

IONA

JOURNAL OF
ECONOMICS



VOLUME VI



IONA

JOURNAL OF
ECONOMICS

VOLUME VI
2021

Vancouver School of Economics

University of British Columbia
Vancouver, BC

Cover Design: Rachel Lee
Logo Design: Sean Durfee

All published content featured in the *IONA Journal of Economics* is
available online at ionajournal.ca.

Please refer to the excluded appendices and references lists in our
digital version.



This work is licensed under the Creative Commons
Attribution-NonCommercial-NoDerivatives 4.0
International License.

To view a copy of this license, visit
<http://creativecommons.org/licenses/by-nc-nd/4.0/>.

Printed in Canada.

First Printing, 2021.

Contents

- II Faculty Foreword
NATHAN NUNN
- IV Student Foreword
JENNY LIU
- VI Letter from the Editors-in-Chief
FELIPE GROSSO AND AKASH UPPAL
- VIII Letter from the Director of Business
Operations
RACHEL LEE

Contributors

I. PAPERS

- I The Effect of Increased Intensity of
Fatalities on the Popularity of Partido
Demokratiko Pilipino-Lakas ng Bayan:
Evidence from the Mayoral 2019 Elections
THOMAS MATTHEW ARANETA
- 29 Are Women Who Out-earn and Out-
work their Husbands Less Happy?:
Evidence from Canada
ANTON BURI, IA MANTECÓN GARCÍA, & JESSICA WU
- 51 The Effect of Trump's Buy American
Hire American Order on H-1B and
American Workers
JUSTINE ENGEL
- 79 Intrahousehold Determinants of Timely
School Attendance in Uganda
JEANNE LEGUA
- 101 The Effectiveness of the Menstrual
Hygiene Scheme in Improving Female
Educational and Autonomy Outcomes
in Target Indian Districts
MANIKA MARWAH

II. HONOURS THESIS

- 127 Does Membership Come With
Benefits? The Effect of ETFs
on Firm Value
RAPHAËL GRACH

III. REFERENCES & APPENDICES

A. PAPERS

B. HONOUR'S THESIS

- i Authors
- iii IONA Journal Team



Faculty Foreword

Dear Reader,

I am very excited to be writing this year's foreword for the sixth volume of the IONA Journal of Economics, an annual publication of top undergraduate research at the University of British Columbia. Founded in 2016, the Journal has provided visibility to outstanding undergraduate research in economics and served as the launchpad for discussions between economics students and faculty at the Vancouver School of Economics. In a few short years, The Journal has already developed a reputation as a high-quality outlet that produces excellent scholarly research that significantly improves our understanding of the world around us.

This year's editorial team has produced an excellent volume that features six impressive original research papers by UBC undergraduate students.

In the early stages of my career as an economist – starting first as an undergraduate at Simon Fraser University in the late 1990s, then as an M.A. and Ph.D. student at the University of Toronto in the early 2000s, before coming to UBC as an Assistant Professor from 2005-2007 – I (falsely) viewed the heart of the profession as being those top economists whose articles we all read, published in journals like the *American Economic Review* or the *Quarterly Journal of Economics*. While there is certainly value in these scholars and in these publications, with age I have come to learn that the most important part of the profession, and its engine of growth and its dynamism, lies in the students. Yes, the Ph.D. students. But as importantly, also our Undergraduates who are the Ph.D. students and professors of the future.

It is the students who are reading the current frontier of research, thinking deeply about it, and then making the big innovations that move the frontier forward. Real change comes from the younger generations of scholars: the next decade of intellectual innovators who are now undergraduates doing research such as that produced in this volume. In my mind, this is what is truly exciting about the research highlighted in the Journal. It provides the reader a glimpse and sampling into the origins of tomorrow's intellectual breakthroughs.

The fresh ideas that younger generations of scholars bring to the profession are on full display in this volume. The topics covered by the articles are not your traditional topics covered by past generations of economics. The research explains topics beyond firms, labour markets, goods markets, and consumer behavior and offers deep insights into pressing big-picture, real-world globally-important issues such as the effects of the extra-judicial killings during President Duterte's war on drugs; relative earnings in marriages and its effect on spousal happiness; household-level determinants of child education in rural Masaka, Uganda; the effect of access to menstrual hygiene technologies on girls education in India; firm-level consequences of exchange-traded funds; and the effects of Trump's immigration policies.

I encourage you to catch a glimpse of the future of our profession as you read the most recent edition of the IONA Journal of Economics. As you will see, the future of scholarship within economics looks very bright!

Sincerely,

Dr. Nathan Nunn
Frederic E. Abbe Professor of Economics
Harvard University

Student Foreword



Dear Reader,

This sixth edition of the IONA Journal comes after a tumultuous year of challenges, change, and adaptation. On behalf of the Vancouver School of Economics Undergraduate Society (VSEUS), I would like to sincerely congratulate the incredible IONA Journal team this year on a successful launch. The resilience to continue showcasing the incredible papers written by Economics students at UBC is remarkable and shows the journal's dedication to the community. Together, VSEUS hopes that our shared mandate for enhancing student academic experiences at the Vancouver School of Economics (VSE) can continue to flourish.

I'd like to particularly recognize the innovation and hard work that this year's IONA Journal leadership has brought to the table: thank you to Co-Editors-in-Chief Akash and Felipe, Director of Business Operations Rachel, and the larger team for the outstanding work that you have done this year – it has not gone unnoticed. Our community is built upon the voluntary hours that student leaders like yourselves spend on tasks such as brainstorming new engagement strategies and reviewing countless submissions to successfully curate such an excellent collection of student research. VSEUS is committed to continue supporting IONA Journal's growth and the original research pursuits of VSE students, as we strongly believe the power of our education extends far beyond the classroom and well into real-world issues. IONA Journal is a critical component of our student community, and VSEUS is honoured to continue supporting its operations.

Readers, thank you for picking up this edition and engaging with the incredible initiative of IONA Journal. I hope you learn something new from the papers of our remarkable student scholars in the pages to come, perhaps through learning more about topics that sparked your entry into Economics or stumbling across a surprising empirical finding that defied your previous assumptions. Enjoy the sixth instalment of the IONA Journal of Economics, as we celebrate the incredible accomplishments of our talented students. Cheers!

Sincerely,

Jenny Liu
President
Vancouver School of Economics Undergraduate Society

Letter from the Editors-in-Chief



Dear reader,

Not unlike the staunch trees that intersperse the Vancouver landscape, the ideas of scholars begin as seeds. Braving winters of regression and weathering the scorch of critique, researchers quickly discover that the academic writing process is variable and non-linear. The iterative nature of nourishing one's study into a replicable and empirically-sophisticated analysis can be fulfilling in retrospect, but prospectively challenging, if not frustrating. Undergraduate researchers often feel inundated by the deluge of econometric tools at their disposal, intimidated by the uncharted waters ahead of them. Yet, these researchers persist, laying the roots for further study and inspiring others to turn the solitary sapling into a forest.

A half-decade after its inception, the IONA Journal of Economics remains committed to showcasing the forefront of undergraduate research in the discipline of economics. Not only do the topics range from economics to finance to political economy, but they investigate policies across borders. Half of Volume VI's articles find their focus in North America, examining the link between life satisfaction and traditional gender roles, exploring the effect of ETFs on firm value, and studying the effect of a Trump-administration executive order on H-1B visas

and American workers. The other half turn their attention internationally, detailing the Menstrual Hygiene Scheme's impact on female educational and autonomy outcomes in target Indian districts, characterizing the intrahousehold determinants of timely school attendance in Uganda, and connecting drug violence with 2019 mayoral election outcomes in the Philippines. The work of the authors featured in Volume VI is outstanding and a testament to the ingenuity and creativity of a thriving scholarly community at the Vancouver School of Economics. More broadly, they reflect a trend towards diversity and rigor in undergraduate economics research.

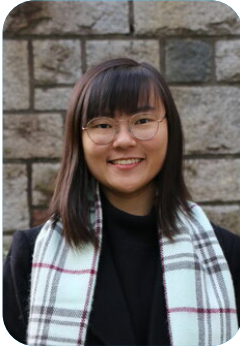
The IONA Journal of Economics is the product of the efforts of dozens on our editorial board, operations staff, VSEUS leadership, and outstanding members of our faculty review committee. Without their work and their flexibility in adjusting to online communication tools, this initiative would not be possible; thank you all for connecting our community and producing the IONA Journal of Economics Volume VI. Finally, and perhaps most importantly, we are grateful to you, the reader, who has chosen to pick up this journal and engage with the scholarly discourse. We hope the pages ahead satiate your intellectual curiosity and quest for knowledge. In your search for answers, may you find better questions.

With much love and gratitude,

Akash Uppal & Felipe Grosso
Editors-in-Chief
IONA Journal of Economics Volume VI

Letter from the Director of Business Operations

VIII



Dear Reader,

As you explore Volume VI of the IONA Journal of Economics, I wish for you to consider the theme of “impact”. This notion has appeared in multiple contexts.

One of the ways that Volume VI explores the idea of “impact” is through the journal’s research papers: we received outstanding studies that explore the potential impacts of policies, development, financial markets, and more. It has been an honour to review these papers – especially the ones that have been selected for publication in IONA’s Volume VI. To all the authors: you have truly made an impact on your peers on the Editorial Board, VSE faculty in our Faculty Review Committee, and the future of economics research.

Additionally, the unique insights and thoughtful reviews of our Faculty Review Committee have created significant impacts on the authors’ research, and on the final version of Volume VI. Thank you to all the faculty for your insightful comments, and for supporting undergraduate economics research. Perhaps Volume VI has had the most in-depth impacts from our Editorial Board, comprised of bright and thoughtful Senior Editors, Junior Editors, and our Managing Editor Cecilia Pang. Working alongside each of you has been enlightening, especially with how your diversity in fields of study, year levels, and life experiences have made your perspectives and impacts unique. Thank you for every conversation and connection throughout the Editorial Review and during this distinct year of virtual operations.

Finally, my deepest gratitude is dedicated to Akash Uppal and Felipe Grosso, the Editors-in-Chief for Volume VI. As you read this publication, you’ll be witnessing the impacts that both of them have had

in the development of this year's volume: novel ideas that enhanced each paper, detailed editorial suggestions, and a quality of work ethic that has inspired every person involved with the IONA Journal. Akash and Felipe have been the best team to work with while producing Volume VI, and I hope that my impacts could complement their work in the best way.

Perhaps reading these research ideas will create their own impacts on you as you pursue your academics, career goals, and life. With appreciation, I hope that Volume VI of the IONA Journal of Economics will continue to inspire communities and will stay true to the value of "impact".

All my best,

Rachel Lee
Director of Business Operations
IONA Journal of Economics Volume VI

Contributors

EDITORS-IN-CHIEF

Felipe Grosso & Akash Uppal

DIRECTOR OF BUSINESS OPERATIONS

Rachel Lee

MANAGING EDITOR

Cecilia Pang

SENIOR EDITORS

Pulkit Aggarwal

Annie Chang

Livia de Oliveira

Peter Jiang

Erin Lee

Isha Trivedi

Taurak Uppaputthangkul

Sarah Wappel

JUNIOR EDITORS

Pranav Aatreya

Megumi Bernardo

Lucienne Chang

Mahak Dugar

Florent Dusenge

Pius Lau

Valentina Ramirez

Angela Sequeira

Sonya Sula

Ashtosh Verma

Sam Volpé

Jin Wang

Carrie Wu

Amy Ye

AUTHORS

Thomas Matthew Araneta

Anton Buri

Justine Engel

Ia Mantecón García

Raphaël Grach

Jeanne Legua

Manika Marwaha

Jessica Wu

FACULTY REVIEW COMMITTEE

<i>Dr. Siwan Anderson</i>	<i>Vancouver School of Economics</i>
<i>Dr. Jonathan Graves</i>	<i>Vancouver School of Economics</i>
<i>Dr. Wendy Kei</i>	<i>Vancouver School of Economics</i>
<i>Dr. Thomas Lemieux</i>	<i>Vancouver School of Economics</i>
<i>Dr. Matt Lowe</i>	<i>Vancouver School of Economics</i>
<i>Dr. Guillermo Marshall</i>	<i>UBC Sauder School of Business</i>
<i>Dr. Angela Redish</i>	<i>Vancouver School of Economics</i>
<i>Dr. Munir Squires</i>	<i>Vancouver School of Economics</i>
<i>Dr. Felipe Valencia Caicedo</i>	<i>Vancouver School of Economics</i>

SPECIAL THANKS

<i>Dr. Nathan Nunn</i>	<i>Harvard University</i>
<i>Dr. Patrick Francois</i>	<i>Vancouver School of Economics</i>

SPONSORS

Vancouver School of Economics
Vancouver School of Economics Undergraduate Society

The Effect of Increased Intensity of Fatalities on the Popularity of Partido Demokratiko Pilipino-Lakas ng Bayan: Evidence from the Mayoral 2019 Elections

Thomas Matthew Araneta

ECON 494

ABSTRACT

President Rodrigo Duterte of the Philippines has implemented a war on drugs that has resulted in thousands of extra-judicial killings of drug suspects. It is yet to be rigorously explored how the killings from his war on drugs have affected his popularity. Using plausibly exogenous variation in the distance of each municipality to its closest sea and air port, I examine the causal effect of drug related fatalities on the vote share for Partido Demokratiko Pilipino-Lakas ng Bayan (PDP Laban). I conclude that there is moderate evidence that a more intense measure of fatalities has a statistically significant and positive relationship with the vote share for PDP Laban, the political party Duterte is associated with. Additionally, there is a statistically significant and positive relationship between drug related fatalities and the probability that a PDP Laban candidate will run in a given municipality.

INTRODUCTION:

Since Rodrigo Duterte was elected as president in 2016, the Philippines has become a hot bed of extra-judicial killings of drug suspects. While estimates vary on the death toll, according to the United Nations High Commissioner for Human Rights, there were at least 8663 deaths as early as July 2016. Consequently, the war on drugs has garnered international condemnation from various groups including Human Rights Watch and Amnesty International. Moreover, according to Quimpo (2017), the drug war has gone largely unprosecuted which is in part due to Duterte's refusal to punish police officers. There is also anecdotal evidence that these deaths have mostly been low profile individuals, such as addicts and low level pushers. For example, according to the Human Rights Watch 2018 World Report, the deaths have mostly been among poor Filipinos. Despite all of this, Duterte has consistently polled well throughout his term.

While there has been a lot of media coverage of the drug war, there hasn't yet been a rigorous analysis of how the drug war has affected political outcomes. Such an analysis is important for the role of the drug war in future discourse. For example, one important consideration is if the aggregate support for Duterte is driven primarily by people who have not been exposed to extra-judicial killings. In other words, if Duterte's administration is supported by the individuals least affected by its drug war policy, that has serious implications for how the administration conducts the drug war. Understanding the relationship between the exposure to the drug war and support for Duterte's administration will reveal how Filipinos update their perceptions of the drug war's efficiency. This paper aims to provide this rigorous analysis and fill the present gap. The specific question I ask is "How does a higher number of drug related killings in your municipality affect the vote share outcome for Partido Demokratiko Pilipino-Lakas ng Bayan? (PDP Laban)". I use an instrumental variable approach using the distance of each municipality to its closest airport and its closest seaport to construct a plausibly exogenous measure of fatalities. I find moderate evidence of a positive relationship between the measure of fatalities and the vote share for PDP Laban. I also find stronger evidence that the measure of

fatalities positively affects the probability that a PDP Laban candidate ran in a given municipality.

The relationship between conflict and economics has been explored extensively in the past. Increased conflict insecurity has been linked to distrust in the government as well as increased palatability of authoritarianism (Blanco 2012). It has also been heterogeneously linked to economic performance, with some works showing positive outcomes (e.g. Miguel and Roland 2011) and others showing negative outcomes (e.g. Hodler 2018). However, there has been no paper that has linked increased intensity of conflict to the popularity of a politician or political party that champions the conflict driving policies. This paper attempts to make this specific link. And while Ravanilla, Sexton, and Haim (2020) demonstrate the increased popularity of mayors who more vigorously implemented the drug war, they are unable to establish a causal relationship. I attempt to establish a causal mechanism between increased intensity of fatalities resulting from the drug war to the popularity of PDP Laban by using plausibly exogenous variation in the distance of each municipality to its closest air and sea port.

The dataset that I use to conduct my analysis is a combination of different datasets. I use data from the Armed Conflict Location and Event Data Project for the data on the drug related fatalities. I use electoral data provided to me by Professor Cesi Cruz at the Vancouver School of Economics at the University of British Columbia who scraped the Philippine COMELEC database. I also use various government data including data on poverty incidence and census data collected by the United Nations Office for the Coordination of Humanitarian Affairs. Finally, I use seaport data from the World Food Program, and airport data from Geoportal Philippines, to construct my distance instruments.

This paper proceeds as follows: Section II looks at the previous literature. Section III presents the data. Section IV presents the estimation strategy and the proposed instruments. Section V presents the results and analysis. Section VI concludes.

REVIEW OF LITERATURE:

A) The Philippine Drug Trade

It is important to set the stage for the Philippine Drug Trade. On the drug supply side, according to a 2012 report by the Philippine Drug Enforcement Agency (PDEA), there are two transnational groups that operate and smuggle drugs into the Philippines: the African Drug Syndicate and the Chinese / Chinese-Filipino drug syndicates. In 2013, the Chinese-Filipino drug syndicates were linked by the PDEA to the Mexican Sinaloa Drug Cartel. Luong (2015) notes that because of its extensive coastline, the Philippines is an ideal location in the region to use as a transit hub for these transnational groups to smuggle contraband. On the demand side, Simangan (2017) cites PDEA police reports and finds that the drug usage in the Philippines, based on drug related arrests, decomposes into the following percentages: methamphetamine hydrochloride, locally known as Shabu, accounts for 95.47%, marijuana accounts for 4.29%, and other drugs (e.g. cocaine) account for 0.24%. Simangan also cites a 2012 Dangerous Drug Board Survey that found that there could be up to 1.3 million drug users in the Philippines.

Since being elected in 2016, President Rodrigo Duterte has quickly escalated the drug war in the Philippines. Johnson and Fernquest (2018) cite a New York Times editorial that puts the number of drug related extra-judicial killings (EJK's), committed by either the Philippine National Police or vigilante groups, at 9400 after the first 10 months of Duterte's presidency. In addition, they cite human rights groups that put the death toll from Duterte's drug war as of January 2018 between 12,000 and 16,000. They also note that Duterte has been highly encouraging of the killings, on multiple occasions having made statements such as "I don't care about human rights, believe me. There is no due process in my mouth." Despite all this, Duterte's popularity remains high, and they note that the average popularity of Duterte through 2016-2017, according to Pulse Asia polls, was 84%.

B) Prior Work on Duterte's Drug War

The main work in political economy on the Philippine Drug War is being done by Ravanilla, Sexton, and Haim

(2020) in a working paper. They argue that since Duterte was a political outsider in the Philippine context (i.e. not from one of the traditional patronage networks), he would not have had the same political apparatus to enforce his drug war and other policies in the local municipalities that a traditional politician would have. As a consequence, they predict that outsider mayors who won closely contested elections in 2016, themselves in need of both political resources and popularity to leverage, would have been more compliant with his drug war policy. For instance, they might have been less restraining of the local police branches compared to a mayor from the Liberal Party, the incumbent party in 2016. Using difference in differences and regression discontinuity designs, they find that the municipalities where outsider mayors won closely contested elections had a higher rate of police reports of drug related crime (indicating more stringent police monitoring of drug use) and a higher probability of observing a police killing of a drug suspect (without any significant change in the probability of observing a vigilante killing of a drug suspect). They also find that outsiders (as they defined in the 2016 elections) performed about five percentage points better than former insiders in the 2019 midterm elections. However, they are unable to establish the drug war as the causal mechanism that drives this improvement in the electoral performance of outsiders because of plausible endogeneity (e.g. voters choosing former outsiders because of the association with Duterte's popularity, rather than with the drug war directly). The aim of this paper is to directly address this causal question of how the drug war, and more specifically a higher incidence of drug related killings (both by the state and by vigilante groups) causally affects Duterte's party's performance in the 2019 midterm elections.

C) The Relationship of Conflict with Economic Indicators

There is a wide range of literature that seeks to address the effect of conflict on economic, social, and political indicators. In many cases, there is an identification problem for the causal effect of conflict on the outcome of interest.

One strategy used to circumvent these identification problems is the creation of "synthetic" controls. Abadie and

Gardeazabal (2003) note that in addition to being unable to exploit the time-series variation to evaluate the effect of the terrorism of the Basque Homeland and Liberty group in the Basque Country due to increased terrorist intensity being simultaneous with a Spanish economic downturn, they were also unable to directly compare the Basque Country to other regions in Spain because of differences in attributes that would have affected economic growth prior to the onset of intense terrorist activity. To work around this, they construct a “synthetic” Basque Country by weighting different regions to construct an optimally similar region with respect to pre-1975 determinants of economic growth. They find that terrorism causes a negative impact on GDP per capita relative to the synthetic region they construct. Hodler (2018) uses a similar methodology to analyze the effect of the Rwandan Genocide on economic indicators. He finds that the Rwandan Genocide had a negative effect on Rwandan GDP and Rwanda took about 17 years to recover. On the other hand, Bove et al. (2016) argue that the negative effect of conflict on economic indicators is not always significant, and its magnitude depends on the specific nuances and nature of the conflict in its own context. They use the same “synthetic” control technique and show that civil war has heterogeneous economic impacts: it is not always negative and significant, and is sometimes positive and significant. One specific example of this is explored in Miguel and Roland (2011), who use the distance to the 17th parallel as an instrument for the bombing intensity in Vietnam and find a marginally positive relationship between bombing and poverty levels in the 1990s, as well as a positive relationship between bombing and growth rates of per capita expenditures in the 1990s. This indicates that while the bombing had a marginal negative effect on the level of poverty immediately after the war, more intensely bombed places quickly caught up.

Others have explored the effect of the economic situation on conflict. A similar problem arises because of the possibility that conflict and the economic situation are determined endogenously. Bazzi and Blattman (2014) use plausible exogenous variation in commodity prices to examine the effect of household and state income on conflict intensity. They find that higher commodity prices are linked to lower conflict intensity, lending credence to “state capacity” and

“opportunity cost” arguments, namely that states are more capable of dealing with conflict when they have more revenue and that individuals are less likely to participate in conflict when the opportunity costs of conflict are higher, respectively. They find little support for the “state prize” argument, namely that as the state’s revenue increases, there is a higher return to usurping it. Dube and Vargas (2012) use the same methodology and find heterogeneous results. They find support for the opportunity cost argument in labour intensive commodities, while also finding support for the “rapacity” effect for non-labour intensive commodities, which is similar to the “state prize” argument.

D) Blanco (2012) and the Effect of Conflict on Trust in Government

With respect to trust in democratic institutions, Blanco (2012) notes that perceptions of insecurity (a measure of how safe people feel) in Mexico increased because the violent drug conflict in the country intensified in 2006. They then explore the effect of the increased perceptions of insecurity on trust in democracy and government institutions. They find that in Mexico, higher perceptions of insecurity have a significant negative relationship with support for democracy. Additionally, Blanco (2012) finds that higher perceptions of insecurity also lead to decreased trust in Mexican institutions, especially in the judiciary and the police. These results are affirmed by Blanco and Ruiz (2013) who find that in Colombia, exposure to violent conflict leads to decreased satisfaction with democracy, decreased trust in the government, and decreased trust in political institutions.

This result somewhat contrasts with the case in the Philippines. As noted earlier, Duterte has consistently polled high in the Pulse Asia surveys. While a Filipino might give Duterte a rating based on a variety of factors, when one considers the findings of Blanco (2012) in Mexico and Blanco and Ruiz (2013) in Colombia, one would expect that Duterte’s violent drug war would result in him having a lower trust and approval rating. This is especially peculiar since Blanco (2012) finds that the most significantly affected institutions when there are high perceptions of insecurity are the judiciary and the police, and the arm of the state responsible for the extra

judicial killings in the Philippines is the police.

On the other hand, Blanco (2012) finds that higher perceptions of insecurity are associated with increased palatability of authoritarianism. It might be the case that previous frustration with the drug trade and with an inefficient justice system has had a similar effect in the Philippines as it did in Mexico, pushing people to demand more extreme measures of dealing with crime. This paper aims to conduct a rigorous analysis to see which channel will dominate in the Philippine context, and this paper hopes to address these questions directly as well.

DATA SUMMARY:

I will be using is a combination of different datasets, and will discuss the sources and transformations in this section.

A) Data Sources

The conflict data I used is based on a dataset constructed by The Armed Conflict Location & Event Data Project (ACLED). They have geocoded incidents of conflict in their data bank, identifying the municipality, province, and region in the Philippines where the conflict took place. Using their extraction tool, I extracted all the incidence of violence against civilians in the Philippines. Their data has a list of incidents from 2016 up until the end of 2019, and a count of the number of fatalities associated with each incident.

The electoral data I use is a dataset sent to me by Cesi Cruz, a professor at the University of British Columbia. Professor Cruz scraped the Philippine COMELEC for vote outcome data for the 2019 Mayoral elections. The unique unit of observation in this dataset is a precinct candidate combination (for example, candidate x in precinct y is a unique observation). The dataset contains the total votes a candidate received in a specific precinct, the total votes gathered in the precinct, and the candidate's party. The observations are also geocoded, allowing me to identify the region, province, and municipality.

Table 1: Summary Statistics of Main Controls by Region

Region	Poverty Incidence in 2015		Log of population in 2015	
	mean	Std. Dev	mean	Std. Dev
Bangsamoro Autonomous Region in Muslim Mindanao (BARMM)	0.55	0.12	10.22	0.57
Cordillera Administrative Region (CAR)	0.21	0.09	9.5	0.89
National Capital Region (NCR)	0.03	0.01	13.1	0.89
Region I (Ilocos Region)	0.12	0.04	10.25	0.92
Region II (Cagayan Valley)	0.15	0.04	10.22	0.95
Region III (Central Luzon)	0.13	0.05	11.05	0.75
Region IV-A (CALABARZON)	0.13	0.1	10.99	0.96
Region IV-B	0.26	0.09	10.32	0.8
Region V (Bicol Region)	0.39	0.09	10.63	0.66
Region VI (Western Visayas)	0.22	0.05	10.69	0.62
Region VII (Central Visayas)	0.3	0.11	10.49	0.82
Region VIII (Eastern Visayas)	0.4	0.08	10.04	0.73
Region IX (Zamboanga Peninsula)	0.46	0.11	10.45	0.63
Region X (Northern Mindanao)	0.41	0.14	10.42	0.79
Region XI (Davao Region)	0.31	0.13	11.02	0.74
Region XII (SOCCSKSARGEN)	0.41	0.12	11.19	0.53
Region XIII (Caraga)	0.39	0.06	10.14	0.76
Observations	1615			

Notes: This table shows the main controls, namely the log of population and poverty incidence of each municipality

For my controls, I have a dataset from the United Nations Office for the Coordination of Humanitarian Affairs (OCHA) in the Philippines which combines Philippine Standard Geographic Codes with poverty indicators taken from the Philippine Statistics Authority. The data contains each municipality's poverty incidence in 2009, 2012, and 2015. Once again, the dataset also contains the municipality's associated region and province. In my main analysis, I use the poverty incidence and the log of population in 2015.

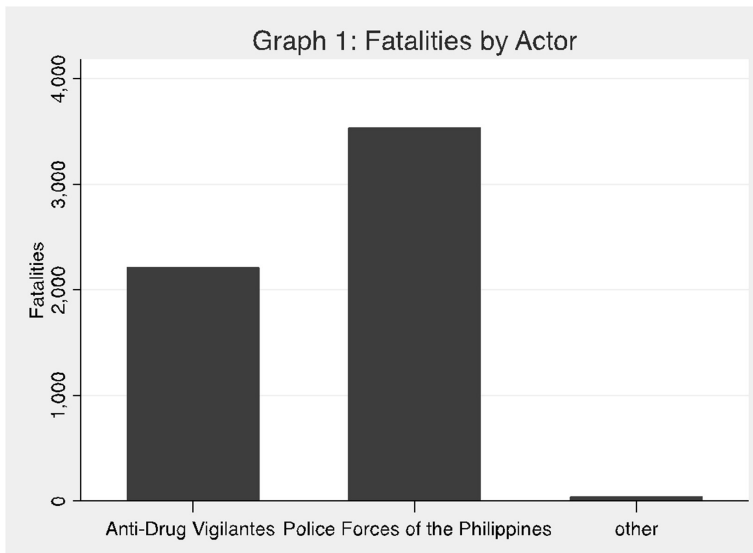
B) Data transformations

I perform multiple transformations to my datasets to integrate them together and to construct the variables of interest.

ACLED data:

The ACLED data contains all the observations of violence against civilians from 2016 to 2019, including observations unrelated to the drug war. In addition, the ACLED data identifies both the actor perpetrating the violence and the associated actor or the recipient of the violence. This allows

me to isolate the drug related incidents for my analysis. I retain only the observations that include violence against drug suspects, and drop all the observations unrelated to the drug conflict. For instance, I drop observations where the associated actor is a “government official” rather than a “drug suspect” because the former is unrelated to the drug conflict. In this dataset, the two main actors committing the overwhelming majority of killings are the Police Forces of the Philippines and Anti-Drug Vigilantes. Note that the dataset occasionally, and in no discernible pattern, specifies the branch of the police that was involved in the incident (e.g. special weapons and tactics, regional intelligence unit, provincial intelligence unit, etc). I amalgamate all these different branches into “Police Forces of the Philippines” by recoding them. The remaining actors are different armed groups in the Philippines, as shown in Graph 1.



Since each unit of observation is a different incident of drug related violence, I collapse my data, aggregating fatalities at the municipality level while keeping the province of the municipality to improve my merge with the electoral dataset. After collapsing, each municipality is now a unit of observation and contains the count of the drug related fatalities in that municipality from 2016-2019.

Electoral data:

Each unit of observation I start with is a precinct-candidate combination. Every municipality has different precincts, and each precinct has the candidates that are running for election in that municipality. If a municipality has 3 precincts and 4 candidates, then that municipality will have 12 observations. I need to aggregate the total votes in each municipality, and the total votes each candidate receives. Within each precinct, there is a variable that indicates the total votes in the precinct, and the total number of votes a given candidate received within that same precinct. To aggregate, I sum the total votes at the municipality level and the candidate votes at the candidate level. That is, for each municipality I add up the total votes in its precincts. And for each candidate, I add up their total votes across the same precincts. Since each candidate only runs in one municipality, I then generate a variable for the candidate's vote share, equal to the candidate's votes divided by the total votes in the municipality.

Note that the unit of observation is still a precinct candidate combination, but the vote share variable should not be changing for the same candidate. If candidate x ran in a municipality with 5 different precincts, then that candidate's name appears in 5 different observations. In each of those observations, the candidate's total votes, the total votes in the municipality the candidate ran in, and the ratio of the two should not change. Sometimes the municipality that a candidate ran in was miscoded in a specific observation out of the several that a candidate might appear in. For example, if candidate x ran in municipality y which had 5 precincts, "y" should appear as the municipality for each of the 5 observations of candidate x. But sometimes, "y" is miscoded as another municipality, or is missing, whereas in the other observations for candidate x, "y" is correctly indicated. This causes the same candidate to have different values for vote share in different observations, which should not happen. To work around this, I drop all the observations when the vote share variable is greater than 1. Afterwards, there were still 8 candidates who appeared in more than one municipality. Since I cannot determine which municipality they are correctly associated with, I drop all these candidates. This removes 8 candidates and 12016 observations,

leaving me with 3953 candidates and 233677 observations. After this step, the candidates had uniquely associated municipalities.

Variable	Independent or Dependent	Description
PDP vote share	Dependent	Continuous measure of a PDP Laban candidate's vote share outcome
Binary elect PDP variable	Dependent	Dummy variable equal to 1 if vote share is > 0.5 for PDP Laban Candidates
Fatalities	Independent	Continuous count of fatalities in a municipality
Binary fatality variable	Independent	Dummy variable equal to 1 if fatalities is > 0 in a municipality.

Since the main variable I am interested in is the vote share outcome for PDP Laban, which is a percentage, I don't need to keep the observations for other candidates. For municipalities without a PDP Laban candidate, I code the PDP Laban vote share as 0%. I am then finally able to merge my controls and my independent and dependent variables into a single dataset that I can use for my analysis.

ESTIMATION STRATEGY:

I use a two-stage-least-squares instrumental variable approach to identify the effect of increasing numbers of fatalities on the popularity of PDP Laban, the party of the incumbent Philippine president Rodrigo Duterte. I use robust standard errors.

A) Second stage:

$$PDP\ vote\ outcome_c = B_0 + B_1 \widehat{fatalities}_c + B_2 Poverty_c + B_3 Population_c + u_c$$

Here the dependent variable is the outcome for the PDP Laban candidate in a given municipality. I run two regressions, one where I include municipalities where PDP Laban candidates did not run, and set PDP vote share equal to 0, and one where I only include municipalities with PDP Laban candidates. I do this each time for all the different

independent-dependent variable combinations. My other dependent variable is a dummy 0-1 variable, equal to 1 if the PDP vote share variable is greater than 0.5 (or 50%), indicating that a municipality elected a PDP Laban candidate. My other independent variable is a dummy 0-1 variable, equal to 1 if the fatality variable is greater than 0, indicating that a municipality observed a fatality.

Table 2: Summary of Main Variables of Interest

Variable	Mean	Std. Dev.
PDP Vote Share	0.17	0.23
Binary Elect PDP variable	0.05	0.22
Total Count of Fatalities in Municipality	2.89	14.63
Binary Fatality variable	0.35	0.48
Log of Population	10.50	0.91
Poverty Incidence in 2015	0.29	0.16
Distance to Closest Sea Port (km)	42.11	33.97
Distance to Closest Air Port (km)	32.03	20.95
Number of Observations	1615	

Notes: This table shows the summary statistics for the main variables of interest

I also control for the log of each municipality's population level in 2015, along with each municipality's poverty incidence in 2015.

B) Endogeneity concerns

Controlling for population and poverty does not guarantee my coefficient on fatalities is well identified. It is possible that there are confounders. For example, Ravanilla, Sexton, and Haim (2020) point out that the ability for Duterte to execute the drug war also depends on the willingness of local government units to cooperate. Consequently, they find that municipalities that elected an outsider mayor in a close election in 2016 observed more fatalities because outsiders need to authentically signal their support for Duterte (more fatalities signals compliance with the drug war). They also found that in the 2019 elections, outsider mayors and members of the Liberal Party(LP), the former incumbent party, both tried to switch to PDP Laban. Among the outsiders and LP

members in the close election subsample who tried to switch, outsiders did better in the elections. We know that outsider mayors who won closely contested elections in 2016 observed higher fatality counts, and we also know that of the outsider mayors and LP mayors who switched to PDP Laban in 2019, outsider mayors had better vote outcomes - it is therefore possible that the success of outsider mayors is driving both fatalities and PDP Laban vote share outcomes (it is worth noting that I expect the degree of this bias to be small since there were only 189 close elections as defined by Ravanilla, Sexton, and Haim (2020)). It is also possible that there is endogeneity, with support for PDP Laban also affecting the count of fatalities. As noted earlier, Duterte needs compliance to successfully implement his drug war. It is possible that in places where there is latent support for Duterte's party, there are also more fatalities. One can imagine that constituents who want a pro-Duterte mayor are also less likely to protest about police killings, or are themselves willing to join vigilante groups. The latent popularity of Duterte is merely reflected in the 2019 elections, but is perhaps present before the actual elections and influences the count of fatalities. To correct for this, I propose an instrumental variable approach.

C) First stage:

In my first stage, the instruments I propose are the distance of each municipality to its closest seaport, and the distance of each municipality to its closest airport. I use a Stata package called *geodist* to construct this instrument, using 2 shape files: one with geocoded data on the airports from Geoportal Philippines, and the other with geocoded data on the seaports from the World Food Program.

$$\widehat{fatalities}_c = \gamma_0 + \gamma_1 Distance(seaport)_c + \gamma_2 Distance(airport)_c + \gamma_3 Poverty_c + \gamma_4 Population + \epsilon_c$$

D) Instrument validity

Instrument Relevance:

The distance to ports should increase the cost of drug related activity because of transportation costs. Reid, Devaney, and Baldwin (2006) find that in Southeast Asian drug

trafficking rings, heroin is produced in Myanmar, transferred to neighboring countries like Vietnam, and from Vietnam trafficked by sea and by air to the Philippines along with other destinations. Olario (1999) notes that the Philippines is an ideal location to serve three roles in the drug trafficking scene in Southeast Asia: a producer, exporter, and consumer of marijuana, an importer of methamphetamine hydrochloride (shabu), and a transit hub for heroin and cocaine. Olario (1999) also notes that the penetrable air and sea ports of the country, along with its long and irregular coastline make it suited for these functions. Given these facts, the cost mechanism predicts that towns more closely located to air and sea ports will have more intense drug activity. This should be true for both the supply side (e.g., if the municipality is far from a port, then one would expect it would be relatively more expensive to serve as a production location compared to a town closer to a port) and the demand side (being further away from ports will increase transportation costs, raising the price of drugs). This cost mechanism has been relied on in past literature. For example, while Blanco (2012) does not use an instrumental variable strategy, they check the effect of distance to the US border on trust in the Mexican government. They find a positive and significant relationship between the distance to the US border and the trust in the Mexican government, meaning that municipalities closer to the border had lower trust in the government than those further away. The most likely channel for this is intensity of drug activity: municipalities closer to the US border are more likely to have intense drug activity (and therefore more violence) than places further away because proximity reduces the cost of trafficking. Dell (2015) builds a network of predicted drug trafficking routes between the US and Mexico as a function of two main variables: the distance to the US border and the performance of the National Action Party in proximate municipalities in Mexico. The model they build relies on the assumption that the cost of transporting drugs is increasing in physical distance to the US border. This is the cost mechanism at play. And while the analysis done by Blanco (2012) and Dell (2015) is focused mostly on drug trafficking organizations, the same rationale should apply in my analysis: if there is less intense drug activity, the government and vigilante groups would have less incentive to act in those locations.

Table 3: First stage results

VARIABLES	(1) Fatalities
Distance to seaports	-0.0047 (0.0044)
Distance to airports	-0.0262*** (0.0084)
Poverty Incidence in 2015	-9.7452*** (1.1794)
Log of population	5.8063*** (1.3879)
Constant	-54.2393*** (14.0027)
Observations	1,615
F-test	5.055
Prob > F	0.00648

Robust standard errors in
parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The F-test is testing if the
distance to airports and distance to
seaports are jointly equal to 0

We can see from this table that the distance to airports has a negative and significant relationship with fatalities, indicating that the cost channel dominates here, while distance to seaports is not statistically significant. However, since my F-statistic is less than 10, I cannot rule out a weak instrument problem based on the heuristic normally used in instrumental variable papers. I also use the estat firststage command in Stata to do additional tests for a weak instrument.

Table 4: First-stage regression summary statistics

Variable	R-sq.	Adjusted R-sq.	Partial R-sq.	F-statistic (2,1610)	Prob > F
Fatalities	0.1711	0.169	0.0021	5.05514	0.0065

This table presents additional tests of a weak instrument using the estat firststage command in Stata

The results of these additional tests confirm that my instruments are jointly weak based on the low explanatory power of my model and the low partial R-squared values of my instruments. In order to address this problem, I use the

weakiv package in Stata to generate confidence intervals of the coefficients on my endogenous regressors that are robust to weak instruments. This is discussed in further detail in the results section.

Exclusion Restriction:

The exclusion restriction is that distance from air and sea ports affects PDP Laban's vote share only through its effect on fatalities. In other words, distance from air and sea ports cannot directly affect how a municipality votes. The distance of municipalities to sea and air ports is plausibly exogenous: the geography and topography of the Philippines that makes it suitable for sea and air ports is determined by nature. This means that the municipality's distance to the closest air or sea port is potentially as random as the location of the municipalities themselves. Since municipalities are located all over the Philippines, I am treating the distance of municipalities to ports the same way I would the distance of municipalities to strictly geographic features, such as the coast, which is certainly random. However, since airports and seaports are built by people, it is possible that airports and seaports are built with nearby municipalities in mind (e.g. maybe an airport is built closer to population centers to take advantage of economies of scale). To account for this, I once again include in my first stage controls for population and poverty incidence. This means that I only exploit geographic/topographic variation in the distance variables, and not variation that is correlated with other economic factors. This should circumvent the endogeneity concerns mentioned earlier, since this geographic/topographic variation is uncorrelated with the latent support levels for Duterte or the success of outsider mayors. Running this two-stage-least-squares specification is equivalent to taking the portion of fatalities that can be explained by exogenous variation in the first stage, and using these predicted values in my second stage. This results in an unbiased estimate of the coefficient on fatalities.

ANALYSIS AND RESULTS:

Hypothesis:

The Philippines offers an interesting context: On one hand, the Philippine drug war is essentially state sponsored

violence, largely against low profile civilians. The type of extra judicial killing ubiquitous in the drug war could lead to higher perceptions of insecurity by civilians and distrust in the government. On the other hand, the Philippines has a long history of inefficient courts. For example, it ranks 90th globally on the overall ranking and 99th globally in the Civil Justice ranking (both out of 126 countries) in the World Rule of law index. The inefficient justice system in the Philippines might also lead to constituents welcoming the drug war and the party championing it because of its perceived efficiency in dealing with crime.

Despite the high death count, support remains high for the drug war: according to the latest available Social Weather Stations data, the satisfaction rating of the drug war is at 82% and Duterte has also polled positively according to the most recent Pulse Asia surveys. Therefore, it would appear that in aggregate the perceived efficiency of the drug war is the dominant mechanism. The question I attempt to answer here is how that support changes in response to more intense measures of the fatalities from the drug war.

My hypothesis is that the relationship is monotonically positive: as the measure of fatalities intensifies, support for the PDP Laban party increases. It is possible that as one's exposure to the drug war intensifies, their support for the party implementing the drug war wanes. Even if more people than not support the drug war, if the majority of the people supporting the drug war don't have direct exposure to it, then they might have misperceptions about its human toll. One alternative to my posited hypothesis is that as people close the perception gap towards the drug war's human toll by observing more drug related fatalities in their municipality, they feel less safe and their support for the drug war diminishes. However, I believe that my posited hypothesis is more plausible. For example, as of February 2019, according to the Social Weather Stations, 66% of Filipinos believed that the number of drug users in their area had decreased. According to a 2020 article in *The Economist*, even people who had lost family members to the drug war still support it. It is worth investigating if the perceived efficiency of the drug war indeed dominates its perceived human toll as fatalities intensify.

H1: There is a positive and significant relationship between the measure of fatalities and the vote share for PDP Laban.

Results:

Table 5: Outcome Variable: Vote Share for PDP Laban

VARIABLES	(1) OLS	(2) Reduced Form	(3) 2SLS
Fatalities	-0.0002 (0.0002)		0.0239** (0.0114)
Poverty Incidence in 2015	0.0628 (0.0389)	0.0692* (0.0385)	0.3008** (0.1203)
Log of population	-0.0038 (0.0061)	-0.0066 (0.0057)	-0.1456** (0.0660)
Distance to seaports		-0.0000 (0.0002)	
Distance to airports		-0.0007*** (0.0003)	
Observations	1,615	1,615	1,615

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Column 1 shows that the OLS results are negative and not statistically significant. However, as mentioned earlier, there are several endogeneity concerns with this. Column 3 presents the instrumental variable results to account for this, and shows a positive and significant relationship between fatalities and the vote share for PDP Laban, which supports H1. In particular, increasing the count of fatalities by 1 results in about a 2.39 percentage point increase in the vote share for PDP Laban. This is evidence that the perceived efficiency of the drug war dominates the effect of the perceived human toll of the drug war. Finally, column 2 shows the reduced form results

and reveals a negative relationship between the vote share for PDP Laban and the distance to airports and an insignificant relationship between vote share and seaports. The distance to airports being negatively related to PDP Laban vote share in column 2 is consistent with the second stage: fatalities are positively correlated with PDP Laban vote share; fatalities are negatively correlated with distance to airports as demonstrated in the first stage results presented earlier, therefore distance to airports and vote share for PDP Laban must be negatively correlated.

Table 6: Effect of fatalities on multiple PDP vote outcomes using all the sea and air ports to construct the distance instrument

VARIABLES	(1) Vote share for PDP	(2) Vote share for PDP	(3) Binary elect PDP variable	(4) Binary elect PDP variable	(5) Vote share for PDP	(6) Vote share for PDP	(7) Binary elect PDP variable	(8) Binary elect PDP variable
Fatalities	0.0239** (0.0114)	-0.0012 (0.0248)	-0.0030 (0.0081)	-0.0360 (0.0389)				
Poverty Incidence in 2015	0.3008** (0.1203)	-0.0482 (0.1809)	0.0027 (0.0931)	-0.2329 (0.2756)	0.5748 (0.3612)	-0.0291 (0.1896)	0.1013 (0.2231)	0.3066 (0.2447)
Log of population	-0.1456** (0.0660)	0.0067 (0.1725)	0.0165 (0.0468)	0.2530 (0.2536)	-0.1463 (0.0988)	-0.0058 (0.0687)	-0.0202 (0.0602)	-0.1039 (0.0884)
Binary fatality variable					0.6629 (0.4616)	0.0155 (0.2728)	0.0899 (0.2838)	0.4268 (0.3521)
Constant	1.5436** (0.6351)	0.2260 (1.6827)	-0.1149 (0.4437)	-2.3934 (2.4762)	1.3075* (0.7743)	0.3421 (0.5721)	0.2015 (0.4697)	0.9295 (0.7356)
Observations	1,615	989	1,615	989	1,615	989	1,615	989
PDP Candidate ran		YES		YES		YES		YES
Binary fatality variable					YES	YES	YES	YES
Binary elect variable			YES	YES			YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Notes: This table uses all my control variables, and uses all the ports in my dataset to construct the distance instruments. PDP Candidate ran indicates if I am limiting my sample only to the municipalities where at least one candidate was from PDP Laban.

I also present the results using the same instrumental variable strategy on multiple outcome variables:

Column 1 is the same Two-stage least squares (2SLS) specification presented in column 3 of table 6. In column 2, I show what happens when I use the same continuous fatality variable and limit the sample to municipalities where at least one PDP Laban candidate ran. Limiting the sample in this way results in a loss of significance on fatalities. This could be due to the loss in variation, since restricting my sample to municipalities where a PDP Laban candidate ran reduces my observations by almost 50%. In Columns 3 and 4, I once again use the continuous fatality variable but this time I use a binary elect PDP variable. Since these results are not statistically significant, this tells me that the effect of observing one more fatality, while affecting the election results, does not change who ultimately won the overall election. In columns 5-8, I repeat the steps I did for the continuous fatality

variable but use a binary fatality variable instead, equal to one if the municipality observed at least one fatality. These results are never statistically significant, meaning going from a municipality that did not observe a fatality to a municipality that observed at least one fatality does not significantly affect PDP vote outcomes. All of this is to say that while we find evidence for HI in the first specification, that result does not hold up to alternative specifications. Therefore, the evidence for HI should be taken with a grain of salt.

Note that the observed differences in the significance levels of the effect of fatalities on PDP Laban vote share between the complete sample and the restricted sample could also arise if PDP Laban candidates are more likely to run in municipalities where there are more fatalities. This intuitively makes sense because the vote share variable for municipalities where PDP Laban candidates did NOT run is coded to 0 by choice. Therefore, if PDP Laban candidates are more likely to run in municipalities where there are more fatalities, this will also necessarily result in a significant effect of fatalities on PDP vote share in the unrestricted sample.

Table 7: Outcome variable: Did a PDP Laban candidate run

VARIABLES	(1)	(2)	(3)
	OLS	Reduced Form	2SLS
Fatalities	0.0009 (0.0007)		0.0890*** (0.0335)
Poverty Incidence in 2015	0.3308*** (0.0767)	0.3384*** (0.0768)	1.2014*** (0.3253)
Log of population	-0.0199 (0.0151)	-0.0207 (0.0145)	-0.5386*** (0.1942)
Distance to seaports		-0.0002 (0.0004)	
Distance to airports		-0.0025*** (0.0006)	
Observations	1,615	1,615	1,615

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

To check for this, I construct a dummy variable equal to one if a municipality observed a PDP Laban Candidate running in the 2019 elections and equal to 0 otherwise. I then regress this variable on the continuous fatality variable. In column 3 of table 7, the 2SLS coefficient on fatalities indicates that an increase of 1 in the count of fatalities leads to about an 8.9

percentage point increase in the probability that a municipality will observe a PDP candidate run. I believe that this result still shows a positive relationship between the number of fatalities and the popularity of PDP Laban candidates as even the decision to run reflects candidates' beliefs about their probability of success. The political economy literature shows that politicians respond to the environment they face. For example, Cavalcanti, Danielle, and Galletta (2018) show that politicians are more likely to field highly educated candidates when a high level corruption incident has been exposed, as supposed to less educated candidates when a low level corruption incident has been exposed. This is done in order to compensate for the negative popularity impacts of a high level corruption scandal, although Cavalcanti, Danielle, and Galletta (2018) find no significant change in the actual electoral performance of that party (i.e. the party with the corruption scandal is still punished in the election). If observing more fatalities in a municipality emboldens PDP Laban candidates to run, then that still reflects a positive relationship between observing fatalities and the perceived popularity of Duterte's party. Therefore, the "worst case" of the findings presented here is that observed fatalities only have a direct effect on a candidate's willingness to run as supposed to their actual popularity. If we assume candidates' perceptions of their own popularity are at least somewhat accurate, and candidate's mostly run when winning is likely, then this may still be taken as an indirect measure of the effect of fatalities on the popularity of PDP Laban. Note that I rule out reverse causality, namely that there are more fatalities when PDP Laban candidates are likely to run because of the instrument validity arguments presented in the previous section.

Explicitly addressing instrumentation concerns:

As noted in the Instrument Validity Section, distance to airports and seaports are jointly weak instruments. In order to account for this, I use the *weakiv* package in Stata to construct confidence intervals that are robust to weak instruments. The *weakiv* package uses multiple types of tests to construct confidence intervals that are robust to weak instruments. The test I will primarily focus on is the Conditional Likelihood Ratio (CLR) test. According to Finlay, Magnusson, and

Schaffer (2014), weakly constructs the CLR test for over-identified instruments. As the main specification utilizes one endogenous regressor (fatalities) and two instruments (distance to airport and distance to seaport), the instruments used here are indeed over-identified. We limit this additional testing to the two specifications where a significant result was found: namely when we regress PDP vote share on fatalities using the entire sample and when we regress the probability a PDP Laban candidate ran on fatalities (Table 6 Column 1 and Table 7 respectively).

Table 8: Weak instrument robust confidence Intervals

Model 1 Effect of Fatalities on PDP vote share, Complete Sample				Model 2 Effect of fatalities on the probability that a PDP candidate ran			
Test	Statistic	p-value	Conf. Set	Test	Statistic	p-value	Conf. Set
CLR	7.45	0.0092	[0.006277, ...]	CLR	18.33	0.0000	[0.045228, ...]
K	7.34	0.0067	[0.007181, ...]	K	17.77	0.0000	[0.045228, ...]
J	0.25	0.6183	entire grid	J	0.78	0.377	[-0.03975, ...]
AR	7.59	0.0225	[0.003566, ...]	AR	18.55	0.0001	[0.037262, ...]
Wald	4.39	0.0362	[0.001533, 0.046262]	Wald	7.05	0.0079	[0.02332, 0.154771]

All tests are performed at the 95% significance level

Focusing on the CLR tests, we see that both specifications hold up to weak instrument robust confidence intervals (and do so in other tests such as the Anderson-Rubin and Wald tests) as the confidence intervals exclude 0. This affirms that the significant results found earlier are valid even correcting for the weak instrument problem discussed earlier. While we cannot give an exact value of the coefficient on fatalities using the CLR test, we can provide a lower bound for the coefficient using these confidence intervals. Increasing the count of fatalities by 1 leads to at least a 0.6277 percentage point increase in the vote share for PDP Laban. Increasing the count of fatalities by 1 leads to at least a 4.5228 percentage point increase in the probability that a PDP Laban candidate ran. However, estimates are imprecise and we cannot rule out that the effects are much larger. It is worth noting that these lower bound coefficients are smaller than what was found with just the normal 2sls specifications.

Alternative specifications/ Robustness checks:

Table 9: The effect of the log of fatalities on PDP vote outcomes

VARIABLES	(1) Vote share for PDP	(2) Vote share for PDP	(3) Binary elect PDP variable	(4) Binary elect PDP variable
Log of fatalities	0.3018** (0.1391)	0.0043 (0.5636)	-0.0068 (0.1002)	0.2892 (0.7436)
Poverty incidence in 2015	0.4558** (0.1871)	-0.0356 (0.5693)	0.0233 (0.1393)	0.3024 (0.7512)
Log of population	-0.1611** (0.0712)	-0.0044 (0.3093)	0.0025 (0.0512)	-0.1581 (0.4086)
Constant	1.6070** (0.6404)	0.3327 (2.8485)	0.0202 (0.4586)	1.5337 (3.7638)
Observations	1,615	989	1,615	989
PDP Candidate ran		YES		YES
Binary elect variable			YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: This table uses the effect of the log of fatalities, instead of the count, on PDP Laban vote outcomes

I now present alternative specifications with the same instruments. First, I use the log of fatalities instead of the count. This makes sense since the count of fatalities, which ranges from 0 to 428, is quite large relative to the PDP vote share variable and the binary elect PDP variable which both go from 0 to 1. Additionally, there is a wide spread in the fatalities variable with some municipalities observing fatalities in the hundreds and others observing only a handful. It is therefore useful to think about the effect of percentage changes in fatalities rather than just a linear change in the count. The results here are consistent with the 2SLS specification presented in columns 1-4 of table 6. As with table 6, only the specification where I use the entire sample and the continuous vote share for PDP Laban yields a result significant at the 5% significance level. This result implies that a 10% increase in fatalities results in about a 3 percentage point increase in the vote share for PDP Laban. The other specifications lose significance. Nonetheless, I take this as moderate evidence for H1.

Next, I construct a fatalities per capita variable, equivalent to the count of fatalities divided by the total population in each municipality multiplied by 10000.

