The effect of immigration shocks on native fertility outcomes: evidence from a natural experiment

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control estimator and an extended individual difference-in-differences analysis, the results from this study indicate that the immigration shock had an overall negative, though short-lived, impact on the fertility of Miami women. In addition, fertility effects are found to vary by homeownership: While the immigration shock had a considerable negative impact on the fertility of women living in rented homes, it had no effect on those living in owned homes. This differential impact was likely due to the rise in local housing rents accompanying immigration, making childbearing less affordable for those living in rented homes.

JEL Classification: J61, J13

This paper examines whether immigration shocks have a causal effect on native fertility patterns. While there has been a plethora of previous research examining the effects of immigration on the labour market outcomes of destination countries, relatively few studies exist with a focus on exploring how immigration affects the host society in other ways. An area which research has hitherto neglected is the relationship between immigration shocks and native fertility behaviour. Yet, a synthesis of the research conclusions from studies exploring the effects of immigration on local destination markets with those investigating how fertility outcomes respond to economic conditions reveals that it is possible for native fertility outcomes to be altered by such inflows.

Immigration shocks may influence the childbearing decisions of natives through at least four channels: Firstly, among labour market participants, if natives view immigrants as competitors competing for scarce employment opportunities in the labour market, then an immigration influx may lead to increased perceptions of job insecurity among these workers. Presupposing that childbearing decisions depend positively on employment and income security (Sobotka et al. 2011), an increase in perceived job and income insecurity may act to reduce fertility (either contemporaneous/tempo or completed fertility, or both) by inducing individuals to delay their childbearing plans or to forgo having a child altogether.[1] Secondly, an immigration shock may lead, at least in the short run, to an increase in housing prices and rents. To the extent that housing is a precondition for childbearing, an increase in housing rents/prices lowers a household's real income and exerts a negative income/substitution effect on the demand for children

increase in low-skilled immigration may work to increase native fertility (Furtado and Hock 2010; Furtado 2016).

There are two main research questions which this paper seeks to address: Firstly, do immigration shocks affect native childbearing decisions? Secondly, do childbearing responses to an immigration shock vary by homeownership status? The second question arises because, by changing the price of living space, immigration could potentially change the cost of childbearing differently for homeowners and non-homeowners.

This study uses a "natural experiment", exploiting the 1980 Mariel Boatlift where some 125,000 Cuban immigrants arrived in Miami over a 5-month period (May to September 1980) in order to generate an exogenous variation in immigration flow so that the fertility consequences of Miami women in the aftermath of the shock can be studied. The fertility impact of the immigration shock is measured in two ways. First, I apply the synthetic control estimator, using aggregate-level data, and assess the significance of the estimates using placebo tests. Second, I apply the traditional difference-in-differences estimator with inference techniques based on individual-level data. Both lead to the same conclusion: The immigration shock led to short-term declines in native childbearing activity in 1983 and in 1986, although these declines were compensated by fertility increases in later years. The short-term declines in native childbearing activity after the immigration influx were possibly due to individuals delaying their childbearing plans.

2 Background

The link between employment/income uncertainty and fertility behaviour has also been well explored, with many studies reaching conclusions which support the hypothesis that increases in job and income insecurity generally lower fertility—either temporarily, through delayed childbearing, or permanently, through a reduction in the number of children considered optimal (Ranjan 1999; Adsera 2004; Hondroyiannis 2010; Adsera and Menendez 2011; Bhaumik and Nugent 2011; Sobotka et al. 2011).

The effects of immigration on the housing market have been considered by a number of researchers. The general consensus, especially for studies examining housing markets in the USA, is that immigrant inflows lead to higher local housing prices/rents in the short run (Saiz 2003, 2007; Ottaviano and Peri 2012; Gonzalez and Ortega 2013).

Though recent, a number of studies which examine the relationship between the price of living space and fertility have also found evidence that housing affordability impacts fertility positively. For example, Simon and Tamura (2009) find that increases in housing rents lead to delayed childbearing in the USA. Using data on Hong Kong, Yi and Zhang (2010) find that an increase in the price of housing significantly decreases total fertility rates in the territory. In contrast to these papers, two other studies demonstrate that movements in housing prices can affect the fertility decisions of homeowners and non-owners differently. Dettling and Kearney (2014) find that while an increase in housing prices in the USA leads to a fall in the birth rates among non-homeowners, it has the opposite effect for homeowners. They argue that this arises because an increase

Taken together, the above studies seem to imply that it is possible for increased immigration flows to induce a fall in native fertility. However, this need not necessarily be the case. On the contrary, a greater share of immigrants—especially if these are low-skilled—can be consistent with an increase in native fertility.

This could be the case, for instance, if the immigration shock is composed of workers who are very substitutable to native women (Blau and Mackie 2017). If so, any immigration-induced fall in wages and/or employment opportunities for this group of natives may decrease the opportunity cost of childrearing and therefore increase their likelihood of having children.

In addition, prior empirical studies that investigate the effects of immigration on prices in the destination countries have found that an increase in the share of immigrants may lead to a reduction in the prices of goods and services in the locality (Lach 2007; Cortes 2008). To the extent, therefore, that immigration reduces the cost of child bearing and rearing and the conflict that native women have over childbearing and work, it is possible, as well, for increased immigration flows to affect native fertility positively (Furtado and Hock 2010; Cortes and Tessada 2011; Furtado 2016).

The above review has highlighted the possibility that immigration flows may have an effect on native fertility in the destination countries. However, the direction in which native fertility moves with immigration is a priori ambiguous. While an increase in the

emigrate from the country were free to do so. Card's estimates suggest that 50% of the Mariel immigrants eventually settled in Miami, increasing the overall labour force of Miami by approximately 7%. In a related study examining the response of housing markets to immigration shocks, Saiz (2003) estimates that the influx increased Miami's renter population by at least 9% in just 1 year (in 1980).

