

# The Economic Impact of Migrants from Hurricane Maria

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## ABSTRACT

We examine the economic impact of the large Puerto Rican migration into Orlando following Hurricane Maria in 2017. Using a synthetic control approach, we find non-Hispanic employment increased in Orlando, and find positive aggregate labor market effects for less-educated workers. The employment effect was particularly large in the construction sector. While we find that construction earnings decreased slightly, this was balanced by earnings growth in retail and hospitality. This is consistent with immigration having small negative impacts on earnings in sectors exposed to a labor supply shock, offset by positive effects in sectors impacted by an associated positive demand shock.

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## I. Introduction

In September 2017 Hurricanes Irma and Maria struck Puerto Rico in rapid succession, bringing catastrophic loss of property and life across the island. In the months following the hurricane, over 120,000 individuals and families rapidly fled Puerto Rico for the U.S. mainland. This paper examines the short-run economic impact of this sudden migration event on Orlando—the city which, because of pre-existing network connections, received the largest plurality of these migrants. We study the impact of this sudden migration event on the aggregate Orlando labor market, as well as by sector. We also examine whether there is evidence of effects on employment and earnings for natives and for less-educated workers. To do so, we employ a synthetic control approach using a data source that provides virtually-complete coverage of county-level employment outcomes at a monthly or quarterly frequency throughout our study period.

We find that, one year after the hurricanes, this migrant inflow had a significant positive effect on Orlando's overall employment, as well as on less-educated employment. The construction sector saw the largest boost in employment, while retail employment also grew. Native and less-educated earnings (for each group, collectively) in the construction sector fell slightly, but this was fully offset by earnings growth for these groups in the much larger retail and hospitality sectors,<sup>1</sup> such that this immigrant inflow had no negative impact on average earnings.

Several papers have analyzed the local labor market effects of sudden waves of immigration. Examples include the analysis of the inflow of Czech workers in Germany (Dustmann and Stuhler, 2017), Soviet Jews to Israel during the 1990s (Cohen-Goldner and Paserman, 2011); the inflow of Syrians to Turkey in 2013-15 (Cengiz and Tekgüç, 2021) and (Ceritoglu et al., 2017); and the inflow of Algerians to France in the 1950's (Hunt, 1992). The most studied migration event in the U.S.—potentially *over*-studied, given the limited amount of data available and the time period—is the Mariel Boatlift.<sup>2</sup> This was an episode in which approximately 100,000 people fled from Cuba,

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<sup>1</sup> Respectively accounting for 12% and 15% of local employment, while construction was 6%.

<sup>2</sup> Another weather event similar to Maria, namely Hurricane Katrina, has also been studied, although most of the papers focus on the impact on the *out*-migrants who fled New Orleans and

with most relocating to Miami. While early studies established that this large inflow had no impact on local wages even in the short run (Card, 1990), recent re-analyses of the event have generated some disagreement on the effects, especially with respect to the impact on the small subgroup of male, less-educated, native workers (Borjas, 2017; Peri and Yasenov, 2019; Clemens and Hunt, 2019). The details of the controversy are centered on measurement, sample choice, and methods used.

The event we study here is more relevant than the Mariel Boatlift for informing our current understanding of the local labor market impact of immigration in the U.S. for several reasons. First, this event is much more recent, and the economy of Orlando in 2018 is more comparable to current U.S. metropolitan areas than was the economy of Miami in 1980. Second, the impact of this episode is a consequence of migration in response to an extreme weather event—an occurrence likely to become more frequent in the future. Third, the case of immigration from Puerto Rico involves people with similar levels of education to native mainland U.S. workers, while the Cubans in the Mariel Boatlift (Marielitos) had much less education. This means that the Hurricane Maria migration resembled the type of immigration the U.S. has received in the last two decades (2000-2018)—which included a large share of college-educated immigrants—while the low-skilled immigration of Marielitos was more similar to the immigration of the 1990s. We also note that, similarly to the Cuban Marielitos, immigrants from Puerto Rico had immediate access to the U.S. labor market and other benefits available to U.S. citizens, which implies that their potential crowding-out impact on native workers could be achieved in the short-run. Lastly, this episode of migration in Orlando resembles, in size, the more common inflows of immigrants to the US which occurred in the 1990's and 2000's (averaging 0.3 to 0.5% of the labor force each year), and is therefore more useful if we want to draw relevant policy conclusions applicable to US the surrounding area (rather than on people in the receiving communities). De Silva et al. (2010) look at the impact on wages of a large relocation of evacuees on Houston, and find that there was an aggregate, small wage depression of 0.7 percent in low-skill industries. Compared to De Silva et al. (2010), our paper uses a more recently developed and, we argue, more appropriate estimation strategy to test a similar set of outcomes.

immigration.<sup>3</sup>

An advantage of our study, relative to the previous literature which often relies on small, weighted samples of survey data from the Current Population Survey (CPS), is that we use data from the Quarterly Census of Employment and Wages (QCEW) and the Quarterly Workforce Indicators (QWI). These are county-by-industry-level administrative data covering 95+% of all workers, observed on a quarterly basis.<sup>4</sup> The detail and precision of our analysis is therefore significantly greater than previous studies looking at impacts of migration on local labor markets.

Using FEMA application data and referencing several other data sources, we establish that the Orlando commuting zone (CZ) received significantly more migrants as a result of the hurricanes than did any other city.<sup>5</sup> Over 120,000 people left Puerto Rico for the U.S. mainland between September 2017 and March 2018. We estimate 24,000 ended up in Orlando, equivalent to around 1 percent of Orlando's pre-hurricane population and around 2 percent of pre-hurricane employment.

While we implement the Synthetic Control Method (SCM), the validity of our identification strategy is based on similar conditions as the validity of a shift-share instrument approach, commonly used in the study of migrants' impacts on destination economies. We argue that the migrant inflow to Orlando was largely driven by the "attraction" exerted by the existing network of Puerto Rican, rather than any pre-hurricane economic characteristics that may have been attractive to migrants. Still, as the pre-hurricane presence of Puerto-Rican in Orlando was not random, in order to obtain estimated treatment effects free of omitted variable bias we require a plausible choice of a counterfactual economy that is similar to Orlando except for the migrant inflow, against which we

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<sup>3</sup> As the inflow of migrants to Orlando was a significantly smaller fraction of the labor force, relative to the Mariel Boatlift, measurement error could be larger. Additionally some migrants were only temporarily in Orlando, as a number likely returned to Puerto Rico one year later. However, as our data allow substantially greater coverage, this reduces measurement error. Additionally we only analyze short-run effects. It is thus reasonable to argue that our paper speaks to the short-run impact of medium-size immigration flows.

<sup>4</sup> Employment is observed on a monthly basis in the QCEW. Other variables are observed quarterly.

<sup>5</sup> Five counties comprise the Orlando CZ: Lake, Orange, Osceola, and Seminole—which comprise the Orlando Metropolitan Area—plus Sumter County. Orlando alone received 20% of the FEMA applications, while the top-five treated CZs received a combined 34% of the FEMA applications.

can compare the evolution of Orlando’s outcomes of interest. Accordingly, for each outcome we estimate a “synthetic control” (SC) for Orlando as a convex combination of untreated “donor pool” CZs which did not receive migrants, selected and weighted to best match Orlando’s pre-treatment labor market and trends in the outcome variable. Conditional on a good pre-treatment match (or “fit”) between Orlando and its synthetic control, the migrants’ selection of Orlando is plausibly orthogonal to any differential pre-treatment economic conditions that could drive post-treatment differentials in the outcome variable. Given the eventual CZ composition of our synthetic controls, our estimated effects should be interpreted as the economic impact of immigration on a city with a large existing immigrant network that could ease assimilation (Martén, Hainmueller, and Hangartner, 2019), and not necessarily as the effect on an average US city.

We study the effect of this migration event on the Orlando economy as it happened between September 2017 and September 2018. We look first at *aggregate* effects on employment and earnings per person, then shift our focus to specific sectors—namely the *construction*, *retail* trade, and accommodations & food services sectors (hereafter, *hospitality*). We analyze first all workers, then specifically focus, individually, on non-Hispanic and less-educated subgroups (those with a high-school degree or less education) as proximate measures, respectively, of the native-worker population (Orlando non-Hispanic residents are likely to be U.S.-born, or “natives”) and of those workers who may be in stronger competition with the new Puerto Rican arrivals who could easily take simple low-skilled jobs in the short-run.

Focusing on specific sectors in the short run helps us to *separately* identify effects of the immigrant wave on labor demand and on labor supply.<sup>6</sup> We argue that the construction sector was most likely to experience the labor supply shock in the short run, as construction workers were disproportionately Hispanic pre-hurricane, and most construction jobs require limited use of English. Conversely, retail and hospitality were more likely to experience positive demand shocks

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<sup>6</sup> The previous standard for this type of analysis was Bodvarsson, Van den Berg, and Lewer (2008) and Bodvarsson and Van den Berg (2006), who tested possible demand effects of immigrants, looking at local demand shocks in a tradable sector.

in the short run, as new additions to a local population require accommodations, retail goods, and food services. By quantifying the effect on employment and wages in these three sectors (which together employed 33% of Orlando's workforce), we are able to show that immigration boosts labor demand as well as labor supply.

A final novelty is that we consider how the number of establishments—a rough proxy for capital investment—responded to the migration event. The speed and magnitude of the response by firms is relevant for understanding the mechanisms through which the labor supply shock was absorbed by the local economy in the short run. While our estimated effects are all positive, their pre-treatment trends are noisy enough that they are not significant at conventional levels.

Our results are consistent with a story in which the arrival of the Puerto Rican migrants in Orlando constituted a labor supply shock concentrated in the construction sector, and also constituted a local demand shock for goods and services which was especially visible in the retail and hospitality sectors, translating into increased labor demand in those sectors. While we find evidence of a negative impact on construction sector earnings for native workers collectively and less-educated workers collectively, 12 months after Hurricane Maria, we find symmetric evidence that retail earnings increased for those workers over that period. Looking in aggregate, despite the sudden influx of tens of thousands of new workers, we find no evidence of any negative impact on wages, suggesting that the sector-specific positive wage effects more than offset the negative ones in the aggregate. This aggregate balancing effect holds for native workers as well. In fact, we also find positive employment effects for both non-Hispanic and less educated workers (for each group, collectively) 12 months after the hurricane. These results are robust to several falsification tests and alternative specifications, and suggest that this large inflow of workers was quickly absorbed into the local economy without adversely affecting average wages.

The rest of this paper proceeds as follows: in [Section II](#), we describe the demographic and employment trends of immigrants from Puerto Rico in the mainland United States. We then provide a measure of the immigrant wave and justification for focusing on Orlando as the (most intensely)

treated unit. [Section III](#). describes our data and empirical methodology. [Section IV](#). describes the results from our analyses, and presents a series of robustness checks—including a test for whether our results were impacted by Hurricane Irma’s effects on Orlando, estimates using three or five metropolitan areas/commuting zones rather than Orlando only, and an analysis of average housing prices before and after the migrants’ arrival. [Section V](#). concludes.

## II. Characteristics and Trends of Puerto-Rican Immigration

### A. *Historical Migration and Characteristics of Migrants*

Due to strong migration from Puerto Rico to the mainland U.S. during the latter part of the 20th century, significant concentrations of Puerto Ricans developed in New York and, especially when looking at shares of the local population, in Orlando (see Online Appendix Figure A1).<sup>7</sup> Orlando was a particularly popular destination in the years leading up to 2017, prior to Hurricane Maria. Puerto Ricans’ full access to labor opportunities in the mainland U.S. suggests that job availability may have been a key driver of mobility from the island to the mainland.<sup>8</sup>

[Table 1](#) shows differences in average characteristics of Puerto Rican migrants who arrived in the year preceding Hurricane Maria, compared to reference groups.<sup>9</sup> This gives an idea of the type of selection prevailing among Puerto Rican migrants right before the hurricane. The first column shows the difference with US natives, the second with residents of Puerto Rico, the third with US natives in Florida.<sup>10</sup> The largest difference between recent Puerto Rican immigrants and US natives is in their average age, as Puerto Rico immigrants are significantly younger and less likely

<sup>7</sup> The figures are constructed using American Community Survey from 2005 to 2017, downloaded from IPUMS USA (Ruggles et al., 2021).

<sup>8</sup> Migrants coming to the U.S. are often positively “selected” from their country in terms of education, mainly because of the large skill premium paid in the U.S. relative to countries of origin (Grogger and Hanson, 2011). Borjas (2008), however, documented negative selection of pre-2000 Puerto-rican immigrants, in terms of education.

<sup>9</sup> Individuals are restricted to those who are in the labor force.

<sup>10</sup> A negative value implies that the Puerto Rico migrants have a smaller value for that variable relative to the comparison group.

to be married than natives. They are also more likely to be male. On the other hand, education levels are similar. While the difference in average years of schooling compared to U.S. natives is statistically significant, compared to the average Florida native it is small and *not* significant (0.38 and 0.13 years of schooling, respectively). There is no significant difference in the share of college-educated immigrants from Puerto Rico and natives in the U.S. or in Florida. The inflow can therefore be characterized as mostly younger than but similarly educated to natives.

