THE EFFECT OF IMMIGRATION SHOCKS ON NATIVE FERTILITY OUTCOMES: EVIDENCE FROM A NATURAL EXPERIMENT*

Kelvin Seah

Department of Economics

University of Otago

October 2012

ABSTRACT

This paper investigates whether immigration shocks have a causal effect on aggregate native fertility. It uses a natural experiment, exploiting the large, unexpected and localised immigration of Cuban nationals to the Miami area in the United States in 1980 in order to examine the fertility consequences for Miami women. The impact of the immigration shock is estimated by comparing the evolution of fertility outcomes for women living in Miami, after the shock, to those for women living in other areas of the United States unaffected by the Cuban immigration. This is done in 2 ways: Firstly, by applying the synthetic control estimator developed by Abadie et. al. (2010) and assessing the significance of the impact estimates using placebo tests. Secondly, by applying the traditional difference-in-differences estimator and using inference techniques based on actual person-level data to assess the significance of the impact estimates. Both methods lead to the same conclusion: The immigration shock led to short-term declines in native childbearing in Miami during the years 1983 and 1986. The negative fertility impacts in both years were economically large and statistically significant. In addition, fertility effects are found to vary by residential tenure: While the immigration shock had a considerable negative impact on the fertility of women living in rented homes, it had practically 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-Codes: J15, J13

Keywords: Immigration, Native Fertility, Natural Experiment, Synthetic Control

[*] I am grateful to my advisors Steven Stillman, Tarja Viitanen, and Murat Genç for their invaluable guidance, support, and suggestions on this paper.

1. INTRODUCTION

This paper examines whether immigration shocks have a causal effect on aggregate native fertility. 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, reveal that it is possible for native fertility outcomes – both at the individual decision-making and at the aggregate level – to be altered by such inflows.

Immigration shocks may influence the childbearing decisions of natives through at least three 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 amongst these workers. Presupposing that childbearing decisions depend positively on employment and income security (Sobotka et al., 2011), then any 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[2], an increase in housing rents / prices lowers a household's real income and exerts a negative income / substitution effect on the demand for children (Yi and Zhang, 2010). By increasing the cost of raising a child, an increase in the price of living space can also impact fertility by increasing the likelihood that individuals postpone childbearing (Ranjan, 1999). Lastly, immigration can affect native fertility through local price changes. Prior studies that investigate the effects of immigration on prices (Cortes, 2008) and female labour supply (Cortes and Tessada, 2011) find that an increase in the share of low-skilled immigrants in a city's labour force leads to a reduction in the local prices of immigrant-intensive services such as housekeeping and babysitting. Insofar as low-skilled immigration reduces the cost of childbearing and the conflict that native women have over childbearing and work, an increase in low-skilled immigration may work to increase native fertility.

[1] Although it is recognized that the impact of job insecurity on fertility will vary by characteristics such as sex, ethnicity, age, level of completed education, socioeconomic status and/or current family size, there is consensus within the fertility literature that high levels of economic uncertainty are generally expected to have a negative influence on childbearing decisions (Sobotka et al., 2011).

[2] Housing has been found to be a precondition for childbearing, especially in societies where nuclear living arrangements are the norm (Mulder, 2006).

There are two main research questions which this paper seeks to address: Firstly, do immigration shocks affect native childbearing decisions and aggregate fertility outcomes? Secondly, do childbearing responses to an immigration shock vary by population characteristics? That is, do childbearing responses vary by characteristics such as ethnicity, level of completed education, number of children already born to the person, or one's tenure of housing residence? The second question is important because it is unlikely that all individuals will respond to an immigration shock in the same manner. It seems more likely that how individuals respond to an immigration shock in regards to their fertility behaviour will depend very much on their socioeconomic characteristics.

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 native Miami women in the aftermath of the shock can be studied. About half of these immigrants settled in Miami, increasing the Miami labour force by roughly 7% (Card, 1990) and the renter population by at least 9% in 1980 (Saiz, 2003). Of the remaining 50%, thousands were detained and imprisoned by U.S. authorities for crimes committed in Cuba against Castro's government (Masud-Piloto, 1996) while the rest found their way into other parts of the United States. Due to the relatively dispersed nature of their subsequent settlement locations, making for only a very diluted immigration effect in the rest of the United States, the assumption that other metropolitan areas in the United States, apart from Miami, were unaffected by the immigration shock, seems reasonable.

We measure the fertility impact of the immigration shock in 2 ways. First, we apply the synthetic control estimator, using aggregate-level data, and assess the significance of the estimates using placebo tests. Second, we apply the traditional difference-in-differences estimator with inference techniques based on actual person-level data. Both methods lead to the same conclusion: The immigration shock led to short-term declines in native childbearing activity in 1983 and 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.

The rest of this paper is organised as follows: Section 2 provides a review of the relevant literature on immigration and fertility and presents the background of the Mariel Boatlift in greater detail. The data selected for the study and the raw trends in fertility outcomes in Miami before-and-after the Mariel Boatlift are discussed in Section 3. We briefly review the synthetic control estimator in Section 4 before using the synthetic control method to measure the impact of the Mariel Boatlift on the fertility outcomes of native Miami residents. The robustness of the impact estimates are additionally assessed in Section 5 using the traditional difference-in-differences estimator with inference techniques based on actual individual-level survey data. Finally, we conclude with a discussion of our findings in Section 6.