Although the original analysis of the wage impact of the Mariel Boatlift by Card (1990) did not find any notable impact of the supply shock on the labour market outcomes of Miami residents, recent re-examinations by Borjas (2017a, b) and Borjas and Monras (2016) suggest that Card's findings might have been flawed due to the way the comparison cities were chosen. In particular, Borjas (2017a) points out that the four cities used by Card (1990) to form the comparison group—Atlanta, Los Angeles, Houston, and Tampa-St. Petersburg—were selected partly based on their similarity in labour market trends with Miami *after* the Boatlift. This is problematic because, if the immigration shock truly worsened labour market conditions for existing residents in Miami after the shock, then the cities in Card's comparison group would be chosen, so that they too would have experienced worse labour market outcomes in that same period. As such, trends exhibited by this comparison group would spuriously resemble those exhibited by Miami in the post-Mariel period, giving one the false impression that the Boatlift had no impact on the labour market outcomes of Miami residents.

Borjas' (2017a) re-examination, which involved selecting the comparison group based

It should be noted that the findings of Borjas (2017a) have been criticised by a number of studies, including Peri and Yasenov (2017) and Clemens and Hunt (2017). Peri and Yasenov (2017) argue that the large adverse wage impacts found in Borjas (2017a) is likely an artefact of measurement error due to the small sub-sample analysed and the matching of Miami to a control group of cities based on an inadequately short pre-treatment period.[5] Any difference in labour market trends between Miami and the control group in the post-Boatlift period is therefore likely to be largely reflecting chance variation rather than the impact of the Boatlift, they argue. Clemens and Hunt (2017) argue that right around the time of the Boatlift in 1980, there had coincidentally been an increase in the share of Blacks in the Miami subsample which was analysed by Borjas (2017a). This compositional change was specific to Miami and was due to reasons unrelated to the Boatlift. They contend that because the wages of Blacks were lower than those of non-Blacks, the compositional change gave a misleading appearance that wages of the least-skilled workers in Miami had been negatively affected by the Boatlift.

Borjas attempts to respond to these criticisms in Borjas (2017b), where he adjusts for the increasing number of Black men in Miami's low-skilled workforce, by examining trends in race- and age-adjusted wages for Miami before and after the Boatlift. He shows that the results in Borjas (2017a) are robust to racial composition adjustments and demonstrates that the racial composition explanation offered by Clemens and Hunt (2017) is unlikely to be the reason for the observed fall in wages of the least-skilled Miami workers.

While the increase in the price of living space in Miami is likely to exert a negative price effect (or a negative real income effect) on the childbearing decisions of non-homeowners, it may have a positive income effect on the fertility decisions of homeowners (Ranjan 1999; Dettling and Kearney 2014).

Although there has not been any research documenting the effects of the Mariel influx on the local prices in Miami, the study by Cortes (2008) gives an indication of how the price of goods complementary to childbearing in Miami was likely to change following the Boatlift. Using data from the US consumer price index, the study finds that a higher share of low-skilled immigrants in the labour force reduces the price of immigrant-labour-intensive services such as babysitting and housekeeping. This suggests that the prices of goods and services complementary to childbearing could have declined (or at least risen at a slower rate than it otherwise would) in the aftermath of the Boatlift. This may have produced a positive price effect for fertility outcomes in Miami.

3 Data

3.1 Current Population Survey June Supplements

The main data source for this study is the United States Bureau of Labor Statistics' 1973 to 1988 Current Population Survey (CPS) June Supplements.[7] The survey is administered during the month of June annually to persons in the civilian non-institutional population of the USA and is intended to supplement fertility information on respondents in addition to its primary purpose of providing information on the

be extrapolated to the wider population. Because geographical information on a respondent's residence is given, this allows respondents to be sorted by metropolitan areas (MSAs) within the USA. A total of 27 MSAs exist in the dataset.[8] One limitation with the use of the CPS data is that prior to 1994, information on a respondent's nativity/citizenship is not provided. There is therefore no way to tell if a respondent was native or foreign born. Hence, we will take "natives" to mean all persons of non-Cuban origin residing in the USA.

Although the use of the difference-in-differences technique to identify changes in fertility outcomes in Miami following the Mariel Boatlift only requires aggregate data on fertility outcomes (that is, it simply requires one to have information on, say, aggregate birth rates), using micro-level data to construct aggregate fertility rates as with the CPS brings about several advantages over the use of readily available aggregate birth rate data.

Firstly, it allows one to control for individual-level characteristics that may influence individual fertility decisions and aggregate fertility outcomes.

Secondly, because the CPS allows for individuals in the sample to be identified based on ethnicity; it allows one to construct aggregate fertility rates pertaining only to non-Cubans. Excluding Cubans from the analysis is important because (1) we intend to restrict the scope of this study to evaluate the effects of an immigration shock on the

metropolitan fertility since it normalises the number of births occurring in a metropolitan area to the entire metropolitan population rather than to the population most likely to have children (i.e. women of childbearing age). Say for the sake of exposition that a metropolitan area's crude birth rates increase over time. We cannot be sure if the increase in birth rates is due to an actual increase in childbearing activity in the area, which is what we are really interested to know, or to a relative decline in the (male) population in that area during this time. The aggregate fertility measure I construct using the CPS, known as the general fertility rate—defined as the annual number of births occurring in a metropolitan area per woman age 15 to 44 (i.e. the childbearing age)—overcomes this shortcoming by normalising births to a more "at risk" population (i.e. women of childbearing age), enabling me to conduct the analysis with a more refined measure of fertility (Namboodiri 1996).

3.2 The sample

I include in the sample all females age 15 to 44. Women in this age range are typically found to have the greatest chance of childbearing. Because a considerable share of childbirths occurred out of wedlock, there is reason to believe, at least for the years analysed, that fertility outcomes need not be tightly linked to the institution of marriage in the USA (Willis 1999).[9] Both married and unmarried women are therefore included in the sample. The non-Cuban sample from Miami includes an average of approximately 200 observations per year while the sample including Cubans includes an average of roughly 250 observations per year.