[[Table 1](#) about here]

### ***B. Size of the migration wave After Hurricane Maria***

Hurricanes Irma and Maria hit Puerto Rico in September 2017. Initial estimates, based on Facebook usage data, indicated that 44,000 people moved from Puerto Rico to Florida in the aftermath of hurricane Maria *and stayed* there until at least March 2018 (Alexander, Zagheni, and Polimis, 2019). Estimates based on flight passenger lists similarly confirm between 30,000 and 50,000 people moved out of Puerto Rico to Florida in the months after the hurricane (Rayer, 2018). These numbers likely underestimate the flow. Complementing these data, Özek (2021) documents that 12,000 children, who were Maria migrants registered for school in Florida after the hurricane.<sup>11</sup>

A U.S. Census Bureau (2018) analysis of ACS/PRCS data from the U.S. Census Bureau puts the number of out-migrants from Puerto Rico between July 2017 and July 2018 at the much larger figure of 123,399, while the Center for Puerto Rican Studies estimates this number to be more than 135,000 (Centro, 2018). We complement these estimates with additional evidence to get an idea of the distribution of those evacuees across the U.S. mainland. Data from applications for disaster relief from the Federal Emergency Management Agency (FEMA) are useful to determine the geographic distribution of evacuees. These application data, obtained through a Freedom of Information Act (FOIA) request, represent claims made to FEMA to obtain disaster relief funds, filed by people who had a home in Puerto Rico which was damaged by Hurricanes Irma and/or Maria.

<sup>11</sup> Özek (2021) shows also that one third of those children were in a large, anonymous Florida school district we believe to be Orlando Unified.



By looking at the ZIP code of residence before the hurricane and the ZIP code at the time of filing (after Hurricane Maria occurred), we can identify the likely residence of these Puerto Ricans on the mainland in the months after the hurricane. The distribution of these applications (aggregated to commuting zones and shown in Figure A2 in the Online Appendix, in both level and per capita terms) reveals that these FEMA applications were heavily concentrated in relatively few areas. Especially in per capita terms, the Orlando area exhibits by far the largest concentrations: with nearly 4,000 applications, Orlando had more than two-and-a-half times as many applications as the next most heavily affected commuting zones (Fort Lauderdale-Miami and New York-Nassau-Suffolk). These FEMA applications in the Orlando area represent 20 percent of the total received.

Our best estimate of the size of the migrant wave into Orlando, combining the U.S. Census Bureau estimate of 123,399 out-migrants from Puerto Rico (between July 2017 and July 2018) with Orlando's share of the FEMA applications, is around 24,000 migrants who moved and stayed till July 2018. This represents over 1 percent of Orlando's population, 1.5 percent of its working-age population (aged 16-64), and around 2 percent of its pre-hurricane total employment. We conservatively estimate that *at least* half of these migrations are directly attributable to the hurricanes (based on out-migration trends from Puerto Rico in the years prior). This inflow was smaller than that of Cubans arriving in Miami after the Boatlift (8% of the Miami labor force), but was still a significant shock to Orlando's population and labor force, especially as the large majority of this inflow took place within a relatively short period of 1-3 months.<sup>12</sup> For context, an inflow of immigrants equal to one percent of the labor force in one year would be similar to the annual inflow of Mexicans into the US during the 1990s, when Mexican immigration was at its peak. Beginning in late 2018 migrants began returning to Puerto Rico. As such, we focus on the short-run effects up to the third quarter of 2018. We note that, based on an analysis of the Puerto Rican Community Survey (PRCS), downloaded from (Ruggles et al., 2021), we do not see any evidence of selective

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<sup>12</sup> As we demonstrate in robustness checks using the three- and five-most treated CZs, this shock was more than large enough to generate the effects we observe.

return migration into Puerto Rico by age or education level during this period.<sup>13</sup>

### ***C. Sector Distribution of Puerto Rican Migrants***

Prior to the hurricanes, the distribution of Puerto Rican migrants across sectors of employment in Orlando was not very different from that of natives (Online Appendix Table A3). As of 2016, Puerto Rican natives were slightly more concentrated in local services such as *transportation* and *retail*, and slightly less concentrated in *education* and *management*. However, when we consider the sector distribution of all Hispanics and of Hispanics born abroad, (columns 3 and 4 of Online Appendix Table A3) we see a significant over-representation in the construction sector. Because of language and cultural commonalities, this extended group (and not just Puerto Ricans) can be a very important network in finding jobs in the short run after arrival. The construction sector was, therefore, particularly attractive for new Hispanic workers as, in Orlando, it employs a larger share of workers who do not speak English well relative to any other sector (11.8% do not speak English or do not speak it well). The majority of Puerto Rican residents are not fluent in English (only about a third speak English “well” according to the PRCS), hence a job in construction was likely more attainable in the short run. Additionally, the Bureau of Labor Statistics (2015) shows that in 2014 Hispanic workers in the U.S. were more likely to work in the construction sector than any other sector in the economy. Finally, we use the QWI data to look at employment in Orlando and demonstrate that, between Q2 2017 and Q2 2018, the construction sector saw substantially higher growth in the Hispanic share of employment than did any other industry (Online Appendix Figure A4). The share of Hispanic workers in the construction sector increased by almost 2 percent of employment, which was double that seen in any other sector.<sup>14</sup>

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<sup>13</sup> See appendix for further details on return migration to Puerto Rico from the mainland.

<sup>14</sup> While significant differences in specialization and occupations suggest caution should be exercised when aggregating all Hispanics in this context, the language commonality and the short-run nature of our analysis make it reasonable to think that these migrants found Orlando’s construction sector to be a quickly accessible and attractive source of jobs relative to other sectors.

### **III. Data and Methodology**

#### ***A. Overview***

We adopt a synthetic control approach to estimate the effects on Orlando's (log) employment, compensation per worker, and establishment counts. We analyze the local economy in aggregate, and also focus on three sectors: the construction sector which, as argued, received potentially the largest labor supply increase from the migrants' arrival; the retail trade sector; and the hospitality sector. As argued, retail and hospitality are non-tradable sectors most likely to have experienced a labor demand shock associated with the increased demand for local accommodations, hospitality services, and goods for purchase that was generated by the migrants.

To study potential effects on incumbent workers, we then analyze effects on the employment and wages of non-Hispanic workers, who are, as we show, very likely to be U.S. born. We also try to identify effects on workers who, in the short-run, may be more vulnerable to labor market competition from immigrants. Such workers are typified by low levels of education (high school or less) and relatively low pay. While immigrants from Puerto Rico did not have levels of schooling significantly lower than natives, in the short run they may have been willing to "downgrade" their job expectations to find work (for example, if their English language skills were not particularly strong).<sup>15</sup> In such a case, even with a similar education distribution among immigrants and natives, there may still have been greater competition for less-skilled jobs in the short run.

Finally, we look at changes in the number of local establishments as a proxy of the response of local investment, at least in the short-term. Firm-creation is an important mechanism of adjustment to immigration in the long run, but there has been very little work done studying the rapidity of its response in a local economy. While our data are limited and do not allow us to analyze firm entry or exit in response to the migration event (e.g. Mahajan (2021)), we see the number of firms as a

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<sup>15</sup>Using data from the ACS and looking at recent migrants from PR to the Orlando CZ between the years of 2015 and 2019, we see a steep decline in average English proficiency after the hurricane of about 40 percent.

rough but valid proxy for capital intensity.

## ***B. Data***

Our primary analysis is conducted using data from the Quarterly Census of Employment and Wages (QCEW), published by the U.S. Bureau of Labor Statistics. The QCEW is derived from the Unemployment Insurance (UI) accounting system in each state, and effectively covers 95%+ of all employed individuals from UI-reporting establishments. The data are available at the industry-by-county level down to the 6-digit NAICS level definition, and are observed monthly for employment and quarterly for earnings and establishments.<sup>16</sup> This provides near-complete coverage of employment and earnings. The QCEW provide monthly observations of employment, and quarterly observations of compensation and establishments. This permits quarterly observations of our derived compensation-per-worker variable. We focus on the period 2014 Q1 - 2018 Q3 for the quarterly-observed variables, and January 2014 - August 2018 for employment. This allows us to include three and a half years of pre-Hurricane data and one year of post-Hurricane data.

The analysis restricted to non-Hispanic and less-educated workers is conducted using data from the Quarterly Workforce Indicators (QWI). A product of the U.S. Census Bureau, the QWI data are fed by the Longitudinal Employer-Household Dynamics (LEHD) program, which (as with the QCEW) uses data from state UI accounting systems, as well as other sources. The QWI report employment and earnings of employees at the industry-by-county-by-quarter level, and disaggregate the data by education level and by ethnicity.<sup>17</sup> There are some drawbacks with the QWI relative to the QCEW: employment is only observed on a quarterly basis, and a few states are missing observations for some quarters of interest such that we lose close to 20 percent of our sample when looking one full year after the hurricane.<sup>18</sup> This later issue somewhat restricts the number of

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<sup>16</sup>For cells that are particularly small there may be “suppression” of data due to privacy reasons, but there are very few suppressions for larger NAICS 2-digit ‘Supersectors’.

<sup>17</sup>Cells are more likely to be suppressed as they become more focused on a particular group.

<sup>18</sup>States missing QWI observations for some or all counties include Arkansas, Maine, Minnesota, Mississippi, Missouri, New Jersey, Pennsylvania, South Dakota, Virginia, and Washington.

commuting zones that can be included in the donor pools for the QWI-based estimates, potentially making the full-year QCEW and QWI estimates not comparable. An additional drawback with the QWI is that, among less-educated workers, only non-youth individuals (aged 25+) are observed.

### ***C. Empirical Approach***

#### *1. Synthetic Control Estimator*

Our main econometric analysis uses the synthetic control estimator (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010, 2015) and (Abadie, 2021). Our treatment of interest is the inflow of Puerto Ricans to the city of Orlando, which began immediately after the sudden and unanticipated Hurricanes Irma and Maria devastated Puerto Rico. For each outcome of interest, we estimate a synthetic Orlando as a weighted average of untreated CZs in the “donor pool”, weighted to best match (minimize the differences with) the pre-treatment values of a set of predictor variables, including linear combinations of the period-specific pre-treatment outcomes. This method eliminates the ad-hoc choice of control units and generates an estimated counterfactual outcome path for Orlando in the absence of treatment. For each outcome of interest, the causal estimate of the treatment effect is the difference between the observed value in Orlando and that in the corresponding synthetic control.<sup>19</sup>

Formally, we observe values of an outcome of interest,  $Y_{j,t}$ , and a set of covariates of the outcome, for  $J + 1$  units (commuting zones), where  $j = 1$  is our treated unit (Orlando) and  $j = 2, \dots, J + 1$  units are the untreated units in the donor pool. Each unit is observed  $T$  total periods, indexed by  $t$ , with  $T_0$  total pre-treatment periods and  $T - T_0 > 0$  treated periods. For each  $j$  and  $t$  define  $Y_{j,t}^N$  as the potential outcome if  $j$  is *not* treated at  $t$ , and  $Y_{j,t}^I$  as the potential outcome if  $j$  is

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<sup>19</sup> Abadie, Diamond, and Hainmueller (2010) note that, under a linear factor model with perfect pre-treatment fit and a long-enough pre-treatment period, the synthetic control method yields unbiased estimates of the counterfactual outcome path even if unobserved, time-variant confounds are present. Ferman and Pinto (2021) show such bias is likely mild even if the pre-treatment fit is imperfect when using demeaned data. Thus, given our setup and conditional on good pre-treatment fit, our estimating strategy likely yields estimates relatively free of endogeneity bias even if the migrants’ choice to relocate to Orlando was not entirely exogenous.

treated at  $t$ . Under standard assumptions, the difference (or “gap”) identifies the treatment effect:

$$\alpha_{j,t} = Y_{j,t}^I - Y_{j,t}^N \quad (1)$$

The goal is to estimate the dynamic path of treatment effects,  $\{\alpha_{j,T_0+1}, \dots, \alpha_{j,T}\}$ . As only  $j = 1$  is treated and  $Y_{1,t}^I = Y_{1,t}$  is observable  $\forall t > T_0$ , we need only estimate  $Y_{1,t}^N \forall t$ . The synthetic control estimator for  $Y_{1,t}^N$  is a weighted sum of the outcomes values for the non-treated units:

$$\hat{Y}_{1,t}^N = \sum_{j=2}^{J+1} \hat{w}_j Y_{j,t} \quad \forall t \quad (2)$$

We specify a set of carefully selected covariates and linear combinations of the pre-treatment outcome variable values, which comprise the predictor variables. Given a set of non-negative weights,  $v_1, \dots, v_k$ , on the  $k$  predictors,<sup>20</sup> which determine their relative importance in predicting  $Y_{1,t}^N$ , the weights  $\hat{\mathbf{W}} = (\hat{w}_2 \dots \hat{w}_{J+1})'$  are selected to minimize the distance Orlando and the untreated donor pool CZs during the pre-treatment period. That is,  $\hat{\mathbf{W}}$  is selected to minimize:

$$\left( \sum_{h=1}^k v_h (X_{h,1} - w_2 X_{h,2} - \dots - w_{J+1} X_{h,J+1})^2 \right)^{1/2} \quad \text{s.t.} \quad \sum_{j=2}^{J+1} w_j = 1, \quad w_j \geq 0 \quad \forall j \in \{2, \dots, J+1\} \quad (3)$$

where  $X_{h,j}$  is the value of predictor  $h$  in CZ  $j$ , and the non-negativity constraint prevents extrapolation (Abadie, Diamond, and Hainmueller, 2015; Abadie, 2021).

This permits calculation of the  $\hat{Y}_{1,t}^N$ , and finally of the estimated treatment effects:

$$\{\hat{\alpha}_{1,T_0+1}, \dots, \hat{\alpha}_{1,T}\} = \{(Y_{1,T_0+1} - \hat{Y}_{1,T_0+1}^N), \dots, (Y_{1,T} - \hat{Y}_{1,T}^N)\}$$

As it is important that units in the donor pool are not affected by the treatment (see, for example, Cao and Dowd (2019)) we restrict the donor pool by excluding CZs that received any significant number of Puerto Rican evacuees (10 or more Puerto Rican Irma- or Maria-associated FEMA applications per 100,000 population). When analyzing sector-specific outcomes, to reduce the possibility of interpolation bias (that is, to avoid including in the synthetic control some CZs which may match well on certain predictor variables but poorly on others), we further restrict the donor

<sup>20</sup>Given the relatively small number of pre-treatment periods, we follow the advice of Abadie, Diamond, and Hainmueller (2015) and estimate the  $v_h$  weights using the regression-based method.

pool to include only those CZs at or above the 75<sup>th</sup> percentile of sector-specific employment levels. This ensures we are only allowing the synthetic Orlando (for each sector of focus) be comprised of other CZs with large pools of workers in the same sector.<sup>21</sup> For each outcome variable we also drop any commuting zone for which the outcome is not observed in every period.

