and close to zero where the model does converge, as half of the models do not in this case. The coefficients on crowding remain unchanged. These results show there is substantial noise from the varying permit costs, which prevents any conclusions being drawn.

3. FIGURES

Figure A.1: Deaths and Injuries in 24h Distribution (Everest)

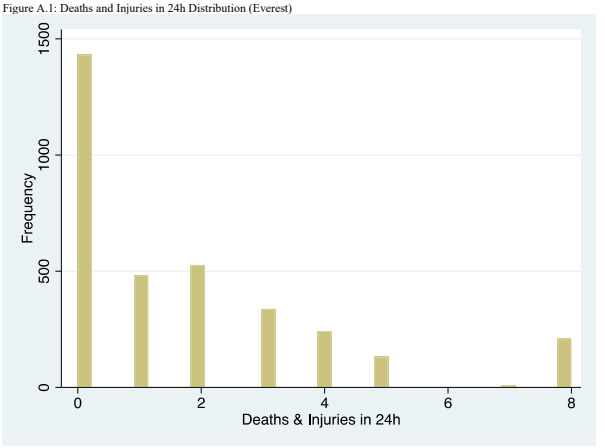


Figure A.2: Deaths and Injuries in 24h > 7900m Distribution (Everest)

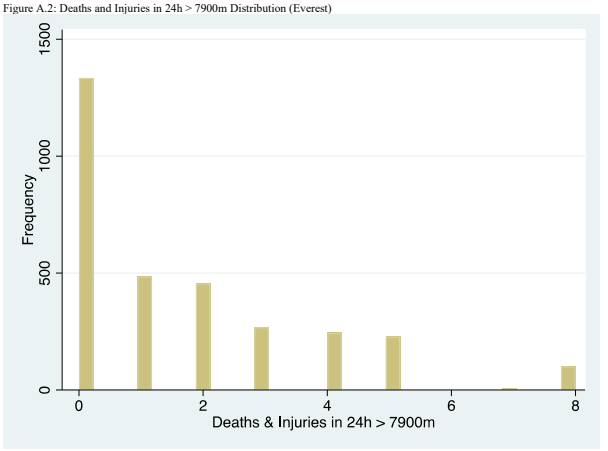
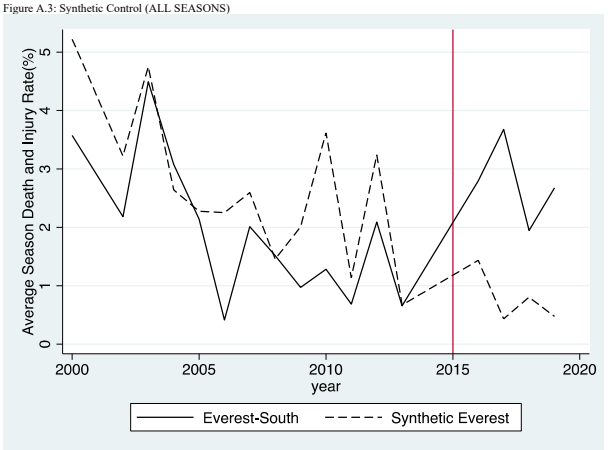


Figure A.3: Synthetic Control (ALL SEASONS)



