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Content

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Editor's Note

Dear BER Reader,

In 2016, the Berkeley Economic Review embarked on its inaugural journey, marking the start of a scholarly endeavor dedicated to showcasing top undergraduate economics research. Since our inception, we have remained steadfast in our commitment to publishing innovative and impactful contributions while nurturing the talents of the economists of tomorrow. Today, BER stands united with over 90 dedicated members, receiving hundreds of submissions and captivating tens of thousands of readers from diverse corners of the world.

Amidst a backdrop of dynamic global shifts, encompassing geopolitical challenges, economic uncertainty, and societal transformations, we commend the unwavering dedication of our authors and team. Despite the complexities we face, we find inspiration in the progress made in critical areas such as sustainable energy and strides towards a more inclusive society.

The arduous yet fulfilling task of our Peer Review team has been to identify exceptional undergraduate research that addresses pressing issues of our time. From delving into the complexities of urban slum growth in Peru to examining the ramifications of environmental policies like the Clean Air Act on equity, these papers delve deep into fundamental questions of fairness and justice that persist in our world. Additionally, we revisit our insightful conversation with Ted Miguel featured in our very first issue, tracing the evolution of his research in parallel with BER's journey since its inaugural issue.

With great enthusiasm, we are thrilled to unveil the 12th volume of the Berkeley Economic Review, a testament to the enduring spirit of scholarly inquiry and the pursuit of excellence.

Sincerely,

Pallavi Murthy & Larry Lin
Editors-in-Chief
Berkeley Economic Review

Ted Miguel

Interviewed by
Griffin Shufeldt & Dhoha Bareche

OPENING INFO: *Ted Miguel is the Oxfam Professor of Environmental and Resource Economics at UC Berkeley. He is a founder of CEGA, and his research focuses on development economics, particularly in sub-Saharan Africa. Two Berkeley Economic Review members got the chance to ask him about his background and research earlier this year.*

Shufeldt: So to start us off, you were interviewed by us all the way back in 2016, when this journal had just started. In that interview you mentioned that you were always interested in international development and it was just a question of how to pursue that, whether it be law, engineering or political science. Could you talk a little bit about how you landed on economics?

Miguel: Yeah, it's definitely the case that even when I was a teenager, I was really interested in global poverty and equality, international relations, all those kinds of issues. I wasn't sure exactly how to pursue that. When I was in undergrad, I thought I might do something in environmental engineering. I thought I might do something in political science. You mentioned even international law, for a while I was interested in that. I think for me, since I really liked math and kind of statistics and those kinds of technical issues, economics felt like a natural fit because I could work in this area and then also use some of those tools and skills. So I think maybe that's part of what attracts a lot of people to economics is you can use a bunch of math skills, coding skills, and whatnot, and use those technical skills to tackle hard problems in international development. And then I was very lucky because when I was an undergrad, I started working as a research assistant for Michael Kramer, who ended up being my co-author, my advisor in grad school. I was really inspired by the kind of work he was doing, so that nudged me in that direction of doing economics.

Bareche: You've mentioned previously that when you decided to do development economics, it was a relatively new field. How have things changed since you were in undergrad? How do you see the field moving forward in the future?
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Miguel: Development Economics within US universities and in a lot of universities globally was just a small field in terms of the research output, the number of faculty and the number of students, and part of it was due to limited data. Historically there just weren't as many good data sets to use; that limited the research. But the field has changed so much since then and it has grown a ton. It's one of the biggest fields here in Berkeley among our PhD students and there's a lot of interest in the undergrad classes with multiple faculty doing it. So I think the field has grown and it'll continue for years to come because on some level we're way below what steady state development economics should look like. When I started grad school–this was in the nineties–seventy to eighty percent of the world was living in low and middle income countries, but in a department there might be one person doing development economics out of 20 or 30. People's life experiences globally just weren't being represented at all in economics departments, so I think development economics growing is part of that growing representation of more of the world and I think it'll just continue to and maybe there'll be twice as many development economists in 10 years as there are today.

Bareche: Another follow up to that, is there a reason why you decided to study sub-Saharan African development instead of, let's say, Latin American development?

Miguel: I think for me it really was an intellectual decision. In my first year of grad school, I was asking myself exactly that question, what region should I focus on? I had different opportunities to do some research work in South Asia, Latin America or Sub-Saharan Africa. And actually a very influential thing for me is in my first year of graduate school, I was a grad student at Harvard at that time, Jeffrey Sachs was a professor at Harvard, and at the time he had started doing work in Sub-Saharan Africa. He was doing more research, and he gave a talk to first year grad students about his research. And after hearing his talk, I said, okay, I'm gonna do research in Sub-Saharan Africa. It was that simple. Hearing his arguments about [Africa] is the poorest continent, has the worst health problems, has so much political violence, there's so much need to understand what's going on, but there's been so little research on the region comparatively. That combination of real need and very little research was attractive for me intellectually. That summer, after my first year in graduate school, I went to Kenya to work with Michael Kramer. And that was the summer I set up the deworming project and I just had a great experience professionally, personally, and, and then I continued on that line of research.

Shufeldt: Pivoting a little bit to transparency. You wrote an article along with a few others about measuring how transparency has changed over time within economics, whether that be like posting code or instruments. In different subfields, across labor or development or economic theory, there's been like a really positive increase in transparency. One measure of transparency that has kind of lagged behind the others is pre-registering hypotheses. Why do you think this may be?

Miguel: It's lagged, although even in the last year or two since I wrote the article you're referring to, the numbers have really taken off a lot. So I think it has lagged, but it's changing. Within development economics now, pre-registering studies has become really standard. Papers in other fields and economics less so. So it's kind of something that development economists are doing and some experimental economists, people who do lab experiments, but not really outside those fields. Some of it has to do with the experimental nature of research. There's RCTs and in experimental research and development, pre-registration is a very natural thing to do for experiments because you have to design the experiment in advance, and so it's natural to register it. For some other studies where people are using administrative data, they often don't feel the need to pre-register, even though it would be very useful to do so. So I think that's been the big reason. You talked about why it has been slower? I think that's been the big divide. Researchers doing observational research, non-experimental research, have been slower to adopt the tool, although some are starting to. I'm not sure how much pre-registration will take off outside of experimental work in economics. I think it's still unknown at this point.

Bareche: Economics has a bad rep for taking a technical and narrow minded approach to certain poverty and other social issues. What's your take on this critique? Does modern day economics do a better job at taking an interdisciplinary approach to the problems facing our world today?

Miguel: I think it's kind of the traditional view or traditional critique of economics that it's just narrow and ignores social factors. Maybe one thing you've seen in ECON 172 is how contemporary development economics is actually pretty interdisciplinary and more attention is paid to issues that I think non-economists don't realize economists are working on. Sometimes when I present to non-economists, whether here on campus or more broadly, the kind of characterizations that are made about what economists do or how economists do research are kind of outdated. So I do think economics has changed a lot. I think the fact that a place like CEGA (Center for Effective Global Action) here from its start dedicated itself to do research on international development in an interdisciplinary way and has affiliates from seven or eight different disciplines who are really active in the center kind of speaks to the contemporary research enterprise being broader than the kind of caricature of what economics is. So I'm proud of that. I'm proud of what we built at Berkeley and again, hopefully in ECON 172 between the historical work and political work and other things, you guys get a sense that development economists are broader than we are sometimes accused of being.

Shufeldt: I think one concern, in economic research in general, is external validity. Especially in development, where a lot of regression discontinuity is used, and we're estimating a lot of the local treatment effects. How can findings in Mozambique for example, be implemented in Nigeria. How can the external validity concerns be addressed?

Miguel: When we're taking maybe a very specific local finding and applying it to a broader setting, maybe somewhere very far away, that's a great point. And, and I think it's something that, not just our research in economics or development or people who use RCTs, but all research is subject to this critique. I mean, qualitative research is subject to that critique. Almost any research, whether it's experimental, observational, quantitative, qualitative economics, sociology is gonna be based on a certain population, and then it's pretty hard to know how those results travel. So I think it's a fundamental research problem. There is some research in what's called meta-analysis or meta research that does find treatment effects estimated in one setting still do have predictive power in other settings. It isn't like there's no external validity. So you were saying, well, what if there's a study in Madagascar or Mozambique and you want to take it to Nigeria? Odds are if there was a big positive effect in Mozambique, there'll still be some sort of positive effect in Nigeria. We don't know for sure, but at least there's some evidence suggesting that. It does speak to the need for more research. You mentioned Nigeria. Nigeria is a country with 200 million people and there's still not enough research on what works and what policies do in Nigeria. So I would say another answer to this is more research in these big important settings. Then we don't have to worry as much about external validity because we'll have that data.

Bareche: What type of research are you working on right now?

Miguel: I'm working in a few different areas. I'll just mention one thing: I'm working in four or five different areas. We've been studying the effects of cash transfers and we've published some work on this, like what are the effects of cash transfers, which have become pretty popular in low and middle income countries in recent years, on the local economy. What we're doing right now is measuring how cash transfer effects spread out over space and studying those effects over time. We're finding some pretty substantial persistence in the benefits of big cash transfer programs–not just for the recipients themselves, but for the local economy. So that's one of the things I'm really excited about because it is really actionable in terms of policy, and at the same time, the economics of it and the theory is really interesting. So it's kind of like the sweet spot for me in terms of my research.

Bareche: Where do you see the field of developmental economics going in the future?

Miguel: I think for me, the ideal project is something that is really intellectually challenging but also important for policy. If I can work in that intersection, then I'm really happy with my research.

We thank Professor Ted Miguel for sharing his research and insight into the future of the field of Development Economics.

Slum Growth in Peruvian Districts

Pulkit Aggarwal[1]

Working Paper - February 2022

Abstract

Rapid urbanization in the developing world has brought different challenges from the historical urbanization experience of today's developed countries. One important consequence of "poor-country urbanization" is the growth and persistence of slums – informal housing characterized by low amenities and low standard of living. This paper presents empirical evidence on the relationship between slum growth and urban wage growth in Peru by exploiting variation in employment and wages across industries and space over time. I compute an exports-growth Bartik-style (Shift-Share) instrument to isolate exogenous variation in wage growth across districts in Peru between 2004 and 2019 and use a cross-sectional first-differences model to estimate the elasticity of slum growth to urban wage growth. I estimate an elasticity of -0.94 for growth in slum population (opposite in direction from estimates in the literature) but no effect on growth in slum population as a share of urban population (unlike global trends).

Keywords: slum growth, wage growth, urbanization, bartik instrument

JEL Codes: F63, O18, P25, R21, R31

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Content

1 Introduction

Historically, the largest urban cities of the world have been in rich countries. This trend has gradually reversed in the post-war period due to what has been called "poor-country urbanization" (Glaeser, 2014). In 2015, the list of the top 30 cities with more than 10 million inhabitants (or "megacities") was dominated by cities in developing countries and these cities are projected to grow at a faster rate than cities in the rich world between 2015 and 2030 (UN, 2014). However, the urbanization experience in the developing world has been different from the historical urbanization experience of today's rich world (Chauvin et al., 2017; Bryan et al., 2020). For example, city size used to be a strong indicator of living standards but this relationship has broken down over time (Jedwab and Vollrath, 2019).

One important consequence of rapid urbanization has been the growth and persistence of slums – informal housing characterized by crowding, poor quality housing, and low standards of living. Slum residents have poor health and educational outcomes (Galiani et al., 2017), lower levels of public services (Galiani et al., 2017), reduced incentives to invest in housing (Field, 2007; Nakamura, 2017), and greater exposure to crime (Felbab-Brown, 2011). Presence of slums also has societal costs by contributing to traffic congestion (Fernandes, 2011), groundwater pollution due to inadequate sanitation in slums (Nyenje et al., 2013), and overall lower productivity (Cai et al., 2018). In some cases, poor living conditions in slums can lead to non-monetary disutility of living in slums in the form of human capital outcomes that are worse than in rural areas (Marx et al., 2013). This can cause rural residents to forgo higher consumption of urban areas which contributes to spatial misallocation of labour (Lagakos et al., 2018).

Urban poverty may seem preferable to rural poverty, as shown by the revealed preference of migration from rural areas to slums in the hope of the opportunities afforded by proximity to the city. This hypothesis supports the "modernization" theory where slums are a transitory phenomenon in the process of rapid growth which will give way to formal housing as economic growth becomes widespread (Glaeser, 2011). However, evidence supporting this theory is ambiguous at best. Marx et al. (2013) show that mobility out of slums is very low, even intergenerationally and over decades, which is at odds with the view that slums are transitory. Additionally, Gollin et al. (2016) show that urban income growth in many developing countries come from exports of capital-intensive industries such as natural resources, which increase inequality and do not improve urban living conditions.

Taken together, these facts are more supportive of the hypothesis that slums are urban poverty traps rather than transitory phases of being in the land of opportunity of cities (Marx et al., 2013). This distinction is important from a policy perspective – considering slums as transitory phases justifies the lack of active government intervention. However, acknowledging slums as urban poverty traps provides strong rationale for interventions that reduce barriers to formalization or improve living conditions in slums (commonly known as "slum upgrading policies"). Understanding the factors behind the growth and persistence of slums is then important to both academics and policymakers. This paper investigates the role of one such factor: urban wage growth.

Two trends in global slum growth are noteworthy. First, the absolute number of slum households has been rising, reaching 880 million in 2014 (UN, 2015). A key source of heterogeneity in this trend is that economically dynamic cities (i.e., cities experiencing substantial growth) have experienced higher rates of slum growth (World Bank, 2009). Second, slum population as a share of urban population has been declining, falling from 39% in 2000 to 30% in 2014 (UN, 2015). This paper investigates both these trends in the case of Peru by asking: what is the elasticity of slum growth to urban wage growth in Peru between 2004 and 2019?

To deal with endogeneity concerns of reverse causality and classical measurement error, I use an instrumental variable approach to isolate exogenous variation in wage growth. The main empirical strategy consists of regressing 2004-2019 slum population growth on 2004-2019 urban wage growth instrumented using an exports growth Bartik-style instrument at the district level. To argue for the validity of the constructed Bartik-style instrument in the Peruvian context, I apply the frameworks proposed in Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2020), two recent methodological papers investigating the theoretical robustness of such instruments.

I estimate an elasticity of -0.94 for slum population growth to urban wage growth (significant at the 10% significance level). The magnitude of this elasticity increases to -1.33 when calculated for only low-wage households (significant at the 5% significance level). The direction of these estimates is unexpected because they imply that higher wage growth in a district causes a decline in the district's slum population, unlike the findings of Alves (2021) who finds a positive elasticity of 1.7 for low-wage workers due to high wage-induced migration but lack of responsive formal housing markets across Brazilian cities. Estimates in the opposite direction suggest potentially elastic formal housing markets in Peru. While I do not consider the housing market in my model and estimate the parameter in only a partial equilibrium model, I discuss my estimates in the context of the interrelated location choice slum growth models presented in Cavalcanti et al. (2019) and Alves (2021) and suggest potential mechanisms. Alternatively, available data (both qualitative and quantitative) suggest that a more likely mechanism specific to Peru is investments into better housing quality by slum residents themselves as a result of wage growth.

This paper contributes to the literature on the determinants of slum growth. My estimates should be understood as part of a general equilibrium model for Peru (as in Alves (2021) for Brazil). The surprising negative elasticity between slum growth and wage growth also raises questions about how the Peruvian labour and housing markets operate differently from those in other developing countries, especially for low-wage earners. In the absence of a formal general equilibrium model for Peru's labour and housing markets, the mechanism of slum decline I argue for – housing investments by slum residents – also points to a potential path out of urban poverty traps.

I conduct two robustness checks. First, I estimate my primary specification using data trimmed at the 5% level on both sides of the distribution for the outcome and independent variables. Second, I winsorize the data the 5% level instead. My estimates are robust to trimming and winsorizing the data for unconditional specifications and the point estimate deviates only a little from the main estimates when controls are added, although the conditional estimates lose statistical significance.

The rest of this paper is organized as follows: Section 2 reviews the literature on slum growth; Section 3 describes the data sources and key variables; Section 4 explains the empirical strategy; Section 5 presents results and discusses mechanisms; Section 6 conducts robustness checks; and Section 7 concludes.

2 Literature Review

Empirical work on structural estimates on the determinants of slum formation and growth is relatively recent in the urban development literature. Cavalcanti et al. (2019) is one of the first papers to empirically estimate the parameters of a general equilibrium model with informal housing and heterogenous agents (in terms of labour productivity and location choice) using data from one slum in Rio de Janerio. The model has two key ideas. The first is that of an opportunity cost of protecting informal land plots in the form of lost labour income. Evidence for the existence of such opportunity costs in the Peruvian context is documented in Field (2007) who shows that a large land titling program between 1996 and 2003 (which increased security of tenure) led to an increase in labour supply of slum residents. The second idea is that per capita income is positively associated with slum growth, which may be due to high incomes pushing up formal housing rents which prevent slum residents from being able to afford formal housing.