Assuming it takes approximately 2 to 2.5 years for the Mariel Boatlift to affect the childbearing outcomes of Miami residents through the hypothesised channels, the trends observed in Table 1 are consistent with a temporary delay in the timing of childbearing after the Boatlift (so that contemporaneous fertility falls sharply from 1982 to 1983 and remains low till 1984) and a "catch up" phase where fertility rises above its "normal" levels in the later years (from 1984 to 1985).[10] It is not clear how the sharp fall in fertility rates observed in 1986 (and the recovery thereafter) should be interpreted and whether it is appropriate for us to attribute it to the Mariel Boatlift since we would expect any fertility impacts to diminish with time. The fall in 1986 is consistent, though, with results from Borjas (2017a, b), which show that the adverse wage effect of the Mariel Boatlift was not at its largest right after the immigration shock, but rather, only some 3 to 4 years later in 1984–1986. The patterns therefore do not preclude the possibility that a double decline in fertility may have occurred.

4 Synthetic control analysis

This paper employs a "natural experiment" to examine whether immigration shocks have a causal effect on native fertility. I adapt the study, originally used by Card (1990), to investigate the impact on native fertility in Miami after it received a large and unexpected inflow of migrants from Cuba between May to September 1980. The impact of the Mariel immigration shock is estimated by comparing the evolution of fertility outcomes for Miami after the immigration shock to those for comparable geographic regions within the USA which were unaffected by the shock.

during the 1981 survey (conducted in June 1981), this would mean that the birth(s) must have occurred between the period July 1980 and June 1981. Because it takes an average of 42 weeks (or 9.7 months) between the time of conception and the time of delivery, the decision to have the child must have been made, *at latest*, between September 1979 and August 1980. Although the Mariel immigrants arrived in Miami between May 1980 and September 1980, slightly overlapping with this period, it is unlikely that either perceived job market security or the price of living space and goods complementary to childbearing would have adjusted fast enough so as to alter the childbearing outcomes reflected in the 1981 data. In any case, to be sure, robustness checks were conducted in all analyses to see if the results changed in any way when 1981 and 1983 were instead taken to be the start of the post-treatment period. The results remained practically unchanged when these alternative years were taken to be the years in which the treatment occurred. I therefore take 1982 to be the treatment year.

The raw data showing the evolution of fertility rates in Miami indicated that the rates fell sharply twice in Miami (once in 1983 and once in 1986) after the Boatlift. Although this is consistent with an interpretation that the Mariel Boatlift had a short-term negative effect on the fertility of non-Cuban women, we cannot be certain that the observed declines in fertility were due to the Boatlift rather than other confounding factors (e.g. common fertility shocks affecting all metropolitan areas in the USA) unless we know how fertility outcomes would have evolved in Miami in the absence of the immigration influx.

to closely mirror Miami in terms of its characteristics and attributes. The fertility changes pertaining to such a comparison area will provide a counterfactual for the fertility changes for Miami.

Because I rely on the aforementioned strategy to identify the casual effect of the immigration shock, the comparison areas have to be carefully selected so that they are representative of Miami. There are a number of ways to select the comparison areas against which the fertility path of Miami can be compared. One strategy is to "hand-select" comparison metropolitan areas on the basis of subjective measures of similarity between the treated (i.e. Miami) and non-treated units (see, for example, Card (1990)). However, this approach has been criticised for being "adhoc" and of lacking clarity and objectivity in the way comparison units are chosen (Abadie et al. 2010). An alternative strategy, first proposed by Abadie and Gardeazabal (2003) and later formalised by Abadie et al. (2010), involves constructing comparison areas based on a data-driven procedure. This strategy—known as the synthetic control method—involves constructing a comparison area based on a convex combination of individual metropolitan areas with weights chosen so that the resultant comparison area resembles the treated area closely in term of its characteristics (i.e. characteristics relevant for predicting the outcome(s) of interest) before the treatment.

The weighted average of the contributing metropolitan units is conceptualised as the "synthetic twin" of the treated area and the outcome changes pertaining to this

This section briefly reviews the material developed in Abadie et al. (2010) in order to demonstrate the rationale behind the synthetic control technique. Interested readers may refer to Abadie et al. (2010) for further details on the method.

Suppose there are $t = 1, 2.... , T$ time periods. Let T_0 be the number of pre-treatment periods, where $1 \leq T_0 \leq T$. Also, let \({\boldsymbol{X}}_1=\left({Y}_{11},\dots .,\right. {Y}_{1{T}_0},{\boldsymbol{Z}}_1^{\prime}\Big){}^{\prime } \) be a vector of pre-treatment characteristics for Miami that includes the fertility outcomes in each year of the pre-treatment period (i.e. \({Y}_{11,\dots, }{Y}_{1{T}_0} \)) as well as covariates that are predictive of metropolitan area fertility (given by the vector Z_1). Similarly, let X_0 be a matrix containing the same variables for each of the J metropolitan areas potentially contributing to the comparison synthetic control unit.[11] The idea behind the synthetic control method is to choose an optimal weighting vector \({\boldsymbol{W}}^{\ast}=\left({w}_2^{\ast },\dots .,\right.{w}_{J+1}^{\ast}\Big) {}^{\prime } \) such that it minimises:

$$ \left\Vert {\boldsymbol{X}}_{\mathbf{1}}- {\boldsymbol{X}}_{\mathbf{0}}\boldsymbol{W}\right\Vert $$

(1)

subject to

in fertility outcomes that would have occurred in Miami had there not been the Boatlift.[13] Any deviation in fertility trends experienced in Miami from that of the synthetic control in the post-treatment period is an indication of the impact of the immigration shock.

