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DISCUSSION PAPER SERIES

IZA DP No. 14646

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ABSTRACT

De Facto Immigration Enforcement, ICE Raid Awareness, and Worker Engagement*

We explore whether fear of apprehension affects immigrants' labor market engagement by examining how ICE removals due to immigration violations and increased awareness of immigration raids impact their labor market outcomes. We find that ICE deportations are associated with reductions in the labor force participation and employment of likely undocumented immigrants when compared to similarly skilled foreign-born U.S.citizens. Effects are particularly strong among women, especially those with children, as well as in industries likely targeted by ICE raids. Controlling for perceived threats and *de jure* immigration policies has little impact on these results.

JEL Classification: J15, J61, J2, J3

Keywords: undocumented immigrants, immigration raids, labor supply

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1. Introduction

The immigration debate continues to garner significant attention in public media and policy circles in the United States. While specific legislative proposals appear to come in and out of the public consciousness, executive enforcement has gained prominence as an elective measure of *de facto* immigration policy by the executive branch.[1] Anecdotal evidence suggests that media reports of immigration raids bring about apprehension fear, affecting the ease with which immigrants move about their daily lives, possibly having a chilling effect on their willingness to engage in economic activity (Carman and Selk 2017, Uhler 2017).

In this context, we investigate whether deportations due to immigration violations, along with increased awareness of immigration raids, have an impact on likely undocumented migrants' labor market outcomes. To that end, we link data on labor force outcomes from the Current Population Survey (CPS), Immigration and Customs Enforcement (ICE) data on deportations, and Google search data on immigration enforcement related terms, among other data sources. The combination of these datasets provides an innovative picture of the impact of actual or *de facto* immigration enforcement, as well as that of awareness of immigration raids—a proxy for the perceived threat of deportation—on a number of likely undocumented migrants' labor market outcomes. To date, most studies have focused, instead, on understanding the labor market impacts of the precursors of such *de facto* measures –namely, *de jure* measures, such as Secure Communities, employment verification mandates or omnibus immigration laws (*e.g.* East *et al.* 2019, East and Velasquez 2019, Amuedo-Dorantes and Lozano 2015, Amuedo-Dorantes and Bansak 2012). While of great relevance, questions remain regarding migrants' awareness of *de*

[1] For instance, the Deferred Action for Childhood Arrivals (DACA) program, which began as an executive order under President Obama and was rescinded by President Trump, has had significant effects on the labor force and schooling outcomes of undocumented immigrants (Amuedo-Dorantes and Antman 2016, 2017).

jure measures, not to mention differences in the strictness with which such measures might be implemented in various localities at various points in time based on its population composition, police department, or political affiliations of local officials –to name a few factors. The analysis herein complements studies evaluating the impact of *de jure* policies, broadening our understanding of the implications of various types of policy actions.

A priori, it is unclear whether undocumented immigrants should increase or decrease their labor supply in response to ICE deportations or the threat of deportations. On one hand, undocumented immigrants fearing deportation may choose to work less to evade apprehension risks associated with leaving the home, particularly if they are secondary household earners and primary caregivers for young children, as is the case for many migrant mothers. On the other hand, immigrants with target saving goals, who migrate primarily to work, remit, or save a sum of money, may respond to increased immigration enforcement by working more, perhaps in expectation they could be deported soon. As a result, the impact of intensified enforcement is theoretically ambiguous and remains an empirical question that we explore here.

Specifically, we compare a group of likely undocumented immigrants to a similar group of immigrants with U.S. citizenship to explore the labor market impacts of removals and the fear accompanying the latter, net of the impact of specific immigration regulations and the overall pro- or anti-immigrant climate they might create. This is important as the final product of intensified immigration enforcement might matter more to migrants than any legislated measure, since the latter might be implemented more or less rigorously in different locations and time periods. Subsequently, we explore whether the impacts being examined are especially pronounced in industries where undocumented immigrants are more likely to work –industries that might face a higher threat of ICE raids.

An important caveat in gauging the impact of any type of immigration enforcement is the fact that both *de jure* measures (*e.g.* signing of a 287(g) agreement or the adoption of the Secure Communities program) and *de facto* measures (*e.g.* raids or increased deportations by ICE) are admittedly non-random, with the former typically preceding and laying the path for the latter. As with studies evaluating *de jure* policy changes, we address such threats to causal identification by adding a wide range of controls, including metro area and month-year fixed effects, as well as metro-specific monthly time trends, while also focusing on a sample of reasonably comparable individuals. In supporting analysis, we also examine the period before and after particularly high "shocks" to immigration enforcement and awareness, the results of which point to our findings not being driven by pre-existing trends. While we remain cautious about interpreting our findings as causal, even if they solely reflect correlations, knowing whether the implementation of tougher *de facto* policies is accompanied by specific labor market patterns among the migrant population being targeted by such measures is of great interest.

Overall, we find that ICE deportations can be linked to a decline in labor force participation and employment among likely undocumented immigrants when compared to similarly skilled foreign-born U.S. citizens. These results are particularly pronounced among women, as well as in industries with relatively high shares of undocumented labor, with even stronger impacts among women with children in those industries. At the same time, there is little evidence to support an impact of perceived threats, as measured by Google searches on immigration raids, over and above actual deportations. Similarly, controlling for *de jure* immigration policies has little impact on the results, even if the measures do exhibit an impact. These findings suggest *de facto* immigration policy, as measured by actual deportations, have real consequences on the labor market activity of undocumented immigrants in the economy beyond those of *de jure* measures.

The remainder of the paper proceeds as follows. Section II describes the various data sets brought together to examine the impacts of immigration enforcement and enforcement awareness on labor market outcomes, to then comment on some descriptive statistics. Section III presents the empirical strategy used in the analysis. Section IV discusses the main results, and Section V reviews extensions and robustness checks. Lastly, Section VI summarizes and concludes the study.

2. Data and Descriptive Statistics

Our aim is to gain a better understanding of how intensified enforcement captured by increased removals due to immigration violations, as well as increased awareness about work raids, is impacting immigrants' labor market outcomes and work engagement. To that end, we combine several data sets.

A) Individual Labor Force Outcomes

i. Identifying the Undocumented Population in the United States

A major challenge when examining undocumented immigrants is getting information on this population. Most data sets do not record information on immigration legal status and, as we recognize below, some may fear responding to government surveys. Because of our interest in examining labor market responses to immigration policies at a monthly level, we make use of the Current Population Survey –the data source for the official unemployment statistics in the United States. The CPS presents some clear advantages, as well as disadvantages. A main disadvantage is that undocumented individuals might fear being identified and, in turn, their presence in the survey might be lower than in the country.[2] According to the Census Bureau, Census and CPS data undercount the undocumented. Based on Camarota, Richwine and Zeigler (2020), the undercount is approximately 7.5 percent based on a comparison of the Center for Migration Studies (2018) illegal immigration population estimates of roughly 10.6 million and the authors' total using the CPS of 9.8 million. This undercount had also been noted by prior literature, including Passel (2005), as well as Hoefer, Rytina and Campbell (2006).

Despite the above limitation, which is likely to be present in any official dataset, the CPS offers some important advantages. First, it provides information on monthly, repeated-cross sections on a national sample of individuals that span the pre- and post-period surrounding the policies object of study. This is crucial, as many other datasets only provide yearly information, interfering with the more precise merging of individual employment data with enforcement data and awareness data varying at the monthly level. Secondly, because of its frequency and scope, the CPS provides information on large samples, which is critical given our focus on a narrowly defined population subgroup. Third, the CPS is designed to gather information on the labor force and, as such, it is ideal for examining labor market outcomes. Finally, despite its undercount, it includes information on undocumented immigrants and allows for the identification of a strong proxy –namely, low-skilled non-citizen Hispanics.

Because of the advantages noted above, many researchers have used the CPS to study the behavior of the so-called likely unauthorized or undocumented. For instance, early studies, such as Heer (1979), used the CPS to estimate the new flow of undocumented immigrants. More recently, Passel (2005) and Camarota, Richwine and Zeigler (2020) use it to derive estimates of

[2] Related concerns regarding the ability to identify undocumented individuals in U.S. surveys range from misreporting of citizenship status in the American Community Survey (Brown *et al.,* 2019) to non-response to the citizenship question in the CPS (Bernhardt and Wunnava, 2020). Non-response rates in our sample are negligible.

the size and characteristics of the undocumented population and to examine their employment situation. Other authors have used the CPS to examine the impact of other immigration related policies, such as in-state tuition for undocumented immigrants (*e.g.* Kaushal, 2008; Amuedo-Dorantes and Sparber, 2014; Potochnick, 2014). And, perhaps most relevant to our study, recent papers have relied on the CPS to study the labor supply and earnings of undocumented immigrants (*e.g.* Borjas, 2017; Borjas and Cassidy, 2019).

ii. Data from the Current Population Survey (CPS)

First, we gather data on the labor market outcomes of working-age individuals from the monthly Current Population Surveys (CPS) covering the January 2004 through October 2017 period. The CPS provides detailed information on educational attainment, race/ethnicity, and other basic demographics, such as the decade of arrival, for those born outside the United States. Of particular relevance to us is the fact that it gathers representative level data on the labor market engagement of individuals residing in the United States on a monthly basis, allowing us to merge *de facto* measures of immigration enforcement and awareness of these measures at a higher frequency, while accounting for month-year fixed effects and monthly location-specific time trends to address endogeneity concerns.[3] Given our main aim, we consider the following outcomes: whether the individual is in the labor force, whether s/he is currently working and, in the latter case, the log of weekly hours worked and real hourly wages.

Since the CPS does not ask specific questions regarding work authorization, we use demographic characteristics common among the unauthorized population to produce a sample that is more likely to include them. Expert studies of the undocumented population suggest a majority

[3] We also experimented with restricting the sample to the 12 largest MSAs. Results are qualitatively similar, but the drop in sample size is considerable and comes at the cost of lost precision in the estimates.

are of Hispanic origin (Passel and Cohn 2018, 2009), and almost half of the working-age population has less than a high school degree (Passel and Cohn 2009). Thus, we begin by limiting the sample to working age (18-65), Hispanic foreign-born individuals with less than a high school degree. Up until very recently, a majority of the population of unauthorized immigrants was of Mexican-origin (Passel and Cohn 2019, 2009). Thus, in extended analyses, we further limit the sample to Mexican non-citizens as a demographic with a higher propensity to be unauthorized over the time span under consideration. Finally, up until 2010, Passel and Cohn (2019) also report that a majority of immigrants had been in the United States for less than 10 years. Since the median observation in our data set runs through 2008, we also limit the sample to individuals that have been in the United States for less than 10 years in the expectation that this population is more likely to be unauthorized. Furthermore, we expect more recent arrivals to have developed fewer networks and, therefore, possibly be more limited in their ability to navigate the complex U.S. legal system, making them more vulnerable to immigration enforcement when compared to their counterparts who have been settled for longer.