Table 10: Effect of fatalities per capita on PDP vote outcomes

VARIABLES	(1) Vote share for PDP	(2) Vote share for PDP	(3) Binary elect PDP variable	(4) Binary elect PDP variable
Fatalities per capita	0.5045* (0.2642)	-0.0035 (0.4000)	-0.0167 (0.1737)	0.0891 (0.4645)
Poverty incidence in 2015	0.6065** (0.2749)	-0.0399 (0.4038)	0.0160 (0.1903)	0.0986 (0.4680)
Constant	-0.1279 (0.1422)	0.2905 (0.2138)	0.0497 (0.0970)	0.0316 (0.2480)
Observations	1,615	989	1,615	989
PDP Candidate ran		YES		YES
Binary elect variable			YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: This table shows the effect fatalities per capita

It is scaled by a factor of 10,000, so for instance in column 1, for each 10,000 increase in fatalities per capita, the PDP vote share variable increases by about 50 percentage points. This rescaling is necessary because the ratio of fatalities per capita to the population is very small: the mean of fatalities in each municipality is about 3, whereas the mean for populations in municipalities is 60,570. If I did not rescale fatalities per capita, I would have coefficients in the thousands which does not make sense since the vote share for PDP is measured as a percentage. In this specification, column 1, which includes all the municipalities and uses a continuous measure of the PDP vote share, retains a coefficient on fatalities per capita significant at the 10% significance level. I take this result as marginal evidence for H1. However, I do not put too much stock into this result because the ratio of fatalities to population is very low and the rescaling required to make sense of the coefficient can only be interpreted with a count of fatalities well out of the scope of the observed fatalities (i.e. there is not one municipality with a count of fatalities at 1,000, let alone 10,000).

Finally, we use the same alternative measures of fatalities to test if the effect of fatalities on the probability that a PDP Laban candidate ran is robust to different specifications.

Table 11: Effect of alternative measures of fatalities on the probability that a PDP Laban candidate ran

VARIABLES	(1)	(2)	(3)	(4)
	PDP candidate ran	PDP candidate ran	PDP candidate ran	PDP candidate ran
Fatalities	0.0890*** (0.0335)			
Binary fatality variable		2.4582* (1.4905)		
Log of fatalities			1.1223*** (0.4159)	
Fatalities per capita				1.4707* (0.8214)
Poverty incidence in 2015	1.2014*** (0.3253)	2.2135* (1.1613)	1.7762*** (0.5442)	1.5404** (0.6758)
Log of population	-0.5386*** (0.1942)	-0.5386* (0.3192)	-0.5950*** (0.2136)	-0.2299* (0.1203)
Constant	5.6650*** (1.8854)	4.7658* (2.5046)	5.8914*** (1.9274)	2.2223** (0.8864)
Observations	1,615	1,615	1,615	1,615

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The effect of fatalities on the probability that a PDP Laban candidate ran is robust to different specifications. For example, it is still significant at the 1% significance level when we use the log of fatalities (Table 11, Column 1). Note that the interpretation of the coefficient does not make much sense since it would imply that a 100% increase in fatalities yields about a 112 percentage point increase in the probability of a PDP Laban candidate running. We may instead choose to read this as a 10% increase in fatalities yielding an 11 percentage point increase in the probability that a PDP Laban candidate ran. The effect of fatalities on the probability that a PDP Laban candidate ran when we use the fatalities per capita measure is only significant at the 10% significance level (Table 11, Column 4). Using the same reasoning from the discussion of Table 10, I do not put too much stock into this result as well.

Finally, we also consider the effect of a binary fatality variable on the probability that a PDP Laban candidate ran (Table 11, Column 2). This result is also only significant at the 10% significance level. One possible reason for this is purely mechanical – the variance of the fatality variable is about 23 times larger than the variance of the binary fatality variable and this reduced variation inherently increases the standard error of the coefficient. It is easy to imagine that municipalities that observe at least one fatality are highly heterogeneous. For example, a provincial town and a city in Metro Manila might both observe at least one fatality, but the provincial town

might have one local drug pusher while Metro Manila is one of the centers of the drug trade. A simple dummy variable that only checks if a fatality was experienced in a municipality will dilute this richer variation.

One note I would like to make is that these results lose significance when I use clustered standard errors. This is likely due to the loss in variation when I cluster. For example, if I cluster at the regional level, I will only have 17 clusters. If I cluster at the provincial level, I will have 84 clusters, but only 20 observations per cluster. This all means that using clustered standard errors will severely reduce the variation, likely driving the loss of significance. These results are covered in greater detail in the appendix.

CONCLUSION:

The results provide moderate evidence for a monotonically positive relationship between the intensity of the measure of fatalities and the vote share for PDP Laban. While the measure of fatalities is not systematically linked to whom ultimately won the election, the results still reveal that PDP Laban does better when there are more drug war fatalities. However, these results do not hold up to some alternative specifications. In particular, the results lose significance when I limit the sample to municipalities where there was a PDP Laban candidate who ran. In light of this, I also check if municipalities that observe more intense measures of fatalities are more likely to observe a PDP Laban candidate run. This yields a positive and statistically significant relationship between the observed count of fatalities and the probability a PDP Laban candidate ran which, unlike the relationship between fatalities and PDP Laban vote share, is robust to almost every specification presented. This is evidence that observing more fatalities emboldens PDP Laban candidates to run which is itself an important result.

There is therefore some evidence that popularity of the drug war is not just an aggregate outcome. That is, the people who support the drug war outnumbering those who do not may be an incomplete picture. It is also plausible that the party implementing the drug war receives more support in municipalities where the drug war is more vigorously enforced. At a minimum, there is strong evidence that politicians who

are members of PDP Laban are more likely to run when there are more drug related fatalities in their municipality.

Policy implications:

It is paramount that the Philippines resolves its inefficiencies in its courts and justice system. Otherwise, there is an incentive for politicians to use measures like the drug war, and other populist measures which undermine democracy and human rights, to improve their electoral outcomes. Philippine courts are glacial and corrupt. They need to be streamlined and cleaned up to generate buy in for the judicial system. Investigative procedures need to be improved to increase the likelihood that justice will be served for victims of crime.

The Philippines has a poor history of authoritarian leaders and violence against civilians. If it is indeed the case that the fatalities of the drug war result in a positive electoral performance for PDP Laban, this sends the wrong signal to prospective despots. Improving the justice system in the Philippines is paramount lest history repeat itself.

Are Women Who Out-earn and Out-work their Husbands Less Happy?: Evidence from Canada

Anton Buri, Ia Mantecón García, & Jessica Wu

ECON 490

ABSTRACT

This paper investigates the effect of relative spousal earnings and work hours in Canadian households on life satisfaction. To conduct our analysis, we use microdata collected through the 2011 Canadian General Social Survey, Cycle 25: Family. Our main hypothesis states that men who out-earn and out-work their wives experience higher levels of life satisfaction, whereas women who out-earn and out-work their husbands experience lower levels. Our findings support this hypothesis, showing that when households deviate from traditional gender roles, specifically when the wife out-earns and/or out-works her husband, both spouses report lower levels of life satisfaction. In light of existing literature around gender norms and labour market participation for women, we provide evidence that the disparity in this relationship may be attributed to the division of household tasks, which is likely driven by attitudes towards gender roles. The findings from this analysis may have implications for bargaining power within marriage for women, which could result in greater investment in human capital, childcare, and social welfare across society and in the public sphere.

INTRODUCTION

The male-breadwinner female-homemaker marriage model is based on the concept of comparative advantage, where marriage is a microcosm of markets and spouses follow task-specialization to maximize productivity. Men solidified their comparative advantage in paid labour during the Industrial Revolution, and women were resultantly assigned child-rearing and household chores. In recent decades, however, there has been an increase in the number of households that do not adopt the traditional division of labour. Extensive research shows that labour force participation among Canadian women has risen significantly (Moyser, 2019). Following this trend, many Canadian households have shifted away from the traditional division of labour model of a male breadwinner and female homemaker, and towards a model in which both spouses are significant contributors to household income.

As of 2007, approximately 85 percent of two-parent families in Canada consisted of two or more earners (Vanier Institute of the Family, 2010; p. 82). Similarly, the percentage of two-parent families with just a single earner fell by 22 percent from 1976 (p. 82). In addition, Canada has seen an increase in the number of women who act as primary breadwinners of their households, in many cases out-earning and out-working their male counterparts. It is estimated that 28 percent of Canadian women in dual-earner households out-earned their husbands in 2007, a jump from only 12 percent in 1976 (Vanier Institute of The Family, 2010; p. 102).

As a result of the shift away from the traditional division of labour in households in recent decades, a growing body of research seeks to determine the value of relative spousal income and hours worked as determinants of marital outcomes and life satisfaction. In this paper, we add to this discussion by analysing data from the 2011 Canadian General Social Survey (GSS) Cycle 25: Family, to explore the effect of relative income and relative work hours on life satisfaction of individuals who are married or in common-law relationships. We hypothesize that a higher relative income and ratio of hours worked for men will have a positive effect on life satisfaction, while a higher relative income and ratio of work hours for women will have negative effects. Our findings from this research support this

hypothesis, suggesting that for men, ratio of work hours and income share are positively related with life satisfaction, with a one unit increase in income share and ratio of work hours increasing life satisfaction by 0.226 and 0.124, respectively. For women, however, we find an inverse relationship, with life satisfaction decreasing by 0.178 as their income share increases by one unit, and 0.714 as their ratio of work hours increases by one unit.

Given extensive literature on the generally positive relationship between income and happiness, as well as a long history of men taking up the role of primary breadwinner in the household, the positive relationship between relative earnings and work hours with life satisfaction for men is perhaps not surprising. By contrast, an explanation as to why this is not the case for women is perhaps less so. Economic theory behind this disparity points to the persistence of traditional gender norms as the main explanatory factor. While recent decades have seen a rise in labour force participation for Canadian women, it is possible that patterns in gender norms related to the division of household tasks such as childcare and daily chores are not changing at a proportional rate (Milan, Keown, and Urquijo, 2011, (MKU)).

While research does show that the gap in the division of household tasks between married men and women has narrowed, it is apparent that Canadian women are still seen as primarily responsible for things such as childcare and household chores, regardless of their work status outside of the home (MKU, 2011). In this way, women who work outside of the home and take on the majority of household duties can be seen as working a 'double shift' (Hochschild, 1989). This increased workload may then result in higher levels of stress, and thus lower levels of life satisfaction. Evidence that men are not affected by their wives working longer hours than them supports this argument (Fleche, Lepinteur, and Powdthavee, 2018).

Alternative findings, however, suggest that men show lower levels of life satisfaction when they are not the primary earner in their household (Elmslie and Tebaldi, 2014). In this case, it is argued that men in a position of earning less than their wives may feel guilty or insecure about their financial position in the household, given that this contradicts long-

held gender expectations. These negative sentiments may then be reflected in the quality of marriage, and as a result, in individual perspectives on life satisfaction for both spouses. This argument is consistent with literature on greater relative earnings among wives and increased divorce rates, as well as provides an explanation for the relationship between both women's and men's relative earnings and work hours and life satisfaction in our own analysis.

The significance of our topic is clearly demonstrated by labour market trends for women in the previous two decades. Despite narrowing in the latter half of the 20th century, the change in the gender wage gap has slowed dramatically over the last 20 years (Fortin, 2005). In an attempt to explain this, Fortin (2005) points to the persistence of gender norms as a potential contributor. It suggests that as female participation in the labour market grows and a greater number of women become the primary breadwinners in their households, the effect of relative income within the household on individual happiness becomes increasingly significant. While wage equality and greater labour market opportunities for women continue to improve, growing significance is placed on understanding how long held attitudes towards gender roles change with it.

Furthermore, relative income between spouses has been found to be a strong predictor of bargaining power within a marriage (Iyigun and Walsh, 2007): as one spouse's income increases relative to his or her partner's, that spouse's ability to make decisions around the household (for example, what to spend household income on) should increase with it. However, the possibility that relative income has varying effects on life satisfaction for men and women engenders questions about how relative income might have an adverse impact on bargaining power for women, potentially putting them at a disadvantage. As we will discuss later in this paper, effects, such as these, have the potential to carry over to a greater societal context – it's possible that greater bargaining power for women may lead to greater public spending in areas such as social welfare and childcare, while also increasing investment in human capital and growth in labour supply (Fernandez, Fogli, and Olivetti, 2004). The remainder of this paper proceeds in the following manner: in Section II, we review existing literature around gender norms, relative spousal income and work hours and

their effects on both marriage outcomes and life satisfaction. Next, we give an overview of the data used in our analysis: the 2011 Canadian General Social Survey (GSS) Cycle 25: Family. In Section IV, we provide insight into the results of our analysis, as well as the empirical strategy used to obtain them. Finally, we conclude with a discussion on the meaning of our findings, future research opportunities accordingly brought to light, and potential policy implications.

LITERATURE REVIEW

A growing body of literature focuses on how the rise of dual-earner households in various countries affects marital outcomes and life satisfaction among spouses. Earlier contributions to the literature, albeit greatly contested, suggest that deviation from the traditional division of labour, (the male-breadwinner and female-homemaker model), is unfavourable for households (e.g., Becker (1991), Bergmann (1996)). More recent research presents similar findings, however, it points to a variety of different reasons.

One such study is found in Elmslie and Tebaldi (2014). This paper suggests that individuals in households that deviate from the traditional division of labour suffer more than individuals in households that do not, focusing specifically on households where there is more than one breadwinner. Using data from the U.S. General Social Survey (GSS) from 1991 to 2012, Elmslie and Tebaldi (2014) shows that for both men and women, a traditional division of labour is preferred. Specifically, it points to a strong negative relationship between husbands' overall satisfaction and the number of people earning income in his family. Similarly, it finds that women who do not work are 3.1 percent more likely to report being very happy in marriage than those who are in the labour market (p. 3461). The findings from Elmslie and Tebaldi (2014) suggest that relative income and relative working hours between spouses are strong predictors for both marital and life satisfaction. Furthermore, it indicates that the effects of these predictors may vary between genders. Though the authors suggest that gender ideologies might play a role in this, they do not give an in-depth discussion as to why this might be the case.

A more direct approach to exploring how determinants of marital and overall life satisfaction differ between spouses is taken in Bertrand, Pan and Kamenica (2013) (BPK). BPK (2013) explores how deviating from prescriptive gender norms is associated with lower levels of marital stability. Using data from the National Survey of Families and Households (NSFH) from 1988 to 2002, the study finds that in marriages where the wife out-earns her husband, both spouses are 15 percent less likely to report being very happy in their marriage, and 46 percent more likely to report having discussed separating in the previous year (p. 24-25). BPK (2013) also implicates attitudes around traditional gender norms to be a driving force behind these findings. While existing research shows that there is generally a positive relationship between individual income and life satisfaction (e.g., Easterlin, 2001), BPK (2013) suggests that for married women who out-earn their husbands this may not be the case, likely due to the persistence of traditional gender norms.

Further insight into this idea can be found in Wang, Yan, and Zhang (2019) (WYZ). Using data from the 2015 Chinese General Social Survey (CGSS), this paper tests the hypothesis that absolute income remains an important determinant of subjective well-being, but that this relationship may be weakened by social comparisons and self-expectations. Specifically, it observes the effects of social comparison and self-expectation on individuals' subjective well-being. WYZ (2019) finds that the effect of absolute income on subjective well-being for urban residents in China diminishes when controlling for factors based on relative income such as reference groups and self-expectations. These findings suggest that the relationship between individual income and happiness can be undermined by perspectives on relative earnings, particularly those involving one's spouse. While a wife's individual income increases, it is possible that her life satisfaction does not, as her income relative to that of her husband's is also increasing.

Further support for these findings is demonstrated in Van der Broeck and Maertens (2017), albeit under moderately different circumstances. Van der Broeck and Maertens (2017) provides an analysis using both quantitative and qualitative data obtained from a self-conducted survey that observes the effects of wage employment on self-reported happiness for

Senegalese women in the horticultural industry. The paper finds that within this population, wage employment for women improves subjective well-being only for those who are living below the poverty line. In contrast, women who live in households where total income is above the poverty threshold do not see the same effect. Van der Broeck and Maertens (2017) attributes this primarily to the income effect or possibly lack thereof. The paper suggests that wage employment for poorer women increases happiness because of increased income and therefore increased living standards, whereas this relationship is undermined for better-off women through more influential channels. Specifically, the paper identifies these effects as relating to increased workload and low job satisfaction due to unfulfilled expectations. While Van der Broeck and Maertens (2017) points to factors outside of the household that may explain why women are experiencing adverse effects from increased wage employment, its results demonstrate an inverse relationship between increased earnings and satisfaction for women, while suggesting that this may be related to an increased workload. A limitation of this study in terms of its contribution to the topic at hand, is a lack of comparison between the effects of wage employment on the happiness of Senegalese women to that of men.

Our paper is perhaps most closely related to Fleche, Lepinteur and Powdthavee (2018) (FLP). Building on a previous study by the same authors which finds that women's propensity to drop out of the labour force is higher among couples where the wife out-works her husband, this paper finds that women who work longer hours than their husbands report significantly lower levels of life satisfaction. By contrast, they find that the same relationship does not hold for men. The paper's main argument is that this difference is best explained by the perceived fairness of household division of labour, which is largely driven by attitudes around gender roles. One of the main hypotheses of the paper is that the negative effect from working longer hours for women is caused by dissatisfaction from their husbands not helping with household chores, rather than by feelings of guilt for violating traditional gender norms, as suggested by Becker (1991). To conduct its analysis, FLP (2018) uses longitudinal data from the 2015-16 U.S. Panel Study of Income Dynamics (PSID) Well-Being and Daily Life

supplement, FLP (2018). The findings support their hypothesis, suggesting that dissatisfaction decreases significantly when the indicator of the husband's contribution to household tasks increases. The paper suggests that the reason behind men not showing lower levels of life satisfaction when outworked by their female counterparts could be because they were not dissatisfied with the division of household chores.

Further support showing that women take on the brunt of housework can be found in Argyrous and Rahman (2014). Using data from the Australian Bureau of Statistics 2006 Time-Use Survey, Argyrous and Rahman (2014) provides evidence that mothers in Australia perform a larger share of childcare than fathers regardless of their relative earnings or hours worked. Specifically, they find that mothers' work hours have no effect on fathers' time spent on childcare, while a one hour increase in work hours for fathers is associated with an increase of 10.6 minutes in time spent on childcare for mothers (p. 392). They find a similar result when observing work schedules. In addition, like FLP (2018), the analysis finds that the use of childcare can mitigate these effects.

Similarly, Fortin (2005) shows that attitudes around gender norms remain a dominant driving force of labour market outcomes for women. Using data from the 1990, 1995 and 1999 World Value Survey (WVS), Fortin (2005) observes how gender role attitudes and work values affect labour market decisions for working women across 25 different OECD countries. To determine whether the slowdown of economic progress of women can be explained by a stabilization in attitudes toward gender roles among post World War II cohorts, Fortin (2005) shows that attitudes that support traditional gender roles such as 'when jobs are scarce, men should have more right to a job than women' and 'being a housewife is just as fulfilling as working for pay', are powerful explanatory factors for varying female employment rates and gender wage gaps across countries (p. 417). Specifically, the analysis finds that in countries where a greater proportion of those interviewed agreed with the previous statements, female employment was lower, and the gender wage gap was higher. These findings suggest that across various countries, gender norms regarding women and labour are a driving force behind labour market outcomes for women.

Furthermore, a substantial body of literature suggests

that gender norms present an additional challenge when capturing income-related effects. One such account is provided in Roth and Slotwinski (2019), which uses data from the largest Swiss labour market survey, Schweizerische Arbeitskräftererhebung (SAKE), in the years 2012 and 2015, to show that misreporting of income in surveys accounts for the largest portion of discontinuity in reported female income shares in Switzerland. Precisely, it merges the data from SAKE with individual-level administrative data to capture differences in surveyed and administrative earnings. The paper finds that there is a significant spike in surveyed income share just below the point at which women would out-earn their husbands, an overall discontinuity of about four percentage points (p. 9). Furthermore, the frequency estimate just above this threshold is just one fourth of that of the frequency estimate just below the threshold (where income share equals 50 percent) (p. 9). Given that the paper finds no other incentives for misreporting incomes on the survey side, the authors argue that gender norms are the primary driver for the discontinuity. Similar findings in terms of the distribution of reported relative incomes are demonstrated in BPK (2015) and Wieber and Holst (2015). Given that survey data has been the main basis of research on labour market outcomes on gender norms and relative earnings, Roth and Slotwinski (2019)'s findings give rise to questions around if existing research may be less informative of real labour market behaviours than originally thought¹.

Our research contributes to the existing literature by offering an analysis that uses calculated share of household income and ratio of work hours of the respondent to predict the effect of these two factors on life satisfaction. To do this, we use data on married and common-law partners from the 2011 Canadian General Social Survey (GSS), Cycle 25: Family. By using a calculated method for obtaining a measure of relative income and relative work hours rather than a direct response to survey questions, we hope to, to some extent, provide findings that are robust to the misreporting biases outlined in the previous paragraph. Furthermore, by using data from the

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

Canadian GSS, our contribution is among the first to explore the effect of relative spousal earnings and relative work hours on life satisfaction in Canadian households.

DATA

The data used in our analysis is from Statistics Canada's General Social Survey, Cycle 25: Family (GSS25), collected in 2011 and reported in 2013. This survey gathers data on social trends and other in-depth topics in order to monitor changes and developments in social policy issues, well-being, and living conditions of Canadians. The data was collected through computer-assisted telephone interviews (CATI) in all ten provinces in Canada, producing a total of 22,435 observations. Of these 22,435, we use approximately 4,300 of them for each of our models. In addition, to capture an accurate representation of Canadian society, we manipulate the data using frequency weights. As a result, the number of observations in our weighted dataset for each model increases to approximately 5.7 million.

As the primary focus of our analysis is on gender differences in the relationship between relative earnings and work hours and life satisfaction, we first selected observations where respondents report being in heterosexual marriages or common-law relationships. Next, we filter for observations that report their primary source of income to be from employment (including self-employment), such as wages, salary, and bonuses. In doing this, we exclude households whose primary source of income was from streams, such as investment or compensational income. Our logic is that in these cases, it is more likely that work hours and earnings from labour are strong predictors of life satisfaction. In addition, we reverse-code three variables: health, mental health, and education, in order to keep consistency in the direction of relationships across variables.

One of the challenges in analysing relative spousal income using the GSS25 is that the data for both personal income and household income is categorical, reported in 12 and 13 different income ranges, respectively. Further, the ranges are not equal in size. Therefore, to convert the categorical income data into continuous, numerical data in order to calculate each respondent's share of household income, we generate seven new

numerical variables. MININCOME represents the minimum possible individual income of the respondent, with its value coming from the lower bound of the GSS25 categorical income variable. Similarly, MAXINCOME represents the maximum possible individual income of the respondent, with its value coming from the upper bound of the GSS25 categorical income variable. Next, MINHOUSEHOLDINCOME and MAXHOUSEHOLDINCOME get their values from the lower and upper bounds of the GSS25 categorical household income variable, respectively. Finally, MININCOMESHARE and MAXINCOMESHARE are the minimum and maximum possible shares of total household income that the respondent can be contributing to total household income. They are found by dividing minimum income and maximum income by maximum household income and minimum household income, respectively, then multiplying by 100. As a result, we obtain INCOMESHARE, which reflects the mean value of the individual's minimum share of household income and maximum share of household income.

Given the historical prevalence of the male-breadwinner and female-homemaker model of division of household labour, we predict greater life satisfaction with greater values for INCOMESHARE for men, but the contrary for women. A visualization of our hypothesis can be seen in Figure 1. Our reasoning behind this is based on existing literature on the role of gender norms around household division of labour. Specifically, women's satisfaction may not increase with their share of household income due to the increased burden of working both outside and inside the home.

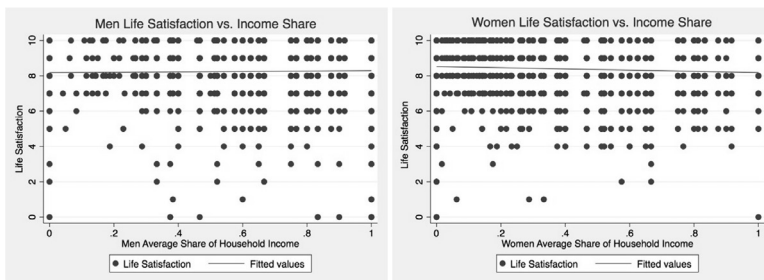


FIGURE 1: INCOME SHARE VS LIFE SATISFACTION

Our second model uses the ratio of work hours between spouses as the main explanatory variable². To observe that, we generate a new variable, RATIOWORKHOURS, where the respondent's work hours is divided by the sum of their own and their partner's work hours. To avoid both undefined and non-meaningful results, we exclude observations where neither respondent nor the spouse/partner of the respondent are working. Similar to the idea of income share and life satisfaction, Figure 2 shows our prediction for when ratio of work hours is the main explanatory variable. We expect that as the respondent's work hours increase relative to that of their spouse, life satisfaction will increase if the respondent is a man and decrease if she is a woman.

The dependent variable in both our models is self-rated life satisfaction, a categorical variable that reports the score of each respondent's overall life satisfaction on a scale of 0 to 10. Following the existing literature on life satisfaction and happiness, we include independent variables that are highly associated with the error term. Both of our regressions include control variables for age, health, mental health, household, number of children, and dummy variables indicating whether the respondent is religious (RELIGIOUS) and whether the respondent is a visible minority (MINORITY), where a '1' indicates if the respondent is religious/a minority and '0' if not. Based on the literature, we expect that religiousness is associated with higher levels of life satisfaction, whereas being a visible minority is associated with lower levels.



FIGURE 2: RATIO OF WORK HOURS VS LIFE SATISEACTION

² Our analysis includes a total of four models: Income share vs Life satisfaction for men, income share vs life satisfaction for women, ratio of work hours vs life satisfaction for men, and ratio of work hours vs life satisfaction for women. Our hypothesis for the first two models is shown in Figure 1, while our hypothesis for the second two is shown in Figure 2.

Following existing research, the respondent's number of working hours (WORKHOURS) is included in the income share regressions as we expect to see an overall negative correlation between work hours and life satisfaction for women, and a positive relationship for men. To further analyze how specific levels of education affect our dependent variable, we generate five education dummies (SOMESECONDARY, HIGHSCHOOL, SOMEUNIVERSITY, DIPLOMA, DEGREE) and exclude the post-secondary degree variable (DEGREE) to avoid multicollinearity. We expect coefficients for all four of the education dummies in our analysis to be negative, indicating that lower levels of education are associated with lower levels of life satisfaction. Furthermore, given our interest in gender norms and division of household labour, we generate four dummy variables for household chores (MEALS, DISHES, LAUNDRY, and GROCERIES) to observe who in the household is primarily responsible for these tasks. Each dummy takes on a value of '1' when the respondent is 'mostly responsible' for the task and '0' if otherwise. We expect these dummies to show negative coefficients. Table 1 reports the

TABLE 1
SUMMARY OF
STATISTICS

Variable	Obs.	Mean	Std. Dev.	Min	Max
LIFESATISFACTION	6,661	8.329	1.402	0	10
INCOMESHARE	5,987	0.534	0.274	0	1
RATIOWORKHOURS	6,251	0.54	0.256	0	1
AGE	6,691	44.919	11.563	18	79
SOMESECONDARY	6,688	0.103	0.304	0	1
HIGHSCHOOL	6,688	0.148	0.356	0	1
SOMEUNIVERSITY	6,688	0.126	0.332	0	1
DIPLOMA	6,688	0.344	0.475	0	1
HEALTH	6,696	3.741	0.944	1	5
MENTALHEALTH	6,696	5.971	0.884	1	7
HOUSEHOLDINCOME	5,832	10.524	1.884	2	13
CHILDREN	5,454	1.135	1.083	0	4
RELIGIOUS	6,703	0.794	0.404	0	1
MINORITY	6,703	0.115	0.319	0	1
WORKHOURS	6,444	36.734	15.427	0	75
MEALS	6,703	0.374	0.484	0	1
DISHES	6,703	0.299	0.458	0	1
LAUNDRY	6,703	0.389	0.488	0	1
GROCERIES	6,703	0.319	0.466	0	1

summary statistics for the variables.

There are a few potential concerns with the data used in our analysis that should be noted. First, we are limiting our data to households where both partners are under the age of 80. We are unable to numerically define the value for '80 years and over', and we therefore lose 1,383 of our observations. However, it is unlikely that including these observations would significantly change our results as we can assume that the majority of individuals above the age of 80 are out of the workforce. Second, there may be recall bias in our data as 288 observations, as approximately 6 percent of our data portray individual income as greater than household income. It is possible that the respondents inaccurately responded to some questions regarding their income and/or household income at the time of the survey. Hence, those observations are excluded to avoid complications when calculating the share of household income. Third, given the method we use to determine the share of household income for an individual, we are unable to include observations where the respondent has an income of \$100,000 or more and lives in a household where total household income is \$150,000 or more. In other words, our study is constrained by data availability. As a result, we drop and fail to represent 667 (or 8.8% of our total) observations that represent high-income respondents who live in high-income households in our analysis. Moreover, given the scenario that total household income is greater than \$150,000, we are unable to calculate the average share of household income as the respondents' maximum share of household income is equal to infinity. To accommodate this, we set the minimum share of household income to be 0, the value that it will be approaching. This alteration has a large impact on the accuracy of our measures, as it affects 8.1% of our sample (553 observations), and therefore, it makes its average value (INCOMESHARE) imprecise.

METHODOLOGY AND RESULTS

To identify the effects of average income share (INCOMESHARE) and ratio of work hours (RATIOWORKHOURS) on life satisfaction (LIFESATISFACTION) among spouses we use two OLS regressions with LIFESATISFACTION as the dependent

variable. In the first regression, INCOMESHARE is the main explanatory variable, whereas RATIOWORKHOURS is the main explanatory variable in the second. We run each regression twice - once using only observations where the respondent is male and again on observations where the respondent is

$$1.) \quad Y_i = \beta_0 + \beta_1 IncomeShare_i + \beta_2 Age_i + \beta_3 Age_i^2 + \sum \beta_4 X_{i,4} + \beta_5 Health_i + \beta_6 Health_i^2 + \beta_7 MentalHealth_i + \beta_8 MentalHealth_i^2 + \beta_9 HouseholdIncome_i + \beta_{10} NumberOfChildren_i + \beta_{11} Religious_i + \beta_{12} Minority_i + \beta_{13} WorkHours_i + \sum \beta_{14} X_{i,14} + \varepsilon_i$$

$$2.) \quad Y_i = \beta_0 + \beta_1 RatioWorkHours_i + \beta_2 Age_i + \beta_3 Age_i^2 + \sum \beta_4 X_{i,4} + \beta_5 Health_i + \beta_6 Health_i^2 + \beta_7 MentalHealth_i + \beta_8 MentalHealth_i^2 + \beta_9 HouseholdIncome_i + \beta_{10} NumberOfChildren_i + \beta_{11} Religious_i + \beta_{12} Minority_i + \sum \beta_{13} X_{i,13} + \varepsilon_i$$

female. The equations for our regressions can be seen below:

For regression 1) and 2), β_0 is the intercept, β_1 is the coefficient of interest, β_x 's, X_x is a set of education dummies, and ε_i is the error term. X_{14} and X_{13} are the set of household chore dummies for regressions 1) and 2), respectively. Consistent with our hypothesis, we find that the average share of household income and ratio of hours worked, and life satisfaction are positively related for men and negatively related for women. These findings suggest that as men contribute more to household income and work longer hours relative to their wife, their happiness increases. On the other hand, women who contribute more and work more, experience decreases in happiness. OLS coefficients for INCOMESHARE and RATIOWORKHOURS for both men and women are statistically significant at the 1% level.

We present the results for Regression 1) in Table 2. Column 1 presents the results for observations with male respondents, and Column 2 presents the results for observations with female respondents. The coefficient for INCOMESHARE in Column 1 suggests that a one unit increase in share of household income results in an increase of 0.226 in level of self-reported life satisfaction on the 0-10 scale for men. The coefficient in Column 2 suggests that an increase in one unit of share of household income results in a decrease of 0.178 in level of self-reported life satisfaction on the 0-10 scale for women.

Results for Regression 2) are also shown in Table 2. Column 3 presents the results for observations where the

respondent is a man, and Column 4 presents the results for observations with female respondents. The coefficient of 0.124 for RATIOWORKHOURS in Column 3 suggests that a one unit increase in ratio of work hours results in an increase of 0.124 in level of self-reported life satisfaction on the 0-10 scale for men. The coefficient in Column 4 suggests that an increase in one unit of share of total income results in a decrease of 0.174 in level of self-reported life satisfaction on the 0-10 scale for women.

The values for R^2 and adjusted R^2 from our regressions all fall into the range of 0.23 and 0.28; therefore, we can be confident that our model explains between 23% and 28% of the variation in life satisfaction.

The coefficients for the control variables are all consistent with the literature on life satisfaction (with the exception of the dummy variables for education, for which we get positive coefficients). We find that age is negatively related to life satisfaction, while age-squared is positively related, which suggests that there exists a U-shaped relationship between the two variables. That is, age and life satisfaction are negatively related up until a certain age, after which the relationship between the two becomes positive. Using the results from Regression 2), we find the turning point to be 45.6 years for men and 40.4 for women. Furthermore, as we expected, the coefficients for mentalhealth-squared are negative across all specifications, which suggests that for higher levels of mental health, a one-unit increase has a smaller effect on life satisfaction. The same relationship applies for health and life satisfaction, albeit only in the regressions with female respondents (Columns 2 and 4 of Table 2).

We argue that the negative relationship between life satisfaction and income share, and life satisfaction and ratio of work hours for women, can be explained in large part by the costs associated with deviating from traditional gender norms, specifically the male-breadwinner and female-homemaker

*Are Women Who Out-earn and Out-work their Husbands
Less Happy?: Evidence from Canada*

TABLE 2:
INCOME SHARE & RATIO OF WORK HOURS

Dependent variable:	Life Satisfaction			
	(1)		(2)	
	Men	Women	Men	Women
Income Share	0.226*** (0.00293)	-0.178*** (0.00338)		
Ratio of Work Hours			0.124*** (0.00290)	-0.714*** (0.00286)
Age	-0.0738*** (0.000415)	-0.0284*** (0.000472)	-0.0732*** (0.000421)	-0.0344*** (0.000472)
Age ²	0.000816*** (0.00000449)	0.000330*** (0.00000539)	0.000803*** (0.00000457)	0.000426*** (0.00000538)
Some Secondary Education	0.323*** (0.00232)	0.212*** (0.00300)	0.291*** (0.00231)	0.170*** (0.00295)
Highschool Diploma	0.145*** (0.00212)	0.207*** (0.00217)	0.153*** (0.00215)	0.197*** (0.00214)
Some University	0.156*** (0.00190)	-0.0413*** (0.00201)	0.149*** (0.00190)	-0.0574*** (0.00201)
Diploma/Certificate	0.0635*** (0.00152)	0.163*** (0.00152)	0.0727*** (0.00152)	0.130*** (0.00151)
Health	0.0979*** (0.00452)	0.321*** (0.00526)	0.111*** (0.00485)	0.278*** (0.00504)
Health ²	0.0112*** (0.000615)	-0.0181*** (0.000705)	0.0103*** (0.000656)	-0.0123*** (0.000679)
Mental Health	0.719*** (0.00879)	0.714*** (0.00927)	0.796*** (0.0105)	0.597*** (0.00827)
Household Income	0.109*** (0.000410)	0.0749*** (0.000383)	0.101*** (0.000404)	0.0732*** (0.000365)
Number of Children	-0.0106*** (0.000725)	-0.0236*** (0.000762)	-0.0159*** (0.000726)	-0.0371*** (0.000770)
Religious	0.0915*** (0.00151)	-0.00244 (0.00159)	0.0939*** (0.00151)	0.0153*** (0.00161)
Visible Minority	-0.187*** (0.00199)	-0.175*** (0.00235)	-0.187*** (0.00199)	-0.183*** (0.00236)
Work Hours	0.000156*** (0.00000572)	-0.00661*** (0.00000530)		
Meals	0.0267*** (0.00218)	-0.0331*** (0.00142)	0.0552*** (0.00217)	-0.0434*** (0.00142)
Dishes	-0.139*** (0.00186)	0.0126*** (0.00132)	-0.146*** (0.00186)	-0.00235* (0.00133)
Laundry	-0.199*** (0.00242)	0.0648*** (0.00145)	-0.191*** (0.00241)	0.0431*** (0.00147)
Groceries	-0.189*** (0.00177)	-0.0629*** (0.00135)	-0.190*** (0.00178)	-0.0601*** (0.00134)
Constant	4.452*** (0.0262)	3.971*** (0.0272)	4.361*** (0.0301)	4.540*** (0.0247)
N	3082950	2734178	3026349	2667990
R ²	0.232	0.267	0.233	0.280
Adjusted R ²	0.232	0.267	0.233	0.280

Robust Standard Errors in Parentheses
* p<0.1 ** p<0.05 *** p<0.01

model.

*Are Women Who Out-earn and Out-work their Husbands
Less Happy?: Evidence from Canada*

TABLE 3
RATIO OF HOUSEHOLD CHORES

Ratio Workhours >.5				Income Share >.5			
Women	Observations			Women	Observations		
	"Mostly you"	Total	Ratio		"Mostly you"	Total	Ratio
Dishes	1065	2497	42.65	Dishes	1279	2988	42.80
Meals	1598	2497	64.00	Meals	1834	2988	61.38
Laundry	1768	2497	70.80	Laundry	1981	2988	66.30
Groceries	977	2497	39.13	Groceries	1282	2988	42.90

Men	Observations			Men	Observations		
	"Mostly you"	Total	Ratio		"Mostly you"	Total	Ratio
Dishes	524	3935	13.32	Dishes	613	4463	13.74
Meals	334	3935	8.49	Meals	415	4463	9.30
Laundry	254	3935	6.45	Laundry	313	4463	7.01
Groceries	543	3935	13.80	Groceries	630	4463	14.12

We hypothesize that one of the channels through which this happens is the ‘double shift’, which refers to women who work outside the home but are still held responsible for taking on the majority of the work inside the home as well. This doubling of responsibilities may translate into higher levels of stress and feelings of discomfort or unfairness with regards to the division of labour within the marriage, which might then result in lower levels of life satisfaction. Further, it is possible that men in relationships where the woman works more and/or earns more feel insecure due to the reversal of traditional gender norms. This insecurity may affect the quality of marriage and consequently affect women’s life satisfaction. The positive effect of income-share ($\beta_1 = 0.226$) and ratio-workhours ($\beta_1 = 0.124$) on life satisfaction for men can be explained by the fact that men have traditionally been the sole breadwinners in the household, and thus experience no change in life satisfaction when they continue to be so. Given the traditional role of women as the homemaker, however, there are direct costs for women who out-earn and/or out-work their husbands.

Based on the data, we can find further support for the ‘double shift’ argument. Using observations where women earn more than half of household income or work greater hours than their spouses and the number of men who do the same, we computed the ratio of women and precedingly the same ratio of men who responded that they mainly take care of four household chores: Dishes, Meals, Laundry and Groceries. On average, we find that women report doing a greater share of these household chores even when they contribute more to household income. For example, women report being responsible for laundry in 66.38 percent of relationships in which they contribute more than half of household income, as opposed to 7.01 percent of men who do the same. Similarly, they are responsible for preparing daily meals in 61.38 percent of these relationships, as opposed to 9.30 percent for men. The percentages fall below 50 percent when looking at doing dishes and shopping for groceries: women are responsible for these chores in 42.80 percent and 42.90 percent of relationships in which they earn more than half of household income, respectively. However, these numbers are significantly higher compared to relationships where the man makes more than half of household income: in these relationships, men are responsible for doing dishes and shopping for groceries in only 13.74 percent and 14.12 percent of the cases, respectively.

We use the Breusch-Pagan test to assess for heteroscedasticity among all our model specifications. We get p-values close to zero for all our specifications, so we have to reject the null hypothesis that the variance of the residuals is homogenous. To address this, we use robust estimators of variance to ensure that our coefficients are robust to misspecifications. In order to test for multicollinearity among the independent variables, we apply a variance inflation factor (vif). Except for the variables for which we also include squared terms (AGE, HEALTH, and MENTALHEALTH), all vif values are well below the tolerance parameter of 0.1. We use the interquartile range (iqr) test to filter for outliers. The test reveals that for both specifications there are severe outliers at the lower ends sufficient to reject normality at the 5% level. To address this, we drop all outliers from our regressions. We are able to do this without concern that our results will be affected because of our large dataset. We compare the outliers

to investigate what the observations that are being dropped have in common but find no evidence of similarities between them.

Our study does an important job of shedding light on the ways in which traditional gender norms continue to shape and constrain women's happiness within heterosexual marriages. However, our study is constrained by data availability. Given that the variable for income from the GSS25 is categorical, our method for calculating personal income, household income, and thus the average share of income is fairly imprecise. In addition, the categories for personal income and household income have no upper bounds, therefore it was impossible to calculate the share of household income for those observations. Hence, individuals who had an income of over \$150,000 are not included in our analysis. Our results would likely be improved with research that accounts for high-income individuals that uses continuous data to represent income share.

CONCLUSION

This research outlined in this paper shows that deviating from the traditional division of labour within the household, as measured by relative income and ratio of work hours, results in lower life satisfaction for both men and women. We find that men, who have traditionally acted as the sole or primary breadwinner of the household, tend to be unhappier when their wife out-works or out-earns them. Similarly, women, who have long played the role of homemaker, are less happy when their income or number of hours worked relative to their husband's increases. The male-breadwinner female-homemaker marriage model became the norm during the Industrial Revolution, when men solidified a comparative advantage in paid labour and women were resultantly assigned child-rearing and household tasks. Recent decades, however, have shown an increase in the number of households that shy away from the traditional division of labour. Women now make up 47.7 percent of the Canadian labour force, an increase of 30 percent from 1976, and Canadian women are currently obtaining university degrees at higher rates than men (Catalyst Research, 2019). This reversal is reflected in the division of labour, as 85 percent of Canadian households are dual-earner

households (Vanier Institute of the Family, 2010; p. 82). For this reason, it is unlikely that the negative relationship for women between life satisfaction and income share and ratio of work hours can be explained by foregone productivity or a case of comparative 'disadvantage.' Rather, the findings of our analysis suggest that despite improving labour market outcomes for women in recent years, traditional gender norms about the division of household labour are 'sticky,' and women and men are still expected to adhere to their traditional gender roles, even when the underlying economic justification for doing so is no longer valid. Our research adds to the literature that shows that spouses that follow the division of labour along traditional lines are happier (e.g., Fleche, Lepinteur, and Powdthavee, 2018; Bertrand, Pan and Kamenica, 2013). This is not, however, a prescription, but an opportunity to analyse how, despite great improvements in women's labour market outcomes, the life satisfaction of men and women as it relates to marriage continue to be shaped by gender norms.

One of the most cited explanations for this phenomenon is that women are still expected to take on a greater proportion of household labour, regardless of their work status outside the home (Hochschild, 1989). A policy proposal that has been presented by Fleche, Lepinteur, and Powdthavee (2018) is to make subsidized child-care available to working mothers. The rationale behind this is that childcare is often delegated to mothers, regardless of their labour-market status. Hence, working mothers' stress from doing a 'double shift' might diminish if the work they have to do inside the home decreases. Investing to decrease the costs that accompany women who work more or earn more than their spouse might be a goal in itself, but there are also valuable social and economic implications that must be taken into account. For example, relative income between spouses has been found to be a strong predictor of bargaining power within a marriage, and although not always psychologically favourable, women who stay in the workforce are more likely to have a higher relative income share, and therefore are also more likely to obtain greater bargaining power within the household. It is possible that this would translate into greater societal spending on social welfare and childcare. Research shows that attitudes toward working mothers are passed on across generations, as wives who were

raised in households with a working mother are more likely to work themselves (Fernandez, Fogli, and Olivetti, 2004). Thus, policies that address women's constraints today would serve to socially train future generations' views on gender roles.

We know that when women earn more or work more hours than their spouses, they are punished for doing so, as their life satisfaction decreases. Following the literature, we have attributed this to two main causes: 1) women take on more household labour even as they earn more or work more and 2) husband's guilt and/or insecurity decreases the quality of the marriage and in turn the wife's life satisfaction. Fleche, Lepinteur, and Powdthavee (2018) explore these two causes and conclude that the unfair allocation of household tasks explains most of the negative relationship. However, there are research opportunities to quantitatively investigate to what extent each cause contributes to the decrease in life satisfaction caused by a one-unit increase in income share and/or ratio of work hours. Further, given that we know that income share and ratio of work hours affect life satisfaction via gender norms, it would be valuable for future research to investigate if and how the relationship we found varies in magnitude across areas that can independently affect the strength of gender norms, such as socio-economic class, ethnicity, age cohort, and religion.

The Effect of Trump's Buy American Hire American Order on H-1B and American Workers

JUSTINE C. ENGEL

ECON 490

ABSTRACT

Since the start of Donald Trump's presidential campaign, he has made it a priority to crack down on immigration. He targeted the H-1B specialty-skills work visa program that enables 65,000 high-skilled foreigners to work in the United States every year, exemplified by the administration's 'Buy American Hire American' executive order in 2017. The order demanded higher scrutiny of the H-1B program with the goal of helping American workers. Using data from the Current Population Survey and Labor Conditions Applications, I use a standard difference-in-difference regression model to evaluate the order's effectiveness in achieving this goal. I find that the order positively impacted the productivity of H-1B workers across the U.S. The impact of the order on Americans who compete with H-1B workers is not economically significant, suggesting the order was not successful in attaining its objective. A number of alternative specifications and sample restrictions demonstrate the robustness of these results. These findings suggest that Trump's order was not an effective labor market tool for helping Americans.

INTRODUCTION

Since its founding in the late 18th century, the United States of America has been one of the top destinations for immigrants from around the world. Immigration into the United States peaked during the Age of Mass Migration of the late 19th to early 20th century, when the foreign-born population made up about 15 percent of the total U.S. population (Gibson & Jung, 2006). With such high levels of immigration came restrictions, the first of such being the Chinese Exclusion Act of 1882 that prohibited immigration from China after the U.S. government showed concern about the inflow of cheap labor (Cohn, 2017). Later, the Immigration and Nationality Act of 1924 was passed, setting national quota limits on immigration based on economic concerns (H.R. 4122, 68th Cong. (1924)). These strict quota limits would not be relaxed until reforms were passed in 1964 that abandoned the national quota system (H.R. 2580, 89th Cong. (1965)). After this point, many new migration programs were established. One such program was the H-1B specialty occupation work visa, a non-immigrant visa first established in 1990 that enabled high-skilled foreign nationals sponsored by U.S. companies to work and reside in the U.S. for a period of 3 to 6 years (American Immigration Council, 2020).

The H-1B program has long been subject to political debate. Proponents laud the program for resulting in higher spending that increases America's GDP and overall tax revenue (Gogol, 2020). Opponents often argue that the visa encourages cheap foreign labor and is harmful to American workers and the American economy, stating that H-1B workers take jobs that would otherwise go to Americans (Preston, 2015). This debate around immigration and H-1B visas is also of demonstrated interest to the general public. In the 2016 U.S. presidential election, 70% of voters identified immigration as a topic that was "very important" to them (Pew Research Center, 2016). Despite being overshadowed by the coronavirus outbreak, immigration continued to be a "very important" issue to over half of voters in the 2020 presidential election (Pew Research Center, 2020). As such, politicians may try to address these labor market concerns through policy. One such example of this is the April 2017 Buy American Hire American [BAHA]

executive order by President Donald Trump. Although not a law passed by Congress, the order explicitly directs government agencies to:

“Hire American. In order to create higher wages and employment rates for workers in the United States, and to protect their economic interest, it shall be the policy of the executive branch to rigorously enforce and administer the laws governing entry into the United States of workers from abroad, including section 212(a)(5) of the Immigration and Nationality Act (8 U.S.C. 1182(a)(5))” (Executive Order No. 13788, 2017).

The order then mandates that all heads of government agencies review policies to ensure compliance with the order and assess and restrict the use of waivers during application processes, explicitly referencing the H-1B program. With regards to immigration, the order specifically orders government agencies to issue new policies and guidelines to “protect the interest of United States workers in the administration of our immigration system, including through the prevention of fraud or abuse” (Executive Order No. 13788, 2017). The United States Citizenship and Immigration Services [USCIS] responded to this by releasing guidance that directs officers to apply higher scrutiny to H-1B extension applications (USCIS, 2017), requires certain H-1B petitioners to provide more evidence with the applications (USCIS, Feb. 2018), and increases the info-sharing capacity between federal agencies to tighten fraud detection (USCIS, Apr. 2018). The Wage and Hour Division of the U.S. Department of Labor (2017) directly responded to the BAHA guidelines by issuing guidance stating they will enhance fraud detection and subject Labor Conditions Applications (a necessary part of the H-1B application process) to further scrutiny.

Another example of H-1B restrictions, closely related to and released at about the same time as Trump’s BAHA order, is the March 31, 2017 USCIS policy that changed what is considered to be a “specialty occupation” in terms of the H-1B visa. Since 2000, computer programmers had been deemed specialized under the visa program, but the March 2017 policy

rescinded this rule and immediately ordered that computer programmers be no longer automatically considered a specialty occupation (USCIS). To that end, computer programmer applicants must provide more proof that they are highly skilled. Together, the directives from the BAHA executive order, its resulting policy changes, and guidance from federal agencies aim to protect labor market outcomes for American workers by tightening requirements on and enhancing fraud detection for H-1B visa applications while keeping the H-1B cap at the same level since 2005.

In this paper, I evaluate the effectiveness of Trump's BAHA order in achieving its goal of addressing cheap foreign labor and helping American workers by investigating its effects on the productivity of H-1B and native workers. Specifically, I investigate whether or not the BAHA order and its derivatives caused a change in the productivity of H-1B workers, and whether or not the order and its derivatives affected the productivity of native workers who compete with H-1B workers.

Using data from the Current Population Survey (provided by IPUMS) and Labor Conditions Applications from the U.S. Department of Labor, I perform a difference-in-difference regression analysis to evaluate the impact of the BAHA order on the average yearly wage of H-1B workers and H-1B-affected native workers, who are defined as college-educated U.S. citizens over 25 working full time in industries where H-1B workers are more likely to be employed. Given that certain industries and states may have endogenous differences in the yearly wage rate, I add controls for these variables in my model. The control group in my analysis are non-college educated U.S. citizen workers over age 25 working full-time. This group was chosen as the control because they do not compete with H-1B workers for jobs (given that H-1B workers must have at least a bachelor's degree), and thus should not be affected by the BAHA order. A key assumption for the causality aspect of my model is the assumption of parallel trends in average yearly wages between the treatment group and the above control group.

My results provide important insight on the effectiveness of Trump's order. I find that the average yearly wage of H-1B workers increased by \$11,703 as a result of the order, which is

statistically and economically significant given the pre-BAHA average yearly wage of this group. Americans who compete with H-1B workers saw their average yearly wages rise by \$2,155. This is statistically, but not economically, significant given the pre-BAHA average yearly wage of these workers. These results are magnified when restricting my analysis to the top 5 states with the highest proportion of H-1B workers. When comparing the effects of the BAHA order between states with a relatively high and relatively low proportion of H-1B workers relative to citizens, I find that H-1B workers in high-H-1B states get a higher wage-boost than when looking at the whole country, while H-1B workers in low-H-1B states received a reduced or economically insignificant change to their wages. The results for H-1B-affected native workers using this specification is similar, with those in high and low-H-1B states getting a higher positive and negative, but still economically insignificant wage change after the order, respectively. This suggests that the proportion of H-1B workers relative to citizens in a state affects the effectiveness of the order. Industry-specific specifications and varying the post-BAHA order period of analysis do not result in economically significant changes to my original result. Overall, I conclude that Trump's BAHA order positively impacted the productivity of H-1B workers, while not having a significant impact on that of H-1B affected Americans.

My results contribute to policy debate on how to address perceived immigration-related challenges by evaluating the short-term labor market impact of a policy intended to help Americans. Further, my work adds to an existing literature on the efficacy of immigration restrictions, which is discussed in Section II.

The rest of my paper is organized as follows: Section II provides context on the H-1B program and a discussion of relevant literature, Section III discusses data sources and modifications, Section IV explains my methodology, Section V focuses on results from my primary specification, Section VI discusses alternative specifications and the robustness of my findings, and Section VII offers my conclusions.

BACKGROUND AND CONTEXT

A. The H-1B Visa Program

The H-1B specialty occupation work visa is a non-immigrant visa first established in 1990 that enables high-skilled foreign nationals sponsored by U.S. companies to work and reside in the U.S. for a period of 3 to 6 years, dependent on the job and the applicant's nationality (American Immigration Council, 2020). High-skilled, in this case, is defined as having a bachelor's degree or higher. Each year, a cap is placed on the number of visas that can be issued. In 1990 at the program's beginning, the cap was set at 65,000. This number increased to a high of 195,000 available visas from 2001 and has been set at 65,000 since 2004. An additional 20,000 visas on top of this cap are allotted for applicants with graduate or professional degrees (American Immigration Council, 2020). Applying for a visa is a multistep process. Before a petition is evaluated by USCIS, the applicant must first file a Labor conditions Application [LCA] with the U.S. Department of Labor [DOL] (FLAG, n.d.). The purpose of this application is to evaluate if the proposed H-1B position meets program requirements, specifically that the pay for the petitioner is at or above the prevailing market wage of the same position in the U.S. Once an applicant receives approval from the DOL, the petition then goes on to USCIS for approval. Once the final petition is approved by USCIS, the 65,000 available visas and 20,000 additional graduate-level visas will be distributed by lottery.

B. Existing Literature

There is much conversation within the academic community surrounding the effects of immigration on the American economy. The consensus on this topic has changed over time. The prevailing opinion on this topic initially came from David Card (2001) who argues that inflows of immigrants have a significant and negative effect on the wages and employment of native workers in the United States, with this effect magnified in regions with higher immigration rates. Borjas (2003) similarly argues that immigration has a large and negative effect on native wages. However, recent arguments on this topic have been changing. De Brauw and

Russell (2014) revisit Borjas's methods to argue that the wage effects of immigration are not negative, and that welfare gains from immigration are overall positive. Ottoviano and Peri (2012) estimate the labor substitutability between native and immigration workers of similar education and skill level in the U.S. to argue that immigration in fact has a positive effect on native wages.