Cai et al. (2018) consider a dynamic model with demand for property rights as endogenous and find that the high cost of obtaining property rights (rather than of protecting informal housing) may be a key source of friction stopping slum residents from formalizing as well, although they calibrate the model using simulations, not real-world data. These ideas make it unclear in which direction wage growth would affect slum growth. If wage growth is large enough, it may allow residents to afford the costs associated with formal housing, causing decline in slum population. On the other hand, if wages increase but are not high enough to afford the costs of formalizing, then the higher wages may allow residents to spend more time protecting their informal plots while maintaining the same level of consumption, potentially causing growth in slum population.

More recently, Alves (2021) expands the general equilibrium model to multiple cities by focusing on the interrelated labour and housing markets across cities in Brazil. He decomposes the labour market into low-income and high-income workers and the housing market into informal (i.e., slums) and formal housing, and presents an important descriptive fact – the number of high-income households living in informal housing is negligible. I use this fact to estimate the relationship between slum growth and wage growth separately for low-income workers. For identification, he uses wage Bartik instruments as shocks to local labour markets and migration Bartik instruments as shocks to the housing markets, innovating over past identification strategies of slum growth models.

The current paper should be understood as estimating one parameter of the model presented in Alves (2021). Unlike the general equilibrium model in Alves (2021), I focus on only the labour market and estimate the impact of the dynamics of this market on slum growth, assuming nondynamic housing markets. Alves (2021) estimates an elasticity of 1.7 for low-income workers but a much smaller and statistically insignificant elasticity for high-wage workers. He concludes that wage growth causes slum growth because low-income households are attracted to high wage growth cities, but because formal housing markets in these cities are relatively inelastic to migration, the new migrants end up living in informal housing. Importantly, note that this mechanism is different from the hypotheses in previous models. While previous models point to the role of costs of formalizing borne by slum residents, Alves (2021) shows that another key source of friction is the inelastic supply of formal housing.

3 Data

3.1 Housing and Household Characteristics Data

Data on housing characteristics, employment, and wages come from the Peruvian National Household Survey (Encuesta Nacional de Hogares - ENAHO). ENAHO is a repeated cross-sectional nationally representative survey at the individual- and household-level conducted annually by the Peruvian Statistical Agency (INEI) since 1998. The survey is national in scope, covering all 26 regions of Peru, both urban and rural, and collects data on demographics and living conditions of the population. Relevant to my research question, module 1 of the survey contains detailed questions on housing quality and living conditions, and module 5 collects data on wages and industry of employment. The first year of my data is 2004 (rather than 1998) because INEI adapted a new methodology in 2004 that has been consistent since then for the modules of interest for my analysis. I focus on a 15-year period from 2004 to 2019 because changes in slum levels in response to changes in wage levels would be a medium- to long-term process, necessitating a long study period to identify the relationship robustly. For example, Alves (2021) studies slum growth in Brazil over a 20-year period using census data from 1990 and 2010. Restricting the study period to 2019 also avoids any bias resulting from the Covid-19 pandemic in 2020.

For all aggregations up to the district-level and district-industry level, I use survey weights (called "expansion factors" in the raw data) provided by ENAHO to make the sample representative of the population. ENAHO's sample design involves stratified sampling using probabilities proportional to size (PPS), meaning these survey weights correspond to the inverse of the final selection probability of each respondent.

3.1.1 Defining Urban Districts

District boundaries include both urban and rural populations. First, at the district-level, I restrict my sample to districts that are at least partially urban in either 2004 or 2019. In other words, my sample consists of only districts that had at least one household surveyed in an urban Primary Sampling Unit (PSU) in 2004 or 2019. ENAHO defines a PSU to be urban if it has a population of 2,000 habitants or more. Of the 412 districts in my final sample, 309 districts were urban in 2004 only and 391 districts were urban in 2019 only. These urban districts correspond to an average urban population of 65,310 in 2004 and 62,933 in 2019. Second, at the individual-level, I restrict the sample to only residents in urban PSUs in these districts and drop incomplete surveys as indicated by a survey status variable in the data.

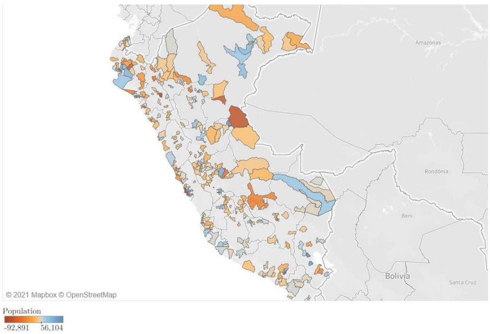
As slums are an urban concept, entirely rural districts imply a slum population of zero. This would introduce bias in my results because of concentration of data points with a slum population value of zero. However, districts that were entirely rural in 2004 but at least partially urban in 2019 are central to my research question because they represent urbanization of Peru between 2004 and 2019. For these districts, I include rural households in the sample. Conceptually, interpreting change in slums as measuring change in standard of living in districts that were urban in 2019 (rather than the strictly spatial definition of slums measuring urban housing location choice) allows me to include rural households in my sample. Section 3.1.2 discusses this conceptual interpretation in more detail.

3.1.2 Defining Slums

For my outcome variable, change in slum population, I adapt the UN Habitat's definition of slums an urban household is categorized as a slum if it lacks any of the following five amenities: (i) access to safe water, (ii) access to sanitation facilities, (iii) security of tenure, (iv) structural quality and durability of dwelling, and (v) sufficient living area (UN, 2003). Building on the literature which uses the first two amenities only, I adapt the first three amenities to my data and define a household to be a slum household if it (i) lacks access to a local water network connection inside the house, (ii) lacks access to a toilet inside the house, or (ii) does not have a title for the house[2]. I use this definition for two reasons. First, this definition provides a more accurate measure of slums than the one used in the literature (such as in Alves, 2021) which only considers the first two amenities due to data limitations.

Second, the first three criteria are binary in nature and correspond to specific questions in the ENAHO data, but the literature lacks consensus on the thresholds for the last two criteria. I aggregate the number of slum residents to the district-level to arrive at the district-level slum population. Figure 1 maps the outcome variable, change in slum population between 2004 and 2019, for the 412 districts in my sample.

Note that this definition of slums is robust to administrative definitions of slums that many local and national government agencies use to classify



Notes: Darker shades of red indicate slum decline and darker shades of blue indicate slum growth. Source: Own processing of ENAHO data.

2 Note that "security of tenure" for rental housing does not translate to having a title for the house but instead having a formal contract with the landlord. However, due to data limitations, I use only having title as a measure of security of tenure.

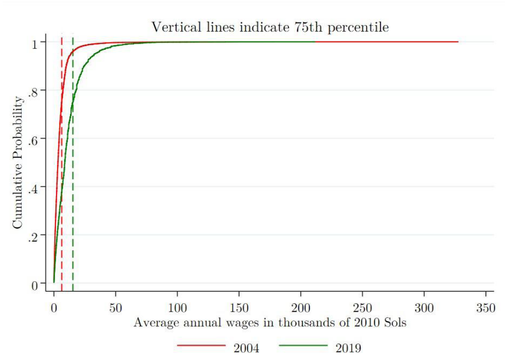
Rains et al. (2018) shows that administrative definitions can deviate significantly from ground reality and vary even across agencies within a city by comparing administrative demarcations of slums in India to surveys conducted in these areas and manually coding slum clusters in satellite images. However, also note that slums are intuitively a spatial concept that are not accurately captured by the UN Habitat definition. While this definition would have a high true positive rate because it is well documented across contexts that a majority of slum households have no source of private water, no private latrine, and live in an overall unhygienic environment (Marx et al., 2013), we should also expect a non-zero false positive rate due to the inclusion of households outside slums that lack access to safe water, sanitation, and security of tenure. Hence, my results should be interpreted as indicating urban standard of living rather than strictly urban location choice.

3.1.3 Wages and Employment

I compute my explanatory variable, change in annual urban wages at the district-level, by taking the 2004-2019 difference of average annual wages (monetary, in-kind, and profits from self employment) earned by urban respondents in their main occupation. To compute annual wages, I multiply the total income earned in the previous pay period by the frequency of payment. For example, for respondents paid biweekly, I multiply the wages earned in the last two weeks by 26 to arrive at annual wages. Following Alves (2021), I define low-wage workers as those below the 75th percentile of the wage distribution for each year. The 75th percentile corresponds to 15,365 Sols in 2019 and 6,325 Sols in 2004[3]. Figure 2 plots the cumulative distribution function of wages showing wage growth for low-wage workers between 2004 and 2019 as indicated by the rightward shift of the wage distribution in 2019.

3 This roughly equals US $43,400 in 2019 and US $17,850 in 2004 (in constant 2010 US $).

Figure 2: Cumulative Distribution Function of Wages, 2004 and 2019



Source: Own processing of ENAHO data.

ENAHO also maps the industry of employment of the respondent to the 4-digit International System of Industrial Classification (ISIC) Revision 3 level for both 2004 and 2019. I aggregate employment levels up to the district-industry level at the ISIC Rev. 3 2-digit level (rather than the 4-digit level) in order to have enough observations per industry for inclusion in the computation of the Bartik instrument, while also preserving a certain degree of variation. My final sample includes 33 industries at the ISIC Rev 3 2-digit level.

3.2 Trade Data

I use data on Peru's exports from BACI (Base pour l'Analyse du Commerce International). BACI provides data on bilateral trade flows disaggregated by product categories at the 6-digit Harmonized System 2002 (HS-02) level. This dataset is built by CEPII (Centre d'Etudes Prospectives´ et d'Informations Internationals) using data reported by countries to UN Comtrade (UN's International Trade Statistics Database). Trade flows included in this dataset are restricted to those whose value exceeds US \$1000. First, I aggregate Peru's export values to all countries up to the HS02 6-digit product level. Second, to merge this industry-level exports data with district-industry level employment data from ENAHO, I use correspondences from World Integrated Trade Solution (WITS) from the World Bank to convert the HS-02 6-digit product codes to ISIC

Rev. 3 4-digit industry codes. Third, I aggregate the export values up to the ISIC Rev. 3 2-digit level and merge with the ENAHO data. Finally, I use Peruvian Consumer Price Index (CPI) and exchange rate data from the World Bank to normalize export values, wages, and rents to constant 2010 Peruvian Sols.

Table 1 presents district-level summary statistics for the two years in my sample. Column 3 shows the two-sample t-test difference and significance level. Slum growth is negative while wage growth is positive, suggesting a negative relationship between the two. Table 2 decomposes these variables by slum status and presents individual-level summary statistics for 2004. Wages of slum residents and rents paid by slum residents are significantly lower than non-slum urban residents.

4 Empirical Strategy

I use a cross-sectional first-differences model and instrument for wage growth using an exports growth Bartik-style instrument. The main outcome variable is 2004-2019 log growth in slum population and main explanatory variable is 2004-2019 log growth in average annual urban wages measured in 2010 Sols. Using log transformations for both the outcome and explanatory variables has two advantages. First, it linearizes the distribution of slum growth and wage growth and reduces the implicitly higher weight of outliers, allowing me to estimate the relationship using a linear model. Second, it allows me to interpret the estimated regression coefficient as an elasticity. Figure 3 presents the raw Ordinary Least Squares (OLS) correlation between the two variables.

Estimating the relationship between slum growth and wage growth using an OLS model suffers from at least two sources of statistical endogeneity. First, we can expect potential reverse causality between slum growth and wage growth. Wage growth affects the purchasing power of the population which in turn would affect demand for slum and formal non-slum housing, affecting changes in slum levels. At the same time, depressed wage growth may be a result of living in slums because of lack of investments in human capital formation (for example, in the form of investments in education or health) and being trapped in a poverty trap (Marx et al., 2013). For Peru specifically, Field (2005) and Field (2007) show that giving property rights to slum residents causes an increase in investment

Table 1: District-Level Summary Statistics, 2004 and 2019

	(1) 2019	(2) 2004	T-test Difference
Variable	Mean (std error)	Mean (std error)	(1)-(2)
Urban Population (in 1000s)	40.74 (2.65)	44.49 (3.23)	-3.75
Slum Population (in 1000s)	17.72 (1.09)	21.05 (1.14)	-3.33**
Slum Share	0.52 (0.01)	0.68 (0.02)	-0.16***
Urban Wages (in 1000s of 2010 Sols)	10.28 (0.21)	3.95 (0.12)	6.32***
Urban Rents (in 1000s of 2010 Sols)	2.34 (0.09)	1.41 (0.09)	0.93***
Urban Female share	0.52 (0.00)	0.50 (0.00)	0.02***
Urban Age	34.09 (0.29)	28.15 (0.26)	5.94***
Urban Gini coefficient	0.18 (0.00)	0.16 (0.00)	0.02***
Number of Districts	412	412	

Notes: Imputed rents are used for non-rental housing (as provided in ENAHO). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Individual-Level Summary Statistics for 2004, Slum and Non- Slum Urban

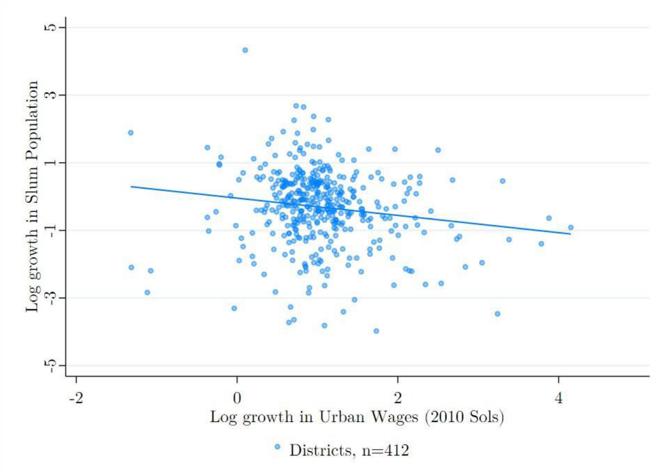
	(1) Non-Slum Urban	(2) Slums	T-test Difference
Variable	Mean (std error)	Mean (std error)	(1)-(2)
Wages (in 1000s of 2010 Sols)	6.25 (0.09)	3.43 (0.04)	2.83***
Rents (in 1000s of 2010 Sols)	2.99 (0.02)	0.83 (0.01)	2.16***
Female share	0.52 (0.00)	0.49 (0.00)	0.02***
Age	29.64 (0.13)	25.72 (0.11)	3.92***
Share Employed	0.39 (0.00)	0.33 (0.00)	0.05***

Notes: Imputed rents are used for non-rental housing (as provided in ENAHO). Employment is defined as having positive earnings. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

in residential quality and an increase in labour supply, providing direct evidence of the causal link from slum status to labour-market outcomes. This reverse causality would bias the estimates upwards. Second, data on wages may suffer from classical measurement error as it is comes from self-reported individual-level surveys. This would introduce attenuation bias and bias the estimates towards 0.

To address these endogeneity concerns, I instrument for wage growth using a Bartik-style exports growth instrument and use a first-differences model to estimate the causal effect of wage growth on slum growth. First-differencing controls for any time-invariant district-level characteristics such as proximity to the coast or mountains (which may affect mobility and migration, and hence slum growth), and instrumenting for wage growth isolates plausibly exogenous variation in wage growth.

Figure 3: Log Slum Growth (Y) vs Log Wage Growth (X)



Source: Own processing of ENAHO data.

4.1 Bartik Instrument

Bartik instruments have been used extensively in the urban economics literature (and in spatial economics more generally) to isolate sources of exogenous variation in local labour demand. The idea is to purge the equilibrium wage of local endogenous labour market characteristics and use the exogenous part of labour demand (Baum-Snow and Ferreira, 2015).

4.1.1 Bartik Instruments in the Literature

Most notably in the trade literature, Autor et al. (2013) estimate the effects of Chinese import competition on US local labour-markets by computing a Bartik-style variable. They compute this variable by interacting national growth in Chinese imports by industry with local labour-market employment composition by industry. This measure proxies for the intensity of Chinese import exposure in US commuting zones and is used as the main explanatory variable (rather than an instrument). They exploit cross-sectional variation in commuting zones and the exogeneity in their analysis comes from instrumenting for Chinese imports to US using Chinese imports to other high-income countries.

Mansour et al. (2021) compute a similar import exposure intensity variable to study the differential effects by gender of Chinese imports on local labour outcomes in Peru, but additionally follow the methodological approach proposed in Goldsmith-Pinkham et al. (2020) to argue for exogeneity of the instrument. Specifically, they check that the baseline level of local labour-market industry shares are not correlated with local labour-market characteristics. The idea here is to verify that industry shares that contribute the most to the cross-sectional variation in import exposure are not correlated with other baseline levels of local labour-market characteristics that may be driving labour-market outcomes.

An application of Bartik instruments that is closest to my empirical strategy is Alves (2021) which estimates, among other things, the elasticity of slum growth to wage growth in Brazil between 1990 and 2010. He constructs a Bartik instrument using national wage growth instead of national import intensity to instrument for local labour-market wage growth. Alves (2021) does not argue for the validity of the instrument like Mansour et al. (2021) and instead relies on the extensive use of Bartik instruments in the literature to justify its validity. However, as two recent methodological papers show, some of the canonical applications of Bartik instruments in the literature (including the dataset used by Autor et al. (2013)) do not satisfy the exclusion restriction when examined using theoretically robust tests (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2020).