This yields 148–170 commuting zones (depending on the sector, and including Orlando) for which data is consistently available at a monthly or quarterly frequency for the time period before and after Hurricane Maria (September 2017) and which meets our qualification thresholds for inclusion (see [Figure 1](#)).<sup>22</sup> These criteria exclude every CZ which borders Orlando, and nearly every CZ in Florida, which also minimizes the risk of any treatment spillover on the control group. Finally, to increase homogeneity and comparability across commuting zones, we cleaned each outcome of seasonal variation and the local intercept (details in the Online Appendix).

[[Figure 1](#) about here]

The first *treated* period,  $T_0 + 1$ , for employment is September 2017 or quarter Q3 2017, depending on the frequency with which the particular outcome variable is observed. The pre-treatment period starts at the beginning of 2014, when the recovery from the Great Recession had firmly taken root and includes a reasonably long period of sustained expansion of US labor markets.<sup>23</sup> Depending on the dataset, either QCEW or QWI, we include either  $T_0 = 44$  or  $T_0 = 14$ , respectively, pre-treatment months (or quarters) and consider effects up to one half year and one full year after the hurricanes hit Puerto Rico. For each outcome of interest we specify  $K = 11$  predictor variables.<sup>24</sup>

<sup>21</sup> As Abadie (2021) notes, “Including in the donor pool units that are regarded by the analyst to be unsuitable controls because of large discrepancies in the values of their observed attributes... or because of suspected large differences in the values of the unobserved attributes... relative to the treated unit is a recipe for bias.”

<sup>22</sup> 128–137 CZs for the 12-month QWI estimates.

<sup>23</sup> One of our robustness checks extends the pre-treatment period back to the beginning of 2013.

<sup>24</sup> The predictor variables we include are: average quarterly local construction and hospitality employment, each as proportions of aggregate local employment. To match the economy of the synthetic Orlando with that of the actual Orlando and pre-treatment values of the outcome at 6-month intervals from 2014 to 2015, and at quarterly intervals from 2016–Q2 2017. We ensure

Figure 1 shows the CZs included in the donor pool when analyzing employment in aggregate and in the construction, retail and hospitality sectors. Figure 2 shows those CZs that receive positive weight in the synthetic control for the employment and earnings outcomes, in aggregate and in the construction sector. Note that the Las Vegas, NV CZ, the Reno, NV CZ, and some CZs in southern California often receive large positive weights. This is reasonable as their economies, which strongly rely on tourism and construction and have large numbers of Hispanic workers, broadly resemble the economy of Orlando.<sup>25</sup>

[Figure 2 about here]

## 2. Inference

Once we have estimated the treatment effects, a key question is whether they are significantly different from zero. Hypothesis testing using a synthetic control approach comes with challenges, as the synthetic control estimator does not produce standard errors, and large-sample inferential approaches are not appropriate. Most of the proposed approaches involve the construction of a test statistic based on some form of falsification test.<sup>26</sup> Abadie, Diamond, and Hainmueller (2015) propose using the “ratio of the treated-period mean squared prediction error (MSPE) to the pre-treatment-period MSPE”, called RMSPE. One substantial benefit of a test statistic based on the RMSPE is that, by construction, post-treatment deviations from the null are normalized by the pre-treatment fit, such that large post-treatment deviations are not attributed undue significance if

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some pre-treatment outcomes are excluded, as Kaul et al. (2015) caution that including *all* pre-treatment outcomes as predictors will result in certain zero  $\hat{v}_k$  weights for some covariates that may be important predictors. The existence of sparse solutions to Equation (3) with at most  $K + 1$  strictly positive weights  $\hat{w}_j$  follows from Carathéodory’s theorem, while uniqueness obtains with a maximum of  $K$  strictly positive weights provided Orlando does not fall within the convex hull of the donor pool units, and provided the columns of the predictor matrix are in general position (Abadie and L’Hour, 2019).

<sup>25</sup>Online Appendix Table A4 lists the positively-weighted CZs for these outcomes.

<sup>26</sup>The literature on conducting inference with synthetic control estimators is relatively young and rapidly evolving. See for instance Abadie, Diamond, and Hainmueller (2015); Doudchenko and Imbens (2016); Hahn and Shi (2017); Ferman and Pinto (2017); Chernozhukov, Wuthrich, and Zhu (2017); Firpo and Possebom (2018); Abadie and L’Hour (2019)



the pre-treatment fit is poor. Firpo and Possebom (2018) find the RMPSE test has several desirable properties.

However, Hahn and Shi (2017) argue that permutation tests like the RMSPE may suffer from incorrect statistical size, and suggest the Andrews (2003) end-of-sample instability test as an alternative. Chernozhukov, Wuthrich, and Zhu (2017) propose a *moving block* permutation test which may be ideal but is not the current standard. These tests are parsimoniously described the Appendix of Wiltshire (2021). We apply all three tests to our estimates and present the corresponding  $p$ -values in Table 2. Table A4 of the Online Appendix presents the Andrews and moving block  $p$ -values for our secondary estimates. As the RMPSE is often the most conservative of these tests (e.g. Table 2 shows the RMSPE  $p$ -value is larger than the Andrews  $p$ -value in 63% of cases and larger than the Moving Block  $p$ -value in 92% of cases) and is the current standard for synthetic control inference, we mostly base our claims of statistically significant treatment effects on RMSPE  $p$ -values, and report on RMSPE  $p$ -values for our secondary estimates in Table 3 and for our robustness checks in Online Appendix Tables A4-A6.

The RMSPE  $p$ -value is constructed by repeating the synthetic control estimation procedure for each commuting zone in our donor pool, effectively conducting falsification or ‘placebo’ tests by reassigning treatment to each of the  $j \in \{2, \dots, J+1\}$  *untreated* CZs in our donor pool to estimate  $\hat{Y}_{j,t}^N \forall t$ . For each  $j \in \{1, \dots, J+1\}$  we then calculate the summary statistic:

$$RMSPE_j = \frac{\sum_{t=T_0+1}^T (Y_{j,t} - \hat{Y}_{j,t}^N)^2 / (T - T_0)}{\sum_{t=1}^{T_0} (Y_{j,t} - \hat{Y}_{j,t}^N)^2 / T_0} \quad (4)$$

We construct our test statistic as a  $p$ -value based on the empirical distribution of these  $RMSPE_j$ :

$$p = \frac{\sum_{j=1}^{J+1} \mathbb{1}[RMSPE_j \geq RMSPE_1]}{J+1} \quad (5)$$

If the deviations between observed post-treatment outcomes and the synthetic control relative to the pre-treatment fit are large enough in Orlando relative to the distribution of differences from our placebo tests, our  $p$ -values will be small and we will reject our null hypothesis of no effect.<sup>27</sup>

<sup>27</sup> We calculate the RMSPE  $p$ -values for our estimates of the ATEs using the three- and five-most

## IV. Results

### A. *Effects on All Workers*

Results for employment are illustrated in the four panels of [Figure 3](#). Each panel of the figure plots the observed path of (de-seasonalized, logarithm of) employment in Orlando against that of its synthetic control, first in aggregate (Panel A), then for construction (Panel B), retail (Panel C), and hospitality (Panel D). We set the value to zero at the beginning of our pre-treatment period (January 2014). The month in which the hurricane hit, September 2017, is the shaded area. The six months following the shaded area are those in which Orlando experienced the largest inflow of people from Puerto Rico fleeing Hurricane Maria. If they caused any significant short run effect on Orlando’s labor market relative to the synthetic control, it should be visible to the right of the shaded area. The post-September 2017 distance—or “gap”—between the line representing Orlando and that of its synthetic control represents our estimates of the causal effect of the inflow of Puerto Ricans as a result of the Hurricane. These calculated gaps are shown as percentages in [Figure 4](#), as the solid black line in each panel.

Three facts emerge from inspection of [Figures 3 and 4](#). First, for each of our outcomes, the pre-Hurricane match between Orlando and its synthetic control is remarkably good. This implies that the combination of commuting zones constituting our synthetic controls mimics well the short run fluctuations and long run trends in Orlando employment before September 2017.<sup>28</sup> Second, [Figure 3](#) shows that, for aggregate as well as sector-specific employment, Orlando realized positive treated CZs by constructing a distribution of 1,000 randomly sampled (weighted) averages of the placebo gaps, as described in [Wiltshire \(2021\)](#).

<sup>28</sup> [Figure 2](#) shows the commuting zones included in each particular donor pool. In particular, nine donor pool CZs were assigned positive weights for Orlando’s aggregate deseasonalized log employment synthetic control. These donor CZs are, in ascending order of weights: Fort Walton Beach-Pensacola, FL (2.7%); Fresno-Visalia-Tulare-Parterville, CA (5.3%); Las Vegas, NV-AZ (6.5%); Boise City, ID (6.7%); El Paso, TX-Las Cruces, NM (7%); Nashville, TN (11%); Provo-Orem, UT (15.3%); Fayetteville-Springdale-Rogers, AR (18.6%); and Gainesville, GA (26.9%). [Online Appendix Table A4](#) provides these lists for a broader set of synthetic controls. Comparing this map with [Figure A2](#) in the [Online Appendix](#), we can see that none of the donor CZs received any meaningful number of evacuees.

employment effects as a result of this migration event.<sup>29</sup> Third, this positive employment effect is largest and most clearly seen in the construction sector, and begins a few months after the hurricane hit Puerto Rico. One year after the hurricane hit, Orlando’s construction employment was about four log points (4 percent) larger than its synthetic control.

[Figures 3 and 4 about here]

Treatment effects from Figure 4 are quantified in the first two columns of Table 2. The entries are the treatment effects 6 months (column 1) and 12 months (column 2) after September 2017. The table also shows the RMSPE statistics and their  $p$ -values, as well as the Andrews and the moving block  $p$ -values.

The treatment effect on aggregate employment in Orlando, 12 months after the hurricane, is 0.4 percent and significant at the 5% level using any of our three  $p$ -value calculation procedures.

[Table 2 about here]

The construction sector experienced the largest and most significant increase in employment, consistent with this sector having received a supply “shock” from the inflow. We estimate a 1.5 percent increase in construction employment 6 months into the treated period, although this estimate is not quite significant at the 10 percent level using our most conservative  $p$ -value calculation. The 12-month estimated treatment effect, however, corresponds to 4 percent increase in construction employment and is significant at the 1% level.

Looking at the retail sector, employment did significantly increase—slowly at first, with a 0.3 percent increase seen after 6 months and a 0.9 percent increase seen after 12 months (RMSPE  $p$ -values of 0.04 and 0.06, respectively). We also estimate a 1.2 percent increase in hospitality sector employment 12 months after treatment, though here significance is only seen using the Andrews and moving block  $p$ -values (0.02 and 0.05, respectively).

Focusing next on earnings, the panels of Figure 5 show the plots relative to (de-seasonalized, natural logarithm of) per-worker earnings for Orlando relative to synthetic control, and follows a

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<sup>29</sup>This is more obviously shown by the black lines in Figure 4.

similar structure to [Figure 3](#). Our estimates of treatment effects on earnings, 6 and 12 months after the hurricane, are reported in columns 3 and 4 of [Table 2](#). The table also shows the significance of the estimates from each of our three  $p$ -values.

Interestingly, in spite of the significant increase in aggregate employment driven by the evacuees' inflow, earnings per worker was stable in aggregate over the entire study period (+0.3%, not significant). When looking at the sector-level, we do not find any negative treatment effect on earnings per worker in the sectors considered above. In the construction sector, the average compensation per worker actually grew by 3.3 percent (only marginally significant and imprecisely estimated). This average positive effect include the immigrants themselves who could have had higher skills than incumbents, and is not very precisely estimated. A more accurate estimate of the effect on incumbent (non-Hispanic or less educated) wages is in the next section and will turn out to be negative and more precisely estimated.

[\[Figure 5 about here\]](#)

The hospitality sector also experienced a significant effect on earnings per worker (using the RMPSE  $p$ -value, and due especially to the particularly good fit in the pre-treatment period). Wages were 1.4 percent higher 12 months after the migration event (RMSPE  $p$ -value of 0.03).<sup>30</sup> The estimated effects on retail earnings per worker are positive but small, and not significant.

Finally, columns 5 and 6 of [Table 2](#) show the estimated treatment effects on numbers of establishments. In aggregate, the treatment effect on the number of establishments amounted to a positive, though not statistically significant, increase of 0.5 to 0.6 percent. Similarly, for each of our sectors of interest we have positive effects around 1 percent, but no statistical significance. This is suggestive (but not conclusive) evidence that even as quickly as a few months after the arrival of the evacuees, firms may have started to open new local establishments to take advantage of higher labor supply and consumer demand. However, noise in the pre-hurricane period and the relatively short post-hurricane period prevent us from being able to produce robust and conservative statisti-

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<sup>30</sup> We estimate earnings per worker grew by a statistically significant 0.1 percent 6 months after treatment began, but view this estimate as too small to be considered economically significant.

cally significant results on this outcome. For example, the notable dip in Orlando establishments in mid-2015 in Panels B and D of [Figure 6](#) eliminates the significance of the RMPSE  $p$ -values. On the other hand, if we instead consider the *moving block*  $p$ -value for construction establishments, the treatment effect of around 1% is significant ( $p$ -value of 0.06).