4. TABLES

Table A.1: Full Main Regression Table

Dependent Variable: Deaths & Injuries in 24h	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS								
Permit Royalty	-0.0292	-0.0193	-0.0180	-0.0150	-0.0157	-0.0166		
	(0.0255)	(0.0237)	(0.0238)	(0.0238)	(0.0207)	(0.0236)		
Bad Weather	0.309***	0.164***	0.173***	0.162**	0.142**	0.160**		
	(0.0746)	(0.0620)	(0.0624)	(0.0644)	(0.0664)	(0.0634)		
Bad Conditions	3.281***	1.561***	1.561***	1.697***	2.175***	1.539***		
	(0.265)	(0.182)	(0.182)	(0.196)	(0.220)	(0.183)		
Crowding		3.985***	3.992***	3.944***	3.207***	3.998***		
		(0.143)	(0.143)	(0.145)	(0.130)	(0.143)		
Age			-0.00411	-0.00453*	-0.00336	-0.00441*		
			(0.00252)	(0.00254)	(0.00240)	(0.00253)		
Oxygen			0.118	0.196	0.179	0.229		
			(0.182)	(0.183)	(0.177)	(0.183)		
Previous 8000m attempt			-0.0110	-0.0121	-0.00840	-0.0117		
			(0.00742)	(0.00770)	(0.00713)	(0.00767)		
Climbers: Hired Climbers				0.0257	0.0338	0.0253		
				(0.0300)	(0.0296)	(0.0273)		
Team Size				-0.00380	-0.00826***	-0.00350		
				(0.00336)	(0.00320)	(0.00336)		
R-Squared	0.427	0.622	0.623	0.626	0.603	0.626		
Panel B: Zero-Inflated								
Permit Royalty	-0.00779	-0.0109	-0.0113**	-0.00990*	-0.00916**	-0.00992*	-0.0102**	-0.00990*
	(0.00609)	(0.00533)	(0.00533)	(0.00514)	(0.00422)	(0.00514)	(0.00516)	(0.00514)
Bad Weather	0.199***	0.177***	0.182***	0.235***	0.229***	0.236***	0.211***	0.235***
	(0.0522)	(0.0542)	(0.0546)	(0.0558)	(0.0557)	(0.0560)	(0.0514)	(0.0558)
Bad Conditions	1.606***	1.131***	1.128***	1.108***	1.264***	1.103***	1.135***	1.108***
	(0.0900)	(0.107)	(0.108)	(0.114)	(0.108)	(0.114)	(0.107)	(0.114)
Crowding		0.529	0.533***	0.527***	0.354***	0.528***	0.523***	0.527***
		(0.0427)	(0.0428)	(0.0430)	(0.0367)	(0.0430)	(0.0424)	(0.0430)
Age			-0.00151	-0.00149	-0.000790	-0.00147	-0.00101	-0.00149
			(0.00104)	(0.00104)	(0.000966)	(0.00104)	(0.00105)	(0.00104)
Oxygen			-0.0493	-0.00132	-0.0284	-0.00223	0.00713	-0.00132
			(0.0606)	(0.0632)	(0.0584)	(0.0632)	(0.0653)	(0.0632)
Previous 8000m attempt			-0.00410	-0.00376	-0.00187	-0.00372	-0.00378	-0.00376
			(0.00291)	(0.00292)	(0.00272)	(0.00292)	(0.00291)	(0.00292)
Climbers: Hired Climbers				0.0416	0.0508***	0.0407***	0.0389**	0.0416***
				(0.0144)	(0.0149)	(0.0144)	(0.0153)	(0.0144)
Team Size				-0.000522	-0.00293**	-0.000522	-0.000521	-0.000522
				(0.00139)	(0.00131)	(0.00139)	(0.00139)	(0.00139)
Zero-Prediction: Same day Summitters	-0.0540***	-0.0541***	-0.0541***	-0.0597***	-0.0558***	-0.0594***	-0.0605***	-0.0597***
	(0.00282)	(0.00285)	(0.00285)	(0.00312)	(0.00305)	(0.00310)	(0.00303)	(0.00312)
Bad Weather	0.0186	0.0256	0.0268	0.270	0.234	0.242		0.270
	(0.164)	(0.168)	(0.168)	(0.164)	(0.159)	(0.165)		(0.164)
Age							0.0162**	
							(0.00789)	
Oxygen							0.449	
							(0.442)	
Climbers: Hired Climbers							-0.0945	
							(0.0622)	
lnα	-16.90***	-17.34***	-17.27***	-16.60***	-17.25***	-17.14***	-17.52***	
	(0.0309)	(0.0572)	(0.0309)	(0.0298)	(0.0217)	(0.0183)	(0.0473)	
$\chi 2$	18196.8	18607.8	17416.7	16475.0	26885.5	16467.1	17027.7	16475.0
Degrees of Freedom	20	21	24	26	26	27	26	26
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Season FE	No	No	No	No	No	Yes	No	No
Observations	3123	3123	3123	3055	3055	3083	3055	3055