More formally, the average treatment effect is:

$$ \delta =\left(\overline{y_{2,\mathrm{Miami}}}-\overline{y_{1,\mathrm{Miami}}}\right)-\left(\overline{y_{2,\mathrm{Control}}}-\overline{y_{1,\mathrm{Control}}}\right) $$

(2)

where $\overline{y_{2,\mathrm{Miami}}}$ and $\overline{y_{1,\mathrm{Miami}}}$ denote the average fertility in Miami in the post and in the pre-treatment period respectively, and where $\overline{y_{2,\mathrm{Control}}}$ and $\overline{y_{1,\mathrm{Control}}}$ are analogously defined for the "synthetic Miami".

The significance of these treatment impact estimates have to be probed subsequently using placebo tests. Put simply, the placebo test involves the following: for each metropolitan area in the potential collection of units contributing to the synthetic Miami, the process as described for Miami is repeated so that a synthetic comparison control is constructed for each area. Estimates of the treatment effects are then computed for each

estimated for Miami is large relative to the placebo effects estimated for some other metropolitan area drawn at random.

4.2 Estimated impacts on fertility outcomes

I assume that the immigration shock is localised only to Miami and that all other metropolitan areas in the USA contributing to the comparison groups are unaffected by it. Further, I assume that the immigration shock in Miami does not produce any spill-over effects by affecting the contemporaneous fertility decisions and outcomes of women living in the "unaffected" metropolitan areas.

We begin by discussing the evolution of fertility rates in Miami and its synthetic control for the period 1973–1988. The metropolitan areas contributing to synthetic Miami are New York, Chicago, Philadelphia, Baltimore, Cleveland, Houston, San Diego, New Orleans, Tampa St. Petersburg, and Portland, with panel A in Table 2 showing the exact weights assigned to these areas for the purposes of constructing the control. These metropolitan units were chosen based on the minimisation process given by condition (1) in Section 4.1 so as to best approximate Miami in terms of its characteristics (i.e. characteristics that are predictive of metropolitan fertility rates) and the fertility trends exhibited in the pre-treatment period. To this end, I include in the set of characteristics to be matched—the MSA marriage rate,[14] the proportion of females in the labour force, the female unemployment rate, the male unemployment rate, the female unemployment rate 1 year ago, the male unemployment rate 1 year ago, the proportion of women in the

of income flows can lead women to refrain from entering into long-term financial commitments and in so doing, postpone or cancel childbearing plans (O'Donoghue and O'Shea 2006).

Table 2 Metropolitan areas contributing to synthetic controls

The average values of these predictors over the pre-treatment years 1973–1981 are tabulated in panel A of Table 3 for Miami and its synthetic counterpart. Apart from indicators measuring the proportion of women within each ethnic category, the values of the rest of the characteristics are remarkably similar for both Miami and the synthetic control during this period. The reason that the proportion of women in each ethnic category for the synthetic control does not match well those for Miami is because of Miami's unique racial composition during the pre-treatment period (which has persisted till today). Miami was the most immigrant-intensive MSA in the USA, and its population consisted of a much larger proportion of Cubans, African Americans, and Hispanics than any other MSA. As such, no convex combination of other MSAs is able to reproduce the ethnic composition of Miami.

Table 3 Characteristics relevant to predicting fertility rates

demonstrated an increase instead during this time. Fertility rates appear to return to a level close to that experienced by the synthetic control from 1984 to 1985. However in 1986, outcomes diverged again, with fertility rates in Miami declining more sharply from 1985 to 1986 relative to that for its synthetic control. After the occurrence of this second fertility dip in 1986, the fertility rate in Miami followed an upward trend, increasing continuously from 1986 to 1988. These movements are in contrast to those exhibited by the synthetic control, which did not show a sharp fall in fertility in 1986 and which exhibited a falling, rather than increasing, fertility pattern from 1986 to 1988.

Fig. 1

Since the synthetic control is supposed to reproduce the pattern of fertility outcomes that would have occurred in Miami in the years after 1982 in the alternative world where the immigration shock did not happen, the divergences in fertility paths between Miami and the synthetic control after 1982 provide support for our earlier postulate (based on raw fertility rate trends) that the Mariel Boatlift had a short-term negative impact on the fertility outcomes of natives in Miami (in 1983 and 1986). To be exact, actual fertility rates in Miami fell quite substantially—by 3.8 and 2.4% more than those predicted by the synthetic control respectively in 1983 and 1986. These year treatment effects are calculated based on Eq. (2), taking 1983 and 1986 respectively to be the post-treatment years and 1973–1981 to be the pre-treatment period. The negative fertility effect, however, was only temporary and was followed by a compensatory rise in fertility after each dip. This suggests that the Mariel Boatlift may have initially led to a postponement of childbearing among people in Miami but that this was subsequently compensated through a fertility rise in later years.[15]

Calculating the average treatment effect based on Eq. (2), and taking 1973–1981 to constitute pre-treatment years and 1982–1988 to constitute post-treatment years, one finds that fertility *decreased by 0.2%* in Miami after the influx relative to the synthetic control. However the magnitude of this estimate is sensitive to the pre- and post-treatment periods used. For example, if we instead take the pre-treatment period to be 1980–1981 and the post-treatment period to be 1983–1984—reflecting the outcomes just before and just after the treatment—then a very different conclusion is reached: In this case, fertility would have *declined by 3.4%* in Miami after the influx relative to the control

might be compromised (since age is an important predictor of childbirth). To check if the age distribution of women within the childbearing age group in Miami was altered after the Mariel Boatlift, we separate the female sample into six age categories: age 15–19, age 20–24, age 25–29, age 30–34, age 35–39, and age 40–44. An analysis was then conducted, where control units were selected based on the synthetic control method in order to evaluate whether a change in the proportion of women belonging to these age groups occurred. If so, then this would be indicative of a shift in the age composition of women in Miami following the Boatlift. The results from this analysis, which are not shown here for brevity but which are available from the author upon request, indicate that the Mariel Boatlift did not have any systematic effect on the age distribution of women in Miami. There therefore seems to be little need for worry about shifts in Miami's female composition introducing biases to the fertility impact estimates.