While the CPS does not indicate immigrants' undocumented status, it does include a citizenship question that allows us to distinguish our sample of working-age Hispanic foreign-born individuals with fewer than 10 years in the United States based on their citizenship status and, in turn, compare likely undocumented individuals to their documented counterparts.[4] By identifying the impacts on the likely undocumented population relative to the foreign-born population with similar characteristics, we expect to purge the deportations and awareness estimates of anti-immigrant sentiment or climate affecting both groups. In further analysis, we also investigate

[4] In addition, we also experiment with using a proxy for the likely undocumented along the lines of the one used in Borjas (2017), which also examines the labor market outcomes of undocumented immigrants using the CPS. Results prove robust to the use of this alternative proxy (Appendix 1, Table C).

whether there are differential impacts on workers in industries facing a greater threat of deportation, as would be the case with industries with a heavier concentration of undocumented immigrants –namely, agriculture, construction, food processing, restaurants, travel and drinks, services to buildings, landscaping, and apparel manufacturing. Both expert studies of undocumented workers (Passel and Cohn 2018) and descriptive statistics on the concentration of workers by industry in our sample suggest those industries are obvious candidates for ICE raids and, therefore, for exploring heterogeneous effects.

B) Data on Immigration and Customs Enforcement Removals

We merge the data on labor force outcomes with data on ICE deportations due to immigration violations at the month-year level for each metro area.[5] The latter data set is collected by the Transactional Records Access Clearinghouse (TRAC) at Syracuse University through Freedom of Information Act requests and other legal actions.[6] These data do not include deportations made by Customs and Border Protection, unless prolonged detention meant that custody of the individual was transferred to ICE.[7] However, TRAC collects information on deportations resulting from a variety of immigration enforcement programs, not solely those related to Secure Communities or 287(g) agreements. We focus on removals where the most serious criminal conviction (MSCC) was an immigration violation, as these are likely to be suggestive of the least tolerance for unauthorized immigrants. This type of removal includes

[5] In practice, we first merge the TRAC data with the Google Trends data to be described below. While the TRAC data includes the city and state of removal, our data from Google Trends identify the metro area, and we match these by hand based on whether the TRAC city name is included in the Google Trends metro area identifier.

[6] Since 1997, immigrants may be subject to removal based on deportability, and ICE manages these functions. See deportation and removals at https://www.uscis.gov/tools/glossary. Throughout, we use the terms removals and deportations interchangeably.

[7] These data are available at http://trac.syr.edu/phptools/immigration/remove/

See http://trac.syr.edu/phptools/immigration/remove/about_data.html for further details.

instances in which the most serious offense was illegal entry, illegal re-entry, and possession/trafficking of fraudulent immigration documents. We hypothesize that the impact of deportations on workers' labor market outcomes will be extreme for these types of removals, as these could be channeling anti-immigrant sentiments and instill greater fears in the population under study.[8]

In addition to the MSCC, TRAC data contains information on the location (city and state) from which the individual departed, as well as the date (month and year).[9] We aggregate the number of individuals deported from each location. Next, we compute a moving average of monthly deportations from each location using the current and prior months' deportations, thus allowing for the possibility of a delay between arrest and deportation. Finally, we merge the deportation data to CPS data on individual labor market outcomes, as well as to data on awareness of immigration enforcement, which we describe next, in each location at the date in question.

C) Data on Awareness of Immigration Enforcement

Because immigrants may respond to more than actual ICE removals, we also make use of data from Google Trends (GT) capturing the intensity of Google searches on immigration raid-related terms as a proxy for perceived raid threats and immigration enforcement awareness.[10] We focus on searches that are more likely to capture work-related immigration concerns and,

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[8] This indication is corroborated by media reports suggesting that more recent deportations have targeted immigrants who have committed relatively minor offenses (Sacchetti and O'Keefe 2017).

[9] Ideally, we would have information on the location where migrants were apprehended, as migrants apprehended in more remote areas might be deported from the nearest larger metro. However, to the extent that we focus on large metro areas concentrating the largest shares of immigrants (these are listed in Appendix 1, Table A), apprehension and deportation locations are more likely to coincide. Unfortunately, while we know the city where the person was apprehended, we do not know if the apprehension occurred at home, the workplace or on the street, for example.

[10] These data were hand-collected at the metro area level available in the Google Trends database. This level of geographic variation is consistent with our focus on large metro areas, which are also more likely to correspond with media markets. While, in principle, these data could be collected at the city level, in practice, extracting GT data on lower-frequency search terms at a finer level of geographic variation generates many missing values. See https://trends.google.com/trends/ for more details.

consequently, be more closely linked to our labor market outcomes. These include the following search terms: ICE raid, ICE raids, immigration raid and immigration raids.[11] The GT data are merged with the two data sets above at the month-year level for each metro area.[12]

It is important to note that GT data are limited in several ways. First, Google does not release the actual number of searches but, instead, an index that allows researchers to compare the proportion of searches at a particular point in time or geographical location to other points in the sample, with the maximum set to 100 and the range lying between 0 and 100 (Stephens-Davidowitz and Varian 2014). We collected a monthly time series for each metro area so that the GT scores used herein can be interpreted relative to the maximum for a specific location, and thus interpretable within the fixed effects framework we adopt in our analysis. In this context, the GT index measures the fraction of searches that included the relevant terms relative to all searches at that point in time in that particular area, as a proportion of the maximum share of searches in that area. To be precise, we adapt the expressions used in Burchardi *et al.* (2018) and Alsan and Yang (2019). In the analysis below, the GT index for relevant search term *i* in geographical area *m* in period *t* can be represented by the following expression:

$$(1) \qquad G(i,t;m) = [100 * \frac{share(i,t;m)}{max_t share(i,t;m)} \mathbb{1}(\#(i,t;m) > T)]$$

where: $share(i,t;m) = \frac{\#(i,t;m)}{\#(t;m)}$, $\#(i,t;m)$ indicates the number of searches for term *i* in area *m* in month *t*, $\#(t;m)$ indicates the number of all searches in area *m* in month *t*, and

[11] In principle, additional search terms (*e.g.* Spanish translations) could be added to the algorithm. In practice, it proved computationally costly to add search terms, and we did not expect the inclusion of additional terms to identify distinct sources of variation from the ones already captured by the data.

[12] It is standard in the literature to look at the metro area for many questions pertaining to labor market outcomes (*e.g.* Cortes and Tessada, 2011) or the response of undocumented immigrants to intensified immigration enforcement (*e.g.* Amuedo-Dorantes and Arenas-Arroyo, 2019), as we do herein.

$max_t share(i, t; m)$ is the maximum share of searches for term i in area m, taken over all of the months in the location-level sample.

Another challenge of using GT scores is that search scores are only available if they surpass a Google-determined threshold that is not observable to researchers; otherwise, a 0 is reported.[13] Thus, $\mathbb{1}(\#(i, t; m) > T)$ is an indicator function capturing the fact that only observations of $G(i, t; m)$ for which the number of searches for relevant term i in area m in month t exceeds the Google-determined threshold T will be positive. However, only $G(i, t; m)$ is observable to the researcher. Thus, for each area m, the Google Trends index will equal 100 in the month in which the share of searches for term i is the highest, and a smaller, positive number in all months in which the share of searches for term i is smaller, but still above the Google threshold. This number is directly relatable to the proportion of searches in the maximum time period following the expression above. A GT score of 0 should be interpreted as an especially low number of searches relative to the maximum for a particular location, and locations where all values are below this threshold do not offer useful sources of variation.[14] Therefore, we limit the analysis sample using the GT data to areas for which the immigration-related searches are observable. We subsequently match these locations to the cities in the TRAC data set and metropolitan areas from the CPS. The resulting areas covered in the merged data set are listed in Appendix 1, Table A. Because these locations also contain some of the cities with the largest shares of immigrants, the restriction should not significantly impact the generalizability of our findings.

[13] Stephens-Davidowitz and Varian (2014) report that this threshold is tied to the absolute number of searches, so we should expect this limitation to bind in smaller cities, that are also likely to have smaller populations of immigrants.

[14] Note that other studies using data on Google searches do not suffer from missing values or lower-bound limitations because the search terms used are relatively popular throughout the United States (Baker and Fradkin 2017). Still other studies have pioneered methods to overcome missing values in Google search terms (Stephens-Davidowitz 2014), however, these methods require assuming a consistent relationship between search terms across the U.S. and thus we do not make use of them here.

A final challenge in using the GT data is that the index is based on a sample of the total Google search data and, therefore, the GT index may differ depending on the sample (Stephens-Davidowitz and Varian 2014). We follow Stephens-Davidowitz (2014) in drawing multiple samples for each location's time series, so that the GT score for each area-month can be averaged over multiple draws.[15] As a result, the GT score provides a measure of awareness of immigration raids at the location-specific level; thereby, allowing us to link changes in search intensity in that location to changes in labor market outcomes using the empirical strategy we describe below.

D) Some Descriptive Statistics

We link the CPS, GT, and TRAC data sets based on the names of metropolitan areas provided in the first two data sets and the city name available in the latter data set.[16] The result comprises our analysis sample, which includes the 33 GT metro areas listed in Appendix 1, Table A. Since these large metro areas have the largest shares of immigrants, this sample limitation is not likely to meaningfully impact our estimates. At the same time, by focusing on larger metro areas, we are more likely to minimize instances in which the apprehension and deportation locations differ –something more likely to occur when apprehensions take place in more remote areas.