Notes: Notes: OLS and negative binomial zero-inflated regressions on Himalayan Database © dataset on climbers and expeditions on Everest's Nepal side 2000-2019. Weather and Conditions Controls include two indicator variables which take on the value of 1 if climbers abandoned their climbs citing advense conditions or bad weather in the same 24-hour period. Individual Controls include an indicator variable for oxygen use, taking a value of 1 if oxygen was used during the climb, and controls for age and previous experience on an 8000m peak in the Nepal Himalaya, Everest or otherwise. Expedition Controls include the variable for the number of climbers on their expedition team and the ratio of climbers to hired climbers. Crowding is an indicator variable which takes the value of 1 when there are more than 150 climbers on the same route in the same 24-hour period. The zero-inflated model uses the number of climbers reaching their high point in the same day as a given climber as well as the bad weather indicator variable as zero-predictors. α is the dispersion parameter for the negative binomial regression, an alpha closer to 0 indicates a distribution closer to the Poisson distribution. Models (5) and (6) are robustness checks. The dependent variable in model (5) is the number of deaths and injuries observed in 24 hours at the highest altitudes of the mountain above the high camps, or approximately, 7900m. Models (7) and (8) are robustness checks for the zero-inflated model. (7) Uses age, oxygen use, and the ratio of climbers to hired climbers as alternative predictors. Model (8) utilizes a zero-inflated Poisson regression, noting the low value of α across specifications.
Robust standard errors are in parenthesis
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table A.2: Synthetic Everest Weights (ALL SEASONS)

Peak	Weight
Everest-North	0.459
Annapurna I	0.003
Cho Oyu	0.469
Dhaulagiri I	0
Kangchenjunga	0
Lhotse	0.07
Makalu	0
Manaslu	0

Table A.3: Synthetic Everest (ALL SEASONS) Predictor Balance

Predictor	Year	Treated	Synthetic
Average Hired Ratio	2005	1.876475	1.916509
Average Hired Ratio	2009	1.480435	1.655451
Average Hired Ratio	2011	1.163209	2.574624
Average Hired Ratio	2012	1.349298	2.117643
Average Hired Ratio	2013	1.294335	1.777624
Average Previous 8000m Experience	2005	1.452555	1.25741
Average Previous 8000m Experience	2009	1.75188	1.249604
Average Previous 8000m Experience	2011	1.97551	1.47249
Average Previous 8000m Experience	2012	1.679487	1.963571
Average Previous 8000m Experience	2013	1.759259	1.862266
Death & Injury Rate	2000	3.574203	4.890045
Death & Injury Rate	2002	2.18414	3.163356
Death & Injury Rate	2003	4.493578	4.596295
Death & Injury Rate	2005	2.139037	2.402381
Death & Injury Rate	2006	0.4116185	2.599862
Death & Injury Rate	2007	2.012012	2.892954
Death & Injury Rate	2009	0.973236	2.428000
Death & Injury Rate	2010	1.281501	3.588071
Death & Injury Rate	2011	0.6864989	1.368056
Death & Injury Rate	2012	2.09205	2.976118
Death & Injury Rate	2013	0.6557377	0.9916913
Root Mean Squared Prediction Error		1.149481	

Table A.4

Dependent Variable: Deaths & Injuries in 24h	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS						
Permit Royalty	0.0166**	0.0123**	0.0140**	0.0122	0.00642	0.0122
	(0.00652)	(0.00581)	(0.00578)	(0.00749)	(0.00685)	(0.00748)
Crowding		3.978***	3.984***	3.944***	3.206***	3.997***
		(0.144)	(0.143)	(0.145)	(0.130)	(0.143)
R-Squared	0.428	0.623	0.624	0.626	0.603	0.626
Panel B: Zero-Inflated N.B.						
Permit Royalty	N/A	-0.00148	N/A	-0.000397	N/A	-0.000441
		(0.00252)		(0.00285)		(0.00285)
Crowding		0.526***		0.523***		0.524***
		(0.0425)		(0.0428)		(0.0429)
Zero-Predictor:	CONVERGENCE		CONVERGENCE		CONVERGENCE	
Same day Summitters	NOT ACHIEVED	-0.0541***	NOT ACHIEVED	-0.0597***	NOT ACHIEVED	-0.0594***
		(0.00284)		(0.00312)		(0.00311)
$\chi 2$	-	18625.4	-	16273.3	-	164271.2
Degrees of Freedom	-	21	-	26	-	27
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Weather & Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes	Yes	Yes
Expedition Controls	No	No	No	Yes	Yes	Yes
Season FE	No	No	No	No	No	Yes
Observations	3378	3123	3123	3055	3055	3083

