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IZA DP No. 13049

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ABSTRACT

The Economic Impact of Migrants from Hurricane Maria*

Using a synthetic control estimation strategy we examine the economic impact of a large inflow of people from Puerto Rico into Orlando in the aftermath of Hurricane Maria, which devastated Puerto Rico in September 2017. We find that aggregate employment in Orlando increased as a result of the inflow, as did employment in the construction and retail sectors. We also find positive overall employment effects on non-Hispanic and less-educated workers, as well as positive effects on compensation for those same subgroups in the retail sector. In the construction sector – which absorbed the preponderance of this migrant labor supply shock – we find that earnings for non-Hispanic and less-educated (workers likely to be natives) decreased by a modest amount. These results together suggest that, while migrant inflows may have small negative impacts on the earnings of likely-native workers in sectors directly exposed to the labor supply shock, employment and earnings of likely-native workers in other sectors are positively impacted, possibly by increased local demand.

JEL Classification: F22, J15, J21, J61

Keywords: migration, natural disasters, local economies

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1 Introduction

On September 22nd, 2017 Hurricane Maria struck Puerto Rico, bringing devastation on a scale rarely seen before. Most of the island was left without basic infrastructure such as electricity and running water. Large parts of the housing stock suffered catastrophic damage, and hundreds of thousands of families suffered major losses in property and lives. In the months following the hurricane, the response from FEMA was slow, and reconstruction and restoration of electricity, clean drinking water, and road connections lagged. Struggling to see various basic needs met, over 120,000 individuals and families rapidly left Puerto Rico for the U.S. mainland. This paper examines the economic impacts of this sudden migration event in Orlando, the city which received a large plurality of these refugees. Specifically, we examine whether there is evidence of either negative or positive effects on employment and earnings for incumbent workers we identify as highly-likely to be natives. We employ a synthetic control approach using the Quarterly Census of Employment and Wages and Quarterly Workforce Indicators, and conduct a sectoral analysis of these outcomes to examine whether this migration event constituted both a local labor supply shock and a local consumer demand shock, to better understand the local general equilibrium effects of migrant inflows. The sudden and unexpected nature of this event provides us with a quasi-experimental setting to study these impacts given this migration wave was plausibly exogenous to local economic conditions.

Measuring the impact of migrants on local economies has been a central focus of economists for decades. Despite the numerous papers, books, and other scholarly works on the topic, much work remains to be done on numerous outstanding questions, particularly as answers to these questions also carry substantial relevance for policy makers. Several papers have analyzed the local labor market effects of sudden waves of immigration, such as the inflow of Soviet Jews to Israel during the 1990s (Cohen-Goldner and Pasermann, 2010), the inflow of Syrians to Turkey in 2013-15 (Ceritoglu et al., 2015), and the inflow of Algerians to France in the 1950's (Hunt, 1992)). The only significantly studied case in the U.S. on the local impact on incumbent workers—potentially *over-studied*, given the limited amount of data available and that it is now forty years old—is the Mariel

Boatlift.[1] This was an episode in which approximately 100,000 people from Cuba escaped, mainly 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-analysis of the event has generated some disagreement on the effects (Borjas, 2017; Peri and Yasenov, 2019; Clemens and Hunt, 2019). The details of the controversy are centered around measurement, sample choice, and methods used.

For multiple reasons, we consider the event we study to be more relevant than the Mariel Boatlift for providing a modern understanding of the local labor market impacts of immigration. We also consider our data, methodological approach, and the adjustment mechanisms we study to offer several improvements over the Mariel Boatlift studies. First, the episode we study is much more recent and its external validity much stronger: the economy of Orlando as of 2018 is more comparable to current US metropolitan areas than was the economy of Miami in 1980.

Second, the episode we examine in this paper is a consequence of migration in response to an extreme weather event, which is an occurrence likely to become more frequent in the future.

Third, the case of immigration from Puerto Rico involves people who have similar levels of education to local U.S. workers, while the Cubans in the Mariel Boatlift (Marielitos) had much lower levels of education. This episode, therefore, much more closely emulated the sort of immigration that the US has received in the last 20 years (say 2000-2018) which was much more balanced between high- and low-skilled migrants, while the low-skilled immigration of Marielitos was more similar to the immigration of the 1990's, when the inflow of low-skilled immigrants from Mexico reached a peak.

Fourth, this case is quite special (and, in this respect, similar to the case of the Cuban Marielitos) as immigrants from Puerto Rico have U.S. citizenship and hence have immediate access to the labor market and other benefits available to citizens.[2] This feature makes it an extreme case, as it constitutes a large, sudden, and unexpected inflow of predominantly Spanish-speaking migrants

[1]Another weather event similar to Maria, Hurricane Katrina, has also been studied quite substantially, although most of the papers focus on the impact *on* the outmigrants who fled New Orleans and 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—albeit small—0.7 percent depression of wages in low-skill industries. Compared to De Silva et al. (2010), our paper uses much a more sophisticated and, we argue, more appropriate estimation strategy (a synthetic control estimator) to test a similar set of outcomes using a similar natural experiment.

[2]As well as to the ability to vote.

who can legally work as soon as they arrive. Their potential crowding-out impact on native workers would be at a maximum due to these features. Hence our findings of no crowding-out and positive wage effects in some specific sectors is particularly compelling.

Fifth, unlike Card (1990), but like Borjas (2017) and Peri and Yasenov (2019), we employ the synthetic control estimating strategy first proposed by Abadie and Gardeazabal (2003) and further developed in Abadie et al (2010; 2015).[3] The synthetic control method constructs a control city as a weighted average of a subset of untreated donor cities, matched on pre-treatment values of the outcome and a set of covariates. Unlike the difference-in-differences approach taken by Card (1990), the synthetic control method can provide (approximately) unbiased estimates even in the presence of time-variant unobserved confounds. Therefore our "synthetic Orlando" for each outcome should match the relevant features and short-run evolution of the labor market in Orlando, absent the inflow of Puerto Ricans, providing a compelling counterfactual Orlando against which we can evaluate the effects of the immigrant inflow into Orlando. Unlike the Mariel Boatlift papers, which all rely on relatively small, weighted samples of survey data from the Current Population Survey (CPS) to study the impact of the Marielitos on Miami—a significant factor in the different conclusions drawn (see Clemens and Hunt, 2019)—we conduct our analysis using the Quarterly Census of Employment and Wages (QCEW) and the Quarterly Workforce Indicators (QWI), which are county-by-industry-level administrative data (here aggregated to commuting zones) which cover 95+% of all workers observed on a quarterly basis.[4] The detail and precision of our analysis is therefore significantly greater than what was achieved by the analyses of the only other US-based case of a sudden and unexpected, large migrant inflow (the Mariel Boatlift), and is therefore unlikely to be subject to the same criticisms as the those papers.

Sixth, we consider the labor market impacts of this event according to the potential mechanisms at play. Specifically, while we consider the event as a significant *labor supply shock*, and accordingly analyze its potential impact on incumbent workers (non-Hispanic and less-educated), which we view as most likely to be natives through the lens of labor complementarity and substitutability—

[3] Readers are pointed to Abadie (2019) for an excellent current review of the synthetic control literature, which is continually expanding.

[4] Employment levels only, in the QCEW only, are observed on a monthly basis.

we particularly look at labor supply shock impacts in the construction sector, which we argue absorbed a great proportion of the new arrivals—we also consider the consequence of so many new arrivals as a potential *consumer demand shock*—particularly to the retail and accommodations & food services sectors—such that incumbent workers in those industries may face increased labor demand and associated impacts on their employment opportunities and wages (see, for example, Bodvarsson et al., 2008).

Finally, a novelty of this paper relative to most others which analyzed the short-run effects of immigration shocks is that we also analyze the response of establishments (firms). The response of firms, and in-particular their expansion and growth in response to immigration, is an under-researched and poorly understood margin of adjustment, but it is crucial to a full understanding of the economic effects of immigration (recently Beerli et al. (2018) show how important is the firm response to increased availability of immigrants for understanding the labor market effects).

The remainder of the introduction summarizes this paper. We begin our analysis by using data from FEMA applications in the months after Hurricane Maria hit Puerto Rico to establish which areas on the US mainland received the most Puerto Rican refugees fleeing the aftermath of the hurricane. While these data imperfectly measure this migration episode, they clearly show that the Orlando Metropolitan Area was a big positive outlier with respect to the inflow of people from Puerto Rico after the hurricane.[5] Over 120,000 people left Puerto Rico for the U.S. mainland in the months through March 2018, 24,000 of whom we estimate ended up in Orlando. Maria likely played a role in the majority of these decisions to migrate; we conservatively estimate that nearly half of these migrations would not have happened in the absence of the hurricane (based on out-migration trends from Puerto Rico in the years prior). 24,000 people represents around 1 percent of Orlando's pre-hurricane population and around 2 percent of its pre-hurricane total employment. We demonstrate that, both in levels and as a percentage of the population, Orlando received many more refugees than even the next most-impacted metropolitan areas (New York and Miami). Both geographic proximity to Puerto Rico and the historical presence of a substantial Puerto Rican community are likely contributors to this particular concentration of FEMA applications: prior to

[5]We take Orlando to be the five counties which comprise the Orlando Commuting Zone: Lake, Orange, Osceola, and Seminole—which comprise the Orlando Metropolitan Area—plus Sumter County.

2017, Orlando had one of the largest Puerto Rican population shares of all U.S. metro areas. We believe this helped attract people fleeing Puerto Rico who may have had friends and relatives there.

Given these facts, we consider Orlando as the most intensely affected city from this event, and focus on it as the primary treated unit in our analysis. We contend that the large, sudden out-migration of Puerto Ricans from the island and into Orlando, as a result of Hurricane Maria, constitutes a good approximation to a *natural experiment*—namely a sudden and unexpected event, uncorrelated with conditions in the Orlando local economy—which allows us to estimate the causal impact of this migrant inflow on a set of relevant labor market outcomes. We also provide evidence that Puerto Rican migrants were broadly similar to natives in terms of educational attainment, but were also younger and substantially less likely to speak English (less than 40% of residents of Puerto Rico speak "well"). We argue this made it more likely these refugees found work in jobs which did not require English language skills, at least in the short run until they learned the language, and provide evidence suggesting Orlando's construction sector was thus the most likely to have been affected by the labor supply shock.

Having established Orlando as heavily-treated by exposure to Puerto Rican evacuees, we then analyze the impact of the inflow between September 2017 and July 2018 on local employment and earnings using a synthetic control estimating strategy. We hypothesize that these new arrivals increased both the local labor supply and local consumer demand. We first look at potential aggregate effects, then focus on the effects for specific sectors—namely the construction, retail trade, and accommodations & food services sectors. We do this for all workers, as well as specifically for non-Hispanic and less-educated subgroups (those with a high-school degree or less education) as proximate measures of, respectively, the incumbent native-worker population (Orlando non-Hispanic residents are likely to be US-born, i.e. natives) and those who may be in closest competition with the new arrivals from Puerto-Rico in the labor market. This allow us to test whether workers from these demographic groups were in any way adversely affected in terms of employment or earnings per worker as a result of increased labor market competition from these migrants.

Examining the impacts on these industrial sectors and these subgroups helps to paint a picture of the impact of the sudden migrant inflow along different dimensions. The construction sector, for example, already employed a large share of Hispanics before the hurricane. Moreover, most jobs in construction require limited use of the English language, suggesting it would have been an attractive sector for the new immigrants to seek employment in the short-run. While the retail and accommodations/food services sectors are relatively low-skill, labor-intensive industries—meaning they may have absorbed a portion of the new labor supply—it is also true that new additions to a local population require accommodation, goods, and services; thus we hypothesize that the arrival of these migrants resulted in growth in local *demand* for consumer goods and services, and hence for workers in these sectors. Changes in job numbers and wages in these sectors are likely the result of a combination of local supply and demand effects from the new immigrants.

Our analysis also considers the response in terms of the number of establishments in the area. The speed and magnitude of the response by firms to this expansion of both local labor supply and local consumer demand is important for understanding the mechanisms through which the labor supply shock was absorbed by the local economy. Clearly, given the time-horizon of our analysis, we are looking at short-run effects of the inflow, but it is valuable to consider whether firms, investment and capital quickly respond to opportunities in a small local economy. While our estimated effects are all positive, their pre-treatment trends are noisy enough that none of them reach a level of significance according to our test statistic, leaving us unable to draw any conclusions about the impact on local investment.

The results of our analysis provide a rather thorough and consistent picture: we find that, at the aggregate level, the economy of the Orlando metro area experienced significant overall employment growth of 0.4 percent relative to the synthetic control over the year following September 2017, while overall compensation per worker did not change significantly. The employment effects are especially pronounced in the construction sector, which likely absorbed a large share of new immigrants—construction employment increased by 4 percent 12 months after the refugee inflow began. This is consistent with firms quickly responding to the availability of more workers in this sector—and in the retail sector, which saw employment grow by nearly 1 percent. Earnings per worker in the accommodations & food services sector grew 1.4 percent 12 months after treatment began. Broadly, these results are consistent with the Puerto Rican refugees constituting both a labor supply shock concentrated in the construction sector, and a shock to local demand for goods and services in the retail and accommodations and food services sectors, resulting in increased labor demand driving competition for workers to meet this demand among employers in these sectors, the latter of which typically pays lower wages.

Looking specifically at the impact 12 months after the hurricane on non-Hispanic and less-educated workers, who we argue are most likely natives, we find that the absorption of the migrants into the local economy resulted in significant overall non-Hispanic employment growth of 0.8 percent, with no negative effect on the overall earnings of non-Hispanics. The impact on the construction sector—which was likely only exposed to a labor supply increase—was a small yet significant 2.5 percent decrease in wages, while wages for less-educated workers in the retail sector increased by 2 percent, though this latter result is rather noisy and not particularly robust. These results follow similar patterns when looking at less-educated workers, though our estimated positive impact of a 1.2 percent increase in less-educated retail earnings per worker is significant, while the negative impact on less-educated construction earnings per worker shrinks to 1.5 percent.

Taken together, we interpret these results as evidence that, while the earnings of natives in sectors exposed to large inflows of migrant labor may respond to the labor supply shock in a negative way, earnings and employment in other sectors respond positively to the associated consumer demand shock. Looking at all workers, we find no evidence of any negative impact on wages in aggregate (the point estimates are positive yet not significant), suggesting that the positive effects at least offset the negative ones in the aggregate. Summarizing, even in the short run we find no overall impact of the migration event on native earnings, and we find a positive overall impact on native employment in Orlando.

These results are broadly robust to several controls and specifications, and suggest that large inflows of workers are quickly absorbed and boost local demand. The rest of the paper proceeds as follows: Section 2 describes the demographic and employment trends of immigrants from Puerto Rico in the mainland United States, and justifies the use of Orlando as our unit of interest. Section 3 describes our data and empirical methodology. Section 4 describes the results from our primary and secondary analyses, and Section 5 presents robustness checks. Section 6 concludes.

2 Characteristics and Trends of Puerto-Rican Immigration

2.1 Historical Migration and Characteristics of Migrants

To support our choice of Orlando as the main receiving area for the people fleeing Puerto Rico we briefly describe the pre-hurricane history of immigration from that island to the mainland US. Following Puerto Rico's annexation by the U.S. in 1898 after the Spanish American War, and continuing with the 1917 Jones-Shafroth Act which granted all Puerto Ricans American citizenship, Puerto Ricans have enjoyed the same rights as mainland U.S. citizens with respect to mobility and work within the U.S.

During the first half of the 20th century and for several decades thereafter, rates of migration from Puerto Rico to the mainland and the overall size of the Puerto Rican population on the mainland both remained low. (The Puerto Rican population on the mainland was an estimated 1,513 in 1910 and 226,110 in 1950, respectively constituting 0.002 percent and 0.15 percent of the total population at the times)

During the later half of the 20th century and continuing into the 21st, however, Puerto Rican migration to the mainland United States increased significantly, with about 5.4 million Puerto Ricans estimated on the mainland U.S. in 2015 (constituting 1.6 percent of the U.S population). This number is particularly striking when compared to the total population of 3.5 million people in Puerto Rico as of 2018 (Whalen and Vasquez 2005). During these years, large Puerto Rican communities formed in New York City and the surrounding areas, in Philadelphia and Chicago, and in parts of Florida. Migration followed the usual predictable pattern, with new immigrants consistently relocating to areas with relatively large Puerto Rican communities.

Using U.S Census data from 1980, 1990, & 2000, and data from the American Community Survey from 2005 to 2017—a period of heavy Puerto Rican migration to the mainland U.S.—we demonstrate that recent Puerto Rican migrants to the mainland during these years were drawn to areas which already had high presence of Puerto Ricans, leading to further concentration of these communities. Figure A5 shows the share of people from Puerto Rico in each US commuting zone in 2005, and 2017. Both maps show significant concentration in the New York area and Orlando area. The tendency of new immigrants to locate where previous ones from the same country settled is a general feature of networks of migration, and is the basis for the construction of shift-share instruments (Altonji and Card 1991, Card 2001). Figure 14 shows the map of recent immigrants from Puerto Rico, namely those arrived in the previous year. The map confirms the agglomeration in New York and Orlando for the immigrants that have arrived in the last fifteen years, with Orlando becoming a particularly popular destination in the years right before 2017. The map for 2017 is consistent with the idea that migration networks directed a large share of Puerto Ricans to Orlando, offering (beyond our analysis below) reason to believe that, in the aftermath of the hurricane, Orlando received the largest plurality of Puerto Rican refugees.

The uninhibited access to labor opportunities in the mainland United States available to Puerto Ricans suggests that the largest driver of mobility from the island to the mainland has been availability of jobs in the US. Existing studies (e.g. Grogger and Hanson, 2011) show that migrants coming to the United States 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. On the other hand Borjas (2008) argues that migrants from Puerto Rico to the mainland are negatively selected in terms of education. In order to have a sense of the skills of people who migrated from Puerto Rico in the years near 2017 we use the census and ACS data mentioned above to analyze how recent Puerto Rican migrants (defined as having migrated within the past year) to the U.S. mainland compare to mainland natives along a set of observable characteristics.[6] Table 1 shows the average difference in several demographic characteristics of migrants from Puerto Rico living in the mainland, with US natives (column 1), Puerto Rico residents (column 2) and US natives resident in Florida. a negative value implies that the Puerto Rico migrants have a smaller value for that variable relative to the comparison group.