Descriptive statistics on our sample of recent Hispanic immigrants with less than a high school degree are listed in Table 1, where the variable *eligible* refers to our treated group of likely undocumented immigrants –namely, non-citizens, *i.e.* eligible for deportation under immigration

[15] We attempted to draw 100 samples of the time series for each location, but as Google limits researchers to one sample drawn per day and sometimes gives missing observations for the search terms we used here, the number of samples drawn per area fell below that in some cases. We limit our analysis to areas with at least 75 non-missing samples of the GT score time series, and generate an average GT score for each metro-month in the sample.

[16] While this matching approach does not yield a precise MSA-level data set, we expect there to be extensive overlap in the areas identified in all three data sets, given our focus on large metro areas (Appendix 1, Table A).

enforcement policies. They comprise a large portion of observations (94 percent), which is unsurprising given the sample limitations. About 62 percent of the sample is employed, and 73 percent is in the labor force. Approximately 56 percent are men, averaging 33 years of age, and the number of removals due to immigration violations averages 15 per month.

Figure 1 depicts the correlation between data on yearly raids made available from ICE through an author-initiated Freedom of Information Act (FOIA) request, and our measures of immigration enforcement -namely, our proxy for raid awareness via the GT index and the monthly immigration-related removals data from TRAC. Unfortunately, data from the FOIA request are at the Department of Homeland Security fiscal year level running from September through October and, as such, not useful for our analysis below. Nevertheless, they can still inform about the extent to which immigration related removals and our raid awareness measure are related to actual ICE raids. As seen in Figure 1, there is a positive correlation between the two sets of immigration enforcement measures and actual ICE raids, providing credibility to our measures as reflective of actual and perceived intensified enforcement.

To give a sense of the geographic variation in the data, Appendix 1, Figure A further displays the variation over time in our two measures for two metro areas –namely, Atlanta and Los Angeles. Figure A underscores two important facts about the measures being used. First, comparing across the two panels, we observe considerable differences between the two types of immigration enforcement measures within a given metro, as can be seen by comparing removals to raid awareness in, say, Atlanta. This distinction supports the notion that raid awareness might be capturing something different from the actual immigration-related removals data.

A second fact evident from the figures is the distinct patterns that each of the immigration enforcement measures exhibits across metros. For instance, the left panel in Figure A emphasizes how removals proved significantly higher during most of the period in Atlanta than in Los Angeles. Despite that variation, the timing of Google Trends searches that is common across areas is suggestive of broader national attention to ICE raids which may generate greater awareness and fear in the immigrant community. The extent to which these impact labor force outcomes at the local level remains an empirical question and the empirical approach, to be presented next, will address levels of variation across all these dimensions.

3. Empirical Strategy

A) Empirical Specification

To investigate how immigration removals and awareness about immigration raids impact undocumented immigrants' labor force outcomes, we focus on a sample of 18 to 65-year-old low-skilled (with less than a High School education) foreign-born recent Hispanic immigrants. Some of them have naturalized and become U.S. citizens –namely, our control group; whereas others who remain non-citizens comprise our treatment group –what we refer to as likely undocumented immigrants, as discussed in section 2A. While the possibility of complementarities and substitutabilities among citizens and non-citizens preclude us from having a clean control group, we would expect to observe differential impacts across the two groups. Our benchmark model is given by:

$$(2) \quad Y_{imt} = \alpha + \beta_1(Removals_{mt}) + \beta_2(Awareness_{mt}) + \beta_3(Eligible_{imt}) + \\ + \beta_4(Eligible_{imt} * Removals_{mt}) + \beta_5(Eligible_{imt} * Awareness_{mt}) + \\ + X_{imt}\gamma + Z_{mt}\lambda + \mu_m + \delta_t + \mu_m t + \varepsilon_{imt}$$

where Y_{imt} represents the labor market outcome in question for individual *i* in metro area *m* in period *t*. Outcomes considered include whether the individual is in the labor force, currently employed and, in the latter case, the log of weekly hours worked and real hourly wages.

$Removals_{mt}$ is our measure of *de facto* immigration enforcement, capturing the moving average of the present and last months' removals due to immigration violations in thousands,[17] whereas $Awareness_{mt}$ is our measure of immigration raids' awareness based on the Google search index results in metro area *m* in period *t*. Finally, we include a dummy ($Eligible_{imt}$) that equals 1 when the respondent is a non-citizen and is set equal to zero if the respondent is a naturalized citizen. We interact this dummy with the information on removals and raids' awareness to gauge any differential impacts across the two subgroups.

Aside from demographic controls for the individual worker (X_{imt}), such as race, age, marital status, number of children and years in the United States, we also account for the metro area's unemployment rate and, in subsequent specifications, various immigration policies in the metro area over the period under consideration (Z_{mt}). The latter include indexes reflecting the number of police-based immigration enforcement initiatives adopted at the local or state levels (police-based immigration enforcement index), the adoption of employment verification mandates (employment-based immigration enforcement index), and dummies indicative of whether driver licenses are issued to undocumented immigrants. A detailed description of the control variables is available in Appendix 2.

Finally, the model incorporates metro area fixed effects (μ_m), month-year fixed effects (δ_t), and metro-specific linear month-year time trends ($\mu_m t$) to consider other unaccounted for time-varying policies and economic conditions at the metro level. The inclusion of metro area fixed effects allows us to interpret the awareness measure relative to its value within the metro

[17] We experiment with scaling the number of removals using information on the number of foreign-born individuals at the metro level. Results prove robust, which is not surprising given that the analysis already includes metro area fixed effects and metro-specific month-year trends. Differences in the population at risk for enforcement should be captured by those indicators and trends.

area, thus obviating the need for the actual number of searches. Standard errors are clustered at the metro level.

The parameters of interest to us are β_4 and β_5, which gauge the differential impact that tougher enforcement has had on the labor market outcomes of those most likely to be targeted by the measures, when compared to their citizen counterparts. In addition, we are interested in gauging the overall impact that *de facto* immigration enforcement and raid awareness have had on likely undocumented workers –an effect we derive by evaluating the terms: $(\beta_1 + \mu_{Eligible} * \beta_4)$ and $(\beta_2 + \mu_{Eligible} * \beta_5)$, respectively. Finally, we also investigate heterogeneous impacts by limiting the sample to demographic groups more susceptible to deportation fears, such as workers in industries where undocumented workers are more prevalent and ICE raids are more common.

B) Endogeneity Concerns

An important concern when gauging the impact of any policy refers to the potential endogenous nature of the policy itself. We acknowledge that *de facto* policy measures are not adopted randomly, just as *de jure* policies often face questions surrounding endogenous timing and roll-out. Yet, from an econometric standpoint, the causality concern refers to the possibility that the policy measures are endogenous to the labor force outcomes under study–namely, those of likely undocumented immigrants (as opposed to those of natives)—especially after accounting for a wide set of variables, metro area and month-year fixed effects, as well as metro-specific monthly time trends, as we do here.

One common means of bolstering the case for causality in studies evaluating a particular policy is to show pre-existing parallel trends of treatment and comparison groups prior to policy implementation. Our focus on *de facto* immigration policies, naturally measured by continuous measures of enforcement and by the awareness index, limits our ability to conduct an event-study style analysis in the main section of the paper since there is not a simple event we can use as reference. Nevertheless, we can offer some supportive evidence of the absence of pre-existing trends in labor force outcomes prior to significant shocks to immigration enforcement. To do this, we construct a shock indicator equal to one when both immigration removals and the GT score are above median levels within the metro area. We then use the indicator to conduct an event study and gauge the existence of differential pre-trends in labor market outcomes across localities with more vs. less enforcement, before vs. after its intensification. As shown by the four graphs included in Appendix 1, Figure B, we fail to find evidence of systematic differential pre-trends leading up to the shock, suggesting the latter is likely exogenous with regards to the labor market outcomes being examined. At the same time, irrespective of the extent to which we can interpret our estimates as causal, we still view our analysis as relevant and complementary to the existing literature on the impact of *de jure* measures. In additional extensions, we also consider the impact of these *de jure* policies and show that *de facto* policies have an impact over and above those of *de jure* policies.[18]

4. Removals, Raid Awareness and Work Engagement

A) Main Findings

Table 2 displays the results from estimating equation (2) for a sample of similar low-skilled, foreign-born Hispanics of working age who immigrated in the 10 years prior to the survey, before controlling for immigration related policies implemented in the metro area over the period

[18] An additional concern brought about by a recent literature (Abraham and Sun, forthcoming; Callaway and Sant'Anna, forthcoming; Goodman-Bacon, forthcoming) is the potential for biased average treatment effects in difference-in-difference models when there are multiple time periods, variation in treatment timing and the parallel trends assumption holds after controlling for other covariates. Our treatment is not binary and, as a result, the large metros included in this study were continuously treated over the time span under consideration. We acknowledge the possibility of potential biases in our estimates due to the varying intensity of treatment exposure, although it remains unclear how to address these concerns.

under consideration. According to the results displayed in the first three columns, ICE removals appear to have a statistically significant negative impact on the labor market outcomes of likely undocumented individuals relative to their impact on citizens with similar demographic traits. All else equal, an additional 10 removals –an amount close to the average number of removals in the sample– is associated with an 0.855 percentage point decrease in their employment likelihood, when compared to their citizen counterparts.[19] Similarly, relative to other foreign-born Hispanic citizens, a similar increase in removals lowers foreign-born Hispanic non-citizens' propensity of being in the labor force by an 0.596 percentage point and their real hourly wages by about 1.9 percent. In sum, removals appear to have lowered the employment likelihood, propensity to be in the workforce, and hourly wages of likely undocumented Hispanics when compared to similarly low-skilled naturalized Hispanics.[20] The overall decrease in employment and wages experienced by likely undocumented Hispanics when compared to similarly low-skilled naturalized Hispanics is suggestive of labor demand reductions potentially overriding the impact of declines in undocumented labor supply on wages. This is not entirely unsurprising if, for example, employers are trying to evade encounters with immigration officials and the associated negative publicity in the midst of increased removals and work raids. Overall, however, the total impact of an additional 10 immigration removals on the labor market outcomes of low-skilled foreign-born Hispanics remains small, reducing their employment likelihood by 0.349 percentage points and reducing their real hourly wages by about 0.2 percent, all else equal.[21]

[19] Computed as: $(\beta_4 * \Delta_{Removals})$.

[20] These results are qualitatively similar if we drop the unemployment rate control or exclude metro-area time trends. However, accounting for time-varying local labor market conditions is critical in purging the *de facto* immigration enforcement estimates of potentially confounding macroeconomic factors. Results are also very similar if we include a cubic metro-specific time trend (see Table D in the Appendix).