2. BACKGROUND

2.1 Literature Review

A number of studies have documented the way in which natives have responded to immigration inflows. Negative attitudes towards further immigration can arise, in part, due to native workers' perceptions that immigrants constitute a threat to their labour market opportunities and, in part, due to racial prejudice (Dustmann and Preston, 2000; Mayda, 2006; Scheve and Slaughter, 2001; Daniels and Von der Ruhr, 2003). Because certain groups of native workers may perceive immigrants to threaten their labour market opportunities, it seems reasonable to expect that some natives would perceive their jobs and future streams of incomes to be less secure with an immigration shock.

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; Bhaumik and Nugent, 2011; Adsera, 2004; Sleebos, 2003; Hondroyiannis, 2010; Adsera and Menendez, 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 United States, is that immigrant inflows lead to higher local housing prices / rents in the short run (Saiz, 2003, 2007; Ottaviano and Peri, 2007; Gonzalez and Ortega, 2009).

Though recent, a number of studies which examine the relationship between the price of living space and fertility have also found some 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 United States. Using data on Hong Kong, Yi and Zhang (2010) similarly find that an increase in the price of housing significantly decreases total fertility rates in the territory. In contrast to these studies, Dettling and Kearney (2011) show that movements in housing prices can affect the fertility decisions of home owners and non-owners differently. In particular, they find that while an increase in housing prices leads to a fall in birth rates among non-home owners, it has the opposite effect for homeowners. Dettling and Kearney argue that this arises because an increase in housing prices potentially increases accessible home equity for people who own a home and induces a positive income effect on their demand for children. On the other hand, because housing is a major cost associated with (additional) children, an increase in the price of housing exerts a negative substitution effect on the current-period demand for children for people who do not own a home. Therefore, contemporaneous fertility of non-owners falls with rising house prices[3].

The aforementioned studies indicate that immigration shocks may lead to an increase in the perceived job insecurity amongst native workers and to higher housing prices / rents in the destination localities. It also indicates that the price of living space and peoples' perceptions about their future economic positions do affect childbearing decisions. 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.

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 could lead to a reduction in the prices of goods and services in the locality (Cortes, 2008; Lach, 2007). To the extent, therefore, that immigration reduces the cost of childbearing and the conflict that native women have over childbearing and work (Furtado and Hock, 2010; Cortes and Tessada, 2011), it is possible, as well, for increased immigration flows to affect native fertility positively.

The above literature review has highlighted the possibility that immigration flows may have a determining 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 price of living space and a reduction in perceived job security following a positive immigration shock will likely exert a negative effect on the contemporaneous demand for children, the lower price of domestic services complementary to childbearing resulting from largely unskilled or less-skilled immigration flows tends to create a positive effect.

2.2 The Mariel Boatlift

The 1980 Mariel Boatlift presents an interesting opportunity to study the effects of a large and unexpected immigration wave on the fertility outcomes for natives. Card (1990) documents that some 125,000 Cuban immigrants arrived in Miami from May to September 1980, driven by Fidel Castro's announcement on April 20 1980 that Cubans wishing to leave the country could do so from the port of Mariel. Card's estimates suggest that 50% of the Mariel immigrants eventually settled in Miami,

[3] In general, an increase in housing price exerts both a positive income effect and a negative price effect on people who own a home. For current homeowners who may buy a larger house with the addition of a child, the effect of higher house prices on current period fertility will depend on the relative strengths of the positive home equity effect from the increase in value of their existing homes and the negative price effect due to the increase in value of all other houses. Current period fertility will rise if the positive home equity effect dominates the negative price effect. Current period fertility will fall if the converse happens. For non-owners, an increase in house price produces only a negative price effect on the demand for children – no income effect is generated.

increasing the overall labour force of Miami by approximately 7%. In a related study examining the response of housing markets to immigration shocks, Siaz (2003) estimates that the influx increased Miami's renter population by at least 9% in one year (in 1980).

Despite the absence of any notable impact on the labour market outcomes of Miami residents, Card notes that the Boatlift did cause a strain within the Miami community. In particular, he notes the occurrence of a three day riot within several African American neighbourhoods, sparked in part, by the labour market competition posed by the Mariel immigrants. This suggests that the Miami residents – notably African American workers – could have perceived their labour market opportunities to have been threatened by the arrival of the immigrants. Assuming this is true, we would expect the increase in uncertainty to have a negative bearing on fertility as people postpone their childbearing plans in anticipation of tougher economic conditions in the future (Ranjan, 1999; Sobotka et al., 2011).

Siaz's (2003) analysis of the housing market in Miami following the Mariel Boatlift presents evidence of greater real effects generated by the Boatlift. Estimates from the study indicate that the Boatlift led to rents in Miami increasing by 8% to 11% more in real terms relative to those in three groups of comparison metropolitan areas from 1979 to 1981. 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, 2011).

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 micro data from the U.S. consumer price index, the study finds that a higher share of low-skilled immigrants in the labour force reduces the price of immigrant-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 Supplement Series

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

The survey provides information on each respondent's demographic (e.g. age, race, Hispanic origin, sex, etc), educational, income, and geographic characteristics; labour force activity; and marital history. Questions on fertility history were asked of all women aged 18 to 75 regardless of whether they were married and were asked of women aged 15 to 17 if they had ever been married. Responses to the fertility history provide amongst other information, data on the month and year of respondents' most recent child and the number of births that a respondent has had. Approximately 60,000 households are interviewed during each survey in June. The units of observation are individuals within households.

The CPS sample is intended to be representative of the civilian non-institutional population of the United States and weights are provided so that estimates from the samples can 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 United States. A total of 28 MSAs exist in the dataset[4]. 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 United States.