The characteristic most different between recent Puerto Rican immigrants and US natives is the average age, as Puerto Rico immigrants are significantly younger and less likely to be married than natives. On the other hand, education levels are quite similar with only 0.38 years of schooling

[6] We use the variables associated with the census question "Have you migrated within the past five years?" and the ACS question "Have you migrated within the last year?" to define "recent migrants". All comparisons are between recent migrants from Puerto Rico and either those still living in Puerto Rico or to natives (nationally and only those in Florida), and are restricted to those who are in the labor force.

difference (statistically significant but small) and no significant difference in the share of college educated individuals. While Puerto Rican migrants to the mainland are younger, on average, than remaining islanders and mainland natives, in terms of educational attainment they appear neither selected on ability from within the Puerto Rico population nor in comparison to mainland natives. This implies that they are rather similar to recent immigrants to the US, which have been (on average) similar in education levels to US natives. The analysis of their impact on the local labor markets, therefore, is much more relevant as a means of assessing the recent impacts of immigrants than were the analyses of the impacts of Cuban immigrants in Miami in the aftermath of the Mariel Boatlift.

2.2 Migration after Hurricane Maria

Following the devastation of Hurricane Maria, Puerto Ricans evacuated the island in large numbers. While some early estimates put the number of Puerto Ricans who fled to Florida alone at over 200,000 people, later estimates based on Facebook data suggest around 44,000 people moved from Puerto Rico to Florida *and stayed* from October 2017 to March 2018 (Alexander, Polimis, and Zagheni, 2019), while estimates based on flight passenger lists suggest between 30,000 and 50,000 people moved from Puerto Rico to Florida during this period (Rayer, 2018). Those numbers may provide some sense of the actual number of evacuees. We analyze additional evidence to get an idea of the size of the migration flow and of its distribution across the US. We use data from applications for disaster relief from the Federal Emergency Management Agency (FEMA) and examine their geographic distribution. These application data, obtained through a Freedom of Information Act request, represent claims made to FEMA to obtain some disaster relief which were filed by people who had a house in Puerto Rico which was damaged by Hurricanes Irma and Maria. 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.

Figure 1 maps the number of applications for disaster assistance which were filed between

September 2017 and March 2018 for properties located in Puerto Rico across U.S. commuting zones. Panel (a) shows the levels and panel (b) the applications per 100,000 population. A look at the maps reveals that these FEMA applications were heavily concentrated in very 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 all FEMA applications linked to Puerto Rican addresses and relating to Hurricanes Irma and Maria. While (Alexander, Polimis, and Zagheni, 2019) estimated that over 160,000 net out-migrants from Puerto Rico moved to the mainland in the six months following the hurricanes, and the Center for Puerto Rican Studies estimate this number at more than 135,000 (CENTRO, 2018), we adopt the more conservative U.S. Census Bureau estimate of 123,399 net out-migrants from Puerto Rico between July 2017 and July 2018 (U.S. Census Bureau, 2018) as a baseline estimate of short-run net migration from Puerto Rico following the hurricanes. Assuming that FEMA applications offer an approximate estimate of the geographic relocation of these migrants, we conservatively estimate that around 24,000 Puerto Ricans relocated to the Orlando area in the months following the hurricanes. This represents just over 1 percent of Orlando's population and 1.5 percent of its working-age population (aged 16-64). However, outflows from Puerto Rico to the mainland had been relatively high in the decade before the hurricane, and steadily in the mid- to high 60,000's in the three years prior. If we difference out the number of 'International' out-migrants[7] in the year before the hurricane as a baseline flow, we are left with around 55,000 extra migrants who likely would *not* have moved if not for the hurricanes (assuming the out-migration trend of the preceding years would have continued). Again assuming the geographic distribution of FEMA applications are a broadly valid estimate of where these refugees ended up, we estimate around 11,000 Puerto Ricans relocated to Orlando in the twelve months following the hurricanes *who would not have done so were it not for the hurricanes.* This estimate is almost identical to the number of net 'International' in-migrants to the Orlando CZ after

[7]The Census Bureau defines flows between Puerto Rico and the U.S. mainland as 'International' migration.

differencing-out the number from the previous year as a baseline flow (U.S. Census Bureau, 2018). This represents around 0.5 percent of Orlando's population and 0.65 percent of its working-age population (U.S. Census Bureau, 2017) *over and above the inflows which would likely have occurred regardless of the hurricanes.* We interpret these patterns as evidence that the post-Maria Puerto Rican refugee flows provided a significant (although not massive) 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 (the large majority of these FEMA applications were received by November 2017). To put this in context, an inflow of immigrants equal to one percent of the labor force is larger than the annual inflow of Mexicans during the 1990s, when Mexican immigration into the US was highest. This inflow as a percent of Orlando's labor force is thus comparable to or higher than immigration inflows in the peak years for the US as a whole, and it was concentrated in the months between September 2017 and March 2018. In comparison, the Mariel Boatlift was larger in relative terms, adding 5-6 percent to local employment in a few months, but as noted above it was also an unusually low-skilled inflow and was at a time when recent migration had been relatively small in the US (1980). To compare this inflow to another well-known international event, the inflow of Syrian refugees in Turkey was comparable to our setting, at 2.5 percent of the local population over the course of a couple of years (see Tumen 2015). Overall the case of Puerto Rican migrants to Orlando is much more representative of recent immigration to the US than was the Mariel Boatlift, as it was more balanced in terms of skills, and of similar magnitude relative to the local population, and hence the lessons we can learn from it have stronger external validity.

2.3 Sector distribution of Puerto Rican Migrants

Migrants from Puerto Rico to the mainland U.S. work in virtually every industry in the U.S. economy. This suggests that a large labor supply shock of Puerto Rican evacuees may have a diffused impact, increasing potential workers in many sectors. Using data from the American Community Survey (ACS) between the years 2011 and 2016, and focusing on the Orlando area, we find that migrants from Puerto Rico have distribution, across sectors of employment, that is not too dissimilar from that of natives. The employment share of natives and people born in Puerto

Rico across sectors in Orlando as of 2016, i.e. before the Maria inflow, are shown in Table 8 in the appendix. This distribution shows a slightly higher concentration of Puerto Rican natives in local services such as transportation and retail and a slightly smaller concentration in Education and Management. If people born in Puerto Rico constitute an important network to find jobs, then the wave of newcomers might be expected to mirror the distribution of already established Puerto Ricans. It is useful, however, to show the sector distribution of all Hispanics and of Hispanics born abroad as done in columns (3) and (4) of Table 8. Because of language and cultural commonalities this group can also be a very important network reference to find jobs. In this case we see a substantial over-representation of this group in Construction as well as in transport, relative to natives. The construction sector can be particularly appealing for a job in the short-run, as it is the one with the largest share of workers, in the Orlando area, not speaking English or not speaking English well (11.8% of them). As the majority of Puerto Rican residents are not fluent in English, this would make a job in construction more attainable in the short run[8]. The U.S. Bureau of Labor Statistics (2015) also 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 (see Table A7 in the appendix). Finally, looking at the QWI employment data in Orlando, Figure 4 shows that construction saw a substantially larger growth in the Hispanic share of employment between Q2 2017 (before the hurricane hit), and Q2 2018 compared to other industries where we might expect to see new workers who don't speak English well.[9] Putting together the local distribution of Hispanic workers across sectors and the low need for English proficiency, there is a significant presumption that the construction sector labor supply might have been affected the most in the months after September 2017 as people from Puerto Rico fled Hurricane Maria.

[8]Using data from the 2000 and 2010 Puerto Rican Community Survey (PRCS) as well as the 2016 Puerto Rico ACS (PRACS) five year sample administered by the U.S. Census Bureau, we find that just over a third of Puerto Rican residents under the age of 60 speak English at least "well"

[9]We note there is growth in the Hispanic employment share in all sectors, as Orlando has in general been experiencing higher growth in its Hispanic population than in its population as a whole. We also note that an increased Hispanic employment share does not (necessarily) indicate negative employment growth for non-Hispanics, but can rather simply indicate disproportionately high employment growth for Hispanics.

3 Data and Methodology

3.1 Overview

Given the patterns described in the previous section, the sudden out-migration of people from Puerto Rico and into Orlando in the aftermath of Hurricane Maria constituted what is known in the economics literature as a natural experiment. Namely this inflow of evacuees was random, i.e. it was uncorrelated with fundamentals in Orlando and with their evolution over time; it affected only this area (after we remove somewhat-affected CZs from our donor pool), leaving a number of other, similar cities unaffected; and it was sudden/unexpected. The estimates of the effect on several of Orlando's economic outcomes, relative to the other unaffected cities, can therefore be considered as causal. Our analysis focuses on the Orlando-area labor market, in general and with special focus on certain sectors described below. We conduct our analysis at the commuting zone (CZ) level—an aggregation of counties constituting a local labor market—to internalize most of the local labor market impacts within the analyzed unit. These units of analysis are customary when analyzing labor market impacts (see, for instance, Autor, Dorn and Hanson (2013) and Autor and Dorn (2013)).

Focusing on the Orlando CZ as our treated unit of interest, we adopt a synthetic control approach to estimate the impact of the sudden inflow of evacuees on local (log) employment, labor compensation, establishment counts, and compensation per worker, the last of which is mechanically derived as compensation divided by employment and which we view as an approximate measure of average individual earnings. We present estimates for the local economy in aggregate and specifically on three sectors: the Construction sector (NAICS 23) which, as argued in the previous section, received potentially the largest labor supply increase due to the skill and sectoral distribution of Hispanic workers; the Retail Trade sector (NAICS 44-45); and the Accommodation & Food Services sector (NAICS 72). Like construction, retail and accommodations & food services are labor-intensive non-tradable sectors and may have received a (smaller) labor supply shock, but also are most likely to have experienced a labor demand shock associated with the increased demand for local accommodation, hospitality services such as restaurants, and goods for purchase.

Hence we can interpret the employment/wage impact on the construction sector as likely driven by the strongest sector-specific supply change, while the impact on employment in the other local non-tradable sectors is plausibly the combination of supply and demand.

The aggregate effects described above are a combination of the absorption of new workers and consumers in the Orlando economy, and the response of local workers and consumers to such a change. Because of the addition of thousands of people to the local economy, an increase in employment is to be expected. We then focus on a second group of outcomes which approximate the effects of immigration on native (i.e. incumbent) workers, who are usually the focus of these sorts of studies (e.g. in Card (1990), Borjas (2017), and Peri and Yasenov (2019)) and which capture the wage effect and the effect on native employment of this mass-migration episode.

To do this we analyze the employment and wages of non-Hispanic whites (very likely to be US born) both in the overall economy and in the specific sectors where demand and supply effects should be more visible. We also try to identify the group of workers that in the short-run may be more affected by labor market competition from immigrants, especially if they take jobs that do not require sophisticated language skills and are disproportionately found in the construction sector. These are typically workers with low levels of education (high school or less) and who receive relatively low pay (on average). While immigrants from Puerto Rico did not have levels of schooling significantly lower than natives, in the short run they might have been willing to "downgrade" their skills in order to find a job (e.g. if their English language skills were not particularly strong). In such a case, even with a similar education distribution among immigrants and natives there may have been stronger competition for low-skilled jobs.

Finally, we consider changes in the number of local establishments as one plausible measure of the impact on local investment, at least in the short-term. A response of firm-creation in the short run is a sign of local investment in response to the larger availability of labor—a channel often considered as a mechanism of adjustment in the long run, but for which there is little information regarding "rapid response" (i.e. in the short run) in a local economy.

3.2 Data

Our primary analysis is conducted using data from the Quarterly Census of Employment and Wages (QCEW) program, published by the U.S. Census Bureau. 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 quarterly and are available at the industry-by-county level down to the 6-digit NAICS level definition. For cells that are particularly small there may be "suppression" of data due to privacy reasons, but there are almost no suppressions at the 2-digit NAICS level. This is essentially complete coverage of employment and earnings, and the availability of these data each month or quarter (depending on the variable) allow us to conduct a reliable short-run analysis along several dimensions. The detailed coverage and reliability is also a substantial benefit relative to studies of the Mariel Boatlift, which are all based on small, weighted samples from the Current Population Survey microdata. This gain in coverage comes at the expense of observing data aggregated at the county-level (in our case, CZ-level) instead of individually. However, for the purposes of measuring local labor market effects this is not a substantial limitation, especially considering that we can condition on important worker characteristics in additional analyses using supplementary data (below). [10] Of our variables of interest, employment is observed monthly while compensation, establishments, and our derived compensation-per-worker variables are observed on a quarterly basis. 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 consider short-run effects both one-half and one-full year after the hurricanes hit (September 2017).

Our additional analysis is conducted using data from the Quarterly Workforce Indicators (QWI). A product of the U.S. Census Bureau, the QWI data are the result of the Longitudinal Employer-Household Dynamics Program, which (as with the QCEW) is fed by data from state UI accounting systems, as well as other sources. The QWI are particularly suited to our purposes because they report employment and earnings of employees at the industry-by-county level, but they also break the data down by education level and by ethnicity (though not both, together). This, as mentioned

[10] Moreover, the CPS individual data are (broadly) not longitudinal, so using them to examine dynamic impacts on local markets requires they be aggregated to the local labor market level anyway.

above, allows us to measure labor market outcomes for a subset of workers who are likely to be incumbents in the Orlando labor market at the time of the arrival of evacuees (that is, non-Hispanic workers), as well as for workers with high school degree or less, who would suffer more from the competition of new immigrants if migrants primarily access low-skilled jobs (the education levels of workers under age 25 are not observed, limiting this part of the analysis to non-youth workers). In the QWI data, all variables including employment are only observed on a quarterly basis, a few states are missing observations for some quarters of interest, and the available data series do not cover all counties in all states for a full year after Hurricane Maria.[11] This final issue, in particular, has the effect of somewhat restricting the donor pools in the synthetic control analysis because of data access issues—though the loss of observations is relatively small. Thus it is useful to present analyses using both the QWI and the QCEW data, so as to allow each to reinforce and complement the other.

3.3 Empirical Approach

3.3.1 Synthetic Control Estimator

The econometric analysis that we perform adopts the synthetic control estimator introduced by Abadie and Gardeazabal (2003) and further refined by Abadie et al. (2010; 2015). Specifically, our analysis exploits a data generating process in which a single observed unit experiences a continuous event/treatment clearly beginning at a single point in time, while a ‘donor pool’ of potential ‘control’ units do *not* receive treatment.[12] In our case, treatment is the inflow of Puerto Ricans into the city of Orlando, which began immediately after the sudden and unanticipated decimation of Puerto Rico by Hurricanes Irma and Maria. In order to evaluate whether the treatment had an impact on the treated unit, relative to what would have happened in its absence, our estimating strategy identifies a control unit—the *synthetic control*—which is a weighted average of a subset of units in the donor pool (which each meet the requirements for inclusion in the donor pool, described below) matched on the pre-treatment values of a set of predictors of the outcome, including linear

[11] States missing observations for some or all counties include Arkansas, Maine, Minnesota, Mississippi, Missouri, New Jersey, Pennsylvania, South Dakota, Virginia, and Washington.

[12] This differs from e.g. Cavallo et al. (2013), Abadie and L’Hour (2019), and Ben-Michael et al. (2019), which consider multiple treated units.

combinations of values of the pre-treatment outcome plus covariates. The dynamic path of synthetic control outcomes of interest represents the path of counterfactual observations to those actually observed for our treated unit (the Orlando commuting zone), allowing us to estimate the treatment effect on each outcome of interest as the difference between its observed value in Orlando and the corresponding synthetic control.

Formally, we observe $N = J + 1$ units, indexed by j, where $j = 1$ is the single treated unit of interest (Orlando) and the remaining J units are the untreated members of 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. $Y_{j,t}^N$ is the potential outcome observed in $\{j, t\}$ if j is *not* treated at t, and $Y_{j,t}^I$ is the potential outcome observed in $\{j, t\}$ if j *is* treated at t. Then the treatment effect (also often referred to as the "gap") can be defined as:

$$\alpha_{j,t} = Y_{j,t}^I - Y_{j,t}^N \tag{1}$$

The observed outcome in j, t is: $Y_{j,t} = Y_{j,t}^N + \alpha_{j,t} D_{j,t}$. We appropriately restrict inclusion in the donor pool to ensure only $j = 1$ is treated.[13] Accordingly, we have:

$$D_{jt} = \begin{cases} 1 & \text{if } j = 1 \text{ and } t \; > \; T_0 \\ 0 & \text{otherwise} \end{cases}$$

We want to estimate the dynamic path of treatment effects, $\alpha_{\mathbf{1}} = (\alpha_{1,T_0+1}, ..., \alpha_{1,T})$. We note $Y_{1,t}^I$ is observable $\forall \; t > T_0$, so we need only estimate $Y_{1,t}^N$. Our approach is to adopt the synthetic control estimator described in Abadie et al. (2010; 2015):

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

Define a vector $\mathbf{X_1}$ of dimension $K \times 1$, the elements of which are the values of the r selected covariates of the outcome variable in the pre-treatment period plus the M linear combinations of the pre-treatment period observations of the outcome variable, with $K = r + M$, which together

[13] Described below.

function to predict the outcomes of interest in the pre-treatment period for our treated unit of interest (i.e. Orlando). Similarly, define a $K \times J$ matrix $\mathbf{X_0}$ in which row j is the vector of values for the same variables and time periods as in $\mathbf{X_1}$ but for untreated unit $j \in \{2, ..., J+1\}$ in the donor pool. The synthetic control method then selects the vectors of weights $\hat{\mathbf{W}} = (\hat{w}_2 ... \hat{w}_{J+1})' = \hat{\mathbf{W}}(\hat{\mathbf{V}})$ for each of the J units in the donor pool given the matrix of weights $\hat{\mathbf{V}}$ for the K predictor variables,[14] to solve:

$$\hat{\mathbf{W}} = \arg\min_{\mathbf{W}} \sqrt{(\mathbf{X_1} - \mathbf{X_0 W})' \mathbf{V} (\mathbf{X_1} - \mathbf{X_0 W})} \quad \text{s.t.} \quad \sum_{j=2}^{J+1} w_j = 1, \ w_j \ \geq \ 0 \ \ \forall \ \ j \in \{2, ..., J+1\} \tag{3}$$

These estimated weights are used to calculate $\hat{Y}_{1,t}^N$, and finally our estimated elements of $\boldsymbol{\alpha_1}$:

$$\hat{\alpha}_{1,t} = Y_{1,t} - \hat{Y}_{1,t}^N \ \ \forall \ \ t \in \{T_0 + 1, ..., T\}$$

A few notes are in order regarding our implementation of this estimator. First, identification relies on the Stable Unit Treatment Variable Assumption (SUTVA), which essentially assumes that the units in our donor pool are unaffected by the treatment of our treated unit (see e.g. Cao and Dowd, 2019). In practice, we restrict inclusion in our donor pool of J CZs first by excluding all CZs with 10 or more Puerto Rican Irma- or Maria-associated FEMA applications per 100,000 population. This ensures the CZs in our donor pool were essentially untreated (by comparison, recall Orlando received 178 Puerto Rican Irma- or Maria-associated FEMA applications per 100,000 population). To reduce the possibility of interpolation bias, of the remaining CZs (for each sector of focus, individually) we further restrict the donor pool to include only those CZs in or above the 75^{th} percentile of sectoral-specific employment levels. This ensures we are only allowing our synthetic Orlando (for each sector of focus) be comprised of other CZs with large labor pools in the same sector. For each outcome variable individually, we also drop any commuting zones for which the outcome is not observed in every period (the result of occasional confidentiality-suppressions in

[14] Because we restrict our pre-treatment period to the years after the recovery from the Great Recession had firmly taken root, we do not have a substantial number of pre-treatment periods. Accordingly, we follow the advice of Abadie et al. (2015) and do not employ a cross-validation method to select the predictor weights in $\hat{\mathbf{V}}$. Rather, we use the regression-based method adopted as the default in the `synth` command for Stata (Abadie et al., 2011) described in detail in Kaul et al. (2015).

the raw data). This yields the group of 148-170 commuting zones (depending on the sector of focus, and including Orlando) in the United States for which data is consistently available at monthly or quarterly frequency for the time period before and after Hurricane Maria (September 2017) and which meets our qualification thresholds for inclusion. This excludes from the donor pool every commuting zone which borders Orlando, and nearly every CZ in Florida. Thus even if the choice to use commuting zones (broadly, semi-contained local economies) as the unit of interest does not alone ensure that imposing SUTVA is reasonable, the restrictions for inclusion in our donor pool likely do—particularly given the highly local nature of our NAICS sectors of interest.