OLS and negative binomial zero-inflated regressions on Himalayan Database © dataset on climbers and expeditions on Everest's Nepal side 2000-2019. Weather and Conditions Controls include two indicator variables which take on the value of 1 if climbers abandoned their climbs citing adverse conditions or bad weather in the same 24-hour period. Individual Controls include an indicator variable for oxygen use, taking a value of 1 if oxygen was used during the climb, and controls for age and previous experience on an 8000m peak in the Nepal Himalaya, Everest or otherwise. Expedition Controls include the variable for the number of climbers on their expedition team and the ratio of climbers to hired climbers. Crowding is an indicator variable which takes the value of 1 when there are more than 150 climbers on the same route in the same 24-hour period. The zero-inflated model uses the number of climbers reaching their high point in the same day as a given climber as well as the bad weather indicator variable as zero-predictors. Models (5) and (6) are robustness checks. The dependent variable in model (5) is the number of deaths and injuries observed in 24 hours at the highest altitudes of the mountain above the high camps, or approximately, 7900m.

Robust standard errors are in parenthesis

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Authors

Akash Uppal

ECONOMICS

Akash graduated from the University of British Columbia's Bachelor of International Economics program in May 2021. Since joining the Bank of Canada as a Research Assistant in the United States division of the International Economic Analysis department, he has explored frontier issues in applied macroeconomics. He intends to apply this experience in public policy to developing countries, leveraging evidence-based insights from behavioural economics to craft better solutions to bigger problems. In Fall 2022, Akash will begin his Master's degree in International and Development Economics at Yale University.

Avah Hawkins

ECONOMICS

Avah is a 2021 graduate of the University of British Columbia's Bachelor of Arts program, specializing in Economics and minoring in Commerce. She can currently be found working as a Business Analyst in Vancouver. Avah is particularly interested in exploring the intersection between education and mental health, and was inspired to write about this topic as part of her final undergraduate economic research project.

Jingyi Li

ECONOMICS

Jingyi graduated from the University of British Columbia in 2021 with a Bachelor of Arts in Economics. She is originally from Nanjing, China. Currently, she is in Hong Kong and working on her Master's degree in Applied Economics. Her interests include environmental economics, the economics of education and corporate finance. Jingyi intends to graduate in October 2022 and hopes to pursue a professional career in social enterprises or finance corporations, where she can apply her econometric skills and theoretical economic insights.

Candace Sykes

ECONOMICS

Candace Sykes is a recent graduate of the University of British Columbia. Completing her undergraduate degree with a major in Economics in 2021, Candace then went on to complete her Master's of Management degree at Sauder School of Business in early 2022. As a result of her keen interest in the global climate crisis, she hopes to begin a career in business with an emphasis on environmental sustainability.

Kelsey Wong

ECONOMICS

Kelsey graduated with distinction in 2021 from the University of British Columbia's Vancouver School of Economics, with a major in Economics and a minor in Law and Society. She was drawn to economics for its ability to enhance her micro, macro, and global understanding of the past, present, and future world. Kelsey and her co-authors desired to research their topic of how Canadian adulthood mental health is impacted by attaining a post-secondary education so that they could assist in broadening the literature focused on identifying influential factors on mental health.

Bora Hosal

ECONOMICS

Bora graduated in May 2022 from the Bachelor of International Economics program at the University of British Columbia. Through the classes he took and after completing internships in the field, he discovered his passion for international trade and politics. This led him to combine his academic area of focus with his interest in energy economics to write his ECON 494 undergraduate thesis. Throughout his studies, he volunteered in numerous internationally-focused non-profit and student-led organizations and he currently pursues his career working at a Vancouver-based tech startup.