4.1.2 Applying the Bartik Instrument

Unlike Alves (2021), the version of the Bartik instrument that uses national wage growth does not have strong predictive power in my sample when controls are included. As Section 4.1.4 discusses, my IV strategy requires inclusion of controls because of potential alternate channels that I must control for in order for the exclusion restriction to hold. When controls are included, the first-stage F-stat for the wage growth Bartik instrument falls from 10.4 to 7.6 and the second stage standard errors become significantly larger than the point estimates, suggesting that the estimates are too noisy for any meaningful precise estimation. Computation and results of the wage growth Bartik instrument are presented in Appendix A.3 and A.4.

Instead, I employ a Bartik instrument which uses log growth in national exports by industry rather than log growth in national wages by industry. Intuitively, this Bartik instrument can be thought of as the intensity of export exposure of a district. However, unlike Autor et al. (2013) and Mansour et al. (2021), I do not use the computed Bartik instrument as the explanatory variable. I use it to instrument for district-level wage growth because we should expect the relationship between exports growth and slum growth to be mediated by wage growth – it is unclear how export growth would directly affect slum growth. Conceptually, we should expect districts with higher export exposure intensity to experience higher urban wage growth (Brambila et al. (2017) find that exporting firms pay higher wages to their workers in Peru). The first-stage coefficients confirm this relationship. The coefficients on the Bartik instrument are positive and statistically significant in Table 4. As such, my main empirical strategy should be understood as a combination of the strategies used in Mansour et al. (2021) – which uses an imports exposure intensity Bartik as the explanatory variable – and Alves (2021) – which uses a wage growth Bartik as an instrument. The following subsections discuss the computation, validity, and relevance of the constructed instrument in more detail.

4.1.3 Computation

The exports growth Bartik instrument is computed by interacting the log growth of national exports by industry (the "shocks") with districts' 2004 employment composition by industry (the "shares") and then summing up to the district-level. Mathematically, the district-level Bartik instrument, *Bartikd*, is

$$Bartik_d^{wage} = \sum_{i=1}^{I} [(ln\ Wage_{i,-d,2019} - ln\ Wage_{i,-d,2004}) * \frac{N_{i,d,2004}}{N_{d,2004}}]$$

where *Exportsi,2019* are national exports of industry i in 2019, *Exportsi,2004* are national exports of industry *i* in 2004, *Ni,d,2004* is employment in industry *i* in district *d,* and *Nd,2004* is the total employment in all industries in district *d.*

4.1.4 Instrument Validity

4.1.4.1 Source of Exogenous Variation

There are two possible sources of exogenous variation in Baritk-style instruments discussed separately in two recent papers. Borusyak et al. (2020) argue that the exclusion restriction for Bartik-style instruments[4] boils down to orthogonality between the common national shocks and shock-level unobservables. That is, they argue that the same regression coefficients can be obtained from shock-level IV regressions as well, implying that the source of exogenous variation is provided by national shocks, not local industry shares. This approach requires two conditions. First, the variation in the common national shocks must arise from a natural experiment such that they are as-good-as-random. Second, there must be a large number of national shocks for the law of large numbers to kick in at the level of the shock.

On the other hand, Goldsmith-Pinkham et al. (2020) argue that most applications of Bartik-style instruments are essentially a pooled exposure research design where the heterogeneity in baseline level of local

4 They use the term Shift-Share Instrumental Variable (SSIV) rather than Bartik instrument but I use the two terms interchangeably here.

labour-market industry shares measures the differential exposure to common shocks. By showing that a 2-SLS estimator using a Bartik instrument is numerically equivalent to a generalized method of moments estimator using each industry share as a separate instrument, they conclude that the exogeneity of the research design depends on the exogeneity of local industry shares, not national shocks.

While both sides agree that the source of exogenous variation must be determined a priori based on the specifics of the context (rather than applying both frameworks), the lack of clarity and consensus on how to establish an a priori argument is illustrated in the fact that both the papers apply their framework to the same paper - Autor et al. (2013). To guide my argument for the Peruvian context, I focus on one criterion provided in both Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2020), which is also implemented by Mansour et al. (2021) in their study of the effects of Chinese imports on local labour-market outcomes in Peru. A priori exogeneity depends on the number of industries that are central to the identification strategy.

If a large number of industries are important for identification, then there is rationale for shocks being exogenous (as argued in Borusyak et al. (2020)). However, if only a few industries are important, then it is likely that local industry shares provide the exogenous variation (as argued in Goldsmith-Pinkham et al. (2020)).

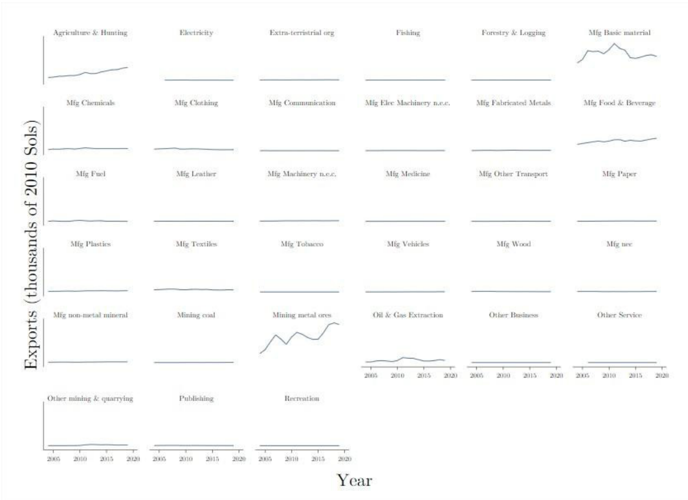
Following Mansour et al. (2021), I visually investigate the heterogeneity in Peruvian exports by industry at the ISIC Rev. 3 2-digit level. Figure 4 presents export values by industry between 2004 and 2019 for the industries in my sample[5]. Industries such as Agriculture & Hunting, Manufacturing of Basic Materials, Manufacturing of Food & Beverages, and Mining of Metal Ores have experienced high export growth between 2004 and 2019 while most of the other industries have experienced relatively low export growth. This indicates that only a few industries provide the variation in national shocks in my sample, suggesting that my identification depends on the exogeneity of the cross-sectional variation in 2004-level of local district industry shares, not the national-level industry export shocks.

5 Note that my investigation into the variation in national shocks differs from Mansour et al. (2021) in that they focus on hinese imports to Peru by industry while I focus on global exports of Peru by industry. This distinction means that industries (and the number of industries) that provide the variation in my sample may be different from those in Mansour et al. (2021).

4.1.4.2 Exclusion Restriction

The exclusion restriction requires that the instrument affect the outcome variable only through the endogenous variable. That is, local industry shares should affect slum growth through no channel other than wage growth. One alternate channel in the Peruvian context is the allocation of housing to workers by their employer. If certain industries provide housing as compensation to their workers, then the share of employment in an industry will directly affect housing demand, potentially affecting slum growth. This is likely in Peru because of its long history of conflict between the mining industry and local communities. Deals struck between mining companies and local communities often involve relocation of residents as a compromise[6]. In my sample, only 79

Figure 4: Peruvian Export by Industry between 2004 and 2019



Notes: The trends that look flat are not because of close to zero export growth in these industries but because of the common Y-axis. Source: BACI (UN Comtrade).

districts in 2004 and 33 districts in 2019 have a non-zero share of the population who were provided housing by their workplace, and the mean of this share across districts is negligible: 1.7% of the district urban population in 2004 and 0.4% in 2019. Figure 9 in Appendix B shows the

6 See www.reuters.com/article/us-mmg-peru-insight-idUSKBN1E10JG for an example of a deal that relocated rural households to urban towns.

distribution for 2004 and 2019 and Figure 10 shows the relationship between the change in population share provided housing by the workplace and the computed Bartik instrument. The figure shows no correlation between the two, indicating that this is not a significant alternate channel that would invalidate my instrument.

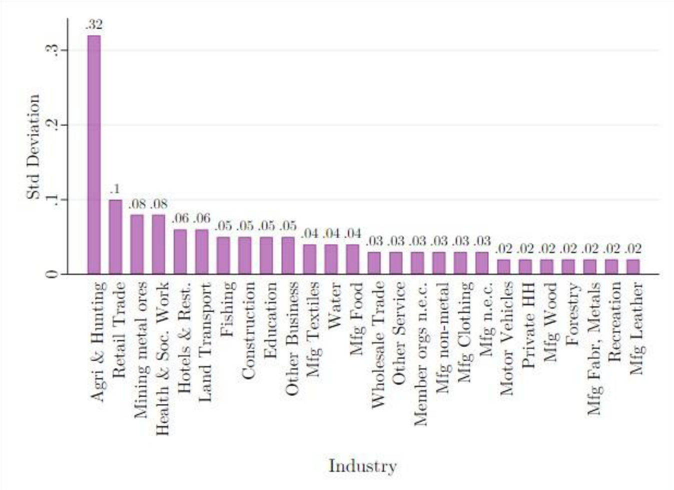
Having established a priori the source of exogenous variation in my Bartik instrument, I now apply the framework proposed in Goldsmith-Pinkham et al. (2020). They provide three tests for the plausibility of the identifying assumption with Bartik instruments. I apply the first test – investigating the correlation of baseline local employment shares of industries with baseline local characteristics that may mediate the relationship between industry shares and slum growth. The underlying exclusion restriction states that the levels of the shares should be exogenous to changes in the error term (i.e., changes in the outcome variable).

The first step is to calculate Rotemberg weights for each industry which indicate the "sensitivity to-misspecification elasticities." These weights tell us which industry shares get more weight in the identification. Higher the Rotemberg weight for an industry, the more sensitive the endogenous variable (wage growth) is to the employment share of that industry. Using these weights, we can identify the top few industries that are most important to the variation and focus the test of the identifying assumption on these industries, rather than all industries. Due to time constraints, I do not compute Rotemberg weights and instead use a more straightforward heuristic to determine which industry shares are contributing most to the cross-sectional variation. For each industry, I calculate the standard deviation of local industry shares across the districts in my sample with the idea that higher the dispersion of industry shares, relatively more the industry contributes to the cross-sectional variation. Figure 5 ranks the industries in terms of this measure.

The second step is to check the correlation of the baseline level of shares of the industries that provide the most variation with baseline level local characteristics. The economic and statistical significance of the correlations indicate the possibility of alternate channels – if the correlations are insignificant, then we can be confident that the exclusion restriction holds. Based on Figure 5, I choose the top ten industries with the highest dispersion. Table 3 presents the correlation of the overall Bartik instrument and the industry shares of the top five industries with local characteristics. Industry correlates for the next five industries are presented in Table 12 in Appendix B.

First note that the R-squared value for the overall Bartik instrument is quite high – 60% of the variation in the computed instrument can be explained by the covariates (Table 3, column 1). Similarly, 59% of the variation in the 2004 industry shares of Agriculture & Hunting can be explained by the covariates (Table 3, column 2). Second, the coefficients for 2004-level of log rents (row 1) are statistically significant for all columns and economically significant for the

Figure 5: Dispersion of Industry Shares across Districts in 2004



Notes: Only industry shares with a standard deviation of greater than 0.01 are shown for better visualization. Source: Own processing of ENAHO data.

overall Bartik instrument (column 1) and shares of Agriculture & Hunting (column 2). A similar pattern holds for 2004-level female share (row 3). These results point to the instrument potentially affecting slum growth through channels other than wage growth. The Bartik instrument that is interpreted as predicting local labour-demand shocks (affecting wages) is also correlated with local labour-supply characteristics (e.g.: female share). Therefore, I control for the baseline level of log rents and female share in my regressions such that the exclusion restriction holds conditional on controls.

Table 3: Industry Share Correlates, Bartik Instrument & Top 5 Industries

	(1) Bartik Instrument	(2) Agri & Hunting	(3) Retail Trade	(4) Mining Metal Ores	(5) Health & Social Work	(6) Hotels & Rest.
Urban Rents 2004 (log)	-0.43***	-0.26***	0.043***	0.0035*	0.022***	0.021***
	(0.02)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Urban Age 2004 (log)	0.071	0.081	0.0051	-0.034	0.0015	-0.0026
	(0.12)	(0.07)	(0.03)	(0.03)	(0.02)	(0.02)
Urban Female share 2004	-0.94***	-0.44**	0.19**	-0.10	0.18**	0.010
	(0.32)	(0.20)	(0.09)	(0.06)	(0.08)	(0.07)
N	412	412	412	412	412	412
R-squared	0.60	0.59	0.19	0.015	0.11	0.11

Notes: Each column reports results of a single regression. Outcome variable is the computed Bartik instrument in column 1 and 2004-level of an industry's share in columns 2-6. Independent variables are 2004-level local district characteristics. District-level regressions, robust standard errors in parentheses.
*p < 0.10, ** p < 0.05, ***p < 0.01

4.1.5 Instrument Relevance

The first-stage of my instrumental variable approach is:

$$ln\ WageGrowth_d = \alpha_0 + \alpha_1 Bartik_d + X_d + \mu_d$$

where lnWageGrowthd is the log growth in district-level urban wages between 2004 and 2019, Bartikd is the computed exports growth Bartik instrument, and Xd is a vector of district-level controls (discussed in the next subsection). Table 4 presents the first-stage estimates. The F-stat falls as controls are added in columns 2 and 3 but remains above 10 and the coefficients on the Bartik instrument in all three columns are highly significant at the 1% significance level, indicating that the computed exports Bartik is a relevant instrument with high predictive power for local district-level wage growth in my sample.

Table 4: First-statge Regression Estimates

	(1)	(2)	(3)
	Log Wage growth	Log Wage growth	Log Wage growth
Bartik instrument	0.51***	0.51***	0.39***
	(0.07)	(0.07)	(0.12)
Slum Population 2004 (log)		0.03	0.03
		(0.03)	(0.03)
Urban Rents 2004 (log)			-0.08
			(0.06)
Urban Female share 2004			-0.46
			(0.61)
Constant	0.73***	0.45	1.27**
	(0.04)	(0.29)	(0.63)
N	412	412	412
F-stat	51.89	26.08	13.92
R-squared	0.17	0.17	0.18

Notes: District-level regressions, robust standard errors in parentheses. Dependent variable is 2004-2019 log growth in average annual urban wages. Bartik instrument is calculated by interacting 2004-2019 log growth in national export value by industry with 2004-level local (district) industry shares (at ISIC Revision 3 2-digit level). Controls in column 2 include 2004-level of slum population. Column 3 adds 2004-level of average urban rents and female share of the district as controls. For non-rental housing, self-reported imputed rents are used (from ENAHO). For districts that were urban only in either 2004 or 2019, rural households are included for the year these districts were not urban.
*p < 0.10, ** p < 0.05, ***p < 0.01

4.2 Estimating Equations

The second-stage equation and my primary specification is:

$$ln\ SlumPopulationGrowth_d = \beta_0 + \beta_1 ln\ \hat{W}ageGrowth_d + X_d + \varepsilon_d$$

where *ln SlumPopulationGrowthD* is the log growth in slum population between 2004 and 2019 for *district D, ln WageGrowth^ D* is the instrumented log growth in district-level urban wages between 2004 and 2019 for district D, and *XD* is a vector of district-level controls. *XD* includes the log of 2004-level of slum population to control for any differential trends in slum growth explained by the baseline level of slum population in a district, log average rents, and female share of urban population to to control for alternate channels as discussed in Section 4.1.4. β1 is the coefficient of interest which gives us the elasticity of slum growth to wage growth. I do not include log 2004level of urban population as a control because of multicollinearity with log 2004-level of slum population (correlation between the two is 0.74). This is reasonable because slum population as a percentage of urban population is high for many districts in my sample. This is visually evident in Figure 7 in Appendix B which shows a large spike for slum population as a share of urban population at the value of one for 2004.

Note that specification (1) includes the entire urban population of districts. However, as shown in Table 2, wages of slum residents are significantly lower than those of urban non-slum residents. It is plausible that due to the aggregation of slum and urban non-slum residents, the estimates of specification (1) are being driven by characteristics of non-slum residents. For my second specification, I decompose the sample into low-wage earners (those earning below the 75th percentile of the wage distribution) and high-wage earners. The resulting equation for the sample restricted to households with at least one low-wage earner is,

$$ln\ SlumPopulationGrowth_d^{low} = \gamma_0 + \gamma_1 ln\ \hat{W}ageGrowth_d^{low} + X_d + \varepsilon_d \tag{2}$$

where *ln SlumPopulationGrowth*^{low}$_D$ is the log growth in slum population for individuals in households with at least one low-wage earner between 2004 and 2019 for district d, lnWageGrowthˆ lowd is instrumented log growth in district-level urban wages for low-wage earners between 2004 and 2019 for district d, and the rest of the variables are defined as in the previous specification but for the restricted sample.