4.3 Placebo tests for inference of fertility impact estimates

The analysis presented in the previous sub-section revealed that actual fertility outcomes in Miami fell dramatically relative to the counterfactual outcomes predicted by the synthetic control in 1983 and 1986. Since the fertility trends exhibited by the synthetic control represent the counterfactual fertility path for Miami in the alternative world where the Boatlift did not occur, this suggests that the immigration shock led to a short-term decline in contemporaneous fertility during these years.

However, it would be sensible to suspect that these results may have been driven by

temporary negative effect on fertility in Miami during these 2 years. On the other hand, if the placebo studies show that the treatment effects for Miami are unusually large relative to the placebo treatment effects estimated for the other metropolitan areas, then we can be more confident that the estimated treatment effects for Miami are not simply artefacts of chance, but that they instead represent genuine fertility impacts occurring in Miami during these periods, possibly driven by the Boatlift. Essentially, the placebo studies allow one to compare the magnitude of the treatment effects for Miami vis-a-vis the placebo effects for the unaffected metropolitan areas and thereby determine if the treatment effects for Miami are large and rare enough so that one can be confident about rejecting a hypothesis of null treatment effects.

As mentioned in Section 4.1, I construct a synthetic control unit iteratively for each of the 26 metropolitan areas in the donor pool and proceed as if each had been affected by the Mariel immigration shock in 1980. As with the case for Miami, the pre-treatment period is taken to be 1973–1981 and the post-treatment period is taken to be 1982–1988. I then measure the differences in outcomes between each metropolitan area and its synthetic control for each year, over the entire period 1973–1988. The differences in outcomes, or gaps, as I shall call them, in each of the post-treatment years represent the placebo treatment effects for each year. Of course, this procedure is also done for Miami in order to derive the actual treatment effects for each year. Such an iterative procedure therefore provides me with a distribution of one treatment and 26 placebo effects.

The results of these placebo tests are presented in Fig. 2. Each panel in the figure to

are 26 placebo estimates and 1 treatment estimate). The estimate which is most negative receives a rank of 1. The second most negative estimate receives a rank of 2 and so on. The *p* value from the test is therefore bounded from below by 0.037 (i.e. 1/27). The lower the *p* value for the treatment effect estimate, the more statistically significant the estimate since it implies that there is a low probability of obtaining placebo effects that are as large as the one obtained for Miami. Results from the more formal test reveal that the impact estimates obtained for Miami are the second most negative in 1983 and most negative in 1986, yielding *p* values of 0.074 (i.e. 2/27) and 0.037 respectively. This demonstrates that there is a low probability of obtaining gaps as large as the ones obtained for Miami during the years 1983 and 1986 and provides one with greater certainty that the estimated negative treatment effects found in these years are statistically significant.

Fig. 2

4.4 Individual difference-in-differences analysis

The earlier analysis relied on aggregate data, albeit constructed from micro-level CPS data.[17] To assess the robustness of the earlier results, I additionally test for impacts on the fertility outcomes of Miami residents using a more traditional difference-in-differences estimator with inference techniques based on CPS micro data.

Similar to the synthetic control approach, the identification strategy used here to estimate the impact of the Mariel influx is to compare native fertility outcomes in Miami, before-and-after the immigration shock, to those in comparable metropolitan areas that were unaffected by the shock. I conduct the difference-in-differences analysis using regression methods to control for differences in sample characteristics which may influence fertility outcomes. The primary regression specification I use is[18]:

$$ \mathrm{FERT}=\alpha +{\sum}_{k=82}^{88} {\delta}_k\left({\mathrm{POSTYR}}_k\times \mathrm{MIAMI}\right)+ {\boldsymbol{Z}}^{\prime}\beta +u $$

(3)

where FERT denotes the fertility status of an individual and is measured by a dummy variable indicating the incidence of a birth in the last 12 months. It is equal to 1 if the observation has had a birth in the past 12 months prior to the survey date and 0

This specification allows the impact of the shock on fertility to vary across the post-influx years and enables one to obtain separately—the treatment effects for each of the years following the Mariel Boatlift (i.e. 1982 to 1988).

The parameters of interest are the ones on the interaction terms, δ_k (with k running 82,...,88), since they measure the year-by-year change in fertility in Miami due to the immigration shock. A negative δ_{82} coefficient, for example, would indicate that the Mariel Boatlift had a negative impact on the fertility of Miami women in 1982. The opposite would be true if this coefficient is positive.

My estimation strategy is to begin with the simplest regression model by including only (a) MSA dummies and (b) year dummies in the vector **Z**. The purpose of the MSA dummies is to capture systematic metropolitan area differences in fertility assumed to be constant over time, while the year dummies are intended to capture time varying effects assumed to be constant across MSAs.

I next add additional covariates which influence fertility but which are likely to be exogenous to individual fertility outcomes. Included in this expanded list of covariates are the following: (c) age dummies—to capture systematic age differences in fertility; (d) dummy variables for ethnicity and family income—intended respectively to capture differences in fertility due to ethnic differences and family incomes; (e) number of previous births by the individual—to allow for the likelihood of a birth occurring to an

for having (more) children, they may respond by choosing a lower level of education so as to increase their chances of fulfilling childbearing goals earlier. Also, since social norms might regard marriage to be a pre-requisite for family formation (Willis 1999), individuals may have a higher likelihood of entering marriage if they have a preference for (more) children.

Finally, I add two interaction terms: (i) an interaction term for ethnicity and education and an interaction term for ethnicity and marital status. These interaction terms are meant to account respectively for the possibility that the effect of education on fertility and the effect of marriage on fertility may be different for women of different ethnicities.