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 us 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 us to control for individual-level characteristics that may influence individual fertility decisions and aggregate fertility outcomes.

Secondly, because the CPS allows for individuals within the sample to be identified based on ethnicity; it allows us to construct aggregate fertility rates pertaining only to non-Cubans. Excluding

[4] To be exact, there were a total of 45 MSAs in the CPS dataset. However, 11 of these MSAs: 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 28 of the original 45 geographically identified MSAs were available for use.

Cubans from the analysis is important because: (1) we intend to restrict the scope of our study to evaluating the effects of an immigration shock on the fertility outcomes of natives – that is, residents already living in Miami prior to the Boatlift (2) it is difficult to distinguish between births occurring to Cubans that settled in Miami prior to the Boatlift from those occurring to the Mariel Cubans. Because theory is unclear on how the fertility outcomes among the Mariel Cubans themselves would be affected by large-scale emigration, all births pertaining to women of Cuban descent are removed from the analysis. In contrast, aggregate birth rate data from the U.S. vital statistics are not tailored to pertain specifically to the non-Cuban population.

Thirdly, the childbearing performance of individuals in a metropolitan area (i.e. metropolitan area-level fertility) is not well captured by readily available aggregate fertility data such as those from the U.S. vital statistics. In particular, vital statistics report crude birth rates – defined as the annual number of births occurring in a metropolitan area per 1000 persons living in the area. This is a somewhat crude measure of metropolitan fertility since it normalizes 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 the number of children per woman in the area, which is what we are really interested in knowing, or to a relative decline in the (male) population in that area during this time. The aggregate fertility measure we 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 normalizing births to a more "at risk" population (i.e. women of childbearing age), enabling us to conduct the analysis with a more refined measure of fertility (Namboodiri, 1996).

3.2 The Sample

We include in our 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 percentage of childbirths occurred out of wedlock[5], 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 United States (Willis, 1999). Both married and unmarried women are therefore included in our sample. The non-Cuban sample from Miami includes approximately 160 observations per year while the sample including Cubans includes roughly 200 observations.

[5] Between the years 1973-1988, approximately 26% of childbirths in Miami occurred out of wedlock. This figure was approximately 20% for all U.S. metropolitan areas.

3.3 Patterns in General Fertility Rates: 1973-1988

The evolution in general fertility rates from 1973 to 1988 for non-Cuban Miami women are shown in Table 1. The fertility rates pertain respectively to: (1) all females of childbearing age (age 15-44); (2) all married females of childbearing age; (3) all females in the prime childbearing ages (age 18-34); and (4) all married females in the prime childbearing ages.

The patterns in fertility rates exhibited by all 4 groups are rather similar throughout the entire period of the analysis. All are relatively constant from 1973 to 1976 and show a sharp decrease in 1977. They remain low in 1979 before returning to levels in line with those observed for 1973-1976 in 1980. In all cases, fertility rates fell sharply in 1983 and remained fairly low till 1984. However, fertility rates seem to return to and even exceeded the high early 1980s levels, in 1985. Fertility rates fell again in 1986 but exhibited an upward trend thereafter.

Assuming it takes approximately 2 to 2.5 years for the Mariel Boatlift to affect the childbearing outcomes of Miami residents through the hypothesized channels, the trends observed in Table 1 are consistent with a temporary delay in the timing of childbearing[6] after the Mariel 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). 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 patterns, though, 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. We adapt the case study, originally used by Card (1990)[7], to investigate the impact on native fertility in Miami after it received a large and unexpected number of immigrants from Cuba between May to September 1980. The impact of the Mariel immigration shock is estimated by

[6] See Sobotka et al. (2011). The literature exploring the relationship between economic activity and fertility has sometimes distinguished between short-term and permanent changes in fertility resulting from a contraction in economic activity. While the latter represents a quantum downward shift in fertility which will likely have consequences for completed fertility, the former usually arises from a shift in the timing of childbearing (i.e. postponement of childbearing) which is typically compensated in later years. Short-term changes in fertility are conceptualized, in the aggregate, by a decline in fertility rates, concentrated at the younger ages, and followed by a "catch-up" increase at older ages.

[7] Card (1990) uses the Mariel Boatlift as a natural experiment to identify the impact of an immigration shock on native labour market outcomes.

comparing the evolution of fertility outcomes for Miami after the immigration shock to those for comparable geographic regions within the United States which were unaffected by the shock.

The idea is that the evolution in the fertility outcomes for the control regions can be used as counterfactuals to estimate how fertility outcomes in Miami would have evolved, in the post-treatment period, had the Mariel Boatlift not occurred. Hence, by comparing the actual evolution of fertility outcomes for Miami after the immigration shock to the counterfactual evolution provided for by the control regions, one is able to estimate the fertility impacts of the immigration shock.

In all subsequent analyses, we take the year 1982 to be the first year for which fertility outcomes in Miami are likely to be affected by the immigration shock. The year 1982 therefore represents the start of the post-treatment period. While it is possible that fertility outcomes might have been affected in as early as 1981, the likelihood of this is low. The reason is that if a respondent reports having a birth in the last 12 months during the 1981 survey (conducted in June 1981), this would mean that the birth(s) must have occurred between the period July 1980 - 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 to 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. We 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 influence on the fertility of native women, we cannot be certain that the observed declines in fertility were due to the Mariel Boatlift rather than other confounding factors (eg. common fertility shocks affecting all metropolitan areas in the United States) unless we know how fertility outcomes would have evolved in Miami in the absence of the immigration influx.