Lorena Edah

ECONOMICS

Lorena graduated in May 2022 from the University of British Columbia's Vancouver School of Economics with a major in Economics and a minor in African Studies. During her undergraduate studies, Lorena solidified her passion for advocacy, policy-making and the policy-research nexus targeting the enhancement of economic progress through key industrial sectors such as health, education, and agriculture. Growing up moving around Africa, more precisely Togo, Ghana, Mozambique, and Burkina Faso, Lorena was inspired by her personal background to write about malaria given its prominence and devastating impact on Sub-Saharan African populations. To further her goal of arduously working towards playing an active role in positive and inclusive economic transformation and development in her home continent of Africa, Lorena will be pursuing graduate studies to achieve a dual degree in public policy & global affairs at both SciencesPo Paris and the University of Toronto.

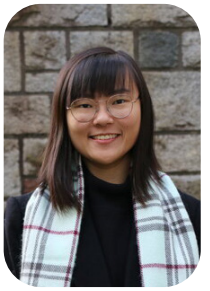
Sarah Wappel

ECONOMICS

Sarah graduated from the University of British Columbia with a Bachelor of Arts (Honours) in Economics in 2021. She is currently completing her Master's in Economics and will be starting a Pre-Doctoral Research Fellowship in Fall 2022, the next step in an exciting journey to obtaining a PhD in Economics. Her main research interests are in history, political economy, and conflict, but the motivation for her undergraduate thesis came from her desire to bridge the gap between her passion for research and her irrational economic-agent love for Type-II/suffer-now-enjoy-later fun in the outdoors.

IONA Team

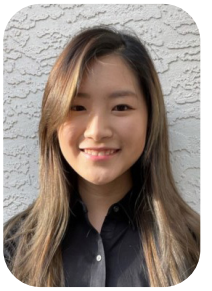
EDITORS-IN-CHIEF



RACHEL LEE

Bachelor of International Economics

As a fourth-year student in the Bachelor of International Economics program, Rachel's interest in economics continues to be developed by her economics classes and life experiences. She became involved with the IONA Journal to gain exposure to the multifaceted field of economics and to have a positive impact on economics undergraduate students and their pursuits. In addition to enhancing her knowledge and interest in environmental economics, Rachel wishes to further her efforts in supporting Indigenous economics and policy development.



ANNIE CHANG

Bachelor of International Economics

Annie is a fourth-year International Economics student. Specifically, she is interested in how economic policies can play a major role in the subjective well-being of individuals across different countries. Annie is fascinated by the intersectionality of economics, culture and rational decision making. She' has been incredibly excited to lead the Volume VII editorial board and has no doubt that it is one of the best.

DIRECTOR OF OPERATIONS

PETER JIANG

Bachelor of International Economics

Peter is a fourth-year year student in the Bachelor of International Economics program and is ecstatic to be managing the nuts and bolts of IONA's operations and logistics. When he isn't trying new teas, or jamming to city pop, you can find him reading up on the newest developments in BC's burgeoning cannabis sector. Economics is a huge joy and passion to Peter, who one day hopes to influence policy in the public sector. He's super excited to be working with such an amazing team, and to contribute to this year's projects.

MANAGING EDITOR

PRANAV AGGARWAL

Bachelor of International Economics

For Pranav, one of the first papers he read was a job market paper on "The Market for Lemons," something he had first heard about in a movie called *Cars*. Pranav found it intriguing and exciting at the same time, with an added element of nostalgia. Nevertheless, Pranav couldn't fathom participating in a process that selected and refined academic papers. Pranav overcame this skepticism when the IONA Journal gave him a platform to explore these abstract economic concepts. He notes it was a daunting challenge but being part of a community of like-minded individuals eased and aided this process. He feels that the IONA Journal gave him an insight into a different side of economics while furthering his interest in the field. In the future, Pranav hopes to take this newfound passion into the field of applied economic research, specifically in the area of International Trade. Pranav believes that any undergraduate student reading this should consider joining this incredible community of scholars.