Second, our estimator imposes the first four constraints considered by Doudchenko and Imbens (2016):

(i) No-intercept: $\hat{Y}^{N}_{0,t} = \mu + \sum_{j=1}^{J} w_j Y_{j,t}$, $\mu = 0$

(ii) Adding-up: $\sum\limits_{j=1}^{J} w_j = 1$

(iii) Non-negativity: $w_j \ \geq \ 0 \ \ \forall \ \ j \in \{2, ..., J+1\}$

(iv) Exact-balance: $Y^{N}_{0,t} = \mu + \sum\limits_{j=1}^{J} w_j Y_{j,t} \ \ \forall \ \ t \ \leq \ T_0$

In the literature these constraints are commonly imposed on synthetic control estimators, and Abadie et al. (2015) emphasize the value of the non-negativity condition, in particular, as it ensures estimates of the $Y^{N}_{0,t}$ are not subject to potential extrapolation bias and helps preserve interpretability (see, also, Abadie (2019)).

Third, for each of our S observed outcomes of interest, $\tilde{Y}^{(s)}_{j,t}$, $s \in \{1, ..., S\}$, in each CZ j and each period t, our LHS values are the residualized outcomes $Y^{(s)}_{j,t} = \tilde{Y}^{(s)}_{j,t} - \hat{\tilde{Y}}^{(s)}_{j,t}$ cleaned of local seasonal variation and local intercept[15]. For notational parsimony, we hereafter drop the superscript (s).

We consider September 2017 as our first treated period, $T_0 + 1$, for our monthly-observed outcome variable (employment in the QCEW), and Q3 2017 as our first treated period for all

[15]This transformation is performed from J individual OLS regressions of a model $\tilde{Y}^{(s)}_{j,t} = \beta^{(s)}_{j} + \pi^{(s)}_{j,p} + \varepsilon^{(s)}_{j,t}$, estimated separately for each j and each s, where p is the quarter (or month) associated with each t, as appropriate for each $\tilde{Y}^{(s)}_{j,t}$ given the data source. $\beta^{(s)}_{j}$ is the local intercept for each $\tilde{Y}^{(s)}_{j,t}$ in commuting zone j, and $\pi^{(s)}_{j,p}$ is the seasonal element of $\tilde{Y}^{(s)}_{j,t}$ in j associated with t. That is, $\pi^{(s)}_{j,p}$ is the quarterly (or monthly) fixed effect associated with t in j, for each $\tilde{Y}^{(s)}_{j,t}$. Then $\hat{\tilde{Y}}^{(s)}_{j,t} = \hat{\beta}^{(s)}_{j} + \hat{\pi}^{(s)}_{j,p}$ is the sum of the values of the associated coefficient estimates. The primary purpose of this effort is to deseasonalize our variables to provide greater approximate homogeneity across commuting zones in each period (allowing us to work with outcomes in particular industries *net* of estimated local seasonal fluctuations). This effort also yields our outcomes of interest *net* of estimated local intercepts given local seasonality.

other variables in our primary analysis and all variables in our secondary analysis, which are each observed on a quarterly basis. The hurricanes hit Puerto Rico in September 2017. In our primary specifications we begin our pre-treatment period at the beginning of 2014, when the recovery from the Great Recession had firmly taken root. This avoids the labor market dynamics that occurred around and right after the great recession which may have differentially affected some of the $J+1$ labor markets considered.

We want our synthetic controls to be chosen to match economic trends prevailing in the years before the treated period, so as to ensure they are valid counter-factual observations of Orlando's outcomes in the treated period. As Orlando's construction and accommodation & food services sectors are some of the largest local employment sectors, and are also the largest non-agriculture employers of Hispanic workers (Bureau of Labor Statistics, 2015), in addition to the donor pool restrictions described earlier, for all outcomes we include as predictor variables in X_0 quarterly local employment in NAICS sectors 23 (Construction) and 72 (Accommodations & Food Services) as proportions of total local employment averaged over the pre-treatment period. These are our covariates. We also include, as additional predictors, the pre-treatment values of the outcome at quarterly intervals in 2016 and 2017 (to capture any residual seasonal fluctuation in the outcome variable), as well as from the first period in 2014 and 2015 and the middle of 2014 (to capture annual movement), ensuring we also leave out many of the pre-treatment values of the outcome as predictors to allow our selected covariates to achieve positive weights (i.e. from estimation of the matrix of predictor weights, $\mathbf{V}$. See Kaul et al., 2015).[16] This is a total of $K = 11$ predictor variables in our primary specification. For each specification of interest and given $\hat{\mathbf{V}}$, the existence of sparse solutions to 3 with no more than $K+1$ 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 $\mathbf{X_0}$ are in general position (see Abadie and L'Hour, 2019).

[16] Kaul et al. (2015) argue that including the full set of pre-treatment outcomes as predictors will result in certain zero $\hat{v}_k$ weights for any additionally-included covariates, regardless of how important these covariates may actually be for predicting future values of the outcome variables. This can bias estimated treatment effects if the covariates matter for predicting the outcome and if the treated unit's distance from its synthetic control donors is large with respect to these covariates.

For our primary analysis using the QCEW data, we have $T_0 = 44$ pre-treatment periods (months) and consider $T - T_0 \in \{6, 12\}$ treated periods (half a year and one year after the hurricanes hit Puerto Rico) for our outcomes observed on a monthly basis (namely employment). For the quarterly-observed outcomes, we have $T_0 = 14$ pre-treatment periods and consider $T - T_0 \in \{2, 4\}$ treated periods. In our alternative analysis using the QWI data, in which all outcomes are observed quarterly, we have $T_0 = 14$ pre-treatment periods. Missing observations in certain states in the QWI data after Q1 2018 arbitrarily restrict our donor pool CZs in order to ensure that we have $T - T_0 = \{2, 4\}$ treated periods in which outcomes for all N commuting zones are observed.

3.3.2 Inference

For each outcome of interest, the estimation strategy described above yields a vector of estimated treatment effects of the inflow of Puerto Rican refugees to Orlando, $\hat{\alpha}_{\mathbf{1}}$, the elements of which are $Y_{1,t} - \hat{Y}^N_{1,t} \;\; \forall \;\; t \in \{T_0 + 1, ..., T\}$. In practice, we have $Y_{1,t} - \hat{Y}^N_{1,t} \;\; \forall \;\; t \in \{1, ...T\}$. This case study contributes new considerations and new evidence to the limited literature on the local labor market effects of sudden immigration waves in the US. The existing literature involving the United States has focused on the old case of the Mariel Boatlift, which occurred in 1980. As discussed earlier, due to measurement error and small samples these previous studies disagree about the effects of those immigrants—who were primarily low-skilled—on local labor market outcomes. When looking at the impact of the Marielitos on native workers in Miami, Card (1990) finds essentially no impact—a finding affirmed by Peri and Yasenov (2019)'s synthetic control approach. Borjas (2017), on the other hand, found a negative impact. It is thus natural to posit a null hypothesis of no effect[17] for each commuting zone $j \in \{1, ..., J + 1\}$, and each time period $t \in \{T_0 + 1, ..., T\}$:

$$H_0 : Y^I_{j,t} = Y^N_{j,t} \tag{4}$$

Hypothesis testing using a synthetic control approach comes with unique challenges, as this method does not produce standard errors. Large-sample inferential approaches are also broadly not appropriate, as synthetic control estimators typically use a decidedly finite J and T. Indeed,

[17] We consider an alternative null hypothesis in Section (5)

as Abadie et al. (2015) point out, including too many units J in the donor pool may lead to over-fitting if the estimator assigns small, positive weights to substantial number of them, leading to the construction of the synthetic control from artificial matches on idiosyncratic variations in the pre-period. Additionally, as explained above, in our primary specification we limit our period of observation to begin in 2014 to ensure matching is not dependent on local fluctuations (seen during recovery from the Great Recession) which do not reflect local economic conditions in the treated period, limiting our T. We thus conduct inference using an alternative approach.

The literature on conducting inference with synthetic control estimators is relatively young and rapidly evolving (see, for example, Abadie et al., 2015; Doudchenko and Imbens, 2016; Hahn and Shi, 2017; Ferman and Pinto, 2017; Chernozhukov, Wuthrich and Zhu, 2017; Firpo and Possebom, 2018; and Abadie and L'Hour, 2019). Most of these proposed approaches involve the construction of a test statistic based on some form of falsification- or "placebo"-tests, such as the *in-time* or *in-space* placebo tests proposed in Abadie et al. (2015). Firpo and Possebom (2018) refrain from endorsing a particular approach to inference, but demonstrate in a series of simulations that, under limited restrictions on the data generating process, the in-space test statistic based on the ratio of the treated-period mean squared prediction error (MSPE) to the pre-treatment-period MSPE, which we refer to as the RMSPE, is uniformly the most powerful test statistic among those tested. A 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 the pre-treatment fit is poor, obviating the need to remove donor pool CZs which have a 'poor' pre-treatment fit according to some subjective criteria. We therefore test our null hypothesis (4) by constructing our test statistic as follows.

We begin by repeating our 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 $j \in \{2, ..., J+1\}$ *untreated* CZs in our donor pool to estimate $\hat{Y}_{j,t}^{N}$ $\forall$ t. For each $j \in \{1, ..., J+1\}$ we then calculate the summary statistic:

$$RMSPE_j = \frac{\sum_{t=T_0+1}^{T}(Y_{j,t} - \hat{Y}_{j,t}^{N})^2/(T - T_0)}{\sum_{t=1}^{T_0}(Y_{j,t} - \hat{Y}_{j,t}^{N})^2/T_0} \quad (5)$$

Our test statistic is then constructed as a p-value based on the empirical distribution of these $RMSPE_j$:

$$p = \frac{\sum_{j=1}^{J+1} \mathbb{1}[RMSPE_j \geq RMSPE_1]}{N} \tag{6}$$

As a reminder, $N = J + 1$ and our actual treated unit (Orlando) is $j = 1$. Thus if the deviations between our 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 (4).

In the following section we present the results of our primary and secondary analyses. For our primary analysis, we use the QCEW data to estimate the treatment effects of the inflow of Puerto Rican refugees for Orlando, as well as placebo estimates for each of the CZs in the associated donor pool, for each of the sectors and LHS variables described above. P-values for the associated RMSPEs are calculated as described above, and statistical significance is then attributed to each estimated treatment effect according to these p-values. We then present the details and results of our secondary analysis, in which we use the QWI data to focus on the impact on non-Hispanic and less-educated workers. We follow that with Section (5), which presents the details and results from a series of robustness checks—including an alternative definition of the treatment effect, an extended pre-treatment period, and an effort to account for any confounding impact of Hurricane Irma on Orlando's labor markets.

4 Results

4.1 Primary Analysis

Our approach is illustrated by first considering Figure A4a, which plots the observed (de-seasonalized natural logarithm of) total employment in Orlando against that of its synthetic control. We set the value to zero at the beginning of our pre-treatment period, which is January 2014. The period in which the hurricanes hit (the initial period of treatment), September 2017, is shown shaded. The months immediately following the shaded area are those in which Orlando experiences the largest inflow of people from Puerto Rico fleeing hurricane Maria.

The gap between employment in Orlando and its synthetic control in each period is then calculated and plotted in Figure A4b, along with the full set of donor pool placebo gaps. As a particularly apt comparison, we also highlight Los Angeles (L.A.) on these graphs as, like Orlando, it is a relatively tourism-intensive, mild-climate economy populated by a large Hispanic population, but did not receive many Puerto Rican refugees after Hurricane Maria devastated the island. Orlando appears to match its synthetic control well in the pre-treatment period, and the marginal treatment effects (the gaps in the treated period)—especially in the late months of 2017 and early 2018—appear large, relative to the synthetic control. This is a sign that employment in Orlando increased in a substantial way after the Puerto-Rican evacuees arrived. We approximate this increase to be around 0.4 percent of employment—a figure nearly exactly what we would expect, given the number of Puerto-Ricans we estimate to have arrived in Orlando, if they participated in the local labor force in equivalent proportions to earlier Puerto Rican arrivals and if those participants were fully absorbed into the local labor market. We also observe a one-month dip in employment exactly in September 2017, before the increase. While we cannot be absolutely certain, it is very likely that this dip is due to the impact of Hurricane Irma, which hit Orlando and disrupted its economic activity for a number of days that month. We note that, as would reasonably have been predicted given our set up, there is no hint of any effect in L.A.

Figure 3 highlights the commuting zones included in this particular donor pool, and makes clear which nine were assigned positive weights and thus contribute to our synthetic control for this outcome (aggregate deseasonalized log employment). 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%). Comparing this map with Figure 1, we can see that none of the donor CZs received any meaningful number of evacuees (assuming FEMA applications are a good proxy for the number of evacuees).

We also present the analogs of Figures 2a & b, below, for (deseasonalized log) employment in the construction, retail, and accommodations & food services sectors respectively. Figures showing compensation-per-worker, both in aggregate and in these sectors, are presented in the appendix.[18] We note the particularly large and sustained jump in construction employment in Orlando following the hurricanes, as well as the smaller one in retail employment.

Based on the empirical distribution of these estimates, as well as those for total compensation, compensation per worker, and total establishments, we calculate our test statistic as described in Section (3) and present all results in Table 2. Our focus is the estimated treatment effects of the inflow of Puerto Rican refugees into Orlando on (log) employment in columns (1) and (2), and (log) compensation per worker in columns (5) and (6), both 6 and 12 months after the hurricanes. We also present estimates for total compensation and total establishments. While log compensation per worker is our preferred measure of individual earnings, we include estimated effects on total compensation paid as an immediate robustness check (alongside the estimated effects on employment) for our earnings estimates. Broadly speaking, we expect any robustly estimated effect on per worker earnings to be corroborated by the estimated effect on industry-level earnings. For each outcome of interest we present the exact number of CZs in each donor pool, which varies by industry because of occasional suppressions in the raw data for confidentiality reasons.

The first set of estimates presented in Table 2 are for the Orlando economy as a whole. We estimate that the inflow of Puerto Rican refugees caused aggregate employment in Orlando to grow by a significant 0.2 percent after 6 months, and by a significant 0.4 percent after 12 months. As discussed earlier, we estimated that approximately 24,000 Puerto Ricans arrived in Orlando over the 6 months following the hurricanes, 11,000 of whom can be attributed to the hurricanes, constituting 0.65 percent of the pre-treatment working-age population. In the years preceding the hurricanes, around 75 percent of Puerto Ricans in Orlando (and more widely) were aged 16 and over, around 60 percent of whom were employed (U.S. Census Bureau, 2017). If these patterns persisted among the 11,000 refugees, this would amount to the new working-age refugees participating in the Orlando labor force constituting a 0.3 percent increase in the local labor force. Thus our estimated treatment effects are highly consistent with our story that those refugees who entered the Orlando labor force were fully absorbed by the Orlando economy, likely causing small, positive spillovers on

[18] Maps of the donor CZs by weight, as well as analogs to Figures 2a & b showing total compensation and the number of establishments, in aggregate and these sectors, are available from the authors upon request.

the employment of local residents. We also find no evidence of a negative impact on compensation per worker in Orlando as a result of this positive shock to the local labor supply.

The rest of Table 2 explores the sector-specific treatment effects. We begin with the Construction sector (NAICS 23), which we have argued was the sector most likely to absorb these refugees, many of whom were unlikely to be fluent in English. While we estimate a 1.5 percent increase in Construction employment 6 months into the treated period, this estimate is not significant at the 10 percent level—though it is close (p-value of 0.11). However, our estimated treatment effect of a 4 percent increase in construction employment 12 months into the treated period is highly significant (p-value of 0.01). This growth in construction employment would account for a full 80 percent of the employed refugees if their age and employment patterns were similar to those of the Puerto Ricans who preceded them, which would mean nearly all of the labor supply shock was absorbed by the construction sector. While compensation per worker was stable over the first 6 months of treatment, we estimate it grew by a significant 3.3 percent 12 months after treatment began (p-value of 0.05), and is corroborated by the estimate for total construction compensation. Thus this large increase in the construction-sector workforce did not have any negative effect *overall* on construction-sector compensation per worker, and may have resulted in higher overall construction earnings—though we urge caution when interpreting this particular result given the results from our secondary analysis and robustness checks do not suggest it is robust.