Ideally, we would like to know how fertility outcomes in Miami would have evolved during the period post-1982 in the absence of the immigration shock so that we can compare this evolution to the actual post-1982 evolution to identify whether the observed declines in fertility were indeed the result of the influx. If we had knowledge regarding how fertility outcomes would have evolved in Miami in the alternative world where Miami had not been affected by the Mariel influx, then we can compare the actual evolution of fertility outcomes in Miami to the counterfactual evolution to identify the impact of the Boatlift on fertility.

Although it is impossible to observe the fertility outcomes in Miami in the absence of the Mariel Boatlift, it is possible to approximate how fertility outcomes in Miami would have evolved in the counterfactual situation if one can find a suitable comparison area (or a group of comparison areas) which was unaffected by the Mariel influx and is able 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 we 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 to. 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 criticized as 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 formalized 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 resultant comparison area in the post-treatment period are taken to reflect how outcomes in the treated area would have changed, absent the treatment. Since the synthetic comparison area is constructed to resemble the treated area, the selection procedure provides one with greater confidence that any observed differences in the changes in fertility outcomes between the treated and control areas are due to the immigration shock rather than to poor identification design – wherein differences arise only because the treated and comparison areas are fundamentally different to start with.

Because of the advantages presented by the synthetic control method developed by Abadie et al. (2010) over the more adhoc technique, the present paper employs this method to reproduce the counterfactual fertility path for Miami in the absence of the Boatlift to provide a reference for which the actual fertility evolution can be compared to.

4.1 The Synthetic Control Method

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 $t = 1, 2 \ldots, T$ time periods. Let T_0 be the number of pre-treatment periods, where $1 \le T_0 \le T$. Also, let $\boldsymbol{X}_1 = (Y_{11}, \ldots, Y_{1T_0}, \boldsymbol{Z}_1')'$ be a vector of pre-treatment characteristics for Miami that includes the fertility outcomes in each year of the pre-treatment period (i.e. $Y_{11,\ldots,}Y_{1T_0}$) as well as covariates that are predictive of metropolitan area fertility (given by the vector $\boldsymbol{Z}_1$). Similarly, let $\boldsymbol{X}_0$ be a matrix containing the same variables for each of the J metropolitan areas potentially contributing to the comparison synthetic control unit[8]. The idea behind the synthetic control method is to choose an optimal weighting vector $\boldsymbol{W}^* = (w_2^*, \ldots, w_{J+1}^*)'$ such that it minimises:

$$\|\boldsymbol{X_1} - \boldsymbol{X_0 W}\| \qquad (1)$$

subject to

$$w_j \ge 0 \quad \text{and} \quad \sum_{j=2}^{J+1} w_j = 1 \qquad \text{for } j = 2, \ldots, J+1^9$$

where w_j represents the weight of metropolitan area j in the synthetic control.

Each metropolitan area's contribution to the synthetic version of Miami would be given by the weights contained in the optimal weighting vector. In doing so, the synthetic control is designed to resemble the actual Miami in terms of its attributes in the pre-treatment period. The fertility outcomes pertaining to the synthetic control in the post-treatment period is then taken to be an approximation of the actual fertility outcomes that would have been observed in Miami had the Mariel Boatlift not occurred. The pre and the post-treatment fertility outcomes are computed for the synthetic Miami by taking the weighted average of fertility outcomes pertaining to the metropolitan areas receiving positive weights. The evolution of fertility outcomes for Miami in the post-treatment period is compared to that for the synthetic control in order to identify the effects of the immigration shock on the fertility outcomes in Miami.

More formally, let the average treatment effect be:

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

[8] Each of the columns in the $(k \ x \ J)$ matrix is a data vector containing pre-intervention fertility outcomes and covariates predictive of metropolitan area fertility for a different metropolitan area.
[9] Where $j = 2, \ldots, J + 1$ are the J metropolitan areas potentially contributing to the synthetic control unit.

where $\overline{y_{2,Miami}}$ denotes average fertility in Miami in the post-treatment period, $\overline{y_{1,Miami}}$ denotes average fertility in Miami in the pre-treatment period; $\overline{y_{2,Control}}$ denotes average fertility in "synthetic Miami" in the post-treatment period; and $\overline{y_{1,Control}}$ denotes average fertility in "synthetic Miami" in the pre-treatment period. $\widehat{\delta}$ is thus an estimate of the effect of the immigration shock on the average fertility of Miami residents. If $\widehat{\delta} < 0$, then this is evidence that the Mariel Boatlift had a negative impact on native fertility outcomes in Miami. The converse is true if $\widehat{\delta} > 0$.

The significance of these treatment 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 metropolitan area as if each had been affected by the Mariel Boatlift in 1980. The distribution of these placebo estimates then form a sampling distribution for the actual impact estimates with which to test against the hypothesis of null treatment effects.

The idea is that since the other metropolitan areas were not affected by the Mariel Boatlift in 1980, the evolution of fertility outcomes for these areas, should in principal, follow those of their synthetic controls closely. In other words, we should not see outcomes for these areas deviating from those of their synthetic versions in the post-treatment period. Hence, by comparing the estimated treatment effect for Miami to these placebo effects, we can ascertain the rarity of the magnitude of the treatment effect. The null hypothesis is rejected if there is clear indication that the changes in fertility outcome in Miami relative to its synthetic control after the Boatlift exceed in magnitude those pertaining to these other metropolitan areas. This inferential technique is akin to a permutation test because it allows one to assess if the effect estimated for Miami is large relative to the placebo effects estimated for some other metropolitan area drawn at random (Abadie et al., 2011).