There is further research that focuses particularly on the impact of high-skilled immigration on native outcomes. Peri et al. (2014) evaluate H-1B data from 1990-2010 to find that the increase in STEM (Science, Technology, Engineering, and Mathematics) workers due to the H-1B program had a significantly positive effect on the wages of similarly high-skilled native workers while also increasing total productivity growth across several U.S. cities. Bound et al. (2018) analyze the increase in H-1B immigration from 1994-2001 to conclude that high-skilled immigration had a positive effect on overall native wages and contributed to increased production in high-skilled industries. However, contrary to Peri et al. (2014), Bound et al. (2017) argue that although overall wages increased, wages of similarly high-skilled native workers would have been higher without the inflow of high-skilled foreign labor. Further adding to the debate over the effects of immigration on productivity, Peri (2012) uses Census data from 1960-2006 to argue that increases in immigration do not crowd out native workers and are significantly correlated with growth in total factor productivity among U.S. states. Other research has studied the impact of high-skilled immigration on other measures of productivity, like innovation. For example, Hunt and Gauthier-Loiselle (2010) use panel data from 1940-2000 to argue that a one percentage point increase in immigration results in a 9-18% increase in patents per capita.

Discourse also exists on the impact of immigration restrictions within the U.S. from a historical perspective. Abramitzky et al. (2019) analyze the impacts of the national quota immigration restrictions of the 1920s on American to find that native earnings decreased as a result. Clemens et al. (2018) evaluate the effect of ending the Mexican *Bracero*¹ program in the 1960s to conclude

¹ *The Bracero program (1924-1964) allowed Mexican workers to come work seasonally in the U.S., predominantly in the agricultural sector. The program was ended in 1964 over concerns of adverse economic impact on Americans (Clemens et al., 2018)*

that the program did not hurt native outcomes in the first place, and that eliminating the program failed to improve them. Mayda et al. (2018) investigate the impacts of the decrease in the H-1B visa cap from 195,000 to 65,000, finding that there was no impact on employment levels of native workers, while employment of H-1B workers fell.

My work adds to this dialogue on the relation between high-skilled immigration and native labor market outcomes by analyzing the impact of federal-imposed orders and policy changes on the productivity of high-skilled workers. Further, much of the existing discourse uses data from over ten years ago in making conclusions. My analysis is unique in that it uses recent labor market data to analyze the short-term effects of presidential policy aimed at protecting the domestic labor market. Although President Trump's immigration policies often cause controversy, little research has been done evaluating and quantifying their effectiveness. My research will be among the first to do so and will add to the existing literature on economic analyses of immigration restrictions.

DATA

Data for my analysis was drawn from two sources: the Current Population Survey [CPS] via the Integrated Public Use Microdata Series [IPUMS] for measuring outcomes for native workers and Labor Conditions Applications [LCA] data via the U.S. Department of Labor [DOL] for measuring outcomes of H-1B workers. Data was first extracted and cleaned individually before being appended into a single dataset.

A. Current Population Survey (IPUMS)

The CPS as obtained from IPUMS is a large collection of microdata based on surveys of individuals. Survey responses are present from 2011 to 2020, where one survey response is one observation in the dataset. In the CPS, survey respondents are asked a wide variety of questions about demographics, work, income, education, residence, and family.

In cleaning up CPS data, a few modifications were made. Part-time workers were first excluded from analysis to eliminate downward wage skews and make the CPS dataset comparable

to the data for H-1B workers. Similarly, respondents below age 25 were excluded to further match up with H-1B data. Although there is no age marker for the LCA data, given that H-1B applicants must possess at least a bachelor's degree, 25 is assumed to be a reasonable minimum age for applicants. Of highest importance was standardizing the wage variables so that an accurate analysis could be conducted. To create a variable for yearly wage, the respondent's weekly earning was multiplied by 52. Hourly wage data was available for some respondents, but such data was relatively sparse and thus weekly earnings were favored in calculating yearly wage. The industry variable was recoded into North American Industry Classification System [NAICS] 2-digit industry codes to ensure uniformity with the dataset for H-1B workers.

B. Labor Conditions Applications (U.S. DOL)

Data on Labor Conditions Applications [LCAs] is available from the U.S. DOL website, by fiscal year [FY]. I chose data from FY2012 to FY2020 in order to have an ample number of observations before and after the 2017 BAHA order. In the LCA dataset, one observation is one application. The majority of applications have one worker per application, but a few have more, meaning n workers for n identical positions. In applications with multiple workers, wage is the same across all workers. As two separate data sources are used to conduct my analysis, data weighting is not possible. However, as the majority of observations pertain to one person, this does not cause a significant issue.

Wage variables were standardized at the yearly level to match CPS data. The base wage variable was used in calculation of yearly wage instead of the high-end wage variable because few observations in the dataset include a value for high-end wage, and, assuming that companies hiring workers are profit-maximizing and cost-minimizing, companies will not hire workers above the minimum wage necessary. Additionally, some data entry errors appeared to be present regarding wages. For example, one application listed the weekly wage of a civil engineer as \$60,000, which comes out to over \$3,000,000 annually. This is unreasonable, given that less than 1% of Americans make over \$500,000 a year, based on

Internal Revenue Service [IRS] data.² Since it is impossible to know whether this was an error in entering too many zeros or in typing the wage unit, I eliminated observations with wages over \$500,000. Further, observations with legitimate wages over this amount would still be data outliers based on the U.S. wage distribution.

It must be noted that LCAs are only the first step in the process for obtaining an H-1B visa, and thus an approved LCA does not necessarily mean an approved visa. However, I assume that a rational, profit-maximizing and cost-minimizing firm looking to hire foreign workers will take note of the provisions outlined in the BAHA order and subsequently want to avoid burdensome legal costs associated with higher H-1B rejection rates and increased Requests For Evidence³. Following this, I can assume that wages offered on LCAs will adjust to BAHA provisions and reflect wage trends in USCIS-approved H-1B applications. Thus, using LCA data to evaluate the wages of H-1B workers is reasonable and accurate. Further, LCA data was chosen over USCIS data for a few reasons. Firstly, LCA data is available at the application level, while USCIS data is not. Micro-level data allows for a more robust analysis. Secondly, USCIS data lacks important variables like the location of H-1B jobs, a standardized measure for evaluation of wages, and industry codes that are necessary for comparing the data with the CPS dataset. It would thus be difficult to come to any conclusion regarding impacts of the BAHA order on native workers using USCIS data.

C. Appended Dataset

The appended dataset is large, with over 5,000,000 observations after all modifications. In general, variables that were not present in both datasets and that hold little analytical value were dropped. Yearly wages at the extreme

² 2017 IRS data puts the 1% income cutoff at \$515,317: <http://www.bloomberg.com/news/articles/2019-10-16/americans-now-need-at-least-500-000-a-year-to-enter-the-top-1>

³ A Request for Evidence [RFE] is a letter sent to an applicant by USCIS when more evidence is required for a decision. More RFEs indicate a higher degree of scrutiny towards applications.

ends of the spectrum (over \$500,000 and below \$10,000⁴) were excluded to adjust for possible data collection errors. Workers in part-time jobs and under age 25 were excluded from analysis as to avoid downward wage skewing and because it is assumed H-1B workers are at least 25 years old, given the education requirements for obtaining a visa. Dummy variables for immigration status and college education were created. It is important to note that weighting was not used due to data constraints. This indicates that there may be slight bias in calculations. However, due to the large size of the dataset, I do not expect this effect to be significant. Table 1 includes summary statistics for select variables of analytical importance.

METHODOLOGY

I use a difference-in-difference regression model to evaluate the causal effect of the BAHA order on H-1B and native workers. I use the marginal product of labor, the wage rate, as a measure of productivity.

A. Primary Specification

To evaluate the effect of the BAHA order on productivity, I construct a model wherein wage (y) is the dependent variable and date in the form of month and year (mm/yy) is the primary explanatory variable. I choose date as the primary explanatory variable because it represents the periods before and after the BAHA order. Several control variables are also necessary to absorb the effects of other factors that would directly affect wage. Ideally, these controls would include age and sex, but as this information is not available in the LCA dataset, this control cannot be included. I include controls for industry and state of residence to account for the fact that some industries or states may have higher or lower wages than others. My primary specification for analyzing the impact of the order on H-1B workers is:

$$(1) \quad y_{it} = \beta_0 + \beta_1(\delta_{it}) + d_{it}^{immstat} + d_{it}^{college} + X'_{it} + \varepsilon_{it}$$

⁴ A \$10,000 salary is below the federal poverty line, as defined by the U.S. Department of Health and Human Services: <https://aspe.hhs.gov/poverty-guidelines>

*The Effect of Trump's Buy American Hire American Order
on H-1B and American Workers*

Table 1: Means of Key Variables

	H-1B		Citizens	
	(1)	(2)	(3)	(4)
1. Yearly Wage				
<i>College Educated</i>	85,632	(31,594)	73,724	(37,898)
<i>No College</i>			45,426	(25,986)
<i>All</i>	85,632	(31,594)	57,229	(34,458)
Minimum/Maximum across all groups	12,480	/ 500,000	10,010	/ 150,000
2. College Educated	1	--	0.417	(0.493)
3. Industry				
<i>Agriculture</i>	0.001	(0.027)	0.009	(0.093)
<i>Mining, Oil, Gas</i>	0.003	(0.053)	0.011	(0.103)
<i>Utilities</i>	0.002	(0.046)	0.014	(0.116)
<i>Construction</i>	0.005	(0.068)	0.057	(0.232)
<i>Manufacturing</i>	0.074	(0.068)	0.117	(0.113)
<i>Wholesale Trade</i>	0.011	(0.011)	0.029	(0.167)
<i>Retail Trade</i>	0.025	(0.025)	0.092	(0.087)
<i>Transport & Warehousing</i>	0.004	(0.004)	0.048	(0.214)
<i>Information</i>	0.051	(0.220)	0.023	(0.149)
<i>Finance & Insurance</i>	0.050	(0.218)	0.062	(0.241)
<i>Real Estate</i>	0.003	(0.052)	0.018	(0.131)
<i>Professional, Scientific, & Tech</i>	0.663	(0.473)	0.075	(0.263)
<i>Management of Companies</i>	0.005	(0.068)	0.000	0.000
<i>Administrative, Support, Waste Management, & Remediation</i>	0.010	(0.101)	0.033	(0.177)
<i>Education</i>	0.051	(0.220)	0.111	(0.314)
<i>Healthcare & Social Assistance</i>	0.035	(0.183)	0.138	(0.345)
<i>Arts, Entertainment, Recreation</i>	0.001	(0.037)	0.015	(0.121)
<i>Accommodation & Food Services</i>	0.003	(0.052)	0.038	(0.191)
<i>Other Services</i>	0.003	(0.054)	0.036	(0.187)
<i>Public Administration</i>	0.001	(0.082)	0.075	(0.263)
4. State				
<i>California</i>	0.188	(0.391)	0.105	(0.307)
<i>Texas</i>	0.102	(0.302)	0.082	(0.274)
<i>New York</i>	0.083	(0.277)	0.061	(0.239)
<i>Florida</i>	0.033	(0.180)	0.061	(0.239)
<i>Illinois</i>	0.053	(0.224)	0.041	(0.199)
<i>New Jersey</i>	0.069	(0.253)	0.028	(0.166)
<i>All Other States</i>	0.472		0.622	
Total Observations	4,275,988		1,178,656	

Note: Columns (1) and (3) contain means of key variables for H-1B and citizen workers, respectively. Columns (2) and (4) indicate standard errors. Sample includes adults over age 25 working full-time. H-1B workers are required to have at least a college degree. Data after March 2020 is excluded to eliminate possible skewing from the COVID-19 pandemic.

where i is one observation, δ represents if the observation is before or after the BAHHA order, $immstat$ is a dummy for immigration status (citizen or non-citizen), $college$ is a dummy for college or non-college educated, X' represents controls, and ε is a residual that encompasses all other effects. The primary specification for analyzing the impact of the order on H-1B affected Americans is almost identical to that for H-1B

workers, with an added dummy for industry, as the treatment group in this specification is limited to college-educated Americans in the Professional, Scientific, and Technological Services industry:

$$(2) \quad y_{it} = \beta_0 + \beta_1(\delta_{it}) + d_{it}^{immstat} + d_{it}^{college} + d_{it}^{H1Bind} + X'_{it} + \varepsilon_{it}$$

These specifications are implemented using a difference-in-difference model as described below.

B. Difference-in-Difference Model

To evaluate the causal impact of the BAHA order, I use a difference-in-difference model in two specifications. A difference-in-difference model compares the difference in changes of an outcome variable between a treatment and control group over two time periods. The difference in changes of the outcome variable, or the DD parameter, measures the causal impact of the treatment effect being tested. The use of double differences makes this model an ideal choice for my analysis, which measures the impact of the BAHA order on certain groups before and after implementation. This model is also ideal in setting my work within the existing literature on the labor market effects of immigration policy, as research in this area often uses a difference-in-difference based approach as the causal method (Abramitzky et al., 2019; Clemens et al., 2018; Mayda et al., 2018).

In the first specification, I analyze the causal impact of the BAHA order on productivity of H-1B workers. The control group are citizen non-college educated workers. This group is an ideal control because their wage trends should mirror the trends of the U.S. economy in general, and, seeing as they are not college-educated, do not in general compete for the same high-skilled jobs as H-1B workers and are thus assumed to be unaffected by the order. The treatment group is H-1B workers. The treatment effect is the imposition of the BAHA order, where $T=0$ is January 2011-April 2017 and $T=1$ is May 2017-February 2020. Data after February 2020 was excluded to eliminate COVID-19 pandemic-related wage skews. The difference-in-difference parameter $[DD_r]$ is as follows:

$$(3) \quad DD_1 = [\text{change in wage of H-1B group over time}] - [\text{change in wage of non college educated citizens over same time}]$$

This parameter is calculated by interacting (*yearly wage*)_{*i*} x (*date*)_{*i*} for each observation in both groups to see the within-group effect, and then comparing this effect between the treatment and control group. If this parameter is positive, then the BAHA order had a positive impact on the productivity of H-1B workers. A negative parameter value indicates a negative impact of the order, while a value of zero means the order has no effect on the productivity of H-1B workers.

The difference-in-difference setup for the second stage of analysis is similar. The treatment effect and time periods are the same. The control group remains as non-college-educated citizens due to the aforementioned reasons. To estimate the effect of the BAHA on Americans, I select my treatment group as college-educated Americans working in the Professional, Scientific, and Technological Services industry. As about two-thirds of the H-1B sample is employed in this industry, and H-1B workers must have at least a bachelor's degree, the treatment group of Americans will compete with H-1B workers for jobs. Thus, measuring the impact of the BAHA order on this group provides a strong indication of the impact of the order on the group it is intended to help. The difference-in-difference parameter for this stage [DD_2] is as follows:

$$(4) \quad DD_2 = [\text{change in wage of H-1B affected Americans over time}] - [\text{change in wage of non college educated citizens over same time}]$$

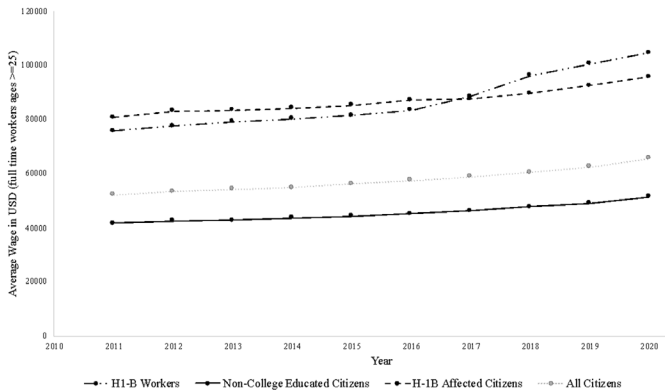
The DD_2 parameter for the second stage is calculated in the same way as in the first stage, except for a different treatment group. Similarly, positive and negative parameter values indicate positive and negative impacts of the BAHA order on productivity of H-1B affected Americans (college-educated citizen in the Professional, Scientific, and Technical industry), respectively. A zero value indicates no impact.

A necessary assumption for the causality aspect of my model is that of parallel trends. That is, I need to assume that if not for the BAHA order, the trend in average yearly wages of both the treatment and control groups would have remained the same over time. This is a reasonable assumption to make, as rational, profit-maximizing, and cost-minimizing firms would have little incentive to raise wages of H-1B workers unless required. Due to the counterfactual nature of this assumption, testing it empirically is impossible.

C. Parallel Trend Assumption

A necessary assumption for the causal aspect of my model to hold is that of parallel trends in average yearly wage between treatment and control groups. This is because in a difference-in-difference model, the causal aspect comes from comparing the effect of the treatment between a treated and untreated group. Since it is impossible for one group to both receive and not receive the treatment effect (which is, in this case, Trump's BAHA order), it is instead necessary to use a control group that follows the pre-treatment trends of the treated group as much as possible. In the context of my model, this means I must assume that if not for Trump's order, my control group of non-college educated citizens and treatment group either H-1B workers or H-1B affected citizen workers would follow the same trend in average yearly wage, the outcome variable of interest.

FIGURE 1: PARALLEL TREND ASSUMPTION- AVERAGE YEARLY WAGES BY GROUP



I evaluate the reasonability of this assumption visually to compare average yearly wage trends for all groups before and after the order. Figure 1 illustrates the average yearly wage by group from 2011 to 2020. The control group of non-college educated citizens is represented by the solid black line, while the two treatment groups are shown with dashed lines. Looking at the figure, wage trends up until 2017 are relatively parallel, with divergence only occurring after this point. This provides strong support for the causality of my model, meaning that my conclusions regarding the causal effect of Trump's order hold.

RESULTS

A. H-1B Worker Productivity

Table 2 presents the results of my primary analysis of H-1B workers. The primary coefficients of interest are found in the first row and show the causal effect of Trump's order on productivity of H-1B workers, measured by wage. The coefficient in column 1 gives the estimate without controlling for the impacts of industry and state of residence. Measured in this way, the BAHA order increased the average yearly wage by \$13,524, which is statistically and economically significant, given that the average yearly wage of this group before the order was \$79,777. This suggests that the order had a significant and positive impact on the productivity of H-1B workers. Columns 2, 3 and 4 provide estimates while controlling for the effects of the workers' industry, state of residence, and both, respectively. The causal effect of the BAHA on H-1B workers decreases to \$11,834 when adding in industry controls. This indicates that there is some variation in the effect of the order by industry, as some industries will have an inherently higher or lower average wage than others. The estimate is also lowered when controlling only for state of residence, indicating that state plays some role in determining wage. When adding in all controls, the effect of the BAHA order on H-1B workers is \$11,703, indicating that the average worker saw an increase in yearly wage by this amount. This corresponds to a significant and positive increase in productivity among H-1B workers due to the order. Although this amount is about \$1,800 lower than the estimate without controls, the overall sign and magnitude of the effect do not differ significantly, indicating that the positive impact of the BAHA order on H-1B holds. In this way, the order could be considered a success if the goal is to address the issue of cheap foreign labor. Coefficients for immigration status are also present, which indicate the effect of being an H-1B worker on yearly wage as opposed to the non-college educated citizen control group. These coefficients are rather large, which is to be expected given the large skill disparity between the two groups. The coefficient for the post-BAHA period indicates the effect of a later time period (May 2017 to February 2020) on average yearly wage.

Table 2: Effect of BAHA on H-1B Workers

VARIABLES	(1)	(2)	(3)	(4)
	Change in Yearly Wage			
1. H-1B workers after <i>BAHA</i>	13,524** (92.9)	11,834** (89.8)	12,907** (90.3)	11,703** (87.8)
2. Immigration Status	37,332** (53.5)	44,922** (61.3)	34,560** (55.5)	41,985** (62.5)
3. After <i>BAHA</i> order	5,020** (86.8)	5,330** (83.9)	5,003** (84.3)	5,376** (82.1)
Constant	43,809** (49.2)	30,626** (308)	40,792** (208)	27,321** (359.00)
Industry Controls	No	Yes	No	Yes
State Controls	No	No	Yes	Yes
Observations	4,962,990	4,938,521	4,962,990	4,938,521
R-squared	0.204	0.271	0.252	0.306

Note: After BAHA Order includes data from May 2017-February 2020. Estimation in column (1) does not include controls, columns (2)-(4) do. Explanatory variable 1 measures the estimated US dollar-impact of the BAHA order on H-1B worker productivity. Standard errors in parentheses.

** p<0.01, * p<0.05, + p<0.1

These results have an intuitive explanation, when evaluated in the context of employer decisions. My analysis assumes that firms hiring H-1B workers are rational decision-makers who are profit-maximizing and cost-minimizing, and thus will not arbitrarily raise wages without a clear incentive. Although Trump's order does not specifically require employers to increase H-1B wages, it states that H-1B applications will be placed under higher scrutiny and indicates supposed low-wage foreign labor as a motivating factor. Knowing this, it is no surprise that firms have an incentive to increase the wages of H-1B workers in order to avoid burdensome legal costs and possible loss of investment that would arise if their application were denied or subject to lengthier processing times because of the BAHA order.

B. H-1B Affected American Worker Productivity

Table 3 presents the results of my primary analysis of H-1B affected American workers, the intended targets of the order. The causal effect of the BAHA order on this group is shown in the first row. Without controlling for industry and state of residence, the coefficient on this variable is 2,202, meaning that the average yearly wage of H-1B affected Americans increased by \$2,202 due to Trump's order. Adding controls for industry and state do not significantly change the result, with the causal effect at \$2,155 when adding all controls.

*The Effect of Trump's Buy American Hire American Order
on H-1B and American Workers*

Table 3: Effect of *BAHA* on American College Educated Workers in H-1B Industries

VARIABLES	(1)	(2)	(3)	(4)
	Change in Yearly Wage			
1. College-educated H-1B industry Americans after <i>BAHA</i>	2,202** (241)	2,266** (234.0)	2,118** (240.0)	2,155** (233.0)
2. College-educated workers in H-1B industries	40,621** (146)	31,188** (213.0)	39,306** (147.0)	30,231** (213.0)
3. After <i>BAHA</i> order	5,021** (70.2)	4,956** (69.1)	5,272** (70.1)	5,232** (70.0)
Constant	43,810** (39.8)	38,824** (299.0)	40,450** (235.0)	35,654** (375.0)
Industry Controls	No	Yes	No	Yes
State Controls	No	No	Yes	Yes
Observations	746,723	731,878	746,723	738,878
R-squared	0.153	0.205	0.164	0.215

Note: After *BAHA* Order includes data from May 2017-February 2020. Estimation in column (1) does not include controls, columns (2)-(4) do. Explanatory variable 1 measures the estimated US dollar-impact of the *BAHA* order on productivity of college-educated American workers in H-1B industries. Robust standard errors in parentheses

** p<0.01, * p<0.05, + p<0.1

While this increase in wage is statistically significant at the 1% level, it is not economically significant when considering the pre-*BAHA* average wage of this group. College-educated Americans in H-1B industries had an average wage of \$84,431 before the order, and the \$2,155 causal impact only translates to about a 2.5% increase in wage, a rate only slightly above that of inflation. This means that Trump's order did not achieve its goal in creating higher wages for Americans, because domestic worker productivity did not change.

Of particular interest is the difference in variance between the primary causal coefficient and the coefficient representing the effect of being a college-educated citizen in an H-1B industry. The coefficient representing the effect of being a college-educated citizen in an H-1B industry (compared to the control group) decreases by over \$10,000 after adjusting for industry and state of residence effects, indicating that the marginal effect of the order may vary between industries and states. However, the primary causal coefficient remains almost the same. The relative consistency of this effect lends further support to my conclusion.

These results can also be interpreted within the context of a firm's decision. For example, when discussing the effects of the order on H-1B workers, I assume that firms are rational decision-makers who are profit-maximizing and cost-minimizing, and thus will not arbitrarily raise wages without a clear incentive. However, unlike with the H-1B worker case,

there is not a clear incentive for firms hiring H-1B workers to raise wages for their non-H-1B workers. Although the goal of the order is to raise native wages, the mechanism through which the order seeks to achieve this is by scrutinizing H-1B applications. Further, H-1B workers do not have a negative impact on the wages or employment of native workers, but oftentimes have a positive effect (Kerr & Lincoln, 2010; Mayda et al., 2018; Ottaviano & Peri, 2012; Peri, 2012; Peri et al., 2014). This suggests that legislative efforts that target the H-1B program as undesirable, like the BAHA order, may not be an effective way to improve economic outcomes for native workers. In this vein, the null effect of the BAHA order on productivity of H-1B affected Americans is unsurprising.

DISCUSSION

A. Influence of the H-1B/Citizen Sample Ratio

My primary specification amalgamates observations across the U.S. in estimating the BAHA order's effects. However, some states have a disproportionately higher or lower amount of the H-1B sample than others. To test the effects of this, I re-run my model under several alternative specifications.

1. Analysis of Top 5 H-1B States

As wages, jobs, and opportunity may differ between states, H-1B workers may be drawn to certain states over others. As such, the impact of Trump's order may differ if a given state has a higher relative number of H-1B workers. To test this, I re-run my model under an alternative specification wherein I restrict the sample to observations from the top 5 "H-1B states". An H-1B state is one where the ratio of the proportion of the H-1B sample to the citizen sample in a given state is greater than 1. The states chosen for this analysis are California, with an H-1B/Citizen ratio of 1.79, New Jersey (2.46), Texas (1.24), New York (1.36), and Illinois (1.29).

The results of this analysis are shown in the double-differences section of Table 4. Columns 1 and 2 show the BAHA impact on H-1B workers, and columns 3 and 4 show the impact on H-1B affected Americans. When including industry controls, the BAHA order increased the average yearly wage of

*The Effect of Trump's Buy American Hire American Order
on H-1B and American Workers*

Table 4: Effect of BAHA in Selected H-1B States

VARIABLES	(1)	(2)	(3)	(4)
<i>DOUBLE-DIFFERENCE</i>	H-1B Workers	H-1B Workers	H-1B Affected Citizens	H-1B Affected Citizens
	Change in Average Yearly Wage			
1. H-1B Workers after <i>BAHA</i>	16,579** (202)	14,986** (196)	--	--
2. College-educated H-1B industry Americans after <i>BAHA</i>	--	--	2,867** (500)	2,890** (488)
3. Immigration Status	38,581** (115)	40,750** (125)	--	--
4. College-educated workers in H-1B industries	--	--	42,655** (307)	32,628** (461)
5. After <i>BAHA</i> Order	5,024** (196)	5,465** (190)	5,024** (167)	5,061** (165)
Constant	45,451** (111)	32,858** (716)	45,451** (94.1)	37,065** (834.0)
Industry Controls	No	Yes	No	Yes
Observations	2,257,622	2,249,761	156,557	153,137
R-squared	0.161	0.231	0.18	0.226
<i>TRIPLE-DIFFERENCE</i>				
6. H-1B workers in top 5 states after <i>BAHA</i>	20,492** (179)	18,152** (173)	--	--
7. College-educated H-1B industry Americans in top 5 states after <i>BAHA</i>	--	--	5,783** (407)	5,751** (396)
Constant	43,386** (54.8)	30,615** (306)	43,386** (44.6)	38,502** (299)
Industry Controls	No	Yes	No	Yes
Observations	4,962,990	4,938,521	746,723	731,878
R-squared	0.216	0.282	0.155	0.206

Notes: After period starts from May 2017. Sampled H-1B states are California, Texas, New Jersey, New York, and Illinois. H-1B states are the top 5 states where the H-1B -- Citizen ratio is over 1. The H-1B -- Citizen ratio is the ratio of the proportion of the H-1B sample in state *i* to the proportion of the citizen sample in state *i*. Control group sample is restricted to adults over 25 working full time. Columns (1) and (2) show the impact on H-1B workers; (3) and (4) show impact on H-1B afflied citizens. Odd numbered columns do not include controls, even numbered ones do. The "double-differences" section compares outcomes across immigration status and time period, while the "triple-differences" section compares outcomes across immigration status, time period, and high vs. non-high H-1B states. Standard errors in parentheses.

** p<0.01, * p<0.05, + p<0.1

H-1B workers in the aforementioned states by \$14,986. This, like my primary analysis, is a statistically and economically significant positive impact on productivity. However, this effect is about \$3,000 higher than the coefficient for the entire country. This indicates that the effect of Trump's order on H-1B workers was stronger in the states with a higher relative number of H-1B workers. A similar story holds for the impact on H-1B affected citizens, who saw an increase in average yearly wage of \$2,980. This amount, while an increase from the primary analysis coefficient, is still relatively economically insignificant when considering the average wage of this group before the order. However, this minor increase indicates that immigration restrictions may have a heavier impact in states with relatively more immigrants. This conclusion is consistent with research

on prior U.S. immigration restrictions (Abramitzky et al., 2019; Clemens et al., 2018; Mayda et al., 2018; Peri et al., 2014). Overall, these findings confirm the conclusions of my primary results.

2. National vs. State-level Analyses

A possible concern with my original difference-in-differences approach is that it relies on variation at the national level. However, entirely national-level data may not entirely capture the phenomenon of decreasing wages of non-college educated workers compared to their college educated counterparts. To address this, I use a triple differences specification that introduces the impact of states more or less affected by Trump's order. The top five H-1B states identified above are added to the treatment group. The triple difference parameters [DDD] are described below for H-1B and H-1B affected workers, respectively.

$$(5) \quad DDD_a = [\text{change in wage of } H-1B \text{ workers in high } H1B \text{ states over time}] - [\text{change in wage of non college educated citizens in non high } H1B \text{ states over same time}]$$

$$(6) \quad DDD_b = [\text{change in wage of } H1B \text{ affected Americans in high } H1B \text{ states over time}] - [\text{change in wage of non college educated citizens in non high } H1B \text{ states over same time}]$$

The results of this analysis are shown in the “triple-differences” section of Table 4. With industry controls, the BAHA order increased wages for H-1B workers in high H-1B states by \$18,152. This is statistically and economically significant. However, this effect is about \$6,000 stronger than in my original specification and about \$3,000 stronger than in my top H-1B state-restricted double-difference estimate. For H-1B affected Americans in high H-1Bs states, the BAHA order increased wages by \$5,751 when adjusting for industry. This is statistically significant but has only minor economic significance. This change amounts to a 6.8% increase to the pre-BAHA wages of this group, which is consistently higher than inflation. This suggests that H-1B affected Americans in high H-1B states experience a moderate productivity boost relative to their counterparts in other states. However, given that the magnitude of this increase is significantly less than that for H-1B workers and is only present in high H-1B states, my overall conclusion that Trump's order did not significantly impact productivity of the target group holds. These disparities emphasize the importance of state-

level variation and indicate that state may play an important role in determining that actual effects of the BAHA order. For instance, there may be a slight upward bias in the national-level estimates for the effects of the BAHA order that are driven by greater productivity boosts in high H-1B states.

Differences in the H-1B/Citizen Sample Ratio.

To further test the impact of the relative H-1B population, I compare specifications of the top 2 states with the highest and lowest H-1B/Citizen sample ratio. These states are New Jersey (2.46), California (1.79), Florida (0.54) and Indiana (0.46). A visual representation of this ratio is offered in Figure 2. It should be noted that the sample size for these specifications is considerably smaller than that for my primary analysis, and thus these results may not be as robust.

Results for this analysis are displayed in Table 5. Regarding H-1B workers, there is considerable variation between the causal coefficient between high and low ratio states. For example, H-1B workers in California see an increase in average yearly wage of \$18,899 after controlling for industry. This is statistically and economically significant and indicates that the productivity of H-1B workers in California increased as a result of the BAHA order. In contrast, H-1B workers in Indiana saw a wage increase of just \$2,686 after controlling for industry, which is statistically but not economically significant. However, in Florida, with a similar ratio as Indiana, the BAHA increased H-1B average yearly wages by \$7,712 after industry controls, which is statistically and economically significant. This indicates that the H-1B/Citizen sample ratio may have a considerable impact on the effect of the BAHA order on H-1B workers, but other factors are likely at play. Further research is needed on why Indiana appears to be an outlier in this aspect. For instance, it may be possible that some states, like Indiana, have GDPs and economic growth rates that fall far behind other states. Data on the economic conditions in different states would provide insight into the possible cause behind this disparity. Overall, it is likely that the causal effect of the order on H-1B workers is slightly upwardly biased. Although this bias may exist, I do not expect it to change my overall conclusion on the positive and significant effect of BAHA on H-1B productivity given the strength of my aggregated result.

The Effect of Trump's Buy American Hire American Order on H-1B and American Workers

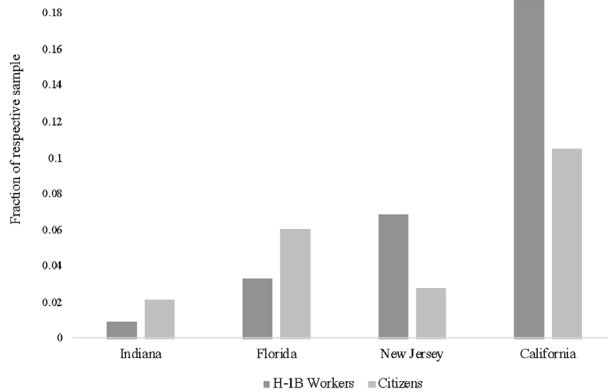


FIGURE 2: COMPARISON OF THE H-1B AND CITIZEN SAMPLE BETWEEN SELECT STATES

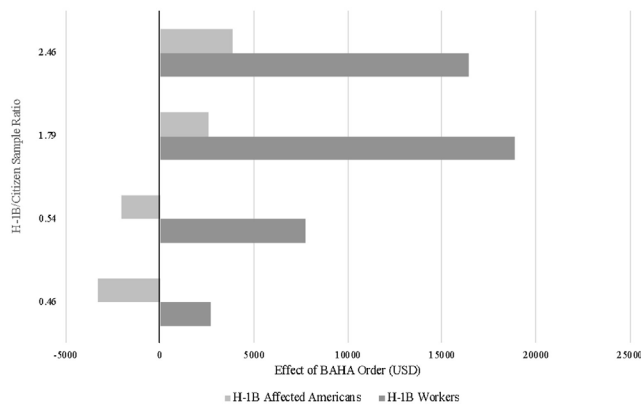


FIGURE 3: EFFECT OF THE BAHA ORDER ON H-1B AND H-1B AFFECTED AMERICAN WORKERS, BY H-1B/CITIZEN SAMPLE RATIO

Note: States from top to bottom are New Jersey, California, Florida, and Indiana.

Unlike with H-1B workers, the effect of the order on H-1B affected Americans does not vary in a way that is economically significant between high and low H-1B states. The causal coefficient when restricting the sample to California is 2,612 when controlling for industry, meaning that H-1B affected Americans saw an increase in average yearly wage by this amount due to the order. However, like my primary result, this increase is not economically significant when considering the pre-BAHA salary of this group. This trend of null significance holds for New Jersey and Florida as well, which experience changes in average yearly wage of \$3,885 and -\$2,086, respectively. The Florida result is significant at the 10% level and draws particular interest as the impact appears to be

negative. However, when evaluated using a consistent standard of economic significance, the overall conclusion regarding the impact of the order on H-1B affected American productivity remains the same. The causal effect of the order in Indiana is not statistically significant, further bolstering this consensus. Figure 3 provides a visual comparison of the effects of the BAHA on each group of workers by state.

B. Industry Tests

It should be noted that about two-thirds of the H-1B sample is employed in the Professional, Scientific, and Technological [PST] sector. While my primary analysis controls for the fixed effects of industry, it is necessary to address the concern that the positive effects of the BAHA on H-1B worker productivity are isolated to workers in this industry, which I label as an H-1B dominant industry. To test for this, I re-run the first stage of my model under two different specifications: one where I look at H-1B workers employed only in the PST industry, and one where I focus on non-PST H-1B workers. My control group is unchanged across these specifications so as to ensure comparability with my primary analysis.

Table 6: Effect of BAHA using H-1B-Dominant Industry

VARIABLES	Change in Average Yearly Wage			
	(1)	(2)	(3)	(4)
	H-1B: Dominant Industry		H-1B: Non-Dominant Industry	
1. H-1B worker in dominant industry after BAHA	12,041** (71.8)	11,892** (39.3)	--	--
2. H-1B worker in non-dominant industry after BAHA	--	--	13,908** (127)	12,942** (125)
3. H-1B dominant industry	32,898** (41.4)	29,774** (43.4)	--	--
4. H-1B non-dominant industry	--	--	46,604** (75.4)	43,603** (79.8)
5. After BAHA Order	5,021** (64.9)	5,154** (62.6)	5,021** (107)	5,121** (105)
Constant	43,810** (36.8)	39,657** (183)	43,810** (60.5)	40,053** (289)
State Controls	No	Yes	No	Yes
Observations	3,513,593	3,513,593	2,136,399	2,136,399
R-squared	0.308	0.359	0.283	0.311

Note: After BAHA Order includes data from May 2017-February 2020. Dominant-industry refers to NAICS code 54- professional, scientific, and technical services. In columns (1)-(2), treatment sample is restricted to H-1B workers in industry 54. Treatment group in columns (3)-(4) is restricted to H-1B workers not in industry 54. Odd numbered columns do not include controls for state of residence, even numbered columns do. Standard errors in parentheses.

** p<0.01, * p<0.05, + p<0.1

Table 6 shows the results of this analysis. I find that the impact of the BAHA order on H-1B workers does not vary significantly between dominant and non-dominant industries. For H-1B workers in the PST industry, average yearly wage increased by \$11,892 due to the order. This result is almost identical to the causal coefficient in my primary analysis, which may initially indicate that the BAHA order's effect on H-1B productivity is driven strongly by workers in this industry. However, H-1B workers in non-dominant (non-PST) industries saw average yearly wages increase by \$12,942 after controlling for state of residence, which is not a significant departure from the result for dominant-industry workers. This means that the positive and significant effect of Trump's order on H-1B worker productivity holds across industries, and the high proportion of H-1B workers in the PST industry does not significantly bias my results.

C. Different Definitions of the "After BAHA Order" Period

In my primary analysis, I specify the post-treatment period as immediately after the order (May 2017) up until the most recently available data before the COVID-19 pandemic (February 2020). This allows for a more robust sample. However, it is possible that the effects of the BAHA order vary depending on how this period is defined. For example, the order may have had an initially stronger effect in the first year post-BAHA that was obscured when analyzing the effect from a 3-year perspective, due to firms being intimidated by the sudden regulations. As such, to test for these possible effects, I re-run my original model under alternative specifications wherein the post-BAHA period is defined as 1 year (May 2017-May 2018), 2 years (May 2017-May 2019), and 3 years (the original time frame).

Table 7 shows the results of this analysis with regards to H-1B workers. I find that while Trump's order has a positive and economically significant effect on H-1B productivity across all three specifications, this effect increases slightly in magnitude as the post-BAHA period is extenuated. By May 2018, H-1B worker average yearly wages increased by \$8,497 after adjusting for controls. This effect increased to \$10,107 by May 2019 and \$11,703 by February 2020. This indicates that

*The Effect of Trump's Buy American Hire American Order
on H-1B and American Workers*

while there was a strong impact to H-1B worker productivity one year after the order, this effect may increase as time goes on. This illustrates the short-term nature of my analysis. Some firms may be slower to adjust wages, which could lead to a delayed appearance of the BAHA effect. Further research should return to this question of the productivity effects of Trump's order after more time has passed to compare the long-term impact with the short-term one.

Table 7: Effect of BAHA on H-1B Workers by Year

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	1 Year		2 Years		3 Years	
	Change in Average Yearly Wage					
1. H-1B workers 1 year after BAHA	9,419** (135)	8,497** (128)	--	--	--	--
2. H-1B workers 2 years after BAHA	--	--	11,117** (106)	10,107** (99.8)	--	--
3. H-1B workers 3 years after BAHA	--	--	--	--	13,254** (92.9)	11,703** (87.8)
4. Immigration Status	37,333** (51.2)	42,550** (63.5)	37,333** (52.0)	42,180** (62.5)	37,333** (53.5)	41,985** (62.5)
5. 1 year after BAHA order	3,245** (126)	3,468** (120)	--	--	--	--
6. 2 years after BAHA order	--	--	3,943** (98.9)	4,157** (93.6)	--	--
7. 3 years after BAHA order	--	--	--	--	5,021** (86.8)	5,376** (82.1)
Constant	43,810** (47.1)	26,722** (392)	43,810** (47.9)	27,206** (373)	43,810** (49.2)	27,321** (359)
Controls	No	Yes	No	Yes	No	Yes
Observations	3,699,632	3,688,218	4,335,464	4,322,932	4,962,990	4,938,521
R-squared	0.170	0.268	0.186	0.287	0.204	0.306

Note: Standard errors in parentheses. Even numbered columns include controls for industry and state of residence, odd numbered columns do not. After BAHA periods begin from May 2017. Observations after February 2020 are excluded from sample due to potential COVID-19 effects.
** p<0.01, * p<0.05, + p<0.1

Table 8: Effect of BAHA on H-1B Affected Citizens by Year

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	1 Year		2 Years		3 Years	
	Change in Average Yearly Wage					
1. H-1B affected citizens 1 year after BAHA	552 (365)	380 (352)	--	--	--	--
2. H-1B affected citizens 2 years after BAHA	--	--	1,606** (281)	1,474** (272)	--	--
3. H-1B affected citizens 3 years after BAHA	--	--	--	--	2,201** (241)	2,155** (233)
4. College-educated worker in H-1B industry	40,621** (143)	30,633** (224)	40,621** (144)	30,496** (218)	40,621** (146)	30,231** (213)
5. 1 year after BAHA order	3,245** (105)	3,537** (102)	--	--	--	--
6. 2 years after BAHA order	--	--	3,943** (81.3)	4,249** (79.5)	--	--
7. 3 years after BAHA order	--	--	--	--	5,021** (70.2)	5,232** (69.0)
Constant	43,810** (39.0)	35,983** (423)	43,810** (39.3)	35,816** (397)	43,810** (39.8)	35,654** (374)
Controls	No	Yes	No	Yes	No	Yes
Observations	585,617	575,133	659,731	648,135	746,723	731,878
R-squared	0.143	0.205	0.147	0.209	0.153	0.215

Note: Standard errors in parentheses. Even numbered columns include controls for industry and state of residence, odd numbered columns do not. After BAHA periods begin from May 2017. Observations after February 2020 are excluded from sample due to potential COVID-19 effects.
** p<0.01, * p<0.05, + p<0.1

Table 8 presents the results of this analysis with regards to H-1B affected American workers. This group does not see a statistically significant change in average yearly wage one-year post-BAHA, with an increase to average yearly wage of only \$380. After 2 years, the average yearly wage increased by \$1,474 when adding controls and increase by an additional \$2,155 by February 2020. While the two- and three-year specifications are statistically significant at the 1% level, this change in wage is not economically significant given the pre-BAHA wage of this group. This means that the overall effect of Trump's order on the productivity of H-1B affected Americans is consistently economically insignificant over the short-term, which supports the conclusion from my primary analysis. However, like with the H-1B worker analysis, the increase in magnitude of the causal effect over the years points to the limitations of a short-term analysis. If this slightly increasing trend continues, the effect of the BAHA order may differ when viewed from a long-term perspective.

CONCLUSION

President Trump's Buy American Hire American executive order had a significant and positive effect on the productivity of H-1B workers but had a relatively insignificant effect on the productivity of Americans who compete with H-1B workers for jobs. Given this lack of impact on its target group, I conclude that the BAHA order did not succeed in its overall objective of creating higher wages for American workers. When viewed in the context of employer decisions, this means that firms reacted to the order by raising the wages of H-1B workers who were at risk of being negatively impacted but were not incentivized to improve wages of their citizen workers who compete with H-1B workers.

The results of my research are limited to the short-term. Data on the long-term effects of this policy is not yet available. Further, it should be noted that my results rely on data from the U.S. DOL rather than USCIS. While this does not significantly impact my overall conclusions given the assumption that firms are rational, profit-maximizing, and cost-minimizing, an analysis done with similarly micro-level USCIS data would

likely have stronger causal implications. Future research should seek to re-evaluate my question should better data from USCIS become available. Additionally, my methodology takes a generalized approach to answering my question, focusing on country-wide and state-level impacts. However, prior research has indicated that there are a number of H-1B dependent firms that rely on the program as a large source of labor (Kerr & Lincoln, 2010). Future research should compare the results of my analysis with one that approaches the question from a firm-based perspective. This would better inform policymakers on how to best design regulations that improve overall welfare.

My work has certain policy implications. Firstly, it supports the consensus that immigration restrictions do not overall improve the welfare of Americans (Abramitzky et al., 2019; Clemens et al., 2018; Mayda et al., 2018; Peri et al., 2014). Secondly, it emphasizes the importance of an incentive-based approach to labor market policy directed at firms. In the case of the BAHA order, although it intended to help American workers, it failed to provide an incentive for firms to increase wages of this group while instead encouraging increased H-1B productivity. Future executive policy should consider where its incentivizing force lies if it aims to produce an effective outcome. Lastly, my conclusions point to the need for immigration policy that is based in empirical evidence and not fear or political aspirations. While it may be attractive to appeal to the fears and concerns some constituents have over foreign labor inflows that are perceived as harming American jobs, empirically informed policy can better preserve and improve American welfare.

Intrahousehold Determinants of Timely School Attendance in Uganda

Jeanne Legua

ECON 490

ABSTRACT

This study examines intra-household characteristics influencing school attendance and pace-for-age levels of children in Kitengesa and surrounding villages in rural Masaka, Uganda. Using the 2018 AFRIPads Economic Baseline Survey results, this study develops a descriptive matrix scan between the following household determinants and schooling pace-for-age trends: average age gap between siblings, the household head's years of education, liquidity status via livestock asset accounting, and total school expense per household. The data consists of baseline information gathered from 246 households, with a multivariate logistic regression model that is applied towards the children of the households sampled ($N = 719$) and cohorts of interest: eldest child, households with a female financial head, and households with at least one full-time occupation. The results from the study show varying trends in school pacing levels once evaluated according to either school level or gender, as well as across each cohort of interest. Overall, high average age gaps between

siblings is determined to be the most consistent indicator of lower pace levels observed in children in comparison to the other determinants, both in school level-based and gender-based analysis. Years of education of the household head is a robust determinant when associated with the financial head factor, while accumulating livestock assets discourage rather than augment timely school pacing. Directional and magnitude changes on school pacing levels is more significant for boys and kids enrolled in primary level. While limitations in the survey sample inhibit the study's ability to develop a conclusive characteristic of rural Masaka as the population of interest, this study remains successful in developing a novel analytical framework for examining the association between various household determinants and children's pace-for-age.

INTRODUCTION

Education is one of the most well-documented development tools associated with lifting families out of the poverty trap; yet, it is common to observe high absenteeism and delay in schooling for children, particularly in poor rural counties. One of the barriers is the time and monetary commitment required from households to finance their children's education, consisting of many hidden costs that are often difficult to anticipate (Wimer & Wolf, 2020). Committing to a school budget becomes more challenging in poor rural households due to lower disposable income – prompting households to allocate income towards shorter-term expenses with immediate yields (such as food and utilities) rather than to save and invest in longer-term investments. The problem is compounded for poorer families by other factors such as higher numbers of dependents, lower educational attainment rates among household heads, and concerns with cash flow. These factors also often affect individual children differently based on their age, birth order, school level, gender, and the socio-economic standing of their household. When households are unable to finance their children's education in a school term, the children are at risk of falling behind their age-appropriate

school level – delaying their attainment of education credentials that are linked to higher income, wealth and overall standard of living (Banerjee & Duflo, 2011).

In the 2018 AFRIPads Economic Baseline Survey report, households in Kitengesa and surrounding villages in rural Masaka, Uganda were surveyed to determine the baseline socio-economic condition of the community. One of the key results observed among dependents is that the median child of school-going age is one year behind their age-appropriate school level. Further, significant school pacing differences are observed between the dependents' gender and age groups. Inspired by this observation, this study adds to the analytical findings of the survey report by developing a descriptive matrix scan of household determinants that will describe the trends observed in children's school pace levels.

The descriptive matrix includes the following household characteristics: average age gap between siblings, the household head's years of education, household financial liquidity and total school expenses per household per term. These characteristics were identified based on similar studies that tested various household determinants affecting school attendance in developing countries (Kaul, 2018; Madhayan et. al., 2017; Mburu, 2017; Plug & Viiverberg, 2005; Prakash et. al., 2017; Schimdt, 2013; Wimer & Wolf, 2020). By categorizing an individual child's pace-for-age level according to quantiles, a multivariate logistical regression analysis is drawn for each individual child per household, tested according to the child's school level and gender. Apart from the aggregate cohort, this study also tests the correlation of school pace levels according to three cohorts of interest: eldest children, households with female financial heads, and households with additional regular income sources. These have been identified as cohorts of interest based on similar correlational studies conducted (Chakraborty & Prabal, 2017; Hedges et. al., 2016; Kaffle et. al., 2018; Kaul, 2018; Lusardi & Mitchell, 2014; Moser, 1998; Roby et. al., 2016; Supanantaroeck et. al., 2017).

Results from the study indicate that high average age gaps between siblings is the most consistent indicator of low pace-for-age levels in children across all cohorts. The years of education factor is an insufficient determinant in aggregate, unless applied specifically to the identified financial head of

the household. Livestock assets negatively influence school pacing levels, confirming findings from related literature. Additionally, primary school children and boys are determined to be more significantly affected by changes in the household determinants.

The formation of the descriptive matrix helps in developing a preliminary multi-level empirical analysis strategy for analysing school pacing levels – the first of its kind to be developed in rural Masaka, Uganda. While the survey is limited by the data gathering method and by the number of observations collected in the sample, this study is nonetheless successful in drawing unobserved correlations from the 2018 survey report, and offers a novel analytical framework for future researchers to investigate other plausible factors affecting school pacing.

LITERATURE REVIEW

Numerous works of literature have explored household determinants that influence school attendance and absenteeism rates of children across poor and rural communities. Household wealth is one of the most popular predictors associated with education investment due to its direct link to levels of disposable income (Banerjee & Duflo, 2011; Hedges, et. al., 2016; Plug & Vijverberg, 2005; Moser, 1998; Prakash, et al., 2017; Roby et al, 2016; Swift-Morgan, 2006). Apart from wealth, studies have also looked into independent compounding factors that influence education expensing patterns and school attendance such as household composition, financial literacy of the household head, and asset ownership.

For household composition, studies observe that factors, such as the eldest child bias, influence education investment patterns in households. Kaul's (2017) study explored gender-based patterns of educational expensing in India. Their findings confirm that rural households prioritize sending their boys to school over girls, with an apparent extended preferential treatment for the eldest son. In rural South Africa, gendered biases towards education investments appear to be influenced mostly by the exposure of the household to secular changes as opposed to other attitudes about gender norms (Madhavan, Myroniuk, Kuhn, & Collinson, 2017).

Other case studies explore the subjective management capacity of the financial head to make rational investment decisions. These explorations use educational attainment as one of the most predominant proxy metrics for financial literacy, and the gender of financial head as one of the key cohort categorizations. Various studies conducted in rural communities observe that women are commonly assigned the role of managing the household budget (Chakraborty and De, 2017; D'Espallier et. al, 2011; Lusardi & Mitchell, 2014; Mburu, 2016).

To help finance recurring expenses, families in rural developing communities rely on productive assets such as livestock. The reliance on asset financing (as opposed to formal employment streams) is generally related to the limited availability of formal sector employment in rural villages such as in Kitengesa¹. Commenting on school expense and budgeting, Hedges et al. (2016) found that household wealth is another likely predictor of education investment. Despite accumulating higher wealth due to the net growth of livestock assets, households would still need to invest time and resources through their dependents to rear their livestock. While beneficial for the household in terms of diversifying revenue generating streams, the demand and reliance on child herding labour is seen to negatively affect school attendance rates (Kafle et. al., 2018; Mburu, 2017). Further, Hedges et. al. (2016) found that boys and eldest children are often responsible for livestock rearing to support the family. Conversely, girls and later-born siblings appear to gain a marginal benefit towards being able to spend more time in school as herding obligations decrease.

EDUCATION IN UGANDA

Uganda has a predominantly young population, following a similar trend that exists in the rest of the continent. The 2017 Ugandan census reported that at least 55% of the population is under 18 years old, and this age group faces a unique set

¹ This detail was clarified from one of the local field staff members that conducted the survey in 2018.

of socio-economic circumstances that affect their timely attendance in school.

The education sector in Uganda faces several institutional challenges, including high levels of teacher and student absenteeism, weak school level management structures, inadequate availability of learning materials, and large class sizes. Uganda's education system generally follows seven years of primary education, followed by six years of secondary school (split between four years of regular and two years of advanced secondary levels) and three years of tertiary education. Progressing in each level requires students to pass a nationally administered exam, such as the Primary Leaving Examination (PLE) for primary education and the Uganda Certificate of Education (UCE) for secondary education. The 2017 census reports indicate that only 64% of students that complete primary level proceed to secondary level. Even further, transitions into the Advanced Secondary level through the UCE drop to 30%. At all stages of education, male students have stronger exam performances and higher completion and transition rates (Uganda Bureau of Statistics, 2017).

The affordability of education remains contingent on the net wealth of households. The Census of 2012 and 2013 also reported the share of monthly expenditure per household, categorized between rural and urban communities. Education comprises a share of 6.3% of monthly expenditure in rural areas (compared to 9.3% in urban areas), making it the fourth highest expense share of the household after food (50.8%), housing utilities (13.8%), and transportation (7%). Notably, Uganda was one of the first East African countries to adopt a Universal Primary Education (UPE) policy in 1997 to promote school attendance. The policy waives school fees for primary level education in public schools. While successful in making primary level schooling affordable, critics of the UPE argue that it has led to a poorer quality of education overall, reflected in the disappointing transition rates to secondary school despite high primary school attendance rates.