We next turn to Retail (NAICS 44-45) and Accommodation & Food Services (NAICS 72). While there is not perfect overlap, we note that the vast majority of jobs in these sectors tend to broadly share the same pool of labor because they do not require a particularly unique set of skills—though wages tend to be slightly higher for retail workers. This is, perhaps, because front-line retail-sector jobs are slightly less routine and require somewhat more flexible interaction with customers, and broadly require a better grasp of conversational English. We estimate that retail-sector employment grew by, respectively, a significant 0.3 percent and 0.9 percent, 6 months and 12 months after treatment began (p-values of 0.04 and 0.06, respectively). Compensation per worker was not affected. In accommodations & food services, employment was unaffected but we estimate that compensation per worker grew by a significant 1.4 percent 12 months after treatment began (p-value of 0.03).[19]

We hypothesize that these patterns may have emerged because the arrival of the refugees may have constituted a shock to local demand for consumer goods and services, which may have been more impactful than any labor supply shock associated with some of the refugees finding jobs in these sectors. As retail-sector jobs are slightly better-paying, retail employers would have had the advantage in attracting new employees to help them meet the increased demand (assuming the workers are drawn from the same labor pool). Employers in the accommodations & food services sector would then have had to raise wages to compete with these retail-sector employers in order to simply maintain staffing levels. While this story is plausible and consistent with the patterns of our estimated treatment effects, our data do not allow us to test it directly.

Overall, Table 2 paints a picture of the Puerto Rican refugees being absorbed into the Orlando labor market quite effectively, and even suggests there were significant positive spillovers onto other sectors which likely did not absorb these new arrivals. The employment effects of the labor supply shock were particularly large in the construction sector, which we argued earlier was the most likely place for the refugees to find work. The effects of the consumer demand shock were an increase in retail employment, and an increase in earnings in the accommodations and food services sector. We find no evidence of significant changes in the number of local establishments—though the point estimates are all positive—and thus cannot draw any conclusions about the impact of this migration event on local investment.

4.2 Secondary Analysis: Non-Hispanic and Less-educated Workers

An ongoing question in the economics of immigration literature is to what extent immigrants impact the wages of natives. To study this question, we use data found in the Quarterly Workforce Indicators (QWI) to focus on potential effects on wages of particular groups of workers in the Orlando area after the migration event. Similar to the QCEW data used in our primary analyses, the QWI collects information on employment and wages for each industry and county, although the employment figures are only observed quarterly and not monthly as in the QCEW. The QWI,

[19]We estimate compensation per worker grew by a statistically significant 0.01 percent 6 months after treatment began, but view this estimate as too small to be considered economically significant.

however, allows us to condition on various worker characteristics, such as ethnicity and educational attainment, when studying county- (or commuting zone)-level employment and wages. One cost of using the QWI for these analyses is that we lose about 20 percent of our sample when looking 12 months after the hurricane, primarily for issues around data-reporting from particular states. For this reason we are not as confident in these sets of results as in those using the more complete QCEW data.

In order to estimate whether the wages of native workers were impacted by the new workers from Puerto Rico who entered the Orlando Construction sector after the hurricane, we use the ACS to characterize workers in the construction sector in ways that allow us to exploit the features of the QWI data. Focusing first on ethnicity, we see that in the five years prior to the hurricane, 65 percent of workers in the construction sector identified as "non-Hispanic". We also see that, of those non-Hispanic workers, 88 percent are "native"—meaning they were born in one of the 50 states in the U.S. If the Puerto Rican migrants that were absorbed by the construction sector had any impact on the wages of natives in that sector, we should be able see it in this subgroup[20].

We thus repeat our primary analysis using the QWI to explicitly focus on the impact on non-Hispanic workers. The results are presented in Table 3. Similar to the construction sector, the vast majority of non-Hispanic workers in Orlando are natives, so we view the impact on non-Hispanics as, effectively, the impact on non-Hispanic natives of an inflow of predominantly Spanish-speaking migrants. Focusing on the Orlando area, we find evidence of a significant positive overall impact on non-Hispanic employment of 0.8 percent percent 12 months after the hurricane. Focusing on Orlando-area non-Hispanic workers in the retail sector, we see a significant increase in total non-Hispanic compensation of over 4 percent 12 months after the hurricane, and our point estimate of the effect on compensation per worker is positive 2 percent, though this latter estimate is not statistically significant. Shifting focus to those working in the construction sector, we find no evidence at all that non-Hispanic construction employment was negatively affected by the large supply shock (our point estimate is actually positive 0.7 percent, though it is not statistically significant). We do

[20]This assumes that Puerto Ricans are highly likely to identify as "Hispanic"; otherwise, our focus on non-Hispanics might be picking up people who migrated to Orlando from Puerto Rico after the hurricane. Using data from the PRCS, we see that just over 1 percent of people in Puerto Rico identify as "non-Hispanic".

find that the large inflow of migrants into construction led to a 2.5 percent decrease in non-Hispanic construction-sector compensation and compensation per worker. Estimates from our primary analysis allow us to place this change in non-Hispanic construction-sector compensation in terms of elasticities, where we define the elasticity of x with respect to y as $E_{xy} = \frac{d\,log(x)}{d\,log(y)}$. Taking the ratio of this decrease in non-Hispanic log construction-sector compensation per worker (-0.0245, from Table 3) and our primary estimate of an unconditional overall increase in log construction-sector employment (0.0402, from Table 2), we calculate that the elasticity of non-Hispanic construction earnings with respect to overall construction employment is -0.61, which is inelastic—that is, it is not particularly responsive. In our setting, where the increase in construction labor supply is due to migrants, we see this as evidence that non-Hispanic construction earnings are largely inelastic with respect to migrant labor supply. We also note the disagreement with our primary estimate on overall construction earnings (which was a positive and significant 3.3 percent), and again urge caution when interpreting either result.

Again using the ACS to look at the characteristics of workers in our sectors of interest before the hurricanes, we observe that 65 percent of workers in construction have a high school degree or less. Looking at retail and accommodations & food services, workers are about just as likely to have a high school degree or less education as they are to have at least some college (49 percent and 45 percent have a high school degree or less, respectively). We also see that for workers in construction with an education of high school degree or less, 60 percent are native, while for retail and accommodation & food services the numbers are 71 percent and 64 percent, respectively. By the same argument as above, looking at the impact on workers with a high school degree or less will help us identify the impact on native workers.

To focus explicitly on the impact of less-educated workers (those with a high school degree or less education), we again repeat our primary analysis using the QWI, though as noted earlier the education levels of workers under age 25 are not observed, meaning this part of our analysis is limited to non-youth workers. Table 4 presents the results. We find no evidence of a significant impact on overall employment or compensation per worker among less-educated workers 6 months after the hurricane. Looking 12 months after the hurricane, however, where we find most of our other results, we see a significant 0.8 percent increase in overall employment for this subgroup (p-value of 0.04), as well as an increase in retail sector earnings of 1.2 percent (which is half as large and still significant 6 months after the hurricane began. The p-values are both 0.02). We have confidence in the robustness of this latter estimate because of the large, significant estimated effect on *total* retail compensation for less-educated workers (3.3%, p-value ¡ 0.01). Construction-sector earnings for less-educated workers are estimated to have decreased by 1.5 percent (p-value of 0.06), but we see no evidence of change in either total less-educated construction compensation or employment (the point estimate of a 1.7 percent increase in less-educated construction employment is not significant, with a p-value of 0.33). If we believe these results, the associated elasticity of less-educated construction earnings with respect to overall construction employment would be -0.37, which is highly inelastic. Finally, we also see that overall compensation per worker among less-educated workers is estimated to have decreased by 2 percent (p-value of 0.06). We view this result with some skepticism as there is no suggestion that total *overall* compensation decreased (the point estimate there is an insignificant 0.9 percent with a p-value of 0.26), and because it is not particularly precise, though it is not inconsistent with the simplistic canonical model of labor supply and labor demand given the significant increase in total labor supply which we observe in post-Maria Orlando.

Overall, our secondary analysis demonstrates that the large inflow of migrants from Puerto Rico led to an increase in overall jobs for both non-Hispanic and less-educated workers, and even increased earnings for less-educated workers in the retail sector. We also see decreases in earnings for non-Hispanic construction workers and possibly for less-educated workers, but find that these earnings responses are broadly inelastic (unresponsive) with respect to the increase in overall construction employment. We interpret these results as evidence that this large inflow of migrants had an overall positive impact on native workers through the consequent positive shock to consumer demand, though native workers in the sector most heavily exposed to the labor supply shock did face some downward pressure on earnings.

We also note that, as discussed, data access issues from certain states placed an artificial restriction on our donor pool for this secondary analysis. The number of donor pool CZs plus

Orlando, N, is 10-30 fewer for our secondary analysis than for our primary analysis using the QCEW. In the appendix, in Tables A2 and A3, we present estimates using the unrestricted donor pool but which was only observed up to Q1 2018, 6 months after the hurricanes hit Puerto Rico. These 6-month estimates are broadly consistent with the 6-month estimates from our restricted donor pool in Tables 3 and 4, above.

5 Robustness Checks

In addition to our primary and secondary analyses, we consider a number of alternative approaches to test the robustness of our results, and use the QCEW data for each analysis.

5.1 Alternative Hypothesis Test

In Section (3) we proposed the *exact null hypothesis* (4), $H_0 : Y^I_{j,t} = Y^N_{j,t}$. We may alternatively wish to account for any differences in pre-treatment outcomes between our observed unit and its synthetic control by testing the more flexible *sharp null hypothesis* proposed by Firpo and Possebom (2018):

$$H'_0 : Y^I_{j,t} = Y^N_{j,t} + f_j(t)$$

which nests the *exact null hypothesis* (4) when $f_j(t) = 0$. H'_0 could be adapted to consider a number of null hypotheses.

As our first robustness check, we consider one such alternative null hypothesis: for each j, we alternatively define our treatment effects as the difference between our observed outcomes and the synthetic control outcomes *minus* that difference in the period immediately prior to treatment. That is, we define $f_j = Y_{j,T_0} - \hat{Y}^N_{j,T_0} \ \forall \ j$. Our alternative estimated treatment effects are then $\hat{\alpha}'_{1,t} = Y_{1,t} - \hat{Y}^N_{1,t} - f_0 \ \forall \ t$, which effectively shrinks the magnitude of any estimated treatment effect by the magnitude of any positive gap in T_0 (and grows it by the magnitude of any negative gap in T_0), defining the treatment effect as the change in the gap between T_0 and T. The associated sharp null hypothesis is then:

$$H'_0 : Y^I_{j,t} = Y^N_{j,t} + f_j \text{[21]} \tag{7}$$

and the p-values used to test it are calculated as in (6) but based on the empirical distribution of $RMSPE'_j$:

$$RMSPE'_j = \frac{\sum_{t=T_0+1}^{T} (Y_{j,t} - \hat{Y}^N_{j,t} - f_j)^2/(T - T_0)}{\sum_{t=1}^{T_0} (Y_{j,t} - \hat{Y}^N_{j,t} - f_j)^2/T_0} \tag{8}$$

We present the results from this analysis in Table 5. The patterns are identical to those from

[21] Note that this null hypothesis can only be formed after observing the estimated gap in T_0.

our primary analysis in Table 2, though some of the estimated treatment effects are up to 0.011 percentage points larger using this approach. This gives us confidence in the results from our primary analysis and the overall conclusions we draw from it.

5.2 Extended pre-Treatment Period

Our second robustness check extends the length of our 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. We were intentional about selecting our preferred pre-period to begin in 2014, due to concerns that the impact and duration of the recovery from the Great Recession was heterogeneous across commuting zones, but that the recovery had firmly taken root across the country by the beginning of 2014. Extending the beginning of our pre-treatment period further back in time helps test whether our results are robust to this restriction on the pre-treatment period, and also addresses concerns that our weights are not unique given the number of units J in our donor pools. It does this at the cost of introducing the possibility that the synthetic control outcomes in the treated period are less valid counterfactual observations for Orlando (as they are predicted using data from a period which we believe may be more volatile and less representative of the post-recovery norm). We accordingly conduct this robustness check by extending the pre-treatment period to the beginning of 2013, first deseasonalizing the raw data over this period, and then implementing our estimating strategy and adding one value of the outcome from 2013 to our $\mathbf{X_0}$ matrix as a predictor, as well as extending the averaging of the pre-treatment sectoral employment proportions to the beginning of 2013.

The results are presented in Table 6, their pattern being again broadly similar to that seen in our primary analysis, though the magnitudes of some have changed, somewhat. We note that estimated growth in construction employment is again statistically significant (p-value of 0.08). We also note that the nearly 3 percent estimated increase in construction compensation per worker is no longer significant (p-value of 0.18) which, along with the opposite-direction estimates of the impact on construction earnings in our secondary analysis, again suggests caution when considering the effect of the migrant inflow on this particular outcome. Nonetheless, the results again tell broadly the same story: the inflow of Puerto Rican refugees led to a substantial increase in construction-sector employment with no evidence of a concurrent decrease in construction-sector compensation per worker overall, while retail-sector employment grew without any reduction in retail compensation per worker, and compensation per worker increased in the accommodations & food services sector.

5.3 Impact of Hurricane Irma on Orlando

As a final robustness check, we test whether our estimated treatment effects may have been affected by the impact of the hurricanes in ways other than through the influx of Puerto Rican evacuees. Hurricane Irma, for example, drove up through central Florida, downing trees and power lines and causing other damage even though it substantially weakened as it moved north (see Figure 8). While the immediate local economic impact was almost certainly negative (witness the notable dip in Orlando's employment in September 2017), it is theoretically possible that the aftermath would yield movement in either direction. 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. It may also be that neither of these things happened, or that both happened simultaneously. This motivates our final robustness check. Specifically, we attempt to gauge whether our estimated treatment effects were simply or even partially the result of the aftermath of exposure to Irma—which did inflict some damage on Orlando—by looking at a large commuting zone which received relatively few Puerto Rican refugees but which, while the Hurricane was of a similar strength, lay approximately the same distance from the path taken by Irma's eye as did Orlando: Jacksonville.

Compared to the 3,972 relevant FEMA applications filed in Orlando, Jacksonville saw a relatively few 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.[22] 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 large inflow

[22] We note this number of applications still disqualified Jacksonville from inclusion in our donor pools for the analyses above.

of Puerto Rican refugees. Additionally, the gap (vs synthetic control) in Jacksonville experienced a very similar dip to the one seen in Orlando in September 2017, when both labor markets were directly exposed to Hurricane Irma (Figures 9a & b). This gives us confidence that Irma indeed affected Jacksonville similarly to the way it impacted Orlando, suggesting it is a valid counterfactual observation of the impact of Irma on Orlando in the absence of the Puerto Rican migrant inflow.

While by no means a perfect approach, under limited assumptions the estimated treatment effects in Jacksonville can suggest the magnitude and sign of the impact of Orlando's exposure to Irma, and potentially be used to correct for associated bias in our estimates. We note that Jacksonville cannot effectively measure the impact of Irma on the retail or accommodations & food services sectors because a substantial number of large retailers began new operations in Jacksonville 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 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. These retailers claimed to have hired a combined 3,000 to 4,000 people for these locations in Jacksonville during this period. Compare this to a single large retail operation opening in Orlando—an Amazon distribution center which opened in August 2018. These large-retailer openings clearly confound any effort to measure the impact of Irma on Jacksonville's retail sector. As Ikea and Walmart also host a large number of food services workers (Ikea through its own restaurants, and Walmart through both its own restaurants and through embedded franchised restaurants such as McDonald's and Subway), these openings also cause Jacksonville to fail as a counterfactual Orlando for the impact of Irma on the accommodations and food services sector. This notwithstanding, Jacksonville appears a valid counterfactual Orlando for the impact of Irma on the overall local economy and on the construction sector, which in any case is where we might expect to see any post-hurricane positive employment effects (i.e. due to repairs and rebuilding).

Table 8 presents our estimates of the effect of Hurricane Irma, overall and on the construction sector. They offer no indication of significant positive effects from Irma: the estimates are all statistically and economically insignificant six months following the hurricane. While the estimated treatment effect on construction employment 12 months after Irma is 2.4 percent, the RMSPE is quite small (0.65), indicating a substantial amount of noise in the period before the hurricane relative to the period afterward. The associated RMSPE p-value, at 0.83, makes clear this point estimate is not in any way significant when compared to the associated placebo runs (nearly 83 percent of the placebo runs yielded a larger RMSPE). Looking at the graph of the dynamic path of Jacksonville's contruction-employment gaps pre- and post-Irma, it is clear that this point estimate is not the result of a sustained increase in the post-Irma period as is seen in Orlando, but is very likely rather just noise similar in magnitude to a number of fluctuations seen in Jacksonville's pre-Irma path of gaps. These results give some validation to the idea that the estimated effects of the arrival of Puerto Rican evacuees in Orlando were not positively biased by a confounding impact from Hurricane Irma and its aftermath, as there is no evidence of such significant positive effects either in construction or in the aggregate for Jacksonville despite the similar exposure to Hurricane Irma for both cities.

6 Conclusions

In September 2017 Puerto Rico was battered by Hurricanes Irma and Maria in rapid succession, killing thousands, devastating the island's infrastructure, and leaving many of its homes uninhabitable. Faced with a long and uncertain recovery, many Puerto Ricans instead opted to move to the U.S. mainland, where as U.S. citizens they enjoy full rights to work and participate in local institutions. Using various data sources, including data from FEMA disaster relief applications, we show that rather than spreading uniformly throughout the contiguous states these evacuees tended to cluster heavily in relatively few areas, and a very large plurality ended up in in Orlando, Florida, during the few months following the hurricane. This provides a unique opportunity to measure with some precision the short-run impact of a sudden, sizeable (although not enormous) increase in predominantly Spanish-speaking labor supply on the local labor market outcomes of natives.