[21] The overall (total) effect of removals on low-skilled foreign-born Hispanics is computed as $[(\beta_1 + \beta_4) * \Delta_{Removals}]$.

The discussed impacts are suggestive of reductions in employers' demand for likely undocumented labor in response to increased monthly removals due to immigration related violations, when compared to their labor demand for citizens with similar demographic traits. The overall pattern is supportive of the notion that, amid greater deportation threats, employers substitute away from undocumented labor toward documented labor. At the same time, we find no significant impacts of raid awareness on the labor supply or compensation received by foreign-born Hispanics, regardless of their citizenship.[22] This finding points to perceived deportation threats, over and above actual removals, as failing to explain the variation in labor force outcomes.[23]

B) Robustness Checks

A potential shortcoming of the results displayed in Table 2 is that they do not consider different *de jure* measures in place, which could be potentially responsible for some of the impacts found. Table 3 addresses this question with the inclusion of two immigration policy indexes –one capturing the various police-based immigration enforcement initiatives in place in any given county, such as local or statewide 287(g) agreements signed between ICE and the respective law enforcement agencies, Secure Communities or omnibus immigration laws; as well as another one capturing the presence of an employment verification mandate in the state. In addition, we account for whether the state issues driver licenses to undocumented immigrants (Amuedo-Dorantes *et al.*

[22] Results prove robust to the use of an alternative proxy for likely undocumented status similar to the one recently used by Borjas (2017, 2019), and which excludes veterans, individuals born in Cuba, and those working in the government sector from the *eligible* population. As shown in Appendix 1, Table C, results prove robust to the use of this alternative proxy.

[23] It is possible that, due to the publicity of raids, raid awareness has a broad reach that expands beyond the local level, in which case it might not be tied to variations in local labor market outcomes of low-skilled foreign-born Hispanics in the same way as local removals. If everyone (regardless of location) were affected in the same way by local raids, then they would be closer to national-level events, and thus common to everyone at a specific point in time, and soaked up by the year-month fixed effects. Thus, we would not expect to see statistically significant coefficient estimates on our independent variables of interest if this were the case.

2018b) –a factor that could significantly impact their labor force participation and wages. An upshot of this analysis is that it allows us to explore the extent to which *de jure* immigration policies, as opposed to *de facto* immigration policy captured by actual immigration removals, can explain the variation in labor force outcomes of likely undocumented individuals.

As can be seen in Table 3, controlling for *de jure* measures do not substantially affect the estimates of interest reported in Table 2. We note that employment-based *de jure* immigration enforcement measures have a statistically significant negative impact on the likelihood of employment and labor force participation of the individuals in our sample, while driver licenses for undocumented individuals have a statistically significant positive impact on their hours worked. However, it is still the case that increased monthly removals primarily dampen the employment, labor force participation, and wages of foreign-born Hispanic non-citizens, when compared to their naturalized counterparts. As before, we do not find raid awareness to have a statistically significant impact on these migrants' labor market outcomes above and beyond the one already captured by monthly removals.

Next, we experiment with focusing on a sample of low-skilled, foreign-born Hispanics that have, over the time period under examination, dominated the undocumented counts –namely, low-skilled Mexicans (Passel and Cohn 2019). The purpose of this exercise is to look closer at a demographic group more likely to consist of undocumented workers. The estimates in Table 4 show the results from this exercise. As we would expect, the same increase in monthly removals leads to significantly larger impacts on the employment likelihood and labor force propensity of non-citizen Mexicans; this is especially true when compared to their naturalized counterparts. Specifically, all else equal, an additional 10 removals lowers the employment likelihood and labor force participation among low-skilled Mexican non-citizens by 1.3 and 1.1 percentage points, respectively, when compared to their naturalized counterparts. However, we no longer find evidence of a statistically significant decline in hourly wages. Additionally, the overall (total) impact of increased removals on the labor force outcomes of low-skilled foreign-born Mexicans remains small, reducing their employment likelihood by 0.42 percentage points with an additional 10 local removals. Finally, raid awareness continues to have no effect on these workers' labor supply above and beyond the one captured by removals.

C) Heterogeneous Impacts by Industry and Gender

Are the observed impacts any different if we narrow our focus to industries employing a higher share of undocumented immigrants? After all, these industries are potentially more likely to be the target of ICE raids.[24] Table 5 addresses this question by restricting the analysis to agriculture, construction, food processing, restaurants, travel and drinks, services to buildings, landscaping, and apparel manufacturing industries.[25] Panel A refers to all working-age, low-skilled, foreign-born recent Hispanic immigrants employed in those industries. Subsequently, we distinguish by gender. Panel B focuses on the women in key industries, omitting the construction industry from the prior list since women are much less likely to be employed in that industry (King 2011). Similarly, Panel C focuses on the men in key industries, omitting apparel manufacturing from the list, as our sample suggests that men are less likely to be employed there.

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[24] ICE raids, along with the share of employed men and women, may vary by industry. Because the choice of industry may be related to existing immigration enforcement measures, we conduct separate analyses to examine the impact of *de facto* immigration enforcement in industries employing high shares of male or female immigrants. Nevertheless, including industry fixed effects to the main specification yields qualitatively similar estimates.

[25] The focus on these industries is supported by the literature examining the industries employing high shares of undocumented workers (Passel and Cohn, 2018; Amuedo-Dorantes and Bansak, 2012; Amuedo-Dorantes and Lozano, 2015), including those studies focused on the occupational distribution of Mexican-born women (King, 2011), as well as the distribution of industries represented in our sample. We drop personal care occupations in private households from the list, as they are less likely to be the targets of ICE raids.

A few findings are worth discussing. First, when we focus on this subgroup of men and women (Panel A), an increase in 10 monthly removals, all else equal, lowers their employment propensity by 0.52 percentage points, when compared to their citizen counterparts. In addition, the same change in removals is associated with a decline in non-citizens' hourly wages of about 3.3 percent when compared to those earned by similar naturalized migrants.

A second result worth noting refers to the differential impact that intensified immigration enforcement, as captured by monthly removals and raid awareness, appears to have on the men and women working in these key industries. An increase in 10 monthly removals has a dramatic impact on the employment, wages, and usual weekly hours of work of low-skilled, Hispanic non-citizen women in key industries (Panel B), cutting down their employment likelihood by 2.2 percentage points and hourly wages by 6 percent when compared to their naturalized counterparts. We also note a modest increase in the usual weekly hours of work of women in this sample. Relative to their citizen counterparts, their weekly hours of work rise by 1.65 percent as monthly removals increase by ten.

At the same time, higher monthly removals do not appear to have significantly altered the labor supply or wages earned by low-skilled Hispanic immigrant men in the key industries examined (Panel C). Only their employment propensity appears to respond, although to increased raid awareness when compared to their naturalized counterparts. Specifically, increasing raids awareness by one unit of the GT score (close to the median in the sample) lowers the employment likelihood of low-skilled Hispanic non-citizen men in key industries by 1.7 percentage points when compared to their naturalized counterparts in those same industries, but is only marginally statistically significant at the 10% level.

The greater response exhibited by women is consistent with broader results from the literature showing that female labor supply is more elastic than male labor supply, especially among undocumented immigrants (Borjas, 2017). This might be, in part, due to their key role in childrearing. Perhaps, undocumented migrant women with children are particularly responsive to the intensification of immigration enforcement if they are more likely to be the primary caretakers of children. To investigate this hypothesis and purge estimates of differences across industries, we focus on the largest single industry employing women in our sample, namely the restaurant industry, at the same time that we limit our sample to individuals with children. As undocumented individuals are more likely to have children in the household (Passel and Cohn 2018), narrowing the sample in this way also brings our sample closer to the undocumented population. Due to the much smaller sample size used in this analysis, we should read these results with some caution. However, the estimates in Panel A of Table 6 support the hypothesis that child rearing plays a role in the differential response to increased deportation threats. An increase in removals significantly lowers the overall employment, labor force participation, and wages of low-skilled Hispanic immigrant women with children in the restaurant industry.[26] An increase of 10 removals reduces their employment likelihood by 4.8 percentage points and lowers their wages by roughly one-quarter.[27]

[26] We also experimented with conducting the analysis for women without children, but the sample becomes rather small, and the main effects disappear. This may be due to a loss of precision in the estimates because of the small sample size. Alternatively, it may be signaling that women without children are less likely to lower their labor supply amid increased removals, which would make sense if they were less risk averse.

[27] Most of the literature examining the impact of interior immigration enforcement focuses on individual *de jure* measures (such as employment verification mandates or Secure Communities) or, when examining labor supply patterns, it restricts the attention to men. Therefore, it is difficult to make comparisons. However, Amuedo-Dorantes and Bansak (2012) use CPS data spanning from 2004 to 2010 to examine the impact that employment verification (E-Verify) mandates had on the employment and earnings of likely undocumented men and women. While their focus in not on *de facto* enforcement measures, they find that universal E-Verify mandates reduced the employment propensity of likely undocumented women by 7-percentage points (or 10 percent) and raised wages.

Panel B in Table 6 repeats the same exercise for a comparable sample of low-skilled and recent immigrant Hispanic men employed in the restaurant industry. Unlike their female counterparts, these men increase their overall labor force participation as removals rise if they report having children living in the household. This evidence, coupled with the strong labor force participation of men to begin with, hints at men's main household breadwinner status and, in turn, their pressure to continue to work during tougher times, as would be the case in an environment with increased deportation threats.

5. Summary and Conclusions

While the nation continues to struggle with a legislative deadlock over immigration policy, deportations over purely immigration-related offenses and ICE raids constitute a form of *de facto* immigration policy by the executive branch. We find evidence that increased removals due to immigration violations have dampened the labor force participation and employment likelihood of low-skilled recent Hispanic non-citizens –our proxy for the likely undocumented population. The fact that these impacts are larger in magnitude among Mexican immigrants, who were more likely to be undocumented during the sample period being examined, as well as significantly lower for non-citizens when compared to their naturalized counterparts, supports the notion that deportations are affecting the labor force outcomes of the undocumented population. At the same time, we find no consistent evidence of perceived threats, as measured by google searches on ICE raids, in explaining likely undocumented migrants' labor force outcomes over and above the impacts of *de facto* immigration policy as measured by actual removals. Controlling for *de jure* immigration policies has little impact on these results. Finally, the analysis focusing on industries employing high shares of undocumented immigrants suggests that the negative impacts are primarily driven by the responses of women. In fact, an exploration of the impacts on individuals with children points to women's role in child-rearing as a possible mechanism behind our results.