Overall, we find evidence that the migration event induced by Hurricane Maria caused employment growth in Orlando, in aggregate and also within sectors most likely to be affected by labor supply and demand shocks.<sup>31</sup> We find no evidence of negative effects on per-worker earnings in aggregate or in any sector we study, and we find positive but non-significant effects on establishment numbers in aggregate and every sector we study. We next consider whether natives or less-educated workers may have been negatively impacted despite these aggregate results.

### ***B. Effects on Non-Hispanic and Less-Educated Workers***

The Quarterly Workforce Indicators (QWI) allow us to repeat our analyses for subgroups of workers stratified by ethnicity or by educational attainment. This allows us to test whether the migration wave into Orlando caused any negative impacts on either native workers or less-educated workers.

We begin by focusing on native workers. While we are not able to directly observe worker nativity, we can approximate native workers quite well by focusing on ethnicity. Using ACS data we calculate that, five years prior to the hurricane, 88 percent of “non-Hispanic” workers were “native”, meaning they were born in one of the 50 states in the U.S. We also observe that the overwhelming majority of people from Puerto Rico (99%) identify as Hispanic. Hence non-Hispanic workers serve as a good proxy for US natives, and we can be certain that group will not include the evacuees when observed in the period after the Hurricane.

[Table 3](#) presents the estimated treatment effects on log employment (columns 1 and 2) and log

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<sup>31</sup>Online Appendix Table A6 also presents results of an analysis on the Transportation and Warehousing sector, in which Puerto Ricans are also over-represented (see Online Appendix Table A3). While we see positive 12-month estimates of the effect on employment and establishments, and negative 12-month estimates of the effect on per-worker earnings, none of the estimate are significant. We thank an anonymous referee for suggesting this analysis.

compensation per worker (columns 3 and 4), 6 and 12 months after Maria. We do not find any evidence of a negative effect on non-Hispanic employment either in aggregate or in the sectors considered. Rather, we estimate a significant +0.8 percent increase in aggregate non-Hispanic employment 12 months after the Hurricane, while the sector-specific non-Hispanic employment estimates are also positive but not generally statistically significant (the construction employment estimate of +0.7 percent is significant according to the moving block  $p$ -value only).

[Table 3 about here]

Focusing on non-Hispanic per-worker earnings, we estimate a very small and non-significant increase in the aggregate, both six and 12 months after the hurricane. We also find a significant negative -2.5 percent impact on construction as well as a positive +2.1 percent impact on retail sector earnings (the latter is significant according to two of our three tests).<sup>32</sup>

We also consider workers with a high school degree or less, as these workers are often considered to be in competition with immigrants for relatively low skilled jobs (Borjas, 2003), and as their wages have declined in recent decades (Autor, Katz, and Kearney, 2008).<sup>33</sup> In the retail and hospitality sectors in Orlando, 49 percent of workers had a high school degree or less as of 2016 (the year before Hurricane Maria hit Puerto Rico), while in the construction sector this share was 60 percent.<sup>34</sup> Columns 5-8 in Table 3 present our results for this group. We find no evidence of a significant impact on overall employment or per-worker earnings in this group 6 months after the hurricane. However, 12 months after the hurricanes we see a significant 0.8 percent increase

<sup>32</sup>That is, according to the Andrews and moving block  $p$ -values (0.07 and 0.00, respectively, identical to those for the construction estimate) reported in Online Appendix Table A5.

<sup>33</sup>We additionally estimate the impact specifically on workers with less than a high school education, to explore whether the estimates for less-educated workers mask particularly large effects for the least-educated in this group. We thank an anonymous referee for suggesting this. The results, presented in Online Appendix Table A7, show this is not the case. The estimated effects on these least-educated workers are highly comparable to those for less-educated workers as a whole, while the negative estimated effect on per-worker earnings 12 months after the hurricane is no longer significant.

<sup>34</sup>Our analysis here is essentially limited to *prime-age* workers with a high school degree or less, as the QWI do not observe education levels of workers under age 25.

in aggregate employment for less-educated workers ( $p$ -value of 0.04) and a 1.9 percent significant decrease in their per-worker earnings. Less-educated construction employment increased by 1.7 percent, with the effect significant according to the Andrews and moving block  $p$ -values. Interestingly, the wage effects for less-educated workers in specific sectors range from a positive 1.2 percent effect for retail workers to a 1.5 percent decline in the construction sector ( $p$ -values of 0.02 and 0.06, respectively).<sup>35</sup> The estimate on hospitality earnings is positive 1.3 percent after 12 months, but not significant according to the RMSPE or the Andrews  $p$ -values.

Summarizing our results for the non-Hispanic and less-educated worker subgroups (Table 3): first, the large flow of evacuees from Puerto Rico into Orlando during the post-hurricane period led to local increases in aggregate employment for each subgroup. Second, we find small positive effects of on construction employment for each subgroup—especially less-educated workers—as well as positive but not significant employment effects on retail and hospitality employment for each subgroup. Third, we find small negative effects on per-worker earnings for both subgroups in the construction sector—which saw the largest increase in labor supply—and balancing positive effects on per-worker earnings in retail and hospitality.

### *C. Interpretation and Discussion of Results*

Putting together all our results, we find evidence that this large Hurricane-induced flow of migrants into Orlando grew the local economy, with positive and significant impacts on employment and no negative impacts on aggregate per-worker earnings. These results hold overall, and also for native workers, while the aggregate less-educated employment also increased.

Our sector analysis reveals that by far the largest increase in employment was seen in the construction sector (+4.0%), which at most saw a +0.7% increase in non-Hispanic employment. This strongly indicates that growth in construction employment was almost entirely driven by the labor supply shock which these new migrants represented. To put this in context, a 4 percent

<sup>35</sup>Positive and significant effects in construction are already evident 6 months after the Hurricane.

increase in construction employment represent 80 percent of these migrants' employment if their participation patterns were similar to those of the Puerto Ricans already in Orlando.

We also find a short-run decrease in native (non-Hispanic) construction wages by 2.5 percent in response to the 4 percent increase in construction employment. This would imply a partial labor demand elasticity in the construction sector of -0.6, which is right in the range of estimated labor demand elasticity, usually between -0.3 and -1. While some studies argue that a large and negative aggregate elasticity of wage to immigrants around -1 is plausible (Borjas and Edo, 2021), we do not find such a large value even when restricting the analysis to the construction sector, which was likely most affected by the labor supply shock. Additionally, we find positive wage effects in the hospitality and retail trade sectors, which can be considered as effects from positive labor demand shocks in these sectors. In a broader and more macro context, these cross-sector effects can also be seen as the result of economy-wide complementarities between native and immigrant labor. As explained in Ottaviano and Peri (2012), if immigrants perform jobs/tasks that are different from natives' tasks, and if production requires combining those tasks, then the increase in supply of one of type of worker increases demand for the other group.<sup>36</sup>

We also find no significant effects on employment of non-Hispanic or less-educated workers in construction, retail, or hospitality (though the point estimates are all positive). Small positive and non-significant point estimates suggest, again, no crowding-out of native workers. This is an important result, which significantly reduces the concerns expressed in Borjas and Edo (2021) linked to significant non-employment effect of immigration on incumbents. Large and selective exit from employment might additionally conceal negative wage effects on incumbents (through composition changes), but that channel does not seem active or significant in this case.

While retail and hospitality employment grew much less than construction—respectively +0.9%

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<sup>36</sup> We also find significant and positive 12-month effects on per-worker earnings in retail and hospitality for those with some college or more education (Online Appendix Table A15). These workers are more likely to be managers and to benefit from “crowding-in” effects, so this further supports the complementarity story. We thank an anonymous referee for suggesting this.



and +1.2%, implying a likely smaller supply shift in these sectors—many migrants required accommodations upon arrival <sup>37</sup> as well as food and other staple consumption goods. As a result, retail and hospitality establishments likely experienced an increase in consumer demand which could have increased their demand for labor. In line with this, we find increases in retail per-worker earnings for non-Hispanic workers (+2.1%) and less-educated workers (+1.2%), and a possibly significant increase in earnings for less-educated hospitality workers (+1.3%). While we cannot observe consumption directly, these findings in these sectors are highly consistent with increases in labor demand resulting from consumer spending by these newly-arrived migrants.

The aggregate findings of a muted wage response, and positive (if not quite significant) growth in establishment numbers are consistent with a short-run response in which workers fill existing capacity in the retail and hospitality sectors and possibly stimulate growth in establishments (as in Beerli et al. (2021)). Additionally, immigrants may increase local firm-creation by acting as entrepreneurs (as in Azoulay et al. (Forthcoming)). These effects are not usually considered in short run studies in the immigration literature such as Borjas (2013), Borjas and Edo (2021). These studies predict negative overall wage effects which are, as we have shown, only partial effects of immigration. The positive labor demand effects of having more local workers, however, are documented well in work from the urban economics field (e.g. Moretti (2010)). This literature finds that these positive labor demand effects are driven by increases in population density as well as increases in productivity or local multiplier effects. Here we argue that some of those effects may emerge already in the short run.

Lastly, comparing our results with those found for the Mariel Boatlift in Borjas (2017) and Peri and Yasenov (2019), the first striking difference is the smaller size and much smaller volatility of the effects estimated here. The inflow of immigrants in the Mariel case was much larger (equal to 8 percent of the labor force and 18 percent of less-educated workers). As the samples used in those studies were much smaller than in this analysis, especially in Borjas (2017), the estimates

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<sup>37</sup> <https://www.orlandosentinel.com/weather/hurricane/puerto-rico-hurricane-recovery/os-ne-study-puerto-ricans-florida-mental-health-20190201-story.html>

of the short-run wage effects on less-educated workers had standard errors in the order of 10 percent. Therefore even the very large negative effects found by Borjas (2017) (up to negative 10-20 percent) were very volatile, imprecisely estimated and sometimes insignificant. Peri and Yasenov (2019), using a larger sample of less educated incumbent workers, instead found short-run effects around -2 percentage points with standard errors of 4 percent. In our study, a 0.4 percent increase in employed immigrants generates a (not significant) 0.3 percent increase in earnings, indicating no significant negative aggregate effects. Focusing on less educated workers, especially in the construction sector we estimate a one to two percent decline in wages in response to four percent increase in employment. This is compatible with the range of short-run elasticity estimated in Borjas (2017). However, in other sectors we find zero impact on less-educated workers, which is more consistent with the results in Peri and Yasenov (2019). Overall, we can rule out negative aggregate elasticity of employment to immigrant labor of the magnitude found in Borjas (2017). For the construction sector, however, and for the less-educated in particular, our estimates are consistent with the elasticity found in Borjas (2017). This is a partial, supply-driven effect in our analysis, offset by (demand-driven) effects on other sectors, so that on average, as Peri and Yasenov (2019) we fail to find negative employment or average-earnings effects for incumbent workers.

#### ***D. Robustness Checks***

To test the robustness of our overall findings, we conduct several additional analyses. We begin by considering an alternative, but much more simplistic, method to conduct inference, adopted in Peri and Yasenov (2019). Specifically, for each outcome we estimate a synthetic control to identify the positively-weighted control units, and then perform difference-in-differences regression-based estimates considering Orlando as the only treated unit and the other units as control(s).<sup>38</sup> Results

<sup>38</sup> Abadie, Diamond, and Hainmueller (2015) note how regression-based estimates implicitly weight the control units in a way similar to how a synthetic control estimator does. The regression approach, however, does not constrain the weights to be non-negative; thus a regression-based approach allows extrapolation outside the support of the data. Additionally, failing to restrict the sample of donor pool units to match the pre-treatment characteristics of the treated unit (condi-

can be found in Tables A8 & A9 in the Online Appendix. In general, the regression results confirm the findings of the synthetic control method and, while they reveal a certain variability of the pre-hurricane differences between Orlando and its control, they do not show systematic and significant departures from our primary analysis.

For our second robustness check, we adopt an alternative hypothesis testing procedure implying that in our RSMPE and relative  $p$ -value calculations we correct for possible deviations between treatment and synthetic control at the time right before treatment. Specifically, we define our treatment effects as the difference between observed outcomes and synthetic control outcomes *minus* that difference in the period immediately prior to treatment. This has essentially the flavor of a difference-in-differences estimator on the treated and synthetic control units. That is, we define  $f_{0j} = Y_{j,T_0} - \hat{Y}_{j,T_0}^N \forall j$  and our alternative estimated treatment effects are then  $\hat{\alpha}'_{1,t} = Y_{1,t} - \hat{Y}_{1,t}^N - f_{01} \forall t$ . This subtracts from any estimated treatment the gap in  $T_0$ , defining the relevant statistic as the change in the gap between  $T_0$  and  $T$ . Using this statistic and evaluating the corresponding  $p$ -value (Table A10 in the Online Appendix) shows very similar significance levels as with the usual method. This indicates that there was no significant deviation between Orlando and control at the time of the shock.

Our third robustness check extends the length of the pre-treatment period. In our primary specification, the pre-treatment period begins in the first period of 2014 (quarter or month, as appropriate) and continues through to the period immediately prior to the hurricane. The reason we begin in 2014 was due to concerns that the impact and duration of the recovery from the Great Recession was heterogeneous across commuting zones and could affect pre-2014 trends. Still, extending the pre-treatment period to the beginning of 2013 helps test the stability of our results.

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tional on included covariates) increases the probability that the “parallel trends” condition will be violated. While this method allows a very simple and straightforward check of our estimates and is intuitive, the standard errors may not be correct, especially when we consider the “synthetic Orlando” as the only control unit in the regression. Moreover, any simple regression model with a single post-treatment period fails to capture any trend-changes post-treatment. For these reasons, we view our regression-based DD estimates with a deal of caution and only as additional evidence on the qualitative findings and robustness of our synthetic control approach.