SAMPLE AND SURVEY METHODOLOGY

Survey Methodology

This study uses household and children unit-level data gathered from the 2018 AFRIPads Economic Baseline survey. The survey features socio-economic information from 246 households and 719 children surrounding AFRIPads' factory location in Kitengesa and surrounding villages in rural Masaka, Uganda. Patterned from the 2014 Demographic Health Survey questionnaire, the survey features five components: household living standards, employment, education of dependents, land assets, and access to banks and capital. The survey was conducted through a uniquely developed geographical cluster sampling technique, and the population of interest was defined using a two-kilometer radius from the AFRIPads factory location². Three local field staff fluent in Luganda and with prior experience in data collection were hired, and permission to conduct household surveys was requested from the local village councils within the surveyed area. The survey was conducted in teams of two (one local field staff and one student surveyor) over ten working days in early July. Each team possessed a geospatial tracking software, surveying households within the cluster boundary. Once identified, the local field staff administered the survey to the household head, or to the proxy head member of the household. Each interview took around 20 to 40 minutes³.

Descriptive Statistics

Information from the survey data revealed various characteristics about the household, children and region at large. There were 719 children included in the survey sample, with 352 boys and 367 girls. The children's mean age was 11 years old. At the time of the survey, a majority of the surveyed children attended school (90.84%); of which roughly 93% of them were boys and 88% were girls. On the other hand, 41 children (6%) did not attend school. About 10% of secondary-level age children did not attend school, compared to 5% of children in primary-level age.

²See Figures 1 and 2 in appendix I.

³See appendix II for excerpt sections of the survey questionnaire.

Table 1.
Household Descriptive Statistics

	Mean	Std. Deviation	Observations
<i>Household Composition</i>			
Number of people in the household	5.56	2.89	245
Number of dependents	3.27	2.51	245
Age of Child	10.86	4.4	756
<i>Respondent Years of Schooling</i>			
Female	8.48	4.71	138
Male	10.89	5.07	81
<i>Household Livestock Assets</i>			
Chickens	1	11.48	189
Pigs	2.16	2.53	170
Sheep	0.16	1.04	111
Goats	1.3	1.98	138
Local Cattle	1	2.3	131
<i>School Expense per Month</i>	781,955	1,028,533	176

Note: n = 246 households

The survey also revealed details about household characteristics in aggregate, as reflected in Table 1. The distribution between the number of household members and dependents was spread out, with an average of 5.56 household members and 3.72 dependents per household⁴. Regarding dependents reported in the households' surveys, there were 220 eldest children, consisting of 107 boys and 113 girls.

The mean years of education in single years among respondents was 9.4. The respondents had lower educational attainment levels: only 65% completed primary level, and an additional 14% ended their education midway through secondary level. Only 18% finished secondary level, and 4% pursued higher education. Conversely, 22% did not finish primary level, and 11% of respondents never attended school.

In terms of identifying the household head who makes financial decisions, roughly 78% of female respondents self-identified as financial heads, compared to 5% of males. Meanwhile, 67% of males and 18% of female respondents reported that both spouses make spending decisions in the household.

The 2018 report also featured a wealth index profile for various household assets categorized as either productive

⁴ The mean number of dependents reported does not include families whose children has already left home, or individuals living alone.

or non-productive assets. A majority of livestock-owning households owned at least one chicken (189 households), with roughly 30% of livestock-owning households having more than five chickens⁵. This is unsurprising, considering that chickens are one of the cheapest livestock assets to own. As for the remaining livestock assets, households tended to own more cattle as they own more goats. Sheep were the least owned livestock asset per household. 57 households did not own any livestock.

The survey also asked for the employment status of household members. The 2018 report showed that roughly 86% of households had at least one household member engaged in full-time employment. The types of cash-paid occupations varied, with some of the most common paid work types observed including construction workers (25%) and retail shop owners (13%). Most households that owned both livestock assets and had full-time employment indicated that they were using both for income (73%), while only about 25% of surveyed households relied on only one of the two income streams.

The amount budgeted per month on school-related expenses appeared to vary widely across households. While the median term-based household budget for school fees in 2018 was 409,000 Ugandan Shillings (UGX), households appeared to spend on average upwards of 781,955 UGX per term.⁶ This result shows that while many households did not allocate money for school fees, those that did spent up to twice the median budget observed in the community. Households spent on average 927,145 UGX per term for sending their kids to primary level school. Sending a child to secondary school appears to require households to allocate on average 1,783,062 UGX — which is almost double the amount from primary level. These results include the total household schooling expenses towards children enrolled in both public and private schools, and pricing differentials between the types of schools are not accounted for in the survey.

⁵See Table 5 for the estimate price table of livestock assets.

⁶This is equivalent to roughly 110 USD, based on the 2018 UGX/USD average exchange rate (3671 UGX = 1 USD)

EMPIRICAL STRATEGY

Intrigued by the results of the 2018 AFRIPads Economic Baseline survey regarding household school budgeting behavior and children’s school pace-for-age levels, this study investigates household determinants supporting or inhibiting the pace of school-aged children in the community. To this end, a descriptive matrix analytical strategy was developed. This section outlines the variables of interest and method of analysis employed in the results section.

Description of Variables

The variable of interest is the child pace-for age level. The definition of the pace-for-age levels are pre-determined in the 2018 AFRIPads Economic Baseline survey report, given the conventional pace levels observed on children attending school in Uganda. Denoted in multimodal years, this variable is derived from the following:

$$ChildOnPaceForAgeYears_c = ChildAge_c - CurrentLevel_c + 5$$

where the age of an individual child c is subtracted from their current school level and “constant”, which is characterized as the youngest year that a child can begin attending primary school. Table 2 outlines the pace-for-age levels in Uganda, and Table 3 summarizes the pace-for-age levels observed from survey results.

Table 2.
Age-appropriate school level in Uganda

School Level (years)	Age
P1 (1)	6 years
P2 (2)	7 years
P3 (3)	8 years
P4 (4)	9 years
P5 (5)	10 years
P6 (6)	11 years
P7 (7)	12 years
S1 (8)	13 years
S2 (9)	14 years
S3 (10)	15 years
S4 (11)	16 years
S5 (12)	17 years
S6 (13)	18 years

Note: primary (P) and secondary (S) levels. Assigned on-pace age-appropriate number of years in school in brackets.

Table 3.
Child pace-for-age at school, by gender and school Level

	Mean	Std. Deviation
<i>All Children</i>	-1.99	3.27
Boy	-2.04	3.02
Girl	-1.75	3.53
 <i>Primary Level</i>	 -1.1	 1.96
Boy	-1.43	1.96
Girl	-0.84	2.06
 <i>Secondary Level</i>	 -1.35	 2.02
Boy	-1.47	2.15
Girl	-1.07	2.23

Note: Mean taken for each category, per child observation.

Overall, the children in the survey sample are two years behind their age-appropriate level. When examined by gender, boys tend to be further behind than girls. Children in primary level is more at pace compared to children in secondary level, with the mean pace levels decreasing by 0.25 as they move into secondary level education.

Next, three characteristics of the household are postulated to be influential determinants to school pace-for-age levels per child: sibling composition within the household, financial behavior of the household head, and the household's liquidity status. The relational strength of these three household determinants to school pace levels are tested through the following measurement proxy units collected from the survey: average age gap of dependents, household head's years of education, and relative cumulative livestock assets.

The average age gap of dependents in each household was computed via the following:

$$AgeGap_h = \frac{\sum (K_i - K_{i-1})}{i - 1}$$

where K represents the age of each child, organized according to birth order i per household h . Based on survey results and as seen in Table 4, the average gap between siblings in a household is 2.91 years, with a 1.29 standard deviation.

*Intrahousehold Determinants
of Timely School Attendance in Uganda*

Table 4.
Descriptive Statistics for Child Pace for Age, according to Household Determinants

	All	Primary Level			Secondary Level			Not Currently In School		
		Total	Girl	Boy	Total	Girl	Boy	Total	Girl	Boy
ChildAgeDev	2.91 <i>(1.29)</i>	2.82 <i>(1.27)</i>	2.92 <i>(1.30)</i>	2.75 <i>(1.24)</i>	3.15 <i>(1.30)</i>	3.35 <i>(1.20)</i>	2.92 <i>(1.37)</i>	2.95 <i>(1.33)</i>	2.85 <i>(1.32)</i>	3.09 <i>(1.36)</i>
	719	450	222	228	114	67	47	155	78	77
RespYearsEduc	9.08 <i>(5.20)</i>	9.60 <i>(5.23)</i>	9.52 <i>(5.16)</i>	9.94 <i>(5.12)</i>	10.10 <i>(5.08)</i>	9.62 <i>(4.91)</i>	10.56 <i>(5.19)</i>	6.72 <i>(4.50)</i>	6.36 <i>(4.13)</i>	7.52 <i>(4.85)</i>
	685	436	212	224	107	62	45	142	72	70
RespYearsEduc (FemaleFinHead)	8.18 <i>(4.66)</i>	8.55 <i>(4.79)</i>	8.68 <i>(4.78)</i>	8.42 <i>(4.36)</i>	8.86 <i>(4.39)</i>	9.14 <i>(4.11)</i>	7.41 <i>(5.09)</i>	6.76 <i>(4.21)</i>	6.45 <i>(3.51)</i>	7.53 <i>(3.70)</i>
	375	231	118	113	57	38	19	87	50	37
LogLivestockWeight	3.71 <i>(1.34)</i>	2.83 <i>(1.33)</i>	3.60 <i>(1.31)</i>	3.72 <i>(1.34)</i>	4.14 <i>(1.29)</i>	4.04 <i>(1.32)</i>	4.24 <i>(1.27)</i>	3.49 <i>(1.36)</i>	3.24 <i>(1.49)</i>	3.71 <i>(1.20)</i>
	637	392	192	200	109	64	45	136	68	68

Note: Mean taken for every child observation, weighted per household. Standard deviation in parenthesis; observations are reported in italics.

The household head's years of education, educational attainment and gender of the financial head are tabulated from the survey questionnaire⁷. As the surveyors only requested details about the educational attainment of the respondent (who may not necessarily be the financial head), comparisons may only be drawn from respondents who self-identified as financial heads. Table 4 summarizes the respondents' years of education in aggregate, and within the female financial head cohort.

To serve as a proxy indicator for household financial liquidity, a cumulative Livestock Index was developed per household, as summarized in Table 4. The index is expressed as follows:

$$f_n(L) = (L_1 + \theta L_2 + \dots) = \sum_h^c \theta L_c$$

where L is the livestock type per unit livestock c and household h .

Table 5.
Price of Livestock and the "Chicken Multiple"

Relative weighted value of Livestock (i.e. "Chicken Multiple")	Livestock	Price of livestock (based on 2018 average entry-level livestock market price)
100	Cattle	800,000 UGX
10	Goat	80,000 UGX
10	Pigs	80,000 UGX
7.5	Sheep	60,000 UGX
1	Chicken	8,000 UGX

Note: 2018 UGX/USD average exchange rate is 3671 UGX = 1 USD

⁷ See appendix II.

For the purposes of this analysis, this study defines the “Chicken Multiple,” which is the relative weighted value of the livestock based ordinal price range from the 2018 average local livestock market price (represented as theta). The “Chicken Multiple” is derived from the average price of a chicken in rural Masaka, in comparison to other livestock assets tabulated in the survey. Table 5 summarizes the formation of the multiple. The “Chicken Multiple” allows relative valuation between varied livestock assets in a household. Each household has a distinct Livestock Weight Index assigned, and its logarithm was taken to balance for outliers. Based on the survey, the average Livestock index is 83.94 units.

Method of Analysis

To test the relational strength of the household characteristics outlined above with school pacing levels, this study conducts a descriptive matrix scan by using a multivariate logistical regression model, as follows:

$$Y_{ih} = \beta_0 + \beta_1 ChildAgeDev_{ih} + \beta_2 RespYearsEduc_{ih} + \beta_3 LogLivestockWeight_{ih} + \beta_4 SchoolExpenseTotal_{ih} + \varepsilon_{ih}$$

where Υ_{ih} represents the pace-for-age level for every child i in household h , reported in multimodal number of years. $ChildAgeDev_{ih}$ is an indicator for household composition, testing the average age gap between dependents in the same household. $RespYearsEduc_{ih}$ is reported by number of years spent by the household head in school. $LogLivestockWeight_{ih}$ is the proxy indicator for household liquidity, using relative cumulative weight of each livestock asset. $SchoolExpenseTotal_{ih}$ reports the total amount allocated by each household per month on school expenses (including fees and other school-related items) which is included given its compounding influence on school attendance rates. β_0 accounts for other household determinants correlated with pace-for-age levels. The coefficients of interest are β_1 , β_2 and β_3 .

Each child in the survey sample was assigned a Υ_{ih} value, based on the difference between their age and the appropriate age for their school level. To examine the varying degrees of correlational strength between households in the sample, the dispersion of Υ_{ih} was divided into quartiles.

The “Bottom Percentile” groups 25% of the total number of children that are the most behind in their schooling based on their age, while the “Upper Percentile” includes the top 25% that are at pace or advanced relative to other children. Majority of the observations are grouped in the “Middle Percentile” range, which account for the bimodal mid-quartile values of median school pacing levels observed in the total survey sample.

The model is applied to the survey sample, as well as to specific cohorts hypothesized to closely influence the decision to spend for their children’s education. The Eldest Child cohort groups all first-born children in each household, as birth order is proven to be a significant trait considered when parents allocate money for school expenses. The Female Financial Head cohort combines all households where the female head is also the main financial decision maker of the household; this angle examines the applicability of the findings theorizing the difference in budget management skills between genders towards our survey sample. Lastly, the Employment cohort includes households that have at least one more full-time occupation that earns money (excluding livestock buy-and-sell). While Livestock Assets are determined to be one of the primary income sources amongst rural counties in Uganda, this angle factors in plausible income stream diversification attitudes practiced in the household towards increased liquidity.

Given that this analysis is an extension of a previously conducted survey, the research design is limited to the conditions set by the survey methodology. It is also to be anticipated that the adoption of a descriptive matrix scan strategy will highlight a combination of variables and cohorts that provide a stronger correlation, while others will have little-to-null significant association to school pacing levels. Lastly, a limitation of the matrix scan model is that endogenous factors are not incorporated in the study design, and so this may be an opportunity for future iterations of this research study.

RESULTS

Overview

Overall, the results show intriguing patterns confirming the explanatory ability of several household determinants towards pace-for-age levels. Within each pace-for-age percentile, the patterns also shift when analysed according to school level or gender, across varying cohorts of children.

Table 6.
Children's Pace for Age and Household Determinants, according to School Level

	School Levels			
	All	Primary	Secondary	Not In School
<i>Bottom Percentile</i>				
ChildAgeDev	-0.672*** (0.244)	-0.523*** (0.165)	-0.343** (0.153)	-0.900*** (0.318)
RespYearsEduc	0.176*** (0.0538)	-0.00445 (0.0302)	0.0199 (0.0379)	-0.0278 (0.100)
LogLivestockWeight	-0.333 (0.242)	-0.210 (0.136)	0.283 (0.239)	-0.626* (0.320)
SchoolExpenseTotal	-3.21e-07 (4.79e-07)	-3.03e-07 (2.85e-07)	1.08e-06 (6.66e-07)	-4.65e-07 (5.75e-07)
Constant	-3.711** (1.419)	-1.281 (0.869)	-4.814*** (1.217)	-3.612*** (1.768)
Observations	123	56	21	46
R-squared	0.152	0.197	0.577	0.234
<i>Middle Percentile</i>				
ChildAgeDev	0.114*** (0.0400)	0.154*** (0.0504)	0.0653 (0.0904)	0.121 (0.106)
RespYearsEduc	0.0123 (0.00933)	0.0156 (0.0112)	0.0123 (0.0215)	0.00333 (0.0298)
LogLivestockWeight	0.0458 (0.0330)	0.0615 (0.0382)	0.130 (0.0952)	-0.0335 (0.109)
SchoolExpenseTotal	-1.48e-07*** (3.95e-08)	-2.47e-07*** (5.91e-08)	-1.10e-07 (7.19e-08)	6.12e-08 (9.70e-08)
Constant	-1.366*** (0.193)	-1.444*** (0.230)	-1.848*** (0.606)	-1.070** (0.460)
Observations	351	240	68	43
R-squared	0.048	0.085	0.056	0.075
<i>Upper Percentile</i>				
ChildAgeDev	0.0386 (0.115)	0.115 (0.0737)	1.696 (1.302)	-0.154 (0.113)
RespYearsEduc	-0.0416 (0.0379)	0.0369 (0.0278)	-0.191 (0.192)	-0.343*** (0.0352)
LogLivestockWeight	-0.112 (0.141)	-0.0863 (0.0946)	-1.020 (0.947)	1.058*** (0.0939)
SchoolExpenseTotal	-4.67e-08 (1.31e-07)	-8.60e-08 (8.21e-08)	-6.40e-07 (9.97e-07)	-2.23e-06*** (2.63e-07)
Constant	2.671*** (0.699)	1.387*** (0.517)	2.416 (4.941)	3.771*** (0.409)
Observations	81	58	13	10
R-squared	0.050	0.068	0.448	0.973

Note: The linear regression model is applied for each child observation with a distinct set of household characteristics: average age gap between dependents (*ChildAgeDev*), household head years of education (*RespYearsEduc*), relative cumulative livestock wealth (*LogLivestockWeight*), and total school expenses per month (*SchoolExpenseTotal*). The result is weighted per unit household for families with multiple children. Each child observation was grouped according to their relative distance from the survey sample pace-for-age level mean. *Lower percentile* includes the bottom 25%, while *Upper Percentile* includes the top 25%. Observations falling in between 25-75% are categorized as *Middle Percentile*. Total number of observations = 555 individual children. Interpretations capped for at least 10% of total observations. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6 summarizes all of the school-going-aged children in the survey sample (N = 555) according to school level. The age gap dispersion between siblings shows a significant inverse relation across the bottom percentile, indicating that a high age

gap between dependents contributes towards children not being at pace for school. The results are more pronounced for primary level students, and this is a significant factor for children that are currently not attending school. The correlation shifts magnitude and direction as the children become more at pace: for the middle percentile group, a higher age gap between siblings is related to a relatively higher likelihood of children to be nearer to the target pace. This result is an attributable indication that for many of the children that are not at pace for their age in schooling, it is likely due to the financial constraints of the household to equitably finance school expenses across all their dependents.

Another intriguing result is the directional relation between the Livestock Weight variable and pace-for-age. While not statistically significant, the coefficients in the bottom percentile indicate that higher relative livestock wealth is inversely related to school pacing— supporting Hedges et al.’s (2016) results on children’s role to tend for their livestock. The Livestock Weight pattern changes in the middle percentile, showing a positive link between the two variables that posit the use of livestock assets to finance their children’s schooling. The pattern re-shifts in the upper percentile, reverting to bottom percentile ranges — indicative of the household’s prioritization of livestock-rearing over school pacing levels. While results for livestock asset trends do not show statistical significance, the directional trend across percentiles supports the liquidity and use of livestock assets for varying expenses. Lastly, the significance of the constant term indicates high likelihood for other household determinants to affect school pacing levels among children in households.

Table 7.
Children’s Pace for Age and Household Determinants, according to Gender

	Lower Percentile			Middle Percentile			Upper Percentile		
	All	Girls	Boys	All	Girls	Boys	All	Girls	Boys
ChildAgeDev	-0.672*** (0.244)	-0.528 (0.329)	-0.609* (0.342)	0.114*** (0.0400)	0.0484 (0.0576)	0.185*** (0.0567)	0.0386 (0.115)	-0.119 (0.158)	0.115 (0.211)
RespYearsEduc	0.176*** (0.0538)	0.317*** (0.0738)	0.0583 (0.0732)	0.0123 (0.00933)	0.00257 (0.0131)	0.0217 (0.0135)	-0.0416 (0.0379)	-0.0195 (0.0480)	-0.0644 (0.0672)
LogLivestockWeight	-0.333 (0.242)	-0.464 (0.363)	-0.388 (0.309)	0.0458 (0.0330)	0.105** (0.0472)	0.00352 (0.0461)	-0.112 (0.141)	-0.294 (0.183)	0.0175 (0.288)
SchoolExpenseTotal	-3.21e-07 (4.79e-07)	-1.01e-07 (6.87e-07)	-2.02e-07 (6.29e-07)	07*** (3.95e-08)	07** (7.47e-08)	07*** (4.99e-08)	-4.67e-08 (1.31e-07)	-9.16e-08 (2.39e-07)	-3.26e-08 (1.75e-07)
Constant	-3.711** (1.419)	-6.103*** (2.076)	-2.152 (1.837)	-1.366*** (0.193)	-1.343*** (0.269)	-1.434*** (0.278)	2.671*** (0.699)	3.622*** (1.017)	2.139* (1.089)
Observations	123	56	67	351	168	183	81	47	34
R-squared	0.152	0.314	0.079	0.048	0.045	0.086	0.050	0.079	0.084

Note: The linear regression model is applied for each child observation with a distinct set of household characteristics: average age gap between dependents (ChildAgeDev), household head years of education (RespYearsEduc), relative cumulative livestock wealth (LogLivestockWeight), and total school expenses per month (SchoolExpenseTotal). The result is weighted per unit household for families with multiple children. Each child observation was grouped according to their relative distance from the survey sample pace-for-age level mean. Lower percentile includes the bottom 25%, while Upper Percentile includes the top 25%. Observations falling in between 25-75% are categorized as Middle Percentile. Total number of observations = 555 individual children; interpretations capped for at least 10% of total observations. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7 looks at similar correlations between school pacing and household determinants across genders. Trend patterns between percentiles vary; however, overall results indicate that the pacing of the boys is more likely to be impacted by the household determinants compared to girls. In particular, high age gap differences between siblings is a significant factor for boys who fall behind in their age-appropriate school levels; conversely, boys that are able to maintain relatively similar pacing levels to the majority of their cohort are more likely to be chosen over their siblings to go to school. Alternatively, the findings about girls and school pacing have different implications. Girls who are the least at pace for their age appear to be more likely to attend school if their household head spent more time in school. Girls who are within the middle range of school pacing are more likely to remain at pace for school when their household has more livestock assets. Similar to the previous findings, the strong statistical significance of the constant term accounts for the influence of other household determinants that were not captured by this study's methodology.

Eldest Child Factor

Next, the study investigates relational strength of the household determinants to the pacing levels of the eldest children in the survey sample (N = 166). Table 8 summarizes the findings according to school levels⁸. A significant inverse pattern is observed in the sibling age gap factor for the eldest children belonging to the bottom percentile, and the inverse pattern becomes less pronounced (but still a significant factor) once the eldest child moves to secondary level. Within the middle percentile, the age gap factor shifts directions, and is highly correlated with age gaps in the primary level. Interestingly, the result shifts direction once the eldest child reaches secondary level. Perhaps this is evidence of the higher likelihood for eldest children to be at pace in primary school, and less at pace once they reach the age-appropriate level for secondary level education. The number of years spent by the

⁸ *The comparison across girls and boys could not be tested due to insufficient observations.*

household head in school is significantly related to the higher pacing levels of their eldest born, becoming more pronounced for students that are at the age-appropriate pace for primary level.

Table 8.
Children's Pace for Age and Household Determinants among Eldest Children, according to School Level

	School Levels			
	All	Primary	Secondary	Not In School
<i>Bottom Percentile</i>				
ChildAgeDev	-0.603* (0.329)	-0.670** (0.245)	-0.358** (0.158)	-1.564*** (0.281)
RespYearsEduc	0.176* (0.0912)	-0.0539 (0.0535)	0.0169 (0.0389)	-0.239* (0.128)
LogLivestockWeight	-0.238 (0.374)	-0.122 (0.223)	0.310 (0.247)	0.258 (0.325)
SchoolExpenseTotal	-4.20e-07 (8.88e-07)	-1.26e-06* (7.04e-07)	1.03e-06 (6.83e-07)	-1.10e-06* (6.21e-07)
Constant	-4.501** (2.152)	-0.557 (1.446)	-4.828*** (1.241)	-5.390*** (1.573)
Observations	68	25	20	23
R-squared	0.116	0.400	0.587	0.666
<i>Middle Percentile</i>				
ChildAgeDev	0.00803 (0.0667)	0.438*** (0.129)	-0.0332 (0.0900)	-0.572 (0)
RespYearsEduc	0.0371** (0.0181)	0.0757*** (0.0240)	0.0306 (0.0262)	0.0205 (0)
LogLivestockWeight	-0.0370 (0.0617)	-0.0525 (0.0856)	0.103 (0.108)	-0.494 (0)
SchoolExpenseTotal	-1.30e-07 (8.23e-08)	-5.93e-07*** (1.62e-07)	-6.64e-08 (9.58e-08)	-2.56e-06 (0)
Constant	-1.362*** (0.299)	-2.092*** (0.391)	-1.935*** (0.655)	1.297 (0)
Observations	89	39	44	6
R-squared	0.086	0.433	0.089	1.000
<i>Upper Percentile</i>				
ChildAgeDev	0.153 (0.0889)			
RespYearsEduc	-0.0336 (0.0179)	-0.000598 (0)	0 (0)	
LogLivestockWeight	0.351*** (0.0561)	0.239 (0)		
SchoolExpenseTotal	-9.00e-07 (4.89e-07)	-2.68e-06 (0)		
Constant	0.502 (0.333)	0.798 (0)	1 (0)	
Observations	9	6	3	
R-squared	0.945	1.000		

Note: The linear regression model is applied for each eldest child observation with a distinct set of household characteristics: average age gap between dependents (*ChildAgeDev*), household head years of education (*RespYearsEduc*), relative cumulative livestock wealth (*LogLivestockWeight*), and total school expenses per month (*SchoolExpenseTotal*). The result is weighted per unit household for families with multiple children. Each eldest child observation was grouped according to their relative distance from the survey sample pace-for-age level mean. *Lower percentile* includes the bottom 25%, while *Upper Percentile* includes the top 25%. Observations falling in between 25-75% are categorized as *Middle Percentile*. Total number of observations = 166 individual children; interpretations capped for at least 10% of total observations. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

In terms of household school expensing, results from both bottom and middle percentiles within the primary level indicate that households decrease spending for their eldest child's education if the child closer to being at pace for their grade level. However, once the eldest child is in secondary

school, expensing patterns shift directions, and this may be due to the contributions of the Universal Primary Education Program in waiving primary school tuition fees.

Female Financial Heads Factor

The next set of tables explore the school pacing of children living in households with a female financial head (N = 283). While households with female financial heads show similar trends to the aggregate findings featured in Table 6, Table 9 showcases noteworthy patterns observed in the pacing levels of children in secondary school.

Table 9.
Children's Pace for Age and Household Determinants among Households with Female Financial Heads, according to School Level

	School Levels			
	All	Primary	Secondary	Not in School
<i>Bottom Percentile</i>				
ChildAgeDev	-0.901*** (0.304)	-0.478** (0.221)	-1.025*** (0.0561)	-1.480*** (0.268)
RespYearsEduc	0.0331 (0.0783)	-0.0381 (0.0428)	0.0907*** (0.0103)	-0.143 (0.102)
LogLivestockWeight	-0.325 (0.359)	-0.273 (0.229)	0.325*** (0.0513)	-0.314 (0.328)
SchoolExpenseTotal	-2.14e-07 (7.10e-07)	-3.63e-08 (4.98e-07)	-7.07e-07*** (1.77e-07)	-3.46e-07 (5.62e-07)
Constant	-1.688 (1.968)	-1.223 (1.270)	-0.724* (0.368)	-2.397 (1.694)
Observations	68	29	14	25
R-squared	0.147	0.192	0.988	0.650
<i>Middle Percentile</i>				
ChildAgeDev	0.194*** (0.0543)	0.220*** (0.0665)	-0.0694 (0.113)	0.118 (0.163)
RespYearsEduc	0.0220 (0.0153)	0.0257 (0.0182)	-0.120*** (0.0358)	0.0146 (0.0505)
LogLivestockWeight	-0.0381 (0.0449)	-0.0609 (0.0550)	0.281*** (0.0976)	0.158 (0.166)
SchoolExpenseTotal	-1.23e-07* (7.39e-08)	-1.98e-07** (9.64e-08)	2.47e-07** (1.18e-07)	8.53e-08 (2.04e-07)
Constant	-1.511*** (0.243)	-1.451*** (0.284)	-1.734*** (0.583)	-2.199*** (0.724)
Observations	166	118	30	18
R-squared	0.088	0.125	0.436	0.219
<i>Upper Percentile</i>				
ChildAgeDev	0.110 (0.103)	0.0568 (0.137)	0 (0)	0.00445 (0.204)
RespYearsEduc	0.0753** (0.0287)	0.0822** (0.0389)	0 (0)	-0.0185 (0.0629)
LogLivestockWeight	-0.0578 (0.111)	-0.0733 (0.172)	0 (0)	0.532 (0.369)
SchoolExpenseTotal	-3.71e-07** (1.77e-07)	-2.79e-07 (3.62e-07)	0 (0)	-8.18e-07 (6.78e-07)
Constant	1.167** (0.565)	1.455 (0.857)	1 (0)	0.579 (1.036)
Observations	49	31	10	20
R-squared	0.233	0.183		0.152

Note: The linear regression model is applied for each child observation living with a female financial head, via the following household characteristics: average age gap between dependents (*ChildAgeDev*), household head years of education (*RespYearsEduc*), relative cumulative livestock wealth (*LogLivestockWeight*), and total school expenses per month (*SchoolExpenseTotal*). The result is weighted per unit household for families with multiple children. Each child observation was grouped according to their relative distance from the survey sample pace-for-age level mean. *Lower percentile* includes the bottom 25%, while *Upper Percentile* includes the top 25%. Observations falling in between 25-75% are categorized as *Middle Percentile*. Total number of observations = 283 individual children; interpretations capped for at least 10% of total observations. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Higher levels of livestock assets are statistically correlated with higher pace-for-age levels amongst children in the middle percentile, which may be indicative of the active role that female financial heads play in the sales and purchases of livestock for financing monthly school expenses. This trend is further corroborated by the high inverse relationship between educational attainment of the female financial head and school pace levels of children at the bottom percentile. Comparing school expense levels from the aggregate sample, female financial heads also seem to allocate more money towards financing their children through secondary school, at par with the median school pacing patterns. Interestingly, higher pace levels amongst secondary level children that belong to the middle percentile is associated with a lower number of years spent in school among the female financial heads.

Table 10 reflects similar patterns based on the aggregate findings in Table 7. Focusing particularly on the significant trend levels in the middle percentile; the pace-for-age levels of children living with female financial heads exhibit higher statistical correlations between the boys and the determinant factors. Boys being at better pace for their school level is associated with higher years of education amongst female financial heads. As well, higher levels of livestock assets appear to negatively impact the pacing of boys, possibly because of the expectation pressed upon boys to help tend to the livestock.

Table 10.
Children's Pace for Age and Household Determinants among Households with Female Financial Heads, according to Gender

	Lower Percentile			Middle Percentile			Upper Percentile		
	All	Girls	Boys	All	Girls	Boys	All	Girls	Boys
ChildAgeDev	-0.901*** (0.304)	-0.644 (0.391)	-0.888* (0.485)	0.194*** (0.0543)	-0.0754 (0.0875)	0.352*** (0.0662)	0.110 (0.103)	0.113 (0.117)	0.00445 (0.204)
RespYearsEduc	0.0331 (0.0783)	0.174 (0.115)	-0.0692 (0.103)	0.0220 (0.0153)	-0.0109 (0.0205)	0.0379* (0.0213)	0.0753** (0.0287)	0.0954*** (0.0323)	-0.0185 (0.0629)
LogLivestockWeight	-0.325 (0.359)	-0.395 (0.535)	-0.738 (0.501)	-0.0381 (0.0449)	0.0303 (0.0682)	-0.106* (0.0566)	-0.0578 (0.111)	-0.172 (0.112)	0.532 (0.369)
SchoolExpenseTotal	-2.14e-07 (7.10e-07)	1.30e-07 (9.57e-07)	1.19e-07 (1.05e-06)	-1.23e-07* (7.39e-08)	-6.99e-08 (9.97e-08)	-1.52e-07 (1.01e-07)	07** (1.77e-07)	07** (1.65e-07)	-8.18e-07 (6.78e-07)
Constant	-1.688 (1.968)	-4.858* (2.771)	1.235 (2.731)	-1.511*** (0.243)	-0.844** (0.385)	-1.725*** (0.306)	1.167** (0.565)	1.368** (0.656)	0.579 (1.036)
Observations	68	35	33	166	82	84	49	29	20
R-squared	0.147	0.182	0.176	0.088	0.029	0.305	0.233	0.456	0.152

Note: The linear regression model is applied for each child observation living with a female financial head, via the following household characteristics: average age gap between dependents (ChildAgeDev), household head years of education (RespYearsEduc), relative cumulative livestock wealth (LogLivestockWeight), and total school expenses per month (SchoolExpenseTotal). The result is weighted per unit household for families with multiple children. Each child observation was grouped according to their relative distance from the survey sample pace-for-age level mean. Lower percentile includes the bottom 25%, while Upper Percentile includes the top 25%. Observations falling in between 25-75% are categorized as Middle Percentile. Total number of observations = 283 individual children; interpretations capped for at least 10% of total observations. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Employment Factor

Lastly, the study analyzes the cohort of children who live in a household with at least one household head who is employed full-time (N = 258). The cohort generally follows the same trend lines as the aggregate in both school level and gender categorizations, indicative that having an additional employment-based income stream does not necessarily improve the chances of a child to remain at pace for school. This holds true across pace-level percentiles, with the exception of the heightened significance observed in household heads' years of education and livestock assets. As observed in Table 11, children that are the most at pace to school relative to their peers tend to live with household heads who spent lower numbers of years in school. When contextualized within the cohort, 41 children living in households with at least one parent engaging in another full-time income-generating activity have been able to maintain better pace levels in school. As well, children that are most at pace for school and are living in households with more than one income stream have significantly lower amounts of livestock assets.

Table 11.
Children's Pace for Age and Household Determinants among Households with other Employment Streams, according to Gender

	Lower Percentile			Middle Percentile			Upper Percentile		
	All	Girls	Boys	All	Girls	Boys	All	Girls	Boys
ChildAgeDev	-1.054** (0.400)	-1.082* (0.533)	-1.024 (0.609)	0.131** (0.0580)	0.0459 (0.0818)	0.221** (0.0839)	0.00893 (0.158)	-0.219 (0.212)	-0.0900 (0.344)
RespYearsEduc	0.201** (0.0826)	0.273** (0.106)	0.129 (0.140)	0.0174 (0.0166)	0.0109 (0.0222)	0.0247 (0.0240)	-0.183*** (0.0607)	-0.157** (0.0750)	-0.216 (0.122)
LogLivestockWeight	-0.455 (0.373)	-0.337 (0.546)	-0.529 (0.546)	0.0311 (0.0452)	0.124* (0.0697)	-0.000258 (0.0593)	-0.437** (0.207)	-0.726** (0.273)	-0.251 (0.501)
SchoolExpenseTotal	-5.43e-08 (6.77e-07)	7.95e-08 (9.60e-07)	2.55e-08 (1.02e-06)	-1.92e-07*** (5.09e-08)	-3.49e-07*** (1.31e-07)	-2.26e-07*** (6.53e-08)	-2.41e-08 (1.66e-07)	-1.53e-07 (3.82e-07)	-3.70e-08 (2.34e-07)
Constant	-2.222 (2.312)	-3.475 (3.208)	-1.443 (3.443)	-1.283*** (0.326)	-1.337*** (0.482)	-1.347*** (0.459)	5.968*** (1.232)	7.594*** (1.815)	5.500** (2.175)
Observations	54	24	30	163	72	91	41	25	16
R-squared	0.208	0.395	0.129	0.087	0.123	0.139	0.299	0.368	0.331

Note: The linear regression model is applied for each child observation living with at least one parent working full time, via the following household characteristics: average age gap between dependents (ChildAgeDev), household head years of education (RespYearsEduc), relative cumulative livestock wealth (LogLivestockWeight), and total school expenses per month (SchoolExpenseTotal). The result is weighted per unit household for families with multiple children. Each child observation was grouped according to their relative distance from the survey sample pace-for-age level mean. Lower percentile includes the bottom 25%, while Upper Percentile includes the top 25%. Observations falling in between 25-75% are categorized as Middle Percentile. Total number of observations = 258 individual children; interpretations capped for at least 10% of total observations. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

CONCLUSION

This study initiated an exploratory exercise investigating intra-household factors that influence school pace-for-age levels for the children in rural Masaka, Uganda. By employing a descriptive matrix scan through a multivariate regression model analysis, the magnitude and directional association between each household determinant and a child's school pacing

levels was determined in aggregate and within cohorts. The average age difference between siblings was determined to be a consistent indicator of low pace-for-age levels across all cohorts. Respondent years of education became a robust indicator once applied to the identified financial head. Livestock assets (normally attributed as a form of liquid exchangeable asset) appeared to have a negative correlation with children's school pacing levels. It has also been shown that the gravitational influence of the household determinants is highest towards the pacing levels of boys and primary school students — indicative that the survey sample's households may benchmark several school investment decisions towards boys and primary school students. Despite the limitations in modeling a robustness check analysis due to limited observations collected from the survey sample, this study was successfully able to initiate one of the first comprehensive multi-level correlational research of its kind developed in the village of Kitengesa and surrounding counties, and this research offers opportunities for further studies to be developed which could further investigate other confounding household determinants that affect school pacing levels.

The Effectiveness of the Menstrual Hygiene Scheme in Improving Female Educational and Autonomy Outcomes in Target Indian Districts

Manika Marwah

ECON 494

ABSTRACT

Sanitary Menstrual Hygiene Technology (MHT) is recognised in medical literature as being an important contributor to female health. Popular media, policymakers, and qualitative health studies often link low female educational attainment and autonomy directly with this paucity of sanitary MHT. Using the exogenous shock of the Menstrual Hygiene Scheme (MHS) launched in select Indian districts, this study quantitatively evaluates two questions: the effectiveness of the MHS as a policy, and the legitimacy of claims suggesting augmenting MHT access has a causal effect on female education and autonomy indicators. Since districts for this policy's implementation were randomly chosen, I use policy implementation as an instrument, as it is plausibly unrelated to my education and autonomy outcomes. I find that as a policy, the MHS was successful, and target females in treated districts were 4% more likely to use sanitary pads as compared to untreated districts — a finding that is robust to the addition of controls on religion, caste, wealth, education, and marital status.² However, contrary to popular belief, this increase in use of pads has no significant impact on female educational attainment, or autonomy indicators.

² *Result from Table A4 Col (6)*

INTRODUCTION

Over the past few years, increased attention has been paid to the paucity of menstrual hygiene products for women in developing countries (Kuhlmann, Henry, & Wall, 2017). Menstrual hygiene is an important facet of female well-being, and the adverse health effects of poor menstrual hygiene are well-established. However, it is argued by the media, public health professionals, and policy makers that this lack of access to proper menstrual hygiene measures has negative effects not only on female health, dignity and comfort, but also on female educational attainment and autonomy. These groups often directly relate low female educational attainment with this paucity of sanitary Menstrual Hygiene Technology- MHT (De la Rosa, 2019). Numerous qualitative public health studies link lack of menstrual hygiene to lower schooling outcomes (Kansal et al., 2016; Sarkar, Dobe, Dasgupta, Basu, & Shahbabu, 2017; Upashe, Tekelab, & Mekonnen, 2015). However, previous research has not attempted to quantitatively evaluate the legitimacy of these claims, especially in the Indian context. This paper attempts to fill that gap by studying the impact of a recent Indian policy- the Menstrual Hygiene Scheme (MHS): its performance as a policy, and any consequent effect on female educational and autonomy indicators in target districts.

I choose to focus my research on the Indian context for two reasons: firstly, India struggles particularly with sanitary menstrual hygiene management; secondly, the MHS serves as a plausibly exogenous shock to study the variability of improved access to sanitary menstrual technology (Waheed, 2018). The Indian Government launched the Menstrual Hygiene Scheme (MHS) in 2011 to combat Indian women's lack of access to sanitary MHT (National Health Mission, 2016). This scheme was launched in select Indian districts wherein a pack of six sanitary napkins (pads) were provided to rural adolescent girls at a heavily subsidized rate. This scheme also aims to improve adolescent awareness of menstrual hygiene. To this end, this scheme aims to increase adolescent access to Accredited Social Health Activists (ASHAs), who are supposed to convene monthly meetings for adolescent girls to focus on menstrual hygiene matters.

Using Demographic Health Survey (DHS) data from

2015-2016, I show, through an instrument variable strategy, that while the policy in question worked successfully to increase access to sanitary MHT (pads) in target districts, this increase did not lead to statistically significant increases in female educational attainment. Additionally, I show that there are no significant effects of increasing menstrual technology use on female autonomy indicators. These findings go directly against the popular narrative on the effectiveness of MHT on development outcomes such as education. These results shed light on the returns from this particular investment in public health. I rationalize this lack of significant effect on education and autonomy through a variety of channels - cultural, anthropological, socio-economical - and argue that augmenting access to sanitary menstrual technology is not enough to combat these larger channels that are at play. I also find that the MHS fails to increase contact with ASHAs, thereby failing to achieve one of its stated goals.

The remainder of this paper is structured as follows: section 2 provides an overview of previous literature, section 3 discusses the dataset used, section 4 explains my estimation and identification strategy, section 5 includes the main empirical results of my paper and a discussion on these results. Section 6 concludes this paper.

LITERATURE REVIEW

This section focuses on discussing past research and literature - economic and public health - on menstrual hygiene in the developing context, with focus on research in India.

In public health and medicine, researchers acknowledge that Menstrual Hygiene Management (MHM) in developing countries, or the lack thereof, has been widely ignored until recently. In fact, researchers cite this lack of MHM in developing countries as a roadblock towards achieving numerous Millennium Development Goals laid out by the United Nations including achieving equality and parity for women, and universal primary education (Kansal, Singh, & Kumar, 2016).

The link between poor MHM and increased chances of urinary and reproductive tract infections, ectopic pregnancies,

cervical cancer and infertility is also widely acknowledged by health professionals and researchers alike (Sarkar et al., 2017; Torondol et al., 2018; Khanna et al., 2005). A qualitative study conducted by Torondol et al. (2017), in Odisha, India established a correlation between poor MHM and urinary tract infections in women aged 18-45 years. This gap in MHM quality and its consequent health effects differ significantly across rural and urban contexts too, with rural women reporting more menstrual irregularity and general problems at the 5% significance level (Ray, Mishra, Roy, & Das, 2010).

A qualitative study conducted by Kansal et al., (2016) also found not only poor utilization of sanitary napkins (the popular technology for menstrual management in India) in Varanasi, Uttar Pradesh, India, but found that a third of respondents were not aware of menarche before onset. Khanna, Goyal and Bhawsar (2005) conducted a similar study in rural context in Rajasthan, a state in India, and found similar results where most women from the sample were unaware of menstruation when they first experienced it. Clearly, besides poor access, menstrual hygiene education is also low in these contexts- indicating the motivation behind the MHS's goal of increasing menstrual education through contact with ASHAs.

As previously mentioned, India particularly struggles in providing females with access to quality menstrual hygiene products, with only 12% of menstruators having access to sanitary products; others resort to old rags, or even sawdust, as reported by the Indian Health Ministry (Waheed, 2018). This has serious consequences on female health and dignity: the Indian Health Ministry estimates 70% of women to be at risk of severe infections due to lack of access to these products (Waheed, 2018). A medical study conducted in India in 2015 indicated that women who use reusable cloth are twice as likely to develop urogenital infections as opposed to women using disposable pads (Das et al., 2015). It is clear that providing access to sanitary menstrual health products and facilities is an important facet of the female public health effort- the effects could be significant with helping women reach their full potential and handle their menstruation with dignity.

At the same time, it is not only a lack of access to sanitary menstrual products, but also the anthropology and cultural superstitions that make menstruation especially difficult in

India. In Hindu culture (the predominant religion of India), menstruation is believed to be a curse, and women are traditionally disallowed from entering the kitchen, performing religious activities, and perhaps most saliently, taking a bath during menstruation (Kaundal & Thakur, 2014). Women are considered unclean during their menstrual period and are often forced to maintain isolation in outhouses/designated rooms for the duration of the cycle (Mudey, Kesharwani, Mudey, & Goyal, 2010). These taboos that strictly limit women's autonomy during menstruation, and at times directly threaten their health, still persist today in many parts of India. Additionally, it is important to recognize that in India, access to clean water, and toilets for women is in and of itself a major public health issue (Singh, 2019).

Through a qualitative cross-sectional study, Sarkar et al., (2017) established the anthropology of periods in a rural area of West Bengal, India. Menstrual hygiene was found to be poor, unsatisfactory and unsanitary amongst the sample. Key findings also indicated that maternal knowledge on menstruation was a strong influencer in MHM in the sample. In fact, in line with Upashe et al.'s (2015) findings, Sarkar et al. (2017) suggest that educating mothers would play a significant role in improving rural MHM- indicating the persistence of poor menstrual hygiene as a cycle. Additionally, cultural superstitions and restrictiveness surrounding periods were also documented in this study: 60% of the sample restricted sour food intake, 86% restricted religious activities, 64% restricted shampooing their hair, and 6% restricted wearing washed, clean clothing during days they were menstruating (Sarkar et al., 2017).

Numerous public health studies also qualitatively link poor MHT access to worse schooling outcomes, low socio-economic status and education amongst women in the developing world (Kansal et al., 2016; Sarkar, Dobe, Dasgupta, Basu, & Shahbabu, 2017). Similar qualitative studies in other developing contexts such as Western Ethiopia have found a relationship where menstrual hygiene levels were correlated with the educational status of the mother- mothers with better education were able to pass on better MHM practices and information to daughters (Upashe, Tekelab, & Mekonnen, 2015). Moreover, an additional study conducted in Ethiopia indicated that women who do not use sanitary napkins are 5.37 times more likely to be absent

from school (Tegegne & Sisay, 2014). Similarly, Davis et al. (2018), conducted a cross sectional survey of schoolgirls aged 12-19 years in four Indonesian provinces; they found that poor MHM was associated with a minimum of one day of missed school and lower grades, significant at the 5% level.

My discussion of public health literature surrounding menstruation and MHM in the developing context establishes a general consensus on the poor state of MHM in the developing world, and its significant impact on female health outcomes. However, in economic literature, there is limited analysis on this key female hygiene issue. The most significant work in the economic context on the matter has been conducted by Oster and Thornton (2010) through a Randomized Control Trial (RCT) in Nepal. In this study, they provided menstrual cups to a random sample of schoolgirls in the seventh and eighth grades, who were required to track their menstrual cycle and conjunctive menstrual cup usage. Contrary to the results on school attendance and menstruation discussed above, Oster and Thornton (2010) found two main results: first, menstruation has close to no effect on school attendance, causing girls to miss only 0.4 out of 180 school days; second, providing improved sanitary technology does not close this small gap. Findings were robust at the 1% significance level. These results are particularly interesting since they are the main econometric analysis done on the correlation between MHM practices, and female school absenteeism, and provide results that are counterintuitive to the popular narrative told by policymakers and the media alike. However, my research differs from this econometric analysis in two notable ways: it is evaluating the effectiveness of a particular policy, and it is conducted in the Indian context. Additionally, despite Oster and Thornton's (2010) findings, the expectations between augmenting menstrual hygiene technology and seeing substantive improvements in female education are still prevalent as indicated by the extensive and growing body of qualitative literature. Thus, in conducting the following econometric analysis I would like to extend upon Oster and Thornton's work on menstrual hygiene development in order to better inform policymakers on the efficacy of such policies, so more advantageous policies can be reviewed and implemented- and the expectations from these policies can be realistic.

While there is widespread discussion citing poor MHM as a significant cause of female school absenteeism, there is limited rigorous study carried out to prove this causality quantitatively. My motivation for conducting this analysis of the Menstrua Hygiene Scheme implemented by the Indian government lies within addressing this knowledge gap, as well as the discrepancy in research findings and popular beliefs. There are numerous factors why simply increasing female access to sanitary pads will not directly have a positive impact on female school attendance. One reason includes the cultural and superstitious beliefs surrounding menstruation discussed above, which persist amongst rural and low socio-economic status individuals today. Another salient reason includes the lack of general female hygiene infrastructure in India-specifically, access to clean water, and toilets for women is, in and of itself, a major public health issue. My research question provides a novel insight on the matter; by taking pervasive alternative factors- cultural taboos and poor toilet access- into consideration, we should question if the provision of menstrual hygiene products such as disposable pads would be enough to improve female attendance rates and augment the overall female education level, and if this is an effective use of public funds, or an effective tool in improving female educational attainment. Additionally, my research aims to evaluate the effectiveness of the MHS as a policy as well.

DATA SUMMARY

The Demographic Health Survey (DHS) program, funded by the United States Agency for International Development collects key health, education and other demographic variables in developing countries. I utilise the DHS data for India for the duration of 2015-2016. Of the offered datasets, I work with the Individual Recode. The individual-level survey is a cross-sectional dataset that provides extensive microdata on over 4000 welfare indicators specifically for females. The unit of observation is the individual. This dataset includes data on menstruation, health and healthcare provision, educational attainment, wealth, caste, and religion, among others. These variables are included as controls in my analysis in order to

eliminate systematic differences in access to menstrual hygiene and focus on the effects of the exogenous policy shock: the implementation of the MHS.

The dataset required minimal transformations. Nevertheless, a number of irrelevant variables were dropped to make the dataset manageable. The MHS focuses on schoolgirls aged 10-19. Therefore, my sample was restricted to only include women aged 15-23. This is because, when the MHS was implemented in 2011, the youngest cohort in the sample, aged 15 in 2015, would be at most 11 years of age, receiving four years of treatment at the time of the survey. Individuals who were older than 19 in 2011 would be at least 23 in 2015, when the DHS survey was conducted. These respondents would not have been treated, and thus, were removed from my analysis. Since the MHS was implemented only in select states, observations from all other states were omitted. Upon dropping non-target states and ages, the sample size is 68,511 women.

Within these states, districts that had the MHS implemented are the treatment group. All other districts where the policy was not implemented (in included states) serve as the control group. I coded a binary variable `policyON` which will be used as the treatment or instrument variable: `policyON=1` for treatment districts, and `policyON=0` for control states. Table I indicates the states and their respective districts that are a part of the treatment group. Appendix A.1 includes a tabulation on `policyON`.

Table I: Treatment Districts

State	Districts	(1) Obs
Himachal Pradesh	Bilaspur, Hamirpur, Mandi, Una	
Uttarakhand	Dehradun, Pauri-Gharwal, Pithoragarh, Almora	
Uttar Pradesh	Agra, Bilaspur, Etah, Muzaffarnagar, Saharanpur	
Punjab	Moga, Ferozepur, Faridkot, Bhatinda, Muktsar	
Gujarat	Vadodra, Surat, Kheda, Bharuch	
Karnataka	Bagalkot Bidar, Belgaum, Gulbarga, Mysore, Raichur	

There are four categories of variables. The first category is the Instrumental Variable: policyON, discussed above. The second category is the independent variable- menstrual technology used (pads). The third category is outcome variables of interest: education variables, autonomy and domestic violence indicators, and ASHA indicators. The fourth category of variables are controls, which include religion, caste, wealth, sanitary conditions, and literacy. Summary statistics for these four categories are shown in Tables II and III.

Table II: Summary Statistics for Independent, Instrument, and Dependent Variables

Variable	(1)	(2)	(3)	(4)	(5)
Obs	Mean	S.D.	Min	Max	
Independent Variable					
Menstrual Tech Used (Pads)	68511	.405	.491	0	1
Instrument Variable					
policyON	68511	.164	.37	0	1
Dependent Variable: Education					
Years of Education	68511	9.167	4.253	0	20
Dependent Variables: Autonomy Indicators					
Person who decides how to spend respondents' earnings	337	.751	.433	0	1
Person who decides on respondent's health care	3753	.649	.477	0	1
Person who decides on large household purchases	3753	.594	.491	0	1
Person who decides on visits to family/relatives	3753	.628	.483	0	1
Person who decides what to do with money husband earns	3526	.632	.482	0	1
Dependent Variables: Violence Indicators					
Beating justified if wife goes out without telling husband	12038	.201	.401	0	1
Beating justified if wife neglects children	12038	.24	.427	0	1
Beating justified if wife argues with husband	12038	.234	.423	0	1
Beating justified if wife refuses sex	12038	.103	.304	0	1
Beating justified if wife cooks improperly	12038	.167	.373	0	1
Dependent Variables: ASHAs					
Met with Anganwadi worker	68511	.173	.378	0	1
Person met with: ASHA	68511	.111	.314	0	1
Services/matters talked about in last 3 months: menstrual hygiene	68511	.01	.102	0	1

Note: Responses to these variables are obtained from the Demographic Health Survey (DHS) conducted in 2015-2016. Menstrual Tech Used (Pads) is a binary variable coded such that 1 indicates the respondent uses pads to manage menstrual bleeding, *policyON* is a binary variable coded so that *policyON*=1 if the district was treated, i.e. the policy of interest was implemented in the districts, and *policyON*=0 otherwise. Years of Education is a continuous variable indicating a respondent's educational attainment. Each of the autonomy and violence indicators are coded as a binary variable to create autonomy/violence indices. For the Autonomy Indicators, when 'variable'=1 it implies some decision-making power, while 'variable'=0 implies no decision-making power. For the Violence Indicators, 'variable'=1 implies beating justified and 'variable'=0 implies beating is not justified for that particular context. The variables under the ASHA category are binary as well, where 'variable'=1 indicates that respondent answered 'yes' to that particular question.

Table II shows summary statistics for independent, dependent and instrument variables. All variables are binary except for the dependent variable for education. This implies that the mean on these binary variables can be interpreted as the proportion of the sample that belongs to a particular

group. For example, 40.5% of the sample reports using pads as their menstrual technology to control bloodstains during menstruation. Years of Education is a continuous variable indicating a respondent's educational attainment. For my sample, on average, respondents have approximately 9 years of education.

Each of the autonomy and violence indicators are coded as a binary variable to create autonomy/violence indices. For the Autonomy Indicators, when 'variable'=1 it implies some decision-making power, while 'variable'=0 implies no decision-making power: at least 60% of participants have autonomy in each of the categories studied. For the Violence Indicators, 'variable'=1 implies beating justified and 'variable'=0 implies beating is not justified for that particular context. At least 10% of respondents in my sample believes it is justified if a husband beats his wife in each of these five contexts. Note that the number of observations in Table II falls for both autonomy and violence indicator categories- this is because these questions were delivered only to a certain subset of the sample. The variables under the ASHA category are binary as well, where 'variable'=1 indicates that respondent answered 'yes' to that particular question. Only 11% of my sample met with ASHAs in the last three months. Only 1% of the overall sample talked about menstrual hygiene issues in the last 3 months, implying the treated observations for this variable are relatively low.