The purpose of starting with regression models with only MSA and year main effects and then adding groups of covariates in a successive manner is to allow one to identify whether there are sets of observable attributes which matter for selection (into whether or not an individual is exposed to the Mariel immigration by living in Miami from 1982). Adding the controls to the regression will change the coefficients on the post-treatment Year–Miami interaction variables if these controls are correlated with the Year–Miami variables, conditional on MSA and year main effects. We include controls for the wide range of demographic and economic characteristics described above because these are potential confounders, likely to be correlated with both the probability of a birth and the probability of being exposed to the Boatlift.

shown in Table 2, panel A so that movements in aggregate fertility for the synthetic control resemble those for Miami in the pre-treatment period.

Table 4 presents results from a variety of regressions showing the estimated impact of the Mariel Boatlift on the fertility of non-Cuban women in Miami. The separate year treatment effects in Table 4 are the estimated coefficients on each of the separate post-treatment Year–Miami interaction variables: Each is obtained by running regression specification (3) using a different set of covariates as discussed earlier. Column (1) displays the estimates from running specification (3) with only the MSA and year dummy variables included in the vector of covariates $\boldsymbol{Z}$. Column (2) shows the estimates when the exogenous variables—namely, the dummy variables for age, ethnicity and family income, the number of previous births occurring to the individual, and the MSA male and female unemployment rates 1 year ago—are added to the vector $\boldsymbol{Z}$. The estimates in columns (3) and (4) are obtained by including the plausibly endogenous variables—namely, the number of years of completed education and a dummy variable for marital status—successively in vector $\boldsymbol{Z}$. Lastly, column (5) presents the estimated coefficients on the Year–Miami interaction terms when the full set of covariates (i.e. all variables listed from (a) to (i)) are included in the regression.[22] As can be seen, the estimated fertility impacts change somewhat from columns (1) to (2) but are very similar across columns (2) through (5). This suggests that while assignment to exposure to the Boatlift depends importantly on the variables (c) to (f) (especially ethnicity and age), it depends less importantly, if at all, on the variables (g) to (i). The estimates from the regression with the full set of covariates included (i.e. column (5)) suggest that the

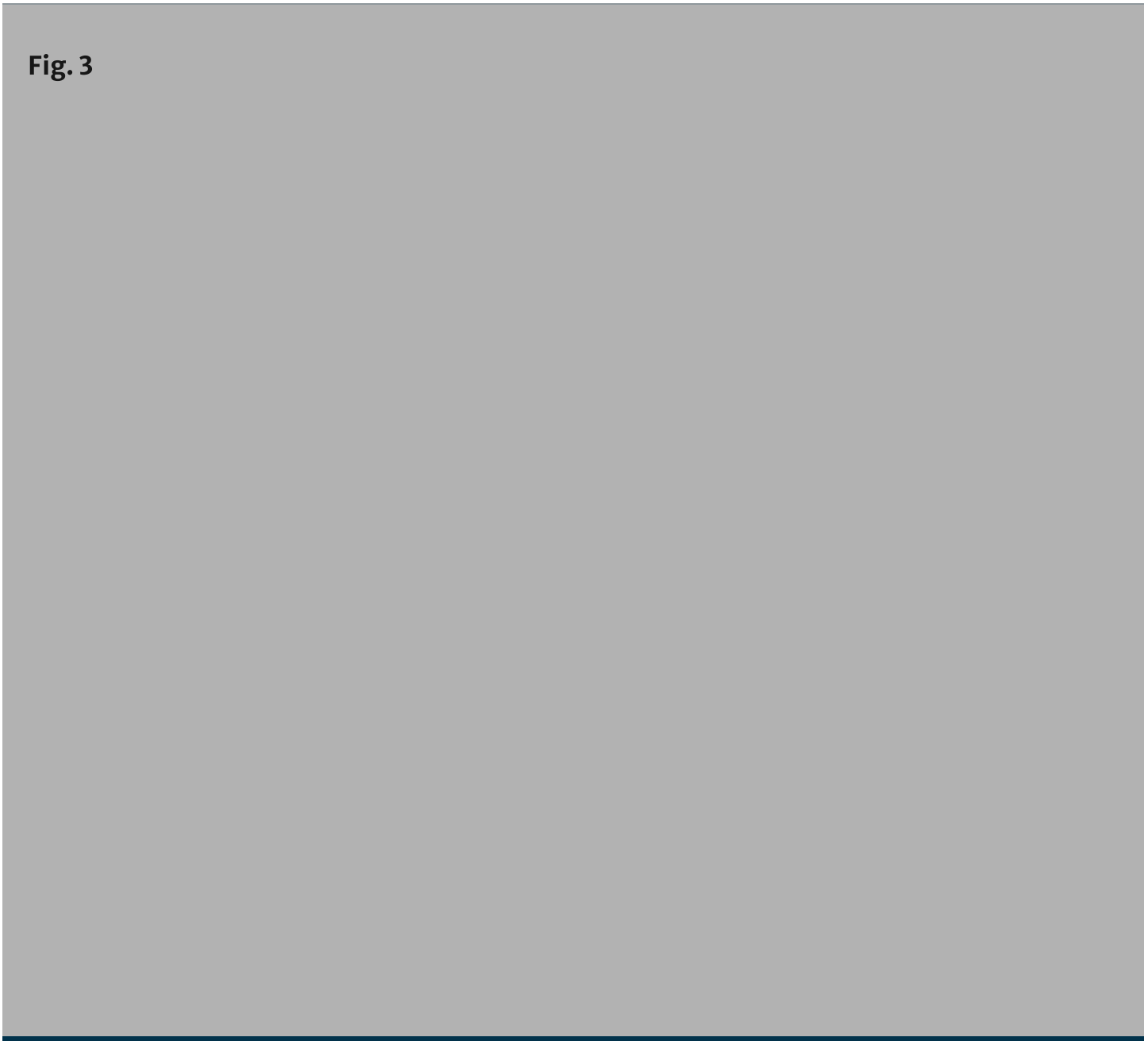
4.4.1 Investigating fertility responses based on differences in homeownership

The impact of immigration shocks on childbearing potentially differs by homeownership status. While an increase in the price of housing accompanying immigration will make childbearing more costly for non-owners (since housing and raising children are complementary), and so decrease their current-period demand for children, it could, by increasing accessible home equity, instead increase homeowners' demand for children (Lovenheim and Mumford 2013; Dettling and Kearney 2014). Further, rent increases due to immigration will leave non-owners with less financial resources for childrearing while providing homeowners with more. Hence, one would expect non-owners and owners to respond differently to the shock. In order to test for heterogeneous impacts, I repeat the above analysis, separating individuals by homeownership status.