I do not include first-differences of these control variables in the regressions because they may be outcomes of wage growth and hence be bad controls. For example, we can expect high urban wage growth to cause high rent growth (through stronger purchasing power and stronger housing demand). However, because urban population growth is a potential confounder in the relationship between slum growth and wage growth, I control for it in my third specification by using change in slum population as a share of urban population as the outcome variable:

$$SlumShareGrowth_d = \theta_0 + \theta_1 ln\ \hat{W}ageGrowth_d + W_d + \varepsilon_d \tag{3}$$

where *SlumShareGrowth*$_D$ is the growth in slum population as a share of urban population between 2004 and 2019 for district D, W_D is same as X_D except it omits 2004-level of slum population and includes 2004-level of slum share, and the rest of the variables are defined as in specification (1). Specification (3) lets me investigate whether the global trend of a decline in slum populations as a share of urban population (UN, 2015) is reflected in the Peruvian context.

5 Results and Discussion

5.1 Growth in Slum Population

Table 5 presents the results of my primary specification. Columns 1-3 contain OLS estimates and columns 4-6 contain 2-SLS estimates with each column progressively adding controls. The coefficient on urban wage growth in column 6 implies that a 1% increase in a district's urban wage causes a 0.94% decrease in the district's slum population.

The larger magnitude of 2-SLS estimates than the OLS estimates suggests two possibilities. First, OLS estimates may be attenuated because the wage data suffer from classical measurement error which the 2-SLS estimates get rid of. Second, this is potentially just a mechanical result of using an instrumental variable to predict wage growth because interpretation of OLS and 2-SLS estimates differs in terms of the variation in the independent variable they use. OLS estimates use all the variation in the independent variable to explain the dependent variable, giving us the Average Treatment Effect (ATE). On the other hand, 2-SLS estimates use only the variation in the independent variable that is explained by the instrument, giving us the Local Average Treatment Effect (LATE). That is, to estimate the elasticity, my IV strategy uses only the variation in local wage growth that is explained by exports growth intensity in a district.

Table 5: OLS and 2-SLS Estimate. Dependent Variable: Log Growth in Slum Population

	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Urban Wage Growth (log)	-0.26**	-0.25***	-0.08	-1.06***	-1.22***	-0.94*
	(0.10)	(0.09)	(0.09)	(0.25)	(0.25)	(0.55)
Slum Population 2004 (log)		-0.44***	-0.46***		-0.43***	-0.44***
		(0.05)	(0.05)		(0.05)	(0.05)
Urban Rents 2004 (log)			0.33***			0.12
			(0.06)			(0.15)
Urban Female share 2004			-0.02			-0.72
			(0.88)			(1.18)
Constant	-0.05	4.11***	1.93***	0.80***	5.08***	4.40**
	(0.13)	(0.50)	(0.69)	(0.26)	(0.57)	(1.77)
N	412	412	412	412	412	412
First-stage f-stat				51.89	26.08	13.92
R-squared	0.02	0.19	0.26	.	.	0.04

Notes: OLS and 2SLS district-level regressions, robust standard errors in parentheses. Dependent variable is 20042019 log growth in slum population. The independent variable of interest is instrumented 2004-2019 log growth in urban wages. Controls in columns 2 and 5 includes 2004-level of slum population. Columns 3 and 6 add 2004-level of average urban rents and female share of the district as controls. For non-rental housing, self-reported imputed rents are used (from ENAHO). For districts that were urban only in either 2004 or 2019, rural households are included for the year these districts were not urban.
* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

This implies that the 2-SLS standard errors would be larger than the OLS standard errors because of the limited variation in the independent variable being used in 2-SLS, which is the case in Table 5. An important implication of LATE is that the 2-SLS estimates depend on the choice of the instrument because different instruments would explain different parts of the variation in wage growth. This means that the magnitude of the difference between the OLS and 2-SLS estimates may be different if an alternate instrument were used. As such, this makes the ATE and LATE estimates not strictly comparable.

5.2 Growth in Slum Population For Low-Wage Earners

Table 6 presents the estimation results for the sample restricted to households of low-wage earners. The main coefficient on urban wage growth in column 6 implies that a 1% increase in a district's urban wage for low-wage earners causes a 1.33% decrease in the district's low-wage population living in slums. The larger magnitude than the estimate for the entire sample indicates that lowwage individuals are more responsive to the channel of escaping slums than high-wage individuals. Potential channels of slum decline are discussed in Section 5.4.

Table 6: OLS and 2-SLS Estimates. Dependent Variable: Log Growth in Slum Population, sample restricted to Low-Wage Earner Households

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	2SLS	2SLS	2SLS
Urban Wage Growth (log)	-0.39***	-0.40***	-0.12	-1.09***	-1.28***	-1.33**
	(0.11)	(0.10)	(0.12)	(0.22)	(0.21)	(0.64)
Slum Population 2004 (log)		-0.42***	-0.45***		-0.43***	-0.43***
		(0.05)	(0.05)		(0.05)	(0.06)
Urban Rents 2004 (log)			0.35***			0.01
			(0.06)			(0.19)
Urban Female share 2004			0.06			-1.30
			(0.86)			(1.44)
Constant	-0.92***	3.08***	0.71	-0.35**	3.89***	4.52**
	(0.11)	(0.49)	(0.74)	(0.18)	(0.53)	(2.20)
N	393	393	393	393	393	393
First-stage f-stat				84.13	43.22	29.29
R-squared	0.04	0.19	0.26	.	.	.

Notes: OLS and 2SLS district-level regressions for low-wage earner sample, robust standard errors in parentheses. Dependent variable is 2004-2019 log growth in slum population. The independent variable of interest is instrumented 2004-2019 log growth in urban wages. Controls in columns 2 and 5 includes 2004-level of slum population. Columns 3 and 6 add 2004-level of average urban rents and female share of the district as controls. For non-rental housing, self-reported imputed rents are used (from ENAHO). For districts that were urban only in either 2004 or 2019, rural households are included for the year these districts were not urban. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

5.3 Growth in Slum Share

Table 7 presents estimation results for specification (3) with the outcome variable as growth in slum share. The 2-SLS coefficients on wage growth lose statistical significance when controls are added in columns 5 and 6. The 95% confidence intervals for the point estimates overlap with 0 but are small, suggesting that the effect of wage growth on growth in slum share is statistically indistinguishable from 0 (rather than the point estimates being too noisy).

Table 7: OLS and 2-SLS Estimates. Dependent Variable: Growth in Slum Share, 2004-2019

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	2SLS	2SLS	2SLS
Urban Wage Growth (log)	-0.17***	-0.09***	-0.10***	-0.50***	0.07	-0.03
	(0.03)	(0.02)	(0.02)	(0.08)	(0.08)	(0.10)
Slum Share 2004		-0.58***	-0.72***		-0.68***	-0.73***
		(0.04)	(0.06)		(0.07)	(0.06)
Urban Rents 2004 (log)			-0.07***			-0.06**
			(0.02)			(0.03)
Urban Female share 2004			-0.16			-0.11
			(0.23)			(0.24)
Constant	0.02	0.32***	0.99***	0.37***	0.23***	0.81***
	(0.03)	(0.03)	(0.19)	(0.08)	(0.05)	(0.31)
N	412	412	412	412	412	412
First-stage f-stat				51.89	26.08	13.92
R-squared	0.13	0.45	0.47	.	0.35	0.45

Notes: OLS and 2SLS district-level regressions, robust standard errors in parentheses. Dependent variable is 20042019 growth in slum population as a share of urban population. The independent variable of interest is instrumented 2004-2019 log growth in urban wages. Controls in columns 2 and 4 include 2004-level of slum population as a share of urban population. Colums 3 and 5 add 2004-level of average urban rents and female share of the district as controls. For non-rental houses, self-reported imputed rents are used (from ENAHO). For districts that were urban only in either 2004 or 2019, rural households are included for the year these districts were not urban.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.4 Discussion

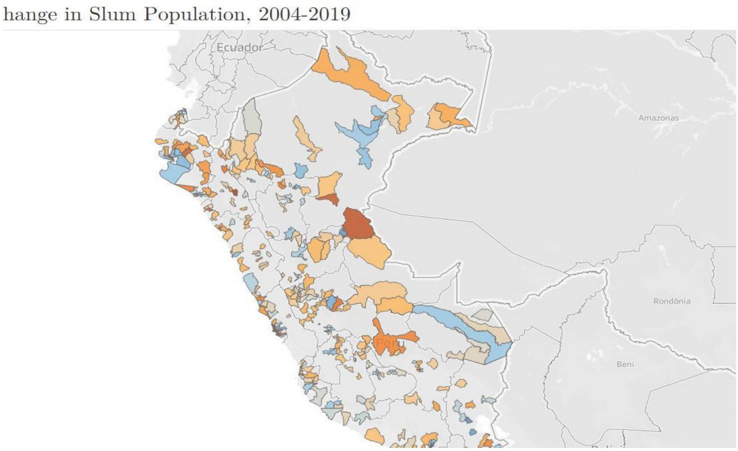
The negative elasticities for slum population growth are surprising given theoretical models of migration that predict migration to urban areas induced by the urban-rural wage gap (Harris and Todaro, 1970) and recent empirical findings that low-wage earners are more responsive to higher urban wages in terms of migrating to urban areas (Alves, 2021; Diamond, 2016). While it is hard to pin down the mechanism causing the decline in slums without a general equilibrium model which includes the housing market, I briefly discuss two channels here. First, I consider the interrelated models presented in Cavalcanti et al. (2019) and Alves (2021) – these relate to the location choice interpretation of slums. Second, I investigate changes in investments in housing quality by slum residents – this relates to the standard of living interpretation of slums discussed in Section 3.1.2.

In the framework proposed by Cavalcanti et al. (2019), a decline in slum population is consistent with high wage growth such that the opportunity cost of protecting informal plots is so high that slum residents are better off living in formal housing. This must require wage growth to be high enough to afford higher rents of formal housing, putting upward pressure formal housing rents. However, Alves (2021) shows for the case of Brazil that formal housing rents are almost four times more responsive to housing demand shocks than informal housing rents. This implies that informal housing supply is more responsive than formal housing supply to housing demand. For the channel in Cavalcanti et al. (2019) to be the primary channel here would require urban wage growth in Peru to be

high enough for formal housing to be both, responsive and affordable. This is unlikely given poor urban planning, burdensome bureaucracy, and lack of adequate zoning in urban Peru which hinders supply of low-income formal housing (Bonilla and Barrantes, 2011).
A more likely mechanism of slum decline in the Peruvian context is investments in housing quality by slum residents themselves. Qualitative interviews with residents in slums in Lima, the capital, indicate that residents and the slum community invest substantially in improving their living conditions (ODI, 2015). Data from ENAHO also suggests that investments in housing have increased between 2004 and 2019. Figure 6 shows that the amount of credit received for home improvements or extensions was on average higher in 2019 than in 2004 by low-wage earners (however this is suggestive evidence at best because only 0.02% of low-wage earners in 2004 and 0.04% in 2019 reported having taken a loan for home improvement).

Note that investments in housing quality also indicate that slum residents are aware of low mobility out of slums. This provides evidence against the “modernization” theory of Glaeser (2011) as well because if slums were transitory, residents would have lower incentive to improve their current housing quality. However, note that this channel also suggests that perhaps slums in Peru do not act as poverty traps either. Through housing investments, residents can improve their living conditions and human capital, leading to better labour-market outcomes and potentially entering a virtuous cycle.

Figure 6: Distrubition of Credit for Home Improvement, 2004 and 2019



Source: Own processing of ENAHO data.

6 Robustness Checks

I conduct two robustness checks. A visual inspection of Figure 3 suggests that a few data points at the extreme ends of the slum growth or wage growth distribution may be driving the results. To check if the results are robust to removal of these outliers, I first run my specifications with slum growth and wage growth trimmed at the 5% and the 95% level. For my second robustness check, I winsorize the variables at the 5% and the 95% level instead of trimming.

The Bartik instrument still has high predictive power for both modified datasets as indicated by the high first-stage F-stats in Tables 8 and 9 in Appendix A. While the second-stage estimates change a little from the main estimation results (in Table 5), note that the estimate for the main specification with controls (Column 6) only deviates a little. It goes from -0.94 for unmodified data to -0.83 for trimmed data and -0.97 for the winsorized data. They lose significance because the standard errors increase by a little which is expected when the sample size falls (as in trimmed data) or the variation in the data is reduced (as in winsorized data). Even though the estimates lose statistical significance, the small deviation in the point estimate and standard errors is suggestive that the relationship between slum growth and wage growth is not entirely due to the extreme ends of the distribution.

7 Conclusion

I compute an exports-growth Bartik instrument to instrument for wage growth and use a first-differences model to estimate the elasticity of slum growth to urban wage growth between 2004 and 2019 in Peru. I estimate a statistically and economically significant negative elasticity of -0.94, implying that urban wage growth causes a decline in slum population in Peruvian districts. These results are opposite in direction from estimates in the literature for other countries, suggesting that further research into the Peruvian context is required, especially using general equilibrium models that study the dynamic relationship between the urban labour and urban housing markets. In the absence of such models, I propose alternate mechanisms specific to Peru such as housing investments by slum residents to improve their living conditions. An important implication of housing investments by slum residents as a result of wage growth is the possibility of slums not acting as poverty traps.

My estimates are limited by the limited number of districts sampled in ENAHO. While Peru has 1,874 districts in total, ENAHO sampled only 1,259 districts in 2019 and 880 districts in 2004. Data on close to all the districts in Peru would improve the precision of these estimates. Another limitation of my analysis is that the definition of slums I use captures standard of living better than it captures urban location choice. Importantly, the definition I use does not capture the social stigma and social exclusion that many slum residents experience from living in slums – this is an inherently location-based effect. As such, it is hard to interpret my estimates to be a result of location choice by slum residents. Future research should consider a different measure of slums that captures location choice better. One such measure is identification of slum clusters by processing satellite images using machine learning (as in Gechter and Tsivanidis (2020)).

Finally, migration in response to wage growth is likely to be a two-step process (rather than a contemporaneous process) where potential migrants in rural areas first learn about wage growth in urban areas and second, decide to migrate in response. Hence, a natural extension to the analysis would be studying the lagged relationship between slum growth (e.g.: 2006-2019) and wage growth (e.g.: 2004-2017).

8 References

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8 Appendix

8.1 A.1 Robustness Check: Trimmed Data

Table 8: Estimates for data trimmed at 5%. Dependent Variable: Log Growth in Slum Population

Table 8: Estimates for data trimmed at 5%. Dependent Variable: Log Growth in Slum Population

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	2SLS	2SLS	2SLS
Urban Wage Growth (log)	-0.29**	-0.35***	-0.15	-1.31***	-1.61***	-0.83
	(0.11)	(0.11)	(0.11)	(0.32)	(0.35)	(0.74)
Slum Population 2004 (log)		-0.21***	-0.23***		-0.28***	-0.26***
		(0.04)	(0.04)		(0.05)	(0.05)
Urban Rents 2004 (log)			0.26***			0.15
			(0.05)			(0.13)
Urban Female share 2004			0.85			1.06
			(0.76)			(0.90)
Constant	0.03	2.09***	-0.14	1.08***	4.11***	1.48
	(0.12)	(0.41)	(0.63)	(0.32)	(0.74)	(1.89)
N	339	339	339	339	339	339
First-stage f-stat				47.56	27.62	14.46
R-squared	0.02	0.08	0.17	.	.	0.06

Notes: OLS and 2SLS district-level regressions, robust standard errors in parentheses. Dependent variable is 20042019 log growth in slum population. The independent variable of interest is instrumented 2004-2019 log growth in urban wages. Log growth in slums and log growth in wages trimmed at the 5th and 95th percentile. Controls in columns 2 and 5 includes 2004-level of slum population. Columns 3 and 6 add 2004-level of average urban rents and female share of the district as controls. For non-rental housing, self-reported imputed rents are used (from ENAHO). For districts that were urban only in either 2004 or 2019, rural households are included for the year these districts were not urban.
*p < 0.10, ** p < 0.05, *** p < 0.01

8.2 A.2 Robustness Check: Winsorized Data

Table 9: Estimates for data winsorized at 5%. Dependent Variable: Log Growth in Slum Population

Table 9: Estimates for data winsorized at 5%. Dependent Variable: Log Growth in Slum Population

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	2SLS	2SLS	2SLS
Urban Wage Growth (log)	-0.31***	-0.32***	-0.10	-1.19***	-1.35***	-0.97
	(0.10)	(0.09)	(0.10)	(0.26)	(0.25)	(0.59)
Slum Population 2004 (log)		-0.35***	-0.37***		-0.36***	-0.37***
		(0.04)	(0.04)		(0.04)	(0.04)
Urban Rents 2004 (log)			0.30***			0.11
			(0.05)			(0.14)
Urban Female share 2004			0.34			0.17
			(0.77)			(0.91)
Constant	0.02	3.37***	1.14**	0.93***	4.54***	3.33**
	(0.12)	(0.35)	(0.57)	(0.26)	(0.48)	(1.62)
N	412	412	412	412	412	412
First-stage f-stat				73.28	36.52	18.80
R-squared	0.03	0.17	0.25	.	.	0.07

Notes: OLS and 2SLS district-level regressions, robust standard errors in parentheses. Dependent variable is 20042019 log growth in slum population. The independent variable of interest is instrumented 2004-2019 log growth in urban wages. Log growth in slums and log growth in wages trimmed at the 5th and 95th percentile. Controls in columns 2 and 5 includes 2004-level of slum population. Columns 3 and 6 add 2004-level of average urban rents and female share of the district as controls. For non-rental housing, self-reported imputed rents are used (from ENAHO). For districts that were urban only in either 2004 or 2019, rural households are included for the year these districts were not urban.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

8.3 A.3 Wage Growth Bartik: Computation

The wage-growth Bartik instrument is computed by interacting the log growth of national average wages by industry with districts' 2004 employment composition by industry and then summing up to the district-level. Note that national average wage growth by industry for a district does not include wage growth in that district. This is important for the exogeneity of the instrument because including the district wage growth in the calculation of the national wage growth might lead local labour supply shocks in that district to (non-trivially) affect the instrument, weakening the interpretation of the instrument as a local labour demand shifter. Mathematically, the districtlevel wage growth Bartik instrument, *Bartik*-$_D{}^{wage}$, is

$$Bartik_d^{wage} = \sum_{i=1}^{I} [(ln\ Wage_{i,-d,2019} - ln\ Wage_{i,-d,2004}) * \frac{N_{i,d,2004}}{N_{d,2004}}]$$

where $Wage_{I,-d,2019}$ is the average urban wage in industry *i* in all districts except district *d* in 2019, Wagei,–d,2004 is the average urban wage in industry i in all districts except district d in 2004, $N_{i,d,2004}$ is employment in industry *i* in district *d*, and $N_{d,2004}$ is the total employment in all industries in district *d*.