4.2 Estimated Impacts on Fertility Outcomes

The fertility outcomes over the period 1973 to 1988 presented in Section 3.3 showed that trends in fertility rates were similar for: (1) all women of childbearing ages, (2) married women of childbearing ages, (3) women of prime childbearing ages and, (4) married women of prime childbearing ages. Because of the considerably larger sample sizes provided by women of childbearing ages relative to the other three groups, we conduct the rest of our analysis taking all females of childbearing age (i.e. females age 15 to

44) to be the reference population with which fertility rates are derived and with which changes in fertility are analysed[10].

We assume that the immigration shock is localised only to Miami and that all other metropolitan areas in the United States contributing to the comparison groups are unaffected by it. Further, we 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.

Let us 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 St. Louis, Cleveland, Houston, San Diego, Tampa St. Petersburg, and Portland, with Panel A in Table 2 showing the exact weights assigned to these areas for the purposes of constructing our control. These metropolitan units were chosen based on the minimization 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, we include in the set of characteristics to be matched – the MSA marriage rate[11], 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[12], the proportion of women in the MSA falling within each ethnic category[13], the proportion of women in each MSA falling within each family income category[14], the proportion of women falling within each 5 year age category[15], and the proportion of women falling within each educational attainment category[16]. These variables were chosen because they have been identified in the fertility literature as being the most relevant for predicting fertility rates in MSAs. Notice that we include male and female unemployment rates separately as predictors of fertility outcomes. This

[10] All persons of Cuban origin are removed from our samples. This is done primarily because it is difficult to distinguish between the earlier Cubans that settled in Miami before the Mariel occupation from those arriving with the Boatlift. Recall that what we are really interested in knowing is how the childbearing performance of Miami natives – defined as all persons living in Miami *prior* to the Boatlift – changed in the years following the immigration shock.

[11] 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.

[12] In addition to the current male and female unemployment rates and the 1-year lagged male and female unemployment rates, I initially also included 2-year lagged male and female unemployment rates in the list of predictor variables. However, the inclusion of these unemployment rates led to relatively higher mean squared prediction errors, indicating that these variables do not predict contemporaneous MSA-level fertility rates well. They were therefore removed from the analyses.

[13] Individuals can fall into 1 of 4 of these ethnic groups: White, African American, Hispanic, or Other race.

[14] Individuals can fall into 1 of 6 of these family income categories: having a family with annual income (1) below US$5,000, (2) between US$5,000 to US$7,499, (3) between $7,500 to US$9,999, (4) between US$10,000 to US$14,999, (5) between US$15,000 to US$24,999, or (6) above US$25,000.

[15] Individuals can fall into 1 of these 6 age categories: (1) age 15 to 19, (2) age 20 to 24, (3) age 25 to 29, (4) age 30 to 34, (5) age 35 to 39, or (6) age 40 to 44.

[16] Individuals can fall into 1 of 11 of these educational categories: (1) 0 to 8 years of completed education, (2) 9 years of completed education, (3) 10 years of completed education, (4) 11 years of completed education, (5) 12 years of completed education, (6) 13 years of completed education, (7) 14 years of completed education, (8) 15 years of completed education, (9) 16 years of completed education, (10) 17 years of completed education, or (11) 18 or more years of completed education.

is meant to account for the fact that male and female unemployment can impact the fertility outcomes for women differently. While higher male unemployment in an MSA is generally expected to lower fertility rates since male incomes are important in providing the financial resources necessary for childbearing goals, it is unclear how higher female unemployment would affect fertility. On one hand, unemployment amongst females may induce them to enter motherhood since the opportunity costs associated with childbearing are now lower. On the other, unemployment and the reduced stream of income flows can lead women to refrain from entering into long term financial commitments (having a child is necessarily a long-term financial investment) and in so doing, postpone or cancel childbearing plans (O'Donoghue and O'Shea, 2006).

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 falling within each ethnic category, the values of the rest of the characteristics are remarkably similar for both Miami and the synthetic during this period. The reason that the proportion of women falling in each ethnic category for the synthetic does not match those for Miami well 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 United States 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.

Figure 1 (Panel A) plots the fertility rates experienced in the two areas over the period 1973-1988. The movements in fertility rates for Miami and its synthetic are quite similar for the entire pre-treatment period (1973-1981). The similarity in pre-treatment fertility trends and characteristics between Miami and the control suggests that the synthetic control provides a reasonable counterfactual for Miami in the absence of the Mariel Boatlift. The fertility patterns exhibited by the synthetic control over the post-treatment period (1982-1988) can therefore be used to predict how fertility outcomes in Miami would have moved from year to year in the absence of the Boatlift. As can be seen, outcomes begin to diverge between Miami and the control after 1982. While fertility rates in Miami declined sharply from 1982 to 1983, the rates in the synthetic control demonstrated an increase instead during this time. Fertility rates appear to return to step 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, 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.

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 occur, the divergences in fertility paths between Miami and the synthetic after 1982 provides 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 3.7% and 2.5% more than those predicted by the synthetic control respectively in 1983 and 1986. These year treatment effects are calculated based on the formula in equation (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[17].

Calculating the average treatment effect based on the formula in equation (2), and taking 1973-1981 to constitute pre-treatment years and 1982-1988 to constitute post-treatment years, one finds that fertility *decreased by 0.25%* 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 prior to and after the treatment – then a very different conclusion is reached: In this case, fertility would have *declined by 3.3%* in Miami after the influx relative to the control. This is not surprising because the declines in fertility notably occur in 1983 and 1984. The smaller impact estimate obtained from using 1973-1981 and 1982-1988 as the relevant pre- and post-treatment periods masks the actual negative impact of the immigration shock because the decreases in fertility were only temporary, with fertility rising above predicted levels after each episode of fertility decline[18].