SENIOR EDITORS

PRANAV AATREYA

Bachelor of International Economics

Pranav is a third-year International Economics major with an interest in sustainability in production and business, particularly, aligning markets with environmental cost structures. He's back to work with the Iona Journal for a second year because he loves the culture and the fact that he can learn through the editorial process. He's also a huge basketball nerd, and keeps trying to use his statistical knowledge to get an edge in his fantasy basketball league (even though it's often unsuccessful).

COLBY CHAMBERS

Bachelor of Arts - Honours Economics

Colby is a third year student in the honours economics program. His interests lie at the intersection of behavioural economics and political economy. In particular, he is fascinated by the events and incentives which can influence individuals' decisions to support particular social movements, laws, and political figures. He is most interested in how these decisions play out within Canada and the United States. Colby is incredibly excited to gain deeper exposure into the editorial process, while working with and learning from such a talented team. He knows that it'll be great.

TINGYU CHEN

Bachelor of International Economics

Tingyu is a fourth-year student in the Bachelor of International Economics program who is particularly drawn to the field of development, labour, and public policy. She is excited to explore a wide array of timely policy topics related to employment, distribution, and the welfare state. As a member of IONA journal, she hopes to help foster innovative ideas among the student scholar community, while gaining a deeper level of insights into contemporary economic research.

NICHOLAS HARTERRE

Bachelor of International Economics

A fourth-year student in the Bachelor of International Economics, Nicholas has a continuously expanding passion for economics. He is specifically interested in how policymakers and the private sector can efficiently leverage economics to address issues relating to climate change. Nicholas is looking forward to working with some of UBC's best undergraduate economic researchers in shaping this year's edition of the IONA Journal.

SONYA SULA

Bachelor of International Economics

Sonya is a fourth-year student in the Bachelor of International Economics program. Her studies at UBC revolve around economics and French. She is particularly interested in the economic analysis of environmental problems and behavioural economics. You can usually find her admiring the amazing views from the Iona Building. She is excited to be back as an Editor at the IONA Journal and looks forward working with such a great team!

TAURAK UPPAPUTTHANGKUL

Bachelor of International Economics, Minor in Asia Studies

Taurak is a fourth year student in the Bachelor of International Economics program with a minor in Asian Studies. She has been exposed to various topics of economics and is most interested in behavioural economics and rational decision-making. In addition to strengthening her skills on other fields in economics and advocating gender equality, she also hope to make meaningful connections and discussions through the IONA Journal.

SAM VOLPÉ

Bachelor of International Economics

Sam Volpé, currently in her 4th year of the BIE program, is also co-authoring a paper on the relationship between economic growth episodes and state fragility with political economist Eric Werker. Passionate about all kinds of social betterment, she is Co-President of the Women in Economics & Policy club and Co-Founder of the Disabilities United Club. You can almost always find her in the Iona Building (her happy place), often remarked upon for her approachability. Ask Sam about anything, be it advice on involvement at the Vancouver School of Economics or on Netflix! She loves getting to know people and she's always down for a chat!

JUNIOR EDITORS

ANDIE BARTOLOME

Bachelor of International Economics

Andie is a second-year International Economics student. She's particularly interested in the field of development and the role economics plays in improving the quality of life of people. She'd love to pursue this field and see how much change data-driven solutions can enact in regions stricken with social inequality. Andie is very excited to join the IONA Journal of Economics team as a junior editor and can't wait to show what the team has in store.

MEGUMI BERNARDO

Bachelor of Arts - Economics

Megumi is a second-year Economics student with an interest in labour economics and public policy. In particular, she is fascinated by how the tools of economics can be applied to alleviate poverty and socioeconomic inequality across various countries. A keen reader of economic literature, she is excited to explore a variety of rigorous research conducted by fellow undergraduate students through her work in the IONA Journal of Economics.

BRENDAN CHENG

Bachelor of International Economics, Minor in Data Science

Brendan is a first-year student in the Bachelor of International Economics with hopes to minor in Data Science. Although his dive into the vast depth of economics has only just begun, he has developed a great interest in the significance of fair and free trade and their roles in the global economy, especially during the COVID-19 pandemic. During his involvement with the IONA Journal, he hopes to work alongside many bright-minded students while exposing himself to countless economic research.