We conduct this robustness check by extending the pre-treatment period to the beginning of 2013, de-seasonalizing the raw data over this period, and then implementing our estimating strategy as before. The results are presented in Online Appendix Table A11 and confirm our primary findings from [Table 2](#), showing very similar treatment effects for all variables considered.

For our fourth robustness check, we consider the role of Hurricane Irma, which hit Florida in September 2017, downing trees and power lines and causing other damage (although the damage was much less severe than that caused by Irma and Maria in Puerto Rico). The immediate local economic impact was almost mechanically negative as a result of Irma (lost working days) and is likely captured in the significant dip in Orlando's employment in September 2017, evident in [Figures 3, 4, and 5](#). It is theoretically possible that the aftermath would yield movement in economic activity in either direction (see, for example, Groen, Kutzbach, and Polivka (2020)). Damage to infrastructure, for example, could hamper economic activity for months, or there could be a boost to the economy as the result of rebuilding efforts. This motivates our attempt to isolate potential consequences of exposure to Irma. To do this, we consider a large commuting zone which received very few Puerto Rican migrants but which lay approximately the same distance from the path taken by Irma's eye as did Orlando: Jacksonville, FL.

Compared to the 3,972 relevant FEMA applications filed in Orlando, Jacksonville saw fewer than one-seventeenth (221) relevant FEMA applications. On a per-capita basis (around 17 applications per 100,000 population), Jacksonville saw less than a tenth of those seen in Orlando, which saw 178 applications per 100,000 population.<sup>39</sup> Any robust estimated labor market treatment effect in Jacksonville—particularly in the construction sector, which we view as the most likely to demonstrate any positive post-hurricane impact as a result of reconstruction efforts—is thus much more likely to measure the lingering impact of exposure to Irma rather than the impact of an inflow of Puerto Rican evacuees.

The gaps for aggregate employment and construction employment between treatment and syn-

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<sup>39</sup> We note this number of applications still disqualified Jacksonville from inclusion in our donor pools for the analyses above.

thetic control for Orlando and Jacksonville, respectively, are shown in Panels A and C of [Figure 6](#) (Table A12 of the Online Appendix quantifies the results), along with the placebo gaps for all donor pool units. Two things are clear from these figures. First, the employment gaps (vs synthetic control) in Jacksonville experienced very similar dips to those seen in Orlando in September 2017, when both labor markets were directly exposed to Hurricane Irma, for both aggregate employment and construction employment. This confirms the similarity of potential effects of Irma. Second, the employment gaps—both in aggregate and in the construction sector—6 and 12 months after the Hurricane hit are significantly larger for Orlando than for Jacksonville. While we observe some ups and downs in the gap for construction employment in Jacksonville both before and after Irma, we do not see anything comparable to the significant increase in Orlando (which also has more stable gap paths in the pre-Hurricane period). In Jacksonville the estimated treatment effect on construction employment 12 months after Irma is 2.5 percent, but the RMSPE is quite small (1.01), reflecting the substantial amount of noise in the period before the Hurricane relative to the period afterward. The associated RMSPE  $p$ -value, at 0.81, makes clear this point estimate is not in any way significant when compared to the associated placebo tests (nearly 81 percent of the placebo runs yielded a larger RMSPE).

Overall, Jacksonville seems a valid counterfactual of the impact of Irma on Orlando in the absence of the Puerto Rican migrant inflow.<sup>40</sup> These results show that the one-month dip in employment can be attributed to that event, while the one-year surge in employment in Orlando, especially in construction, should be attributed to the impact of the migrants.<sup>41</sup> We also plot the results for

<sup>40</sup> Notice, additionally, that Jacksonville represents a good check for potential statewide spillovers driven by state policy responses to the arrival of Puerto Ricans. An example is the emergency order of October 7th 2017 that suspended occupational licensing fees for Puerto Ricans in Florida, for one month. If employment effects derived from such policy, rather from the inflow itself, they would have affected Jacksonville as well. The fact that neither this nor other Florida cities experienced the employment growth in construction, implies that such measure was unlikely to have an impact on native employment.

<sup>41</sup> Jacksonville may not be a good counterfactual for the retail or hospitality sectors because several large retailers started operations here in the treatment period. Amazon opened a distribution center in September 2017 and another in October 2017, both of which were around 1,000,000

establishments in Orlando and Jacksonville in Panels B and D. While none of our synthetic control estimates on Orlando establishment numbers were significant, they were all positive (around 1 percent) and these Panels demonstrate how the DD estimates (averaged over the whole post-treatment period) were significant. They also show that Jacksonville had no apparent establishment response post-Irma, further supporting our claim of no positive bias from Irma in the Orlando estimates.

[Figure 6 about here]

Our fifth robustness check extends our analysis to analyze the average treatment effect (ATE) on the top-three and top-five most treated CZs as defined by per-capita FEMA requests. This addresses concerns that when considering only one treated unit some other events in the Orlando labor market could have produced the result. These top-five most treated CZs include (in descending order of treatment exposure) Orlando, FL; Lakeland-WinterHaven, FL; Springfield, MA; Daytona Beach, FL; and Tampa-St. Petersburg-Clearwater, FL. They together received 34% of the FEMA applications, compared to 20% for Orlando alone. Table A2 in the Online Appendix shows that each of these CZs were among the top ten destinations for Puerto Rican migration per-capita in 2015, and all but one were among the top ten destinations in the year directly preceding the hurricane. Hence the sudden outflow of Puerto Ricans, combined with pre-existing network density, would indicate these five (and three) CZs as most likely to receive migrants.

We use a stacked synthetic control estimator (Dube and Zipperer, 2015; Abadie, 2021; Wiltshire, 2021) to “pool” the estimates from the treated CZs and construct the ATEs, weighting the contribution of each treated CZ by per-capita FEMA applications. Figure 7 and Figure 8, as well as Online Appendix Table A13, present these ATEs. We see virtually the same patterns as when focusing on just Orlando: positive effects on aggregate employment, as well as employment and wage effects in construction and positive employment effects in both retail and hospitality.<sup>42</sup> We

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square feet, and opened a sorting center in September 2018; Ikea opened a store in November 2017; and Walmart opened a new Supercenter in June 2018 and began hiring in August 2018 for another new store which opened in November 2018.

<sup>42</sup> Conversely, the distributions of average placebo treatment effects tighten as the number of placebo donors in each placebo average increases.

note the one big difference from Orlando alone is in the hospitality sector: we now see a significant positive impact on hospitality employment, while the growth in hospitality earnings is only significant according to the Moving Block and the Andrews  $p$ -values. These results demonstrate that the shock to Orlando (by far the most-heavily treated CZ) was consistent with effects on top-treated areas, affirming our primary focus on Orlando. Moreover, these results suggest more broadly that we can consistently estimate the earnings and employment effects of immigration shocks even when they are not too large—that is, on the order of a fraction of a percent of the labor force.

[Figures 7 and 8 about here]

Finally, we consider the possibility that Orlando experienced a housing boom before September 2017 which continued into the treatment period, and whether such a boom might be driving our construction employment results. First, in Figure 9 we plot the (log) Zillow Home Value Index of all homes in Orlando and any of the MSAs approximately corresponding to commuting zones which receive positive weights in our construction employment synthetic control unit. We normalize this value to be equal to one in September 2017 for all units and plot the values through May 2020. We see that during the pre- and post- Hurricane periods Orlando's trend appears to be rather average and is in no way indicative of a housing price boom relative to the control units. We also estimate a difference-in-difference model similar to those mentioned earlier, using as the outcome the same (log) Zillow Home Value Index but restricting the treatment period through September 2018 for consistency with our 12-month estimates.<sup>43</sup> The regression results confirm the visual inspection and do not show any evidence that Orlando experienced a housing price boom in the periods before or after September 2017.

These results give us confidence that there were no secular trends or trend breaks in housing prices in the Orlando area, relative to cities in the construction employment synthetic control, before or around the period of Hurricane Maria. More broadly, the combined results of our robustness checks strengthen our confidence that our estimates reflect the un-confounded causal impacts of

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<sup>43</sup>Results can be found in Table A14 of the Online Appendix.

the inflow of Puerto Rican migrants.

[Figure 9 about here]

## V. Conclusions

In this paper, we study the short-run local economic impact of a large wave of Puerto Rican migrants who abruptly flowed into Orlando after Hurricanes Irma and Maria devastated Puerto Rico. Using high frequency county-level data covering over 95% of workers, we find the migrant inflow caused overall employment in Orlando to increase by 0.4 percent and construction employment to increase by 4 percent, 12 months after the inflow began. Given the number of these migrants that we estimate are attributable to the hurricanes and who went to Orlando, these employment effects are consistent with full absorption of the migrant wave, with no crowding-out effects, and with a large share of the migrants finding employment in the construction sector—which is easier than other sectors for Hispanic workers to access. We also find that, one year after the inflow began, retail-sector employment rose by nearly 1 percent, and per-worker earnings in the hospitality sector grew by 1.4 percent. We argue these effects were the result of labor demand growth in these sectors, which was a response to the consumer demand shock for goods and services generated by these new arrivals. Our results are broadly robust to a series of falsification tests and alternative estimation strategies. When we focus specifically on natives or less-educated workers, we find the effect of this migrant wave was to increase employment within each of these groups by 0.8 percent, with little effect on aggregate earnings per worker. We also find that per-worker earnings increased for non-Hispanic and less-educated retail workers, while earnings decreased for native and less-educated workers in the construction sector which absorbed the majority of the labor supply shock.

Our results support a story in which predominantly Spanish-speaking immigration wave constituted a positive shock to both local labor supply and local consumer demand, which spurred greater labor demand in the local economy. The new workers were absorbed by the local economy



without displacing native workers and without any significant overall negative effect on earnings. While we do find that there was some downward pressure on earnings for natives and less-educated workers in the construction sector, this was offset by increases in wages for these groups in both retail and hospitality. By identifying this balancing effect of wages across sectors as the result of a large migration event, we are able to provide a clearer picture of how immigrants can influence the local economy in the short run.

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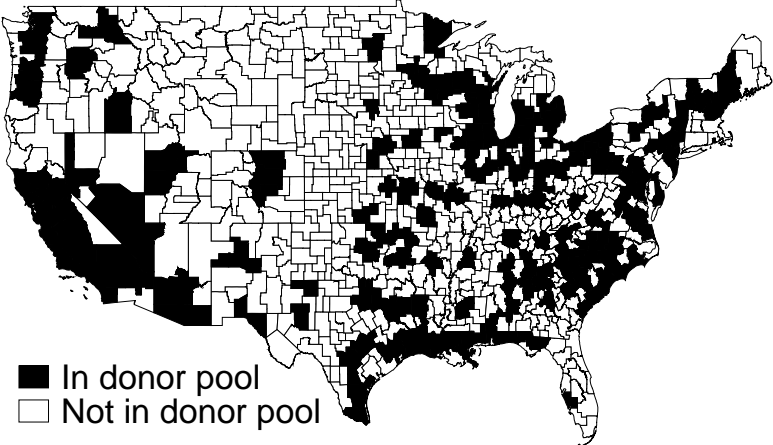
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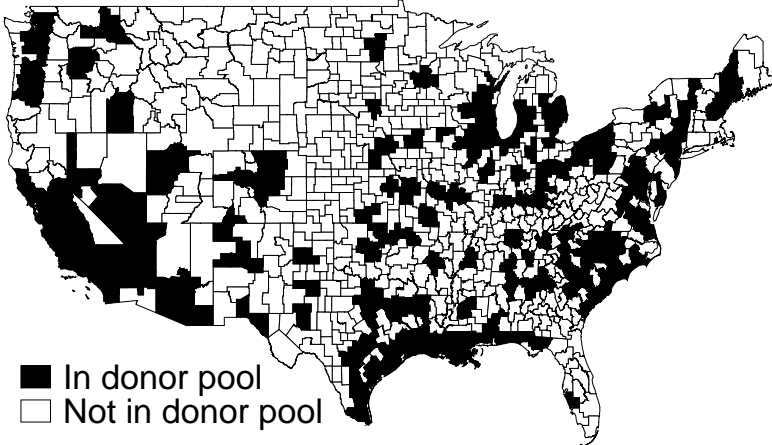
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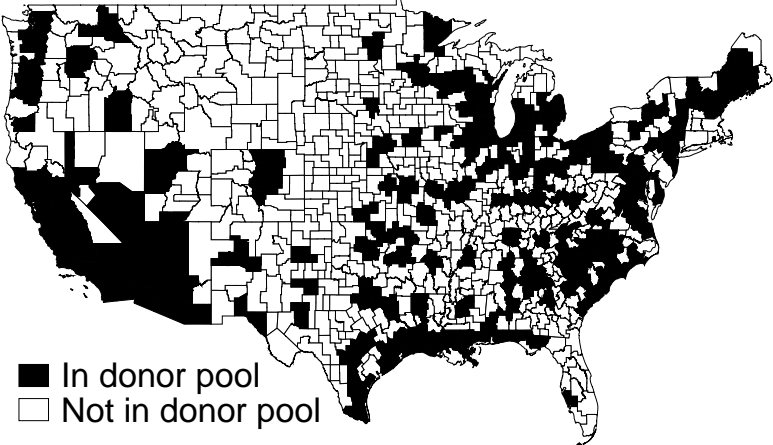
Panel A: Aggregate