---

[17] Both Card (1990) and Saiz (2003) report that population growth rates in Miami slowed after the Mariel Boatlift. This could have occurred because natives living in Miami responded to the immigration inflow by moving out of Miami. It could also have occurred because the inflow deterred potential migrants living within the rest of the United States from moving to Miami. To the extent that the age distribution of women within the childbearing age (i.e. 15-44) group was significantly altered by the out-migration of existing Miami residents, inference on the effects of the immigration shock on fertility 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, the female sample was separated into 6 age categories: (1) age 15-19, (2) age 20-24, (3) age 25-29, (4) age 30-34, (5) age 35-39, and (6) 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 as a result of the Mariel Boatlift. If so, then this would be indicative of a shift in the age composition of women in Miami following the Boatlift. The results from the analysis, which are not shown here for brevity, indicate that the Mariel Boatlift did not have any systematic effect on the age distribution of women in Miami. Hence, there seems to be little need for worry about shifts in Miami's female composition introducing biases to the fertility impact estimates.

[18] 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 same time as when the Mariel Boatlift occurred, we additionally examine whether the childbearing intentions among Miami women changed after the Boatlift (Schoen et al., 1999). As with analyzing changes in fertility outcomes, synthetic comparison controls for Miami were constructed to approximate how movements in the

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, it may be inferred 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 entirely by chance. Could it be possible that the divergences occurred simply because of the inability of the synthetic control method to reproduce the counterfactual fertility paths for Miami in the post-treatment period? If so, then our estimated treatment effects may be biased[19]. In order to test if the estimates are statistically significant, a series of placebo tests are performed. This involves applying the synthetic control method to each of the 27 metropolitan areas in the donor pool (i.e. metropolitan areas which were unaffected by the Mariel Boatlift in 1980) to see if the placebo effects generated for 1983 and 1986 for each metropolitan area are as large as the one obtained for Miami. In effect, what we want to know is whether we would see a negative fertility effect in 1983 and 1986 that is as large as the one seen for Miami if we happen to pick at random another metropolitan area for the study rather than Miami. If the placebo studies yield placebo treatment effects that are as large as the ones estimated for Miami, then this indicates that our analysis fails to provide sufficient evidence for the Mariel Boatlift having a temporary negative effect on fertility in Miami during these two 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 some real 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, we construct a synthetic control unit iteratively for each of the 27 metropolitan areas in the donor pool and proceed as if each had been affected by the Mariel immigration

---

fertility intentions (as measured by the proportion of women in an MSA that expect to have more children in the years ahead) among non-Cuban women in Miami would have evolved in the absence of the Mariel Boatlift. Results from the analysis showed that relative to the synthetic control, the actual proportion of women in Miami expecting to have additional children fell by approximately 12.9% and 12.5% respectively in 1981 and 1985 – the years preceding the actual fertility declines. Estimates of the average treatment effect indicate that the proportion of women in Miami expecting to have additional children fell by approximately 6.6% after the Boatlift.

[19] Furthermore, because micro-level survey data were employed to construct MSA-level fertility rates, any differences in the evolution of outcomes might be reflecting the additional source of uncertainty from sampling errors as opposed to true causal effects of the Boatlift.

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. We 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 we 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. This iterative procedure therefore provides us with a distribution of 1 treatment and 27 placebo effects.

The results of the placebo test are presented in Figure 1 (Panel B). Each grey line in the figure traces the yearly differences in fertility rates between each metropolitan area in the donor pool and its synthetic counterpart over the period 1973-1988. The red line denotes the estimated yearly differences for the case of Miami. As can be seen from the diagram, the estimated gap for Miami during the years 1982-1983 and 1985-1986 are large relative to those estimated for the other metropolitan areas in the donor pool. To reduce the subjectivity of this interpretation based on simple visual identification, we additionally conduct a more formal test of the significance of the treatment effect estimates as suggested in Bohn et al. (2011). This involves comparing the actual treatment effect estimates for 1983 and 1986 to the complete distribution of the other 27 placebo effect estimates to determine if the treatment effect estimates for 1983 and 1986 are indeed much more negative than the placebo estimates. A simple p-value for the one-tailed test of the likelihood of observing an estimate at least as negative as that for Miami can be computed for 1983 and 1986 by taking the rank of the estimates obtained for Miami and dividing this by 28 (since there are 27 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 0.036 (i.e. 1/28). 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. Table 4 summarizes the fertility impact estimates obtained for women age 15-44 during the years 1983 and 1986 and indicates the p-values of the likelihood of observing placebo estimates that are at least as negative as the ones obtained for Miami. As the results in Table 4 show, the impact estimates obtained for Miami are the second most negative in 1983 and most negative in 1986, yielding p-values of 0.071 (i.e. 2/28) and 0.036 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 us with greater certainty that the estimated negative treatment effects we found in these years are statistically significant.

5. INDIVIDUAL DIFFERENCE-IN-DIFFERENCES ANALYSIS

The earlier analysis relied on aggregate data, albeit constructed from micro-level CPS data[20]. To assess the robustness of the earlier results, we additionally test for impacts on the fertility outcomes of Miami residents using a more traditional difference-in-differences estimator with inference techniques based on actual CPS micro data.