CAILYN COLLINS

Bachelor of International Economics

Cailyn is a first-year Bachelor of International Economics student from Vancouver Island, BC. Because of her interest in the intersection between government policy and economics, she hopes to further research global financial inequality and how economic policies can change in order to improve standards of living for those experiencing poverty. Through working on the IONA Journal, she hopes to meet other students who share a passion for these issues and broaden her own knowledge of modern economic research.

MATTHEW LADD

Bachelor of Arts - Honours Economics

Matthew is a fourth-year student in the Honours Economics program. He is interested in econometrics, labour economics, and environmental economics. He joined the IONA Journal of Economics to meet other students and learn more about economics.

SILAS KWOK

Bachelor of Arts - Major in Economics

As a first-year student in the Bachelor of Arts program, Silas is looking forward to applying economic theories to current events, along with exploring the ideas that shape and govern individual decision-making and policy. He joined the IONA Journal team to meet like-minded peers and advance his interest in reading, researching, and writing about economics. Silas hopes to develop his understanding of economics as a field, discover new applications of economic principles, and learn more about central economic issues. He enjoys reading the literature around health economics, econometrics, and the economics of technological change.

PIUS LAU

Bachelor of International Economics

Pius is a second-year student in the BIE program who is very excited to be working on IONA Volume VII. After working on IONA Volume VI, he is super excited to return to IONA for Volume VII. Pius is interested in the field of developmental economics, with a particular interest in education and working conditions. Pius hopes to gain valuable experience in this field and other economics areas in his time at the IONA Journal. He is super excited to join the IONA Journal and get to work!

RIGEL LIN

Bachelor Arts - Major in Economics, Minor in Commerce

Rigel is a fourth-year student majoring in Economics and minoring in Commerce. His interests, which arose from his studies at UBC, encompass a variety of topics, such as public finance, banking systems, and macroeconomic theory. He is also fascinated by the interactions between economics, politics, and business. Upon graduation, Rigel intends to pursue a career in finance, where his passions for both economics and business can intertwine. He is ecstatic to be working with the editorial team on this year's edition of the IONA Journal.

SHANTANU MEHTA

Bachelor of International Economics

Shantanu Mehta is from Bangalore, India, and a second-year student in the Bachelor of International Economics program. He represented India at the 13th Asia Pacific Young Leaders Summit in Singapore, where he brought to the world stage India's views on globalization. He hopes his interest in development economics will allow him to fight poverty in India and work with government bodies to reform the country's economic policies in the future.

RAYAN MOHAMED

Bachelor of Arts - Major in Economics

Rayan is a second-year student declaring a major in economics. His focus is on financial economics on how the private and public sectors intertwin together. He decided to join the IONA Journal of Economics to expand his knowledge from existing and new research while working with like-minded individuals. Outside of economics, his interests are: competitive running, philosophy and building fluency in French as a fourth language. Rayan plans to merge his background in economics with a legal education after his undergraduate years.

VALENTINA RAMIREZ

Bachelor of International Economics

Valentina is a second-year BIE Student. Whether it be economic development, economic history, environmental or political economy, she will be there in the front row (in this case, first Zoom row), ready to discover new theories and applications. Her main motivation for joining IONA is connected with her desire to help spread economics ideas, proposals, and critical analysis. Being the creator of her first High School Newspaper, and an IONA Junior Editor in past editions, she has witnessed the positive externalities that these projects have upon our generation, and the future to come.

BREANNA SPENCE

Bachelor of International Economics

Breanna Spence is in her first year in the Bachelor of Economics program. She is interested in understanding foreign relations to improve international policy. Specifically through studying how critical race theory and gender affect international communities' economic development. Breanna became a member of the journal to learn how to understand the world through centric economic theory and to be able to contribute to the new age of economics! Breanna is ecstatic to join the editorial team and work with everyone to create a phenomenal Volume this year.

CARRIE WU

Bachelor of International Economics

Carrie is a fourth-year student in the Bachelor of International Economics program and has gained newfound curiosity and interest in the subject through her classes and studies. She hopes that through her involvement with the IONA Journal she will gain more insight into the vast field of economics and find the one she is most passionate about. Alongside the rest of the team, she is excited for the opportunity to work on Volume VII of the journal and hopes that others will find this resource invaluable.

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