We aggregate the QCEW data by commuting zone, and implement a synthetic control estimation strategy to measure the impact of these migrant inflows on outcomes in Orlando. We find that overall employment in Orlando increased by 0.4 percent one year after the inflow began, and increased by 4 percent in the construction sector. We view this as evidence the local economy was able to absorb and productively use the additional labor supply brought by this large inflow of immigrants. We also find that, one year after the inflow began, retail-sector employment rose by nearly 1 percent and compensation per worker in the accommodations & food services sector grew by 1.4 percent, which we view as the result of employers responding to growth in local demand for consumer goods and services resulting from the consumption habits of these new arrivals. These results are broadly robust to a series of checks. We perform a secondary analysis with the QWI data, and find that when focusing on subgroups more likely to be native or less-educated, the migration event increased overall employment by 0.8 percent and increased earnings in the retail sector. We also see a decrease in earnings for native workers in the construction sector, which absorbed the majority of the labor supply shock.

Our results admit a story in which this migration event yielded concurrent labor supply and consumer demand shocks on the local economy which received the migrants. Orlando's construction sector, in particular, was exposed to the labor supply shock, while the retail and accommodation & food services portions of the Orlando economy were primarily exposed to the consumer demand shock. The new workers were fully absorbed by the local economy without displacing any native workers and without any overall negative effect on earnings, though there was some downward pressure on construction earnings for natives, specifically. However, native retail employment and earnings were both positively impacted, such that the overall effect on natives was to increase their employment without any clear impact on their earnings.

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7 Tables and Figures

Table 1: Difference in Means, Recent Puerto Rican Migrants vs Selected Comparison Groups

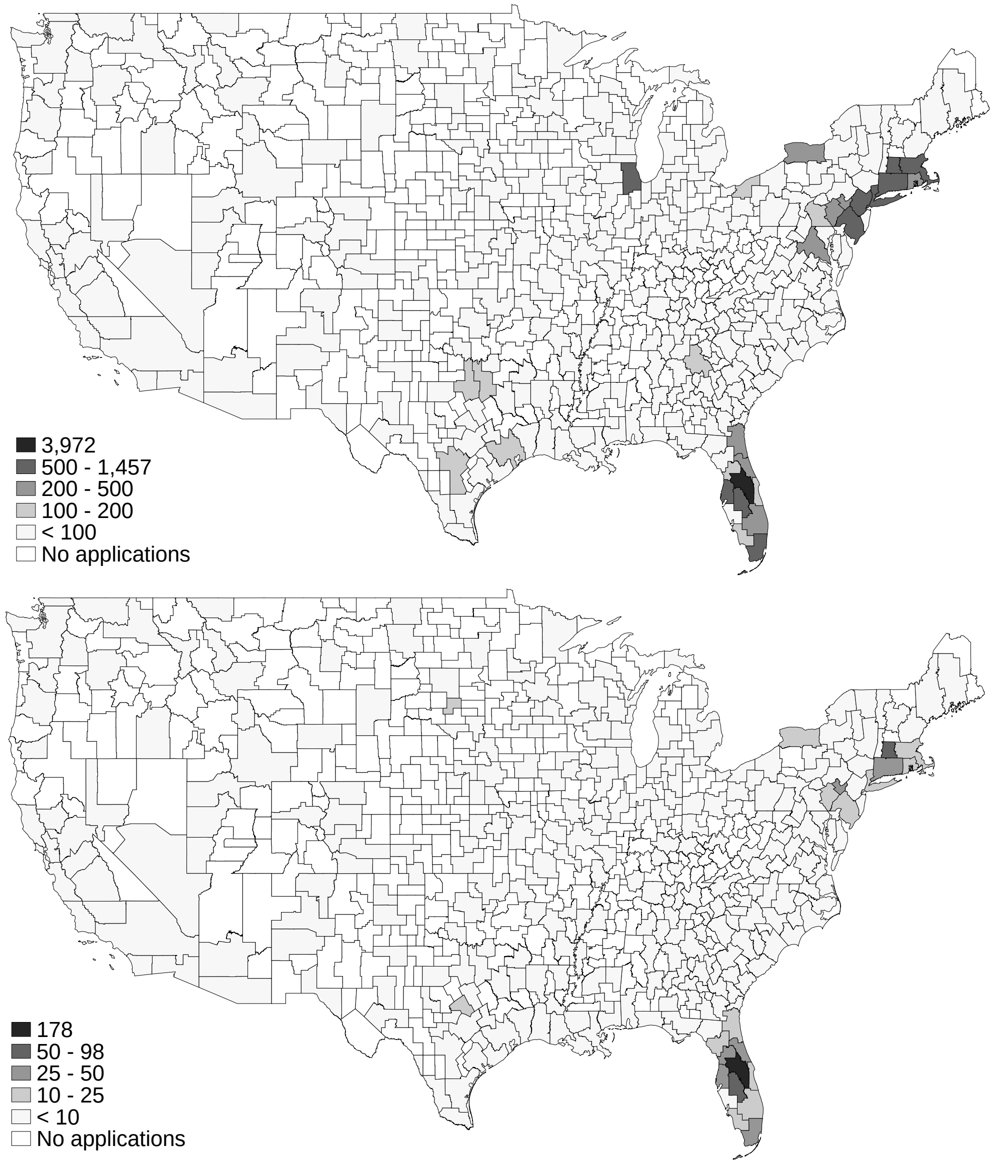
	Natives (all mainland)	Puerto Rican Islanders	Natives (Florida only)
	(1)	(2)	(3)
Age (years)	-9.383***	-7.445***	-8.827***
	(-18.29)	(-15.83)	(-9.59)
Male	0.0698***	0.0675***	0.0352
	(4.07)	(3.90)	(1.16)
Married	-0.157***	-0.0279	-0.117***
	(-9.17)	(-1.64)	(-3.84)
Yrs. education completed	-0.386***	-0.253***	-0.132
	(-5.27)	(-3.04)	(-1.02)
High school graduate	-0.0648***	-0.0272***	-0.0469***
	(-8.17)	(-2.68)	(-3.33)
4+ years of college	-0.0154	-0.0207	0.0292
	(-0.95)	(-1.27)	(1.03)
Comparison observations	6,431,276	48,855	333,182

Authors' own 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.

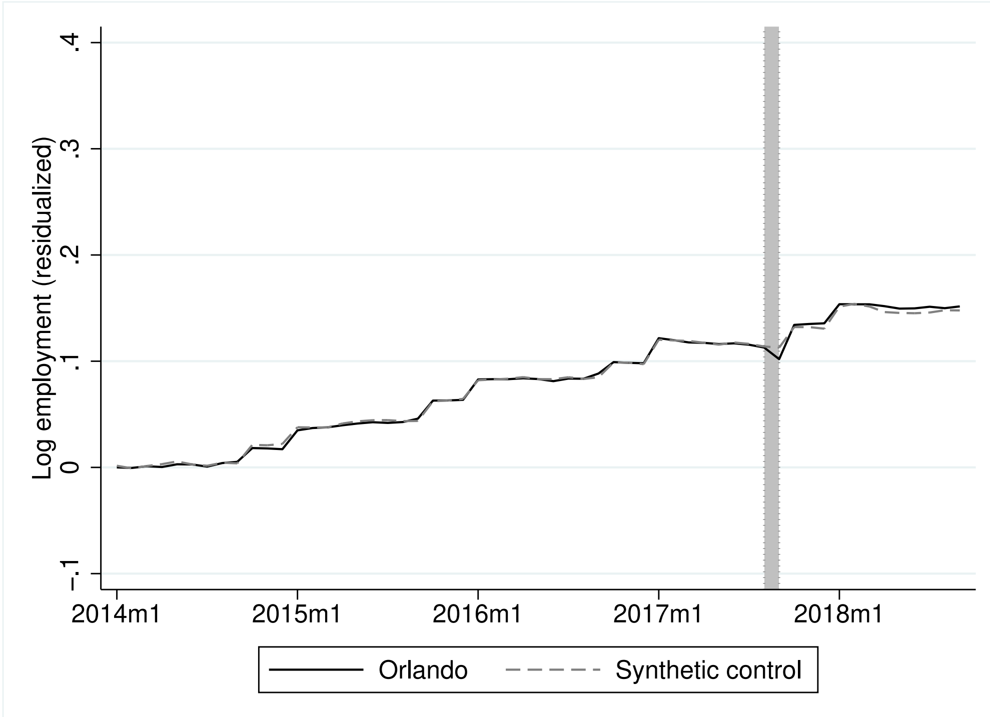
Figure 1: Puerto Rican FEMA applications by commuting zone (a) levels (b) per 100,000 population

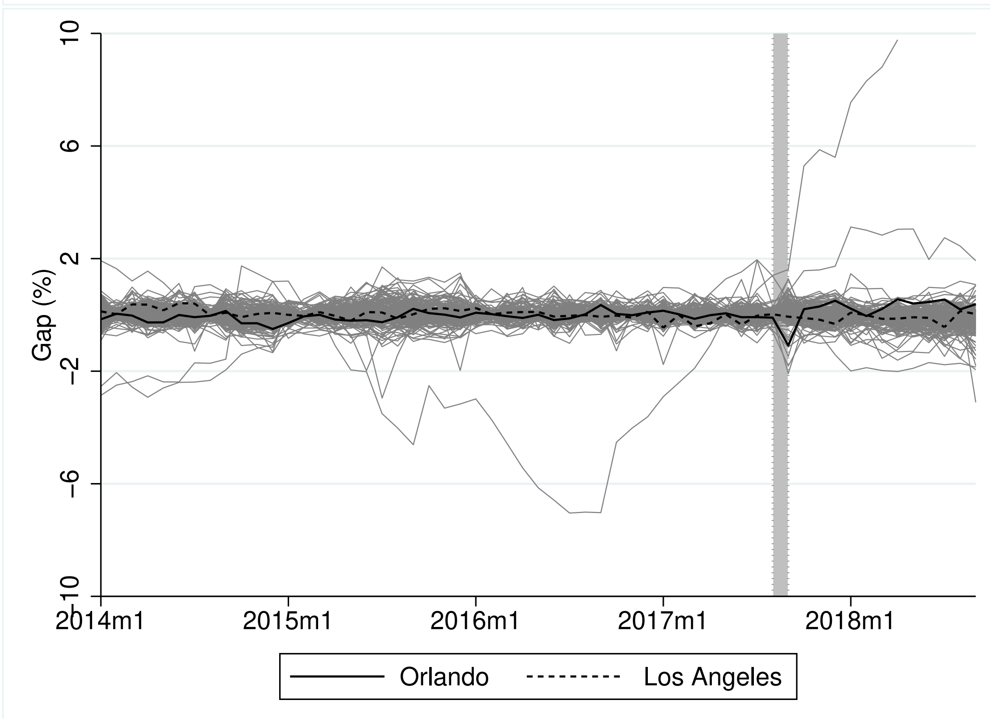


Population figures taken from the 2010 Decennial Census

Figure 2: Aggregate employment

(a) Orlando vs synthetic control (b) Gap (Orlando vs donor pool)

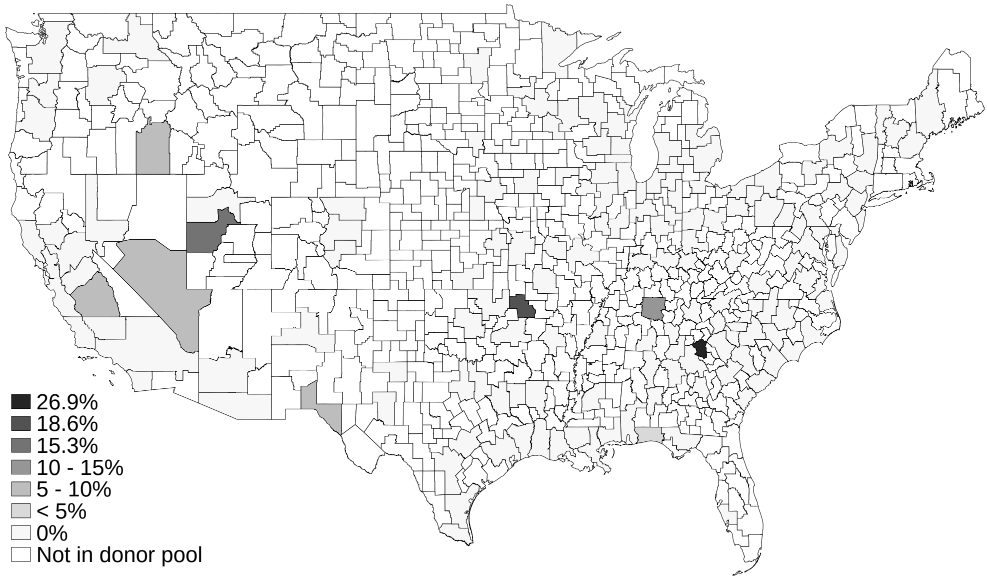




Authors' own analysis using data from the QCEW

Figure 3: Synthetic control donor pool (by weight)

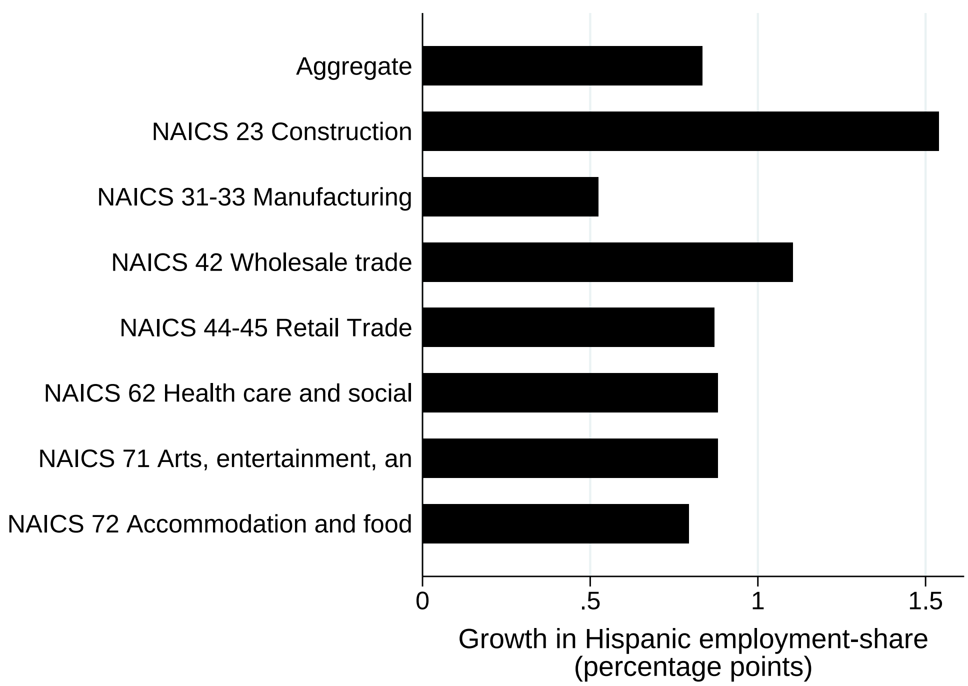
Aggregate employment



Authors' own analysis using data from the QCEW

Figure 4: Growth in Hispanic share of total employment by selected sectors

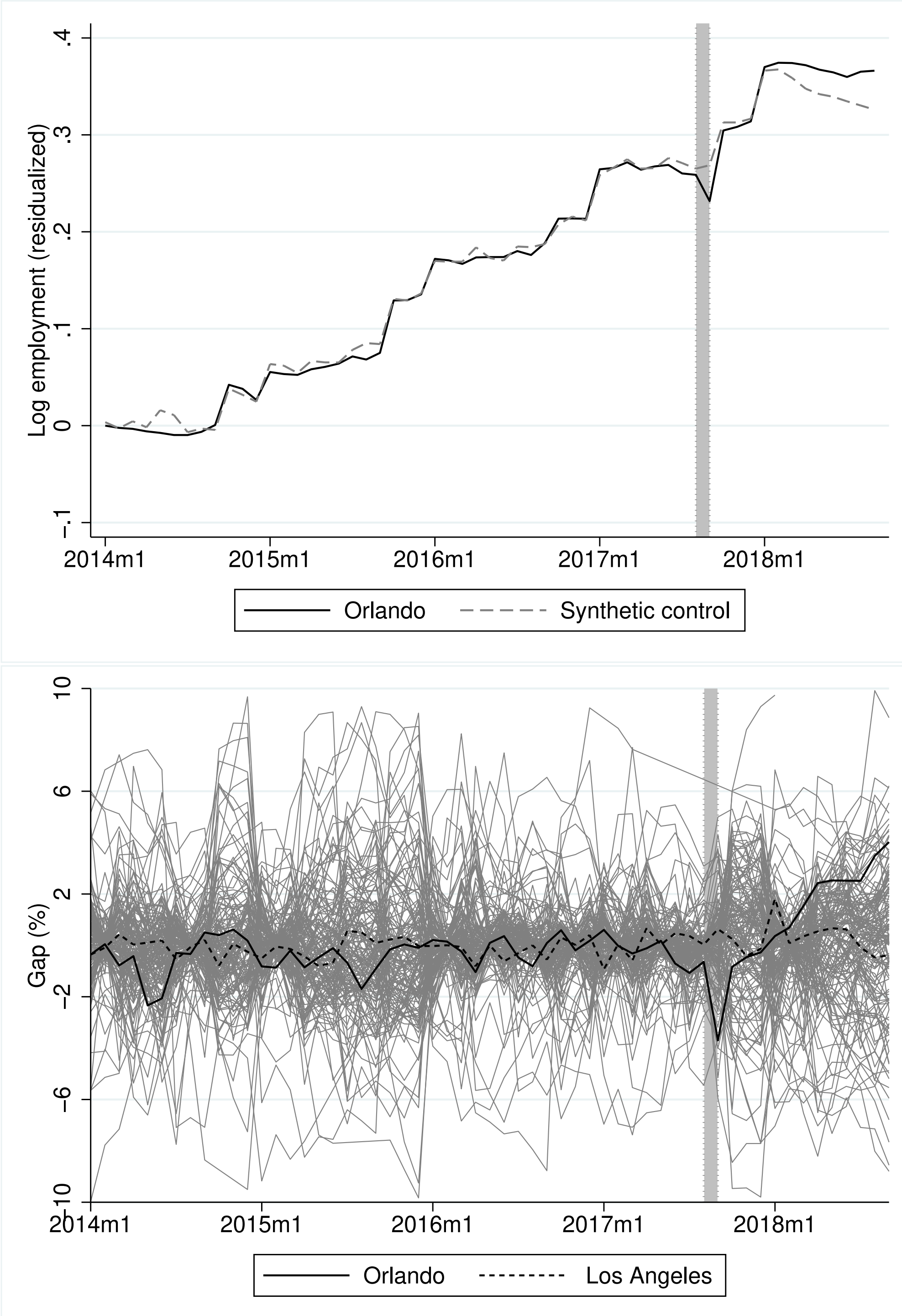
Orlando, Q2 2017 - Q2 2018



Authors' own analysis using data from the QWI.
Sector-selection reflects likelihood of absorbing English non-speaking workers.