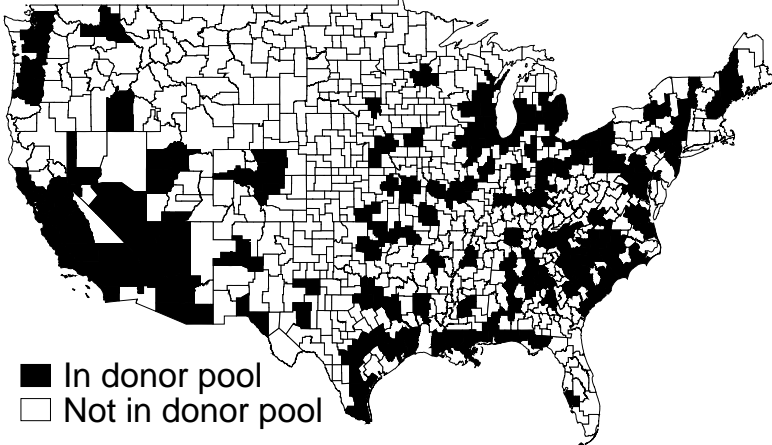
Panel B: Construction



Panel C: Retail



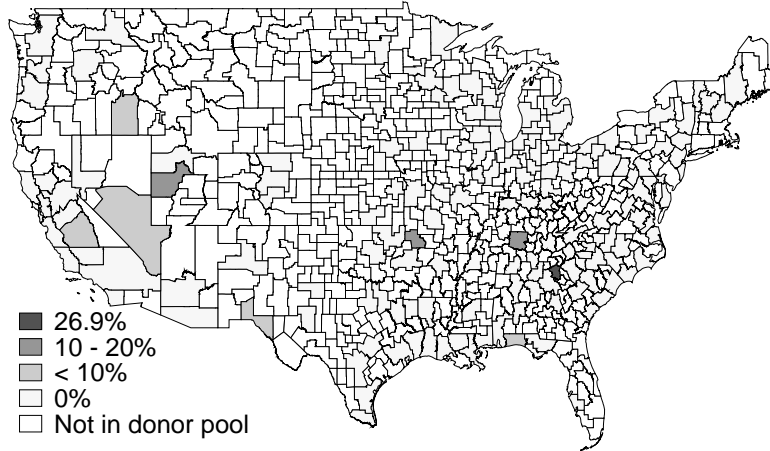
Panel D: Hospitality



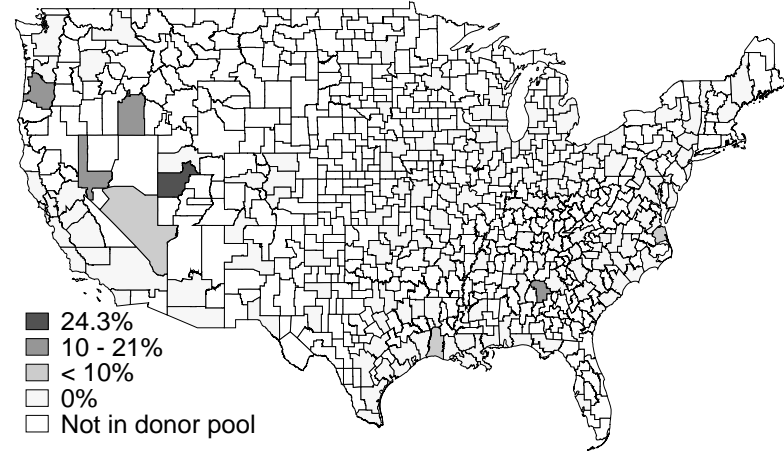
**Figure 1**  
*Synthetic Control Donor Pools, by Sector*

Notes: Each panel represents the entire donor pool of commuting zones for the synthetic Orlando, for the indicated outcome. The donor pools are restricted to exclude CZs which received more than .0001 FEMA applications per capita or were below the 75<sup>th</sup> percentile of industry-specific employment levels.

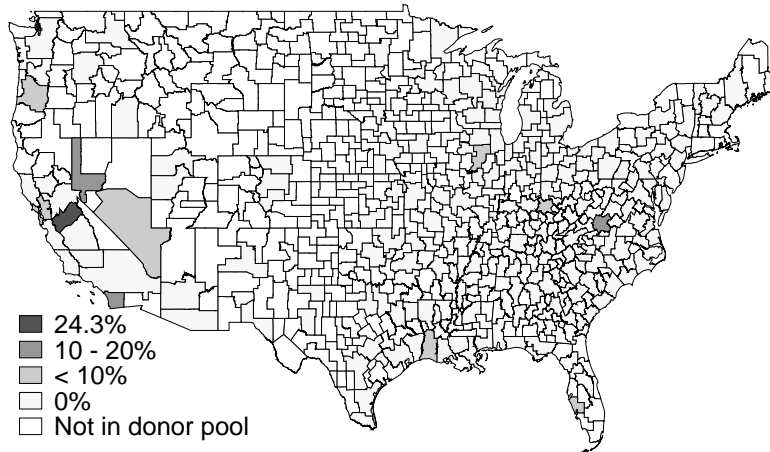
Panel A: Aggregate Employment



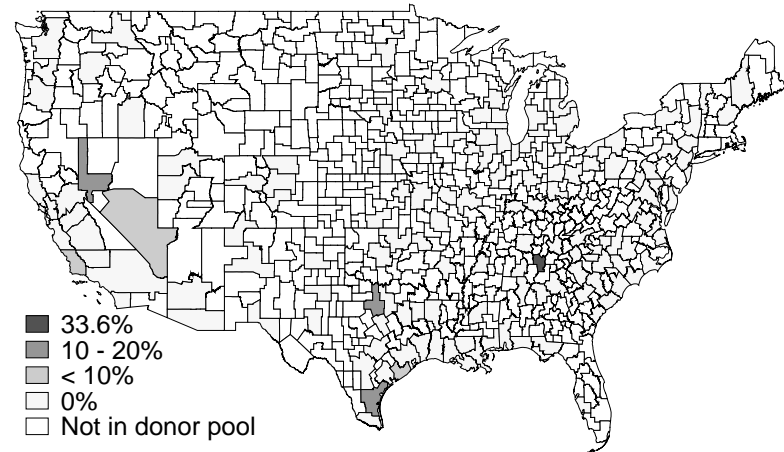
Panel B: Construction Employment



Panel C: Aggregate Earnings per Worker



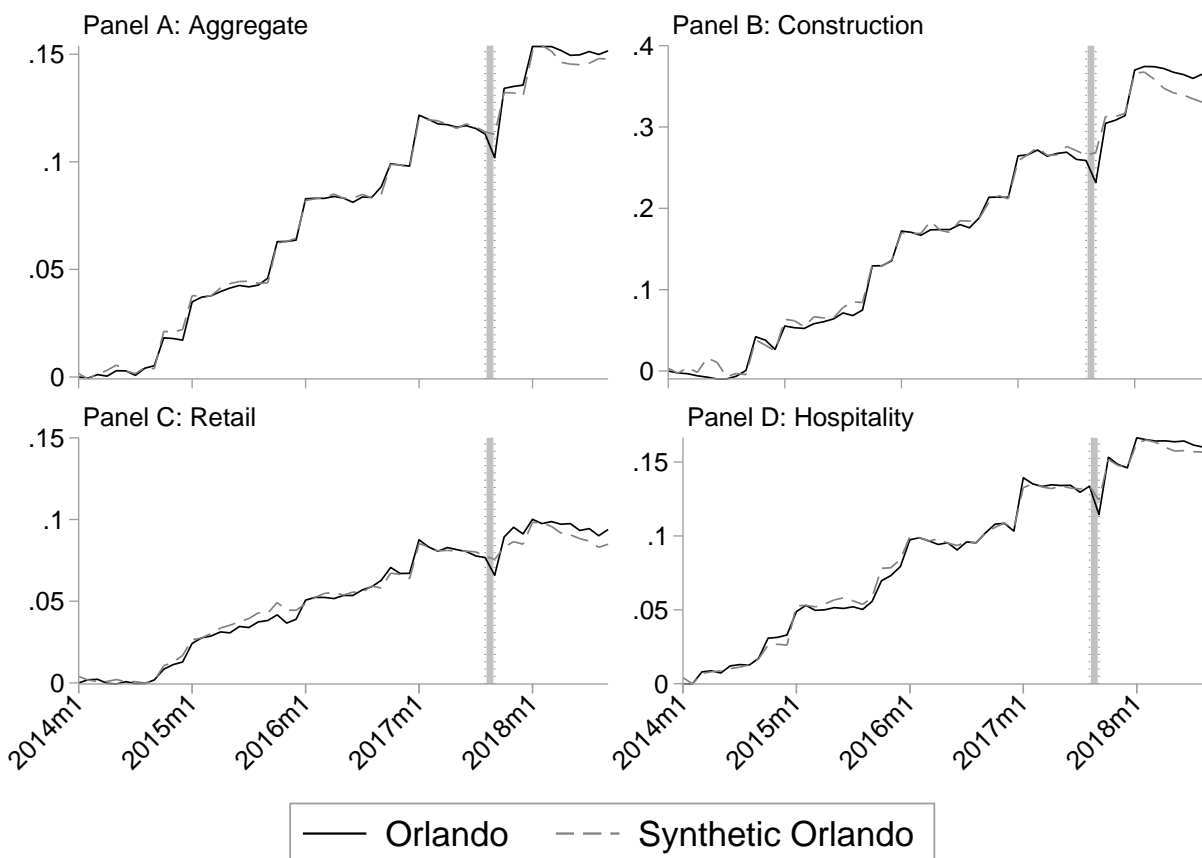
Panel D: Construction Earnings per Worker



**Figure 2**  
*Synthetic Control Donor Pools, by Weight*

Notes: Each panel represents the donor pool of the synthetic Orlando for the indicated outcome by its assigned weight. Shown for aggregate and construction-sector employment and earnings. The data, from the QCEW, are at a monthly frequency for employment and a quarterly frequency for earnings.