Together, these findings suggest that *de facto* immigration policy, as measured by actual deportations, have real consequences on the labor market activity of undocumented immigrants in the economy. However, endogeneity concerns surrounding the adoption of immigration measures limit the ability to interpret our estimates as strictly causal. We address such concerns by restricting our treatment and comparison groups to similar individuals and controlling for a wide array of metro area and month-year fixed effects, as well as metro-specific monthly time trends. While our supporting analysis examining the period before and after particularly high "shocks" to immigration enforcement and awareness suggests that these results do not appear to be driven by pre-existing trends, caution should still be taken in interpreting the findings as causal. Still, even if the results were to solely capture correlations, they complement our knowledge regarding the impacts of *de jure* immigration policies.

This analysis is crucial, as much of the debate over immigration enforcement often revolves around the adoption of specific regulations (or *de jure* measures) with outcomes that vary widely from place to place –a fact not surprising given differences across police departments collaborating with ICE, as well as communities across the country. Furthermore, it is easier for the executive branch to request a tougher implementation of existing immigration regulations than to change the regulations themselves. Consequently, there may be few observable signs of a change to effective immigration policy other than the outcome of the legal measures already in place. Thus, as the results of this study suggest, assessments of the impacts of immigration policies should properly account for *de facto* immigration enforcement measures and future research should not neglect to consider variation in effective enforcement, as opposed to simply changes in *de jure* policies.

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Table 1: Sample Descriptive Statistics of Low-Skilled Hispanic Recent Immigrants

Variable Name	N	Mean	S.D.
Employed	9518	0.6213	0.4851
In the Labor Force	9518	0.7291	0.4444
Ln (Real Hourly Wages)	5301	2.8421	0.9505
Ln (Weekly Work Hours)	5301	3.630568	.2596795
GT Score	9518	2.2004	3.7212
Average Monthly Immigration Related Removals Per 1,000	9518	0.0151	0.0368
Average Yearly Raids per 1,000	6077	1.0892	0.9221
Eligible	9518	0.9445	0.2289
Male	9518	0.5584	0.4966
Black	9518	0.024	0.1529
Other Race	9518	0.0372	0.1892
Age	9518	32.607	10.4504
Married	9518	0.5416	0.4983
Number of Children	9518	1.0298	1.2406
High School Education	9518	0	0
More than High School Education	9518	0	0
Foreign-born	9518	1	0
Mexican	9518	0.6457	0.4783
Years in the U.S.	9518	4.7545	3.0058
Unemployment Rate	9518	5.086	1.7895
Police-based Immigration Enforcement (IE) Index	9518	0.2152	0.3911
Employment-based Immigration Enforcement (IE) Index	9518	0.1288	0.3349
State Grants Driver Licenses to Undocumented Immigrants	9518	0.0281	0.1651

Table 2: Main Findings for Low-Skilled Hispanic Immigrants

Regressors	(1) Employed	(2) In LF	(3) Ln(Real Hourly Wages)	(4) Ln(Weekly Work Hours)
Eligible*Removals	-0.855**	-0.596*	-1.932**	0.385
	(0.379)	(0.324)	(0.816)	(0.334)
Removals	0.506*	0.538*	1.764***	-0.497*
	(0.280)	(0.283)	(0.546)	(0.286)
Eligible*GT	0.002	0.001	0.006	-0.003
	(0.008)	(0.006)	(0.026)	(0.005)
GT Score	-0.002	-0.002	0.003	0.003
	(0.008)	(0.006)	(0.025)	(0.005)
Eligible	-0.070*	-0.055**	-0.122*	-0.006
	(0.035)	(0.025)	(0.063)	(0.015)
Male	0.389***	0.416***	0.256***	0.123***
	(0.025)	(0.024)	(0.038)	(0.009)
Black	-0.029	-0.014	-0.003	0.031
	(0.040)	(0.033)	(0.065)	(0.023)
Other Race	0.040	0.038*	-0.023	0.009
	(0.033)	(0.022)	(0.049)	(0.020)
Age	0.043***	0.047***	0.024**	0.013***
	(0.005)	(0.003)	(0.009)	(0.002)
Age Squared	-0.001***	-0.001***	-0.000**	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.046***	-0.074***	0.032	-0.004
	(0.013)	(0.010)	(0.028)	(0.008)
Number of Children	-0.027***	-0.031***	0.011	-0.006*
	(0.005)	(0.004)	(0.013)	(0.003)
Years in U.S.	0.003*	-0.001	0.006	0.001
	(0.002)	(0.002)	(0.005)	(0.001)
Unemployment Rate	0.017***	-0.006**	-0.049***	0.014***
	(0.004)	(0.002)	(0.013)	(0.005)
Dep. Var. Mean	0.6213	0.7291	2.8421	3.6306
Observations	9,518	9,518	5,301	5,301
R-squared	0.240	0.304	0.105	0.130

Notes: All regressions include a constant term, as well as metro area fixed effects, month-year fixed effects, and metro-specific time trends. Standard errors are clustered at the metro level. *** p<0.01, ** p<0.05, * p<0.1. Sample is limited to low-skilled Hispanic recent immigrants. Non-eligible individuals are U.S. citizens. Eligible individuals are not U.S. citizens.

Table 3: Robustness Check #1: Adding Further Immigration Policy Controls

Regressors	(1) Employed	(2) In LF	(3) Ln(Real Hourly Wages)	(4) Ln(Weekly Work Hours)
Eligible*Removals	-0.879**	-0.615*	-1.936**	0.395
	(0.377)	(0.322)	(0.824)	(0.337)
Removals	0.549*	0.563*	1.774***	-0.508*
	(0.273)	(0.279)	(0.586)	(0.293)
Eligible*GT	0.002	0.001	0.006	-0.003
	(0.008)	(0.006)	(0.026)	(0.005)
GT Score	-0.002	-0.002	0.004	0.003
	(0.008)	(0.006)	(0.025)	(0.005)
Eligible	-0.069*	-0.054**	-0.122*	-0.006
	(0.035)	(0.025)	(0.063)	(0.015)
Male	0.389***	0.416***	0.256***	0.123***
	(0.025)	(0.024)	(0.038)	(0.009)
Black	-0.029	-0.013	-0.000	0.032
	(0.040)	(0.033)	(0.064)	(0.024)
Other Race	0.039	0.037	-0.024	0.008
	(0.034)	(0.022)	(0.049)	(0.021)
Age	0.043***	0.047***	0.024**	0.013***
	(0.005)	(0.003)	(0.009)	(0.002)
Age Squared	-0.001***	-0.001***	-0.000**	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.046***	-0.074***	0.032	-0.004
	(0.013)	(0.010)	(0.028)	(0.008)
Number of Children	-0.027***	-0.031***	0.012	-0.006*
	(0.005)	(0.004)	(0.013)	(0.003)
Years in U.S.	0.003*	-0.001	0.006	0.001
	(0.002)	(0.002)	(0.005)	(0.001)
Unemployment Rate	0.012**	-0.009***	-0.052**	0.014*
	(0.005)	(0.003)	(0.020)	(0.007)
Police IE	0.074	0.031	0.038	0.004
	(0.045)	(0.025)	(0.151)	(0.052)
Employment IE	-0.104*	-0.081**	-0.054	-0.005
	(0.060)	(0.035)	(0.181)	(0.072)
DL for Undocumented	0.005	0.009	0.084	0.068**
	(0.039)	(0.038)	(0.117)	(0.025)
Dep. Var. Mean	0.6213	0.7291	2.8421	3.6306
Observations	9,518	9,518	5,301	5,301
R-squared	0.240	0.305	0.105	0.131

Notes: All regressions include a constant term, as well as metro fixed effects, month-year fixed effects, and metro-specific time trends. Standard errors are clustered at the metro level. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Robustness Check #2: Focusing on Low-Skilled, Mexican Immigrants

Regressors	**(1)**	**(2)**	**(3)**	**(4)**
	Employed	**In LF**	**Ln(Real Hourly Wages)**	**Ln(Weekly Work Hours)**
Eligible*Removals	-1.312***	-1.062**	-1.109	0.614
	(0.430)	(0.414)	(0.878)	(0.431)
Removals	0.893**	1.067**	1.640**	-0.705*
	(0.436)	(0.478)	(0.796)	(0.372)
Eligible*GT	-0.003	-0.001	0.009	0.007
	(0.009)	(0.006)	(0.031)	(0.005)
GT Score	0.001	-0.001	-0.008	-0.006
	(0.008)	(0.007)	(0.031)	(0.005)
Eligible	-0.067*	-0.052*	-0.207*	-0.026
	(0.035)	(0.027)	(0.102)	(0.021)
Dep. Var. Mean	0.6048	0.7026	2.8100	3.6355
Observations	6,146	6,146	3,343	3,343
R-squared	0.310	0.374	0.133	0.172

Notes: All regressions include a constant term, as well as metro area fixed effects, month-year fixed effects, and metro-specific time trends. Additional regressors include gender, race, age, age squared, marital status, number of children, years in the United States, local unemployment rates, local and state level immigration enforcement policies (police-based immigration enforcement index and employment-based immigration enforcement index) and a dummy for whether the state grants driver licenses to undocumented immigrants. Standard errors are clustered at the metro level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Heterogeneous Impacts for Low-Skilled Hispanic Immigrants in Key Industries