Figure 5: Construction employment

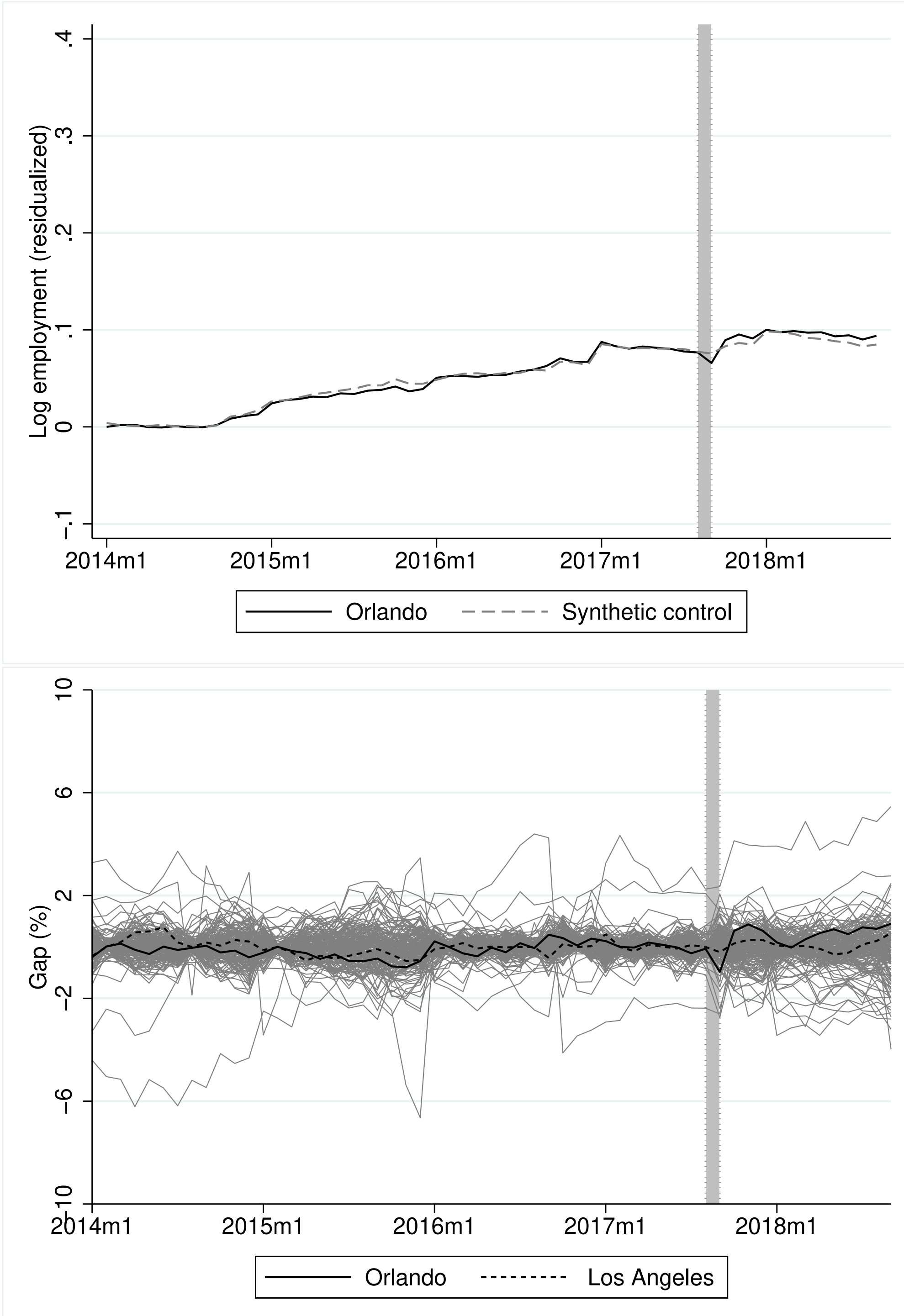
(a) Orlando vs synthetic control (b) Gap (Orlando vs donor pool)



Authors' own analysis using data from the QCEW

Figure 6: Retail employment

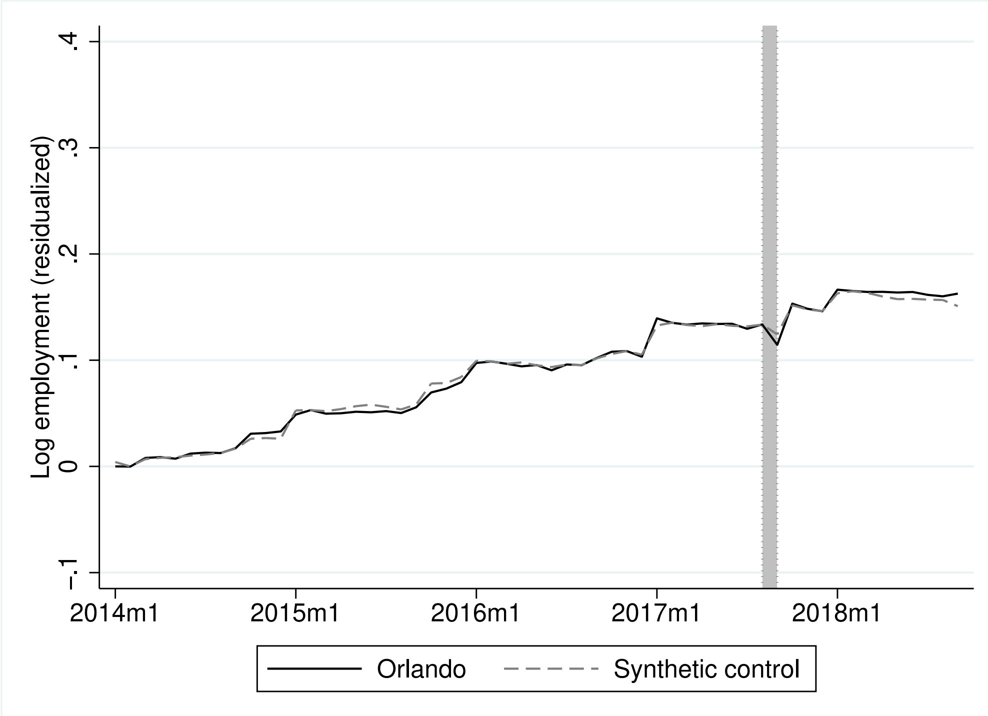
(a) Orlando vs synthetic control (b) Gap (Orlando vs donor pool)

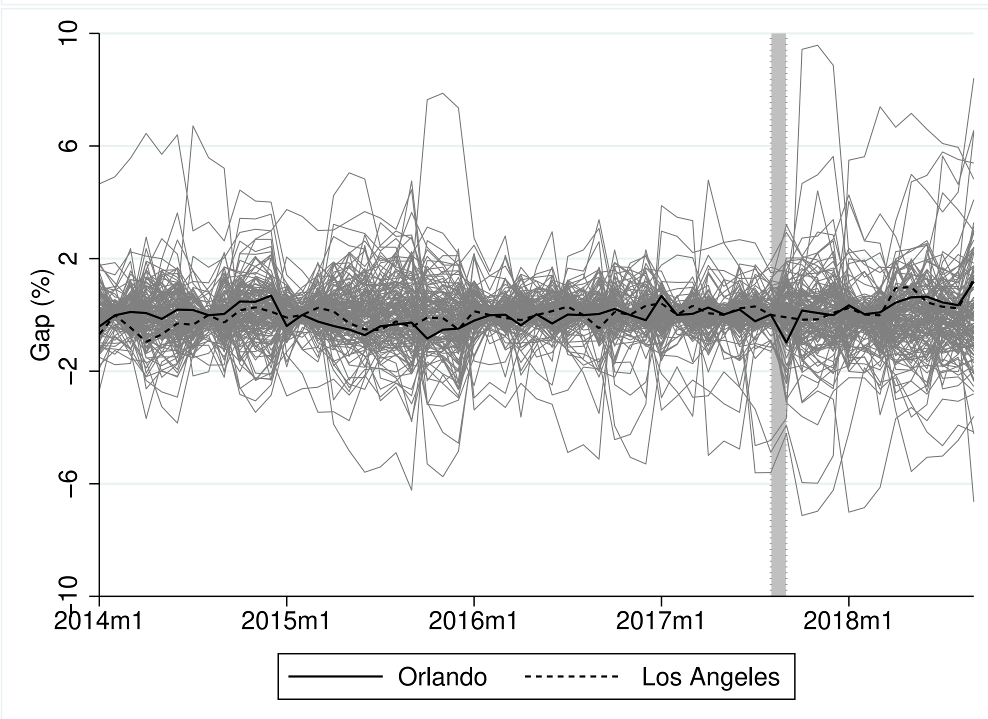


Authors' own analysis using data from the QCEW

Figure 7: Accommodations & Food Services employment

(a) Orlando vs synthetic control (b) Gap (Orlando vs donor pool)





Authors' own analysis using data from the QCEW

Table 2: Estimated Treatment Effects

All workers

Sector	Log employment		Log total compensation		Log compensation per worker		Log establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0021**	0.0038**	-0.0032	0.0055	0.0005	0.0028	0.0054	0.0062
RMSPE	4.9907	5.6548	1.1000	2.3468	1.2649	2.5392	0.5200	1.3319
p-value	0.0121	0.0121	0.5879	0.4061	0.5273	0.4000	0.6424	0.6061
N	165	165	165	165	165	165	165	165
Construction	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0153	0.0402**	0.0152	0.0386*	0.0092	0.0331*	0.0125	0.0095
RMSPE	2.7560	7.3772	3.3448	6.1653	2.6499	6.1444	1.6652	2.4089
p-value	0.1141	0.0134	0.1611	0.0604	0.2617	0.0537	0.3289	0.4430
N	149	149	149	149	149	149	149	149
Retail	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0030**	0.0090*	-0.0000	0.0001	0.0005	0.0033	0.0037	0.0159
RMSPE	2.3109	3.2463	1.6973	1.1393	0.1802	0.5209	0.0667	1.4696
p-value	0.0412	0.0588	0.3706	0.7176	0.9000	0.9588	0.9765	0.6882
N	170	170	170	170	170	170	170	170
Accom. & Food Svcs	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0009	0.0118	-0.0017***	0.0193***	0.0014**	0.0141**	0.0025	0.0076
RMSPE	0.8354	1.9175	29.5804	29.3914	8.5740	9.0942	0.0638	0.4569
p-value	0.3986	0.2635	0.0068	0.0068	0.0270	0.0338	0.9595	0.9054
N	148	148	148	148	148	148	148	148

Authors' own analysis using data from the QCEW. * Significance at the 10% level; ** significance at the 5% level; *** significance at the 1% level.

Table 3: Estimated Treatment Effects

Non-Hispanic workers (restricted donor pool)

Sector	Log employment		Log total compensation		Log compensation per worker	
	(1)	(2)	(3)	(4)	(5)	(6)
Total	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	-0.0003**	0.0082**	-0.0004	-0.0056	0.0001	0.0009
RMSPE	10.9700	16.2198	0.2596	2.8507	0.8780	1.9797
p-value	0.0448	0.0448	0.9254	0.4552	0.6791	0.6119
N	134	134	134	134	134	134
Construction	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0004	0.0068	-0.0000*	-0.0178*	0.0004**	-0.0245**
RMSPE	2.0665	3.7870	4.9478	8.4406	6.7462	7.4052
p-value	0.4141	0.3203	0.0625	0.0703	0.0234	0.0469
N	128	128	128	128	128	128
Retail	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	-0.0001	0.0009	0.0000	0.0447*	0.0036	0.0205
RMSPE	1.4148	3.4864	3.7928	9.7594	1.0713	3.0396
p-value	0.3869	0.2993	0.1679	0.0511	0.5839	0.3577
N	137	137	137	137	137	137
Accom. & Food Svcs	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	-0.0016	0.0120	0.0000	0.0085	-0.0002	0.0028
RMSPE	0.5137	0.9949	2.2818	2.0303	1.2531	3.2994
p-value	0.7883	0.8540	0.3212	0.5839	0.4818	0.2993
N	137	137	137	137	137	137

Authors' own analysis using data from the QWI.

*Significance at the 10% level; ** significance at the 5% level; *** significance at the 1% level.

Table 4: Estimated Treatment Effects

Less-educated non-youth workers (restricted donor pool)

Sector	Log employment		Log total compensation		Log compensation per worker	
	(1)	(2)	(3)	(4)	(5)	(6)
Total	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	-0.0006	0.0080**	-0.0021	0.0089	0.0008	-0.0199*
RMSPE	4.1896	9.7029	0.7285	3.9280	1.7400	7.5027
p-value	0.1765	0.0441	0.6544	0.2574	0.3750	0.0588
N	136	136	136	136	136	136
Construction	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0082	0.0166	0.0027	0.0007	0.0007**	-0.0147*
RMSPE	3.6389	3.8433	3.5327	3.2030	8.8591	8.4125
p-value	0.2857	0.3308	0.1203	0.2105	0.0226	0.0602
N	133	133	133	133	133	133
Retail	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	-0.0004	0.0089	0.0004***	0.0327***	0.0060**	0.0119**
RMSPE	0.1467	0.8946	36.8509	60.0308	6.6239	7.1356
p-value	0.9265	0.8015	0.0074	0.0074	0.0221	0.0221
N	136	136	136	136	136	136
Accom. & Food Svcs	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	-0.0041	0.0042	-0.0001	-0.0025	0.0003	0.0134
RMSPE	1.3909	1.1233	0.6426	0.6747	0.9401	1.1408
p-value	0.4962	0.8647	0.7744	0.9248	0.6165	0.8045
N	133	133	133	133	133	133

Authors' own analysis using data from the QWI.

* Significance at the 10% level; ** significance at the 5% level; *** significance at the 1% level.

Table 5: Alternative Estimated Treatment Effects

All workers

Sector	Log employment		Log total compensation		Log compensation per worker		Log establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0031***	0.0048***	-0.0042	0.0045	0.0005	0.0027	0.0024	0.0032
RMSPE	6.0573	7.9095	1.7917	3.1497	1.2984	2.5882	0.0409	0.2197
p-value	0.0061	0.0061	0.3879	0.2909	0.5030	0.3697	0.9576	0.9758
N	165	165	165	165	165	165	165	165
Construction	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0218	0.0466**	0.0262	0.0496*	0.0107	0.0347**	0.0089	0.0059
RMSPE	2.7769	10.3091	1.8373	5.3932	2.9023	7.4234	0.4355	0.6790
p-value	0.1074	0.0134	0.3087	0.0738	0.1745	0.0403	0.6980	0.8792
N	149	149	149	149	149	149	149	149
Retail	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0037**	0.0097**	0.0000	0.0002	0.0008	0.0036	0.0028	0.0150
RMSPE	2.9423	4.3453	1.6745	1.1255	0.2320	0.6319	0.0468	1.1838
p-value	0.0353	0.0471	0.3471	0.6588	0.8471	0.8706	0.9882	0.7412
N	170	170	170	170	170	170	170	170
Accom. & Food Svcs	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0008	0.0117	-0.0007***	0.0203***	0.0013**	0.0140**	-0.0020	0.0031
RMSPE	0.8249	1.8271	22.3655	23.5570	8.7727	9.2822	0.0983	0.1300
p-value	0.3378	0.2703	0.0068	0.0068	0.0270	0.0338	0.9257	0.9865
N	148	148	148	148	148	148	148	148

Authors' own analysis using data from the QCEW. * Significance at the 10% level; ** significance at the 5% level; *** significance at the 1% level.

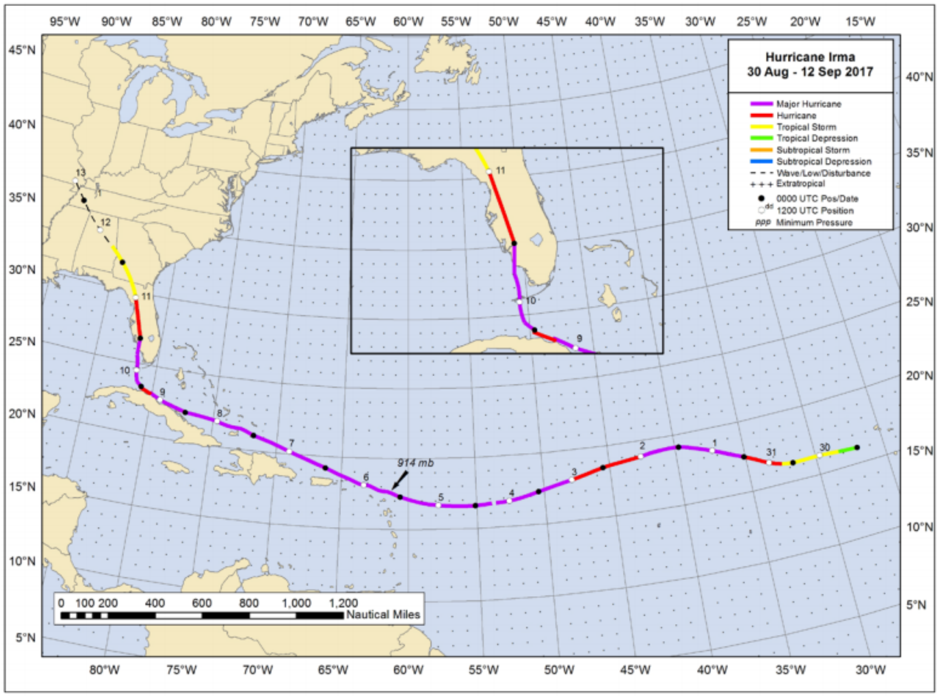
Table 6: Estimated Treatment Effects

Extended pre-treatment period

Sector	Log employment		Log total compensation		Log compensation per worker		Log establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0010**	0.0065**	-0.0052	0.0058	0.0012	0.0031	0.0066	0.0060
RMSPE	4.2303	4.6431	1.9364	2.6054	1.3734	3.4682	0.6513	1.5101
p-value	0.0305	0.0488	0.4451	0.3720	0.6037	0.2683	0.7561	0.6524
N	164	164	164	164	164	164	164	164
Construction	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0147*	0.0325**	0.0252	0.0443	0.0120	0.0293	0.0155	0.0113
RMSPE	3.2886	5.7525	2.6893	5.3899	1.5365	3.4800	2.3619	3.3752
p-value	0.0816	0.0408	0.2721	0.1020	0.4694	0.1837	0.2313	0.2585
N	147	147	147	147	147	147	147	147
Retail	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0055**	0.0067**	0.0037	0.0102*	0.0094	0.0087	0.0097	0.0165
RMSPE	3.2476	4.2282	3.2002	4.9401	2.2786	3.0057	0.4398	1.7527
p-value	0.0417	0.0417	0.1548	0.0833	0.2857	0.2262	0.8095	0.6310
N	168	168	168	168	168	168	168	168
Accom. & Food Svcs	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	-0.0016	0.0125	0.0015**	0.0167**	0.0074*	0.0141*	-0.0016	0.0053
RMSPE	1.2029	2.0035	9.0218	8.6020	5.6238	7.0253	0.0960	0.2514
p-value	0.3333	0.2381	0.0136	0.0204	0.0748	0.0612	0.9864	0.9728
N	147	147	147	147	147	147	147	147

Authors' own analysis using data from the QCEW. * Significance at the 10% level; ** significance at the 5% level; *** significance at the 1% level.