Table III includes descriptive statistics for select variable used as controls³. Each of these variables has been coded as a binary variable, where 'variable'=1 indicates that the respondent belongs to that variable group (i.e. respondent answered yes to the survey question). For these, means can be interpreted as the proportion of the population for which 'variable'=1. This type of dummy variable was created for all controls except controls that were either already binary, or controls that were continuous variables (such as height, weight etc., which are included in Appendix A.1). The statistics in Table III indicate that 76% of the sample is Hindu, and 23% are Scheduled Caste⁴.

³ Note that not all the variables used as controls are reported here, only ones of significant interest. For all additional control variables, refer to Appendix A.1.

⁴ "Scheduled Castes", "Scheduled Tribes" and "Other Backward Classes" are traditionally considered lower castes in Hinduism, India's dominant religion. The caste system is a pervasive social hierarchy that still exists unofficially (yet widely) in India today.

An additional 46% belong to Other Backward Classes. 15% of the sample belongs to the poorest wealth index category. Additionally, 7.5% of the sample shares a toilet with at least one other household⁵.

Table III: Summary Statistics for Select Control Variables

Variable	(1)	(2)	(3)	(4)	(5)
	Obs	Mean	S.D.	Min	Max
Hindu	68511	.764	.425	0	1
Muslim	68511	.173	.378	0	1
Christian	68511	.004	.065	0	1
Scheduled Caste	68511	.235	.424	0	1
Scheduled Tribe	68511	.048	.213	0	1
Other Backward Classes	68511	.461	.498	0	1
Poorest	68511	.15	.357	0	1
Poorer	68511	.208	.406	0	1
Middle	68511	.223	.416	0	1
Richer	68511	.214	.41	0	1
Richest	68511	.205	.404	0	1
Shared Toilet	42936	.12	.325	0	1
Higher Education	68511	.179	.383	0	1
No Education	68511	.098	.297	0	1
Primary Education	68511	.092	.289	0	1
Secondary Education	68511	.631	.482	0	1

Note: Responses to these variables are obtained from the Demographic Health Survey (DHS) conducted in 2015-2016. This table includes descriptive statistics for select variable used as controls. Each of these variables has been coded as a binary variable, where 'variable'=1 indicates that the respondent belongs to that variable group (i.e. respondent answered yes to the survey question). For these, means can be interpreted as the proportion of the population for which 'variable'=1. For the variable Shared Toilet, it is coded so that when Shared Toilet=1, it means respondents share a toilet with at least one other household, and when Shared Toilet=0, respondents have independent toilet facilities for their households. Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, which changes the interpretation of this mean, and reduces the number of observations.

ESTIMATION STRATEGY

For my identification strategy, I exploit the natural experiment that occurred as a result of the Menstrual Hygiene Scheme (MHS) implemented in India in 2011. I use an Instrument Variable (IV) technique as part of my identification strategy to assess the effectiveness of the Menstrual Hygiene Scheme, and its effect on female welfare outcomes. For my instrument, I will use policyON=1 if the district (indicated by d) has the MHS implemented, and policyON=0 if not. The independent variable is the use of menstrual technology by individual i or $Tech_Used_{i,d}$.

⁵ Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, which changes the interpretation of this mean from Table II to be 7.5%.

My research attempts to answer two primary research questions:

1. *Does the MHS effectively increase access to menstrual technology- pads- in target districts? Specifically, did the program do its job?*

The first stage regression will establish if the MHS policy successfully improved the menstrual technology used (indicated by the variable $Tech_Used_{i,d}$).

First Stage Regression:

$$Tech_Used_{i,d} = \alpha + \alpha_2(policyON_{i,d}) + \alpha_1(controls_i) + \varepsilon_{i,d} \quad (1)$$

2. *Contingent upon the existence of a first stage:*

2.1 Does increasing access to menstrual technology improve female educational outcomes?

Second Stage I: For Educational Outcomes

$$Years\ of\ Education_i = \pi_1 + \pi_2 Tech_Used_{i,d} + \pi_3 controls_i + \varepsilon_{i,d} \quad (2)$$

2.2 Does increasing access to menstrual technology positively affect female autonomy and household bargaining indicators positively?

Second Stage II⁶: Autonomy and Violence Outcomes

$$[Autonomy_Indicator]_i = \delta_1 + \delta_2 Tech_Used_{i,d} + \delta_3 controls_i + \varepsilon_{i,d} \quad (3)$$

Contingent on the existence of a first stage, I evaluate the correlation between the independent variable - increase in menstrual hygiene technology - and welfare indicators including educational attainment, and indicators of autonomy and domestic violence (detailed in Table II). The evaluation of correlation between the fitted value of the independent variable and welfare indicators is the second stage of the IV strategy⁷.

⁶ Equation (3) includes controls for education. The second stage is evaluated using the fitted value from the first stage as the independent variable. Note that for equation (3), the fitted value of $Tech_Used_{i,d}$ is calculated from a first stage that includes controls for education. The first stage for equation (2) does not include these controls for education, since for equation (2), education is the outcome variable of interest. This is done to ensure that the set of controls used in the first and second stage are the same for regression (2) and (3) respectively.

⁷ For further reference, the reader can refer to Appendix A.3 which includes regression outputs on the reduced form for the second-stage outcomes of education, autonomy, and violence indicators respectively.

Using an IV technique allows me to rid this experiment of omitted variable bias. The districts for this policy's implementation were randomly chosen. Therefore, I can plausibly assume there is no systematic difference that could be influencing the uptake/use of menstrual hygiene technology amongst target women. However, to ensure that certain endogenous individual characteristics are not affecting either the first, or second stage, I also use additional controls on socio-economic, marital, educational, and shared toilet conditions.

In addition to my primary research questions, I also look at two additional secondary research questions:

3. *A stated goal of the MHS is increasing adolescent female access to ASHAs. Does the scheme succeed in achieving this goal?*

$$[\text{Communication with ASHA_Indicator}]_i = \theta + \theta_1(\text{policyON}_{i,d}) + \theta_2(\text{controls}_i) + \varepsilon_{i,d} \quad (4)$$

4. *Does increased contact with ASHAs result in greater use or uptake of sanitary menstrual technology pads?*

$$\begin{aligned} \text{Tech_Used}_i = & \gamma + \gamma_1[\text{Communication with ASHA Indicator}]_i + \gamma_2(\text{policyON})_{i,d} + \\ & \gamma_3\{[\text{Communication with ASHA Indicator}]_i * (\text{policyON})_i\} + \gamma_4(\text{controls})_i + \varepsilon_{i,d} \end{aligned} \quad (5)$$

Question 3. evaluates the effectiveness of the MHS on another layer, and another of its stated goals. Question 4. attempts to uncover if talking to an ASHA actually improves menstrual hygiene by studying its effects on menstrual technology uptake. Note that both these regressions are evaluated through simple OLS regressions. For regression (5), an interaction term is used to refine the identification strategy and uncover the effect of talking to an ASHA when belonging to a district that is targeted by the policy. For each of these five equations, I cluster standard errors by district to ensure robust results.

ESTIMATION RESULTS AND DISCUSSION

Next, I turn to my estimation results. First, I evaluate whether my instrument works- if the policy successfully increased menstrual technology access in target districts. I did not expect the results to indicate a positive or significant

relationship on the outcome of interest in equation (1) or (4) due to reports of poor implementation. Second, upon the existence of a first stage, I evaluate research questions 2 and 3. While popular opinion expects that an investment in menstrual hygiene infrastructure will boost schooling rates for females, pervasive cultural and socio-economic factors could make this unlikely. Therefore, I do not expect a positive or significant effect of increase in menstrual technology use and education, or autonomy and violence indicators. I also evaluate research question 5, independent of the IV strategy. Here, I expect a positive relationship between menstrual technology use, and contact with an ASHAs.

5.1 Establishing a First Stage

In this section, the outcome of interest, $Tech_Used_{id}$, from equation (1), is a binary variable where a value of 1 indicates that the respondent uses pads during menstruation. Table IV indicates these first stage regression results. As indicated by the significance of coefficients in col (1) - (6), on $policyON$, the first condition of instrument variables is met: the instrument- $policyON$ - has a statistically significant relationship with the independent variable. This indicates that the MHS succeeded in its first key goal in these studied districts: increasing female adolescents' access to the sanitation technology of pads.

In addition to significant coefficients, each regression in Table IV also has an $F\text{-stat} > 10$. Col (1) is a simple linear regression, for which standard errors are not clustered by districts. Here, the coefficient on $policyON$ is significant at the 1% level. Col (2) serves as a robustness check, and clusters errors by districts, while the standard error indicated in parentheses below the coefficient increases, the coefficient is still significant at the 1% level. These results suggest that the MHS was successful in increasing access to MHT (pads). Specifically, the coefficient on $policyON$ indicates that belonging to a treatment district causes a 12 percentage point increase in probability of having access to sanitary pads.

Cols (3) - (6) show that the positive relationship between $policyON$ and $Tech_Used_{id}$ is robust to the addition of controls on religion, caste, wealth, shared toilet, and marriage, though statistical significance reduces from 1% to 10%. Coefficients

on controls provide some additional interesting observations. For example, females who identify as Scheduled Caste and Scheduled tribe are respectively 7% and 12% less likely to use sanitary MHT- significant at the 5% level. Additionally, coefficients on the wealth index controls in Cols (5) and (6) indicate that there is a positive relationship between wealth and MHT use. These findings point to the powerful effect of socioeconomic status in influencing access and use of sanitary menstrual hygiene technology.

Table IV: First Stage Regression of Type of Menstrual Technology Used (Pads) with Controls

VARIABLES	(1) Tech Used	(2) Tech Used	(3) Tech Used	(4) Tech Used	(5) Tech Used	(6) Tech Used
policyON	0.120*** (0.00504)	0.120*** (0.0265)	0.101*** (0.0212)	0.0440** (0.0189)	0.0622** (0.0247)	0.0309* (0.0165)
Religion and Caste Controls						
Hindu			-0.179 (0.178)			-0.124 (0.208)
Muslim			-0.230 (0.177)			-0.224 (0.208)
Christian			-0.0853 (0.186)			-0.119 (0.213)
Scheduled Caste			-0.0706** (0.0301)			-0.00470 (0.0301)
Scheduled Tribe			-0.119*** (0.0406)			-0.0398 (0.0435)
Other Backward Classes			-0.0558* (0.0299)			-0.0342 (0.0299)
No caste			0.0910*** (0.0309)			0.0259 (0.0304)
Wealth Index Controls						
Poorest				-		-0.101*** (0.0129)
Poorer				0.112*** (0.00840)		-
Middle				0.241*** (0.0107)		0.106*** (0.00976)
Richer				0.373*** (0.0118)		0.222*** (0.0112)
Richest				0.519*** (0.0141)		0.343*** (0.0132)
Sanitation Control						
Shared Toilet					-0.0523*** (0.0117)	0.0180* (0.00975)
Constant	0.385*** (0.00204)	0.385*** (0.0133)	0.588*** (0.182)	0.134*** (0.00789)	0.499*** (0.0128)	0.346 (0.221)
F- stat	563.90	20.48	208.12	318.33	12.82	76.03
Observations	68,511	68,511	68,511	68,511	42,936	42,936
R-squared	0.008	0.008	0.045	0.134	0.004	0.094

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The independent variable, *policyON*, is the instrument, and is a binary variable where *policyON*=1 if the district in question is one where the Menstrual Hygiene Scheme was implemented. The dependent variable- Tech Used- is also a dummy variable, indicating whether an individual is using pads as the type of menstrual technology to manage bloodstains during menstruation. Cols (2) – (6) all cluster standard errors by districts, indicating a more robust regression. Additional controls also include smaller religious groups: Buddhist, Sikh, Jain, Jewish, Parsi, and no religion- included in the regressions for col (1) and col (6); and controls on marital status in regression for col (6). Also note: '-' indicates an omitted category for wealth index controls, implying that the other four categories are to be interpreted in terms of the omitted category All control variables are dummies where a value of 1 indicates the respondent belongs to that particular group. For the variable Shared Toilet, it is coded so that when Shared Toilet=1, it means respondents share a toilet with at least one other household. Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, which changes the sample size of col (5) and col (6).

5.2 Evaluating the Second Stage: Education and Autonomy Outcomes

This section looks at the second stage estimation results for outcomes on education, autonomy, and violence. Table V displays the 2SLS regression results for the regression

specification in equation (2). This table identifies the relationship between the fitted value of use of MHT (pads) $Tech_Used_{i,d}$ from equation (1), on educational outcomes of the treated females. Col (1) is the only regression for which the effect of using menstrual technology has a statistically significant positive relationship to years of schooling. Coefficients on $Tech_Used_{i,d}$ are not statistically significant once robustness measures such as clustered standard errors and controls are implemented in cols (2) – (6), implying that an increase in access to menstrual hygiene technology does not affect educational outcomes for females. Each column in Table V adds the same set of additional controls as the respective columns in Table IV.

As in Table IV, the coefficients on the controls in Table V offer some interesting observations. Col (3) shows that belonging to Scheduled Caste or Scheduled Tribes lowers respondent schooling by approximately one year on average - significant at the 1% level. Similarly, wealth index controls in both col (4) and col (6) show the strong effect of income on schooling. This highlights the strong socio-economic barriers to respondent schooling.

Next, I move to research question 2.2: does increasing access to menstrual technology positively affect female autonomy and household bargaining indicators positively? I run the regression specification in equation (3) for the following 10 autonomy and violence indicators:

- Autonomy Indicator A: Person who decides how to spend respondents' earnings*
- Autonomy Indicator B: Person who decides on respondent's health care*
- Autonomy Indicator C: Person who decides on large household purchases*
- Autonomy Indicator D: Person who decides on visits to family/relatives*
- Autonomy Indicator E: Person who decides what to do with money husband earns*
- Violence Indicator 1: Beating justified if wife goes out without telling husband*
- Violence Indicator 2: Beating justified if wife neglects children*
- Violence Indicator 3: Beating justified if wife argues with husband*
- Violence Indicator 4: Beating justified if wife refuses sex*
- Violence Indicator 5: Beating justified if wife cooks improperly*

Table V: 2SLS Regression of Years of Education with Controls

VARIABLES	(1) Years of Education	(2) Years of Education	(3) Years of Education	(4) Years of Education	(5) Years of Education	(6) Years of Education
Menstrual Tech Used ^d	1.787*** (0.352)	1.787 (1.984)	1.854 (1.771)	-11.84 (9.369)	-2.393 (5.235)	-12.24 (9.186)
Religion and Caste Controls						
Hindu			1.230 (1.501)			1.132 (2.615)
Muslim			-1.361 (1.526)			-3.238 (3.109)
Christian			1.350 (1.570)			0.682 (2.737)
Scheduled Caste			-1.219*** (0.287)			-1.016** (0.449)
Scheduled Tribe			-1.239*** (0.377)			-0.929 (0.777)
Other Backward Classes			-0.442* (0.262)			-0.881 (0.553)
No caste			0.765** (0.305)			0.465 (0.489)
Wealth Index Controls						
Poorest				-11.38** (4.889)		-10.54** (4.136)
Poorer				-8.626** (3.832)		-7.797** (3.179)
Middle				-6.007** (2.604)		-5.363** (2.200)
Richer				-3.389** (1.378)		-2.825** (1.140)
Richest				-		-
Sanitation Control						
Shared Toilet					-1.701*** (0.292)	-0.378 (0.243)
Constant	8.443*** (0.143)	8.443*** (0.828)	7.987*** (1.775)	19.52*** (6.199)	11.32*** (2.693)	16.95** (7.117)
Observations	68,511	68,511	68,511	68,511	42,936	42,936
R-squared	0.076	0.076	0.149			

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a continuous variable that measures the years of education achieved by respondent. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Additional controls also include smaller religious groups: Buddhist, Sikh, Jain, Jewish, Parsi, and no religion- included in the regressions for col (1) and col (6); and controls on marital status in col (6). Also note: '-' indicates an omitted category for wealth index controls, implying that the other four categories are to be interpreted in terms of the omitted category. All control variables are dummies where a value of 1 indicates the respondent belongs to that particular group. For the variable Shared Toilet, it is coded so that when Shared Toilet=1, it means respondents share a toilet with at least one other household. Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, which changes the sample size of col (5) and col (6).

Table VI and VIII display the 2SLS regression results for the second-stage regression specified in equation (3) for outcomes on autonomy and violence indicators respectively. In both Table VI, and Table VII there is no statistically significant effect of increased use of menstrual hygiene technology and improvement in autonomy or violence indicators. Note that the fitted value of Tech Used - $Tech_Used_{t,d}$, stored from the first stage regression specification⁸ is used for the second stage regression estimations for both autonomy and violence indicators. These autonomy and violence indicators are

⁸This table is calculating regression results for research question 2.2. Since education is not an outcome in this equation, education controls are included in this regression. This means that the first stage for equation 3 is different from the first stage for 2, which does not include education controls since education is the outcome variable of interest. The first stage regression specification for equation 3 is included in Appendix A.2. Please note that this first stage holds, since the F-stat>10. This is done so as to ensure the integrity of the IV process, so controls used in the first and second stages are identical.

Table VI: 2SLS Regression of Autonomy Indicators A-E

VARIABLES	(1)	(2)	(3)	(4)	(5)
	Indicator A	Indicator B	Indicator C	Indicator D	Indicator E
Menstrual Tech Used	0.368 (0.767)	1.014 (1.752)	2.394 (3.829)	1.324 (2.776)	-0.460 (2.642)
Religion and Caste Controls					
Hindu	0.0194 (0.205)	-0.836 (0.881)	-1.597 (1.923)	-0.995 (1.386)	-0.136 (1.319)
Muslims	-0.0339 (0.177)	-0.890 (0.922)	-1.627 (2.011)	-1.036 (1.451)	-0.141 (1.382)
Christian	0.146 (0.227)	-0.785 (1.125)	-1.657 (2.436)	-0.886 (1.758)	0.00816 (1.670)
Scheduled Caste	-0.105 (0.176)	-0.0584 (0.110)	-0.112 (0.195)	-0.0340 (0.122)	-0.0391 (0.122)
Scheduled Tribe	-0.292 (0.304)	0.0669 (0.262)	0.201 (0.552)	0.179 (0.388)	-0.0750 (0.380)
Other Backward Classes	-0.165 (0.167)	-0.0191 (0.212)	0.0533 (0.436)	0.0265 (0.311)	-0.0906 (0.300)
No Caste	-0.128 (0.158)	-0.0445 (0.132)	-0.0397 (0.246)	0.0155 (0.168)	-0.0251 (0.168)
Wealth Index Controls					
Poorest	-0.271 (0.240)	0.337 (0.702)	0.913 (1.522)	0.470 (1.099)	-0.227 (1.039)
Poorer	-0.0999 (0.198)	0.181 (0.457)	0.540 (0.991)	0.251 (0.711)	-0.165 (0.669)
Middle	-0.225** (0.103)	0.222 (0.353)	0.501 (0.764)	0.238 (0.549)	-0.0529 (0.532)
Richer	-0.108 (0.115)	0.132 (0.239)	0.305 (0.520)	0.143 (0.376)	-0.0753 (0.376)
Richest	-	-	-	-	-
Sanitation Control					
Shared Toilet	0.0303 (0.0849)	0.0542 (0.0792)	0.00247 (0.173)	0.0588 (0.120)	0.0716 (0.119)
Education Controls					
Higher Education	-0.00192 (0.127)	-0.0451 (0.116)	-0.0919 (0.237)	-0.0700 (0.173)	0.110 (0.159)
No Education	0.0927 (0.270)	0.157 (0.318)	0.392 (0.697)	0.173 (0.504)	-0.0722 (0.501)
Primary Education	0.238* (0.131)	0.108 (0.254)	0.316 (0.564)	0.157 (0.405)	-0.105 (0.389)
Secondary Education	-	-	-	-	-
Constant	0.806 (0.708)	0.913*** (0.337)	0.735 (0.710)	0.841 (0.514)	1.100** (0.528)
Observations	337	2,306	2,306	2,306	2,179
R-squared	0.026				

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary autonomy indicator taking on the value of 1 if the individual has at least some degree of autonomy on the decision in question. Col (2) - (6) all cluster standard errors by districts, indicating a more robust regression. Additional controls also include smaller religious groups: Buddhist, Sikh, Jain, Jewish, Parsi, and no religion- included in the regressions for col (1) and col (6); and controls on marital status in regression for col (6). Also note: '-' indicates an omitted category for wealth index controls, implying that the other four categories are to be interpreted in terms of the omitted category. All control variables are dummies where a value of 1 indicates the respondent belongs to that particular group. For the variable Shared Toilet, it is coded so that when Shared Toilet=1, it means respondents share a toilet with at least one other household. Note that the number of observations is smaller than the full sample because these questions were delivered only to a smaller subset of the sample.

binary, as explained in Section 3. Each column includes controls on religion, caste, marital status, shared toilet, wealth index, and education, and clusters standard errors by district.

While Table VI shows that there is no significant relationship between augmenting menstrual hygiene use and autonomy indicators, it also leads to an interesting side note: there are close to no significant coefficients on any controls either. This leads to the surprising implication that there is no significant difference across socio-economic, marital, and educational status and women's corresponding autonomy. Since the sample size for autonomy and violence questions falls, the lack of significance across socio-economic controls is likely due to small sample size, and as such, should be interpreted with caution.

Table VII: 2SLS Regression of Violence Indicators 1-5

VARIABLES	(1) Indicator A	(2) Indicator B	(3) Indicator C	(4) Indicator D	(5) Indicator E
Menstrual Tech Used	-0.0503 (0.336)	0.311 (0.406)	0.410 (0.484)	0.600 (0.382)	0.501 (0.417)
Controls for Religions and Caste	Yes	Yes	Yes	Yes	Yes
Controls for Wealth	Yes	Yes	Yes	Yes	Yes
Controls for Marital Status	Yes	Yes	Yes	Yes	Yes
Controls for Toilet Condition	Yes	Yes	Yes	Yes	Yes
Controls for Education	Yes	Yes	Yes	Yes	Yes
Clustered Standard Errors	Yes	Yes	Yes	Yes	Yes
Constant	0.0335 (0.446)	-0.602 (0.531)	-0.765 (0.616)	-0.792 (0.497)	-0.692 (0.552)
Observations	7,669	7,669	7,669	7,669	7,669
R-squared	0.030				

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary domestic violence indicator taking on the value of 1 if the individual believes a husband beating his wife is justified in the given context. Note that the number of observations is smaller than the full sample because these questions were delivered only to a smaller subset of the sample.

Taken together, the results from Table V, Table VI, and Table VII imply that while the MHS increased access to menstrual hygiene technology in target districts, this increase did not have a consequent significant effect on female educational outcomes, or autonomy and violence indicators. These effects – particularly those on education - go directly against the popular narrative of policymakers, media, and even qualitative health studies highlighted above. These results imply that lack of access to menstrual technology is not a cause for women dropping out of school, and for low female educational outcomes. However, it is worth noting that my variable for education - years of schooling - may be too broad to detect smaller effects that an increase in menstrual technology access would have. A variable such as school absenteeism may pick up on this better - this is a limitation of my study and my data set, since I did not have school absenteeism data. Additionally, while it may increase comfort, menstrual technology does not appear to improve female’s household bargaining power through improvements in autonomy. These results on the second stage were in line with my expectations. For Table VI, and Table VII, I have only included my most robust regressions with all controls, and only clustered standard errors. If the reader would like to see a similar addition and variation of controls as in Table V, please refer to the tables in Appendix C.

5.3 Secondary Research Questions

This section looks at estimation results for research questions 3 and 4. Table VIII looks at the OLS regression specification from equation (3), with clustered standard errors, and socio-economic, toilet sanitation, marital and educational controls added for robustness. The independent variable is *policyON*, while the outcomes of interest are also binary variables that take on the value of 1 if the respondent answered ‘yes’ to the respective questions on contact with ASHAs. As is indicated by the coefficient on *policyON*, there was no significant difference between females who lived in treatment districts, and those that lived in control. As discussed in the literature reviewed, misinformation and lack of knowledge on menstrual health is rampant in India. The MHS aimed to rectify this through its target procedure of increased contact with ASHAs. Table VIII shows that the MHS was unable to meet this target. This result is important from a policy evaluation perspective.

Table VIII: OLS Regressions of Indicators of Communication with ASHAs

VARIABLES	(1) Met with Anganwadi Worker	(2) Met with an ASHA	(3) Discussed Menstrual Hygiene
<i>policyON</i>	0.0141 (0.0147)	0.00653 (0.00919)	0.00153 (0.00485)
Controls for Religions and Caste	Yes	Yes	Yes
Controls for Wealth	Yes	Yes	Yes
Controls for Marital Status	Yes	Yes	Yes
Controls for Toilet Condition	Yes	Yes	Yes
Controls for Education	Yes	Yes	Yes
Clustered Standard Errors	Yes	Yes	Yes
Constant	0.0915 (0.0793)	0.0355 (0.0561)	-0.0197*** (0.00648)
Observations	42,936	42,936	42,936
R-squared	0.130	0.087	0.008

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable, *policyON*, is a binary variable where *policyON*=1 if the district in question is one where the Menstrual Hygiene Scheme was implemented. In col (1) and col (2) the dependent variable (also binary) is whether the respondent met with an Anganwadi Worker- a type of social worker, or an ASHA (Accredited Social Health Activist) in the last three months. In col (3) the dependent variable is measuring if the respondent discussed menstrual hygiene with the social worker they met with. All standard Errors are clustered by districts. Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, therefore controlling for toilet condition reduces the sample size from 68,511 to the 42,936 in this table.

This leads me to my fourth and last research question: does increased contact with ASHAs lead to higher use of menstrual hygiene products? This question aims to evaluate if contact with ASHAs is an effective intervention to improve

adolescent knowledge of menstrual hygiene. Here, use of menstrual hygiene products serves as a crude indicator on better knowledge of menstrual hygiene management. The rationale is that if females are uninformed about menstrual hygiene, this may affect their uptake and use of sanitary menstrual hygiene technology. The output for the regression specification in equation (5) is shown in Table IX.

Table IX: OLS Regression of MHT Used as Affected by Contact with ASHAs

VARIABLES	(1) Tech Used	(2) Tech Used	(3) Tech Used	(4) Tech Used	(5) Tech Used	(6) Tech Used
policyON	0.124*** (0.0263)	0.0404*** (0.0151)	0.120*** (0.0257)	0.0405*** (0.0153)	0.120*** (0.0263)	0.0385** (0.0161)
Met with Anganwadi Worker	-0.0472*** (0.00969)	-0.0180 (0.0112)				
Interaction1	-0.0167 (0.0283)	-0.0125 (0.0294)				
Met with an ASHA			-0.0673*** (0.0102)	-0.00373 (0.0121)		
Interaction2			-0.00527 (0.0323)	-0.0251 (0.0329)		
Discussed Menstrual Hygiene					0.238*** (0.0450)	0.187*** (0.0430)
Interactions3					-0.0951 (0.113)	-0.0700 (0.116)
Controls for Religions and Caste	No	Yes	No	Yes	No	Yes
Controls for Wealth	No	Yes	No	Yes	No	Yes
Controls for Marital Status	No	Yes	No	Yes	No	Yes
Controls for Toilet Condition	No	Yes	No	Yes	No	Yes
Controls for Education	No	Yes	No	Yes	No	Yes
Clustered Standard Errors	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.393*** (0.0137)	0.231 (0.225)	0.393*** (0.0134)	0.229 (0.224)	0.383*** (0.0132)	0.233 (0.224)
Observations	68,511	42,936	68,511	42,936	68,511	42,936
R-squared	0.010	0.110	0.010	0.110	0.010	0.111

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. The independent variables are: binary variable where policyON=1 if the district in question is one where the Menstrual Hygiene Scheme was implemented. and; Col (1) and (2): whether the respondent met with an Anganwadi Worker- a type of social worker; Col (3) and (4) whether the respondent met with an ASHA (Accredited Social Health Activist) in the last three months; Col (5) and (6) if the respondent discussed menstrual hygiene with the social worker they met with; Interaction 1: policyON with "Met with and Anganwadi Worker". Interaction 2: policyON with "Met with ASHA". Interaction 3: policyON with "Discussed Menstrual Hygiene". Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, therefore controlling for toilet condition reduces the sample size from 68,511 to the 42,936 in this table.

In Table IX, the outcome of interest is use of MHT (pads). The independent variable in each column is *policyON* - the binary instrument. For the regressions in Col (1) and (2), "Met with Anganwadi Worker", and an interaction term for "Met with Anganwadi Worker" and *policyON* are also independent variables (with varying controls for robustness). Regressions in Col (3) and (4) include the independent variable "Met with ASHA" and its interaction with *policyON*. Similarly, in Col (5) and (6) "Discussed Menstrual Hygiene" and its interaction with *policyON* are the independent variables.

The lack of significance on coefficient on each of the three interaction terms in Table IX indicates that there was no difference in contact with ASHAs based on belonging to a treatment district or not. Additionally, the coefficients on “Discussed Menstrual Hygiene” in Col (5) and (6) indicate a highly statistically significant and positive relationship between discussing menstrual hygiene with a social worker and using sanitary MHT- so there are some benefits to contact with ASHAs as indicated through this very specific measure on contact. However, the most important finding from this table is that the overall effectiveness of this policy is not impacted significantly by contact with ASHAs. That is, the uptake and use of MHT is not affected by contact with ASHAs. Therefore, even though the results in Table VIII imply that the policy did not achieve its goal of increasing contact with ASHAs as measured across these three measures, the results in Table IX suggest this does not make a significant difference at least to the primary policy goal of increasing sanitary MHT use and access.

CONCLUSION

This paper presents three main findings. The first finding is regarding policy effectiveness: the Menstrual Hygiene Scheme was successful in its primary goal of increasing menstrual hygiene technology access and use in target districts. However, the policy goal of increasing contact with ASHAs as a means to improve menstrual health education amongst adolescent females was not achieved. The second finding shows that increased access to menstrual hygiene technology has no significant effect on female education or autonomy indicators. This finding has consequences for policymakers since it shows that this policy does not target the development outcome- education- that is commonly assumed it would. The third finding indicates that the effectiveness of this policy is not impacted by contact with ASHAs. However, discussing menstrual hygiene with a social worker has a significant positive effect on uptake, therefore the value of this channel of menstrual hygiene education cannot be entirely negated.

I would like to specify that this paper is not arguing against the provision of menstrual hygiene products to

adolescents- as mentioned in previous sections, the positive health and comfort effects of sanitary menstrual technology are inarguable. Indeed, if the results in this paper are to be followed, the MHS's success implies it could be extended to additional districts to improve MHT access. However, this paper quantitatively evaluates the popular claim that improved menstrual technology improves female educational attainment. The results suggest this mechanism is not quite so direct, and therefore using this as a justification for investing public funds would be inaccurate. As shown through the powerful effects of socio-economic controls on educational outcomes, these channels exert a much more prominent effect on educational attainment than access to menstrual hygiene technology. Additionally, pervasive cultural norms and taboos surrounding menstruation would also play a significant role in dictating how women handle their menstruation. If women are not permitted to leave their homes during menstruation because they are assumed to be impure, providing pads will not change that. Culture would be another mechanism explaining why providing pads does not increase educational attainment or improve autonomy indicators regarding freedom of movement.

Of course, my measure of education- years of school- could be too broad to pick up on smaller effects on attendance. To mitigate this limitation of my research and data, future research could look at 'days of school attended' as an outcome. Similarly, I had limited measures of autonomy available in my data. An autonomy indicator such as: 'can you leave the house while menstruating', could detect more specific effects that providing MHT has on autonomy. Similarly, for the section evaluating if contact with ASHAs improves menstrual hygiene knowledge, instead of use of technology as an outcome, a more targeted outcome which judges if this intervention changes cultural perceptions would be valuable. Such future research could help shed light on effective measures to change limiting cultural taboos surrounding menstruation. These results also focus on the very specific geographic context of India- my results may not be applicable beyond the Indian, or South Asian context.

Overall, the findings of this paper highlight the importance in evaluating such policies using econometric analyses in order to gain insight on their efficacy and benefit in society, a finding that can be valuable for policy makers and researchers.

Does Membership Come With Benefits? The Effect of ETFs on Firm Value

Raphaël Grach

ECON 499

ABSTRACT

In recent years, passive investment instruments such as Exchange-Traded Funds (ETFs) have been met with growing enthusiasm from both academic and finance-industry professionals. This recent global popularity comes from their relative accessibility and their low trading costs. This paper studies the potential effects of ETF ownership on the value of the firms that are being tracked. It aims at complementing the larger literature studying the effects of ETFs on markets. Several measures of firm value are used, and following an exploration using regression discontinuity, the results suggest no significant effect of ETF ownership on firm value indicators.

Keywords: Russell, ETFs, passive investing, institutional investors, firm value.

INTRODUCTION

Exchange-traded funds (ETFs) have gained a global interest among both investors and academics in recent years. They represent sound investment opportunities as they ease the access to financial values and techniques that cannot be traded by traditional retail investors. They are financial instruments aimed at replicating the performance of an underlying entity, usually, ETFs mimic a basket of securities, but in theory really any type of asset could be followed by an ETF¹. Their benefits include following the performance of previously non-tradable indexes such as the entire S&P 500 index. In the past, an investor willing to track the index would need to compose a portfolio of the index components, implying significant trading costs. With instruments such as ETFs, it is possible to replicate the performance of the whole index by buying a single share of the SPDR² S&P 500 ETF Trust (Ticker: SPY). They also grant access to non-conventional trading methods, such as short-selling and leverage, to individual and non-professional investors. Unlike traditional index funds, ETFs are traded daily on exchanges, allowing for greater liquidity and are better suited for higher frequency trading.

ETFs can help realize significant benefits to investors, even in market selloffs. In the financial downturn following the 2020 COVID-19 pandemic, with Wall Street recording its worst trading day since the 2008 financial crisis, the ProShares VIX Short-Term Futures ETF (Ticker: VIXY) following the performance of the Chicago Board Options Exchange's Volatility Index (VIX) recorded a one-month performance of about +128.74% over March only³. ETFs' total assets under management (AUM) skyrocketed in the 2010s to reach almost \$6.2 trillion in 2019, which is almost twice the total AUM of hedge funds standing at \$3.2 trillion in the same year⁴. Figure 1 retraces the evolution of the global AUM of both asset classes over time.

¹ For more institutional details on ETFs, see Ben-David et al. (2017)

² SPDR stands for Standard & Poor's Depositary Receipts

³ Source: MarketWatch, as of 03/29/2020.

⁴ Source: Statista and BarclayHedge.

With rising uncertainty and limited time to react, ETFs may appear as a sound investment, especially for investors that lack time to perform thorough stock analysis on a market they do not understand well. Overall, trackers appear to be a significant financial innovation bringing substantial liquidity and accessibility to individuals and professional traders. However, with all these advantages, one may wonder if ETFs have any unwanted side effects, especially after looking at their mechanism. Since ETFs continuously attract large numbers of investors that trade at different frequencies, it is worth questioning if they are at risk of creating potential distortions⁵.

ETFs can track the performance of an underlying basket of assets through an arbitrage mechanism. Figure 2 comes from Lettau et al. (2018) and sums up the ETF environment and how it differs from mutual funds. ETF managers tie legal links with "Authorized Participants" (APs), which will benefit from the first issuance of ETF shares. They can later trade these on financial markets in exchange for a basket of underlying securities. APs are usually large financial institutions which act as middlemen between the ETF manager and financial markets. This means the manager does not directly interact with the market. The primary role of APs is to arbitrage any difference that could exist between ETF shares and the underlying basket assets. For instance, if ETF shares are deemed to trade at a premium against the basket of securities, the AP can retrieve ETF shares from the ETF manager in exchange for a basket of the tracked securities, and then sell these newly emitted ETF shares on the market. The AP can then decide to keep or sell the newly acquired basket of stocks (Lettau et al. (2018)). A distinguishing feature of ETFs is their arbitrage mechanism. Unlike other types of institutional investing where trading decisions remain at the manager's discretion, the impact of ETFs on the underlying securities risks being more critical due to its arbitrage channel (Ben-David et al. (2018)).

Some notable investors, such as Michael J. Burry, who predicted the 2008 crisis, see a new threat in passive investing, of which ETFs could be a significant part. Burry argues that there is a disconnect between the volumes at which indexed

⁵*Just like other forms of institutional investing can exert pressure on equity markets (for further discussion, see Coval and Stafford (2007), Vayanos and Woolley (2013)).*

securities trade and the amount tied to them through passive investing, and this could lead to the next financial crisis. In the case of a massive selloff, there is a risk that not enough shares are outstanding to absorb the shock⁶. This risk becomes greater when you consider ETFs, which remain particularly easy to sell, just like collateralized-debt-obligations (CDOs) were before the 2008 crisis. Dr. Burry's predictions resonate, particularly in the current (April 2020) global financial turmoil.

Following these issues, this paper will try to answer the question of the effect of ETF ownership on the value of underlying firms. Several measures of firm value will be used, such as cyclically adjusted price-to-earnings ratio, also called Shiller P/E (Campbell and Shiller (1988)), the price-to-earnings ratio (P/E) and price-to-book ratio (P/B). More details on these variables will be given in section V.

Conclusions from past literature on ETFs vary on whether they have a beneficial or adverse effect (see discussion in section II). However, one recurring theme is their potential risk of creating distortions. Indeed, Morck and Yang (2001) find a significant Q-ratio⁷ premium for firms indexed to the S&P 500. This index is tracked by the largest outstanding ETF, the SPDR, with \$178 billion in assets as of September 2017 (Lettau et al. 2018). The paper will then aim at determining if this result applies to a broader asset class by studying stocks in the Russell 3000 that covers almost 98% of U.S. equities and the extent to which ETFs participate in it. The hypothesis we want to verify is that ETFs might cause some form of valuation premium. It is important to note that this paper will only focus on ETFs replicating index performance and will not mention ETFs that track strategies from actively traded funds. Investing decisions in "passive", index-linked ETFs are independent of stock analysis since they are only based on a rule of thumb (i.e., whether a security is in a particular index and the weight it has). It can be interpreted as a risk of potential distortions.

⁶ His exact words are: "[the] theatre keeps getting more crowded, but the exit door is the same as it always was" (Kim and Cho 2019).

⁷ Tobin's Q is a measure of firm value, taking the ratio of the firm's market value to its replacement costs. For more details, see Tobin (1969) and Hayashi (1982). Tobin's Q was initially the choice of measure of firm value, however, due to a lack of data from the available database, not enough stocks could be matched to their respective Q value.

The paper will have the following structure. Section II considers the global literature on ETFs and frames this study within the wider context. Section III exhibits the economic reasoning on which the hypothesis is founded. Section IV describes the data used to perform the statistical analysis. Section V outlines the estimation strategy. Results from the statistical analysis are presented in section VI and discussed in section VII. Section VIII concludes.

LITERATURE REVIEW

Academic research on ETFs is relatively recent as these instruments have only started gaining significant interest since the 2008 financial crisis. Economists are still in the early stages of understanding their effects. This study relates to the broader academic research on the effects of indexing. For more detailed inquiries, we could look at Wurgler (2010) who studies the effect of index-investing on stock prices, with the notable conclusion that a stock included in the S&P 500 has a tendency to “detach” from the rest of the market, demonstrating higher co-movement with the rest of its 499 partners than with the rest of the market. The “detachment” identified by Wurgler (2010) builds on the works of Morck and Yang (2001) that studied the growth in value of members of the S&P 500, mostly attributable to index membership. Chang, Hong, and Liskovich (2015) set up a significant regression discontinuity (RD) design using the yearly Russell 3000 reconstruction to study the effect of index inclusion on prices. They identify price increases linked with Russell 2000 inclusions and declines linked with removal from the index. They proceeded by matching each stock in the index with another one as close as possible outside the S&P 500 and comparing their market valuation. Between 1978 and 1997, they identify a value premium of indexed firms close to 50%⁸. Such a critical premium suggests that market participants, notably managers, are probably aware of these effects. However, since the Russell 2000 inclusion rule relies mostly on market capitalization ranking (the 1000 largest ones

⁸ *Although their data was updated after the release of the paper, this is what is available from their 2001 study.*

are part of the Russell 1000 and the following 2000 are in the Russell 2000), arbitrageurs have no interest in anticipating index inclusions if their trades have a price impact. If a stock is about to be included in the Russell 2000 (i.e., passes above the 1000th largest market cap), hedge funds would have no interest to buy it ahead of time since they risk raising its share price, and thus its market cap, excluding it from the Russell 2000. The converse is also true for stocks that are about to get excluded from the index. Arbitrageurs trying to short this stock risk creating a negative price impact potentially reducing its market cap and keeping it in the index.

Studies of passive investing and ETFs have mostly treated very technical aspects of their effects. It has been shown that there is a positive correlation between ETF ownership and stock liquidity (see Boehmer and Boehmer (2003); Hamm (2011)). Ben-David et al. (2018) find that demand shocks in ETF shares can transfer to the underlying securities through the arbitrage conducted between the APs and the manager, increasing stock volatility. Agarwal et al. (2018) find similar results about co-movements in liquidity. Other studies found that ETF ownership was positively associated with higher co-movement in returns (see Da and Shive (2013)), limiting the efficiency of portfolio diversification as a risk-minimizing strategy. Meanwhile, findings from Israeli, Lee, and Sridharan (2015) are twofold. First, increased ETF ownership is associated with higher trading costs for the underlying firm. They explain this phenomenon by referring to uninformed traders leaving the market and preferring to turn to an ETF to follow their values of interest. Second, they find that ETF ownership reduces the benefits of acquiring information, leading to lower overall price efficiency. It materializes notably through a drop in analysts covering the security or decreased earnings from futures trading.

This paper also adds to the literature by studying the effects of forms of passive investing beyond market value (see Black (1992), Romano (1993) and Del Guercio and Hawkins (1999)). Notably, Appel et al. (2017) suggest that passively managed funds do affect firm governance, associated with greater director independence, increased equality in voting rights, and reduced takeover defense. In an additional, unreported regression, they do find a positive relationship

with Tobin's Q when controlling for index switching. The contribution of this paper is its specific investigation of ETFs' effects and its primary focus on firm value, rather than using it as additional evidence to support a broader hypothesis.

This paper will specifically focus on the effect of ETFs, using a less volatility-sensitive measure of firm value, bringing an alternative view on the matter. This study will expand upon the existing work by testing two different identification strategies on a new dependent variable.

ECONOMIC THEORY

ETFs are investment vehicles that aim to replicate the performance of an underlying basket of assets. This paper seeks to explore the potential effect they can have on the value of a tracked firm. The work of Ben-David et al. (2018) explores their effect on stock volatility through what they call the "arbitrage channel". Indeed, the performance replication of ETFs is maintained through an arbitrage mechanism. The AP is in direct contact with equity markets and benefits from its privileged link with the ETF fund manager to get ETF shares on the primary market. If the AP believes ETF shares trade at a premium against the basket of underlying securities, he or she can buy a basket of stocks and sell it to the ETF manager in exchange for newly emitted ETF shares. The AP is then free to sell these shares or to keep them. Following demand or liquidity shocks, we observe a propagation of these shocks to ETF shares that are then transmitted to the net asset value (NAV) of the fund. This transmission is what causes volatility. Eventually, ETF shares and NAV may come back to their initial value.

Considering the findings of Ben-David et al. (2018) using this arbitrage channel to highlight increased volatility, and the works of Morck and Yang (2001) and Wurgler (2010) exposing growing premia linked to indexing and index investing, this paper aims at narrowing the study to a specific case of passive investing (ETFs) and a specific measure (firm value), as measured by cyclically adjusted price-to-earnings ratio (CAPE). As the use of ETFs have become increasingly widespread, their coverage of principal U.S indexes has also increased significantly

over time. The share of common stock owned by ETFs (called ETF ownership) was almost multiplied by 50 for stocks in the S&P 500 and Russell 3000 universes (Ben-David et al. 2018). Figure 3 displays the evolution of average ownership over time for different types of institutional investors for the S&P 500 and the Russell 3000.

The main hypothesis can be seen in two ways. The first is that a positive relationship between ETF ownership and firm value would be observed. This would be coherent with previous findings and would have implications both at the market and at the firm level. The fund manager could anticipate these valuation premia and make more educated decisions, while the firm manager could consider the value of his or her firm is pushed by ownership of ETFs. This first hypothesis represents a departure from the canonical model established by Modigliani and Miller (1958) stipulating that under certain conditions, firm value is independent of capital structure. These conditions are notably competitive markets and perfect information, along with an absence of additional costs linked to agency or bankruptcy. However, the intuition behind the first hypothesis relies on imperfect information. Indeed, uninformed traders represent a critical pool of potential ETF clients as these instruments are specifically suited for those who are not willing to engage in traditional stock analysis. Since ETFs ease the access to capital markets to traders that would have not otherwise entered it, they may create situations where at least some agents operate with imperfect information, generating potential distortions. Since they provide easy access with guaranteed benchmark returns, ETFs could even raise the cost of acquiring information and create more uninformed traders. This conclusion would be consistent with the results of Israeli, Lee, and Sridharan (2015) since they conclude that ETF ownership is associated with lower price efficiency for this specific reason. Lastly, ETFs represent a particular form of ownership, since it depends on a triple relationship (involving the ETF manager, AP, and capital markets) and not only on the standard shareholder-stock relationship that was commonplace before their creation. Their influence on capital structure thus also depends on their particular architecture. As evidence of potential distortion, Figure 4 retraces the stock price of Navistar International

once it switches indexes from the Russell 1000 to the Russell 2000. Being now tracked at higher weights by Russell 2000 ETFs, we can observe the trajectory of its stock price and tie it to the overall ETF share value. One can observe that the days following the yearly ranking comes with a great deal of volatility, as suggested in Ben-David et al. (2019).

The second interpretation of the hypothesis is that ETFs may also not affect firm value. This could be explained by market participants considering the role of APs and acting according to their anticipations. This hypothesis would conform to the proposition that firm value is independent of its financing. However, seeing ETFs cause a significant discount of firm value would be rather surprising. This would mean that ETF ownership would be perceived by market participants as a depreciating factor. On the other hand, this could also mean that more complex forms of ETFs, such as inverse ETFs or “short ETFs”, may have a larger impact than previously expected.

DATA

Section IV retraces the whole composition of the dataset, from data sources to variable definitions. It is critical to note that data collection for this study was extremely challenging and that the final dataset is far from being optimal, taking into account missing data, unreliable information and unmatched datasets, even within the WRDS portal. Most of the literature using comparable data obtained specific authorization from data owners and had access to otherwise private information⁹. Data that was unavailable through Compustat or CRSP, like the Russell 3000 index composition, was queried via a Bloomberg terminal. Additionally, the events marking the beginning of the year 2020 made fixing missing data almost impossible due to the closing of most services¹⁰. Each

⁹ A detail that the author recognizes he was not aware of before starting data collection. Considering the time constraint of this task, he decided to pursue the study with this suboptimal dataset.

¹⁰ The global COVID-19 pandemic led to several countries shutting down all economic activity. The WRDS database remained fully available but functioning remotely; however, in the author's current location (France), all services and firms that are not of prime necessity were closed. Since the UBC Bloomberg terminal was not accessible anymore, and the only terminal that the author had access to is in a closed office, no Bloomberg data could be accessed after March 16th, 2020.

subsection identifies potential areas of concern, including missing or incomplete data. Table I displays the summary statistics of the relevant variables.

A. Data sources

The data is retrieved from the Center for Research in Security Prices (CRSP), Compustat, and Bloomberg. CRSP and Compustat data panels were queried through the Wharton Research Data Services (WRDS) portal. These data sources publish accessible data regularly, which allows for precision in this paper's query. The index reconstruction takes place at the end of June with May market capitalizations, making the multiple data sources compatible with each other for the query. Since FTSE Russell does not disclose market capitalizations at the end of May, the paper builds a proxy based on data from CRSP and Compustat that has a high success rate in predicting index allocation (see subsection C for the detailed construction). Figure 5 displays index allocation using the ranking proxy.

Data concerning ETF holdings are drawn from the CRSP Mutual Fund Holdings panel. This only covers data after 2003. The Russell 3000 composition on the last trading day of May and June is the one used in Ben-David et al. (2018) and was cross-checked via Bloomberg.

B. Coverage and variation

The paper focuses its analysis on U.S. listed companies from 2003 to 2006¹¹. The scope of the study had to be shortened to preserve the pertinence of the results, most notably concerning matching problems between the Bloomberg data and the CRSP/Compustat-generated sample after 2007. Additionally, pre-2007 data is less constrained since index allocation follows a more straightforward rule

¹¹ *Methodology for post-2007 data is commented in Appendix 2, though no regression could be run. The proper instrumental variable approach could not be realized because of missing data, notably the cumulative market cap of the Russell 3000E. This piece of data would only have been accessible via a Bloomberg terminal (that is no more available).*

based solely on market cap ranking¹² (see section V for a full discussion). More complex forms of ETFs, such as "leveraged" or "inverse" ETFs, including those involving the replication of active management methods (so-called "smart beta" ETFs), are excluded since the objective is to study the effect of passive forms of ETFs. Omitting those components does not harm the dataset significantly since these active forms of ETFs only represented 2% of all ETFs (Blackrock, 2014). This paper then restricts itself to only studying ETFs that replicate the performance of an underlying basket. As their functioning is based upon an arbitrage mechanism, their adjustment is quasi-instantaneous and thus relevant for our study. Since the dependent variable is firm value, firms that had missing information in the determinant variables were dropped since they did not produce output.

According to FTSE Russell (2019), two dates are effectively relevant for the construction of the dataset. The first one is the day where all securities are ranked by their total market capitalization, which is referred to as the "ranking date." This is also the date where each firm is assigned to an index. Prior to 2007, index assignment was based solely on market capitalization ranking, including the top 1000 market caps in the Russell 1000 and the bottom 2000 in the Russell 2000. The second key date is the day reconstruction effectively takes place. The "ranking date" was set to be the last trading day of May up to 2015¹³, which does not affect the sample since it stops in 2006. The reconstruction date was set to be the final business day of June until 2004, where it became the last Friday of June. This change does not affect the final dataset since the mutual funds records only matched the firms for the years 2005 and 2006. Index membership is determined on the "ranking date" in May, however, it only becomes effective after the reconstitution date in June.

¹² Up to 2006, stocks were assigned to the Russell 1000 to 2000 following a simple market capitalization ranking rule. The largest 1000 firms in terms of the market cap were assigned to the Russell 1000, whereas the bottom 2000 were assigned to the Russell 2000. FTSE Russell changed its methodology and added new assignment rules after 2006 to provide excessive switching between the two indexes (FTSE Russell 2019). From 2007 onwards, two new rules were added: whether the stock was in the index the previous year and whether it falls within a certain distance of the cutoff (equal to 2.5% of the cumulative market cap of the Russell 3000E).

¹³ After that date, it was changed to be mid-May.

The data is directly collected from publicly available information, namely through the University of British Columbia's subscription to data providers like CRSP, Compustat, and Bloomberg. Each observation in this study is a company with variables constructed from its daily or monthly financial data. The sample of selected firms remained identical for the last trading day of May and the last trading day of June. The variables of interest are ETF ownership, firm value, and market capitalization. Each of them will be computed for the last trading day of May. However, the market cap will be computed in April, May, and June to construct the lagged and forward market cap after the ranking takes place in May.

For consistency and simplicity, modifications of the Russell 3000 index occurring during the year are excluded since they are exceptional. These exceptions encompass new firms being incorporated following their initial public offering (IPO) or firms being removed from the index following bankruptcy or acquisition by another company. In the case of an IPO, historical data about the company would be lacking since the company just went public, and variables of interest (say, the Shiller P/E) may not be computable. In the case of a firm exiting the index, the company has two options: It can disappear from the public market and lose its relevance to this study, or it can have its components redirected towards another company, meaning the company's data remains in the dataset in another form.

C. Dataset construction

The dataset was constructed using external sources for the composition of the Russell 3000 index. From there, every piece of information queried on those components was obtained in the WRDS environment using their integrated SAS Studio. The pre-2007 index composition is retrieved from Ben-David et al. (2018) and was cross-checked with Bloomberg data. Each firm had its ticker (short market identifier) noted and carefully mapped to its respective CRSP and Compustat identifier, its PERMNO (permanent number), and GVKEY (company key). For identification purposes, its CUSIP (unique firm ID) was also mapped. Dataset construction and design closely follows Ben-David et al. (2019).

In Section 5 of their methodology, FTSE Russell (2019) outlines precise criteria for index inclusion¹⁴. They exclude preferred stocks (including convertible and participating preferred stocks), redeemable shares, rights, warrants, depositary, installment, and trust receipts. All these criteria are included in the market capitalization ranking proxy used in this paper. What is included is presented in section 6: all standard share classes that are non-restricted, partnership units and membership interests are also included¹⁵.

To construct the market capitalization proxy, this paper relies heavily on CRSP's Daily Stock Files for information on stock price and the number of shares. The Compustat Securities Daily database is used as an additional resource to query over the counter (OTC) data (like non publicly traded stocks) should data from CRSP be unavailable. Then, if a company has one or more common shares that are not publicly traded, the Compustat Quarterly database is used to query aggregated shares outstanding at the quarter-end (CSHOQ) and multiply it by a weighted average of publicly traded shares¹⁶. The CSHOQ variable represents all common shares outstanding in both Forms 10-Q and 10-K¹⁷.

After that, the market capitalization of nonpublic shares is added to the ones that are publicly traded to form the second market capitalization proxy using Compustat. The paper chooses the CRSP-based market capitalization as a base measure and uses the Compustat-generated market cap if the CRSP value is missing or lower than the Compustat one. This ranking method, used in Ben-David et al. (2019), had a success rate of 99.7% in attributing stocks to the Russell 2000 and Russell 1000.

¹⁴ *Notably a minimum price of \$1, minimum voting requirements, \$30 million market cap, and 5% floating share minimum.*

¹⁵ *Index inclusions by IPOs were added in 2005 on a quarterly basis.*

¹⁶ *Using this approach does not alter ranking outcomes in any significant way (Ben-David et al. (2019))*

¹⁷ *10-Q and 10-K Forms are financial performance reports mandated by the U.S. Securities and Exchange Commission (SEC) for all publicly traded companies. The 10-Q Form is a quarterly report, and the 10-K Form is an annual report.*

D. Main variables of interest

i. ETF ownership

The paper attempts to measure ETF ownership by computing the total number of shares held by ETFs of a certain stock. Considering that ETF AUM was not reported by WRDS, multiplying the weight of each stock in the ETF portfolio by its total AUM was not an option. In this study, the proxy used was to multiply the total amount of shares held by ETFs at a certain date, by the share price at that date. Scaling by the market capitalization of that firm is then done to smooth the variable. It, of course, created availability issues since the holdings data were not available for every date. For pre-2007 data, only the years 2005 and 2006 had relevant data for the list of Russell 3000 components.