8.4 A.4 wage Growth Bartik: Results

Table 10: First-stage Regressions. Dependent Variable: LOg Growth in Urban Wages

Table 10: First-stage Regressions. Dependent Variable: Log Growth in Urban Wages

	(1) Log Wage growth	(2) Log Wage growth	(3) Log Wage growth
Bartik instrument	0.92***	0.94***	0.19
	(0.29)	(0.29)	(0.33)
Slum Population 2004 (log)		0.02	0.03
		(0.03)	(0.03)
Urban Rents 2004 (log)			-0.19***
			(0.05)
Urban Female share 2004			-0.45
			(0.55)
Constant	0.30	0.05	2.09***
	(0.22)	(0.38)	(0.61)
N	404	404	404
F-stat	10.39	5.35	7.62
R-squared	0.04	0.05	0.11

Notes: District-level regressions, robust standard errors in parentheses. Dependent variable is 2004-2019 log growth in average annual urban wages. Bartik instrument is calculated by interacting 2004-2019 log growth in national wages by industry (excluding wage growth of the district) with 2004-level local (district) industry shares (at ISIC Revision 3 2-digit level). Controls in column 2 include 2004-level of slum population. Column 3 adds 2004-level of average urban rents and female share of the district. For non-rental housing, self-reported imputed rents are used (from ENAHO). For districts that were urban only in either 2004 or 2019, rural households are included for the year these districts were not urban.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: OLS and 2-SLS Regressions.Dependent Variable: LOg Growth in Slum Population

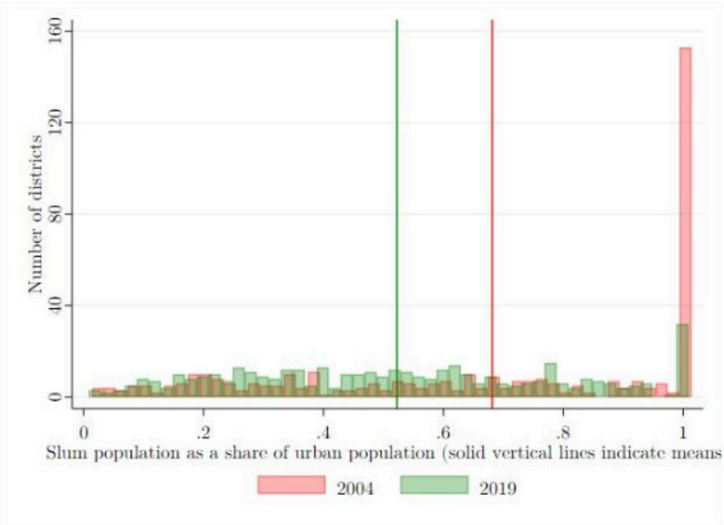
Table 11: OLS and 2-SLS Regressions. Dependent Variable: Log Growth in Slum Population

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	2SLS	2SLS	2SLS
Urban Wage Growth (log)	-0.24*	-0.21*	-0.04	-2.04***	-2.30***	-6.48
	(0.12)	(0.11)	(0.11)	(0.76)	(0.79)	(11.05)
Slum Population 2004 (log)		-0.44***	-0.47***		-0.41***	-0.27
		(0.05)	(0.05)		(0.07)	(0.39)
Urban Rents 2004 (log)			0.33***			-1.01
			(0.06)			(2.30)
Urban Female share 2004			-0.00			-2.84
			(0.89)			(6.39)
Constant	-0.06	4.13***	1.96***	1.77**	5.94***	17.09
	(0.14)	(0.50)	(0.70)	(0.76)	(0.96)	(26.17)
N	404	404	404	404	404	404
First-stage f-stat				10.39	5.35	7.62
R-squared	0.02	0.19	0.26	.	.	.

Notes: OLS and 2SLS district-level regressions, robust standard errors in parentheses. Dependent variable is 20042019 log growth in slum population. The independent variable of interest is instrumented 2004-2019 log growth in urban wages. Controls in columns 2 and 5 includes 2004-level of slum population. Columns 3 and 6 add 2004-level of average urban rents and female share of the district as controls. For non-rental housing, self-reported imputed rents are used (from ENA-HO). For districts that were urban only in either 2004 or 2019, rural households are included for the year these districts were not urban.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

9 B Supplemental Tables & Figures

Figure 7. Distribution of Slum Share, 2004 and 2019



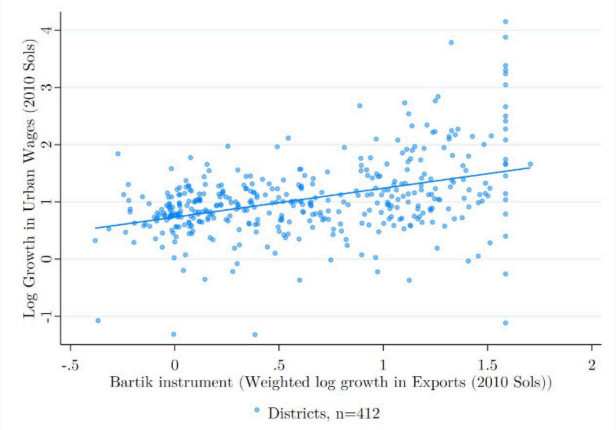
Source: Own processing of ENAHO data

Table 12: Industry Share Correlates, Next 5 Industries

	(1)	(2)	(3)	(4)	(5)
	Land Transport	Fishing	Construction	Education	Other Business
Urban Rents 2004 (log)	0.021***	-0.00044	0.014***	0.013***	0.016***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Urban Age 2004 (log)	-0.018	-0.0084	-0.011	-0.0015	-0.0053
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Urban Female share 2004	0.040	-0.033	-0.036	0.11**	-0.0058
	(0.05)	(0.06)	(0.04)	(0.05)	(0.04)
N	412	412	412	412	412
R-squared	0.13	0.0028	0.071	0.083	0.095

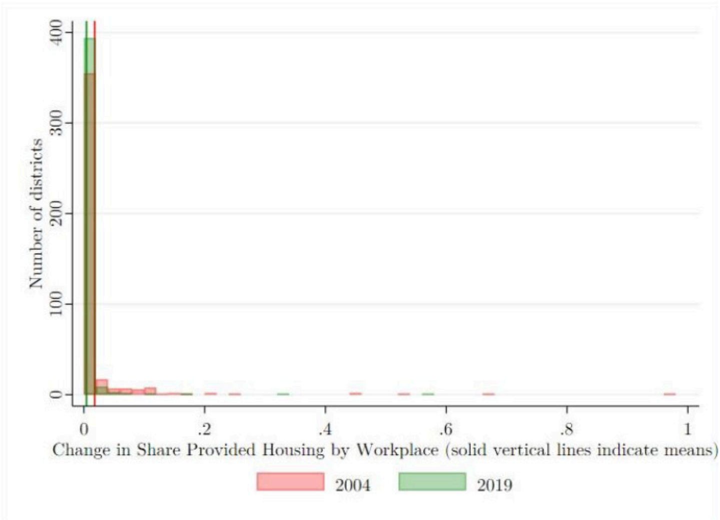
Each column reports results of a single regression. Outcome variables are 2004-level of an industry's share in columns 1-5. Independent variables are 2004-level local district characteristics. District-level regressions, robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$

Figure 8. First-stage: L0g wage Growth (X) vs Bartik Instrument (Z)



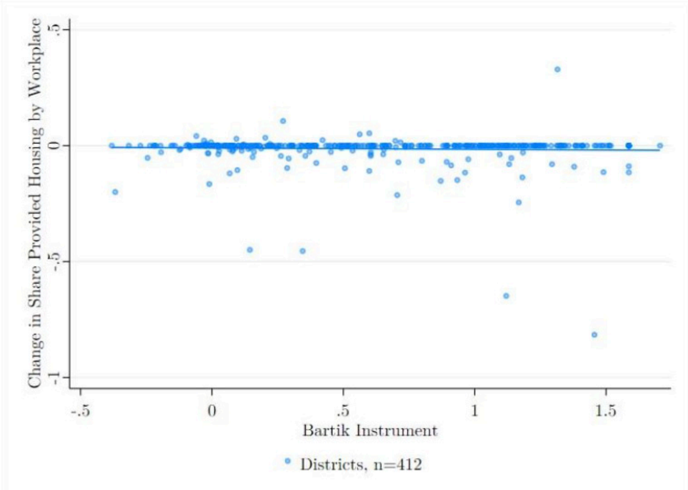
Notes: The clustering of data points at 1.6 on the x-axis is because of districts that employ workers only in one industry - Agriculture & Hunting - meaning the share of employment in Agriculture & Hunting for these districts equals 1. Source: Own processing of ENAHO data and BACI (UN Comtrade) data.

Figure 9: Distribution og Share of Urban Population Provided Housing by the Workplace



Source: Own processing of ENAHO data

Figure 10. Share of Urban Population Provided Housing by the Workplace vs Bartik Instrument



Source: Own processing of ENAHO data

Did the Clean Air Act Improve Environmental Justice Disparities?

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Abstract

This paper analyzes the differential impacts of the 1990 Clean Air Act Amendments (CAAA) on the racial pollution exposure gap, also known as the Environmental Justice (EJ) gap. Using recently developed, Census tract-level satellite data of PM2.5 pollution, I test whether CAAA non-attainment status and resulting State Implementation Plans decreased pollution in high-percentage Black and Hispanic areas more than in non-high percentage Black and Hispanic tracts. My results confirm that the CAAA reduced overall pollution concentrations in the U.S. and decreased the absolute level of the Environmental Justice gap. A heterogeneity analysis provides evidence that the results are primarily driven by air quality gains in Black communities in California and the Rust Belt.

Keywords: Environmental Justice, Air Pollution, Clean Air Act, State Implementation Plans, Public Economics

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1 Introduction

Regulating pollution is among the government's most important and controversial undertakings. On the one hand, regulating pollution has immense importance in preventing adverse human health impacts. Regulations are especially important for historically disadvantaged communities, where pollution can interact with other systemic problems to compound harm and create disparities. On the other hand, regulations can disrupt capital investments, jobs, and other economic activities and decision-making. This dichotomy makes the design and analysis of environmental policies of the utmost importance for public welfare.

This paper evaluates one of the most prominent air pollution enforcement mechanisms in the U.S., State Implementation Plans (SIPs), on pollution disparities across racial and demographic characteristics. Under the Clean Air Act (CAA) and its 1990 Amendments (CAAA), the Environmental Protection Agency (EPA) regulates the release of criteria air pollutants, setting maximum permissible pollution limits known as National Ambient Air Quality Standards (NAAQS). While the EPA sets NAAQS and deems whether areas meet those standards, the Agency leaves enforcement to states. When an area is deemed out of compliance, state, tribal, or local governments develop State Implementation Plans (SIPs), which lay out specific plans for bringing the area into compliance with NAAQS (EPA 2021a). The stricter regulation in non-attainment areas versus all other counties provides a convenient and useful source of variation to examine the effects of CAA regulations. Given the staggered implementation of policies over time (attainment status can "turn on and off" every year based on concentrations), there is ripe opportunity for experimental quantitative research methods to examine the impacts of these regulations.

Figure 1

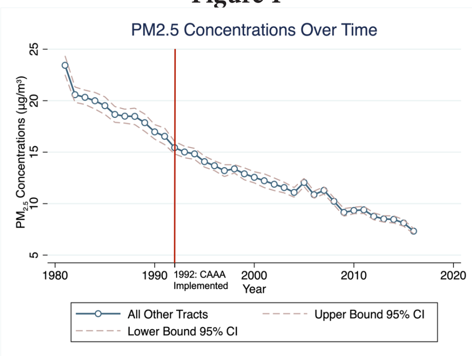


Figure 1 shows that pollution concentrations of Particulate Matter (PM)[3] 2.5 have fallen significantly since 1980. Prior research shows SIPs almost certainly played an important role in that progress (McKitrick 2007, Currie et al. 2020). However, gains are not evenly spread. Of particular concern are violations of Environmental Justice (EJ), defined as differential pollution or other environmental hazards faced by marginalized people, which continue to be documented in the popular press and peer-review literature (Shaw and Younes 2021, Cushing et al. 2018, Hernandez-Cortes and Meng 2021). These disparities fluctuate depending on demographics, economics, and policy changes. As EJ concerns increase in importance in the minds of policymakers, it is worth examining what impacts historic Clean Air Act Amendments' SIP rules have had on pollution exposure for disadvantaged communities. To get at this question, I ask whether the Clean Air Act Amendments decreased the quantitative pollution exposure gap between EJ-impacted communities and the rest of the population. I extend previous work, exploring both national trends in policy impacts across minority[4] and non-minority communities and conducting heterogeneity analysis of individual SIPs to ask where the most progress and EJ reductions have been made. My results confirm that the CAAA reduced pollution concentrations and that the EJ gap has shrunk since 1981. A heterogeneity analysis provides evidence that my results are driven by the significant gains experienced in the Rust Belt, Tri-State area, and Southern California.

2 Background: Air Pollution

2.1 Clean Air Act History and Policy

The Air Quality Act of 1967 was the country's first air pollution control program.[5] It established the NAAQS, specifying maximum allowable concentrations for six criteria air pollutants. The 1970 CAA and the 1977 CAAA shaped air policy into what we know today. The CAA requires states to develop SIPs that outline specific steps of how they will meet the NAAQS. SIP requirements are much stricter for regions in non-attainment, regions where a criteria pollutant exceeds the allowed NAAQS

3 The 10 and 2.5 in PM10 and PM2.5 mean x micrometers or smaller. See Section 2.2 for further definition.
4 Throughout this paper I will use the terms "minority," "people/communities of color," and "POC" interchangeable depending on circumstance to refer to Black and Hispanic individuals. Furthermore, while I would prefer to use the term "Latino" or "Latinx," it is standard practice in the literature to use "Hispanic" due to Census Bureau classifications. This nomenclature is not ideal, but I come to this research with the utmost deference to those communities who have experienced years of environmental racism and other forms of hidden oppression.
5 The regulatory details are summarized from: (Reitze 2004) and (EPA 2020).

concentration (42 U.S.C. 7401). SIPs are designed by a committee of the local regulatory body, elected officials, and representatives of local organizations and must be subsequently approved by the EPA (EPA 2020). The plans are approved or denied solely on emissions reductions criteria ("Revisions to Appendix…" 2018). As a result, neither justice-based concerns about disproportionate or cumulative impacts nor economic burdens are factored into the decision. The 1977 CAAA introduced the first technology-based performance standards on particulates from fossil-fuel fired power plants (Aldy et al. 2022).

Initially, the EPA prohibited the construction of new major polluting stationary sources[6] for non-attainment regions. However, they later adopted an offset policy where a new facility could be permitted by paying another facility in the same region to reduce emissions permanently (Reitze 2004). The policy has drawn scrutiny from EJ advocates, who fear that local governments permit new facilities to be built in disadvantaged communities, while allowing pollutant concentrations to decrease in affluent areas within the same county.

After the first ten years of the program, over 60 regions with a population of nearly 100 million remained in non-attainment of the 1977 NO2 and Ozone NAAQS (Reitze 2004). In response, the 1990 CAA Amendments divided attainment status by pollutant into classifications like moderate, serious, and severe. The classifications in some cases trigger a SIP policy mechanism like an offset market, inspection/maintenance program, or command and control requirements. As CAA regulations have been revised, technology evolves, and economic conditions change, states may submit revisions to their SIPs subject to approval by the EPA. One such regulatory change was the adoption of two separate standards for PM10, annual and 24-hour (EPA 2021b). The long-term measure protects against chronic respiratory conditions, while the daily measure protects against respiratory irritation and decreased cognitive function (Shehab 2019).