5.1 Regression Analysis of the Impact of the Mariel Boatlift on Native Fertility Outcomes in Miami

Similar to the synthetic control approach, the identification strategy we use here to identify and 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. We conduct our difference-in-differences analysis using regression methods to control for differences in sample characteristics which may influence fertility outcomes. The primary regression specification we use is:

$$FERT = \delta_0 + \delta_1 y82.MIAMI + \delta_2 y83.MIAMI + \delta_3 y84.MIAMI + \delta_4 y85.MIAMI + \delta_5 y86.MIAMI + \delta_6 y87.MIAMI + \delta_7 y88.MIAMI + \boldsymbol{Z}'\theta + u \qquad (3)$$

Where "$FERT$" represents 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 coded 1 if the observation has given birth in the past 12 months prior to the survey date and 0 otherwise. "$y82$" is a dummy variable coded 1 if the observation is from the year 1982 and 0 if it is not; "$y83$",…,"$y88$" are dummy variables similarly defined for the years 1983,…,1988. "$y82.MIAMI$" represents an interaction of the 1982 year dummy with the "$MIAMI$" dummy; "$y83.MIAMI$",…, "$y88.MIAMI$" are similarly defined. $\boldsymbol{Z}$ is a vector of MSA- and individual-level covariates which control for other factors influencing individual-level fertility outcomes. Finally, u represents the error term which captures unobserved effects influencing fertility.

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

The parameters we are interested in are on the interaction terms "$y82.MIAMI$",…, "$y88.MIAMI$" since these measure the year-by year change in fertility in Miami due to the immigration influx. A negative coefficient on the "$y82.MIAMI$" variable, for example, would provide evidence that the fertility outcomes of Miami women in 1982 were negatively affected by the immigration shock. The opposite is true if a positive coefficient estimate is obtained.

[20] MSA-specific birth rates, fertility intentions, and MSA-specific characteristics were derived using data from the CPS.

Our estimation strategy is to begin with the simplest regression model by including only (a) MSA dummies and (b) year dummies in the vector $\boldsymbol{Z}$. 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.

We next add additional covariates which influence fertility but which are likely to be exogenous to individual fertility decisions and outcomes. Included in this expanded list of covariates are: (c) age dummy variables – to capture systematic age differences in fertility; (d) dummy variables for ethnicity and family income[21] – 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 individual to change as birth parity increases, and (f) MSA-level male and female unemployment rates 1-year prior to the survey[22] – to allow for the likelihood of a birth to vary with economic conditions[23].

Next, we add covariates that potentially determine fertility outcomes but which are at the same time possibly endogenous to fertility outcomes. We add these variables one-by-one to the regression specification to see whether their inclusion changes the estimated coefficients on the Year–Miami interaction terms in any considerable way. These covariates are (g) a variable indicating the level of completed education of an individual, and (h) a dummy variable for marital status.

Covariates (g) and (h) are essentially choice variables that might depend on an individual's preference for having children. For example, if individuals have a preference 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, we add two interaction terms: (i) an interaction term for ethnicity and education and an interaction term for ethnicity and marital status.

[21] Observations fall into 1 of 4 ethnic groups: White, African American, Hispanic, or Other race. Observations may fall into 1 of 6 of family income (annual) categories: income of (1) under US$5,000, (2) US$5,000-US$7,499, (3) US$7,500-US$9,999, (4) US$10,000-US$14,999, (5) US$15,000-US$24,999, or (6) above US$25,000.

[22] Initially, I included unemployment rates in the year of the survey and unemployment rates with a 2-year lag as explanatory variables in the regressions. However, these variables do not explain the variation in fertility well. In most cases, their coefficient estimates were insignificant. Because parsimonious models are preferred, they were therefore removed from the regressions.

[23] 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.

The covariates listed in (c) to (h) are intended to account for the possibility that the sample characteristics and economic conditions may be markedly different within the same geographic area before-and-after the immigration influx and also the possibility that the sample characteristics and economic conditions may be different across geographic areas in the same year. The covariates in (c) to (h) therefore act to control for possible differences in sample characteristics which might affect fertility. The interaction terms listed in (i) 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. Covariates (c) to (i) have been found to be important determinants of contemporaneous fertility in the demography literature.

Because the approach taken basically identifies impacts by comparing group averages of outcome variables, the standard errors presented are cluster-robust standard errors that allow for arbitrary correlation in individual error terms within metropolitan area cells.

The selection of metropolitan areas to form comparison groups which serve as counterfactuals for Miami in the absence of the Boatlift is done in 2 ways: Firstly, by applying the synthetic control method discussed in Section 4.1 to select and weight comparison units. This means that the metropolitan areas contributing to the comparison groups are the same as those presented in Section 4.2. Essentially, we first sort the individual-level observations by metropolitan areas, before using the weights derived from the synthetic control method to weigh the contributions for each observation to the comparison group. Secondly, by constructing a comparison group consisting of all metropolitan statistical areas apart from Miami. This comparison group is intended to capture national trends in fertility and economic conditions.

We begin by focusing on the fertility outcomes pertaining to non-Cuban women of childbearing ages (women age 15 to 44). As with the synthetic control approach, the first control group consists of observations from St. Louis, Cleveland, Houston, San Diego, Tampa St. Petersburg, and Portland. We weigh each individual-level observation in these metropolitan areas by the weights shown in Table 2, Panel A so that movements in aggregate fertility for the synthetic resemble those for Miami in the pre-treatment period.