It is possible to sum up the value of ETF ownership by the following equation:

$$Ownership_{i,t} = \frac{\sum_{j=1}^J H_{i,j,t} \times P_{i,t}}{(Mkt\ cap)_{i,t}}$$

Where the value $H_{i,j,t}$ accounts for holdings of ETF j for month t of the stock of firm i . For instance, if four different ETFs hold respectively 200, 150, 300, and 250 shares of a stock at a certain date, then the value of H would be 900. For the purpose of this study, ownership by ETFs was split into two categories: the dollar value of ETF ownership, sometimes referred to as the "cumulative" value of stock held by ETFs, and the smoothed value, scaled by the market cap. The cumulative value of ETF ownership corresponds simply to $\sum_{j=1}^J H_{i,j,t} \times P_{i,t}$. The two variables were used separately in different regressions. Table I provides details for the summary statistics of all relevant variables, including these two.

b. Firm value

The various measures of firm value used in this paper were chosen because of their direct relationship to firm performance and intrinsic valuation rather than picking up market movements. Among those is the cyclically adjusted price-to-earnings ratio (CAPE), also known as the Shiller P/E ratio (see Campbell and Shiller (1988)). The CAPE is closely related to the price-to-earnings ratio (P/E), which will also be used as a measure of firm value, as well as the price-

to-book ratio (P/B). These are all slightly different measures which contribute to identifying the effect and locality of ETF ownership. As reported on WRDS, the data for these multiples were not available for every firm and thus forcibly reduced the stock universe.

ii. Standard price-to-earnings ratio

A price-to-earnings (P/E) ratio is one of the most common measures of firm values. It is called a "value multiple" in the financial industry since it takes the ratio of the firm's market price to its earnings per share. For a given firm, it is defined as:

$$PE_i = \frac{(\text{Market Share Price})_i}{(\text{EPS})_i}$$

where EPS denotes the earnings per share of the firm. Most of the time, the P/E ratio of a firm is checked against a comparable group of firms. A relatively higher P/E ratio may lead the firm to be considered overvalued or show that market participants are expecting strong performance in the future¹⁸. Though widely used, it also has limitations. If a company reports losses, the ratio will become a negative number and lose its significance as a measure of firm value. If the company reports zero earnings, then the ratio would simply be unavailable. Since it is a direct measure computable for every trading day, it can be volatile due to intra-day adjustments to new information. This paper uses a version of P/E ratio, excluding extraordinary items.

Shiller P/E

The cyclically adjusted price-to-earnings ratio (CAPE), also called Shiller P/E, is very close to the standard measure of the P/E ratio but the denominator is different. Rather than use EPS as the denominator, CAPE uses the inflation-adjusted earnings from the previous five or ten years (moving average). This paper will use the five-year moving average as

¹⁸ For instance, Amazon usually displays a P/E ratio way above most of its counterparts indexed to the NASDAQ (82.64 against 21.48 as of 03/31/2020), however, it does not necessarily mean that the firm is overvalued considering the remarkable performance of Amazon compared to numerous other internet firms (Source: MarketWatch and YCharts).

its denominator since it is the only available option (WRDS (2016)). It has been argued that smoothing the earnings of the firm over a longer period is a better way to assess a firm's true value, rather than a one-year earnings ratio which is too volatile to convey accurate information. CAPE is also considered to be less sensitive to cyclical effects, making it more efficient than the simple P/E ratio at predicting future values (Campbell and Shiller (1988)).

It can be computed as follows for a firm i at year t :

$$CAPE_{i,t} = \frac{(\text{Market Share Price})_{i,t}}{1/5 \sum_{t-5}^t (\text{Earnings})_{i,t}^A}$$

where the superscript A stands for “adjusted for inflation”. The mathematical formulation of the CAPE takes simply the average of the five past years of earnings, adjusted for inflation as its denominator while keeping the share price as its numerator. Nevertheless, the CAPE ratio still has limitations. As it takes the average earnings over the past five years, it has been critiqued as backward-looking instead of focusing on potential future values. It is, for example, insufficient for adepts of rational expectations since it is only averaging past values of earnings. Additionally, Siegel (2016) highlighted that forecasts made using the CAPE ratio have become overly pessimistic due to the changes in the computation of GAAP (Generally Approved Accounting Principle) earnings. Siegel argues that when GAAP earnings are replaced with some other value to measure the earning power of the firm, such as NIPA (National Income and Product Account) after-tax corporate profits, models using CAPE regain their explanatory power. This paper addresses those concerns by making its own dataset, using multiple sources of data and P/E ratio computation. For example, WRDS uses a slightly different computation method by taking the multiple of the market value of equity to the 5-year moving average of the company's net income.

iii. Price-to-book ratio

The price-to-book ratio (P/B) is the third measure of firm value that is used in this paper. The book value of an asset is the value attributed to it on a firm's balance sheet. It can also be considered the total value of tangible assets of a firm as they appear on the balance sheets, meaning the difference between

total assets and intangibles like patents. The P/B multiple is measured by taking the ratio of a firm's share price to its book value per share. Relatively low P/B ratios can be interpreted as undervaluation or the presence of fundamental flaws within the company of interest. For firm i , it is computed as follows:

$$PB_i = \frac{(\text{Market Price Per Share})_i}{(BVPS)_i}$$

where BVPS stands for “book value per share”. The BVPS is simply the book value of the firm divided by the total amount of shares outstanding. Simplifying it to the maximum, it can be reduced to:

$$BVPS_i = \frac{(\text{Total Shareholder Equity})_i - (\text{Preferred Equity})_i}{(\text{Total Shares Outstanding})_i}$$

This represents the firm's value remaining for common shareholders after preferred shareholders, such as debtors, have been compensated.

ESTIMATION STRATEGY

This paper relies on the yearly Russell 3000 reconstruction as a quasi-natural experiment to study exogenous variations in ETF ownership. Considering the particularity of the identification strategy, there is an ongoing debate in the literature about using either a regression discontinuity (RD) or an instrumental variable (IV) approach (see Appel et al. (2019)). This paper tried to explore both even though the instrument revealed itself to be weak, probably because of the relatively small exploitable sample.

This paper relies on a regression discontinuity design to conduct the analysis, closely resembling the econometric methods demonstrated in Glossner (2019). In addition, the instrumental variable methodology of Ben-David et al. (2018) will also be used as an additional analysis tool, detailed in Appendix 2. Results from the IV are reported in the appendix since the instrument used (index switchers) is considered to be weak in the framework of this study, due to its insufficient F-statistic. The contribution of this paper will be to study the effect of ETFs on more intrinsic measures of firm value

than the market price, which is subject to higher volatility. The objective is to provide insight for longer-term investment decisions concerning firms with a significant share of stock held by ETFs, usually the top firms in terms of market capitalization since they tend to be tracked by more ETFs than firms with lower market caps.

A. OLS regression

This first run of regression aims at identifying any correlation in the dataset. Controls are used for year change, and stock fixed effects were built in. Standard errors are clustered at the stock level. Regressions on every measure of the value of firm i at time t ($V_{i,t}$) were conducted as follows:

$$V_{i,t} = \alpha_0 + \alpha_1 \text{Ownership}_{i,t} + \sum_{n=1}^N \alpha_{2,n} (\ln(\text{MayCap}_{i,t}))^n + AX_{i,t} + \delta_t + \varepsilon_{i,t}$$

where $\ln(\text{MayCap}_{i,t})$ is the logarithm of the firm's market capitalization at the end of May, A is a vector of coefficients, $X_{i,t}$ a vector of controls, δ_t represents time fixed effects and $\varepsilon_{i,t}$ is the error term. The summation sign describes checks for higher-order polynomials of order n . Both measures of Ownership, the standard and the "cumulative" ones, were used. Nonetheless, as with any OLS regression, there is risk of omitted variable bias and reverse causality. For causal inference, the paper turns to a RD analysis exploiting the 1000th rank cutoff that determines index assignment. This cutoff represents the ranking at which a company switches from the bottom of the Russell 1000 (lower ETF ownership) to the top of the Russell 2000 (larger ETF ownership). This change in ownership comes from the fact that the Russell 3000 (that englobes the Russell 1000 and 2000) is a value-weighted index (more development in Part V.B.).

B. Regression discontinuity analysis

To study some of the variation following the Russell 3000 index reconstitution, using regression discontinuity as in Glossner (2019) and Appel et al. (2019) is a suitable option. Comparing firms around the 1000th cutoff made it possible to identify the effect of ETF ownership, since the Russell 3000 is a value-weighted index that attributes higher weights to firms

ranking higher in the Russell 2000 or 1000. Since more passive funds track the top firms of the Russell 2000 (as they have higher weights), the discontinuity can be observed when a firm crosses the 1000th market cap cutoff. Firms around the cutoff are similar in size and market share. This means that crossing the cutoff can be considered a random event, since the market cap is influenced by the stock price, which varies exogenously.

The effect of crossing the cutoff is proportional to the weights of the different ETFs tracking the value. As an example of weight change, consider the largest Russell 2000 ETF, the iShares Russell 2000 ETF (Ticker: IWM) from Blackrock, which has a weight of approximately 0.37% on its current (April 30th, 2020) second top value, Novocure Ltd (Ticker: NVCR). With the latest share price of \$73.54 and 1,857,165 shares owned by IWM, NVCR's ETF ownership is of about \$136.6 million. This value represents 0.37% of the cumulative market value of all IWM holdings, that is about \$36.6 billion¹⁹. Should NVCR become IWM's top security and see its weight increase from 0.37% to 0.74% (the weight of the current top value), it means that the total value of NVCR in IWM's portfolio must jump to approximately \$271.2 million. In order to do so, the iShares Russell 2000 ETF will need to buy approximately 1 829 999 NVCR shares at \$73.54, which is a non-negligible move on the market since it represents about \$135 million, or 1.85% of NVCR's current \$7.26 billion market capitalization²⁰.

At first, it could be tempting to go for a sharp RD analysis considering that the threshold is strict, and that the probability of treatment jumps to one in almost every case. However, since the actual running variable (the FTSE Russell-computed end-of-May market capitalization) is not observable, academic studies can only rely on imperfect proxies. Even if the proxy used in this paper has a high success rate, it is still different from 100% and is not comparable to the actual running variable used by FTSE Russell (Appel et al. (2019)). It is then possible to switch to a fuzzy RD estimation using imperfect proxies for end-of-May market caps. In practice, fuzzy RD requires a jump in the probability of treatment when crossing the cutoff. This is what happens in the case of this dataset, in which 99.2% of stocks are treated when crossing the cutoff.

¹⁹ Source: *iShares as of April 30th 2020*.

²⁰ Source: *YCharts*

Two distinct fuzzy RD analyses can be conducted. First, the main study of the paper: the effect of ETF ownership on firm value. The paper proceeds to a two-stage least squares estimation using as its first step:

$$Ownership_{i,t} = \beta_0 + \beta_1 \tau_{i,t} + \sum_{n=1}^N \beta_{2,n} (Rank_{i,t})^n + \sum_{n=1}^N \beta_{3,n} \tau_{i,t} \times (Rank_{i,t})^n + u_{i,t}$$

where $\tau_{i,t}$ stands for the treatment indicator, and is equal to 1 if the firm is effectively assigned to the Russell 2000 and zero otherwise. $Rank_{i,t}$ is the ranking of firm i at time t . The second stage of the estimation is as follows:

$$V_{i,t} = \theta_0 + \theta_1 \widehat{Ownership}_{i,t} + \sum_{n=1}^N \theta_{3,n} (Rank_{i,t})^n + \sum_{n=1}^N \theta_{4,n} \tau_{i,t} \times (Rank_{i,t})^n + e_{i,t}$$

In a second time, considering the limited amount of data, it could be interesting to look one step backward and investigate if there is any effect of firm value that could be attributed to the Russell 2000 assignment. This analysis is conducted in Appendix 1.

Regression discontinuity graphs are plotted in Figure 6 and include all measures of firm value.

RESULTS & DISCUSSION

A. OLS regression

Table II displays the ordinary least squares (OLS) regression results. The results observed are mostly insignificant for the relevant independent variable (ETF ownership) and for the different measures of firm value that were tested (CAPE, P/E and P/B). Only tables that display some level of significance are reported and commented on. The CAPE panel does not show any significance, which points to an absence of relationship between the two variables. However, due to the significant limitation of the dataset due to information restriction, this result is not generalizable. P/E and P/B ratios also don't display any significant relationship with ETF ownership.

The P/B ratio did show a significant relationship with the first-order polynomial of the logarithmic May market capitalization. However, this effect is not robust to controls. In the case of standard ETF ownership, there is at first a positive coefficient that eventually turns negative and insignificant,

which could be linked to the model picking up mean reversion. Should this relationship have statistical meaning, a one-point increase in ETF ownership would create an approximative decrease of 1 point in CAPE when adding controls. This suggests, somewhat surprisingly, that increased ETF ownership would be a burden that penalizes the underlying firm. One may think this is a repercussion of some of the adverse effects previously ascribed to ETFs, like volatility (Ben-David et al. (2018)). A similar trend is observed in P/B ratio, though the coefficients are extremely close to zero.

When looking at estimates for the P/E ratio, however, coefficients turn positive. Considering that CAPE is only a smoothed version of P/E, this result suggests that immediate benefits from increased ETF ownership are offset when the effect is averaged over the last five years. In sum, the benefits of ETF ownership might only be short-term and volatile, not to mention the potential reverse causality and omitted variable bias effects.

B. Regression discontinuity analysis

Table III reports the results of the fuzzy RD analysis. Table III(a) describes the effect of ETF ownership on firm value, effects that turn out to be insignificant even after checking for several bandwidths. Table III(b) studies the effect of index assignment on the different measures of firm value. Finally, Table III(c) reports the results of the effect of indexing on firm value. Estimates were controlled for alternate bandwidths, and “manipulation testing” in line with McCrary (2008) was performed following the methods in Cattaneo, Jansson, and Ma (2015). The paper controls respectively for bandwidths of 50, 100, 200, and 300 observations around the cutoff. McCrary tests showed no evidence of sorting around the cutoff, and density plots of McCrary tests are available in Figure 7. To ensure that no other covariate was moving around the cutoff, formal balance tests of the key controls were performed and reported in Table IV. No significant move in any covariate was reported around the cutoff.

Crossing the 1000th market cap cutoff might potentially affect other things besides ETF ownership. The exclusion restriction hypothesis is violated if an omitted variable that

varies at the cutoff can affect firm value. Possible candidates include ownership by other types of funds, such as mutual funds or hedge funds. Ownership data for these funds could not be obtained by the author because of their unavailability. However, Ben-David et al. (2019) performed a verification around the cutoff for other types of funds and noted no significant discontinuity, neither for active funds nor for hedge funds. Evidence of discontinuity was present for index fund ownership, encouraging the authors to add it as a control in their final regression. However, it turned out to have no significant impact on their final (second stage) results. As this paper uses the same dataset, we can assume that the same result would hold should this data be available. Though this is an imperfect proxy, similar tests ran on an identical dataset should in theory hold the same results. RD plots for other types of funds are available in Figure 8. Additionally, Ben-David et al. (2019) had access to a substantially larger dataset with a greater temporal coverage and stock universe, strengthening the robustness of their results.

Firm value could also be affected by media coverage due to inclusion in the Russell 2000. Evidence of this is found by Andrei and Hasler (2015), who show that prices react to fundamental information which generates volatility. However, Crane et al. (2016) find that inclusion in the Russell 2000 does not increase media coverage, as do Appel et al. (2016), who find no significant change in analyst coverage after inclusion in the Russell 2000.

An additional potential check of the exclusion restriction hypothesis is to analyze the outcome variable in light of the interaction between ownership and an instrument. The impact of ownership should be greater when the instrument is large, which is precisely the finding in Ben-David et al. (2019): The effect of switching indexes is stronger when ETF ownership is greater.

None of the coefficients concerning standard ETF ownership are statistically significant, and all are quite noisy. Estimates for the first stage regression were not reported but were equally insignificant, suggesting an absence of a strong statistical relationship. Another noticeable pattern emerges across all three measures of firm value: coefficients closer to the cutoff are positive and turn negative once the bandwidth

is increased, which can be explained by the construction of the ranking variable. Crossing the cutoff from the 1000th to the 1001st position translates into having a lower market capitalization: the higher the rank, the lower the market cap. Since the Russell 3000 is a value-weighted index, the top firms in both indexes benefit from larger weights in ETF portfolios; as we get further from the cutoff, weights fall and so does the effect of ETF ownership. Consequently, the effect of ownership is the highest the closer the firm's market cap is to the cutoff. As the bandwidth increases, the effect of ranking might progressively offset the impact of ownership and lead to a decrease in firm value. All three measures of firm value are dependent on market price, so they will increasingly pick up the effect of higher rank (lower market cap) as the effect of ETF ownership attenuates.

A similar pattern is observable for cumulative ownership, though the coefficients are remarkably close to zero. Quite surprisingly, coefficients for cumulative ownership showed a high significance at the third-degree polynomial. Cumulative ownership is an unscaled variable with a high variability, so observing massive non-linear movements in that variable is more likely than for the scaled measure of ETF ownership. For CAPE and P/B ratios, though the coefficients are small, increased cumulative ownership does create a firm value premium for cubed values of the ranking variable at the 1% significance level.

CONCLUSION

The primary objective of this paper was to provide an inquiry into the effects of ETFs beyond the marketplace and their impact at the firm level. Different measures of firm value were chosen to test if potential trends could be determined. There were two initial hypotheses: either ETF ownership would cause a positive premium of firm value, or it would not since those measures are sufficiently insulated from movements in passive investing. This paper's analysis points to the second possibility. ETF ownership, be it scaled to market capitalization or not, does not seem to impact firm value in any way after several controls. This insignificance is

driven by extremely noisy estimates that probably come in part from the reduced sample size, due to private information and exceptional circumstances (April 2020), reducing the author's access to specific databases.

Though no significant relationship between ETFs and firm value stood out, this result does not mean there are no implications. Such a conclusion would be good news for both analysts, who could now adapt their strategies and recommend ETFs without fearing distortions in financial multiples, and for firm managers, who would not have to worry that a rising share of ETF ownership might affect their firm.

Current literature on ETFs was focused mostly on its market effects and distortions they could create. This paper tried to contribute to the emerging literature about their effects beyond the market and used a new set of dependent variables to perform this study. However, considering the limited amount of data that could be matched with the stocks composing the index, the results need to be taken with a pinch of salt. Further research, with the help of larger means of data collection and matching, could investigate if the results of this study are consistent with more massive datasets and if they extend beyond 2007. The effects of ETFs beyond the market also require more research. Since they represent such an essential part of the current financial world, they could be used to study herd behavior in financial crises like the one following the current COVID-19 pandemic. Though ETFs facilitate liquidity in markets and allow a drastic reduction of transaction costs, as a popular saying in Economics would put it, "there is no such thing as a free lunch". It is important for these easily accessible financial tools to be more thoroughly studied.

Contents

REFERENCES AND APPENDICES

I. PAPERS

- 155 The Effect of Increased Intensity of Fatalities on the Popularity of Partido Demokratiko Pilipino-Lakas ng Bayan: Evidence from the Mayoral 2019 Elections
THOMAS MATTHEW ARANETA
- 165 Are Women Who Out-earn and Out-work their Husbands Less Happy?: Evidence from Canada
ANTON BURI, IA MANTECÓN GARCÍA, & JESSICA WU
- 173 The Effect of Trump's Buy American Hire American Order on H-1B and American Workers
JUSTINE ENGEL
- 179 Intrahousehold Determinants of Timely School Attendance in Uganda
JEANNE LEGUA
- 185 The Effectiveness of the Menstrual Hygiene Scheme in Improving Female Educational and Autonomy Outcomes in Target Indian Districts
MANIKA MARWAH

II. HONOURS THESIS

- 195 Does Membership Come With
Benefits? The Effect of ETFs
on Firm Value

RAPHAËL GRACH

A. References

B. Appendices

C. Tables and Figures

*The Effect of Increased Intensity of
Fatalities on the Popularity of Partido
Demokratiko Pilipino-Lakas ng Bayan:
Evidence from the Mayoral 2019 Elections*

Thomas Matthew Araneta

ECON 494

I would like to thank my thesis advisor Professor Siwan Anderson for all her feedback throughout the term. I would also like to thank Professor Cesi Cruz for sending me the electoral data for my thesis and all the guidance she gave me in conducting this research. I would like to acknowledge Professor Jaime McCasland for her guidance on my topic. I'm also grateful to our teaching assistant Meng Ying for her help, and to my peers in the BIE who helped me improve my work. Finally, I would like to thank Professor Jonathan Graves and the staff at the IONA Journal of Economics for the feedback they gave me in revising my paper.

A. REFERENCES

- Abadie, A., & Gardeazabal, J. (2003). The economic costs of conflict: A case study of the basque country. *American Economic Review*, 93 (1), 113-132.
- Bazzi, S., & Blattman, C. (2014). Economic Shocks and Conflict: Evidence from Commodity Prices. *American Economic Journal: Macroeconomics*, 6(4), 1-38. doi: 10.1257/mac.6.4.1
- Blanco, L. R. (2012). The Impact of Insecurity on Democracy and Trust in Institutions in Mexico. RAND Labor and Population working paper series WR-940
- Blanco, L., & Ruiz, I. (2013). The impact of crime and insecurity on trust in democracy and institutions. *American Economic Review*, 103 (3), 284-88.

Bove, V., Elia, L., & Smith, R. P. (2016). On the heterogeneous consequences of civil war. *Oxford Economic Papers*. doi: 10.1093/oep/gpw050

Cavalcanti, F. L., Daniele, G., & Galletta, S. (2018). Popularity Shocks and Political Selection. *SSRN Electronic Journal*, 165, 201–261. doi: 10.2139/ssrn.3141242

Dell, M. (2015). Trafficking Networks and the Mexican Drug War. *The American Economic Review*, 105(6), 1738–1779. www.jstor.org/stable/43495438

Dube, O., & Vargas, J. F. (2013). Commodity price shocks and civil conflict: Evidence from colombia. *The Review of Economic Studies*, 80 (4), 1384–1421.

Felab-Brown, V. (2017, August 8). The human rights consequences of the war on drugs in the Philippines. The Brookings Institution.

Finlay, K., Magnusson, L., & Schaffer, M. (2014). "WEAKIV10: Stata module to perform weak-instrument-robust tests and confidence intervals for instrumental-variable (IV) estimation of linear, probit and tobit models," *Statistical Software Components S457910*, Boston College Department of Economics.

Geoportal Philippines. National Airports

Hodler, R. (2018). The economic effects of genocide: Evidence from rwanda. *Journal of African Economies*.

Human Rights Watch. (2017). *World Report 2018: Events from 2017*.

Johnson, D., & Fernquest, J. (2018). Governing through Killing: The War on Drugs in the Philippines. *Asian Journal of Law and Society*, 5(2), 359–390. doi: 10.1017/als.2018.12

Luong, H. T. (2015). Transnational Drugs Trafficking from West Africa to Southeast Asia: A Case Study of Vietnam. *Journal of Law and Criminal Justice*, 3(2), 37–54. doi: 10.15640/jlcj.v3n2a4

Miguel, E., & Roland, G. (2011). The long-run impact of bombing vietnam. *Journal of Development Economics*, 96 (1), 1-15.

Olario, A. R. (1999). Current Situation of Transnational Organized Crime in the Philippines. *National Criminal Justice Reference Service* , 171-180.

Philippine Drug Enforcement Agency. (2013). "Annual Report 2013/ The Philippine Drug Situation"

Philippine Drug Enforcement Agency. (2012). "Annual Report 2012/ The Philippine Drug Situation"

Pulseasia. (2019, October 7). September 2019 Nationwide Survey on the Performance and Trust Ratings of the Top Philippine Government Officials. Retrieved from <http://www.pulseasia.ph/september-2019-nationwide-survey-on-the-performance-and-trust-ratings-of-the-top-philippine-government-officials/>

Quimpo, N. G. (2017). Duterte's "War on Drugs". In *A Duterte Reader* (pp. 145-166). Quezon City, Philippines: BUGHAW.

Raleigh, Clionadh, Andrew Linke, Håvard Hegre and Joakim Karlsen. (2010). "Introducing ACLED-Armed Conflict Location and Event Data." *Journal of Peace Research* 47(5) 651-660.

Ravanilla, N., Sexton, R., & Haim, P., *Deadly Populism: How Local Political Outsiders Drive Duterte's War on Drugs in the Philippines*, Working Paper

Reid, G., Devaney, M. L., & Baldwin, S. (2006). Drug production, trafficking and trade in Asia and Pacific Island countries. *Drug and Alcohol Review*, 25(6), 647-650.

Simangan, D. (2017). Is the Philippine "War on Drugs" an Act of Genocide? *Journal of Genocide Research*, 20(1), 68-89. doi: 10.1080/14623528.2017.1379939

Stations, S. W. (n.d.). Social Weather Stations: Second Quarter 2019 Social Weather Survey: Net satisfaction with anti-illegal drugs campaign at "Excellent" 70. Retrieved from <https://www.sws.org.ph/swsmain/artcldisppage/?artcsyscode=ART-20190922154614>

Still the People's Choice. (2020, February). *The Economist*, pp. 34-36.

United Nations High Commissioner for Human Rights. (2020). Situation of human rights in the Philippines : Report of the United Nations High Commissioner for Human Rights (Rep.). Human Rights Council.

United Nations Office for the Coordination of Humanitarian Affairs office. (2019, Jul). Philippines Poverty Statistics. The Humanitarian Data Exchange.

United Nations Office for the Coordination of Humanitarian Affairs office. (2017, Sep). Philippines - 2015 Census Population (administrative level 1 to 4). The Humanitarian Data Exchange.

World Food Program. (2017, Jan). Global Ports. WFPGeoNode

World Justice Project. (2019). World Rule of Law Index 2019.

B. APPENDICES

*A1: Alternative instrument construction**i. 5 biggest airports and seaports*

Instead of using all the ports, I only use the largest air and sea ports in the Philippines. I use the 5 largest airports by passenger volume in 2016. I should note that for the sea port data, I was not able to find geocoded data on some of the ports. Instead of using the 5 largest ports by cargo shipped, I take the 2 largest ports that I have data on for northern Luzon, and the largest port I have data on for Southern Mindanao, Northern Mindanao, and the Visayas (I omit Southern Luzon because after its largest port which I do not have data on, the volumes shipped are comparatively very small, so I believe that using the 2 largest ports in Northern Luzon offer more information since North and South Luzon are part of the same contiguous landmass).

Table A1 (a): First stage using 5 biggest airports and seaports

VARIABLES	(1) Fatalities
Distance to the closest of 5 biggest seaports	0.0019 (0.0036)
Distance to the closest of 5 biggest airports	-0.0042 (0.0039)
Poverty Incidence in 2015	-10.1134*** (1.1626)
Log of population	5.7717*** (1.4373)
Constant	-54.4674*** (14.7929)
Observations	1,615
F-test	1.211
Prob > F	0.298

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The first stage results for the continuous fatality variable are much weaker than when I use my entire sample of seaports and airports. If the Philippines is a porous transit point for drugs, which I believe is plausible given how the previous literature often cite the extensive coastline of the Philippines as a reason for why it is a popular drug transit location, then limiting my instrument in this way drastically reduces the information my instrument contains.

Table A1 (b): instrument using the 5 biggest airports and seaports

VARIABLES	(1) Vote share for PDP	(2) Vote share for PDP	(3) Binary elect PDP variable	(4) Binary elect PDP variable	(5) Vote share for PDP	(6) Vote share for PDP	(7) Binary elect PDP variable	(8) Binary elect PDP variable
Fatalities	-0.0216 (0.0226)	0.0145 (0.0104)	-0.0187 (0.0202)	0.0052 (0.0103)				
Poverty Incidence in 2015	-0.1484 (0.2276)	0.0574 (0.0959)	-0.1522 (0.2046)	0.0444 (0.0977)	-0.4610 (0.2955)	0.4351 (0.3525)	-0.2270 (0.2061)	0.4259 (0.3439)
Log of population	0.1220 (0.1454)	-0.1036 (0.0704)	0.1088 (0.1288)	-0.0364 (0.0718)	0.1407* (0.0817)	-0.1686 (0.1222)	0.0707 (0.0573)	-0.1458 (0.1181)
Binary fatality variable					-0.6831* (0.3794)	0.6819 (0.4980)	-0.3366 (0.2679)	0.5981 (0.4813)
Constant	-1.0056 (1.3967)	1.3034* (0.6920)	-0.9943 (1.2358)	0.4343 (0.7004)	-0.9338 (0.6430)	1.6756* (1.0057)	-0.5088 (0.4503)	1.2723 (0.9720)
Observations	1,615	989	1,615	989	1,615	989	1,615	989
PDP Candidate ran		YES		YES		YES		YES
Binary fatality variable					YES	YES	YES	YES
Binary elect variable			YES	YES			YES	YES

Robust standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Notes: this table provides the results when I construct my instrument using the largest sea and air ports I have data for

In this table, the only marginally significant result is in column 5 when I use my whole sample and use the binary fatality variable and a continuous measure of vote share. This specification diminishes the significance of my earlier results, especially since there is a negative coefficient on the binary fatality variable. However, I don't put too much stock into this result because as mentioned earlier limiting my instrument in this way severely constrains the information it contains.

ii. Using only the distance to airports

I try creating an instrument using only the distance to airports, since it is this distance variable that has a statistically significant relationship with fatalities.

The first stage results here are stronger than the one from my main instrument where I use both my airports and seaports. It could be argued that I ought to use this instrument specification instead. However, since the first stage when I use both my airports and seaports is still statistically significant, I decide to use that specification instead because including the seaports as well should capture more information about the cost mechanism.

Table A1 (c): First stage using distance to airports only

VARIABLES	(1) Fatalities
Distance to airport	-0.0283*** (0.0089)
Poverty Incidence in 2015	-9.6670*** (1.1655)
Log of population	5.8266*** (1.3939)
Constant	-54.6066*** (14.1106)
Observations	1,615
F-test	10.19
Prob > F	0.00144

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A1 (d): instrument using distance to airports only

VARIABLES	(1) Vote share for PDP	(2) Vote share for PDP	(3) Binary elect PDP variable	(4) Binary elect PDP variable	(5) Vote share for PDP	(6) Vote share for PDP	(7) Binary elect PDP variable	(8) Binary elect PDP variable
Fatalities	0.0249** (0.0115)	-0.0027 (0.0481)	-0.0012 (0.0082)	-0.0615 (0.1125)				
Poverty Incidence in 2015	0.3104** (0.1208)	-0.0582 (0.3308)	0.0200 (0.0931)	-0.4037 (0.7484)	0.7925 (0.5065)	-0.0245 (0.2705)	-0.0036 (0.2483)	0.3568 (0.3750)
Log of population	-0.1514** (0.0677)	0.0170 (0.3357)	0.0062 (0.0474)	0.4312 (0.8124)	-0.2066 (0.1381)	-0.0074 (0.0964)	0.0089 (0.0659)	-0.1216 (0.1334)
Binary fatality variable					0.9458 (0.6467)	0.0221 (0.3877)	-0.0464 (0.3127)	0.4990 (0.5385)
Constant	1.5984** (0.6527)	0.1247 (3.2784)	-0.0166 (0.4495)	-4.1356 (7.9658)	1.7786* (1.0806)	0.3553 (0.7971)	-0.0255 (0.5124)	1.0739 (1.1015)
Observations	1,615	989	1,615	989	1,615	989	1,615	989
PDP Candidate ran		YES		YES		YES		YES
Binary fatality variable					YES	YES	YES	YES
Binary elect variable			YES	YES			YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The results here are very similar to the ones in table 6 when I use my main instrument which is constructed using all the seaports and airports. The only difference is a slightly higher 2SLS coefficient in column 1, which is still consistent with H1.

A2: Using the average of Poverty Incidence over 2009, 2012, and 2015 rather than just the Poverty Incidence in 2015

Since I have data on the Poverty Incidence for these 3 years, I try using a specification where I take the average of the Poverty Incidence for these 3 years rather than just using the Poverty Incidence in 2015. This way, I am able to use information from all 3 years without encountering multi-collinearity problems in my regression. The results here are

consistent with the results presented in table 6 with a slightly higher coefficient on fatalities in column 1, and are consistent with H1.

Table A2: Using the average of Poverty Incidence over 3 years rather than just the poverty incidence in 2015

VARIABLES	(1) Vote share for PDP	(2) Vote share for PDP	(3) Binary elect PDP variable	(4) Binary elect PDP variable	(5) Vote share for PDP	(6) Vote share for PDP	(7) Binary elect PDP variable	(8) Binary elect PDP variable
Fatalities	0.0253** (0.0122)	-0.0019 (0.0252)	-0.0030 (0.0085)	-0.0369 (0.0396)				
Average of Poverty Incidence	0.3485** (0.1434)	-0.0419 (0.2084)	0.0070 (0.1094)	-0.2563 (0.3237)	0.6281 (0.4269)	-0.0115 (0.1977)	0.1412 (0.2620)	0.3386 (0.2530)
Log of population	-0.1482** (0.0683)	0.0115 (0.1720)	0.0169 (0.0477)	0.2550 (0.2536)	-0.1422 (0.1049)	-0.0065 (0.0638)	-0.0253 (0.0634)	-0.0956 (0.0817)
Binary fatality variable					0.6834 (0.5165)	0.0213 (0.2641)	0.1239 (0.3150)	0.4170 (0.3385)
Constant	1.5472** (0.6509)	0.1764 (1.6649)	-0.1211 (0.4460)	-2.4013 (2.4594)	1.2305 (0.7933)	0.3417 (0.5204)	0.2288 (0.4775)	0.8310 (0.6655)
Observations	1,615	989	1,615	989	1,615	989	1,615	989
PDP Candidate ran		YES		YES		YES		YES
Binary fatality variable					YES	YES	YES	YES
Binary elect variable			YES	YES			YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: This table shows the effect of fatalities when I use the average of 3 years of poverty incidence rather than just the poverty incidence for 2015

A3: Clustered standard errors

As mentioned earlier, significance is lost when I use clustered standard errors, likely because of the nature of the cluster sizes. Clustering using regions leaves me with too few clusters, clustering using provinces leaves me with too few observations in each cluster. I include these specifications here.

iii. Clustering using provinces

Clustering with provinces leaves me with 84 clusters, for an average of about 20 observations per cluster. This is too little variation in each cluster and is likely causing the loss of significance.

Table A3 (a): Clustering using provinces

VARIABLES	(1) Vote share for PDP	(2) Vote share for PDP	(3) Binary elect PDP variable	(4) Binary elect PDP variable	(5) Vote share for PDP	(6) Vote share for PDP	(7) Binary elect PDP variable	(8) Binary elect PDP variable
Fatalities	0.0239 (0.0179)	-0.0012 (0.0309)	-0.0030 (0.0098)	-0.0360 (0.0439)				
Poverty incidence in 2015	0.3008 (0.2147)	-0.0482 (0.2315)	0.0027 (0.1300)	-0.2329 (0.2930)	0.5748 (0.5313)	-0.0291 (0.2475)	0.1013 (0.2789)	0.3066 (0.3554)
Log of population	-0.1456 (0.0951)	0.0067 (0.2148)	0.0165 (0.0563)	0.2530 (0.2681)	-0.1463 (0.1481)	-0.0058 (0.0833)	-0.0202 (0.0696)	-0.1039 (0.1163)
Binary fatality variable					0.6629 (0.6795)	0.0155 (0.3262)	0.0899 (0.3293)	0.4268 (0.4637)
Constant	1.5436* (0.9148)	0.2260 (2.0996)	-0.1149 (0.5339)	-2.3934 (2.6228)	1.3075 (1.1740)	0.3421 (0.6989)	0.2015 (0.5410)	0.9295 (0.9653)
Observations	1,615	989	1,615	989	1,615	989	1,615	989
PDP Candidate ran		YES		YES		YES		YES
Binary fatality variable					YES	YES	YES	YES
Binary elect variable			YES	YES			YES	YES

Clustered Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

ii. Clustering Using Regions

Clustering using the regions leaves me with 17 clusters. While this specification has more observations per cluster (about 95 observations per cluster) the count of clusters itself is too low. This is likely causing the loss of significance.

Table A3 (b): Clustering using regions

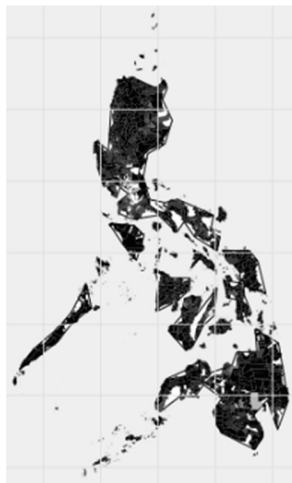
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Vote share for PDP	Vote share for PDP	Binary elect PDP variable	Binary elect PDP variable	Vote share for PDP	Vote share for PDP	Binary elect PDP variable	Binary elect PDP variable
Fatalities	0.0239 (0.0243)	-0.0012 (0.0328)	-0.0030 (0.0101)	-0.0360 (0.0441)				
Poverty Incidence in 2015	0.3008 (0.2568)	-0.0482 (0.2351)	0.0027 (0.1087)	-0.2329 (0.2983)	0.5748* (0.3313)	-0.0291 (0.2267)	0.1013 (0.2155)	0.3066 (0.3420)
Log of population	-0.1456 (0.1273)	0.0067 (0.2283)	0.0165 (0.0599)	0.2530 (0.2750)	-0.1463 (0.1132)	-0.0058 (0.0816)	-0.0202 (0.0617)	-0.1039 (0.0931)
Binary fatality variable					0.6629 (0.4770)	0.0155 (0.3221)	0.0899 (0.2899)	0.4268 (0.3786)
Constant	1.5436 (1.2061)	0.2260 (2.2282)	-0.1149 (0.5750)	-2.3934 (2.6987)	1.3075 (0.9297)	0.3421 (0.6796)	0.2015 (0.4875)	0.9295 (0.7454)
Observations	1,615	989	1,615	989	1,615	989	1,615	989
PDP Candidate ran		YES		YES		YES		YES
Binary fatality variable					YES	YES	YES	YES
Binary elect variable			YES	YES			YES	YES

Clustered standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Either method of clustering results in too little variation. Clustering using provinces results in too few observations per cluster. Clustering using regions results in too few clusters. Because of this, I am not too concerned about my results, even if they aren't robust to clustered standard errors.

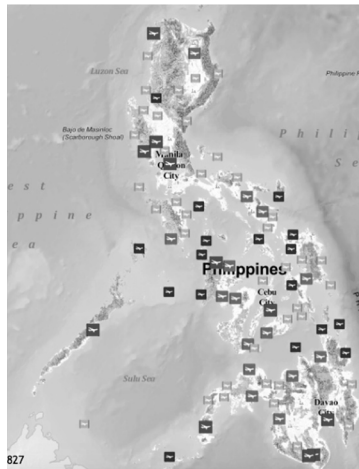
A4: Figures

a) Heat map of the log of fatalities in the Philippines

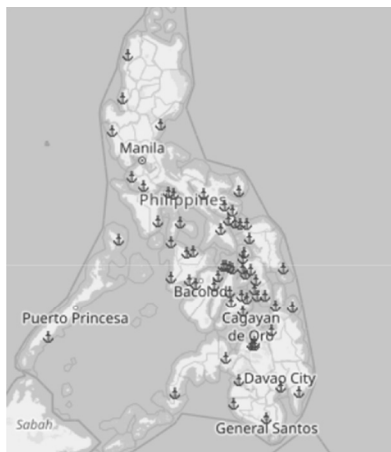


This heat map (generated in Python) shows the log of fatalities mapped to the respective municipalities in the Philippines. It is similar to the homicide rate heat map Blanco(2012) includes in their appendix. I decide to use the log of fatalities rather than the count in this visualization because Python scales the colouring of the log of fatalities better. Using the count of fatalities results in most municipalities looking like they have 0 fatalities (when in reality they have a few) because of the colour scaling.

b) Map of Airports



c) Map of Seaports



*Are Women Who Out-earn and Out-work their Husbands Less Happy?:
Evidence from Canada*

Anton Buri, Ia Mantecón García, & Jessica Wu

*ECON*₄₉₀

Buri: (email: anton.buri@gmail.com); Mantecón García: (email: iamantecongarcia@gmail.com); Wu: (email: jessicaw@alumni.ubc.ca). Support from our Instructor, Dr. Marina Adshade, as well as Austin McWhirter is gratefully acknowledged.

A. REFERENCES

Argyrous, George, and Sara Rahman. 2014. "How Does Paid Work Affect Who Does the Childcare? An Analysis of the Time Use of Australian Couples." *Review of Economics of the Household* 15 (2): 383–98.

Becker, Gary S. 1991. *A Treatise on the Family*. Enl. ed. Cambridge, Mass: Harvard University Press.

Bergmann, Barbara R. 1996. "Becker's Theory of the Family: Preposterous Conclusions." *Challenge* 39 (1): 9–12.

Bertrand, Marianne, Jessica Pan, and Emir Kamenica. 2013. "Gender Identity and Relative Income within Households." Cambridge, MA: National Bureau of Economic Research.

Catalyst Research. 2019. "Women in the Workforce - Canada: Quick Take." Catalyst. (accessed April 16, 2020).

Easterlin, Richard A. 2001. "Income and Happiness: Towards a Unified Theory." *The Economic Journal* 111 (473): 465–84.

Elmslie, Bruce T., and Edinaldo Tebaldi. 2014. "The Determinants of Marital Happiness." *Applied Economics* 46 (28): 3452–62.

Fernández, Raquel, Alessandra Fogli, and Claudia Olivetti. 2004. "Mothers and Sons: Preference Formation and Female Labor Force Dynamics." *The Quarterly Journal of Economics* 119 (4): 1249–99.

Fleche, Sarah, Anthony Lepinteur, and Nattavudh Powdthavee. 2018. "Gender Norms and Relative Working Hours: Why Do Women Suffer More Than Men from Working Longer Hours Than Their Partners?" *AEA Papers and Proceedings* 108: 163–68.

Fortin, Nicole. M. 2005. "Gender Role Attitudes and the Labour-Market Outcomes of Women across OECD Countries." *Oxford Review of Economic Policy* 21 (3): 416–38.

Hochschild, Arlie R. 1989. *The Second Shift: Working Families and the Revolution at Home*. 2012th ed. New York: Penguin Books.

Iyigun, Murat, and Randall P. Walsh. 2007. "Endogenous Gender Power, Household Labor Supply and the Demographic Transition." *Journal of Development Economics* 82 (1): 138–55.

Milan, Anne, Leslie-Anne Keown, and Covadonga Robles Urquijo. 2012. *Women in Canada: A Gender-Based Statistical Report*. Ottawa: Statistics Canada, Social and Aboriginal Statistics Division.

Moyser, Melissa, and Statistics Canada. 2019. *Measuring and Analyzing the Gender Pay Gap: A Conceptual and Methodological Overview*.

- Roth, Anja, and Michaela Slotwinski. 2019. "Gender Norms and Income Misreporting within Households." SSRN Scholarly Paper ID 3338624. Rochester, NY: Social Science Research Network.
- Statistics Canada. 2013. "General Social Survey, Cycle 25: Family, 2011." Canada. (accessed April 16, 2020).
- Van den Broeck, Goedele, and Miet Maertens. 2017. "Does Off-Farm Wage Employment Make Women in Rural Senegal Happy?" *Feminist Economics* 23 (4): 250–75.
- Vanier Institute of the Family. 2010. *Families Count: Profiling Canada's Families IV*. Ottawa: Vanier Institute of the Family = Institut Vanier de la famille.
- Wang, Jichao, Wei Yan, and Jie Zhang. 2019. "Relative Income and Subjective Well-Being of Urban Residents in China." *Journal of Family and Economic Issues* 40 (4): 673–80.
- Wieber, Anna, and Elke Holst. 2015. "Gender Identity and Women's Supply of Labor and Non-Market Work: Panel Data Evidence for Germany." *SSRN Electronic Journal*; 1-44.

B. APPENDICES

TABLE A1:
INCOME SHARE FOR MEN

Dependent variable:	Life Satisfaction							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Income Share	0.0696*** (0.00317)	0.0139*** (0.00317)	0.0103*** (0.00317)	0.0345*** (0.00282)	0.286*** (0.00286)	0.290*** (0.00285)	0.283*** (0.00283)	0.226*** (0.00293)
Age		-0.0899*** (0.000431)	-0.0880*** (0.000433)	-0.0635*** (0.000404)	-0.0797*** (0.000401)	-0.0783*** (0.000409)	-0.0748*** (0.000410)	-0.0738*** (0.000415)
Age ²		0.000984*** (0.00000459)	0.000962*** (0.00000460)	0.000720*** (0.00000429)	0.000889*** (0.00000425)	0.000867*** (0.00000442)	0.000830*** (0.00000443)	0.000816*** (0.00000449)
Some Secondary			0.0522*** (0.00247)	0.267*** (0.00224)	0.427*** (0.00228)	0.424*** (0.00229)	0.369*** (0.00231)	0.323*** (0.00232)
Highschool Diploma			-0.0219*** (0.00234)	0.133*** (0.00215)	0.221*** (0.00212)	0.219*** (0.00212)	0.174*** (0.00213)	0.145*** (0.00212)
Some University			0.0211*** (0.00214)	0.147*** (0.00194)	0.211*** (0.00191)	0.208*** (0.00192)	0.166*** (0.00191)	0.156*** (0.00190)
Diploma/Certificate			-0.0449*** (0.00166)	0.0732*** (0.00151)	0.127*** (0.00150)	0.126*** (0.00150)	0.0862*** (0.00152)	0.0635*** (0.00152)
Health				0.120*** (0.00460)	0.0888*** (0.00449)	0.0851*** (0.00450)	0.111*** (0.00449)	0.0979*** (0.00452)
Health ²				0.0125*** (0.000623)	0.0131*** (0.000610)	0.0136*** (0.000611)	0.00966*** (0.000611)	0.0112*** (0.000615)
Mental Health				0.815*** (0.00921)	0.776*** (0.00871)	0.770*** (0.00870)	0.746*** (0.00870)	0.719*** (0.00879)
Mental Health ²				-0.0303*** (0.000781)	-0.0268*** (0.000740)	-0.0263*** (0.000739)	-0.0238*** (0.000739)	-0.0216*** (0.000746)
Household Income					0.119*** (0.000405)	0.120*** (0.000405)	0.113*** (0.000407)	0.109*** (0.000410)
Number of Children						-0.0124*** (0.000716)	-0.00689*** (0.000727)	-0.0106*** (0.000725)
Religious							0.0896*** (0.00152)	0.0915*** (0.00151)
Visible Minority							-0.191*** (0.00200)	-0.187*** (0.00199)
Work Hours								0.000156*** (0.0000572)
Meals								0.0267*** (0.00218)
Dishes								-0.139*** (0.00186)
Laundry								-0.199*** (0.00242)
Groceries								-0.189*** (0.00177)
Constant	8.292*** (0.00224)	10.26*** (0.0102)	10.23*** (0.0103)	5.112*** (0.0270)	4.195*** (0.0259)	4.210*** (0.0259)	4.201*** (0.0259)	4.452*** (0.0262)
N	3082950	3082950	3082950	3082950	3082950	3082950	3082950	3082950
R ²	0.000	0.013	0.014	0.196	0.221	0.221	0.225	0.232
Adjusted R ²	0.000	0.013	0.014	0.196	0.221	0.221	0.225	0.232

Robust Standard Errors in Parentheses

* p<0.1 ** p<0.05 *** p<0.01

TABLE A2:
INCOME SHARE FOR WOMEN

Dependent variable:	Life Satisfaction							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Income Share	-0.572*** (0.00303)	-0.592*** (0.00303)	-0.611*** (0.00304)	-0.529*** (0.00266)	-0.420*** (0.00273)	-0.426*** (0.00274)	-0.431*** (0.00274)	-0.178*** (0.00338)
Age		-0.0340*** (0.000506)	-0.0348*** (0.000506)	-0.0248*** (0.000448)	-0.0406*** (0.000457)	-0.0378*** (0.000471)	-0.0372*** (0.000470)	-0.0284*** (0.000472)
Age ²		0.000412*** (0.00000565)	0.000432*** (0.00000566)	0.000331*** (0.00000501)	0.000493*** (0.00000510)	0.000449*** (0.00000538)	0.000441*** (0.00000536)	0.000330*** (0.00000539)
Some Secondary Education			-0.215*** (0.00328)	0.0760*** (0.00285)	0.225*** (0.00298)	0.216*** (0.00300)	0.190*** (0.00303)	0.212*** (0.00300)
Highschool Diploma			0.0840*** (0.00234)	0.133*** (0.00208)	0.223*** (0.00213)	0.215*** (0.00216)	0.196*** (0.00217)	0.207*** (0.00217)
Some University			-0.182*** (0.00246)	-0.0845*** (0.00200)	-0.0368*** (0.00201)	-0.0395*** (0.00201)	-0.0497*** (0.00202)	-0.0413*** (0.00201)
Diploma/Certificate			0.0532*** (0.00166)	0.106*** (0.00148)	0.165*** (0.00150)	0.161*** (0.00151)	0.148*** (0.00152)	0.163*** (0.00152)
Health				0.373*** (0.00539)	0.338*** (0.00529)	0.343*** (0.00529)	0.326*** (0.00528)	0.321*** (0.00526)
Health ²				-0.0220*** (0.000723)	-0.0197*** (0.000710)	-0.0202*** (0.000710)	-0.0183*** (0.000708)	-0.0181*** (0.000705)
Mental Health				0.773*** (0.00981)	0.739*** (0.00967)	0.743*** (0.00975)	0.719*** (0.00962)	0.714*** (0.00927)
Mental Health ²				-0.0245*** (0.000831)	-0.0212*** (0.000831)	-0.0216*** (0.000837)	-0.0192*** (0.000827)	-0.0191*** (0.000798)
Household Income					0.0644*** (0.000372)	0.0642*** (0.000372)	0.0594*** (0.000374)	0.0749*** (0.000383)
Number of Children						-0.0228*** (0.000757)	-0.0193*** (0.000759)	-0.0236*** (0.000762)
Religious							-0.00652*** (0.00160)	-0.00244 (0.00159)
Visible Minority							-0.160*** (0.00231)	-0.175*** (0.00235)
Work Hours								-0.00661*** (0.0000530)
Meals								-0.0331*** (0.00142)
Dishes								0.0126*** (0.00132)
Laundry								0.0648*** (0.00145)
Groceries								-0.0629*** (0.00135)
Constant	8.711*** (0.00136)	9.370*** (0.0109)	9.380*** (0.0111)	4.256*** (0.0288)	4.014*** (0.0284)	4.002*** (0.0286)	4.166*** (0.0282)	3.971*** (0.0272)
N	2734178	2734178	2734178	2734178	2734178	2734178	2734178	2734178
R ²	0.014	0.017	0.023	0.251	0.260	0.260	0.262	0.267
Adjusted R ²	0.014	0.017	0.023	0.251	0.260	0.260	0.262	0.267

Robust Standard Errors in Parentheses
* p<0.1 ** p<0.05 ***p<0.01

TABLE A3:
RATIO OF WORK HOURS FOR MEN

Dependent variable:	Life Satisfaction							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ratio of Work Hours	0.0255*** (0.00317)	-0.0505*** (0.00315)	-0.0551*** (0.00318)	0.00152 (0.00288)	0.157*** (0.00290)	0.168*** (0.00289)	0.167*** (0.00288)	0.124*** (0.00290)
Age		-0.0918*** (0.000440)	-0.0906*** (0.000440)	-0.0650*** (0.000410)	-0.0800*** (0.000408)	-0.0777*** (0.000416)	-0.0741*** (0.000417)	-0.0732*** (0.000421)
Age ²		0.00101*** (0.00000470)	0.000992*** (0.00000469)	0.000735*** (0.00000437)	0.000888*** (0.00000434)	0.000855*** (0.00000452)	0.000817*** (0.00000454)	0.000803*** (0.00000457)
Some Secondary			0.0439*** (0.00251)	0.242*** (0.00225)	0.391*** (0.00229)	0.387*** (0.00230)	0.333*** (0.00232)	0.291*** (0.00231)
Highschool Diploma			-0.000930 (0.00236)	0.143*** (0.00217)	0.228*** (0.00215)	0.226*** (0.00215)	0.181*** (0.00216)	0.153*** (0.00215)
Some University			0.0289*** (0.00213)	0.140*** (0.00193)	0.206*** (0.00191)	0.202*** (0.00191)	0.161*** (0.00191)	0.149*** (0.00190)
Diploma/Certificate				0.0859*** (0.00151)	0.138*** (0.00151)	0.135*** (0.00151)	0.0962*** (0.00153)	0.0727*** (0.00152)
Health				0.159*** (0.00491)	0.0969*** (0.00479)	0.0908*** (0.00480)	0.118*** (0.00480)	0.111*** (0.00485)
Health ²				0.00780*** (0.000662)	0.0128*** (0.000648)	0.0137*** (0.000649)	0.00950*** (0.000649)	0.0103*** (0.000656)
Mental Health				0.914*** (0.0109)	0.878*** (0.0104)	0.869*** (0.0104)	0.834*** (0.0104)	0.796*** (0.0105)
Mental Health ²				-0.0385*** (0.000922)	-0.0352*** (0.000883)	-0.0344*** (0.000882)	-0.0311*** (0.000878)	-0.0279*** (0.000884)
Household Income					0.110*** (0.000402)	0.111*** (0.000403)	0.105*** (0.000404)	0.101*** (0.000404)
Number of Children						-0.0181*** (0.000721)	-0.0126*** (0.000731)	-0.0159*** (0.000726)
Religious							0.0887*** (0.00152)	0.0939*** (0.00151)
Visible Minority							-0.189*** (0.00200)	-0.187*** (0.00199)
Meals								0.0552*** (0.00217)
Dishes								-0.146*** (0.00186)
Laundry								-0.191*** (0.00241)
Groceries								-0.190*** (0.00178)
Constant	8.332*** (0.00219)	10.35*** (0.0103)	10.33*** (0.0104)	4.802*** (0.0310)	4.072*** (0.0299)	4.090*** (0.0299)	4.101*** (0.0298)	4.361*** (0.0301)
N	3026349	3026349	3026349	3026349	3026349	3026349	3026349	3026349
R ²	0.000	0.013	0.014	0.199	0.222	0.222	0.225	0.233
Adjusted R ²	0.000	0.013	0.014	0.199	0.222	0.222	0.225	0.233

Robust Standard Errors in Parentheses

*p<0.1 ** p<0.05 *** p<0.01

TABLE A4:
RATIO OF WORK HOURS FOR WOMEN

Dependent variable:	Life Satisfaction							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ratio of Work Hours	-0.716*** (0.00319)	-0.782*** (0.00327)	-0.798*** (0.00328)	-0.729*** (0.00276)	-0.676*** (0.00275)	-0.688*** (0.00275)	-0.695*** (0.00275)	-0.714*** (0.00286)
Age		-0.0334*** (0.000518)	-0.0343*** (0.000517)	-0.0210*** (0.000447)	-0.0407*** (0.000457)	-0.0352*** (0.000469)	-0.0347*** (0.000468)	-0.0344*** (0.000472)
Age ²		0.000438*** (0.00000580)	0.000461*** (0.00000580)	0.000318*** (0.00000500)	0.000523*** (0.00000509)	0.000436*** (0.00000535)	0.000428*** (0.00000534)	0.000426*** (0.00000538)
Some Secondary Education			-0.248*** (0.00329)	0.0448*** (0.00283)	0.209*** (0.00293)	0.192*** (0.00294)	0.165*** (0.00297)	0.170*** (0.00295)
Highschool Diploma			0.0855*** (0.00233)	0.127*** (0.00205)	0.228*** (0.00210)	0.213*** (0.00212)	0.191*** (0.00213)	0.197*** (0.00214)
Some University			-0.190*** (0.00248)	-0.0970*** (0.00201)	-0.0456*** (0.00201)	-0.0508*** (0.00200)	-0.0614*** (0.00201)	-0.0574*** (0.00201)
Diploma/Certificate			0.0344*** (0.00167)	0.0832*** (0.00149)	0.150*** (0.00150)	0.142*** (0.00151)	0.128*** (0.00151)	0.130*** (0.00151)
Health				0.336*** (0.00520)	0.291*** (0.00505)	0.298*** (0.00504)	0.278*** (0.00504)	0.278*** (0.00504)
Health ²				-0.0174*** (0.000699)	-0.0140*** (0.000681)	-0.0145*** (0.000679)	-0.0123*** (0.000678)	-0.0123*** (0.000679)
Mental Health				0.665*** (0.00838)	0.610*** (0.00831)	0.619*** (0.00840)	0.601*** (0.00830)	0.597*** (0.00827)
Mental Health ²				-0.0147*** (0.000724)	-0.00963*** (0.000718)	-0.0106*** (0.000726)	-0.00880*** (0.000717)	-0.00861*** (0.000715)
Household Income					0.0766*** (0.000366)	0.0767*** (0.000366)	0.0719*** (0.000366)	0.0732*** (0.000365)
Number of Children						-0.0442*** (0.000766)	-0.0412*** (0.000767)	-0.0371*** (0.000770)
Religious							0.0135*** (0.00161)	0.0153*** (0.00161)
Visible Minority							-0.176*** (0.00232)	-0.183*** (0.00236)
Meals								-0.0434*** (0.00142)
Dishes								-0.00235* (0.00133)
Laundry								0.0431*** (0.00147)
Groceries								-0.0601*** (0.00134)
Constant	8.795*** (0.00151)	9.394*** (0.0112)	9.417*** (0.0113)	4.590*** (0.0251)	4.406*** (0.0249)	4.369*** (0.0251)	4.512*** (0.0248)	4.540*** (0.0247)
N	2667990	2667990	2667990	2667990	2667990	2667990	2667990	2667990
R ²	0.022	0.027	0.034	0.263	0.276	0.277	0.279	0.280
Adjusted R ²	0.022	0.027	0.034	0.263	0.276	0.277	0.279	0.280
Robust Standard Errors in Parentheses								
*p<0.1 ** p<0.05 ***p<0.01								

*The Effect of Trump's Buy American
Hire American Order on H-1B
and American Workers*

Justine C. Engel

ECON 490

A. REFERENCES

Abramitzky, R., Ager, P., Boustan, L. P., Cohen, E., & Hansen, C. W. (2019). The Effects of Immigration on the Economy: Lessons from the 1920s Border Closure. (NBER Working Paper No. 26536). National Bureau of Economic Research.