2.2 Air Pollution Types and Sources

The six main categories of criteria air pollutants regulated by the NAAQS are sulfur dioxide (SO2), nitrogen dioxide (NO2), carbon monoxide (CO), particulate matter (PM10 and PM2.5), photochemical oxidants measured as ozone (O3), and lead (EPA 2021c).[7]

6 Defined as facilities with 100+ tons pollution/year, i.e. power plants, manufacturing.

7 SO2 is a component of sulfur oxides that is primarily generated by power plants and other industrial facilities and causes respiratory issues in humans, haze in the sky, and chemical reactions that form PM. (Continued on the next page)

The main pollutant of interest for this paper is PM2.5, as it is the most studied within the EJ literature due to its adverse health impacts. Many sources contribute to PM, including direct sources such as smokestacks and fires and indirect sources such as power plants, industrial manufacturing, and automobiles. Unlike the other criteria pollutants, which are chemicals, PM is composed of solid particles and airborne liquid droplets. These particles are 30 times smaller in diameter than a human hair and manifest as dust, soot, and smoke that is inhaled and penetrates the lungs posing a health risk at high exposure levels (EPA 2021d).

2.3 Air Pollution Health and Economic Impact

Pollution negatively affects health outcomes in the short- and long-term, which causes economic harm due to resulting respiratory and cognitive ailments such as low birth weight and reduced educational outcomes. Researchers have documented these impacts across several pollutants and settings (Bell et al. 2010, Stingone and McVeigh 2016). For example, ozone harms individuals with asthma and negatively affects worker productivity, even at levels below NAAQS standards (Zivin and Neidell 2012). Similarly, PM2.5 has constrained the Chinese GDP through reduced labor hours and higher medical expenses (Wu et al. 2017). If pollution reduces productivity, as these findings show, abatement can be seen as a form of human capital investment, boosting productivity and growth. Combined with improved health outcomes, these economic impacts provide a strong motivation for the government to pursue pollution control, as they can strengthen local markets (Chakrabarti and Mitra 2005) and the macroeconomy (Leeves and Herbert 2007).

2.4 Disparate Health Impacts of Pollutions

Communities of color are disproportionately susceptible to pollution's harms due to compounding social/economic factors that correlate with race and income. Recent epidemiological literature links long-term PM2.5 exposure to worsened COVID-19 mortality outcomes (Wu et al.

7 The sources and environmental effects of NO2 are largely the same as SO2, though it also reacts with other chemicals in the atmosphere to cause Acid Rain.
CO primarily comes from fossil-fuel combustion such as vehicles and home heating and causes oxygen limitation in the body and long-term risk of heart conditions.
Ground level Ozone is the main ingredient in smog. It is formed by reactions with nitrogen oxides and volatile organic compounds (VOCs) after emission from vehicles, refineries, chemical plants, and other industrial facilities. Those with asthma are at highest risk from ozone, typically on hot summer days. It also has adverse ecological effects.
Lead gets in the air primarily through smelters like metals processing operations and waste incinerators. It has significant adverse health impacts, especially in children who experience negative neurological effects. The EPA's regulation of leaded gasoline was a major success, as air lead levels dropped almost 100% over 30 years.

2020). The EJ literature also correlates higher COVID vulnerability to co-dependent social factors like urban/industrialized areas, low household income, low educational attainment, and Black population share (Hooper et al. 2020). This problem extends beyond COVID and lung cancer. Populations can be vulnerable to death from PM exposure alone. Whether you examine direct mortality or correlated illnesses, fine particulate matter is undoubtedly a health crisis. Pope et al. (2009) estimate a 0.61-year reduction in life expectancy for each 10 $\mu g/m^3$ increase in sustained exposure to PM2.5. Deryugina et al. (2020) find that higher PM exposure correlates with worse health outcomes and lower socioeconomic status. EJ disparities are pronounced in this context, though positive welfare gains in this century demonstrate that pollution abatement has made a difference. With the Black-White life expectancy gap closing by 1.5 years over a 15-year span (Arias et al. 2019), Currie et al. (2020) calculated that 4% of this improvement could be explained by just a 1 μg/m3 closure of the Black-White pollution gap.

3 Literature Review

3.1 Historical Environmental Justice Literature

The EJ field is interdisciplinary and was spurred by a grassroots social movement. Its first literature was published in the 1980s (Banzhaf et al. 2019). The field's most famous paper was published in 1987 by the Commission for Racial Justice, which documented a correlation between race and proximity to hazardous waste facilities. Since then, scholars have rigorously shown that low-income and people of color are disproportionately exposed to environmental hazards (Evans et al. 2002, Hsiang et al. 2019, Tessum et al. 2021). Disparities are examined as a determinant of interest across numerous socioeconomic variables- including income/poverty (Hsiang et al. 2020), and age (Gray et al. 2010). The field, closely intersecting with epidemiology, links pollution exposure to a myriad of maladies. Pope (1991) presented the first seminal work, studying PM10 pollution from steel production in the Utah and Salt Lake Valleys and associated increases in respiratory-related hospital admissions.

A common theme across much of the EJ literature is that the location and intensity of polluting facilities depend on local economic and demographic factors (Ringquist 2005; Mohai and Saha 2007). Communities of color are more likely to house polluters (Bullard et al. 2008). Some authors have offered lower political participation as an explanation of this phenomenon, relying on findings from political science that race, income, and health all impact political participation and clout (Michener 2017). In a hypothetical scenario where a new coal plant is opening down the road, who is more likely to comment against it at a city council meeting - a wealthy homeowner or a renter working two jobs? Research suggests the former. Cicatielo et al. (2015) find across 47 countries that wealth is positively linked to conventional political participation. On the environmental side, Hamilton (1995) finds that race, educational attainment, and homeownership predict a community's ability to mobilize against the entry of polluting facilities into an area, raising the expected costs to firms of locating in particular areas. Confirming this point with data, Gray et al. (2010) find that polluting plants in higher voter-turnout-areas face greater regulatory activity. This hypothesis motivates the location-specific costs described in my theory section.

3.2 Economic and Pollution Impacts of the Clean Air Act

State Implementation Plans' efficacy and their responsibility for overall pollution declines are disputed. Reitz (2004) classifies them as a "failure" due to uneven implementation costs, increasing population and manufacturing, and overly optimistic abatement projections. Greenstone (2004) finds that non-attainment status only played a minor role in the impressive 80% drop in sulfur dioxide (SO2) pollution since 1970. However, Greenstone (2002) also pins specific declines in polluting industrial activities to non-attainment status designation: 590,000 jobs, $37 billion in capital stock, and $75 billion (1987$) of output over the first 15 years of the first CAA Amendments. Shapiro and Walker (2018) hypothesize and assert that the large decrease in manufacturing emissions is largely a result of environmental regulation, making pollution more costly, though this is not directly on SIPs. Auffhammer et al. (2009) estimate the effects of non-attainment status on PM10 concentrations at ground-level monitors. They find a treatment effect of -12.5% and that the treatment effect occurred independently of SIP implementation, indicating a regulatory anticipation effect is present. I address this in my model in Section 7.1. In an overarching CAA literature review, Alby et al. (2022) find consistent evidence that pollution declines more rapidly at air monitors in non-attainment counties than those in attainment.

3.3 Environmental Justice Outcomes of the Clean Air Act

While national impacts of the CAA are well-investigated, the EJ literature related to the Act is burgeoning and informs my research. As previously mentioned, firms can trade pollution permits to offset their emissions when in a non-attainment area. These markets are one abatement method that areas can use in their SIPs under the circumstance that they want to permit a new polluting facility to be built. EJ advocates have criticized permit trading programs for potentially allowing pollution to move into minority/low-income areas (Cushing et al. 2016). Shapiro and Walker (2021) analyze the criteria pollutant offset markets legislated through the CAA. They find no substantial effects on pollution movement to communities based on race or income in twelve prominent offset markets. A study of one specific NOx market in the heavily polluted South Coast Air Basin found pollution reductions that do not vary significantly across demographics (Fowlie et al. 2012). Ringquist (2011) studies the sulfur dioxide allowance trading program and finds that communities with high percentages of Black and Hispanic residents experience fewer imports of sulfur dioxide. So, the evidence is inconclusive on the overall efficacy of SIPs, but strongly suggests that new facilities under non-attainment do not harm the EJ gap individually.

In an important nationwide study, Colmer et al. (2020) find that though particulate pollution levels have dropped overall, the most and least polluted areas in 1981 remain so today. They examine non-attainment status as a predictor variable and find that Census tracts in PM2.5 non-attainment before 2016 are associated with an average decrease of 7.09 percentile rank points against all other tracts between 1981 and 2016, i.e., SIPs are associated with declining pollution levels. They also observe a general, though not universal, narrowing of the pollution exposure gap in disadvantaged communities. This is a timid quantification, and my study explores it further. My paper is most closely related to Currie et al. (2020). The authors study differential impacts of the CAA on pollution exposure. The authors find that the Black-White pollution exposure gap has closed since 2000. They attribute 60 percent of the racial convergence in PM2.5 exposure to the CAA. Konisky (2009) can help explain this phenomenon, as following updated federal guidance in the mid-1990s to address EJ concerns, there was evidence of increased CAA enforcement in Black communities and lowered enforcement in Hispanic and poor communities.

3.4 Contribution to the Literature

The EJ literature examines disparities as a determinant of interest across numerous socioeconomic variables- including race (Bullard et al. 2008), income/poverty (Hsiang et al. 2019), education (Colmer et al. 2020), age (Gray et al. 2010), and others (Hausman and Stolper 2020). As explained in Section 6.2, race is the most important dimension of heterogeneity in PM2.5 outcomes. My data analysis shows that racial concentrations are much more predictive of pollution than income or poverty. As such, I focus on disparities across race. Regarding disparities across varying racial/ethnic groups, most authors have studied pollution and health outcomes for Black and Hispanic communities (Konisky 2009; Ringquist 2011; Shapiro and Walker, 2021; Mansur and Sheriff 2021), due to the risk factors recounted in Section 2.4. In states where EJ policy accounts for additional socioeconomic and health factors, vulnerable groups are often referred to as "disadvantaged communities," and studies such as Hernandez-Cortes and Meng (2021) examine changes in pollution on those communities defined in statute. The foundation for my paper, Currie et al. (2020), studies the Black-White pollution gap over time. To build on findings in the literature and bring the conversation in line with previous papers, I approximate the Currie et al. research design, but study the heterogenous effects of the CAAA on the EJ gap for Black and Hispanic Americans.

Another avenue of my contribution is in the data. Researchers have used many methods to analyze air pollution outcomes. These include using facility-level data (Shapiro and Walker 2018) and pollution dispersal models (Hernandez-Cortes and Meng 2021) to approximate where pollution is experienced. Other analyses utilize ground-monitor data from government-run stations (Auffhammer et al. 2009) combined with Census and geographical data (Pope et al. 2002). A recent innovation in the field is satellite remote sensing, which provides broad measurements going back decades. My dataset (explained fully in Section 5) is drawn from Colmer et al. (2020) and Meng et al. (2018), which combines all three of the methods above to create annual, Census Tract level pollution measures.[8] Additionally, my data contributes to the literature by connecting the findings of treatment effects across PM10 regulations (Auffhammer et al. 2009) and PM2.5 regulations (Currie et al. 2020) on PM2.5 outcomes.

My basic structure of difference-in-differences and event studies mimics the design of Currie et al. (2020), though it diverges in a few features.

8 There are numerous advantages to this approach, which are laid out in Section 5.

Currie's policy variables are aggregated by "Commuting Zones" (clustered at the local labor market level) to replicate how the regulations work in practice. Their dependent variable is pollution exposure at the individual level. For simplicity and data limitations, I analyze policy at the county and pollution at the tract level. Furthermore, Currie combines DiD with unconditional quartile regression to estimate counterfactual pollution distributions. They define 19 PM concentration cutoffs using re-centered influence function regressions, then for each, estimate the effect of non-attainment on the probability of moving above the cutoff. This technique is advantageous as it provides stronger backing for causality. However, such an approach is beyond the scope of my paper, though my regression results across multiple specifications are in line with Currie's results. Other methods in the literature include Shapiro and Walker (2021) who use a similar difference-in-differences model to test pollution and permit trading activity across Black and Hispanic communities, but do not find significant results. Colmer et al. (2020) use rank-rank correlations to effectively analyze distributional changes over time, though they are correlated findings and do not robustly control for confounders to prove an EJ causation claim. I utilize their data to attempt to extend those findings. With an “EJ gap” as an outcome of interest, Hernandez-Cortes and Meng (2021) use trend breaks of emissions to examine the impacts of California’s Cap and Trade program. As explained in Section 7.1, my model incorporates elements of these studies to attempt to draw a causal picture of the CAAA.

Last, I conduct a novel heterogeneity analysis. The findings illustrate which non-attainment areas drive my average treatment effect and suggest substantial heterogeneity across the U.S. Future work may link this heterogeneity analysis to specific SIP features to better understand the policy mechanisms and local relationships that shape environmental outcomes drive my results.

4 Theoretical Framework

This section summarizes my theoretical framework, documenting economic theories on firm and individual location decisions that may give rise to observed EJ gaps.

4.1 Consumers' Willingness-To-Pay for Clean Air

A central concept in environmental economics is willingness-to-pay (WTP) for clean air. Economists use consumer purchases in a variety of settings, for example, purchases of indoor air purifiers, to estimate individuals' WTP for clean air (Ito and Shuang 2020). Housing is a key market where economists study these issues. When looking at residential housing, more polluted neighborhoods generally have lower property values, ceteris paribus. Economists use heterogeneity in housing characteristics and pollution to estimate WTP for cleaner air based on consumers' locational choices (Bazhaf et al. 2019).

Coase (1960) first hypothesized that firms might locate in poor neighborhoods due to lower potential compensation by the firm to residents (an evaluation of WTP). Following this logic, poorer populations may sort into polluted areas if they prioritize other essentials rather than environmental quality. This process, broadly referred to as Tiebout sorting, links environmental and other social inequalities as a function of wealth and preferences (Banzhaf and Walsh 2013). While observed pollution differences across neighborhoods can cause such exposure gaps to emerge in theory, recent evidence to suggests disadvantaged communities may also have lower WTP for environmental quality due to "hidden" pollution driven by disparities in information about air quality (Hausman and Stolper 2020).

When applying these concepts to PM2.5 pollution in the United States, the data do not support the theory. My statistical analysis in Section 6 demonstrates that the main delineation of PM2.5 pollution inequality in the U.S. is race, not income. It is a finding supported by Currie et al., who statistically show that income differentials explain almost none of the pollution exposure disparities between Black and White populations. While income disparities still persist on many other forms of pollution, additional theory is needed in this research context. To examine outcomes, I draw upon a broader definition of welfare.

4.2 Defining Welfare Beyond Income

In economic theory, social outcomes and expected utility are often modeled as a function of income, with worse outcomes assigned to the poor

(Atkinson 1999). However, the Nobel Prize winning economist Amartya Sen pushed the field of Welfare Economics to quantify a multifaceted, humanist approach. One of Sen's notable contributions was to analyze a person's "capability," rather than their utility when assessing their welfare. Capability refers to their freedom of choice and ability to achieve (Sen 1982). For example, a person's ability/freedom to ride a bicycle is not just determined by their income, but their physical health, knowledge, and environmental endowment.[9] Any of those commodities enhances one's capability to bike. Heavy air pollution, on the other hand, would deteriorate the performance of a child's lungs, their biking capability, and overall welfare. While I do not specifically model capability, it is a useful framework of welfare when examining EJ outcomes.

To explain racial pollution disparities, Sen might assert that, rather than minorities having lower WTP for clean air, we should look to the complex structural factors and endowments determining welfare outcomes beyond income. Key among those factors is a racial wealth gap that exceeds the income inequality gap in the United States (Williams 2017). The racial wealth gap literature diagnoses this problem and frames this paper's link between race and welfare. There are several proposed explanations. Williams (2017) introduces a Wealth Privilege model where wealth is easily transferred down through generations, a process dominated by Whites and furthering economic stratification. Intergenerational wealth transfers are a consensus cause in the literature of the racial wealth gap (McKernan et al. 2014, Darity et al. 2018, Ashman and Neumuller, 2020). In analyzing the impacts of a lack of generational wealth, Herring and Henderson (2016) study "wealth characteristics." They find Black Americans lag behind Whites in ownership of homes, stock, and businesses, as well as receive lower wealth returns to income, education, age, and the previously mentioned assets.

A lack of these wealth characteristics has translated to communities of color being disadvantaged in terms of economics, health, environment, education, housing, and other compounding factors. But why must there be "communities of color" in the first place? Here, the racial wealth gap manifests as residential segregation by race. There are also multiple explanations in the literature for residential segregation. Historically, White government officials and developers used racist policies such as redlining and racial covenants to exclude Black people from their neighborhoods (if not outright intimidation and violence) (Boustan 2013). Recent studies have demonstrated the long-lasting impacts of these policies. Aaron

9 Based on a similar hypothetical presented by Sen (1979).

son et al. (2021) show that 1930's redlining maps led in the following decades to racial segregation and wrought reduced homeownership rates, housing values, and credit access. Whipple (2021) finds that homes with racial covenants in the early 20th century were significantly less likely to be foreclosed during the Great Recession and redlining. In the second half of the 20th century, Logan and Parman (2017) show increasing rates of residential sorting were caused by a combination of White flight, urbanization, and deindustrialization, and increased racial sorting at the household level. Then, as explained in the following section, pollution disparities occur as industrial activity moves into segregated neighborhoods.