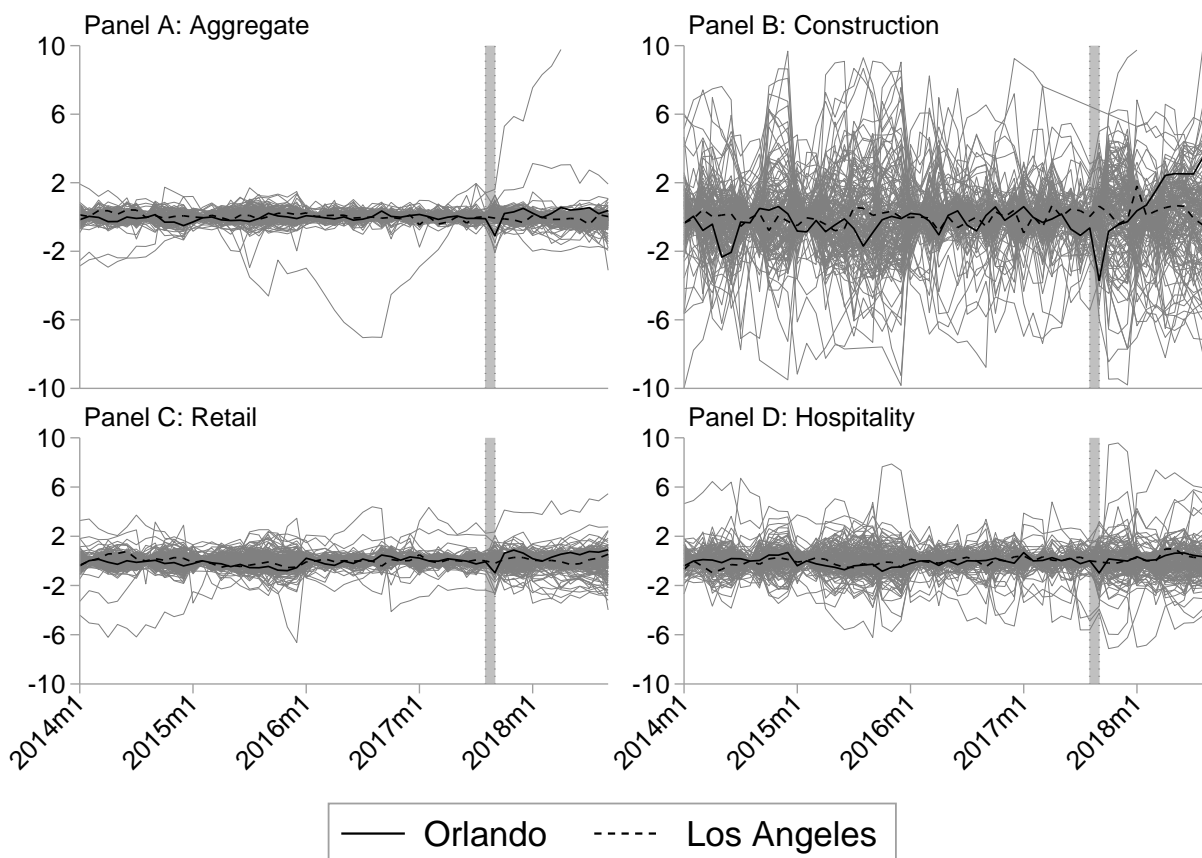




**Figure 3**

*Log Employment, Orlando vs Synthetic Orlando*

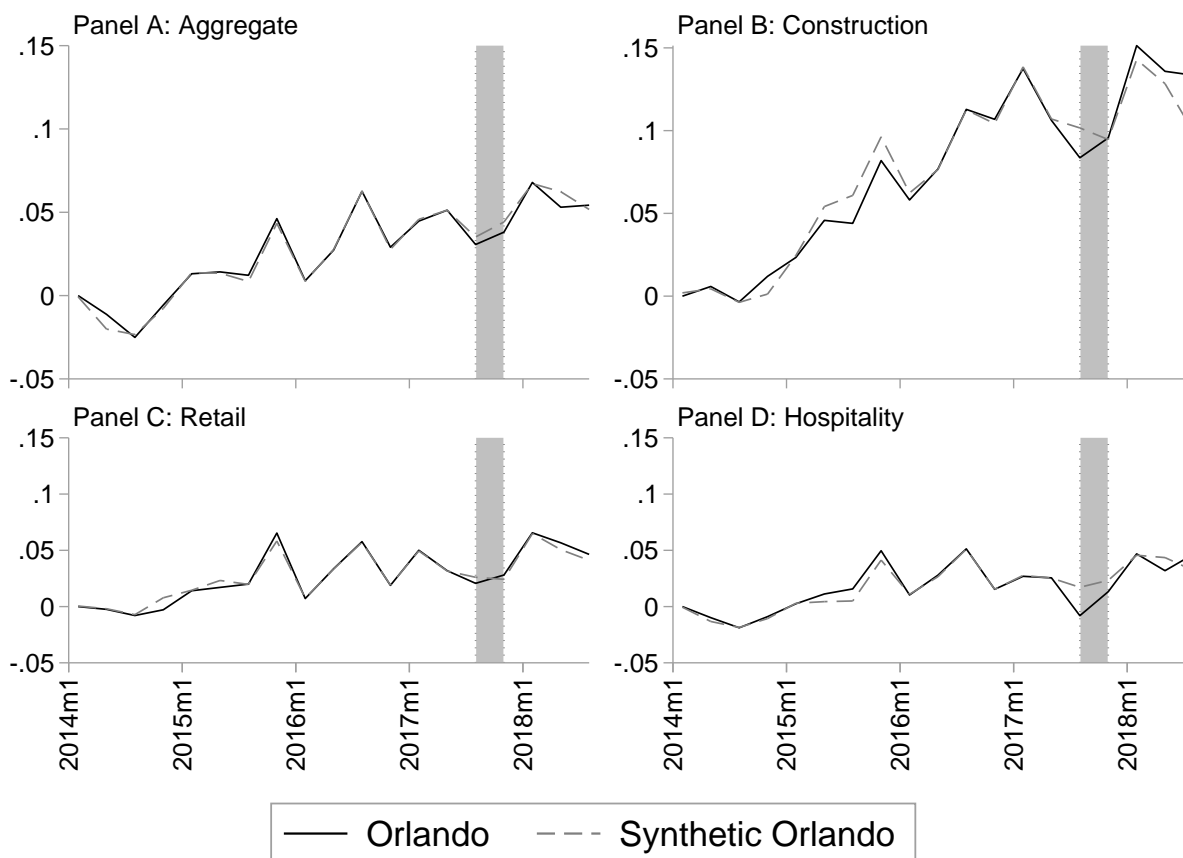
Notes: Each panel represents the residualized (after accounting for seasonal component and intercept) logarithm of employment in aggregate (Panel A), in construction (Panel B), retail (Panel C) and hospitality (Panel D) for Orlando and its synthetic control. The data, from the QCEW, are at a monthly frequency. The shaded period is September 2017, when Hurricanes Irma and Maria hit Puerto Rico and Hurricane Irma hit Florida.



**Figure 4**

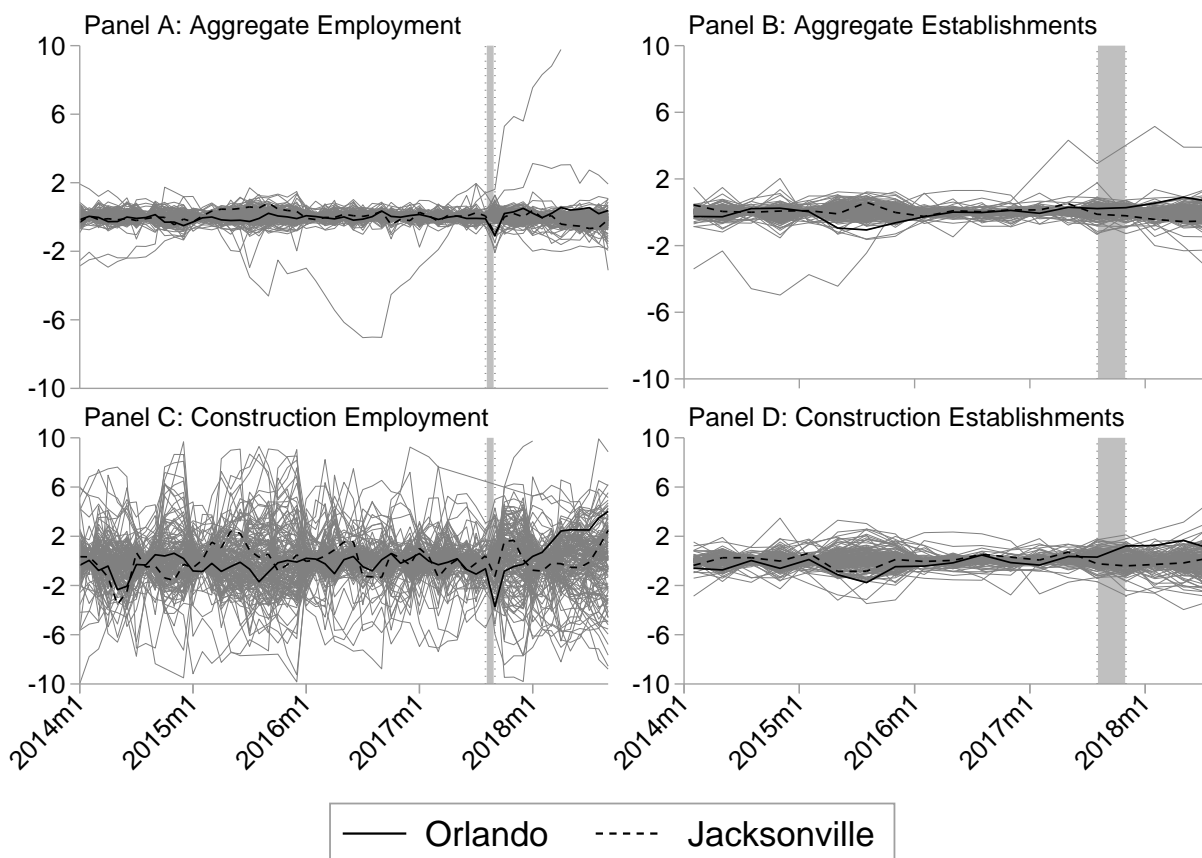
*Estimated Treatment Effects and Placebo Treatment Effects for log Employment in Orlando*

Notes: Each panel represents the % gap between the 'treated' unit and its control, for Orlando and for each commuting zone in the donor pool (placebo treatments), for each outcome's synthetic control. The variable plotted is  $100 \times$  the gap in residualized (after accounting for seasonal component and intercept) logarithm of employment in aggregate (Panel A,  $N = 165$ ), in construction (Panel B,  $N = 149$ ), retail (Panel C,  $N = 170$ ) and hospitality (Panel D,  $N = 148$ ). The data, from the QCEW, are at a monthly frequency. The shaded period is September 2017, when Hurricanes Irma and Maria hit Puerto Rico and Hurricane Irma hit Florida. The dark line represents Orlando, the dashed dark line represents Los Angeles



**Figure 5**  
*Log Earnings per Worker, Orlando vs Synthetic Control*

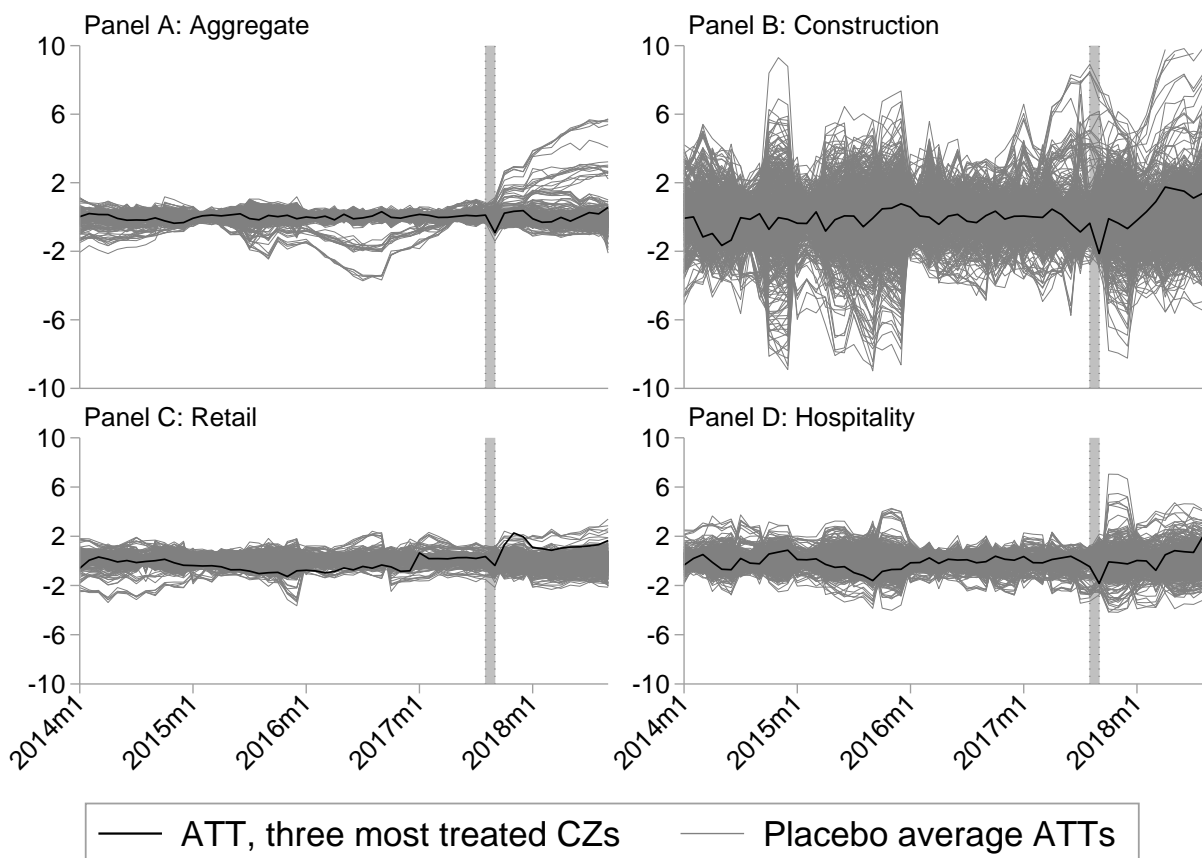
Notes: Each panel represents the residualized (after accounting for seasonal component and intercept) logarithm of earnings per worker in aggregate (Panel A), in construction (Panel B), retail (Panel C) and hospitality (Panel D) for Orlando and its synthetic control. The data, from the QCEW, are at quarterly frequency. The shaded period is Q3 2017, when Hurricanes Irma and Maria hit Puerto Rico and Hurricane Irma hit Florida.



**Figure 6**

*Test for Impact of Hurricane Irma on Orlando Treating Jacksonville as Counterfactual Orlando*

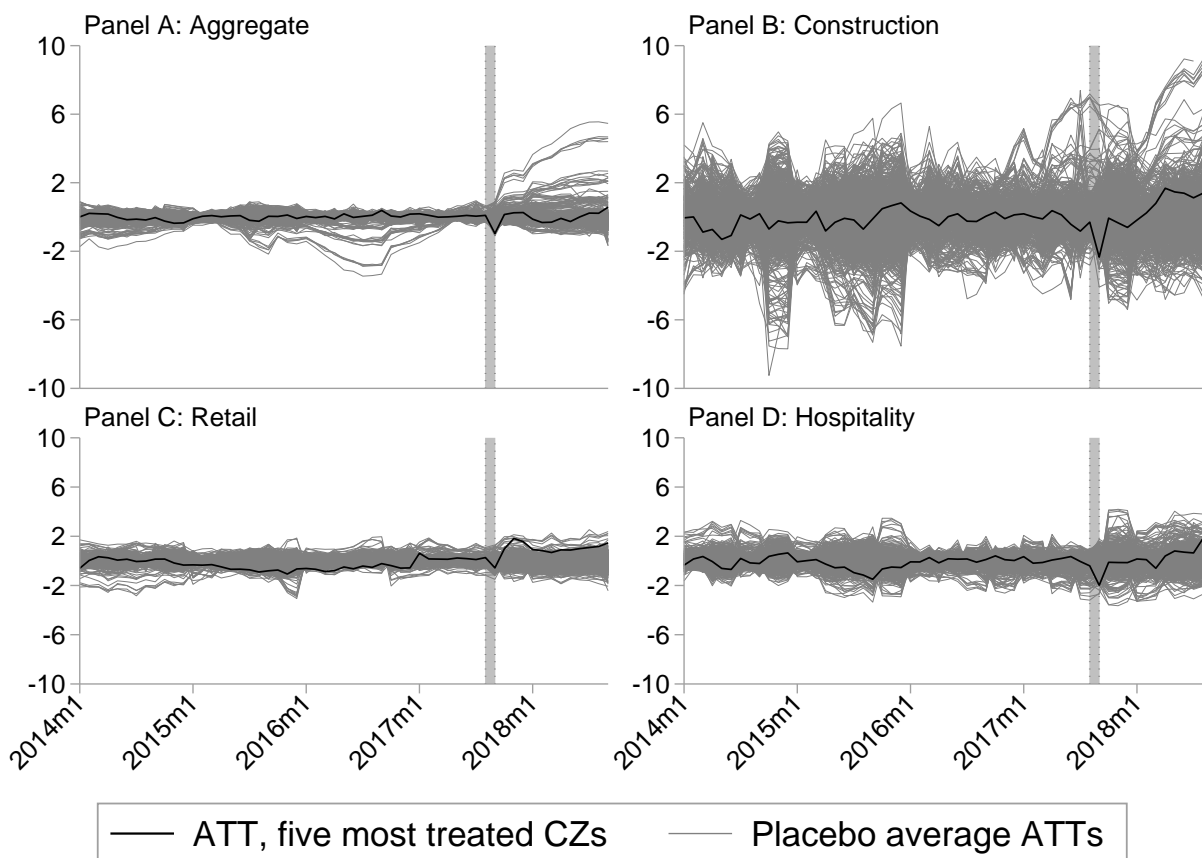
Notes: Each panel represents the gap between treated unit and control for Jacksonville and for each commuting zone in the Jacksonville donor pool, for each outcome's synthetic Jacksonville. The gap for Orlando vs the outcome's synthetic Orlando is also plotted for comparison. The variable plotted is the gap in residualized (after accounting for seasonal component and intercept) logarithm of aggregate employment (Panel A), aggregate establishments (Panel B), construction employment (Panel C) and construction establishments (Panel D). The data, from the QCEW, are at a monthly frequency for employment and a quarterly frequency for establishments. The shaded period is September 2017 for employment and Q3 2017 for establishments, when Hurricanes Irma and Maria hit Puerto Rico and Hurricane Irma hit both Orlando and Jacksonville at the same strength and approximately the same distance from its eye. The dark line represents Orlando, the dashed dark line represents Jacksonville. Because Jacksonville's exposure to Irma was comparable to Orlando's, and because Jacksonville did not receive a notable number of Puerto Rican FEMA applications related to Irma or Maria, any significant and robust post-Irma effects seen in Jacksonville may represent the impact of Irma on Orlando, which would confound our estimates of the impact of the Puerto Rican migrants.