Regressors	(1) Employed	(2) In LF	(3) Ln(Real Hourly Wages)	(4) Ln(Weekly Work Hours)
Panel A: All				
Eligible*Removals	-0.523***	-0.183*	-3.338*	0.547
	(0.191)	(0.094)	(1.696)	(0.340)
Removals	0.064	0.094	3.402*	-0.686**
	(0.323)	(0.114)	(1.785)	(0.309)
Eligible*GT	-0.009	0.001	-0.011	-0.010
	(0.007)	(0.008)	(0.033)	(0.008)
GT Score	0.007	-0.003	0.020	0.010
	(0.007)	(0.007)	(0.034)	(0.007)
Eligible	-0.011	-0.004	0.054	-0.019
	(0.031)	(0.015)	(0.079)	(0.037)
Dep. Var. Mean	0.8371	0.9728	2.8522	3.6299
Observations	4,709	4,709	3,508	3,508
R-squared	0.108	0.076	0.150	0.154
Panel B: Women				
Eligible*Removals	-2.203*	-0.604	-6.347**	1.640*
	(1.226)	(0.631)	(2.786)	(0.902)
Removals	1.728	-0.284	-0.020	-0.600
	(1.228)	(0.941)	(4.992)	(1.427)
Eligible*GT	-0.004	0.013	0.051	-0.020
	(0.017)	(0.016)	(0.034)	(0.027)
GT Score	0.001	-0.018	-0.060	0.021
	(0.017)	(0.015)	(0.039)	(0.026)
Eligible	-0.004	-0.039	0.052	-0.070
	(0.097)	(0.047)	(0.196)	(0.100)
Dep. Var. Mean	0.8027	0.9469	2.6084	3.5184
Observations	1,054	1,054	762	762
R-squared	0.282	0.255	0.339	0.399
Panel C: Men				
Eligible*Removals	-0.242	-0.115	-2.467	-0.399
	(0.387)	(0.231)	(2.006)	(0.403)
Removals	-0.205	0.215	3.364	0.243
	(0.338)	(0.252)	(2.364)	(0.401)
Eligible*GT	-0.017*	-0.005	-0.030	0.002
	(0.009)	(0.004)	(0.058)	(0.007)
GT Score	0.015*	0.004	0.044	-0.001
	(0.008)	(0.003)	(0.059)	(0.007)
Eligible	0.001	0.007	0.025	0.007
	(0.035)	(0.021)	(0.120)	(0.022)
Dep. Var. Mean	0.8468	0.9801	2.9186	3.6607
Observations	3,571	3,571	2,682	2,682
R-squared	0.126	0.078	0.174	0.152

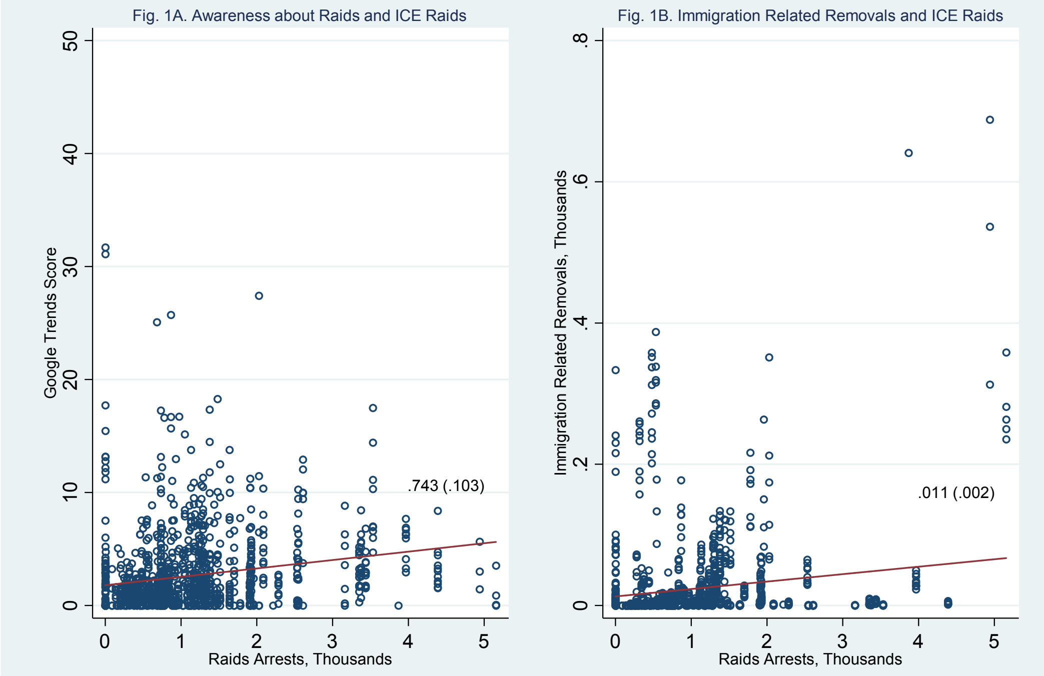
Notes: Key industries for the sample of both men and women (Panel A) include agriculture, construction, food processing, restaurants, travel and drinks, services to buildings, landscaping, and apparel manufacturing. To focus on gender-specific industries, the sample of women (Panel B) excludes construction, and the sample of men (Panel C) excludes apparel manufacturing. All regressions include a constant term, as well as metro fixed effects, month-year fixed effects, and metro-specific time trends. Additional regressors include gender, race, age, age squared, marital status, number of children, years in the United States, local unemployment rates, local and state level immigration enforcement policies (police-based immigration enforcement index and employment-based immigration index), and a dummy for whether the state grants driver licenses to undocumented immigrants. Standard errors are clustered at the metro level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6
Heterogeneous Impacts for Low-Skilled Hispanic Women and Men with Children in the Restaurant Industry

Regressors	(1) Employed	(2) In LF	(3) Ln(Real Hourly Wages)	(4) Ln(Weekly Work Hours)
Panel A: Women				
Eligible*Removals	-7.761***	-3.439*	-29.173***	-0.802
	(2.265)	(1.857)	(10.522)	(3.718)
Removals	4.767*	1.771	23.319*	-2.941
	(2.357)	(2.402)	(11.787)	(5.386)
Eligible*GT	-0.079	-0.034	-0.122	-0.044
	(0.049)	(0.030)	(0.116)	(0.111)
GT Score	0.084	0.027	0.130	0.061
	(0.055)	(0.035)	(0.127)	(0.122)
Eligible	0.525**	0.208	0.748	0.427
	(0.216)	(0.157)	(0.614)	(0.362)
Dep. Var. Mean	0.7930	0.9193	2.3781	3.5049
Observations	285	285	213	213
R-squared	0.704	0.690	0.803	0.809
Panel B: Men				
Eligible*Removals	4.985	2.935*	-26.774	-8.300
	(5.491)	(1.437)	(18.116)	(6.557)
Removals	0.094	1.965**	-18.788	-2.903
	(2.862)	(0.791)	(19.865)	(2.788)
Eligible*GT	0.082	0.033	-1.164	0.181
	(0.137)	(0.029)	(3.425)	(0.624)
GT Score	-0.079	-0.039	1.134	-0.172
	(0.136)	(0.031)	(3.366)	(0.623)
Eligible	-0.553**	-0.018	0.731	-0.397**
	(0.232)	(0.043)	(1.648)	(0.176)
Dep. Var. Mean	0.9140	0.9955	2.8171	3.6980
Observations	221	221	193	193
R-squared	0.810	0.920	0.795	0.827

Notes: All regressions include a constant term, as well as metro fixed effects, month-year fixed effects, and metro-specific time trends. Additional regressors include gender, race, age, age squared, marital status, number of children, years in the United States, local unemployment rates, local and state level immigration enforcement policies (police-based immigration enforcement index and employment-based immigration enforcement index), and a dummy for whether the state grants driver licenses to undocumented immigrants. Standard errors are clustered at the metro level. *** p<0.01, ** p<0.05, * p<0.1

Figure 1
ICE Raids, Awareness and Removals due to Immigration Violations



Coefficient (robust standard error) from bivariate regression noted on graph

APPENDIX 1: Additional Tables and Figures

Table A: Google Trends Metro Areas Used in the Analysis

Atlanta, GA
Austin, TX
Baltimore, MD
Boston, MA-Manchester, NH
Charlotte, NC
Chicago, IL
Cincinnati, OH
Cleveland-Akron (Canton), OH
Columbus, OH
Dallas-Ft. Worth, TX
Denver, CO
Detroit, MI
Houston, TX
Indianapolis, IN
Kansas City, MO
Las Vegas, NV
Los Angeles, CA
Miami-Ft. Lauderdale, FL
Minneapolis-St. Paul, MN
Nashville, TN
New York, NY
Orlando-Daytona Beach-Melbourne, FL
Philadelphia, PA
Phoenix, AZ
Portland, OR
Raleigh-Durham (Fayetteville), NC
Sacramento-Stockton-Modesto, CA
San Antonio, TX
San Diego, CA
San Francisco-Oakland-San Jose, CA
Seattle-Tacoma, WA
Tampa-St. Petersburg (Sarasota), FL
Washington, DC (Hagerstown, MD)

Source: Google Trends data on ICE raid/raids and immigration raid/raids searches.

Table B: Sample Descriptive Statistics by Gender

By Gender	Women			Men		
Variable Name	**N**	**Mean**	**S.D.**	**N**	**Mean**	**S.D.**
Employed	4203	0.3935	0.4886	5315	0.8015	0.3989
In the Labor Force	4203	0.4820	0.4997	5315	0.9246	0.2641
Ln (Real Hourly Wages)	1496	2.6906	0.9473	3805	2.9016	0.9453
Ln (Weekly Work Hours)	1496	3.5388	0.3439	3805	3.6666	0. 2071
GT Score	4203	2.2040	3.6871	5315	2.1975	3.7483
Average Monthly Immigration Related Removals Per 1,000	4203	0.0153	0.0354	5315	0.0150	0.0378
Average Yearly Raids per 1,000	2704	1.0788	0.9054	3373	1.0976	0.9354
Eligible	4203	0.9305	0.2543	5315	0.9556	0.2060
Male	4203	0.0000	0.0000	5315	1.0000	0.0000
Black	4203	0.0274	0.1632	5315	0.0213	0.1443
Other Race	4203	0.0321	0.1763	5315	0.0412	0.1988
Age	4203	33.7545	10.9306	5315	31.6995	9.9623
Married	4203	0.6136	0.4870	5315	0.4847	0.4998
Total Number of Children	4203	1.4154	1.2533	5315	0.7249	1.1418
High School Education	4203	0.0000	0.0000	5315	0.0000	0.0000
More than High School Education	4203	0.0000	0.0000	5315	0.0000	0.0000
Foreign-born	4203	1.0000	0.0000	5315	1.0000	0.0000
Mexican	4203	0.6562	0.4750	5315	0.6374	0.4808
Years in the U.S.	4203	4.9607	2.9588	5315	4.5913	3.0328
Unemployment Rate	4203	5.1948	1.8389	5315	5.0000	1.7448
Police-based Immigration Enforcement Index	4203	0.2241	0.3964	5315	0.2082	0.3867
Employment-based Immigration Enforcement Index	4203	0.1311	0.3375	5315	0.1269	0.3329
State Grants Driver Licenses to Undocumented Immigrants	4203	0.0269	0.1618	5315	0.0290	0.1678