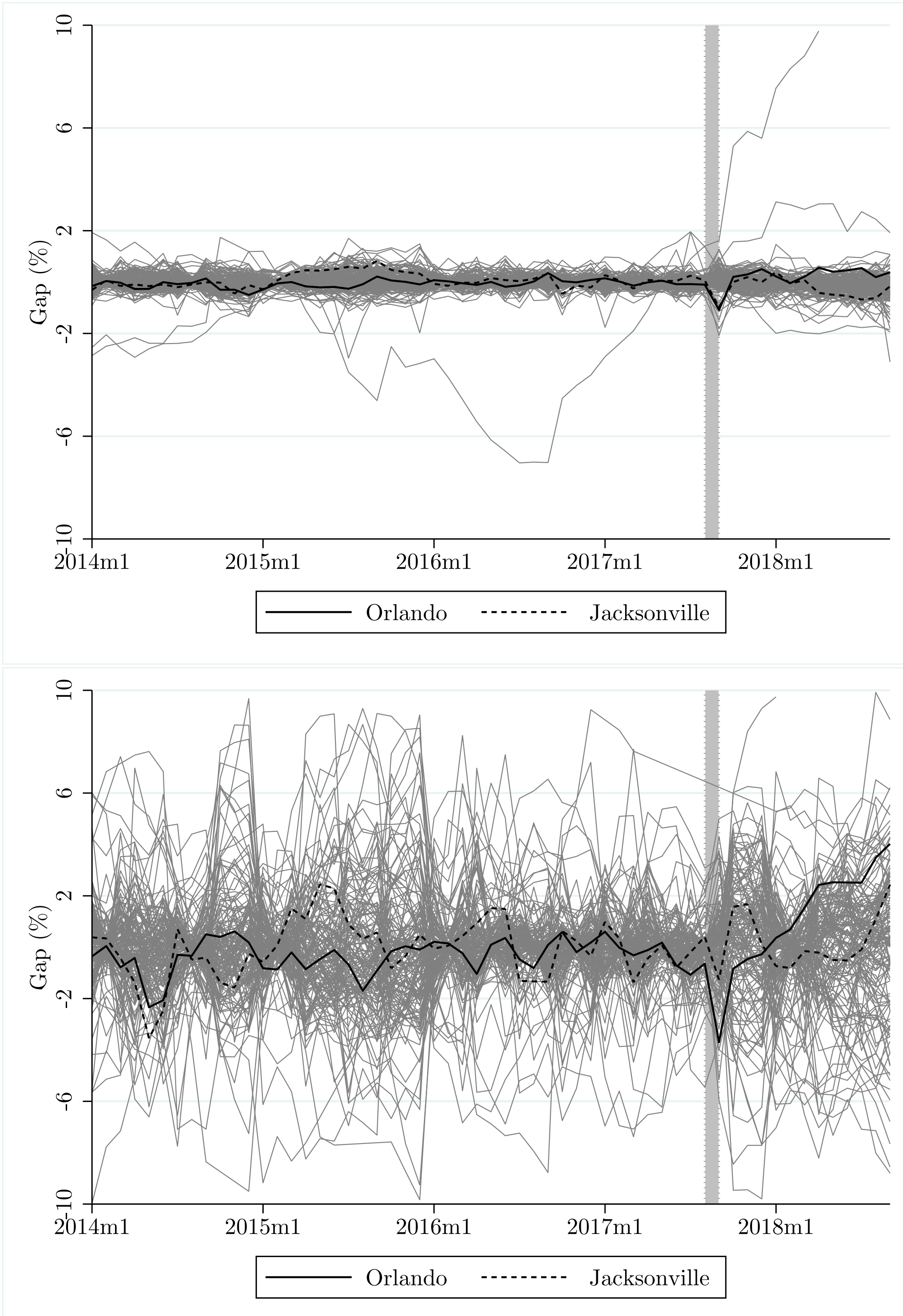
Figure 8: Track of Hurricane Irma through Florida



Source: National Environmental Satellite, Data, and Information Service (2018)

Figure 9: Impact of Hurricane Irma, employment gap vs synthetic control

(a) Aggregate employment (b) Construction employment



Authors' own analysis using data from the QCEW

Table 7: Estimated Impact of Hurricane Irma (Jacksonville Commuting Zone)

All workers

Sector	Log employment		Log total compensation		Log compensation per worker		Log establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0008	-0.0016	0.0003	-0.0085	-0.0001	-0.0009	-0.0040	-0.0050
RMSPE	1.1398	1.7879	1.1952	4.1905	1.8555	2.0689	0.8058	1.8678
p-value	0.2711	0.2711	0.5361	0.1145	0.3855	0.5120	0.5000	0.4036
N	166	166	166	166	166	166	166	166
Construction	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	-0.0015	0.0244	-0.0018	0.0283	0.0007	0.0067	-0.0027	0.0029
RMSPE	0.5040	0.6545	3.4309	2.7543	4.1171	2.8189	0.3328	0.3050
p-value	0.6779	0.8255	0.1544	0.2685	0.1007	0.2886	0.8389	0.9799
N	149	149	149	149	149	149	149	149

Authors' own analysis using data from the QCEW. * Significance at the 10% level; ** significance at the 5% level; *** significance at the 1% level.

Table 8: Estimated Impact of Hurricane Irma (Jacksonville Commuting Zone)

All workers

Sector	Log employment		Log total compensation		Log compensation per worker		Log establishments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	0.0008	-0.0016	0.0003	-0.0085	-0.0001	-0.0009	-0.0040	-0.0050
RMSPE	1.1398	1.7879	1.1952	4.1905	1.8555	2.0689	0.8058	1.8678
p-value	0.2711	0.2711	0.5361	0.1145	0.3855	0.5120	0.5000	0.4036
N	166	166	166	166	166	166	166	166
Construction	6 months	12 months	6 months	12 months	6 months	12 months	6 months	12 months
Treatment effect	-0.0015	0.0244	-0.0018	0.0283	0.0007	0.0067	-0.0027	0.0029
RMSPE	0.5040	0.6545	3.4309	2.7543	4.1171	2.8189	0.3328	0.3050
p-value	0.6779	0.8255	0.1544	0.2685	0.1007	0.2886	0.8389	0.9799
N	149	149	149	149	149	149	149	149

Authors' own analysis using data from the QCEW. * Significance at the 10% level; ** significance at the 5% level; *** significance at the 1% level.

A Online Appendix

Table A1: Sector composition of migrants from Puerto Rico to Orlando, natives in Orlando, Hispanics born outside the US and Hispanics (including natives) in Orlando

	(1) Born in Puerto Rico	(2) Natives	(3) Hispanics (Born Abroad)	(4) Hispanics
Wholesale	2.6%	2.4%	2.4%	2.4%
Construction	5.6%	5.6%	10.6%	8.5%
Retail	14.3%	12.0%	11.8%	12.8%
Information	1.8%	2.5%	1.5%	1.8%
Finance/Real Estate	7.0%	6.8%	6.1%	6.3%
Education/Health	17.0%	18.2%	15.9%	15.6%
Hospitality	17.4%	17.8%	16.5%	17.9%
Manufacturing	4.0%	4.3%	4.6%	4.1%
Transport	7.3%	3.7%	6.6%	5.7%
Other Services	4.8%	4.3%	5.0%	4.4%
Utilities	0.4%	0.1%	0.4%	0.3%
Management	10.0%	12.6%	11.7%	11.9%
Natural Resources	0.2%	0.4%	1.2%	0.8%
Mining	0.0%	0.1%	0.0%	0.0%
Observations	2,628	33,301	6,301	10,102

Authors' own analysis from the 2016 ACS or PRCS five years samples. All results are for individuals that are in the labor force.

Table A2: Estimated Treatment Effects

Non-Hispanic workers (unrestricted donor pool; 6 months after inflows began)

Sector	Log employment	Log total compensation	Log compensation per worker
	(1)	(2)	(3)
Total			
Treatment effect	-0.0000*	0.0002	0.0002
RMSPE	13.4133	0.4290	1.7499
p-value	0.0964	0.9217	0.6747
N	166	166	166
Construction			
Treatment effect	0.0007	-0.0002*	0.0007**
RMSPE	2.5087	9.5323	13.7713
p-value	0.6258	0.0839	0.0323
N	155	155	155
Retail			
Treatment effect	0.0001	-0.0001	-0.0003
RMSPE	3.1760	8.9626	12.2908
p-value	0.3631	0.1488	0.1012
N	168	168	168
Accommodations & Food Services			
Treatment effect	-0.0007	0.0025	-0.0001
RMSPE	0.1698	4.7983	2.2366
p-value	0.9630	0.4198	0.5617
N	162	162	162

Authors' own analysis using data from the QWI. * Significance at the 10% level; ** significance at the 5% level; *** significance at the 1% level.

Table A3: Estimated Treatment Effects

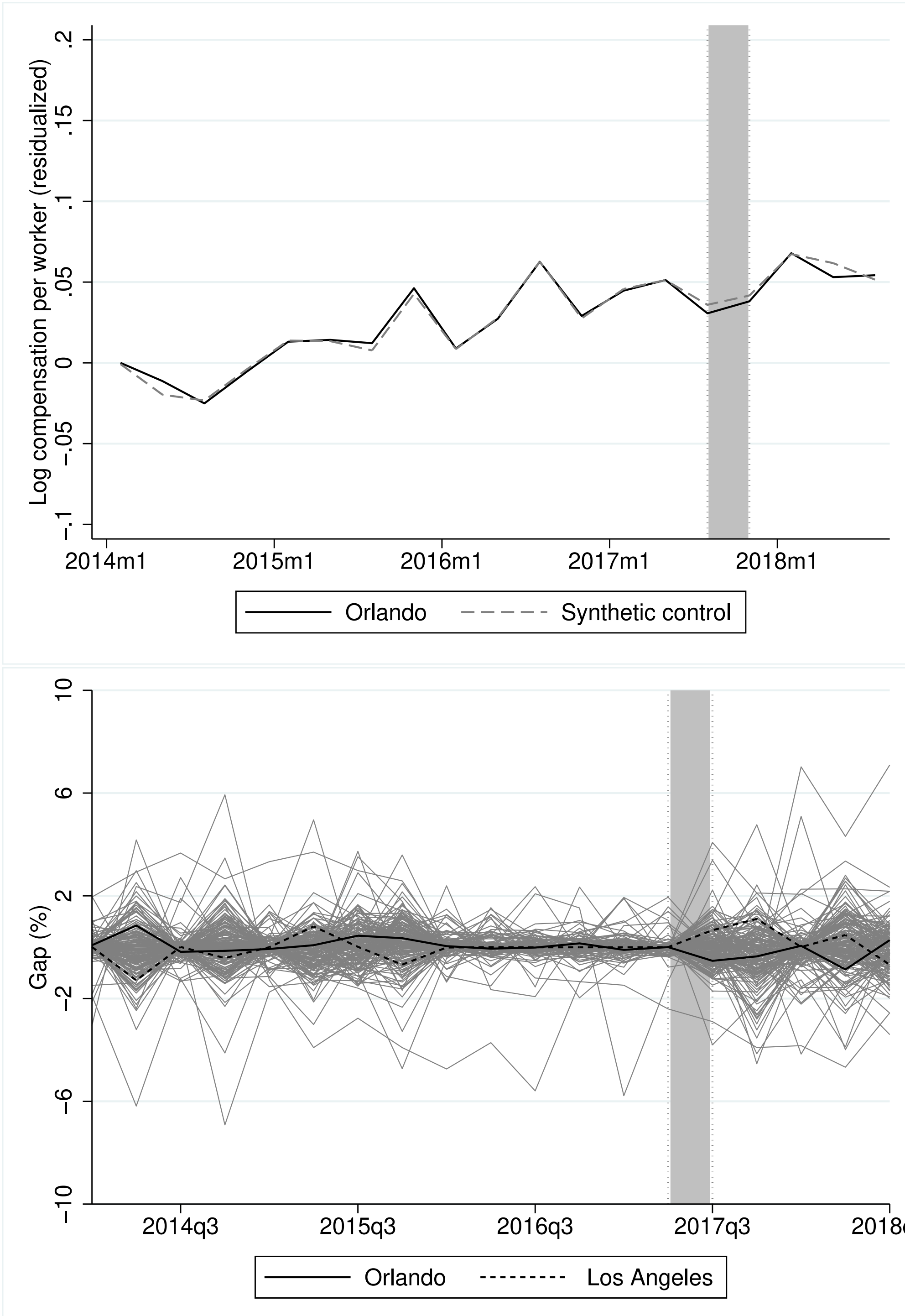
Less-educated non-youth workers (unrestricted donor pool; 6 months after inflows began)

Sector	Log employment	Log total compensation	Log compensation per worker
	(1)	(2)	(3)
Total			
Treatment effect	-0.0005	-0.0026	0.0009
RMSPE	7.9772	1.8003	3.6285
p-value	0.1288	0.6319	0.3865
N	163	163	163
Construction			
Treatment effect	0.0084	0.0033	0.0010**
RMSPE	7.4776	7.1408	18.1278
p-value	0.2803	0.1465	0.0191
N	157	157	157
Retail			
Treatment effect	-0.0004	0.0017***	0.0006***
RMSPE	0.2674	66.3713	69.3695
p-value	0.9518	0.0060	0.0060
N	166	166	166
Accommodations & Food Services			
Treatment effect	0.0028	0.0020	0.0000
RMSPE	2.8371	1.4512	8.2702
p-value	0.5253	0.7278	0.1962
N	158	158	158

Authors' own analysis using data from the QWI. * Significance at the 10% level; ** significance at the 5% level; *** significance at the 1% level.

Figure A1: Aggregate compensation per worker

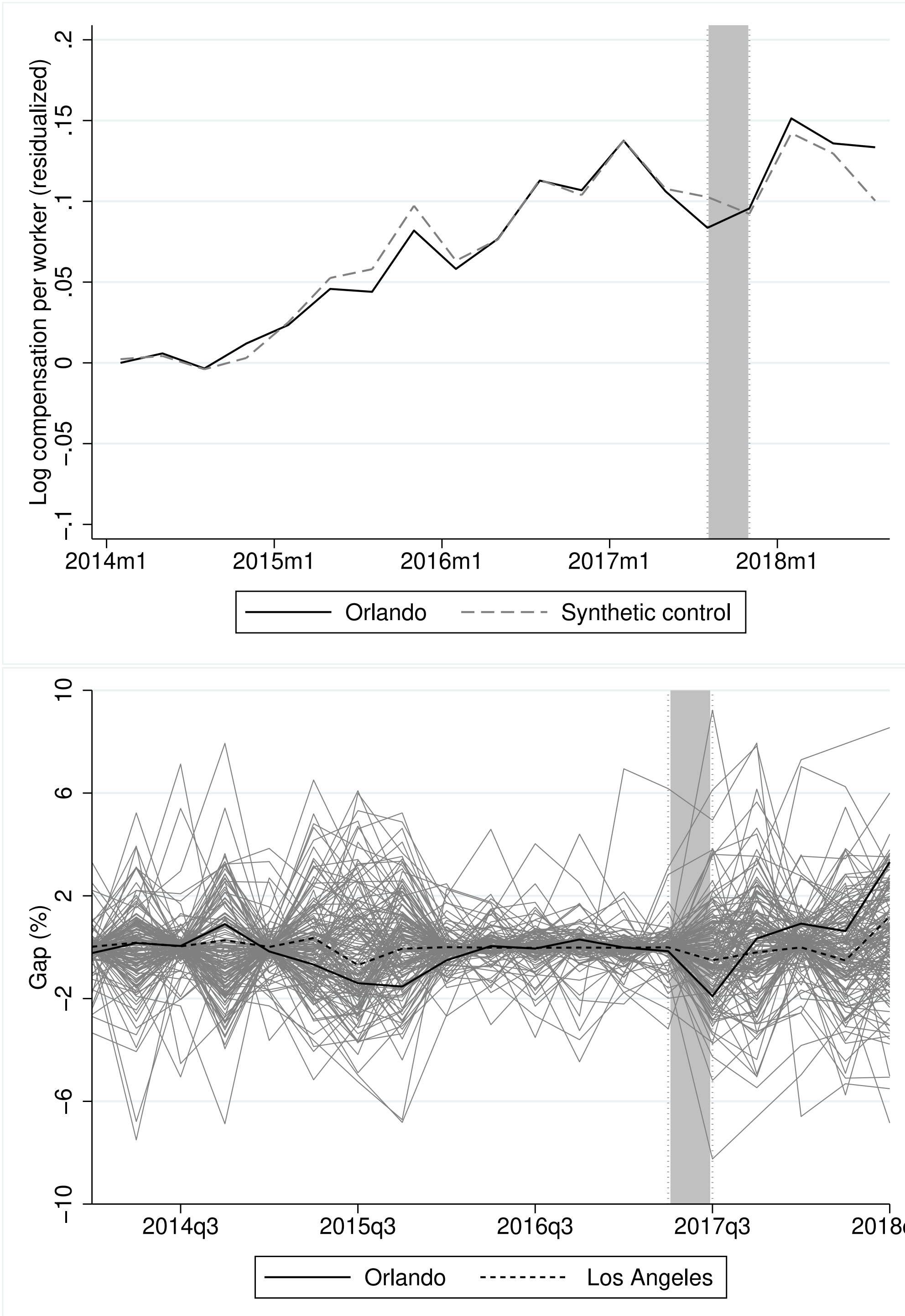
(a) Orlando vs synthetic control (b) Gap (Orlando vs donor pool)