American Immigration Council. (2020, April 20). The H-1B Visa Program: A Primer on the Program and Its Impact on Jobs, Wages, and the Economy.

Borjas, G. J. (2003). The Labor Demand Curve is Downward Sloping: Reexamining the Impact of Immigration on the Labor Market. *The Quarterly Journal of Economics*, 118(4): 1335- 1374.

Bound, J., Khannah, G., & Morales, N. (2017). Understanding the Economic Impact of the H-1B Program on the United States. (NBER Working Paper No. 23153). National Bureau of Economic Research.

Clemens, Michael A., Ethan G. Lewis, and Hannah M. Postel. (2018). "Immigration Restrictions as Active Labor Market Policy: Evidence from the Mexican Bracero Exclusion." *American Economic Review*, 108(6): 1468-87.

Cohn, R. L. (2017). Immigration to the United States. *EH.Net Encyclopedia*.

de Brauw, A., & Russell, J. (2014). Revisiting the Labor Demand Curve: The Wage Effects of Immigration and Women's Entry into the U.S. Labor Force, 1960-2010. (IFPRI Discussion Paper No. 1402). International Food Policy Research Institute.

Exec. Order No. 13,788, 82 F.R. 18837 (2017, April 18).

Flood, S., King, M., Rodgers, R., Ruggles, S., & Warren, J. R. (2020). Integrated Public Use Microdata Series, Current Population Survey: Version 8.0 [dataset]. Minneapolis, MN: IPUMS. <https://doi.org/10.18128/D030.V8.0>

Foreign Labor Application Gateway [FLAG]. (n.d.). Labor Condition Application (LCA) Specialty Occupations with H-1B, H-1B1, and E-3 Programs. U.S. Department of Labor.

Gibson, C. & Jung, K. (2006). Historical Census Statistics on the Foreign-Born Population of the United States: 1850-2000. (Population Division Working Paper No. 81). U.S. Census Bureau.

Gogol, F. (2020, June). The True Economic Impact of H1B Visa Holders. Stilt. <https://www.stilt.com/blog/2020/06/the-true-economic-impact-of-h1b-visa-holders/>

Hunt, J. & Gauthier-Loiselle, M. (2010). How Much Does Immigration Boost Innovation? *American Economic Journal: Macroeconomics*, American Economic Association, 2(2): 31-56

Kerr, W. L. & Lincoln, W. F. (2010). The Supply Side of Innovation: H-1B Visa Reforms and US Ethnic Invention. (NBER Working Paper No. 15768). National Bureau of Economic Research.

Mayda, A. M., Ortega, F., Peri, G., Shih, K., & Sparber, C. (2018). The Effect of the H-1B Quota on Employment and Selection of Foreign-Born Labor. (IZA Discussion Paper No. 11345). IZA Institute of Labor Economics.

Ottaviano, G. I. P. & Peri, G. (2012). Rethinking the Effect of Immigration on Wages. *Journal of the European Economic Association*, 10(1): 152-197.

Peri, G. (2012). The Effect of Immigration on Productivity: Evidence from U.S. States. *The Review of Economics and Statistics*, 94(1): 348-358.

Peri, G., Shih, K., & Sparber, C. (2014). Foreign Stem Workers and Native Wages and Employment in U.S. Cities. (NBER Working Paper No. 20093). National Bureau of Economic Research.

Pew Research Center (2016, July 7). 2016 Campaign: Strong Interest, Widespread Dissatisfaction. Pew Research Center. <https://www.pewresearch.org/politics/2016/07/07/4-top-voting-issues-in-2016-election/>.

Pew Research Center (2020, August 13). Election 2020: Voters are Highly Engaged, but Nearly Half Expect to Have Difficulties Voting. Pew Research Center. <https://www.pewresearch.org/politics/2020/08/13/important-issues-in-the-2020-election/>

Preston, J. (2015, November 10). How the H-1B System Can Hurt American Workers. *The New York Times*. <https://www.nytimes.com/interactive/2015/11/06/us/11visa-listy.html?mtref=journalistsresource.org>

U.S. Citizenship and Immigration Services. (2017, March 31). Rescission of the December 22, 2000 "Guidance memo on H1B computer related positions". (PM 602-0142). U.S. Department of Homeland Security.

U.S. Citizenship and Immigration Services. (2017, October 23). Rescission of Guidance Regarding Deference to Prior Determinations of Eligibility in the Adjudication of Petitions for Extension of Nonimmigrant Status. (PM-602-0151). U.S.

Department of Homeland Security.

U.S. Citizenship and Immigration Services. (2018, February 2022). Contracts and Itineraries Requirements for H-1B Petitions Involving Third-Party Worksites. (PM-602-0157). U.S. Department of Homeland Security.

U.S. Citizenship and Immigration Services. (2018, April 26). Memorandum of Understanding Between US Department of Justice Civil Rights Division and the US Department of Homeland Security US Citizenship and Immigration Services Regarding Information Sharing and Case Referrals. U.S. Department of Homeland Security.

Wage and Hour Division. (June 6, 2017). Protecting American Workers. U.S. Department of Labor.

B. APPENDICES

I. Data Manipulations

Due to the size and nature of the Labor Conditions Applications [LCA] datasets, several technical manipulations were necessary. I provide details on the data manipulation process here. The LCA data takes the form of several large Excel spreadsheets. The first step in cleaning up this data was eliminating unnecessary variables and observations. Unnecessary variables are those that do not contribute to the analysis of wage data (for example, information for the attorney helping submit an application, contact information for the point of contact at the hiring company, location data for headquarters of the hiring company, non-primary worksites, and data on prevailing wages). Next, the data was filtered to only include H-1B related applications, as other visa programs also require an LCA, and to only include “certified” (approved) applications. This is because only certified LCAs are allowed to proceed through the H-1B application process. This initial process was done using Excel.

After I standardized wage variables (as is described in Section III), the next step was to append all the separated Excel sheets into one dataset. Once appended, more standardizing was done to match LCA data with the CPS dataset. Industry data for LCAs was already available in NAICS codes; a new

variable was created to generate the standard 2-digit level of aggregation. This transformation makes industry data comparable with the CPS dataset. Next was to standardize state data across years, as some source data for LCAs coded states with two letters and other used the full name (i.e. TX vs. Texas). I used the state codes from the CPS dataset to do this in order to ensure cross-dataset comparability. Next, I generated an education variable for use in comparing education data with the CPS dataset. As all H-1B application must have at least a bachelor's degree, all observations in the LCAs dataset received the same value.

In general, variables that were not present in both datasets and that hold little analytical value were dropped since it is impossible to compare such a variable across groups. With regards to the CPS dataset, variables like sex, employment status, class of worker, and birthplace were dropped. Variables like employer name, occupation title, job title, and date (dd/mm/yyyy) of application decision, only present in LCA data, were also dropped.

II. Results Tables

Results tables for select specifications as mentioned in Section VI are presented starting from the following:

Table 5: Effect of BAHIA on H-1B Workers and Affected Citizens by High and Low H-1B States

VARIABLES	(1) California	(2)	(3) New Jersey	(4)	(5) Florida	(6)	(7) Indiana	(8)
	Change in Average Yearly Wage							
<i>Effects on H-1B workers</i>								
1. H-1B Workers After BAHIA	20,643** (346)	18,899** (335)	18,203** (512)	16,394** (495)	7,641** (427)	7,712** (424)	1,874* (745)	2,686** (678)
2. Immigration Status	43,746** (201)	43,746** (201)	26,523** (285)	29,951** (316)	31,900** (250)	35,925** (283)	33,319** (445)	43,731** (487)
3. After BAHIA Order	4,395** (337)	4,831** (326)	3,747** (503)	4,191** (486)	4,333** (393)	4,631** (390)	5,945** (662)	6,645** (602)
Constant	47,171** (194)	36,134** (970)	49,386** (280)	35,658** (3,467)	41,936** (227)	32,807** (1,658)	42,048** (385)	34,502** (2,525)
Industry Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	852,405	849,834	305,660	304,863	171,871	170,995	56,631	56,262
R-squared	0.199	0.264	0.186	0.256	0.170	0.200	0.150	0.315
<i>Effects on H-1B affected Americans</i>								
4. College-educated H-1B industry Americans after BAHIA	2,808** (835)	2,612** (814)	4,108* (1,650)	3,885* (1,611)	-2,026+ (1,200)	-2,086+ (1,184)	-3,308 (2,180)	-3,307 (2,127)
5. College-educated workers in H-1B industries	46,591** (526)	34,869** (796)	41,348** (922)	31,369** (1,493)	38,675** (742)	30,936** (1,014)	36,822** (1,334)	30,780** (1,785)
6. After BAHIA order	4,395** (291)	4,591** (287)	3,747** (611)	3,970** (604)	4,333** (339)	4,394** (338)	5,945** (457)	5,944** (451)
Constant	47,171** (168)	37,115** (1,110)	49,386** (340)	39,437** (5,728)	41,936** (196)	38,867** (1,991)	42,048** (266)	37,273** (2,056)
Industry Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	53,944	53,058	13,383	13,144	30,292	29,729	13,573	13,291
R-squared	0.208	0.254	0.184	0.231	0.128	0.159	0.089	0.141
H-1B – Citizen Ratio	1.79		2.46		0.54		0.46	

Note: After BAHIA Order includes data from May 2017-February 2020. Control group sample is restricted to non-college educated citizen adults over age 25 working full time in state i. Odd-numbered columns do not include controls, even numbered columns do. H-1B – Citizen Ratio is the ratio of the proportion of the H-1B sample in state i to the proportion of the citizen sample in state i. Standard errors in parentheses.
 ** p<0.01, * p<0.05, + p<0.1

Intrahousehold Determinants of Timely School Attendance in Uganda

Jeanne Legua

ECON 490

This research adds to the series of analytical reports that myself and a team of student surveyors conducted in 2018 on behalf of AFRIPads Ltd., a social enterprise factory based in rural Masaka, Uganda. I would like to thank Sonia Grinvalds and AFRIPads Ltd. for their active support in the development of the 2018 AFRIPads Economic Baseline Survey, and for lending me access to the survey database for the purpose of this research study.

I would also like to thank Tamara Baldwin, Catherine Douglas and the University of British Columbia's Office of Regional and International Community Engagement (UBC ORICE) for supporting our team of student surveyors, and for orchestrating this unique opportunity to be involved in community development fieldwork. Thanks as well to my fellow student surveyor team members – Kim and Leo, for the three months of laughter and learnings; to all of our colleague supporters in AFRIPads – Irene, Dorothy and Marcus; the local government officials of Kitengesa and Lwanmunda for granting us permission to conduct the survey on behalf of AFRIPads; and our local field staff – Aron, Wilson and Sylvia, for their above-and-beyond support to ensure that our survey collection will be a success.

Lastly, I would like to thank Dr. Cesi Cruz for her support and feedback on the preliminary versions of this study, and to my father, Luar Arjay Legua, for being one of my key sources of guidance and inspiration in the formation of this research.

A. REFERENCES

Banerjee, Abhijit, and Esther Duflo. 2011. *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty*. New York: Perseus Books Group.

Chakraborty, Tanika, and Prabal De. 2017. "Mother's autonomy and child's secondary schooling enrollment in Mexico." *Rev Econ Household* 1037-1053.

D'Espallier, Berf, Isabelle Guerin, and Roy Mersland. 2011. "Women and Repayment in Microfinance: A Global Analysis." *World Development* 758-772.

Global Partnership for Education. n.d. *Education in Uganda*. Accessed April 2018. <https://www.globalpartnership.org/country/uganda>

- Hedges, Sophie, Monique Borgerhoff Mudler, Susan James, and David Lawson. 2016. "Sending children to school: rural livelihoods and parental investment in education in northern Tanzania." *Evolution and Human Behavior* 142-151.
- Kaffe, Kashi, Dean Jolliffe, and Alex Winter-Nelson. 2018. "Do different types of assets have differential effects on child education? Evidence from Tanzania." *World Development* 14-28.
- Kaul, Tara. 2018. "Intra-household allocation of educational expenses: Gender discrimination and investing in the future." *World Development* 336-343.
- Lusardi, Annamaria, and Olivia Mitchell. 2014. "The Economic Importance of Financial Literacy: Theory and Evidence." *Journal of Economic Literature* 5-44.
- Madhavan, Sangeetha, Tyler Myroniuk, Randall Kuhn, and Mark Collinson. 2017. "Household structure vs. composition: Understanding gendered effects on educational progress in rural South Africa." *Demographic Research* 1891-1916.
- Mburu, Samuel. 2017. "Effects of livestock herd migration on child schooling in Marsabit District, Kenya." *A Journal of Comparative and International Education* 545-560.
- Moser, Caroline. 1998. "The Asset Vulnerability Framework: Reassessing Urban Poverty Reduction Strategies." *World Development* 1-19.
- Plug, Erik, and Wim Vijverberg. 2005. "Does Family Income Matter for Schooling Outcomes? Using Adoptees as a Natural Experiment ." *Oxford Journals* 879-906.
- Prakash, Ravi, Tara Beattie, Prakash Javalka, Parinita Bhattacharjee, Satyanarayana Ramanaik, Raghavendra Thalinja, Srikanta Murthy, et al. 2017. "Correlates of school dropout and absenteeism among adolescent girls from marginalized community in north Karnataka, south India." *Journal of Adolescence* 64-76.

Roby, Jini, Lance Erickson, and Chanel Nagaishi. 2016. "Education for children in sub-Saharan Africa: Predictors impacting school attendance." *Children and Youth Services Review* 110-116.

Schmidt, Oliver. 2013. "How do low-income parents pay school fees? A diary-based inquiry from Western Uganda." ResearchGate.

Ssewamala, F M, J S Wang, L Karimli, and P Nabunya. 2011. Strengthening universal primary education in Uganda: the potential role of an asset-based development policy. *International Journal of Educational Development*.

Supanantaroeck, Suthinee, Robert Lensink, and Nina Hansen. 2017. "The Impact of Social and Financial Education on Savings Attitudes and Behavior Among Primary School Children in Uganda." *Evaluation Review* 511-541.

Swift-Morgan, Jennifer. 2006. "What Community Participation in Schooling Means: Insights from Southern Ethiopia." *Harvard Educational Review* 339-368.

Uganda Bureau of Statistics. 2017. "Census Report." Kampala, Uganda.

Wimer, Christopher, and Sharon Wolf. 2020. "Family Income and Young Children's Development." *The Future of Children* 191-211.

B. APPENDIX

I. *Cluster Sampling Method*

Faced with limited civic infrastructure for traditional sampling techniques, data was collected through the formation of a geographic cluster sampling method. Upon consultation with the company management, the households surrounding AFRIPads' new factory location was determined to be the survey's population of interest. The Global Positioning System (GPS) coordinates of the factory location were collected, and the geographic population boundary was set to encircle the factory for two (2) kilometers. All the GPS coordinates within the geographic population boundary were determined at a desirable significance level and ten (10) coordinates within the population boundary were randomly selected through a computer software, as seen in Figure 1. With these coordinates serving as the midpoints of each cluster sampling and pursuant of surveying 20% of the geographic population, the radius of each cluster was determined. Using the new company factory's coordinates serving as the population midpoint, the population and cluster boundaries were manually mapped through a geospatial cloud software, as seen in Figure 2. All the households found within the geographic cluster boundaries were surveyed.

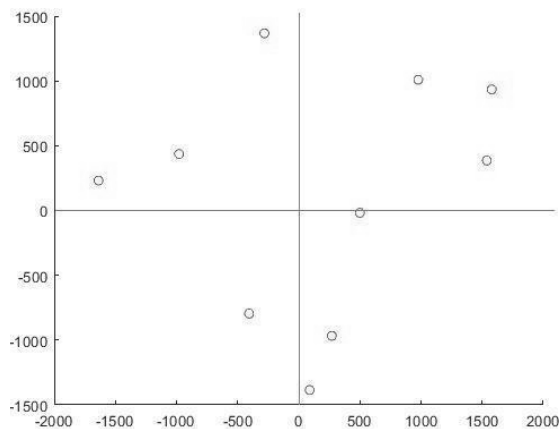


Figure 1. The computer-generated scatter plot showing the center points of the cluster samples. The center point of the graph (0,0) translates to the GPS coordinates of AFRIPads' factory location.

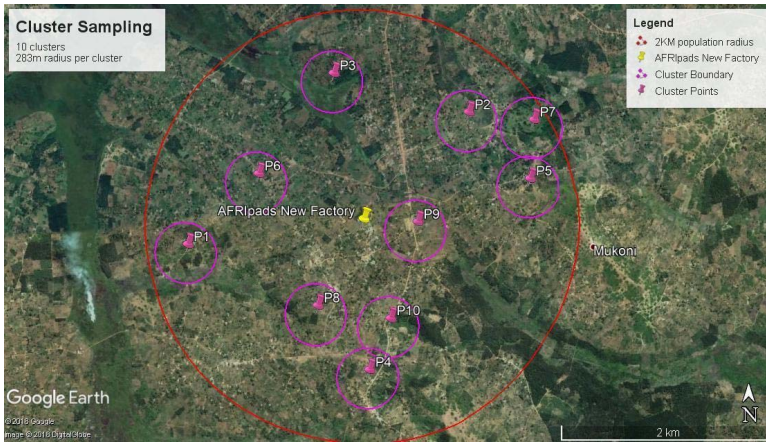


Figure 2. A map generated through Google Earth that shows the boundaries of the geographic population and the ten (10) cluster samples. Found in the center is the new factory location of AFRipads. The map was generated through the tools overlay feature.

II. Survey Questionnaire

Aside from your own housework, have you done any work in the last 12 months? Y / N

[ASK ONLY IF YES] Do you have a full-time job that occupies you for over 30 hours each week? Y / N

[ASK ONLY IF YES] What kind of work do you mainly do?

Are you paid in cash or kind for this work or are you not paid at all?

Cash (1) Not paid (3)

Other (Food, a place to stay, education, et cetera in kind) (2)

Figure 3. Employment Question

How much do you spend on school fees and school items per term?

Total: _____ Fees: _____ Uniform: _____ Transportation: _____ Others: _____

Figure 4. School Expense Question

Identification

1. Name of the Household Head: _____
2. Survey Respondent Sex: Male Female 3. Date of Birth: _____
4. Contact Information: _____ 5. Village: _____
6. How long has the household head resided in Kitengesa / Lwannunda ? ___ years ___ months
7. How many individuals are household members? (Live in household or dependents of members): _____

Household Details:

No.	Name	Age	Sex	Notes
1.			M / F	
2.			M / F	
3.			M / F	
4.			M / F	

Figure 5. Household and Individual Child ID

[Agricultural and Livestock Assets]

Does this household own any livestock, herds, other farm animals, or poultry?

[IF YES] Record numbers ->

- | | | | |
|--|-------|---|-------|
| <input type="checkbox"/> Local cattle | _____ | <input type="checkbox"/> Sheep | _____ |
| <input type="checkbox"/> Exotic/Cross-bred cattle. | _____ | <input type="checkbox"/> Chickens or other poultry. | _____ |
| <input type="checkbox"/> Goats | _____ | <input type="checkbox"/> Pigs | _____ |

Are you growing any of these animals to sell either animal products or the animals themselves? Y / N

Figure 6. Agriculture and Livestock Asset Question

Who usually decides how the money you earn will be used: you, your partner, or you and your partner jointly?

- The woman (1) The partner (2) Together (3)

Figure 7. Financial Head Question

Has (NAME) ever attended school? [Enter respondent number under Y, N]
 Y: _____ N: _____

Did (NAME) attend school at any time during the 2018 school year? [Enter respondent number under Y, N]
 Y: _____ N: _____

What is the highest level of school (NAME) has attended? [Enter respondent number under P,S,H]
 Primary: _____ Secondary: _____ Higher Learning: _____

What is the highest grade that (NAME) completed at that level?

Resp. No. _____ Grade Completed _____	Resp. No. _____ Grade Completed _____
Resp. No. _____ Grade Completed _____	Resp. No. _____ Grade Completed _____
Resp. No. _____ Grade Completed _____	Resp. No. _____ Grade Completed _____

During this school year, what level and grade is (NAME) attending?

Resp. No. _____ Grade Attending _____	Resp. No. _____ Grade Attending _____
Resp. No. _____ Grade Attending _____	Resp. No. _____ Grade Attending _____
Resp. No. _____ Grade Attending _____	Resp. No. _____ Grade Attending _____

Figure 8. Individual Child and Education Data ID

*The Effectiveness of the Menstrual
Hygiene Scheme in Improving Female
Educational and Autonomy Outcomes
in Target Indian Districts*

Manika Marwah

ECON 494

The author would like to thank: her thesis advisor Dr. Siwan Anderson for continued support, guidance, and patience; Dr. Jamie McCasland for support during the early stages of this project and for key information on development research; Lejla Kajevic, Moneet Gill and Serena Meister for their suggestions and editing; Malik Ali (and others) for technical assistance; the BIE program, staff and our entire Graduating Class of 2020 for inspiring and accompanying at every step; and her parents, Capt. Sanjeev Marwah and Rachna Marwah, for unwavering support.

A. REFERENCES

Das, Padma, Kelly K. Baker, Ambarish Dutta, Tapoja Swain, Sunita Sahoo, Bhabani Sankar Das, Bijay Panda, Arati Nayak, Mary Bara, Bibiana Bilung, Pravas Ranjan Mishra, Pinaki Panigrahi, Sandy Cairncross, and Belen Torondel. "Menstrual Hygiene Practices, WASH Access and the Risk of Urogenital Infection in Women from Odisha, India." *PLoS One* vol. 10, 6 (2015). doi:10.1371/journal.pone.0130777.

Davis, J.L., Macintyre, A., Odagiri, Mitsunori, Suriastini, W., Cordova, A., Huggett, C., Agius, P.A., Faiqoh, Budiyani, A.E., Quillet, C., Cronin, A.A., Diah, N.M., Triwahyunto, A., Luchters, S., Kennedy, E. (2018). Menstrual hygiene management and school absenteeism among adolescent students in Indonesia: evidence from a cross-sectional school-based survey. *Tropical medicine and International Health*, 23(12):1350-1363.

De la Rosa, S. 2019. "Report finds 'period poverty' a top reason girls miss school." Education Dive, Feb 3rd <https://www.educationdive.com/news/report-finds-period-poverty-a-top-reason-girls-miss-school/565424/>

Demographic and Health Survey. 2015-2016. "India Survey Data." USAID. https://dhsprogram.com/data/dataset/India_Standard-DHS_2015.cfm?flag=1

Kansal, S., Singh, S., & Kumar, A. (2016). Menstrual Hygiene Practices in Context of Schooling: A Community Study Among Rural Adolescent Girls in Varanasi. *Indian journal of community medicine : official publication of Indian Association of Preventive & Social Medicine*, 41(1), 39-44.

Kaundal, M., & Thakur, B. (2014). A Dialogue on Menstrual Taboo. *Indian Journal of Community Health*, 26(2), 192-195.

Khanna, A., Goyal, R. S., & Bhawsar, R. (2005). Menstrual Practices and Reproductive Problems: A Study of Adolescent Girls in Rajasthan. *Journal of Health Management*, 7(1), 91-107.

Kuhlmann, Anne S., Kashya Henry, and Lewis L. Wall. "Menstrual Hygiene Management in Resource-Poor Countries." *Obstetrical & Gynaecological Survey* vol. 72,6 (2017). 356-376. doi:10.1097/OGX.0000000000000443.

"Menstrual Hygiene Scheme (MHS)." National Health Mission. <https://nhm.gov.in/index1.php?lang=1&level=3&sublinkid=1021&lid=391> (retrieved January 2, 2020).

Mudey, A.B., Kesharwani, N., Mudey, G.A., Goyal, R.C. (2010). A Cross-sectional Study on Awareness Regarding Safe and Hygienic Practices amongst School Going Adolescent Girls in Rural Area Wardha District, India. *Global Journal of Health Science*, 2(2), 225 - 231.

Oster, Emily, and Rebecca Thornton. 2011. "Menstruation, Sanitary Products, and School Attendance: Evidence from a Randomized Evaluation." *American Economic Journal: Applied Economics*, 3 (1): 91-100.

Ray, S., Mishra, S.K., Roy, A.G., Das, B.M. (2010). Menstrual characteristics: a study of the adolescents of rural and urban West Bengal, India. *Annals of Human Biology*, 37(5), 668 - 681.

Sarkar, I., Dobe, M., Dasgupta, A., Basu, R., & Shahbabu, B. (2017). Determinants of menstrual hygiene among school going adolescent girls in a rural area of West Bengal. *Journal of family medicine and primary care*, 6(3), 583-588.

Singh, Pratibha. "How female toilet builders are taking on menstrual hygiene management in India." UNICEF. <https://www.unicef.org/stories/how-female-toilet-builders-are-taking-menstrual-hygiene-management-india> (retrieved January 27, 2020).

Tegegne, T.K., and Mitike, M.S. (2014). Menstrual hygiene management and school absenteeism among female adolescent students in Northeast Ethiopia. *BMC Public Health* vol. 14(1118).

Torondel, B., Sinha, S., Mohanty, J. R., Swain, T., Sahoo, P., Panda, B., Nayak, A., Bara, M., Bilung, B., Cumming, O., Panigrahi, P., & Das, P. (2018). Association between unhygienic menstrual management practices and prevalence of lower reproductive tract infections: a hospital-based cross-sectional study in Odisha, India. *BMC infectious diseases*, 18(1), 473.

Upashe, S.P., Tekelab, T., Mekonnen, J. (2015). Assessment of knowledge and practice of menstrual hygiene among high school girls in Western Ethiopia. *BMC Womens Health*, 15(84).

Waheed, Alia. 2018. "India's sanitary towel hero Pad Man bound for Bollywood glory." *Guardian*, Feb 4th. <https://www.theguardian.com/society/2018/feb/04/india-sanitary-towel-hero-padman-bollywood-glory-arunachalam-muruganatham>.

B. APPENDICES

Appendix A.1 Additional Data Summary Statistics

Table A1: Additional Summary Statistics for Control Variables

Variable	(1) Obs	(2) Mean	(3) S.D.	(4) Min	(5) Max
Sikh	68511	.056	.23	0	1
Buddhist	68511	.002	.04	0	1
Jain	68511	.001	.029	0	1
Jewish	68511	0	.004	0	1
Parsi	68511	0	.005	0	1
No Religion	68511	0	.007	0	1
No Caste	68511	.237	.425	0	1
Married	68511	.305	.461	0	1
Never Married	68511	.69	.463	0	1
Divorced	68511	.001	.039	0	1
Widowed	68511	.001	.037	0	1
Separated	68511	.002	.047	0	1

Note: Responses to these variables are obtained from the Demographic Health Survey (DHS) conducted in 2015-2016. This table includes select descriptive statistics used as controls in my regressions. Each of these variables has been coded as a binary variable, where 'variable'=1 indicates that the respondent belongs to that variable group (i.e. respondent answered yes to the survey question). For these, means can be interpreted as the proportion of the population for which 'variable'=1.

Table A2: Additional Descriptive Statistics on Sample Health Variables

Variable	(1) Obs	(2) Mean	(3) S.D.	(4) Min	(5) Max
Height (in cms)	67944	157.166	66.877	89.4	300
Weight (in kgs)	68488	59.424	111.689	17	180
Body Mass Index (BMI)	67516	20.087	4.218	12.05	50
No previous Births	68511	.847	.36	0	1
One Birth	68511	.132	.339	0	1
Two Births	68511	.02	.141	0	1
Has the Respondent had a hysterectomy	68511	0	.016	0	1
Mild Anemia	68511	.381	.486	0	1
Moderate Anemia	68511	.121	.326	0	1
No Anemia	68511	.469	.499	0	1
Severe Anemia	68511	.011	.103	0	1

Note: Responses to these variables are obtained from the Demographic Health Survey (DHS) conducted in 2015-2016. This table includes select statistics on health. None of these variables were used as controls but have been reported as descriptive measures for the sample population. Height, Weight and BMI are continuous variables. Note that Height, Weight, and BMI were coded so that the 'Max' values can eliminate outliers. All other variables are binary, where 'variable'=1 indicates that the respondent belongs to that group (i.e. respondent answered yes to the survey question). For these, means can be interpreted as the proportion of the population for which 'variable'=1. A note on the variables for height, weight, and Body Mass Index (BMI): Stata appeared not to recognize the units for these variables and dropped decimals. Therefore, both height and weight had to be reparametrized by dividing each variable by 10. BMI has to be reparametrized by dividing the variable by 100.

Table A3: Tabulation of policyON

	Freq.	Percent	Cum.
Control	57,277	83.60	83.60
Treatment	11,234	16.40	100.00
Total	68,511	100	

Notes: this table shows the distribution of district for the instrument variable *policyON*

Appendix A.2 First-Stage Regression for Autonomy 2SLS Process

Table A4: First Stage Regression of Type of Menstrual Technology Used (Pads) with Controls including Education (First stage for regression specification 2.2)

VARIABLES	(1) Tech Used	(2) Tech Used	(3) Tech Used	(4) Tech Used	(5) Tech Used	(6) Tech Used
policyON	0.120*** (0.00504)	0.120*** (0.0265)	0.101*** (0.0212)	0.0440** (0.0189)	0.0622*** (0.0247)	0.0379** (0.0165)
Controls for Religions and Caste	No	No	Yes	No	Yes	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Clustered Standard Errors	No	Yes	Yes	Yes	Yes	Yes
Constant	0.385*** (0.00204)	0.385*** (0.0133)	0.588*** (0.182)	0.134*** (0.00789)	0.499*** (0.0128)	0.229 (0.224)
F-stat	563.90	20.48	.	318.33	12.82	103.48
Observations	68,511	68,511	68,511	68,511	42,936	42,936
R-squared	0.008	0.008	0.045	0.134	0.004	0.110

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The independent variable, *policyON*, is the instrument, and is a binary variable where *policyON*=1 if the district in question is one where the Menstrual Hygiene Scheme was implemented. The dependent variable- Tech Used- is also a dummy variable, indicating whether an individual is using pads as the type of menstrual technology to manage bloodstains during menstruation. This table is the first stage regression output specifically used for the second stage where autonomy and violence indicators are the outcome. For the variable Shared Toilet, it is coded so that when Shared Toilet=1, it means respondents share a toilet with at least one other household. Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, which changes the sample size of col (5) and col (6).

Appendix A.3: Reduced Form Regression for Education; Autonomy, and Violence Indicators

(Note that these controls include controls for education)

Reduced Form Equation for Education:

$$(Years\ of\ Education)_i = \mu_1 + \mu_2(policyON_{i,d}) + \mu_3(controls_i) + \epsilon_{i,d}$$

Table A5: Reduced Form Regression of on Education Outcome- Years of Education

VARIABLES	(1) Years of Education	(2) Years of Education	(3) Years of Education	(4) Years of Education	(5) Years of Education	(6) Years of Education
policyON	0.214*** (0.0439)	0.214 (0.264)	0.187 (0.201)	-0.521** (0.255)	-0.149 (0.293)	-0.379** (0.165)
Controls for Religions and Caste	No	No	Yes	No	Yes	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Clustered Standard Errors	No	Yes	Yes	Yes	Yes	Yes
Constant	9.132*** (0.0178)	9.132*** (0.127)	9.076*** (1.318)	6.556*** (0.194)	10.12*** (0.136)	5.537*** (1.376)
Observations	68,511	68,511	68,511	68,511	42,936	42,936
R-squared	0.000	0.000	0.088	0.158	0.016	0.270

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table is the reduced form output for *policyON*- the instrument, and the outcome of interest- years of education. The independent variable, *policyON*, is the instrument, and is a binary variable where *policyON*=1 if the district in question is one where the Menstrual Hygiene Scheme was implemented. The dependent variable is a continuous variable that measures the years of education achieved by respondent. For the control variable Shared Toilet, it is coded so that when Shared Toilet=1, it means respondents share a toilet with at least one other household. Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, which changes the sample size of col (5) and col (6).

Reduced Form Equation for Autonomy Indicators:

$$[Autonomy_Indicator]_i = \rho_1 + \rho_2(policyON_{i,d}) + \rho_3(controls_i) + \epsilon_{i,d}$$

Table A6: Reduced Form Regression of Autonomy Indicators

VARIABLES	(1) Indicator A	(2) Indicator B	(3) Indicator C	(4) Indicator D	(5) Indicator D
policyON	0.0337 (0.0683)	0.0216 (0.0260)	0.0511* (0.0276)	0.0282 (0.0333)	-0.00682 (0.0350)
Controls for Religions and Caste	Yes	Yes	Yes	Yes	Yes
Controls for Wealth	Yes	Yes	Yes	Yes	Yes
Controls for Marital Status	Yes	Yes	Yes	Yes	Yes
Controls for Toilet Condition	Yes	Yes	Yes	Yes	Yes
Controls for Education	Yes	Yes	Yes	Yes	Yes
Clustered Standard Errors	Yes	Yes	Yes	Yes	Yes
Constant	0.843*** (0.217)	0.857*** (0.134)	0.552*** (0.113)	0.680*** (0.0940)	0.870*** (0.0920)
Observations	181	2,306	2,306	2,306	2,179
R-squared	0.119	0.017	0.019	0.022	0.017

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: This table is the reduced form output for *policyON*- the instrument, and the outcome of interest- five autonomy indicators. The independent variable, *policyON*, is the instrument, and is a binary variable where *policyON*=1 if the district in question is one where the Menstrual Hygiene Scheme was implemented. The dependent variable is different binary autonomy indicator for each column, taking on the value of 1 if the individual has at least some degree of autonomy on the decision in question. For the control variable Shared Toilet, it is coded so that when Shared Toilet=1, it means respondents share a toilet with at least one other household. Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, which changes the sample size of col (5) and col (6).

Reduced Form Equation for Domestic Violence Indicators: $[Violence_Indicator]_i = \tau_1 + \tau_2(policyON_{i,d}) + \tau_3(controls_i) + \varepsilon_{i,d}$

Table A7: Reduced Form Regression of Domestic Violence Indicators

VARIABLES	(1) Indicator 1	(2) Indicator 2	(3) Indicator 3	(4) Indicator 4	(5) Indicator 5
policyON	-0.00254 (0.0171)	0.0157 (0.0195)	0.0207 (0.0226)	0.0304* (0.0155)	0.0253 (0.0184)
Controls for Religions and Caste	Yes	Yes	Yes	Yes	Yes
Controls for Wealth	Yes	Yes	Yes	Yes	Yes
Controls for Marital Status	Yes	Yes	Yes	Yes	Yes
Controls for Toilet Condition	Yes	Yes	Yes	Yes	Yes
Controls for Education	Yes	Yes	Yes	Yes	Yes
Clustered Standard Errors	Yes	Yes	Yes	Yes	Yes
Constant	0.00897 (0.141)	0.122 (0.181)	-0.0599 (0.145)	0.0303 (0.138)	0.0143 (0.143)
Observations	7,669	7,669	7,669	7,669	7,669
R-squared	0.029	0.032	0.033	0.019	0.022

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: This table is the reduced form output for *policyON*- the instrument, and the outcome of interest- domestic violence indicators. The independent variable, *policyON*, is the instrument, and is a binary variable where *policyON*=1 if the district in question is one where the Menstrual Hygiene Scheme was implemented. The dependent variable is different binary domestic violence indicator for each column, taking on the value of 1 if the individual believes a husband beating his wife is justified in the given context. For the control variable Shared Toilet, it is coded so that when Shared Toilet=1, it means respondents share a toilet with at least one other household. Note that only 42,936 out of 68,511 respondents responded to the Shared Toilet question, which changes the sample size of col (5) and col (6).

Appendix A.4 Additional Regression Results for Autonomy and Violence Indicators

Table A8: Second Stage Regression of Autonomy Indicator A- Person Deciding on Spend Respondents' Earnings

VARIABLES	(1) Indicator A	(2) Indicator A	(3) Indicator A	(4) Indicator A	(5) Indicator A	(6) Indicator A
Menstrual Tech Used	0.669 (0.861)	0.669 (0.744)	0.979 (1.358)	1.090 (3.541)	0.714 (0.750)	0.368 (0.767)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.532* (0.283)	0.532** (0.250)	0.292 (1.237)	0.195 (2.372)	0.484* (0.278)	0.806 (0.708)
Observations	337	337	337	337	337	181
R-squared						0.026

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary autonomy indicator taking on the value of 1 if the individual has at least some degree of autonomy on the decision in question. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these autonomy questions were delivered only to a smaller subset of the sample.

Table A9: Second Stage Regression of Autonomy Indicator B- Person who Decides on Respondents' Healthcare

VARIABLES	(1) Indicator B	(2) Indicator B	(3) Indicator B	(4) Indicator B	(5) Indicator B	(6) Indicator B
Menstrual Tech Used	0.337 (0.220)	0.337 (0.253)	0.371 (0)	0.816 (1.217)	0.363 (0.317)	1.014 (1.752)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.521*** (0.0836)	0.521*** (0.1000)	1.073 (0)	0.151 (0.791)	0.510*** (0.136)	0.913*** (0.337)
Observations	3,753	3,753	3,753	3,753	3,753	2,306

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary autonomy indicator taking on the value of 1 if the individual has at least some degree of autonomy on the decision in question. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these autonomy questions were delivered only to a smaller subset of the sample.

Table A10: Second Stage Regression of Autonomy Indicator C- Person who Decides on Large Household Purchases

VARIABLES	(1) Indicator C	(2) Indicator C	(3) Indicator C	(4) Indicator C	(5) Indicator C	(6) Indicator C
Menstrual Tech Used	0.526** (0.242)	0.526* (0.276)	0.580 (0.364)	1.706 (1.994)	0.623* (0.364)	2.394 (3.829)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.395*** (0.0918)	0.395*** (0.109)	1.096*** (0.0634)	-0.473 (1.297)	0.338** (0.157)	0.735 (0.710)
Observations	3,753	3,753	3,753	3,753	3,753	2,306

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary autonomy indicator taking on the value of 1 if the individual has at least some degree of autonomy on the decision in question. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these autonomy questions were delivered only to a smaller subset of the sample.

Table A11: Second Stage Regression of Autonomy Indicator D- Person who Decides on Visits to Family/Relatives

VARIABLES	(1) Indicator D	(2) Indicator D	(3) Indicator D	(4) Indicator D	(5) Indicator D	(6) Indicator D
Menstrual Tech Used	0.439* (0.230)	0.439 (0.336)	0.486 (0)	1.115 (1.755)	0.477 (0.427)	1.324 (2.776)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.462*** (0.0875)	0.462*** (0.130)	1.023 (0)	-0.0351 (1.133)	0.443** (0.181)	0.841 (0.514)
Observations	3,753	3,753	3,753	3,753	3,753	2,306

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary autonomy indicator taking on the value of 1 if the individual has at least some degree of autonomy on the decision in question. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these autonomy questions were delivered only to a smaller subset of the sample.

Table A12: Second Stage Regression of Autonomy Indicator E- Person who Decides on how to Spend Husband's Earnings

VARIABLES	(1) Indicator E	(2) Indicator E	(3) Indicator E	(4) Indicator E	(5) Indicator E	(6) Indicator E
Menstrual Tech Used	-0.00844 (0.228)	-0.00844 (0.356)	-0.0841 (0.463)	-0.435 (1.815)	-0.0599 (0.449)	-0.460 (2.642)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.636*** (0.0879)	0.636*** (0.136)	0.977*** (0.0687)	0.947 (1.191)	0.664*** (0.190)	1.100** (0.528)
Observations	3,526	3,526	3,526	3,526	3,526	2,179

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary autonomy indicator taking on the value of 1 if the individual has at least some degree of autonomy on the decision in question. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these autonomy questions were delivered only to a smaller subset of the sample.

Table A13: Second Stage Regression of Violence Indicator 1- Beating Justified if Wife Goes Out Without Telling Husband

VARIABLES	(1) Indicator 1	(2) Indicator 1	(3) Indicator 1	(4) Indicator 1	(5) Indicator 1	(6) Indicator 1
Menstrual Tech Used	-0.404*** (0.0788)	-0.404*** (0.143)	-0.435** (0.169)	-0.392 (0.305)	-0.408*** (0.156)	-0.0503 (0.336)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.366*** (0.0324)	0.366*** (0.0602)	0.435** (0.169)	0.375* (0.201)	0.366*** (0.0676)	0.0335 (0.446)
Observations	12,038	12,038	12,038	12,038	12,038	7,669
R-squared						0.030

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary domestic violence indicator taking on the value of 1 if the individual believes a husband beating his wife is justified in the given context. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these questions on domestic violence were delivered only to a smaller subset of the sample.

Table A14: Second Stage Regression of Violence Indicator 2- Beating Justified if Wife Neglects Children

VARIABLES	(1) Indicator 2	(2) Indicator 2	(3) Indicator 2	(4) Indicator 2	(5) Indicator 2	(6) Indicator 2
Menstrual Tech Used	-0.215*** (0.0786)	-0.215 (0.151)	-0.165 (0)	0.0683 (0.317)	-0.200 (0.161)	0.311 (0.406)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.327*** (0.0324)	0.327*** (0.0639)	0.165 (0)	0.0923 (0.208)	0.321*** (0.0700)	-0.602 (0.531)
Observations	12,038	12,038	12,038	12,038	12,038	7,669
R-squared			0.006	0.019		

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary domestic violence indicator taking on the value of 1 if the individual believes a husband beating his wife is justified in the given context. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these questions on domestic violence were delivered only to a smaller subset of the sample.

Table A15: Second Stage Regression of Violence Indicator 3- Beating Justified if Wife Argues with Husband

VARIABLES	(1) Indicator 3	(2) Indicator 3	(3) Indicator 3	(4) Indicator 3	(5) Indicator 3	(6) Indicator 3
Menstrual Tech Used	-0.328*** (0.0797)	-0.328* (0.175)	-0.314 (0.208)	-0.157 (0.371)	-0.314* (0.189)	0.410 (0.484)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.368*** (0.0328)	0.368*** (0.0733)	0.314 (0.208)	0.234 (0.241)	0.356*** (0.0811)	-0.765 (0.616)
Observations	12,038	12,038	12,038	12,038	12,038	7,669
R-squared				0.023		

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary domestic violence indicator taking on the value of 1 if the individual believes a husband beating his wife is justified in the given context. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these questions on domestic violence were delivered only to a smaller subset of the sample.

Table A16: Second Stage Regression of Violence Indicator 4- Beating Justified if Wife Refuses Sex

VARIABLES	(1) Indicator 4	(2) Indicator 4	(3) Indicator 4	(4) Indicator 4	(5) Indicator 4	(6) Indicator 4
Menstrual Tech Used	0.0253 (0.0558)	0.0253 (0.107)	0.0799 (0.122)	0.250 (0.260)	0.0465 (0.114)	0.600 (0.382)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.0927*** (0.0229)	0.0927** (0.0440)	-0.0799 (0.122)	-0.102 (0.170)	0.0805* (0.0480)	-0.792 (0.497)
Observations	12,038	12,038	12,038	12,038	12,038	7,669

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary domestic violence indicator taking on the value of 1 if the individual believes a husband beating his wife is justified in the given context. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these questions on domestic violence were delivered only to a smaller subset of the sample.

Table A17: Second Stage Regression of Violence Indicator 5- Beating Justified if Wife Cooks Food Improperly

VARIABLES	(1) Indicator 5	(2) Indicator 5	(3) Indicator 5	(4) Indicator 5	(5) Indicator 5	(6) Indicator 5
Menstrual Tech Used	-0.137** (0.0681)	-0.137 (0.124)	-0.100 (0.150)	0.167 (0.274)	-0.119 (0.133)	0.501 (0.417)
Controls for Religions and Caste	No	No	Yes	No	No	Yes
Controls for Wealth	No	No	No	Yes	No	Yes
Controls for Marital Status	No	No	No	No	No	Yes
Controls for Toilet Condition	No	No	No	No	Yes	Yes
Controls for Education	No	No	No	No	No	Yes
Constant	0.223*** (0.0280)	0.223*** (0.0523)	0.100 (0.150)	-0.0197 (0.181)	0.211*** (0.0576)	-0.692 (0.552)
Observations	12,038	12,038	12,038	12,038	12,038	7,669
R-squared	0.004	0.004	0.015		0.011	

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: the independent variable is the fitted value of Menstrual Technology Used- a binary variable indicating whether the respondent uses pads as the preferred technology to manage bloodstains during periods. This fitted value is calculated from the first stage regression using *policyON* as the instrument. The dependent variable is a binary domestic violence indicator taking on the value of 1 if the individual believes a husband beating his wife is justified in the given context. Col (2) – Col (6) all cluster standard errors by districts, indicating a more robust regression. Note that the number of observations is smaller than the full sample because these questions on domestic violence were delivered only to a smaller subset of the sample.

*Does Membership Come With Benefits?
The Effect of ETFs on Firm Value*

Raphaël Grach

ECON 499

I specifically want to thank Professors Itzhak Ben-David, Francesco Franzoni and Rabih Moussawi for their crucial help throughout the writing of this paper. I also thank Nicolas Ferreira from Raymond James Euro Equities for his support and collaboration and Professor Wendy Kei for her comments in reviewing this paper. Finally, I thank Jaycee Tolentino for his helpful comments in the conduction of the identification strategy.

A. REFERENCES

Andrei, Daniel, and Michael Hasler. “Investor attention and stock market volatility”, *Review of Financial Studies* 28, 2015, 33–72.

Appel, Ian, Todd A. Gormley, and Donald B. Keim. “Identification Using Russell 1000/2000 Index Assignments: A Discussion of Methodologies”. *SSRN Electronic Journal*, 2018. <https://doi.org/10.2139/ssrn.2641548>.

Appel, Ian R., Todd A. Gormley, and Donald B. Keim. “Passive Investors, Not Passive Owners”. *Journal of Financial Economics* 121, no 1 (July 2016): 111–41. <https://doi.org/10.1016/j.jfineco.2016.03.003>.

Ben-David, Itzhak, Francesco Franzoni, and Rabih Moussawi. “An Improved Method to Predict Assignment of Stocks into Russell Indexes”, Working Paper.

“Do ETFs Increase Volatility?” *The Journal of Finance* 73, no 6 (December 2018): 2471-2535. <https://doi.org/10.1111/jofi.12727>.

“Exchange-Traded Funds”, *Annual Review of Financial Economics*, 2017, 23.

Bhattacharya, Ayan, and Maureen O’Hara. “Can ETFs Increase Market Fragility? Effect of Information Linkages in ETF Markets”. *SSRN Electronic Journal*, 2017. <https://doi.org/10.2139/ssrn.2740699>.

Black, Bernard S. “Agents Watching Agents: The Promise of Institutional Investor Voice”. *UCLA Law Review* 39 (s. d.): 85.

Blackrock. “ETP Landscape, Industry Highlights”. December 2014. Retrieved from <https://www.blackrock.com/americas-off-shore/insights/etp-landscape>.

Boehmer, Beatrice, and Ekkehart Boehmer. “Trading Your Neighbor’s ETFs: Competition or Fragmentation?” *Journal of Banking & Finance* 27, no 9 (September 2003): 1667-1703. [https://doi.org/10.1016/S0378-4266\(03\)00095-5](https://doi.org/10.1016/S0378-4266(03)00095-5).

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. “Robust Data-Driven Inference in the Regression-Discontinuity Design”. *The Stata Journal: Promoting Communications on Statistics and Stata* 14, no 4 (December 2014): 909-46. <https://doi.org/10.1177/1536867X1401400413>.

Campbell, John Y. and Robert J. Shiller. “Stock Prices, Earnings and Expected Dividends”. *The Journal of Finance*, 43 no3 (July 1988): 661-676

Cattaneo, Matias D, Michael Jansson, and Xinwei Ma. “Rddensity: Manipulation Testing in Stata”, s. d., 18.

Chang, Yen-Cheng, Harrison Hong, and Inessa Liskovich. “Regression Discontinuity and the Price Effects of Stock Market Indexing”. *Review of Financial Studies* 28, no 1 (January 2015): 212-46. <https://doi.org/10.1093/rfs/hhu041>.

Coval, Joshua, and Erik Stafford. "Asset Fire Sales (and Purchases) in Equity Markets". *Journal of Financial Economics*, 2007, 34.

Crane, Alan D., Sebastien Michenaud, and James Weston. "The Effect of Institutional Ownership on Payout Policy: Evidence from Index Thresholds". *The Review of Financial Studies* 29, n°6, 2016.

Glooner, Simon. "The Effects of Institutional Investors on Firm Outcomes: Empirical Pitfalls of Quasi-Experiments Using Russell 1000/2000 Index Reconstitutions". *SSRN Electronic Journal*, 2018. <https://doi.org/10.2139/ssrn.3180776>.

FTSE Russell. "Russell U.S. Equity Indexes". April 2020. Retrieved from <https://research.ftserussell.com/products/downloads/Russell-US-indexes.pdf?348>.

Guercio, Diane Del, et Jennifer Hawkins. "The Motivation and Impact of Pension Fund Activism", *Journal of Financial Economics*. 1999, 50, 293-340.

Hamm, Sophia J. W. "The Effect of ETFs on Stock Liquidity". *SSRN Electronic Journal*, 2011. <https://doi.org/10.2139/ssrn.1687914>.

Hayashi, Fumio. "Tobin's Marginal q and Average q: A Neoclassical Interpretation". *Econometrica* 50, no 1 (January 1982). <https://doi.org/10.2307/1912538>.

Heejin, Kim and Cho Myungshin. "The Big Short's Michael Burry Sees a Bubble in Passive Investing". *Bloomberg*, 28 August 2019. <https://www.bloomberg.com/news/articles/2019-08-28/the-big-short-s-michael-burry-sees-a-bubble-in-passive-investing>.