4.3 Pollutions Havens vs. the Porter Hypothesis

A key question in the EJ literature is how polluters choose their sites. As summarized in Shadbegian and Wolverton (2010), early location theory papers identified natural resource abundance, labor availability, local wages/unionization, market size/proximity, transportation costs, and various production costs as key criteria for firm location based on the principle of profit maximization. More recent literature has turned its attention to EJ questions such as regulation and community welfare. If individuals in disadvantaged communities have a lower WTP for environmental quality (Bazhaf et al. 2019), firms have lower marginal costs from building facilities in those areas. The theory behind this phenomenon dates back to Olson (1965), who connected it to the free-rider dilemma of public goods. He hypothesized it is cheaper for firms to locate in areas where collective action against expected pollution is less likely. A basic economic model considers this as allocatively efficient, as pollution is produced equal to the value society places on it and the government regulates it as an externality. However, governments often do not properly price the externalities of pollution, resulting in a market failure. Furthermore, EJ scholars advocate for policymakers to integrate the cumulative impacts of pollution on marginalized populations into the social cost of pollution. While the Clean Air Act does not consider disparate impacts, it attempts to enforce that social cost of pollution.

"Pollution havens" are an idea that firms locate where environmental regulations are laxer and hypothesizes a negative impact on firms' competitiveness from increased regulations when they are already sited (Dechezleprêtre and Sato 2017). While the theory is typically applied on an inter-country scale, it has relevancy for intra-country analysis. The hypothesis that environmental regulations, all else being equal, will reduce a firm's economic competitiveness was first raised by McGuire (1982). Pollution havens have been rigorously debated, and their empirical evidence is far from conclusive due to issues of endogeneity (Levinson and Taylor 2008). There is, however, a competing hypothesis, The Porter Hypothesis, that implementing a stringent policy on a firm will trigger the economic incentive for them to invest in pollution abatement technologies. The authors hypothesize further that adopting clean technologies can raise productivity and costs savings to the point where the firm profits from the change in the long run (Porter and van der Linde 1995). This theory and academic conversation provide a narrative of the possible causal relationship around the closing EJ gap. If the Porter Hypothesis holds, communities of color experienced disproportionate gains in clean technology (and possibly industrial productivity). My data exploration and regression results provide quantitative backing for this story, while the heterogeneity analysis introduces additional possibilities.

5 Data

This paper investigates whether the Clean Air Act Amendments decreased pollution exposure disparities, and if so, to what extent were SIPs and non-attainment status responsible for the change? To answer this question, it is necessary to have historical measurements of air pollution that are trustworthy and precise enough to mitigate concerns of environmental fallacy.

Much of the publicly available pollution data in the United States comes from the EPA's monitoring stations. While they are the foundation for much of the country's air quality enforcement, the network is notoriously flawed. First, there are not enough monitors to ensure every American can know their exposure. Not only does monitoring vary by race and income in dense areas (Stuart 2012), there are large swaths of rural America that are not covered at all (Garcia et al. 2016). Perhaps the most significant issue in air monitoring, however, is intermittency. Due to resource limitations, some monitors only collect data once every 6-12 days. With this schedule being public knowledge, some polluters have strategically adjusted their emissions to stay within attainment on those days (Zou 2021).

Herein lies the importance of a recent innovation: satellite data on pol lution. Zou (2021) uses such data to show that air quality is significantly worse on unmonitored days due to arbitrary measurement techniques. The most relevant study using these methods is Meng et al. (2019), who estimated ambient fine particulate matter concentrations across North America from 1981–2016 through chemical transport modeling, satellite remote sensing, and ground-based measurements. This provides a much more accurate picture of PM2.5 distribution than previously available to researchers, which has since strengthened the corollary claims able to be made.

I use pollution data from Colmer et al. (2020). The authors use satellite data of PM concentrations across the U.S. from Meng et al. to construct annual PM measures at the Census tract level. The authors construct the measures using satellite data, a chemical transport model, and ground station measurements of PM2.5 and PM10. PM2.5 and PM10 are highly correlated[10] (Colmer 2020), so one can reasonably be used to estimate the presence of the other. For this reason, while I primarily study impacts of CAAA PM10 regulations, my main outcome of interest is mean annual PM2.5 (μg/m^3) at the tract level. My non-attainment data comes from the EPA Green Book.

6 Descriptive Statistics

6.1 Variable Description

Table 1(a) examines average, tract-level statistics for my variables of interest in 1992. I split the data based on whether their tracts were in attainment of the 1990 EPA PM10 standards. 1992 is significant because it is the year that I specify 90th percentile P.O.C. tracts (1990 Census), and the first year of EPA's PM enforcement. Table 1(b) maintains 1990 Census data while including mean PM concentrations for all years in my sample and splits the data based on whether the tracts were ever in non-attainment.

High (90th percentile) minority tracts comprise around 8% more of total non-attainment tracts than attainment tracts. This relatively small figure confirms that the EJ disparity is driven by high minority tracts being on

10 Colmer et al. notes that "prior to the year 2000, data on PM10 were used in place of ground station and satellite data on PM2.5, when PM2.5 records were unavailable. All estimates indicate that there is very high persistence in rank over time, irrespective of which base year is used." Many studies have examined this correlation, with it ranging from R=0.64 (Munir et al. 2016) to R=0.95 (Janssen et al. 2013). For my purposes, this evidence is sufficient.

the upper tail of PM distribution rather than by a significantly higher number of communities of color being in non-attainment. Furthermore, across both splits, income is notably higher in non-attainment tracts. Compared to the data on racial groups in non-attainment, income seems relatively insignificant as a predictor of PM.

Table 1: Variable Description

(a) **When PM10 Nonattainment in 1992 = 0**		**When PM10 Nonattainment in 1992 = 1**	
N = 53,969 tracts/observations		N = 10,728 tracts/observations	
Variable	Mean	Mean	t-test: Mean(0) - Mean(1)
Mean PM2.5 μg/m3	15.08	17.27	-2.190***
Population	3,825.58	3,773.41	52.169***
Proportion 90th Percentile Minority Tracts	0.09	0.17	-0.088***
Share Population Minority	0.19	0.31	-0.116***
Share Population Black	0.12	0.13	0.00
Share Population Hispanic	0.07	0.18	-0.115***
Share Population in Poverty	0.13	0.14	-0.009***
Income per Capita	14,145.02	15,059.87	-914.849***
(b) **When PM10 or PM2.5 Nonattainment = 0 (1981-2016)**		**When PM10 or PM2.5 Nonattainment = 1 (1981-2016)**	
N = 37,455 tracts/observations		N = 27,250 tracts/observations	
Mean PM2.5 μg/m3	12.43	15.66	-3.239***
Population	3,783.47	3,862.55	-79.087***
Proportion 90th Percentile Minority Tracts	0.07	0.15	-0.082***
Share Population Minority	0.17	0.26	-0.090***
Share Population Black	0.11	0.14	-0.034***
Share Population Hispanic	0.06	0.12	-0.056***
Share Population in Poverty	0.14	0.12	0.021***
Income per Capita	12,961.74	16,129.01	-3167.263***

The value displayed for t-tests are the differences in the means across the groups.
***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

6.2 Nationwide net pollution changes

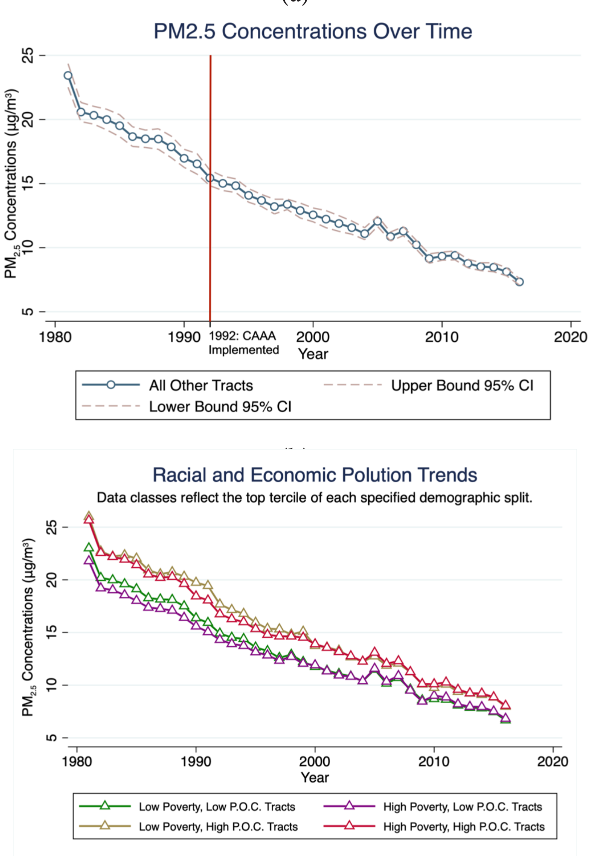
Figure 2 shows key trends that provide a basis for my hypothesis. They display mean PM2.5 concentrations from 1981 through 2016. Subfigure (a) displays concentrations in high-minority tracts against all tracts, where I observe a perceptible shrinking of the racial pollution exposure gap from roughly 4.5 μg/m^3 to 1.5 μg/m^3.[11] It supports the key finding of Currie et al. (2020) over an expanded time frame. This is a highly

11 One may reasonably observe that while the level change of the gap is negative, the gap in percentage terms of overall pollution is has not significantly changed. Many examine inequality in percentage terms. But for pollution, negative health impacts occur at high concentrations, so I define my EJ gap in level terms to reflect low marginal impact at low concentrations.

important finding, because Schwartz et al. (2021) finds an increase in life expectancy of 0.29 years when a population was exposed to 7 μg/m^3 versus 12 μg/m^3 of PM2.5. This is a drop similar in means to my EJ gap finding, so a gap closure is likely improving disparities in health and life expectancy outcomes. Something must be driving the gap closure, whether policy, demographic change, or an omitted variable. Subfigure (b) motivates my model specification. Pollution trends of high and low minority tracts cluster together independent of poverty levels. Moreover, the heterogeneity based on poverty status disappeared around 1998. These two observations tell us that, while everyone experienced cleaner air over time, people of color experienced environmental inequality and exposure changes in the past 40 years. As such, I leave the discussion here to solely focus on race.

Figure 2

(a)



6.3 Maps

Figure 3 maps pollution and attainment status across the country in 1992, the first year of enforcement of the first nationwide Particulate Matter standards (PM10), and in 2005, the first year of the PM2.5 standards. PM2.5 concentrations in 1992 were highest in industrialized areas like the Rust Belt, the Southeast, and Southern California (Figure 3a). Those spatial patterns held, but overall concentrations fell over time (Figure 3b). Non-attainment status with PM10 standards, on the other hand, was primarily concentrated on the West Coast (Figure 3c). Counties in non-attainment for PM2.5 were concentrated in the Rust Belt, the Tri-State Area, and Southern California in 2005 (Figure 3d). There was a disparity between pollution concentration patterns and PM10 non-attainment status in 1992 due to the different particle composition. Despite this, examining the concentrations in 2005 relative to the PM2.5 non-attainment map, it appears the PM10 regulations were successful in reducing pollution.

Figure 3

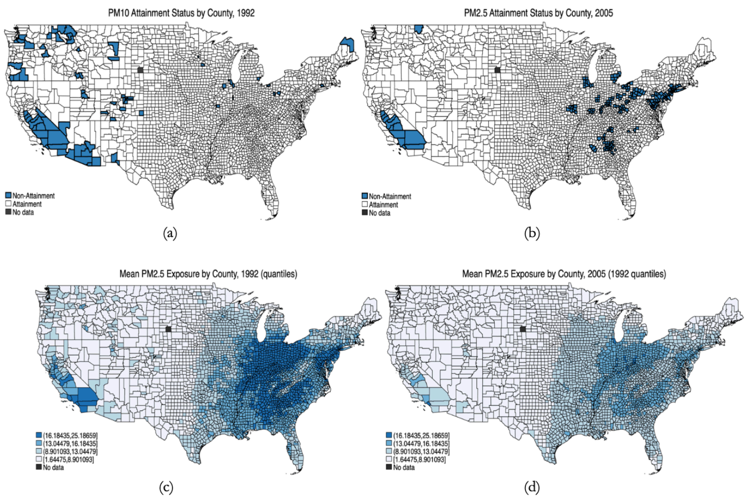
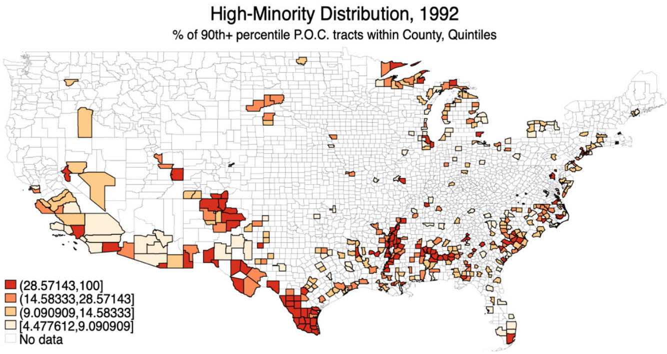


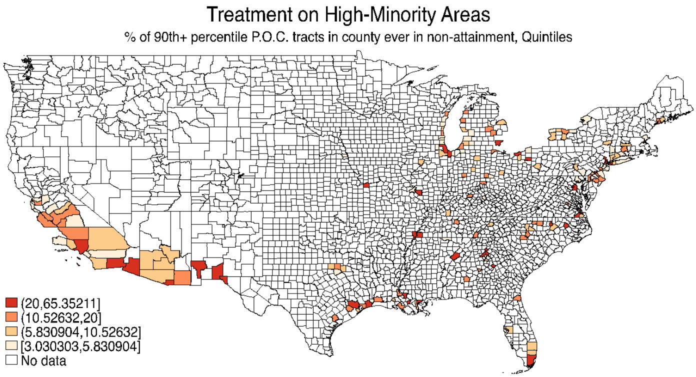
Figure 4 examines the geographic dispersion of my key demographic variable of interest, high-minority tracts. The maps show the percentage of high-minority tracts within each county. Communities of color are densely concentrated around each coast, with other clusters in the Great Lakes region, the inland southwest, and the inland southeast. Cross-ref erencing with the PM maps, correlations between race and pollution are highest in the Southeast and Southern California. Figure 4(b) overlays the location of high-minority tracts with non-attainment status. I observe more instances of treatment for communities of color in the Southwest and the Northeast. Figures 4(a) and 4(b) shows that high-minority areas were covered by both the PM10 and PM2.5 rounds of the CAAA regulation.

Figure 2

(a)



(b)



7 Findings

7.1 Model

I use difference-in-difference (DiD), stacked DiD, and event study models to study the impact of a county being designated as non-attainment on PM2.5 pollution. I estimate my DiD model using two-way fixed effects, evaluating the average treatment effect of county non-attainment status T_c on PM2.5 concentrations in tract i in county c and year t (Y_{ict}) after the county was designated as being in non-attainment ($Post_{ct}$). I use tract and year-level fixed effects to account for unobserved geography-specific and time-specific confounders. In each regression, all standard errors are clustered at the county level, which allows for tracts located in a single county to have correlated errors. My regression equation is:

$$Y_{ict} = \beta_0 + \beta_1\left(Post_{ct} * T_c\right) + \gamma_i + \gamma_t + \varepsilon_{ict} \qquad (1)$$

Recent literature raises concerns with the TWFE model in staggered treatment contexts (Borusyak and Jaravel 2016). Attainment status differs over time for some counties, with counties entering non-attainment status as early as 1992 and as late as 2009. (Goodman-Bacon 2021). This is likely the case in my study since we would expect heterogeneity in treatment effects due to technological advancements, updated legal code, and the anticipation effect of impending regulatory approval and enforcement (Malani and Reif 2015). Further bias may result from more recently treated counties serving as controls for earlier treated counties.

To address these concerns, I first use an event study, exploiting within county variation in treatment status in "event time," or time since each treated county was placed into non-attainment status.[12] This approach has a further advantage since the visualization of the event study regression allows me to investigate parallel trends in the years leading up to treatment. While maintaining the TWFE, I study heterogeneous policy impacts through the event-year indicator j and collinear time indicators [t=τ]. I estimate the equation:

$$Y_{ict} = \sum_{j=-\tau}^{\tau} \beta_j 1[t = j] + \gamma_i + \gamma_t + \varepsilon_{ict} \qquad (2)$$

12 For counties that enter, exit, then re-enter non-attainment, the treatment is measured at the first occasion.

After the basic event study, I then interact all event study terms with indicators for high-P.O.C. tracts in a regression to evaluate heterogeneity.