Authors' own analysis using data from the QCEW

Figure A2: Construction compensation per worker

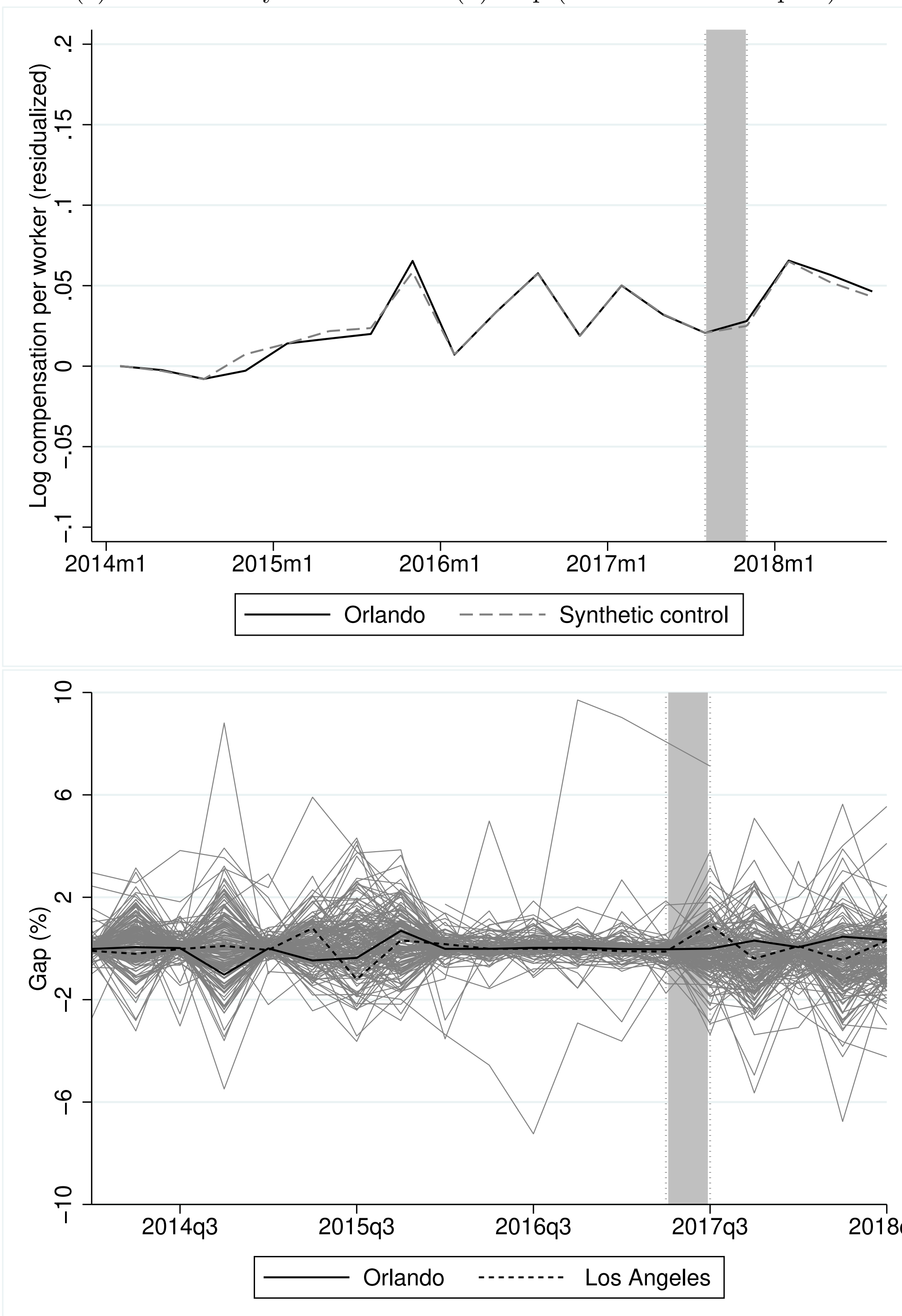
(a) Orlando vs synthetic control (b) Gap (Orlando vs donor pool)



Authors' own analysis using data from the QCEW

Figure A3: Retail compensation per worker

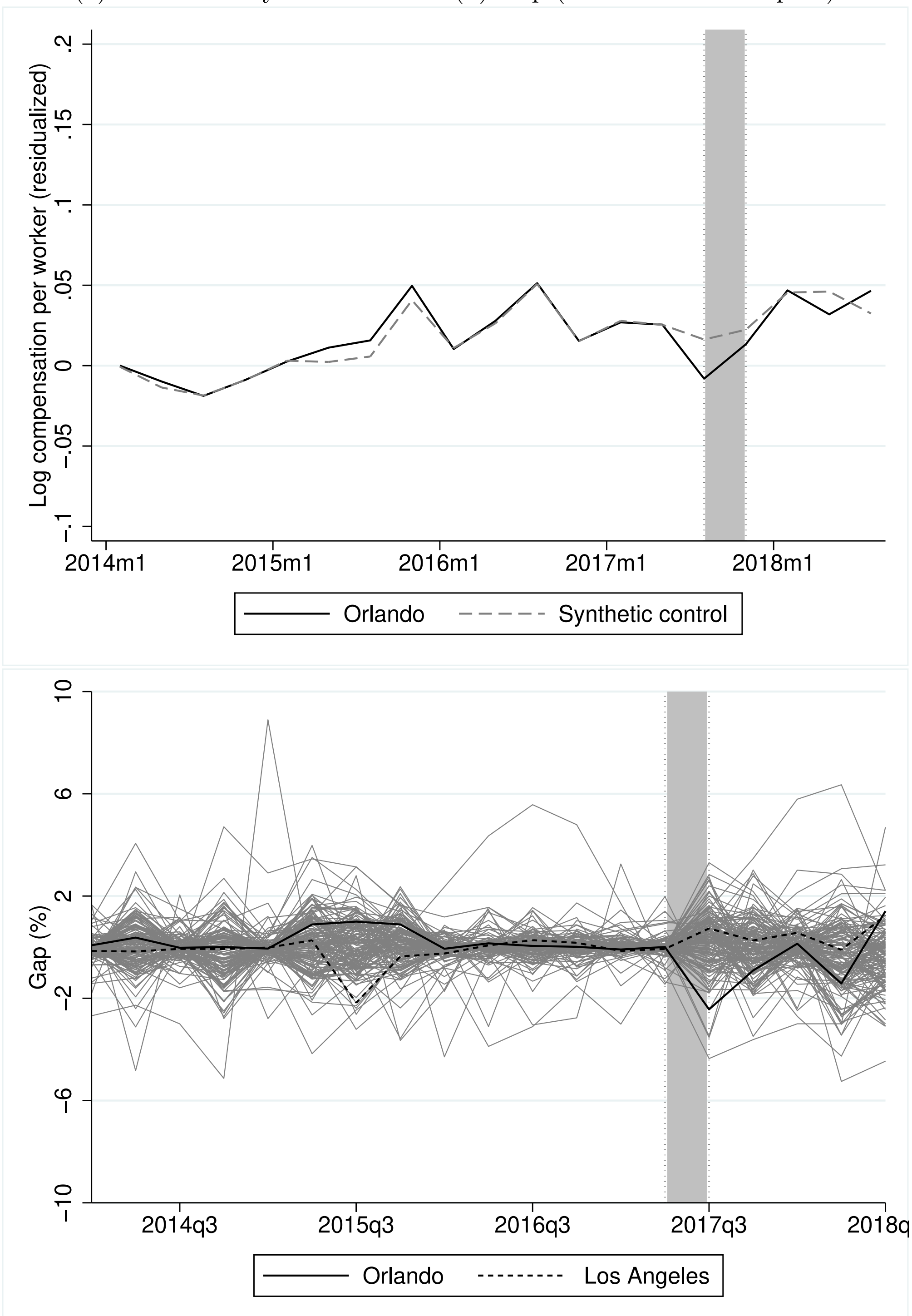
(a) Orlando vs synthetic control (b) Gap (Orlando vs donor pool)



Authors' own analysis using data from the QCEW

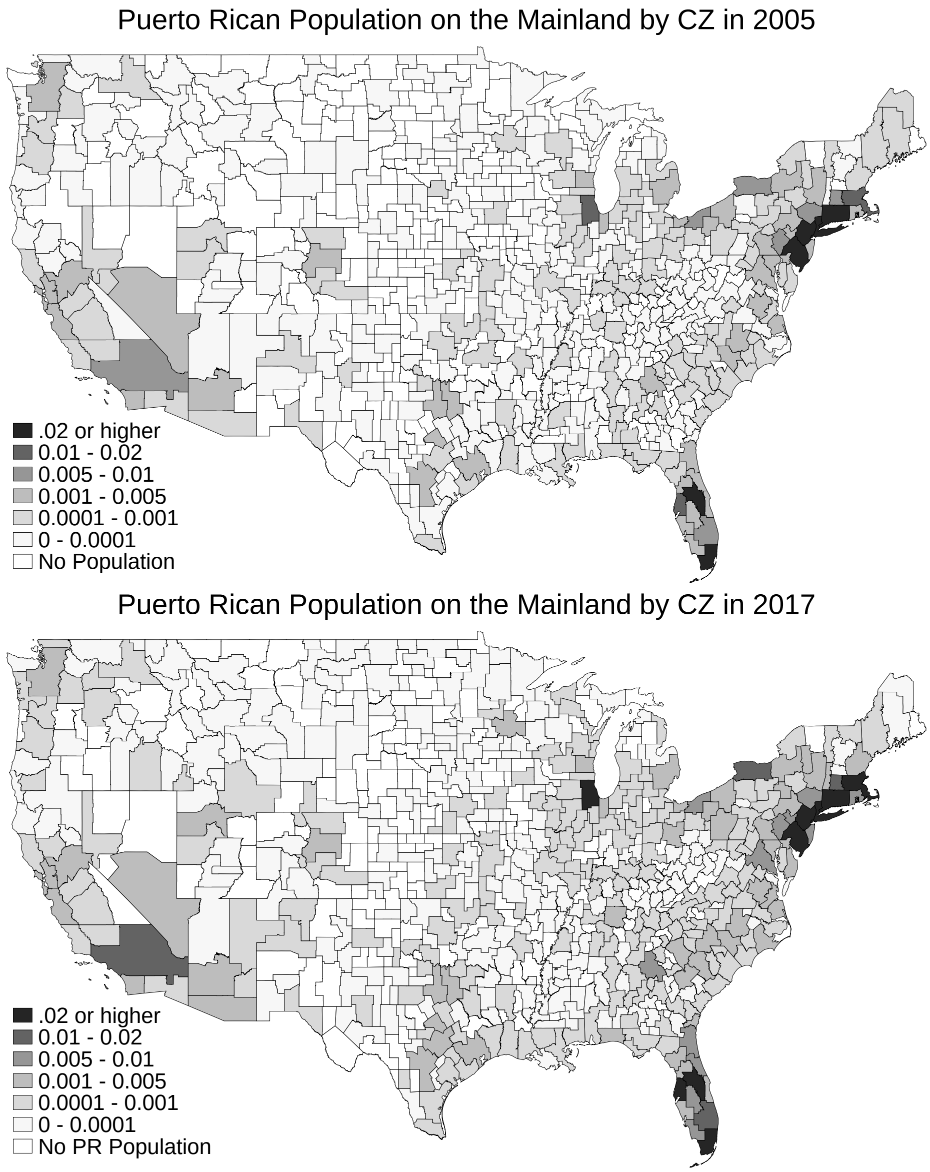
Figure A4: Accommodations & Food Services compensation per worker

(a) Orlando vs synthetic control (b) Gap (Orlando vs donor pool)



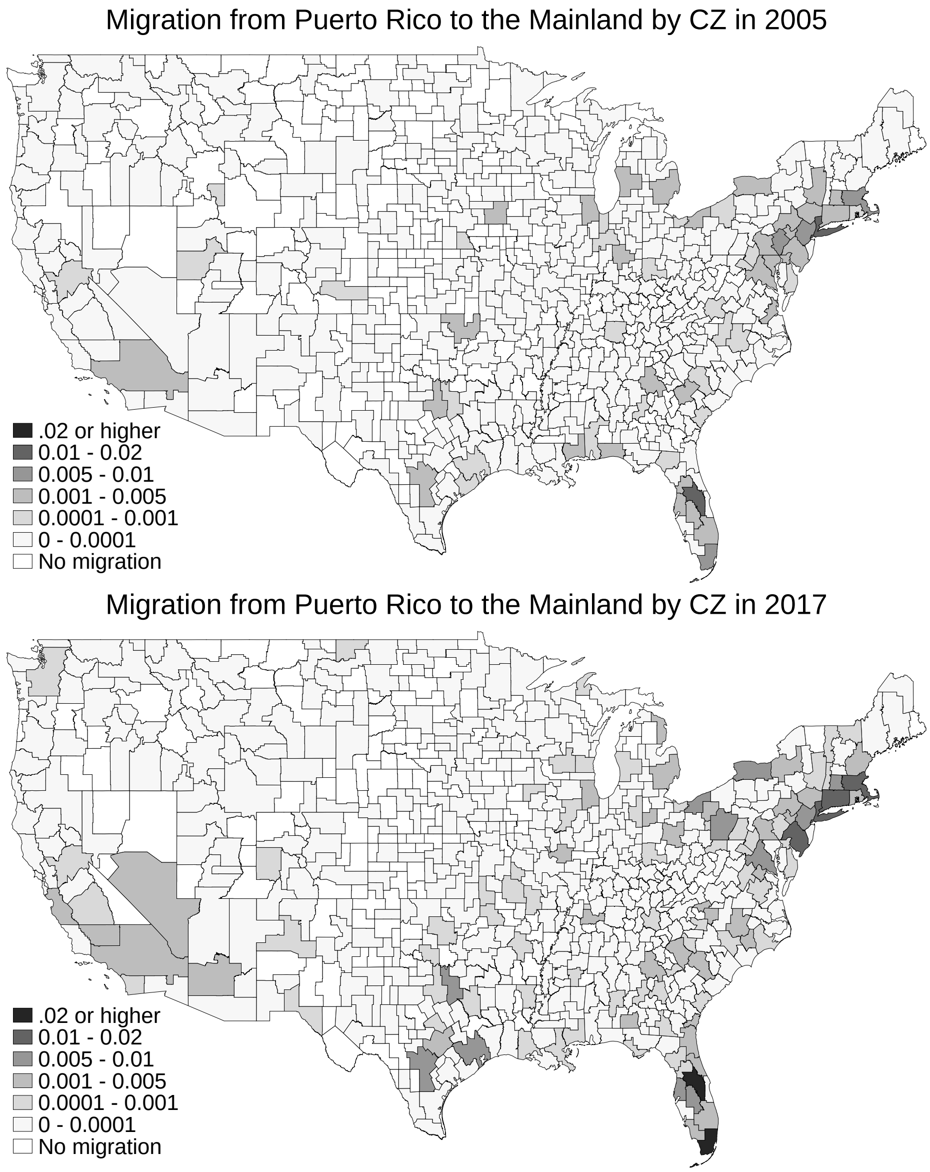
Authors' own analysis using data from the QCEW

Figure A5: Concentration of total U.S. Puerto Rican population by commuting zones (numbers are fraction of total Puerto Rican popuation in the mainland US)



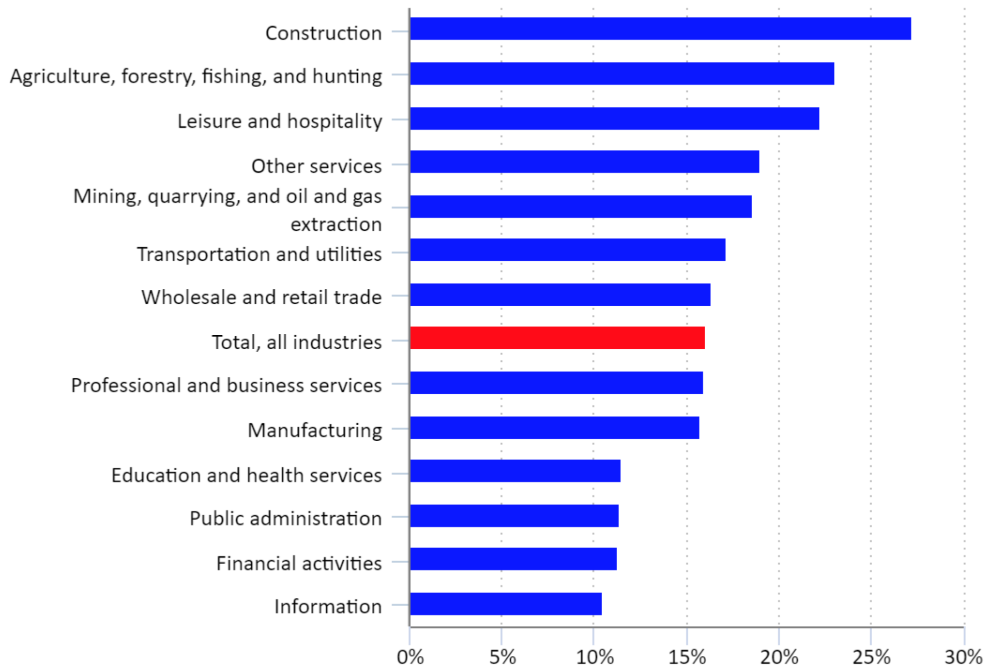
Authors' own analysis using data from the ACS

Figure A6: Concentration of recent Puerto Rican migrants, by commuting zone (numbers are fraction of total recent migrant population)



Authors' own analysis using data from the ACS

Figure A7: Percentage of industry employment that is of Hispanic of Latino ethnicity, 2014 annual averages



Source: U.S. Bureau of Labor Statistics, 2015