Table C: Robustness to Alternative Definition of Eligible

Regressors	(1) Employed	(2) In LF	(3) Ln(Real Hourly Wages)	(4) Ln(Weekly Work Hours)
Eligible*Removals	-0.922**	-0.660**	-2.370**	0.347
	(0.362)	(0.317)	(1.089)	(0.318)
Removals	0.567**	0.595**	2.101***	-0.468*
	(0.250)	(0.274)	(0.694)	(0.272)
Eligible*GT	-0.000	-0.001	-0.014	-0.005
	(0.008)	(0.005)	(0.026)	(0.005)
GT Score	0.000	0.001	0.023	0.004
	(0.008)	(0.006)	(0.027)	(0.005)
Eligible	-0.055**	-0.041*	0.015	0.006
	(0.023)	(0.023)	(0.094)	(0.014)
Male	0.388***	0.416***	0.253***	0.123***
	(0.025)	(0.024)	(0.038)	(0.009)
Black	-0.030	-0.014	-0.004	0.031
	(0.040)	(0.033)	(0.066)	(0.023)
Other Race	0.040	0.038*	-0.026	0.009
	(0.034)	(0.022)	(0.049)	(0.020)
Age	0.043***	0.047***	0.024**	0.013***
	(0.005)	(0.003)	(0.009)	(0.002)
Age Squared	-0.001***	-0.001***	-0.000**	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.047***	-0.074***	0.032	-0.004
	(0.013)	(0.009)	(0.027)	(0.008)
Number of Children	-0.026***	-0.031***	0.013	-0.006*
	(0.005)	(0.004)	(0.013)	(0.003)
Years in U.S.	0.003*	-0.001	0.006	0.001
	(0.002)	(0.002)	(0.005)	(0.001)
Unemployment Rate	0.017***	-0.006**	-0.050***	0.014***
	(0.004)	(0.002)	(0.013)	(0.005)
Dep. Var. Mean	0.621	0.729	2.842	3.631
Observations	9,518	9,518	5,301	5,301
R-squared	0.240	0.304	0.104	0.130

Notes: All regressions include a constant term, as well as metro area fixed effects, month-year fixed effects, and metro-specific time trends. Standard errors are clustered at the metro level. *** p<0.01, ** p<0.05, * p<0.1. *Sample:* Low-skilled Hispanic recent immigrants. Non-eligible individuals meet any of the following criteria: (1) U.S. citizen, (2) veteran, (3) born in Cuba, (4) work in the government sector. Otherwise, they are eligible.

Table D: Robustness to Inclusion of Metro-Specific Cubic Time Trends

Regressors	(1) Employed	(2) In LF	(3) Ln(Real Hourly Wages)	(4) Ln(Weekly Work Hours)
Eligible*Removals	-0.896**	-0.678**	-2.159**	0.364
	(0.410)	(0.326)	(0.854)	(0.347)
Removals	0.586*	0.648**	2.317***	-0.507
	(0.313)	(0.299)	(0.512)	(0.320)
Eligible*GT	0.002	0.001	0.003	-0.003
	(0.008)	(0.007)	(0.027)	(0.005)
GT Score	-0.002	-0.002	0.007	0.003
	(0.008)	(0.007)	(0.026)	(0.005)
Eligible	-0.066*	-0.054**	-0.105	0.002
	(0.036)	(0.026)	(0.066)	(0.015)
Male	0.387***	0.416***	0.257***	0.124***
	(0.025)	(0.024)	(0.040)	(0.009)
Black	-0.029	-0.015	-0.019	0.040*
	(0.040)	(0.034)	(0.065)	(0.022)
Other Race	0.042	0.040*	-0.048	0.011
	(0.033)	(0.021)	(0.049)	(0.022)
Age	0.043***	0.047***	0.026***	0.013***
	(0.005)	(0.003)	(0.009)	(0.002)
Age Squared	-0.001***	-0.001***	-0.000**	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.047***	-0.075***	0.026	-0.005
	(0.013)	(0.010)	(0.027)	(0.008)
Number of Children	-0.027***	-0.031***	0.013	-0.006*
	(0.005)	(0.004)	(0.013)	(0.003)
Years in U.S.	0.003	-0.001	0.006	0.001
	(0.002)	(0.002)	(0.005)	(0.001)
Unemployment Rate	0.015***	-0.009***	-0.035***	0.016***
	(0.005)	(0.002)	(0.010)	(0.005)
Dep. Var. Mean	0.621	0.729	2.842	3.631
Observations	9,518	9,518	5,301	5,301
R-squared	0.244	0.311	0.119	0.148

Notes: All regressions include a constant term, as well as metro area fixed effects, month-year fixed effects, and metro-specific cubic time trends. Standard errors are clustered at the metro level. *** p<0.01, ** p<0.05, * p<0.1. *Sample:* Low-skilled Hispanic recent immigrants. Non-eligible individuals are U.S. citizens. Eligible individuals are not U.S. citizens.

Figure A
Immigration Related Removals and Awareness in Two Metro Areas over Time

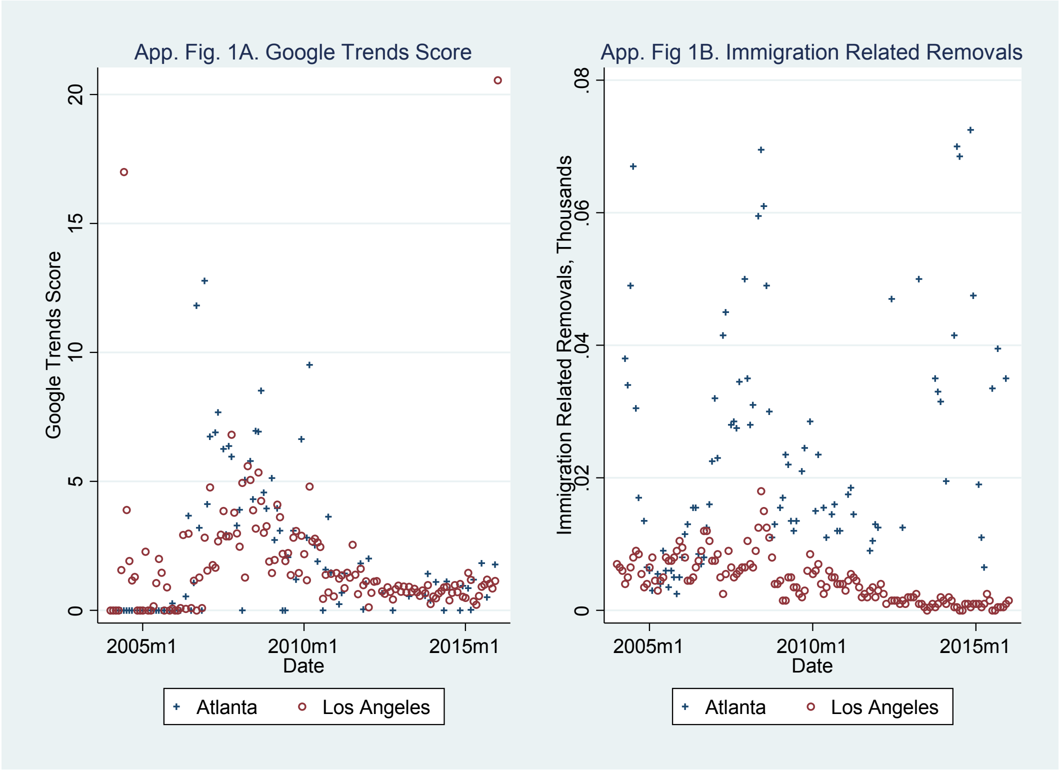


Figure B: Assessing Differential Pre-trends in Labor Market Outcomes

Figure B1
Employment Likelihood Prior to Immigration Enforcement Shocks

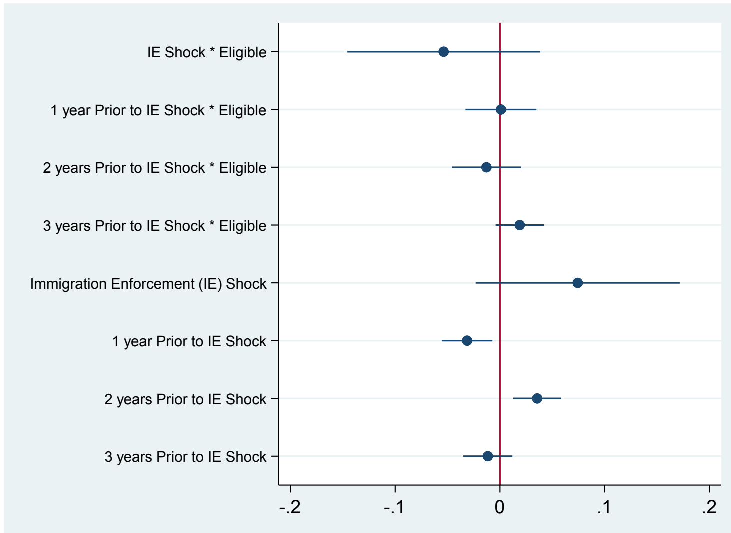
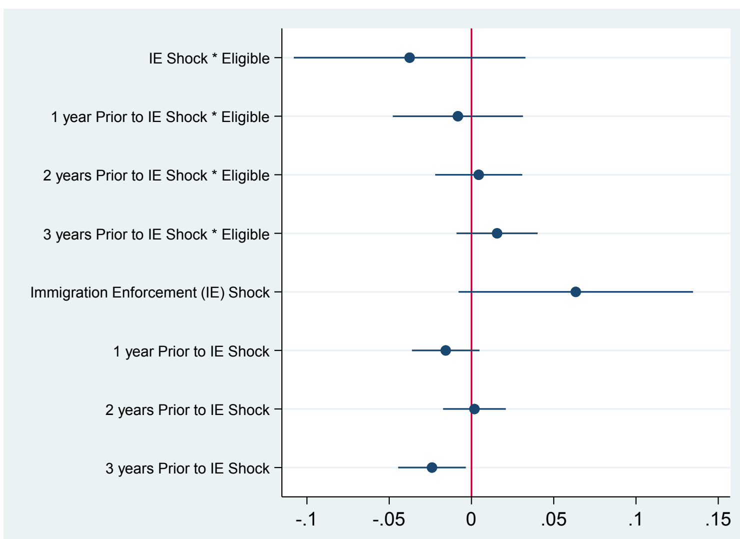