I also implement a 'stacked' difference-in-differences model. This model addresses concerns of time variation in controls by creating 'clean controls' in an event-specific panel dataset (Cengiz et al. 2019; Beatty et al. 2021). For each year where treatment occurs, a dataset is created with 10 years pre- and post-treatment of event time and never-treated counties as the controls. All those datasets are merged ('stacked') to create a dataset where previously treated tracts do not serve as controls for tracts entering non-attainment years in the future. The regression equation is identical to equation (1) with the alternatively constructed data.

I test for differential outcomes from treatment for EJ communities using a triple Difference-in-Differences (DDD) model (Olden and Møen 2020; Gruber 1994; Cunningham 2021). In this case, the initial treatment split is on attainment status (county level), while the additional split is high-minority tracts H_i versus non-high-minority tracts. The outcome Y_{ict} of the DDD model, specified below, denotes PM2.5 levels by year t, tract i, and county c.

$$Y_{ict} = \beta_0 + \beta_1 H_i * Post_t + \beta_2 T_c * Post_t + \beta_3 T_c * H_i * Post_t + \gamma_i + \gamma_t + \varepsilon_{ict} \quad (3)$$

The parameter of interest is β3, the coefficient estimating the difference in pollution for high-minority tracts in non-attainment after the policy.

7.2 Regression Results

Table 2 presents my regression results for the impact of attainment status on average PM2.5 concentrations across all counties. Both columns present model results adjusting for tract and year TWFEs, while column 2 implements the stacked regression dataset. Across both specifications, I confirm prior research and find that the CAAA were responsible for the lower pollution in non-attainment tracts relative to never-treated tracts. This result indicates that non-attainment status can explain around 10% of the ~16 $\mu g/m^3$ drop in nationwide mean PM2.5 concentrations (Figure 1).

Table 2
Difference-in-Difference Results

	(1) TWFE DiD	(2) Stacked DiD
	Mean PM2.5 µg/m3	
Nonattainment (Treatment)	-2.025***	-1.599***
	(0.237)	(0.220)
Constant	14.19***	12.60***
	(0.0473)	(0.00615)
R-squared	0.939	0.935

Robust std. errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Figure 5 presents my event-study model results. In the 'before' period, pollution does not appear to follow any identifiable trend, supporting my research design though the parallel trends assumption. I observe an immediate drop in pollution though year four, then again after year ten. This indicates the presence of a composition effect, where all regulatory impacts are reflected in the short term, while older PM10 regulations are driving further decreases in the later event time. I decompose these year-over-year trends in greater detail in the Appendix (Figure 8).

Figure 5

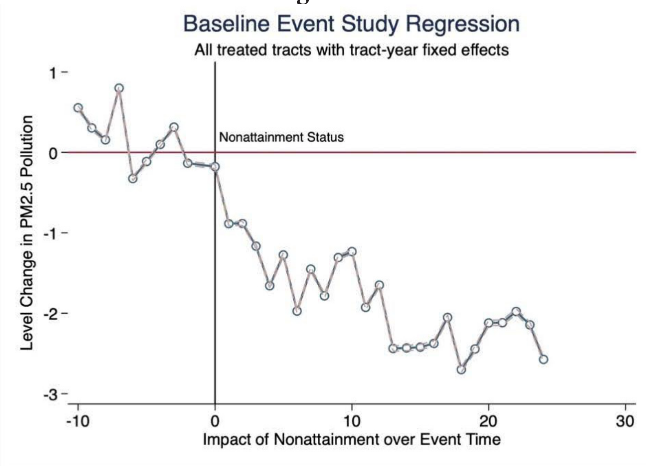


Table 3 is my main regression table. Like before, I specify TWFE and stacked regression models. The DDD controls for the heterogeneity of non-attainment status and high-minority status. The key interaction term in column 2 highlights that high-minority tracts saw a -0.89 μg/m^3 greater drop in PM2.5 on average than non-high-minority tracts after entering non-attainment status. This indicates that non-attainment status can explain around 25% of the 3.5 μg/m^3 decrease in the EJ gap (Figure 2a) over the last 35 years.

Table 3

Triple Difference Regression Results

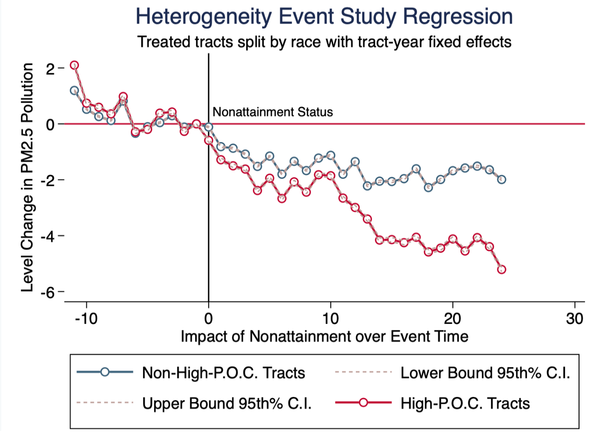
	(1) TWFE DDD	(2) Stacked DDD
	Mean PM2.5 μg/m3	
Nonattainment (Treatment)	-1.815***	-1.464***
	(0.212)	(0.198)
High-P.O.C. Treatment Interaction	-1.383***	**-0.889*** **
	(0.286)	(0.211)
Constant	14.19***	12.60***
	(0.0442)	(0.00582)
R-squared	0.939	0.936

Robust std. errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Lastly, Figure 6 is an additional event study. Treatment groups are split between high and low minority tracts, allowing me to examine differential treatment effects by race (control groups are excluded). In the 'before' period, the change in racial pollution gap does not appear statistically different as evidenced by clustering around zero and overlapping Confidence Interval (C.I.) lines.[13] Parallel trends are again confirmed, this time for the DDD heterogeneity analysis. Following year zero of event time, it becomes clear that high-minority tracts experienced a greater decrease in PM2.5 than non-high-minority tracts. The policy effect appears to grow stronger over time, with the gap of pollution change expanding rapidly around the 10th year of event time. Again, it appears a composition effect is present, as a majority of the gap closure is likely attributable to PM10 regulations when PM2.5 non-attainment disappears from the data.

13 In each regression, all errors are clustered at the county level which allows for tracts located in a single county have correlated errors.

Figure 6



Under all specifications and coefficients, the results indicate that the CAAA were successful both in their stated goal of cutting air pollution and the EJ objective of narrowing the racial pollution gap. As the models increase in complexity, the coefficients shrink, but maintain significance, indicating the fixed effects and stacked treatment groups are capturing unobserved variance while leaving a meaningful signal. This result allows me to reject the null hypothesis that the CAAA did not reduce the racial pollution gap and provides casual evidence that the CAAA were responsible for the change.

7.3 Discussion and Implications

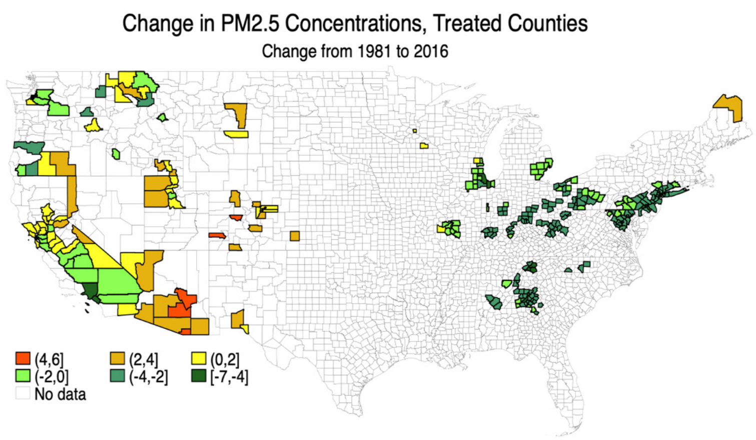
These results and the validity of the model are conditional on a few factors. First, endogeneity bias is a risk in any observational study, so the causality of my results is conditional on non-attainment status being exogenous. While the pre-trends of the event studies support this, it is impossible to prove. Furthermore, as previously discussed in Section 7.1, this analysis faces heterogeneity in treatment effects due to some counties being treated much later than the initial treatment round. As such, it is possible that the early rounds of SIPs and non-attainment designations induced second- or third-hand behavior impacts or policy changes. The event study results indicate that non-attainment status resulted in near-immediate pollution cuts, but it is still possible that an omitted variable responsible for some of the signal, or non-attainment is associated with confounders.

The findings of Currie et al. and the confirmation and expanded validity of this study provide macro-level good news for advocates of environmental quality and EJ. The concerns of pollution permit trading markets resulting in higher inequality should be further allayed. In taking a federalist approach to air, it appears that SIPs were broadly effective in cutting pollution and smoothing out disparities. Despite being repeatedly plagued by legal battles and political uncertainty, the Clean Air Act is a success story.

8 Heterogeneity Analysis

Decomposing these regression results allows me to examine the heterogeneous impacts of the CAAA. I re-run my DiD for each county placed under non-attainment status, comparing average PM2.5 concentrations over time to all non-attainment counties to obtain county-specific treatment effects. Figure 7 maps the results. Results vary from -7 μg/m^3 in the South Coast Air Basin[14] and the East Coast to an increase of 6 μg/m^3 in other areas of the Southwest. Areas that saw increases in pollution are a minority of the county treatment effects but demonstrate that SIPs do not universally decrease pollution.

Figure 7



14 Comprised of Los Angeles and Orange County, California

Table 4 presents correlations between my county-level treatment effects and county demographics (Table 5 in the appendix shows all correlations). Higher PM2.5 reductions correlate with a higher Black population and negatively correlate with the Hispanic population. The latter is a surprising finding given the regression results but makes sense if we compare Figure 4(b) to Figure 7. The treated counties with the greatest increases in pollution overlap some areas with the greatest concentrations of Hispanic population, such as Arizona[15], New Mexico, and Imperial County, California. The negative coefficients on income per capita and percentage population with a high school degree indicate that, consistent with my discussion in Section 4, wealthier and better-educated counties saw more pollution reductions on average.

Table 4

variable	correlation with beta
% White	-0.0126
% Black	-0.391
% Hispanic	0.3296
% < 18y/o	0.2455
% > 60 y/o	-0.0149
% P.O.C.	-0.0404
income	-0.097
% High School Degree	-0.2659
% manufacturing	-0.313
% poverty	0.0967

Another interesting result is the positive correlation between high PM reductions and county share of manufacturing employment. Using the same variable, Colmer et al. (2020) found that lower manufacturing shares led to higher tract percentile rank points of PM2.5 from 1981 to 2016. These two results suggest that PM2.5 SIPs effectively regulate direct source pollution and potentially less effective in reducing non-point source pollution. Direct sources such as power plants, smokestacks, and construction sites are less mobile and easier to regulate. Non-point sources include wildfires and automobiles, whose emissions interact with NO_2, SO_2, and VOCs[16] in the atmosphere to form PM2.5. Using Arizona as a case study, the state had multiple counties in non-attainment

15 Cochise, Pinal, Gila, and Santa Cruz Counties in Arizona saw 4 out of the 6 greatest PM2.5 increases in my treated counties dataset.
16 See section 2.2

for years and saw PM2.5 increases over time. Primary sources of PM2.5 in Arizona include automobiles and cross-border pollution spillovers from Mexico (ADEQ 2022). Located in a valley, Phoenix, AZ has the 7th worst air in the country, with PM2.5 building up from auto traffic, wood fire pits, and fireworks (Stone 2020). So, with manufacturing not a major source of PM2.5 in Arizona, it seems that the state's SIPs have been ineffective in tackling air pollution problems, which manifests as an EJ disparity given the state's significant Hispanic population.

Conversely, there are significant Black populations in the major metropolitan areas of formerly industrial states like Pennsylvania, California, Ohio, Tennessee, and Georgia. These are the states with counties that saw the greatest pollution drops. Thus, the SIPs targeting manufacturing likely had a strong clean-up effect in these areas, which drove the closure of the EJ gap. However, it is important to remember that the CAAA is only statistically attributable to a portion of pollution reductions (as evidenced in this paper and Currie et al. 2020). Deindustrialization was well underway during the CAAA and was accelerated in the 1990s by the signing of NAFTA. Furthermore, as explained in Section 4.3, the Porter Hypothesis leads me to believe that high-POC areas experienced disproportionate gains in clean industrial technology, reducing pollution. Clearly, there were multiple mechanisms at play that make it difficult to pinpoint a counterfactual. These findings are not causal but demonstrate that policy had heterogeneous impacts across different communities.

9 Conclusion

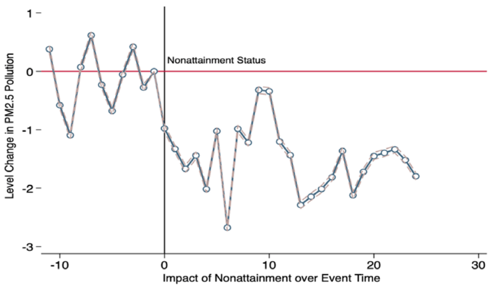
My research robustly finds a decrease in the racial pollution gap and causal evidence that the Clean Air Act was responsible. In summary, I contribute to the literature on several fronts: I add to the burgeoning utilization of satellite PM data in the Meng, Colmer, and Currie papers. I attempt to replicate and extend the results of Currie et al. (2020) who found CAAA's causal reduction in the Black-White gap of PM2.5. I extend the analysis to include PM10 attainment's impact on PM2.5, which brings in 11 years of extra observations. Event study results suggest that a composition effect is present in the results, with PM10 significantly contributing to the EJ gap closure in the long run. I also extend the analysis to Hispanic communities, confirming similar statistical trends and causal identification. I do so by situating this research within the ongoing empirical debate about observational design using Difference-in-Difference. To prevent bias from heterogeneous treatment effects, I confirm my results using a diverse range of specifications: traditional Difference-in-Difference, Triple Difference, Event Study, Two Way Fixed Effects, and Stacked Regression. My research robustly finds a decrease in the racial pollution gap and causal evidence that the Clean Air Act Amendments were responsible. Lastly, I conduct a heterogeneity analysis, decomposing earlier results and showing that air pollution decreases in heavy manufacturing counties were a primary driver of the EJ gap closure over time, while stagnating or worsening air quality in predominately Hispanic communities remains.

Disparate contributions of Black versus Hispanic populations to the EJ gap and its closures is worth future consideration. The results of Currie et al. and this paper suggest that gains in Black communities are driving the closure, while Hispanic communities have seen inconsistent outcomes. Future research should examine this question through decomposition and investigate the roles of residential sorting and PM pollution source types (point versus mobile) on these outcomes. It would also be valuable to extend the heterogeneity to more groups contained within regulators' definition of EJ groups, such as Asian and Indigenous populations. Additional next steps in the research should address the dearth of literature examining SIPs. While specific county/regional policies have been analyzed, I have not found a paper that comprehensively summarizes different regulatory structures of SIPs. Such research would allow comparative examination of effective plans to cut pollution, reduce EJ disparities, or improve public health.

Appendix

Figure 8

(a) Event Study for Tracts treated in 1992



(b) Event Study for Tracts treated in 2005

(b) Event Study for Tracts treated in 2005

Here I decompose the event study regression into tracts that entered non-attainment in 1992 and 2005, the first years of the PM10 and PM2.5 policies. In the former, I observe random distributions prior to the policy, then a noisy but discernable signal of the treatment effect. The original set of SIPs appears to have an immediate, strong treatment effect that is consistent over time. This is supported by Figure 5, the primary event study of all treated tracts, where the signal increases in strength after 10 years of event time (the only tracts remaining after 11 years of event time are those from PM10 regulations). This is likely due to a composition effect of PM2.5 and 10 policies which disappears once PM2.5 data runs out. In the 2005 round, the pre-trends appear to show that the PM10 regulations were impacting the areas soon to enter PM2.5 non-attainment. There is still a significant post-policy treatment effect that is smaller than PM10 initially but grows over time.

Table 5

variable correlations	beta	% White	% Black	% Hispanic	% < 18 y/o	% > 60 y/o	% P.O.C.	income	% GED	% manu-facturing	% poverty
beta	**1.000**										
% White	**-0.013**	**1.000**									
% Black	**-0.391**	**-0.626**	**1.000**								
% Hispanic	**0.330**	**-0.718**	**-0.044**	**1.000**							
% < 18 y/o	**0.246**	**-0.085**	**-0.124**	**0.270**	**1.000**						
% > 60 y/o	**-0.015**	**0.177**	**-0.091**	**-0.145**	**-0.506**	**1.000**					
% P.O.C.	**-0.040**	**-0.989**	**0.685**	**0.697**	**0.108**	**-0.171**	**1.000**				
income	**-0.097**	**-0.067**	**0.064**	**-0.070**	**-0.363**	**-0.272**	**-0.005**	**1.000**			
% GED	**-0.266**	**0.189**	**0.026**	**-0.168**	**-0.122**	**0.597**	**-0.104**	**-0.656**	**1.000**		
% manufacturing	**-0.313**	**0.381**	**-0.106**	**-0.330**	**0.082**	**0.087**	**-0.316**	**-0.227**	**0.413**	**1.000**	
% poverty	**0.097**	**-0.391**	**0.241**	**0.336**	**0.008**	**0.377**	**0.418**	**-0.612**	**0.450**	**-0.195**	**1.000**

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