**Figure 7**

*Estimated Average Treatment Effects and Placebo Treatment Effects for Employment in the three CZs which received the most FEMA applications per capita*

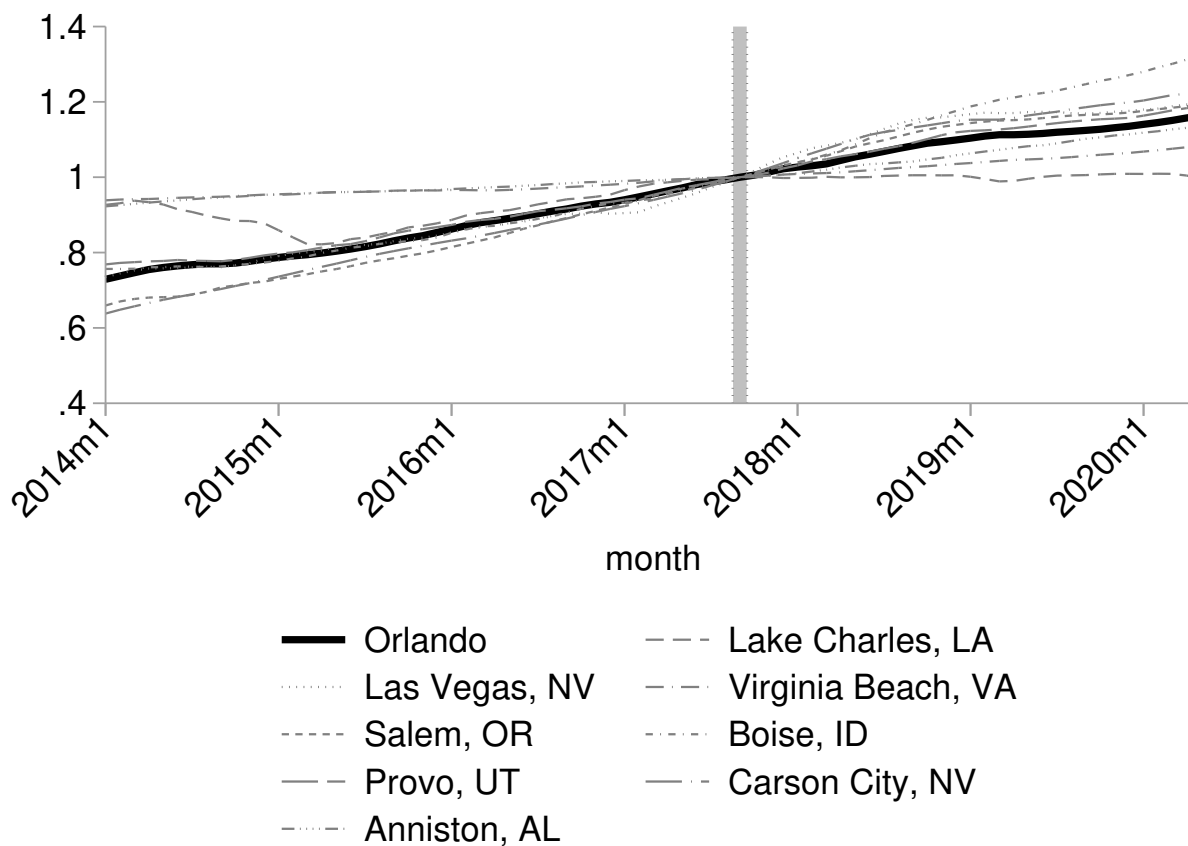
Notes: Each panel represents the applications-per-capita-weighted average % gap between the ‘treated’ unit and its control, for Orlando and for 1000 random samples of three placebo-treated commuting zones in the donor pool, for each outcome’s synthetic control. The variable plotted is  $100 \times$  the average gap in residualized (after accounting for seasonal component and intercept) logarithm of employment in aggregate (Panel A), in construction (Panel B), retail (Panel C) and hospitality (Panel D). The data, from the QCEW, are at a monthly frequency. The shaded period is September 2017, when Hurricanes Irma and Maria hit Puerto Rico and Hurricane Irma hit Florida. The dark line represents the average treatment effect for the top three treated CZs (Orlando, FL; Lakeland-Winter Haven, FL; and Springfield, MA)



**Figure 8**

*Estimated Average Treatment Effects and Placebo Treatment Effects for Employment in the five CZs which received the most FEMA applications per capita*

Notes: Each panel represents the applications-per-capita-weighted average % gap between the ‘treated’ unit and its control, for Orlando and for 1000 random samples of five placebo-treated commuting zones in the donor pool, for each outcome’s synthetic control. The variable plotted is  $100 \times$  the average gap in residualized (after accounting for seasonal component and intercept) logarithm of employment in aggregate (Panel A), in construction (Panel B), retail (Panel C) and hospitality (Panel D). The data, from the QCEW, are at a monthly frequency. The shaded period is September 2017, when Hurricanes Irma and Maria hit Puerto Rico and Hurricane Irma hit Florida. The dark line represents the average treatment effect for the top five treated CZs (Orlando, FL; Lakeland-Winter Haven, FL; Springfield, MA; Daytona Beach, FL; and Tampa-St. Petersburg-Clearwater, FL)



**Figure 9**

*House Prices, Orlando and Synthetic Control Donors*

Notes: The lines plot (the natural logarithm of) the Zillow Home Value Index for Orlando and each of the positively weighted MSAs corresponding to the CZs for the construction sector employment synthetic control (normed to September 2017). The synthetic control weights by commuting zone for construction employment are listed in Online Appendix Table A4.

**Table 1**  
*Difference in Means, Recent Puerto Rican Migrants vs Comparators*

	Natives (all mainland)	Puerto Rican Islanders	Natives (Florida only)
Age (years)	-9.383*** (-18.29)	-7.445*** (-15.83)	-8.827*** (-9.59)
Male	0.0698*** (4.07)	0.0675*** (3.90)	0.0352 (1.16)
Married	-0.157*** (-9.17)	-0.0279 (-1.64)	-0.117*** (-3.84)
Yrs. education completed	-0.386*** (-5.27)	-0.253*** (-3.04)	-0.132 (-1.02)
High school graduate	-0.0648*** (-8.17)	-0.0272*** (-2.68)	-0.0469*** (-3.33)
4+ years of college	-0.0154 (-0.95)	-0.0207 (-1.27)	0.0292 (1.03)
Comparison observations	6,431,276	48,855	333,182

Notes: Analysis using data from the 2016 ACS five year sample. All results are for recipients that are in the labor force. *t* statistics in parentheses. \* Significance at the 10% level; \*\* significance at the 5% level; \*\*\* significance at the 1% level.



**Table 2**  
*Estimated Treatment Effects, All Workers*

Sector	Log Employment		Log Compensation per Worker		Log Establishments	
	6 months	12 months	6 months	12 months	6 months	12 months
<i>Aggregate</i>						
Treatment effect	0.0021**	0.0038**	0.0005	0.0028	0.0054	0.0062
RMSPE	9.9813	8.4823	2.5299	3.8088	1.0399	1.9979
RMSPE <i>p</i> -value	0.0121	0.0121	0.5273	0.4000	0.6424	0.6061
Andrews <i>p</i> -value	0.2222	0.0444	0.8000	0.2667	0.2667	0.2667
Moving block <i>p</i> -value	0.0000	0.0000	0.1875	0.1111	0.3750	0.2778
<i>N</i>	165	165	165	165	165	165
<i>Construction</i>						
Treatment effect	0.0153	0.0402**	0.0092	0.0331*	0.0125	0.0095
RMSPE	5.5120	11.0658	5.2998	9.2167	3.3304	3.6134
RMSPE <i>p</i> -value	0.1141	0.0134	0.2617	0.0537	0.3289	0.4430
Andrews <i>p</i> -value	0.0889	0.0222	0.2000	0.0667	0.1333	0.2000
Moving block <i>p</i> -value	0.0000	0.0000	0.1250	0.0000	0.1875	0.0556
<i>N</i>	149	149	149	149	149	149
<i>Retail</i>						
Treatment effect	0.0030**	0.0090*	0.0005	0.0033	0.0037	0.0159
RMSPE	4.6217	4.8695	0.3605	0.7814	0.1335	2.2044
RMSPE <i>p</i> -value	0.0412	0.0588	0.9000	0.9588	0.9765	0.6882
Andrews <i>p</i> -value	0.3111	0.0222	0.4000	0.3333	0.3333	0.1333
Moving block <i>p</i> -value	0.0600	0.0000	0.5000	0.3889	0.6875	0.3889
<i>N</i>	170	170	170	170	170	170
<i>Hospitality</i>						
Treatment effect	0.0009	0.0118	0.0014**	0.0141**	0.0025	0.0076
RMSPE	1.6708	2.8762	17.1481	13.6413	0.1277	0.6853
RMSPE <i>p</i> -value	0.3986	0.2635	0.0270	0.0338	0.9595	0.9054
Andrews <i>p</i> -value	0.6444	0.0222	0.4000	0.0667	0.6667	0.2000
Moving block <i>p</i> -value	0.4000	0.0536	0.0000	0.0000	0.8125	0.3889
<i>N</i>	148	148	148	148	148	148

Notes: Synthetic control estimates of the impact of the immigration inflow on the residualized (after accounting for seasonal component and intercept) logarithms of the indicated outcomes in Orlando (6 and 12 months after Hurricane Maria hit Puerto Rico), using data from the QCEW, unrestricted donor pool. Significance based on RMSPE *p*-values: \* significance at the 10% level; \*\* significance at the 5% level; \*\*\* significance at the 1% level.

**Table 3***Estimated Treatment Effects, Non-Hispanic and Less-Educated Workers*

Sector	Non-Hispanic Workers				Less-Educated Workers			
	Log Employment		Log Earnings per Worker		Log Employment		Log Earnings per Worker	
<i>Aggregate</i>	6 month TE	12 month TE	6 month TE	12 month TE	6 month TE	12 month TE	6 month TE	12 month TE
Treatment effect	-0.0003**	0.0082**	0.0001	0.0009	-0.0006	0.0080**	0.0008	-0.0199*
RMSPE	10.9700	16.2198	0.8780	1.9797	4.1896	9.7029	1.7400	7.5027
RMSPE <i>p</i> -value	0.0448	0.0448	0.6791	0.6119	0.1765	0.0441	0.3750	0.0588
<i>N</i>	134	134	134	134	136	136	136	136
<i>Construction</i>	6 month TE	12 month TE	6 month TE	12 month TE	6 month TE	12 month TE	6 month TE	12 month TE
Treatment effect	0.0004	0.0068	0.0004**	-0.0245**	0.0082	0.0166	0.0007**	-0.0147*
RMSPE	2.0665	3.7870	6.7462	7.4052	3.6389	3.8433	8.8591	8.4125
RMSPE <i>p</i> -value	0.4141	0.3203	0.0234	0.0469	0.2857	0.3308	0.0226	0.0602
<i>N</i>	128	128	128	128	133	133	133	133
<i>Retail</i>	6 month TE	12 month TE	6 month TE	12 month TE	6 month TE	12 month TE	6 month TE	12 month TE
Treatment effect	-0.0001	0.0009	0.0036	0.0205	-0.0004	0.0089	0.0060**	0.0119**
RMSPE	1.4148	3.4864	1.0713	3.0396	0.1467	0.8946	6.6239	7.1356
RMSPE <i>p</i> -value	0.3869	0.2993	0.5839	0.3577	0.9265	0.8015	0.0221	0.0221
<i>N</i>	137	137	137	137	136	136	136	136
<i>Hospitality</i>	6 month TE	12 month TE	6 month TE	12 month TE	6 month TE	12 month TE	6 month TE	12 month TE
Treatment effect	-0.0016	0.0120	-0.0002	0.0028	-0.0041	0.0042	0.0003	0.0134
RMSPE	0.5137	0.9949	1.2531	3.2994	1.3909	1.1233	0.9401	1.1408
RMSPE <i>p</i> -value	0.7883	0.8540	0.4818	0.2993	0.4962	0.8647	0.6165	0.8045
<i>N</i>	137	137	137	137	133	133	133	133

Notes: Synthetic control estimates of the impact of the immigration inflow on the residualized (after accounting for seasonal component and intercept) logarithms of the indicated outcomes in Orlando (6 and 12 months after Hurricane Maria hit Puerto Rico), using data from the QWI. Columns 1-4 show estimates for non-Hispanic workers. Columns 5-8 show estimates for less-educated workers. The donor pool is restricted to include only those commuting zones which are observed for four quarters after the Hurricane, allowing 12-month estimates of the treatment effects. Significance based on RMSPE *p*-values: \* significance at the 10% level; \*\* significance at the 5% level; \*\*\* significance at the 1% level.