In general, women can fall into one of three homeownership groups: Women can either be living in a home (a) owned or bought by a household member, (b) rented for cash, or (c) where occupation is free of charge (i.e. cash rents need not be paid). Because of the tiny number of observations belonging to category (c), I exclude women in category (c) from the analysis. Hence, women are either classified as living in an owned home (homeowner) or a rented home (renter).[24]

We first estimate the fertility impacts on homeowners and renters, separately, using

Fig. 3



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Evolution of renter and homeowner fertility rates in Miami and synthetic control (1980–1988). **a** Evolution of renter fertility rates in Miami and in synthetic control. **b** Evolution of homeowner fertility rates in Miami and in synthetic control

These results are in stark contrast to the fertility changes experienced by women living in owned homes. Column (2) of Table 5 (panel A) displays estimates of the separate-year fertility impacts for home-owning women in Miami. As with the case of renters, only the results where the full set of controls (a) to (i) has been included are displayed.[28] Apart from the 1984 estimate, which is statistically significant at the 10% level, none of the estimated impacts are sizeable or significant. There is therefore little evidence that the Mariel Boatlift affected the fertility outcomes of home-owning women adversely. If anything, the Boatlift may instead have had a positive impact on their fertility.

Do the above conclusions change, if instead, we ran the analysis using an interaction term with homeownership status rather than over two distinct samples? This question is addressed by estimating Eq. (4), including in the vector Z, the full set of control variables (a) to (i):

$$ \mathrm{FERT}=\alpha +{\sum}_{k=82}^{88} {\delta}_k\left({\mathrm{POSTYR}}_k\times \mathrm{MIAMI}\right)+ {\sum}_{k=82}^{88}{\gamma}_k\left({\mathrm{POSTYR}}_k\times

specification are reported separately for renters and homeowners in Table 5 (panel B).[29] They paint a picture similar to the regressions run over the two distinct samples by homeownership status.[30]

Notice the differential impacts that the Mariel Boatlift had on the fertility outcomes of homeowners and renters. While the immigration shock appears to have led to a short-term decline in fertility among renters, no such effect is observed for homeowners. These observations are consistent with the postulates by Dettling and Kearney (2014), which were earlier discussed in Section 2.1, and suggest that an important channel through which immigration shocks affect fertility is the housing market.[31]

One concern is whether the observed differences in fertility impacts between renters and homeowners might be spuriously driven by underlying heterogeneity along the skill dimension. After all, the correlation between homeownership status and skills is reasonably high, with a correlation coefficient of 0.18 between the binary variable indicating whether an individual is a renter and the binary variable indicating whether the individual is a high school dropout (renters are more likely to be high school dropouts). This concern is unlikely to be a problem here since the fertility impact estimates provided in Table 5 and in Appendix Table 8 were obtained from regressions that controlled for women's educational attainment. They are therefore skill-adjusted and they tell us the fertility impacts on renters and homeowners, conditional on skill level (as measured by education).

therefore be problematic. Note that when analysing heterogeneity by skill level, a modification needs to be made to the age restriction of the sample. In particular, the lower bound of the age restriction should be increased from 15 (to something like 25). This is to ensure that only women who would be old enough to have had a choice between completing or dropping out of high school are analysed.

5 Conclusions

This paper represents the first attempt to identify a causal relationship between immigration flows and native childbearing outcomes. It identifies possible channels through which individual childbearing outcomes may be influenced by an immigration influx. The findings are relevant for public policy, especially for countries that are major providers of international refuge since governments would presumably like to know how a mass migration influx—initiated by, say, war, oppression, or political turmoil in the source countries—would affect the future fertility outcomes of their own native population. They are also useful for governments that have chosen to adopt, or are considering adopting, a strategy of relaxing immigration laws and encouraging both permanent and temporary migration into their territories to counter the problems of an ageing population—a phenomenon now common to many developed countries. The findings from this research provide an indication for whether such labour augmenting strategies are appropriate and the likely consequences for the childbearing outcomes of host country natives.

short-lived. There therefore appear to be little long-term or sustained consequences for the fertility of natives from governments pursuing labour augmenting strategies through more open immigration policies.

In addition, fertility effects are found to vary by homeownership: While the immigration shock had a negative impact on the fertility of women living in rented homes, it had practically no effect on women living in owned homes. This differential impact is likely due to the rise in local housing rents accompanying immigration, making childbearing activities less affordable for women living in rented homes. This suggests that the housing market is a key pathway through which immigration shocks affect native fertility.

Although it would have been insightful to examine whether fertility effects are heterogenous across skill levels, the tiny sample sizes which result when the sample is split by educational level (high school dropouts versus high school graduates and higher) prevent this analysis in practice. So I am unable to examine whether the fertility of high school dropouts was differently affected than those of high school graduates, an implication which Borjas' (2017a, b) findings would seem to suggest. More generally, this prevents me from examining whether the overall observed negative fertility effect might also be working through the labour market channel (job insecurity and wages) and therefore to comment on the plausibility of this mechanism.

3. The Mariel Cubans were disproportionately low-skilled. Borjas' (2017a) estimates suggest that only 10% of them were college graduates while the overwhelming majority (60%) were high school dropouts.