Hong, Harrison, Jeffrey D. Kubik, and Tal Fishman. "Do Arbitrageurs Amplify Economic Shocks?". *Journal of Financial Economics* 103, no 3 (March 2012): 454-70. <https://doi.org/10.1016/j.jfineco.2011.10.007>.

Israeli, Doron, Charles M. C. Lee, and Suhas A. Sridharan. "Is There a Dark Side to Exchange Traded Funds? An Information Perspective". *Review of Accounting Studies* 22, no 3 (September 2017): 1048-83. <https://doi.org/10.1007/s11142-017-9400-8>.

Lettau, Martin and Ananth Madhavan. "Exchange Traded Funds 101 for Economists". NBER Working paper (January 2018).

McCrary, Justin. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test". *Journal of Econometrics* 142, no 2 (February 2008): 698-714. <https://doi.org/10.1016/j.jeconom.2007.05.005>.

Modigliani, Franco and Merton H. Miller, "The Cost of Capital, Corporation Finance and the Theory of Investment". *The American Economic Review* 48, n° 3 (June 1958): 261-297.

Morck, Randall, and Fan Yang. "The Mysterious Growing Value of S&P 500 Membership". NBER Working Paper. December 2001. <https://doi.org/10.3386/w8654>.

Romano, Roberta. "Public Pension Fund Activism in Corporate Governance Reconsidered". *Columbia Law Review* 93, no 4 (May 1993): 795. <https://doi.org/10.2307/1122989>.

Siegel, Jeremy J. "The Shiller CAPE Ratio: A New Look". *Financial Analysts Journal* 72, no 3 (May 2016): 41-50. <https://doi.org/10.2469/faj.v72.n3.1>.

Tobin, James. "A General Equilibrium Approach to Monetary Theory". *Journal of Money, Credit and Banking* 1, no 1 (February 1969): 15. <https://doi.org/10.2307/1991374>.

Vayanos, Dimitri, and Paul Woolley. "An Institutional Theory of Momentum and Reversal". *Review of Financial Studies* 26, no 5 (May 2013): 1087-1145. <https://doi.org/10.1093/rfs/hht014>.

Wharton Research Data Services (WRDS). "WRDS Industry Financial Ratio". August 2016. Retrieved from https://wrds-www.wharton.upenn.edu/documents/793/WRDS_Industry_Financial_Ratio_Manual.pdf.

Wurgler, Jeffrey. "On the Economics Consequences of Index-Linked Investing". NBER Working Paper. (September 2010).

B. APPENDICES

APPENDIX 1. Effect of a Russell 2000 assignment on firm value

This effect can be modeled through a fuzzy RD. and would be computed this way:

$$R2000_{i,t} = \delta_0 + \delta_1 \tau_{i,t} + \sum_{n=1}^N \delta_{2,n} (Rank_{i,t})^n + \sum_{n=1}^N \tau_{i,t} \times (Rank_{i,t})^n + \mu_{i,t}$$

The first stage is used to predict the probability of assignment to the Russell 2000 and use it in the second stage:

$$V_{i,t} = \lambda_0 + \lambda_1 R2000_{i,t} + \sum_{n=1}^N \lambda_{2,n} (Rank_{i,t})^n + \sum_{n=1}^N \lambda_{3,n} R2000_{i,t} \times (Rank_{i,t})^n + v_{i,t}$$

Results are reported in table A1. The first stage coefficients are all significant at the 99% level; the effect on R2000 is positive in crossing the cutoff, which is consistent with the paper’s theory. However, it is not possible to say the same from second-stage coefficients. Though some estimates report significance, sometimes at the 5% level, like for the P/B ratio on a bandwidth of 100, significance usually does not survive the other bandwidth test. Most importantly, none of the coefficients display significance at the optimal bandwidth. Identical trends of alternating positive and negative coefficients are observed, notably for the P/E estimates.

APPENDIX 2. Instrumental variable design for the pre-2006 data

One could conduct a similar identification strategy using an instrumental variable design, although it does change the approach and the potential omitted variable bias. ETF ownership and the dependent variable could be driven by the same factors, such as herd behavior leading numerous investors to compulsively buy certain asset classes, like the Internet bubble of the 2000s. To account for that risk, Ben-David et al. (2018) rely on index switchers as instruments, accounting for the effect of jumping from the Russell 1000 to 2000 and vice versa. This effect is used as an instrument to predict ETF ownership, which in turn will be measured against firm value. The first stage 2SLS would be:

$$\begin{aligned} Ownership_{i,t} = & \eta_0 + \eta_1 I(Switch) + \sum_{n=1}^N \eta_{2,n} (\ln(MayCap_{i,t}))^n + \eta_3 \ln(Float_{i,t}) \\ & + \eta_4 \ln(Mkt Cap_{i,t-1}) + \mathbf{HX}_{i,t} + \delta_t + \omega_{i,t} \end{aligned}$$

where $I(Switch)$ is the index switcher instrument.

After predicting values of ETF ownership, the second stage would take the form of:

$$V_{i,t} = \rho_0 + \rho_1 \widehat{Ownership}_{i,t} + \sum_{n=1}^N \rho_{2,n} (\ln(MayCap_{i,t}))^n + \rho_3 \ln(Float_{i,t}) \\ + \rho_4 \ln(MktCap_{i,t-1}) + \mathbf{RX}_{i,t} + \delta_t + \iota_{i,t}$$

This method could not be performed in this paper due to an insufficient number of switches occurring in the available dataset (73 for 3,296 observations). This method, however, also has its limitations. Notably, Appel et al. (2019) explain that using switchers risks capturing the wrong effects, especially if a specific stock near the cutoff switches indexes several times in a row, which is plausible and is one of the reasons FTSE Russell changed its methodology. By switching indexes several times, the company would experience significant shifts in ETF ownership, from a sharp increase when being downgraded to the Russell 2000 to a reversal when upgraded again to the Russell 1000. This punctual change in ownership risks having no significant lasting effect on any metric ETF ownership could impact. This is particularly true when studying non-market outcomes such as corporate governance.

APPENDIX 3. Post-2007 methodology and equations

After 2007, FTSE Russell changed its index attribution methodology to avoid excessive switches between the two indexes from year to year. Since firms near the cutoff are quite similar, their differences in market capitalization could almost entirely be attributed to random variation in price. The introduction of Russell's banding policy makes identification strategy using their indexes more challenging, but not completely impossible. From 2007 onwards, end-of-May market capitalization ranking alone is no longer enough to determine index assignment. Indexation is now built on three rules: (1) market cap ranking, (2) effective presence in the index in the previous year, and (3) whether the firm's May market cap falls within a bandwidth of 2.5% of the cumulative market cap of the Russell 3000E (the Russell 3000 Extended, composed of roughly 3,600 securities). Because of these new rules, identification strategies for causal inference used prior

to 2007 are no longer applicable in this framework and would most likely preclude the use of any form of standard RD designs for these dates (Appel et al. (2019)).

Researchers usually favor a methodology involving an instrumental variable (IV) for this time period. Due to unmatching datasets between Bloomberg and WRDS, the retrieval of ETF ownership was made impossible for this time period. Cross-validation using Russell 3000 ETF shares did not match the data available on the CRPS Mutual Funds Holdings portal.

To properly account for the effect of the new rules, the econometrician would need to include the new components of the assignment rule (Appel et al. (2019)) in the two stage least square regressions. That is, $Banded_{i,t}$ for being within the “banding” zone of the cutoff, $R_{2000,t-1}$ that indicates if the stock was part of the Russell 2000 index the previous year and an intersection of these two variables. The estimation strategy would be to use the Russell 2000 inclusion as an instrument to predict the share of ETF ownership. The first stage of the 2SLS regression would be:

$$\begin{aligned} Ownership_{i,t} = & \gamma_0 + \gamma_1 R_{2000,i,t} + \sum_{n=1}^N \gamma_{2,n} (\ln(MayCap_{i,t}))^n + \gamma_3 \ln(Float_{i,t}) \\ & + \gamma_4 Banded_{i,t} + \gamma_5 R_{2000,i,t-1} + \gamma_6 (R_{2000,i,t-1} \times Banded_{i,t}) + \Gamma X_{i,t} + \delta_t \\ & + \psi_{i,t} \end{aligned}$$

Once ETF ownership is predicted, the second stage is:

$$\begin{aligned} V_{i,t} = & \phi_0 + \phi_1 \widehat{Ownership}_{i,t} + \sum_{n=1}^N \phi_{2,n} (\ln(MayCap_{i,t}))^n + \phi_3 \ln(Float_{i,t}) + \phi_4 Banded_{i,t} \\ & + \phi_5 R_{2000,i,t-1} + \phi_6 (R_{2000,i,t-1} \times Banded_{i,t}) + \Phi X_{i,t} + \delta_t + \xi_{i,t} \end{aligned}$$

C. FIGURES AND TABLES

Figure 1. Evolution of assets under management of ETFs and hedge funds. The figure plots the evolution of assets under management (AUM) worldwide for the two types of institutional investors from 2003 to 2019. Source: The data was retrieved from publicly available information from Statista and BarclayHedge.

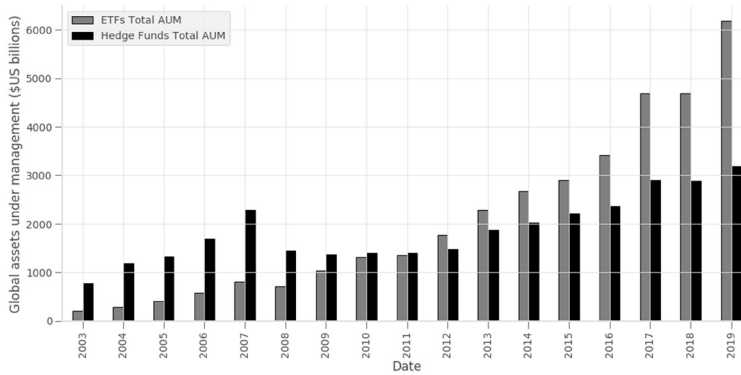


Figure 2. The ETF architecture. The Authorized Participants (APs) act as the arbitrageurs between the ETF Asset Manager and the market. Its role is to arbitrage any difference between ETF shares and the underlying basket of securities. Source: Lettau et al. (2018).

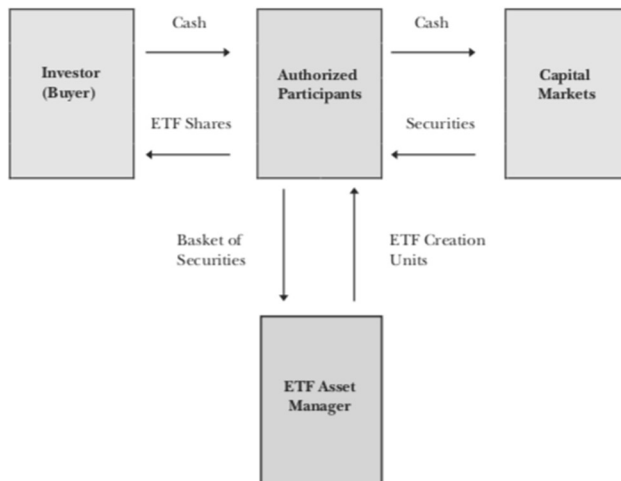


Figure 3. Growth of institutional ownership. It is possible to see that for both the S&P 500 and the Russell 3000, growth in ETF ownership most of the time supersedes growth in other types of funds, with a spike in the early and mid-2000s. Source: Data from Ben-David et al. (2018).

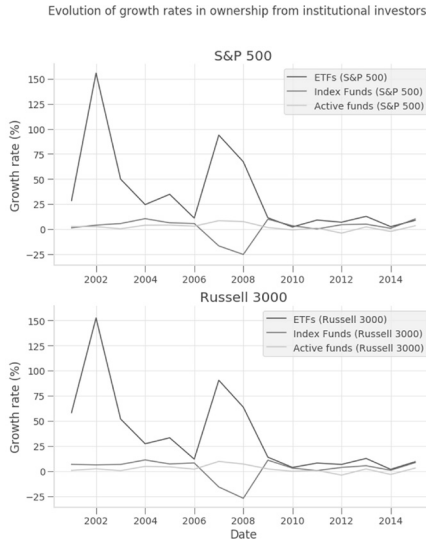


Figure 4. Navistar International and the largest Russell 2000 ETF in May-June 2006. The company had just been included in the Russell 2000 while being in the Russell 1000 the previous year. The blue line represents the closing price while the red and yellow lines respectively represent the highest and lowest prices during the trading day. The dashed line corresponds to the ranking day (31st of May). The orange area represents the difference between the highest and the lowest price in the trading day and can be interpreted as volatility.

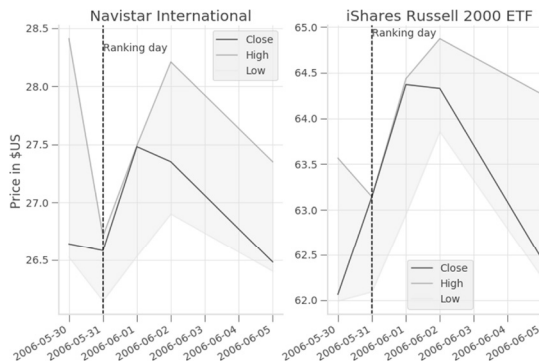


Figure 5. Treatment probability. The figure plots the R2000 assignment (equals to 1 if the firm is in the Russell 2000) against end-of-May market cap rankings. There are a total of 40 bins. Straight lines represent fitted values on both sides of the cutoff.

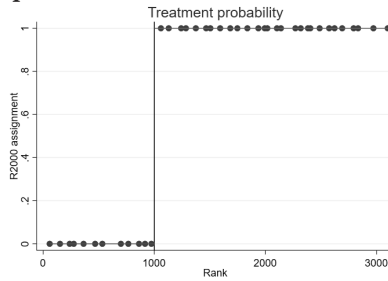


Figure 6. Regression discontinuity designs on the relevant variables. Designs were realized with evenly spaced mimicking variance bins with a triangular kernel centered at the cutoff.

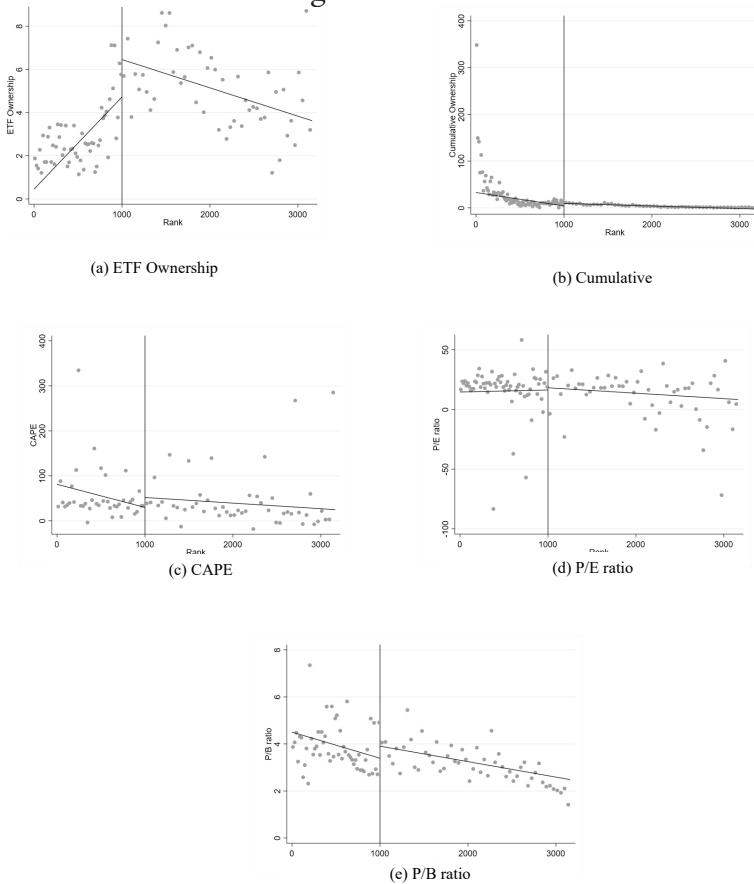


Figure 7. McCrary test plots for different bandwidths. No significant self-selection was observed at the cutoff. Panels A to E represent respectively the optimal bandwidth, then an interval of 50, 100, 200 and 300 observations around the cutoff.

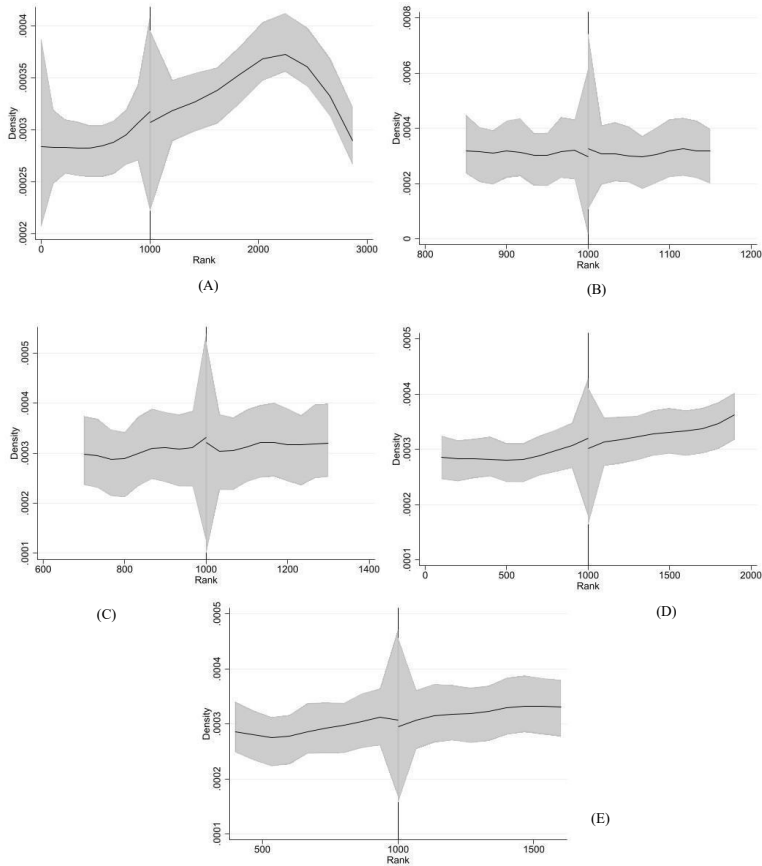


Figure 8. Different fund ownerships around the Russell cutoff. Ownership is measured in percentage. Panel A represents ETF ownership, Panel B index funds, Panel C active funds, and Panel D hedge funds. The average was computed with bins of 10 stocks that have previously been ranked over time. Source: Bend-David et al. (2019).

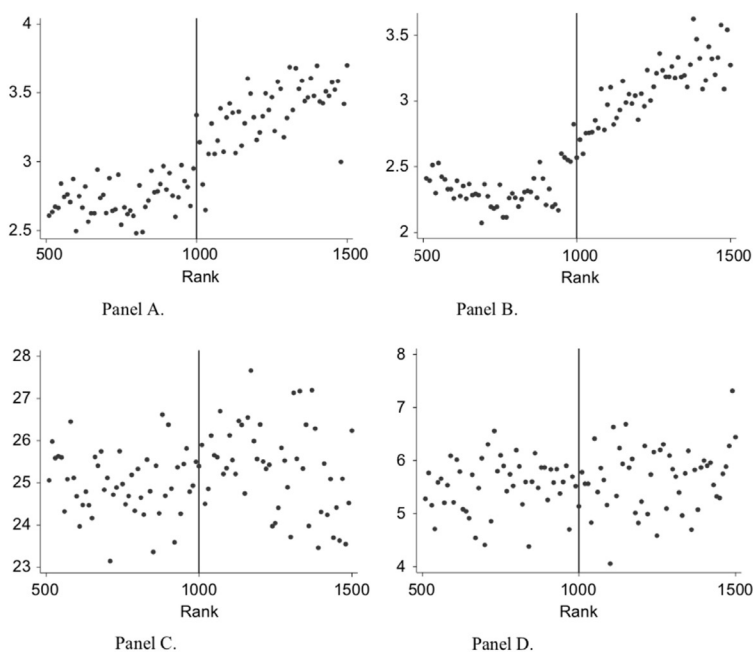


Table I. Summary Statistics of the relevant variables.

Variable	Obs	Mean	Std Dev.	Min	Median	Max
CAPE	3296	44.022	325.959	-1753.700	23.674	10947.200
P/E	3272	13.350	102.668	-1660.001	16.973	600.540
P/B	3236	3.384	3.538	0.425	2.413	34.342
Price	3296	29.983	32.893	1.050	25.245	1312.000
Mkt Cap	3296	4496.436	17335.574	41.460	847.183	3.69e+05
Cumulative	3296	1.11e+07	4.28e+07	1923.720	3.57e+05	1.02e+09
Ownership	3296	4419.976	9075.218	7.114	175.636	1.22e+05
$\ln(MktCap)_t$	3296	7.020	1.371	3.725	6.742	12.817
$1/Price$	3296	0.057	0.056	0.001	0.040	0.952
$\ln(Float)_t$	3296	7.019	1.364	3.806	6.735	12.824
$\ln(MktCap)_{t-1}$	3296	7.026	1.374	3.322	6.742	12.859

Table II(a). OLS regression of ETF ownership on CAPE. This OLS regression uses the Shiller P/E (CAPE) as the dependent variable and ETF ownership as the main independent variable. It then adds a series of controls. A year dummy is added to control for time change and stock fixed-effects are built in. Standard errors are clustered at the stock level.

OLS Regression: Ownership on CAPE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ETF Ownership	-1.147 [2.583]	-1.102 [2.559]	-0.954 [2.552]	-0.957 [2.540]	-0.941 [2.567]	-0.967 [2.603]	-0.961 [2.600]
Year dummy	5.000 [33.865]	6.806 [35.539]	1.714 [32.734]	1.335 [31.905]	0.235 [32.402]	4.267 [46.321]	5.111 [46.046]
Ln(Maycap) _t		-18.302 [93.417]	-620.261 [868.696]	-782.525 [3,394.300]	-765.450 [3,357.379]	-760.295 [3,346.038]	-851.055 [3,348.992]
Ln(MayCap) _t ²			46.218 [65.918]	69.278 [451.367]	70.649 [456.050]	73.722 [460.767]	82.608 [460.660]
Ln(MayCap) _t ³				-1.059 [19.801]	-1.101 [19.964]	-1.223 [20.148]	-1.537 [20.165]
Ln(Float) _t					-31.210 [164.239]	-33.414 [166.183]	-33.185 [166.363]
Ln(MayCap) _{t-1}						-28.351 [181.019]	-26.436 [182.417]
1/Stock price							-205.384 [630.153]
Constant	45.598 [17.984]**	172.624 [649.153]	2,036.796 [2,761.057]	2,407.586 [8,337.754]	2,453.446 [8,463.579]	2,519.528 [8,574.555]	2,819.833 [8,569.082]
R ²	0.80	0.80	0.80	0.80	0.80	0.80	0.80
N	3,296	3,296	3,296	3,296	3,296	3,296	3,296
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in brackets
 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II(b). OLS regression of ETF ownership on P/E. This OLS regression uses the price to earnings (P/E) ratio as the dependent variable and ETF ownership as the main independent variable.

OLS Regression: Ownership on P/E ratio

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ETF Ownership	0.260 [0.564]	0.180 [0.552]	0.193 [0.554]	0.193 [0.555]	0.233 [0.567]	0.227 [0.579]	0.224 [0.579]	0.224 [0.579]
Year dummy	-1.891 [11.091]	-5.115 [11.154]	-5.568 [10.942]	-5.574 [11.058]	-8.439 [11.018]	-7.575 [14.157]	-8.195 [14.582]	-8.195 [14.582]
Ln(Maycap) _t		32.620 [19.840]	-18.924 [122.620]	-21.570 [380.015]	27.111 [386.567]	28.113 [387.144]	92.559 [440.779]	92.559 [440.779]
Ln(MayCap) _t ²			3.956 [9.448]	4.331 [51.930]	7.670 [51.440]	8.347 [51.189]	2.100 [54.807]	2.100 [54.807]
Ln(MayCap) _t ³				-0.017 [2.285]	-0.114 [2.285]	-0.140 [2.263]	0.079 [2.357]	0.079 [2.357]
Ln(Float) _t					-84.275 [69.205]	-84.790 [69.309]	-85.013 [69.104]	-85.013 [69.104]
Ln(MayCap) _{t-1}						-6.099 [48.218]	-7.607 [48.534]	-7.607 [48.534]
1/Stock price							149.224 [387.711]	149.224 [387.711]
Constant	13.514 [6.490]**	-212.886 [138.016]	-53.167 [389.668]	-47.116 [890.913]	71.075 [872.476]	85.730 [880.010]	-128.155 [1,095.810]	-128.155 [1,095.810]
R ²	0.76	0.76	0.76	0.76	0.76	0.76	0.76	0.76
N	3,272	3,272	3,272	3,272	3,272	3,272	3,272	3,272
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in brackets
 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II(c). OLS regression of ETF ownership on P/B ratio. It is possible to note a significant relationship with the logarithmic transform of May market capitalization. However, this effect fades as controls are added.

OLS Regression: Ownership on P/B ratio

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ETF Ownership	0.002 [0.012]	-0.002 [0.011]	-0.002 [0.010]	-0.002 [0.010]	-0.002 [0.010]	-0.003 [0.010]	-0.003 [0.010]
Year dummy	0.011 [0.233]	-0.246 [0.214]	-0.254 [0.217]	-0.271 [0.221]	-0.296 [0.206]	-0.131 [0.270]	-0.115 [0.273]
Ln(Maycap) _t		2.335 [0.716]***	1.012 [3.701]	-7.212 [15.266]	-7.264 [15.337]	-6.476 [15.386]	-8.100 [15.497]
Ln(MayCap) _t ²			0.101 [0.305]	1.259 [2.054]	1.343 [2.096]	1.384 [2.070]	1.536 [2.079]
Ln(MayCap) _t ³				-0.053 [0.085]	-0.056 [0.087]	-0.057 [0.086]	-0.063 [0.086]
Ln(Float) _t					-0.667 [1.264]	-0.707 [1.275]	-0.689 [1.279]
Ln(MayCap) _{t-1}						-1.150 [1.165]	-1.125 [1.157]
1/Stock price							-4.504 [8.377]
Constant	3.366 [0.131]***	-12.846 [5.022]**	-8.710 [10.919]	10.248 [35.068]	12.253 [36.190]	13.438 [35.700]	19.020 [37.137]
R ²	0.93	0.93	0.93	0.93	0.93	0.94	0.94
N	3,236	3,236	3,236	3,236	3,236	3,236	3,236
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in brackets
p* < 0.10, *p* < 0.05, ****p* < 0.01

Table II(d). OLS regression of cumulative ownership on CAPE. No significant relationship is noted despite the consistently negative coefficients.

OLS Regression: Cumulative on CAPE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cumulative	-0.155 [0.209]	-0.157 [0.209]	-0.111 [0.247]	-0.128 [0.294]	-0.123 [0.287]	-0.123 [0.290]	-0.128 [0.292]
Year dummy	-4.589 [25.985]	-1.848 [27.850]	-6.228 [25.117]	-6.648 [22.709]	-7.673 [21.132]	-4.504 [35.796]	-3.403 [34.893]
Ln(Maycap) _t		-21.606 [94.213]	-625.078 [873.371]	-912.384 [3,574.242]	-889.515 [3,526.767]	-886.083 [3,516.864]	-993.406 [3,530.720]
Ln(MayCap) _t ²			46.373 [66.301]	87.275 [479.257]	87.816 [481.489]	90.507 [487.334]	101.239 [488.304]
Ln(MayCap) _t ³				-1.883 [21.207]	-1.886 [21.254]	-1.993 [21.485]	-2.381 [21.542]
Ln(Float) _t					-31.233 [160.178]	-33.130 [162.396]	-32.741 [162.625]
Ln(MayCap) _{t-1}						-23.776 [178.702]	-21.666 [179.987]
1/Stock price							-232.342 [641.839]
Constant	48.950 [17.741]***	198.739 [655.264]	2,065.277 [2,775.419]	2,721.046 [8,722.189]	2,753.641 [8,809.394]	2,811.710 [8,942.957]	3,161.459 [8,975.001]
R ²	0.80	0.80	0.80	0.80	0.80	0.80	0.80
N	3,296	3,296	3,296	3,296	3,296	3,296	3,296
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in brackets
p* < 0.10, *p* < 0.05, ****p* < 0.01

Table II(e). OLS regression of cumulative ownership on P/E ratio.

OLS Regression: Cumulative on P/E ratio

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cumulative	0.043 [0.103]	0.046 [0.101]	0.050 [0.106]	0.053 [0.121]	0.068 [0.127]	0.068 [0.127]	0.071 [0.127]
Year dummy	0.155 [8.509]	-4.067 [8.808]	-4.462 [8.659]	-4.382 [8.736]	-7.101 [8.457]	-6.168 [10.765]	-6.928 [11.379]
Ln(Maycap) _t		33.207 [19.878]*	-19.905 [126.603]	32.940 [423.753]	97.905 [435.866]	98.799 [436.692]	171.521 [486.992]
Ln(MayCap) _t ²			4.080 [9.780]	-3.440 [59.759]	-2.321 [58.926]	-1.506 [58.309]	-8.712 [61.687]
Ln(MayCap) _t ³				0.346 [2.744]	0.355 [2.732]	0.322 [2.696]	0.582 [2.783]
Ln(Float) _t					-85.271 [70.119]	-85.879 [70.336]	-86.206 [70.139]
Ln(MayCap) _{t-1}						-7.038 [47.214]	-8.631 [47.544]
1/Stock price							160.952 [386.308]
Constant	12.767 [6.067]**	-217.450 [138.161]	-53.086 [400.229]	-173.758 [974.740]	-90.275 [943.209]	-72.576 [943.445]	-310.213 [1,149.548]
R ²	0.76	0.76	0.76	0.76	0.76	0.76	0.76
N	3,272	3,272	3,272	3,272	3,272	3,272	3,272
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in brackets
*p < 0.10, **p < 0.05, ***p < 0.01

Table II(f). OLS regression of cumulative ownership on P/B ratio.

The same slightly significant negative relationship is visible; however, it loses its significance as more controls are added.

OLS Regression: Cumulative on P/B ratio

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cumulative	-0.001 [0.001]	-0.000 [0.001]	-0.000 [0.001]	-0.001 [0.001]	-0.001 [0.001]	-0.001 [0.001]	-0.001 [0.001]
Year dummy	0.049 [0.174]	-0.265 [0.153]*	-0.272 [0.158]*	-0.283 [0.161]*	-0.305 [0.150]**	-0.152 [0.211]	-0.133 [0.218]
Ln(Maycap) _t		2.328 [0.714]***	1.003 [3.736]	-8.038 [15.777]	-7.979 [15.821]	-7.198 [15.879]	-8.940 [15.983]
Ln(MayCap) _t ²			0.101 [0.308]	1.376 [2.135]	1.444 [2.172]	1.483 [2.148]	1.650 [2.157]
Ln(MayCap) _t ³				-0.058 [0.089]	-0.061 [0.091]	-0.062 [0.090]	-0.068 [0.090]
Ln(Float) _t					-0.660 [1.270]	-0.702 [1.281]	-0.683 [1.285]
Ln(MayCap) _{t-1}						-1.136 [1.156]	-1.112 [1.149]
1/Stock price							-4.624 [8.358]
Constant	3.357 [0.117]***	-12.793 [5.003]**	-8.655 [11.010]	12.167 [36.128]	13.892 [37.180]	15.087 [36.680]	20.979 [38.089]
R ²	0.93	0.93	0.93	0.93	0.93	0.94	0.94
N	3,236	3,236	3,236	3,236	3,236	3,236	3,236
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in brackets
*p < 0.10, **p < 0.05, ***p < 0.01

Table III(a). Second-stage fuzzy RD results for the effect of ETF ownership on different measures of firm value. The coefficient is computed with several bandwidths using a triangular kernel centered at the cutoff. The data did not have enough variation to the left of the cutoff to allow for varying bandwidths. First stage coefficients were insignificant.

Panel A: CAPE						
Bandwidth:	(1) Optimal	(2) ±50	(3) ±100	(4) ±200	(5) ±300	
$\widehat{Ownership}_{i,t}$	17.29 [286.0]	2.329 [186.4]	859.7 [15229.2]	-13.13 [69.71]	-32.82 [326.8]	
N	3296	3296	3296	3296	3296	
Panel B: P/E ratio						
Bandwidth:	(1) Optimal	(2) ±50	(3) ±100	(4) ±200	(5) ±300	
$\widehat{Ownership}_{i,t}$	-12.11 [377.8]	47.39 [31.24]	-833.7 [175468.7]	-39.55 [85.70]	5.787 [195.2]	
N	3272	3272	3272	3272	3272	
Panel C: P/B ratio						
Bandwidth:	(1) Optimal	(2) ±50	(3) ±100	(4) ±200	(5) ±300	
$\widehat{Ownership}_{i,t}$	-2.982 [14.29]	2.962 [16.80]	8.350 [25.60]	-0.609 [4.434]	-2.063 [19.99]	
N	3236	3236	3236	3236	3236	
Panel D: Higher-order polynomials						
Polynomial of order:	(2)	(2)	(2)	(3)	(3)	(3)
Dependent variable:	CAPE	P/E	P/B	CAPE	P/E	P/B
$\widehat{Ownership}_{i,t}$	165.1 [1043.2]	-79.84 [458.6]	187.7 [8441.7]	42.40 [217.8]	-69.73 [167.9]	0.869 [8.615]
N	3296	3272	3236	3296	3272	3236

Standard errors in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table III(b). Second-stage fuzzy RD results for the effect of cumulative ownership on different measures of firm value. The coefficient is computed with several bandwidths using a triangular kernel centered at the cutoff. Higher order polynomial coefficients are computed using the optimal bandwidth. The data did not have enough variation to the left of the cutoff to allow for varying bandwidths. First stage coefficients were insignificant.

Panel A: CAPE						
Bandwidth:	(1) Optimal	(2) ±50	(3) ±100	(4) ±200	(5) ±300	
$Cumulative_{i,t}$	0.00000689 [0.0000566]	-0.00000294 [0.000111]	0.00584 [0.137]	-0.0000111 [0.0000490]	-0.000465 [0.00155]	
N	3296	3296	3296	3296	3296	
Panel B: P/E ratio						
Bandwidth:	(1) Optimal	(2) ±50	(3) ±100	(4) ±200	(5) ±300	
$Cumulative_{i,t}$	0.00000133 [0.000132]	0.0000261 [0.0000178]	-0.00739 [0.245]	-0.0000211 [0.0000572]	0.000152 [0.000598]	
N	3272	3272	3272	3272	3272	
Panel C: P/B ratio						
Bandwidth:	(1) Optimal	(2) ±50	(3) ±100	(4) ±200	(5) ±300	
$Cumulative_{i,t}$	-2.83e-08 [0.00000380]	0.00000215 [0.0000102]	0.00000338 [0.00000954]	-0.000000649 [0.00000392]	-0.00000667 [0.0000189]	
N	3236	3236	3236	3236	3236	
Panel D: Higher order polynomials						
Polynomial of order:	(2)	(2)	(3)	(3)	(3)	
Dependent variable:	CAPE	P/E	P/B	CAPE	P/E	P/B
$Cumulative_{i,t}$	0.0000880 [0.0000734]	-0.00000740 [0.0000726]	0.00000172 [0.00000245]	0.00000411*** [0.0000135]	-0.00000252*** [0.00000922]	0.00000206*** [6.04e-08]
N	3296	3272	3236	3296	3272	3236

Standard errors in brackets
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table IV(a). Formal covariates test. This table regroups the measures of May market capitalization and its higher order polynomials.

Bandwidth:	(1) Optimal	(2) ±50	(3) ±100	(4) ±200	(5) ±300
$\ln(MktCap)_t$	0.000761 [0.0155]	-0.00240 [0.155]	-0.219 [2.398]	-0.0127 [0.0175]	-0.00200 [0.0252]
N	3306	3306	3306	3306	3306
Bandwidth:	(1) Optimal	(2) ±50	(3) ±100	(4) ±200	(5) ±300
$\ln(MktCap)_t^2$	-0.0488 [0.214]	-0.0358 [2.328]	-3.283 [35.57]	-0.194 [0.251]	-0.0739 [0.362]
N	3306	3306	3306	3306	3306
Bandwidth:	(1) Optimal	(2) ±50	(3) ±100	(4) ±200	(5) ±300
$\ln(MktCap)_t^3$	-0.841 [2.410]	-0.401 [26.21]	-36.87 [395.5]	-2.226 [2.732]	-1.389 [5.218]
N	3306	3306	3306	3306	3306

Standard errors in brackets
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table IV(b). Formal covariates test. This table regroups the additional covariates. Respectively, June float-adjusted market capitalization, lagged market cap and stock size.

	(1)	(2)	(3)	(4)	(5)
Bandwidth:	Optimal	± 50	± 100	± 200	± 300
$\ln(\widehat{Float})_t$	0.0291 [0.121]	-0.0123 [0.156]	0.407 [7.856]	0.0413 [0.108]	0.0314 [0.164]
N	3306	3306	3306	3306	3306

	(1)	(2)	(3)	(4)	(5)
Bandwidth:	Optimal	± 50	± 100	± 200	± 300
$\ln(\widehat{MktCap})_{t-1}$	-0.00570 [0.0817]	0.0181 [0.496]	-0.486 [3.282]	-0.0159 [0.0580]	-0.00203 [0.106]
N	3306	3306	3306	3306	3306

	(1)	(2)	(3)	(4)	(5)
Bandwidth:	Optimal	± 50	± 100	± 200	± 300
$1/\widehat{Stock Price}_t$	-0.00765 [0.219]	0.0118 [0.0397]	-0.0901 [0.896]	-0.00262 [0.0126]	-0.0157 [0.0257]
N	3306	3306	3306	3306	3306

Standard errors in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A1. Second-stage fuzzy RD results for the effect of index assignment on different measures of firm value. The coefficient is computed with several bandwidths using a triangular kernel centered at the cutoff. Higher order polynomial coefficients are computed using the optimal bandwidth. The data did not have enough variation to the left of the cutoff to allow for varying bandwidths. First stage coefficients were significant at the 99% level.

Panel A: CAPE						
	(1)	(2)	(3)	(4)	(5)	
Bandwidth:	Optimal	± 50	± 100	± 200	± 300	
$R\widehat{2000}_{i,t}$	23.14 [19.27]	61.16 [49.99]	52.78* [31.77]	-5.283 [24.80]	15.31 [19.48]	
N	3296	3296	3296	3296	3296	
Panel B: P/E ratio						
	(1)	(2)	(3)	(4)	(5)	
Bandwidth:	Optimal	± 50	± 100	± 200	± 300	
$R\widehat{2000}_{i,t}$	-15.38 [16.01]	57.57* [30.75]	-8.334 [16.50]	-35.79 [28.87]	-13.48 [26.58]	
N	3272	3272	3272	3272	3272	
Panel C: P/B ratio						
	(1)	(2)	(3)	(4)	(5)	
Bandwidth:	Optimal	± 50	± 100	± 200	± 300	
$R\widehat{2000}_{i,t}$	0.0158 [0.669]	-2.757* [1.562]	-2.321** [1.141]	-0.176 [0.922]	0.219 [0.762]	
N	3236	3236	3236	3236	3236	
Panel D: Higher order polynomial						
Polynomial of order:	(2)	(2)	(2)	(3)	(3)	(3)
Dependent variable:	CAPE	P/E	P/B	CAPE	P/E	P/B
$R\widehat{2000}_{i,t}$	13.31 [19.01]	-11.32 [22.87]	0.0568 [0.738]	8.474 [26.68]	-17.93 [15.82]	-0.160 [0.642]
N	3296	3272	3236	3296	3272	3236

Standard errors in brackets

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Authors

i

VOLUME VI



Thomas Matthew Araneta

ECONOMICS

Matthew is an outgoing 5th year BIE student. During his time in the BIE, through the classes he took and the professors he worked for, he realized that he was passionate about development economics and political economy in developing countries. That same passion motivated him to write about his home country, the Philippines, for his 493 undergraduate thesis. For him, his senior thesis class represented the perfect opportunity to apply his passion for economic to his home country which he still holds dear. In the future, he hopes to pursue this passion with further schooling and research.



Anton Buri

ECONOMICS

Anton graduated in May 2020 with a Bachelor of Arts in economics. With a background that includes nonprofit work and time spent abroad, Anton enjoyed taking an economic lens on issues involving international development, trade, and political theory. Anton spent time as an External Director for the Social Enterprise Club, and enjoyed complementing his degree in economics with coursework in computer science and data analytics. Since graduating, Anton joined Venture For America, a US-based fellowship committed to expanding economic opportunities in American cities by connecting recent university graduates with career opportunities with startups. Anton currently lives in Detroit, Michigan where he works full-time with a digital addiction treatment provider that leverages human-centered design and evidence-based practices to provide addiction treatment via telemedicine.



Justine Engel

ECONOMICS

Justine Engel will graduate in May 2021 with a major in Economics and a minor in Spanish. She is keenly interested in the intersection of economics and the law, particularly how immigration systems can be constructed to facilitate social justice and boost economic welfare. During her time at UBC, Justine contributed to multiple Spanish translation projects and participated in the Arts Co-op program where she gained valuable experience working with the Canadian government on immigration issues. To continue her goal of advancing just economic policies, Justine will pursue her J.D. at the University of Texas at Austin School of Law in fall 2021.



Raphaël Grach

ECONOMICS

Raphaël is a fifth-year student that joined the LSE after completing the Honours program in Economics at UBC (Dual Degree with Sciences Po). He is passionate about financial and monetary economics, and new forms of investment. After observing most of his friends investing in the stock market following the rise in popularity of passive investment vehicles such as exchange-traded funds (ETFs), he came up with the idea of writing this paper. Upon graduation from his MSc, he will join the Dual Degree between HEC Paris and Sciences Po. He hopes to work in a field at the crossroads of economic research and professional finance.



Jeanne Legua

ECONOMICS

Jeanne grew up in Manila, Philippines before immigrating to BC, Canada to pursue a degree in Economics and International Relations. Passionate about community engagement and socio-economic development, Jeanne is a strong advocate for working with community from an asset-based approach. In 2018, Jeanne participated in an international service learning placement based in rural Masaka, Uganda, where she helped initiate an economic baseline survey project for AFRIPads Ltd, a renowned social enterprise that produces reusable sanitary products. Jeanne's research adds to the series of analytical reports conducted in the region, and is made possible through the generous support of its community partners. Jeanne graduated in 2020, and is currently working in the BC social service sector as a community engagement lead. Moving forward, she intends to leverage her myriad of experiences with local and international civil society organizations, as well as her academic training in Economic research, as she pursues a career in development public policy.



Ia Mantecón García

ECONOMICS

Ia is a recent Economics and Political Science graduate. She is interested in development economics and public policy, with a particular focus in Latin America. While studying at UBC, Ia worked at the Institute for European Studies and as a Research Assistant on a Uganda-based project that examines how refugee influxes affect media and political discourse in the Global South. It is her work as an RA that cemented her interest in development. She plans to go to graduate school to study international development, ideally in the UK.



Manika Marwah

ECONOMICS

Manika Marwah is a Bachelor of International Economics graduate from 2020. Her research interests include development economics and policy research, particularly focused on her home country India, and development outcomes that target female welfare indicators. This led her to choose the topic of evaluating India's Menstrual Hygiene Policy, and its effect on welfare indicators including education and autonomy, since there is a critical lack of safe, effective menstrual hygiene products provided to a majority of Indian women. Currently, Manika works as a market research analyst at the International Data Corporation, Singapore.



Jessica Wu

ECONOMICS

Jessica is a 2020 graduate from the University of British Columbia's Vancouver School of Economics with a major in Economics, and a minor in Commerce. She became interested in behavioural economics in 2016, after learning the basics of the cost-benefit analysis. Jessica's exposure to the theoretical foundation in economic models and commerce insights such as business finance, strategic management, and organizational behaviour, allowed her to further explore assumptions of utility and profit maximization in comparison to real people's behaviour. Jessica hopes to apply these insights to analyze business strategies and operations to impact how businesses execute decision-making as we are not always just dealing with numbers.

IONA Team

iii

VOLUME VI

EDITORS-IN-CHIEF

FELIPE GROSSO

Bachelor of Arts - Honours Economics and Minor in Philosophy



Felipe is a fourth year student at UBC pursuing a major in Honours Economics with a minor in Philosophy. He is fascinated by the interrelationship between education and the discipline of economics. Felipe wants to explore new ways of thinking about educational systems to decrease educational inequality in his home country of Puerto Rico. After graduation, he plans to look for experiences that will help him develop the skills to research this relationship further.

AKASH UPPAL

Bachelor of International Economics



Akash is in his fourth year of the Bachelor of International Economics program, in which he has been exposed to topics ranging from open economy macro to how behavioural economics can explain decision-making. Through the IONA Journal, he hopes to shine a light on outstanding undergraduate research as he progresses towards a career intertwining economic analysis and policy evaluation.

DIRECTOR OF BUSINESS OPERATIONS

RACHEL LEE

Bachelor of International Economics



As a third-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. One aspect of economics that she is passionate about is how economic efficiency and environmental goals must be balanced. In addition to enhancing her knowledge of environmental economics, Rachel hopes to pursue how values for equality or equity can influence our economic decisions.

MANAGING EDITOR

CECILIA PANG

Bachelor of Arts - Honours Political Science



Cecilia is in her fourth year of a political science honours degree. Her interests within economics focuses on behaviour, public policy, and disruption from technology. Most recently, she worked as an economic policy analyst within the federal economic development agency (WD Canada) and currently, she is researching the effects of digitalization on civic participation. Coming from an artistic background and being an avid writer, she founded ajourney2success.com in 2012 and Art2Heart Foundation in 2014. Outside of school, she can be found running along Kits Beach, curled up with a book, or serving as a WEF Global Shaper.

PULKIT AGGARWAL*Bachelor of International Economics*

Pulkit is a fourth-year student in the Bachelor of International Economics program. He is passionate about development economics and is fascinated by the research process in social sciences. After graduation, he wants to explore the intersection of applied economic research and policy-making in different cultural and economic contexts. Through the IONA Journal, he hopes to learn about the wide variety of economic topics being explored by undergraduates and contribute to the promotion of undergraduate research.

ANNIE CHANG*Bachelor of International Economics*

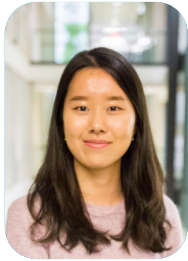
Annie is a third year International Economics student. Specifically, she is interested in how economic policies can play a major role in the subjective well-being of individuals across different countries. Annie is fascinated by the intersectionality of economics, culture and rational decision making.

LIVIA DE OLIVEIRA*Bachelor of Arts - Economics and Political Science*

Livia is in her last year at UBC, studying Economics and Political Science. Although she has enjoyed most of what the fields have thrown at her, she is most fascinated by development economics, and how concerns about income distribution, gender equality and environmental justice can better inform efforts to fix inequities around the world. Through the IONA Journal, she is keen on supporting the exceptional work being done by undergraduate researchers, and highlighting the contributions of the UBC Economics community in general.

PETER JIANG*Bachelor of International Economics*

Peter is a 3rd year student in the Bachelor's of International Economics Program and is ecstatic to be on the editorial board of the IONA Journal. When he isn't trying new teas, or jamming to lo-fi hip hop beats, you can find him scouring through the latest developments on BC's eccentric mining sector. Resource management and 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 an amazing team, and to contribute to this year's publication.

ERIN LEE*Bachelor of Arts - Economics and International Relations*

Erin is in her fourth year at UBC studying Economics and International Relations. She developed an interest in economics through her classes in political economy and international trade. Her current research project is on the effect of household liquidity on the illegal drug trade in the United States. Erin joined IONA Journal to learn from and help highlight the exceptional work done by her peers, and she looks forward to reading this year's edition.

ISHA TRIVEDI*Bachelor of International Economics*

Isha is a third-year Bachelor of International Economics candidate pursuing a minor in philosophy. Her research centers around financial technology, data privacy, and sustainable urbanism. She consciously seeks out diverse opportunities including experience in career services, hospitality management, and the non-profit sector. Following graduation, she hopes to find employment in a flexible and dynamic workplace and continue learning about economics and commerce.

TAURAK UPPAPUTTHANGKUL*Bachelor of International Economics*

Taurak is a third 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.

SARAH WAPPEL*Bachelor of Arts - Honours Economics*

Sarah is a fourth year student in the Honours Economics program with a minor in International Relations. Although she started her undergraduate degree studying Music, she was drawn to economics because of its logical methods and approaches to making sense of a wide variety of areas, from policy to development to decision-making. As part of the IONA team, she wants to help showcase the student community's hard work and gain more insight into the research and publication process. After graduation, she is planning on pursuing further studies in economics at the graduate level.

PRANAV AATREYA*Bachelor of International Economics*

Pranav is a second-year International Economics major, hoping to minor in mathematics. With a strong interest in quantitative economics and sustainability, he hopes to get involved in the field of sustainable investment policy and conscious capitalism. Through the Iona Journal, he intends to get involved with Economics' academic community, both at UBC and beyond.

MEGUMI BERNARDO*Bachelor of International Economics*

Megumi is in her first year at UBC and plans to pursue a major in Economics. Having lived in the Philippines, Singapore, and the United States, she is interested in learning about how the tools of economics can be applied to alleviate poverty and socioeconomic inequality across various countries. Through the IONA Journal, she hopes to gain exposure to different areas of research in the field and share the joy of discovering more about the subject with fellow students.

LUCIENNE CHANG*Bachelor of Arts - Honours English Literature and Economics*

Lucienne is a third-year student double majoring in Honours English Literature and Economics at UBC. Interested in the potential of comparative literature in application to socioeconomic theory, she hopes to research further into how different expressions of art result from broader economic influences. Through the IONA Journal, she hopes to study a multitude of fields under the overarching umbrella of economics while working alongside a fantastic, dedicated team. In preparing for her own future, Lucienne hopes to continue on in academia specifically in her designated disciplines.

MAHAK DUGAR

Bachelor of Arts - Major in Economics and Minor in Environment & Society



Mahak Dugar is a third-year student, majoring in Economics with a minor in Environment and Society. Growing up in India, she noticed the disastrous repercussions that fast-paced economies have on the planet. Therefore, she chose a field where the intersectionality of her disciplines will allow her to gain a deeper understanding on sustainable economies and how they are developed. In the future, Mahak hopes to shift economic dynamics towards the protection of the environment through concrete policy and research opportunities.

FLORENT DUSENGE

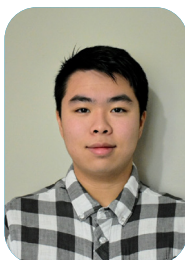
Bachelor of International Economics



Florent is a second-year student in the the Bachelor of International Economics program. After graduation, he hopes to immediately pursue a master's degree in Economics or Business Administration.

PIUS LAU

Bachelor of International Economics



Pius is a first-year UBC student in the Bachelor of International Economics program. He is fascinated by the economic way of thinking and the way it plays into everyday life. He hopes to explore how an economic way of thinking can improve the everyday lives of people all around the world. Through the IONA Journal, he is excited to explore the incredibly diverse field of economics and help promote new, exciting research.

VALENTINA RAMIREZ*Bachelor of International Economics*

Valentina is a first-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, she has witnessed the positive externalities that these projects have upon our generation, and the future to come.

ANGELA SEQUEIRA*Bachelor of Arts - Combined Major in Economics and Political Science*

Angela is a third-year student at UBC pursuing a Combined Major in Economics and Political Sciences. Her primary interests range from policymaking within the international political economy to equity distribution in developmental economics. At IONA Journal, Angela hopes to delve into the dynamics behind economic analysis and gain exposure into the rigorous field of undergraduate research. She looks forward to furthering her knowledge of the diverse field and expects to continue her learning journey after graduation through a career integrating empirical research and welfare economics.

SONYA SULA*Bachelor of International Economics*

Sonya is a third year student in the Bachelor of International Economics program pursuing a minor in French. She is interested in the economics of the environment and the considerations behind choosing environmental policy instruments. Her research interests continue to evolve as she is exposed to more topics. As a member of the IONA journal, she looks forward to playing a role in showcasing the exceptional research of her undergraduate peers.

ASHUTOSH VERMA*Bachelor of International Economics*

Ashutosh is a second-year student in the Bachelor of International Economics program. He is an avid reader who loves discussions on finance, equality, and Indian mythology. Having co-authored two peer-reviewed papers in Economics, he hopes to help others avoid some of the challenges he faced as well as increase his fluency with contemporary economic issues and novel research ideas. Upon graduation, Ashutosh wants to work towards improving the socio-economic situation in his country, India.

SAM VOLPÉ*Bachelor of International Economics*

Sam is a third year student in the BIE program. With a passion for development economics and poverty alleviation, Sam is particularly interested in economic policies that improve quality of life for people struggling at the bottom of the income distribution. She is excited to learn more about the effectiveness of various approaches by critically engaging with contemporary economic research in her role as Junior Editor. You can also ask her about the Women in Economics and Policy Club, where she currently acts as Co-President!

JIN WANG*Bachelor of Arts - Honours Economics and Minor in International Relations*

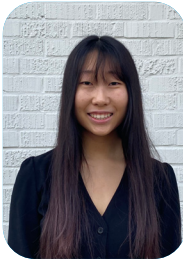
Jin is currently a third-year student in the Economics Honours Program with a minor in International Relations. After realizing that topics such as diplomacy, trades, businesses, international peace and security all essentially revolve around Economics, Jin has decided to dive into learning about the subject. Inspired by the doctrines of Economics and motivated by the passion for improving living standards in the developing countries, Jin hopes to use the knowledge obtained from her study at UBC to contribute to the formulation of effective public policies in the future.

JUNIOR EDITORS

CARRIE WU

Bachelor of International Economics

Carrie is a third-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 VI of the journal and hopes that others will find this resource invaluable.



AMY YE

Bachelor of Commerce

Amy is a third year student at the UBC Sauder School of Business, who is specializing in finance. Through her university courses, she has been exposed to a wide variety of topics in economics, which she has been fascinated by. As part of the team at the IONA Journal, Amy is excited to apply the knowledge that she has gained from her coursework and develop a deeper level of insight into the events occurring within the field of economics.