Figure B2
Labor Force Participation Prior to Immigration Enforcement Shocks



Notes: The figures above display the coefficient estimates and 90% confidence intervals for the variable *Immigration Enforcement (IE) Shock* –an indicator equal to 1 if the metro-period observation has immigration removals and a Google Trend score (searches for immigration raids/ICE raid(s)) that are above the median within that metro over the time span under study; *IE shock* equals 0 otherwise. The *IE Shock* indicator and its interaction with the *Eligible* dummy replaces *removals,* the *GT* score, and their interaction terms with the *eligible* dummy in equation (2). In addition, equation (2) includes three new lead indicators, *1 year prior to IE shock, 2 years prior to IE shock, and 3 years prior to IE shock*, which equal 1 one, two, and three periods prior to the shock indicator turning positive; they are 0 otherwise. These lead indicators are interacted with the *eligible* dummy and included in the model as well, along with the *eligible* dummy, a constant term, metro area fixed effects, month-year fixed effects, and metro-specific time trends. Other regressors include gender, race, age, age squared, marital status, number of children, years in the United States, and local unemployment rates. Standard errors are clustered at the metro level. *** p<0.01, ** p<0.05, * p<0.1.

Figure B3
Hourly Wage Prior to Immigration Enforcement Shocks

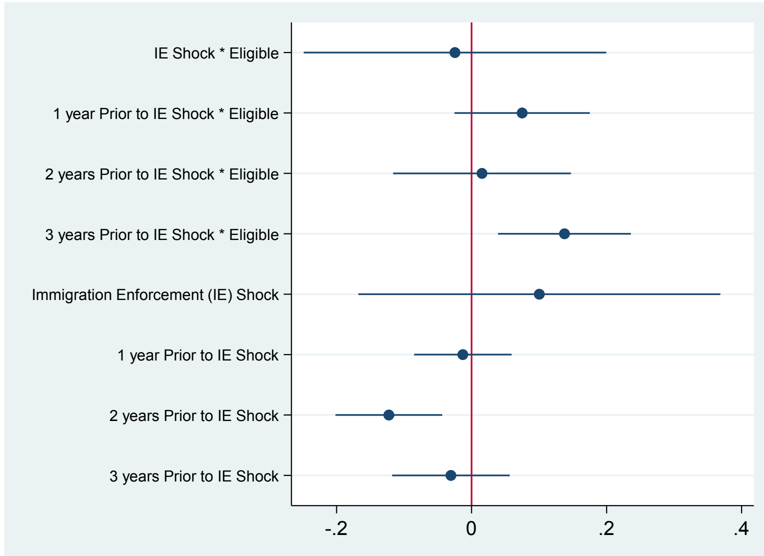
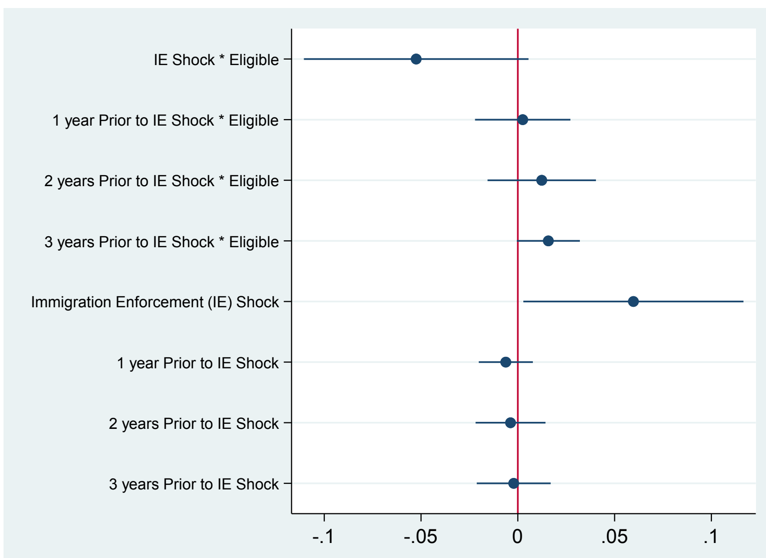


Figure B4
Hours Worked Prior to Immigration Enforcement Shocks



Notes: The figures above display the coefficient estimates and 90% confidence intervals for the variable *Immigration Enforcement (IE) Shock* –an indicator equal to 1 if the metro-period observation has immigration removals and a Google Trend score (searches for immigration raids/ICE raid(s)) that are above the median within that metro over the time span under study; *IE shock* equals 0 otherwise. The *IE Shock* indicator and its interaction with the *Eligible* dummy replaces *removals,* the *GT* score, and their interaction terms with the *eligible* dummy in equation (2). In addition, equation (2) includes three new lead indicators, *1 year prior to IE shock, 2 years prior to IE shock, and 3 years prior to IE shock*, which equal 1 one, two, and three periods prior to the shock indicator turning positive; they are 0 otherwise. These lead indicators are interacted with the *eligible* dummy and included in the model as well, along with the *eligible* dummy, a constant term, metro area fixed effects, month-year fixed effects, and metro-specific time trends. Other regressors include gender, race, age, age squared, marital status, number of children, years in the United States, and local unemployment rates. Standard errors are clustered at the metro level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix 2: Additional Data Description

We use three data sources in this analysis: (1) the Current Population Survey, (2) the Google Trends index, and (3) TRAC data. All three datasets are linked using information on the names of metropolitan areas provided in the first two data sets and the city name available in the TRAC data set. This results in a sample composed of the 33 GT metro areas listed in Appendix 1, Table A. In the text, we describe in greater detail the outcomes and our two key policy measures –namely, the GT index and the "de facto" removals measure. Herein, we provide more detail on the additional controls included in the analysis, including the (*de jure*) immigration enforcement indexes.

The analysis includes information on a series of standard demographic controls on individual respondents (X_{imt}) from the CPS, such as race, age, marital status, number of children and years in the United States. Additional local controls include the metro area's unemployment rate, as well as various immigration policies (Z_{mt}). The latter include a variety of immigration enforcement policies enacted at both the state and local levels included in a police-based immigration enforcement index (*i.e.* 287(g) agreements, Secure Communities, and omnibus immigration laws detailed in Table 2.1 below) and employment-based immigration enforcement index (*i.e.* employment verification mandates, also detailed in Table 2.1 below), as well as a dummy for whether the state is one granting driver licenses to undocumented immigrants.

We follow Amuedo-Dorantes *et al.* (2018a) in the construction of the two interior (*de jure*) immigration enforcement indexes. First, we create indexes indicative of the exposure to various types of interior immigration enforcement measures (described in Table 2.1 below) at the (MSA, year) level. In the case of state level policies, the latter consists of a dummy variable that turns one when the state adopts the policy. In the case of local (county) level policies, we compute the following index for each policy at the (MSA, year) level:

$$(2.1) \quad {EI^k}_{m,t} = \frac{1}{N_{m,2000}} \sum_{\boldsymbol{c \in m}}^{\boldsymbol{m}} \frac{1}{12} \sum_{\boldsymbol{j=1}}^{\boldsymbol{12}} \mathbf{1}\left(E_{c,j}^k\right) P_{c,2000}$$

where k refers to one of the following policies: local 287(g), state-level 287(g), Secure Communities, omnibus immigration laws, and E-Verify. The indicator function: $\mathbf{1}\left(E_{c,j}^k\right)$ informs about the implementation of measure k in county c in month j during the year in question; $P_{c,2000}$ is the population of county c according to the 2000 Census –that is, prior to the rollout of the enforcement initiatives being considered; and $N_{m,2000}$ is the total population in the MSA.

Next, we sum up the indices of the various police-based immigration enforcement initiatives (namely, the ones corresponding to 287(g) agreements, Secure Communities, and Omnibus Immigration Laws) at the (MSA, year) level to create the police-based immigration enforcement index, as follows:

$$(2.2) \qquad \mathrm{EI}_{m,t} = \sum_{k \in K}^{K} EI_{m,t}^k$$

Separately, we use the index described by equation (2.1) above for the E-Verify mandates as our measure of employment-based immigration enforcement.

Finally, information on whether the state grants driver licenses to undocumented immigrants is gathered from Amuedo-Dorantes *et al.* (2018b) to create a dummy that equals one if that is the case in the state (and MSA) in question in a particular year.

Table 2.1 Description of Enforcement Laws

287(g) Agreements (2002-2012) (2017-onwards)
The aim of these policies is to make communities safer by the identification and removal of serious criminals. State and local enforcement entities signed a contract (Memorandum of Agreement -MOA) with the U.S. Immigration and Customs Enforcement (ICE). There are various functions: • **Task Force**: allows local and state officers to interrogate and arrest non-citizens during their regular duties on law enforcement operations. • **Jail enforcement** permits local officers to question immigrant who have been arrested on state and local charges about their immigration status. • **Hybrid model:** which allows participation in both types of programs. Source: ICEs 287(g) Fact Sheet website, Amuedo-Dorantes and Bansak (2014), and Kostandini *et al.* (2013).
Secure Communities (2009-2014) (2017-onwards)
They are enacted in order to identify non-citizens who have committed serious crime using biometric information. The program allows for the submission of biometric information on detainees that is contrasted against records in FBI and DHS databases. *Source:* ICE's releases on activated jurisdictions: https://www.ice.gov/doclib/secure-communities/pdf/sc-activated.pdf
Omnibus Immigration Laws (2010-onwards)
Comprehensive laws that may include: • A "show me your papers" clause, enabling the police to request proper identification documentation during a lawful stop. • Require that schools report students' legal status. *Source:* http://www.ncsl.org/documents/statefed/omnibus_laws.pdf
E-Verify (2006-onwards)
Electronic program that allows employers to screen newly hired workers for work eligibility. *Source:* National Conference of State Legislatures.