4. Borjas (2017a) acknowledges that his finding of adverse wage effects increasing over time is puzzling since theory would predict that wage effects should be largest right after the immigration shock and should weaken with time as capital stock adjusts. However, he rationalises the finding by arguing that wages are sticky downward. Consequently, there might have been a lag before the largest wage effects show up in the labour market.

5. Borjas (2017a) restricts his analysis to the least skilled workers—defined as non-Hispanic men, aged 25–59, with less than high school education.

6. Although there is potentially a difference in the way low-skilled and high-skilled women respond in terms of fertility behaviour to immigration shocks, the direction of this difference is unclear. The increased uncertainty among certain groups of natives (natives, in particular, who are low-skilled, and who might perceive their labour market opportunities to be weaker with the arrival of the Mariel Cubans)

8. To be exact, there were a total of 45 MSAs in the CPS dataset. However, 12 of these MSAs: Milwaukee, Phoenix, Columbus, Rochester, Sacramento, Fort Worth, Birmingham, Albany Schenectady Troy, Norfolk Portsmouth, Akron, Gary Hammond East Chicago, and Greensboro Winston Salem High Point, were created only after 1977. Also, 3 MSAs ceased to exist after 1985: Nassau Suffolk, Newark, and Patterson Clifton Passaic. These MSAs were therefore excluded from our analysis to ensure consistency in the control units throughout the period 1973–1988. Furthermore, the redefinition of MSAs after 1985 resulted in a number of preexisting MSAs being merged with other MSAs: A total of 3 mergers were created: (1) Anaheim Santa Ana Garden Grove—Los Angeles Long Beach, (2) San Bernardino Riverside Ontario—Los Angeles Long Beach, and (3) San Jose—San Francisco Oakland. Hence, observations from the 3 originally independent MSAs: Anaheim Santa Ana Garden Grove, San Bernardino Riverside Ontario, and San Jose, were subsumed into the combined entities. All in all, these meant that only 27 of the original 45 geographically identified MSAs were available for use.

9. Between the years 1973 and 1988, approximately 20% of childbirths in Miami occurred out of wedlock. This figure was approximately 13% for all US metropolitan areas.

10. The literature exploring the relationship between economic activity and fertility

12. Where $j = 2, \ldots . , J+1$ are the J metropolitan areas potentially contributing to the synthetic control unit.

13. Fertility outcomes are computed for the synthetic Miami by taking the weighted average of fertility outcomes pertaining to the metropolitan areas receiving positive weights.

14. This is defined as the proportion of women between 15 and 44 years of age that are currently married and not separated from their spouses.

15. As with all studies using difference-in-differences, we cannot preclude the possibility that the estimated fertility declines might have arisen as a result of some other unobserved shocks specific to Miami at the time (e.g. natural disasters, security threats) rather than due to the Mariel Boatlift. However, the analysis by homeownership status (see Section 4.4.1) does provide some evidence that the Mariel shock had a role to play in the fertility changes, with the relationship operating through the housing price/rents channel.

16. To verify if the observed declines in fertility in 1983 and 1986 had been planned and to ascertain if changes in childbearing decisions were indeed made at around the

18. Subscripts denoting the individual and time are suppressed for simplicity.

19. Initially, I included unemployment rates in the year of the survey and unemployment rates with a 2-year lag as explanatory variables. However, these variables were always found to be jointly insignificant. Hence, they were removed from the regressions.

20. An increase in male unemployment rate in an MSA is expected to lower the likelihood of a birth occurring in that MSA, with a lag. Theory is unclear about how the likelihood of a birth in an MSA would change with an increase in female unemployment rate.

21. The sample includes data over the period 1973–1988, with the exception of years 1975 and 1978.

22. The full set of coefficient estimates for the regressions in Table 4 can be found in Appendix Table 6.

26. Here, only 9 years of data (1980–1988) are considered. This is because information on homeownership was not available prior to 1980. We take 1980–1981 to constitute the pre-treatment years and 1982–1988 to constitute the post-treatment period.

27. Here, the synthetic control is constructed as a weighted combination of 3 metropolitan areas: Los Angeles, Indianapolis, and New Orleans. The precise contributions of the 3 metropolitan areas to the control unit are shown in panel B of Table 2. The same predictors used earlier to analyse fertility outcomes for women age 15–44 are used as characteristics of Miami for which the synthetic control should match (see panel B of Table 3). The results, derived using all other metropolitan areas to form the comparison group, are extremely similar to those found here and can be found in Appendix Table 8 (panel A).

28. Here, the synthetic control is formed by a weighted average of 3 metropolitan areas: Philadelphia, Boston, and Tampa St. Petersburg. The contributions of these metropolitan areas to the synthetic control are given in panel C of Table 2. The same predictors used to analyse fertility outcomes for women age 15–44 are used as characteristics of Miami for which the synthetic control should match (see panel C of Table 3). The results, derived using all other metropolitan areas to form the comparison group, are extremely similar to those found here and can be found

same across both groups of women—homeowners and renters. However, sample sizes are larger with the interaction specification. In any case, both specifications yield similar conclusions: the Boatlift had a negative impact on the fertility of renters but little impact on homeowners.

31. Dettling and Kearney (2014) found that an increase in housing prices decreases the birthrates of non-homeowners and increases the birthrates of homeowners. The latter potentially occurs because a rise in housing price increases home equity for owners. Home equity can, in turn, be extracted through mortgage refinancing or obtaining a home equity loan/line of credit. One reason I find less positive fertility impacts on homeowners in this paper (as compared to Dettling and Kearney) could be due to such loan instruments being less readily available in the 1980s—the time period in which this paper is based.

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Appendix

1.1 Appendix 1

Table 6 Full set of coefficient estimates for Table 4 regressions

1.2 Appendix 2

Table 7 Estimated impacts of Mariel Boatlift (using all other unaffected MSAs as the

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