Tables 5 and 6 present results from a variety of regressions showing the estimated impact of the Mariel Boatlift on the fertility of native women in Miami[24]. The results in Table 5 pertain to those derived when the comparison group is chosen using the synthetic control method while Table 6 shows the results derived when the comparison group is chosen such that it consists of all the other MSAs apart from Miami. As the results are similar for both comparison groups, we shall focus only on interpreting and

[24] Since the dependent variable is essentially binary, for each case, I run the regressions using both probit and ordinary least squares (OLS). However, to reduce clutter, only the OLS estimates are presented here. It is worth noting though that the estimates using probit are very similar to those obtained using OLS.

discussing the results where the comparison group is selected using the synthetic control method (i.e. Table 5). The separate year treatment effects in Tables 5 and 6 are the estimated coefficients on each of the separate post-treatment Year–Miami interaction variables: Each is derived by running regression specification (3) using a different set of covariates as discussed earlier. Columns (1) display the estimates from running specification (3) with only the MSA and year dummy variables included in the vector of covariates $\boldsymbol{Z}$. Columns (2) show the estimates when the exogenous explanatory 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 **Z**. The estimates in columns (3) and (4) are derived by including the plausibly endogenous explanatory variables – namely, the number of years of completed education and a dummy variable for marital status – one-by-one in vector $\boldsymbol{Z}$. Hence, columns (3) show the estimates with only years of completed education added to the pool of covariates while columns (4) show the estimates when both variables – years of completed education and marital status – are added to the pool of covariates. Lastly, columns (5) present 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. As can be seen, the estimated fertility impacts change little across the five columns. The estimates from the regression with the full set of covariates included (i.e. column (5)) suggest that the Boatlift had a varying impact across the 7 post-treatment years, with fertility effects varying from a 1.0% increase in 1982 (insignificant at any conventional level) to a 5.1% decline in 1983 (significant at the 5% level) and a 3.1% decrease in 1986 (significant at the 10% level) to a 1.6% increase in 1988 (insignificant at the conventional levels). However, of all the impact estimates, only the negative point estimates for 1983 and 1986 are statistically significantly different from zero. The estimated impacts for all the other post-treatment years, whether positive or negative, are insignificant at the conventional levels. As the results in Table 6 show, the year-specific impact estimates are very similar in both magnitude and statistical significance if we had taken instead all other unaffected MSAs to constitute the comparison group. These results are in agreement with those derived earlier using the synthetic control approach, demonstrating that the Mariel Boatlift had a statistically significant negative causal impact on the fertility of Miami women in 1983 and 1986.

5.2 Investigating Fertility Responses based on Differences in Residential Tenure

Because childbearing responses to an immigration shock can vary based on individual characteristics (Sobotka et al., 2011), there may possibly be heterogeneity in responses based on differences by: (1) ethnicity, (2) number of children already born to an individual, (3) level of completed education, and (4) residential tenure. In order to test for these possibilities, we repeat the analysis, separating individuals into sub-groups based on differences with respect to these characteristics, to identify possible differences in impacts across groups. The differential impacts on fertility based on differences in ethnicity, birth parity, and level of completed education were not remarkable and therefore we do not elaborate on them further here[25]. More interestingly however, a large differential impact on fertility was found between renters and homeowners, suggesting that fertility responses to an immigration shock differ by housing tenure.

In general, women can fall into 1 of 3 residential tenure 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). For simplicity and because of the tiny sample sizes belonging to category (c)[26], we exclude women in category (c) from the analysis by residential tenure status. Hence, women are either classified as living in an owned home (i.e. home-owner) or a rented home (i.e. home-renter).

We estimate the average fertility impact pertaining to homeowners and renters, separately, using the specification:

$$FERT = \delta_0 + \delta_1 AFTER.MIAMI + \boldsymbol{Z}'\theta + u \qquad (4)$$

Where "$FERT$", "$\boldsymbol{Z}$", and "u" are as defined previously. "$AFTER.MIAMI$" is a dummy variable equal to 1 if the observation is from Miami in the post-treatment period and equal to 0 otherwise. The parameter of interest, δ_1, measures the average treatment effect of the Boatlift on fertility.

Column (1) of Table 7 (Panel A) shows the estimated average fertility impact of the Mariel Boatlift on women living in rented homes in Miami[27]. For brevity reasons, only the result where the selection of the comparison group is based on the synthetic control method and where the full set of controls (a) to (i) has been included is displayed[28]. As the entry indicates, fertility for this group of women fell by approximately 5.1% after the Mariel Boatlift, with the estimated impact being statistically significant at the 10% level.

Column (1) of Table 7 (Panel B) presents results from a specification allowing for fertility impacts to vary across the 7 post-treatment years (i.e. equation (3)). The estimated effects for all the post-treatment years are negative, with magnitudes ranging from a 1.6% decline in fertility in 1985 to a 12.5%

[25] These results are available upon request.

[26] An average of only 0.74% of Miami women belonged in category (c) during the years 1980-1988. Similarly, an average of only 0.82% of women in the U.S. belonged in this category in 1980-1988.

[27] Here, only 9 years of data (1980-1988) are considered. This is because information on residential tenure 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.

[28] Here, the synthetic control is constructed as a weighted combination of 7 metropolitan areas: New York, Los Angeles, Baltimore, Houston, Dallas, Indianapolis, and New Orleans. The precise contributions of the 7 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 should match (see Panel B of Table 3). Though not presented here for brevity, the results derived with the use of all other metropolitan areas forming the comparison group are extremely similar to those found here.

decrease in 1983. 3 of 7 of the year treatment effects – namely, 1983 (significant at the 1% level), 1984 (significant at the 1% level), and 1986 (significant at the 10% level) – are statistically significantly different from zero. Hence, this provides strong evidence that the Mariel Boatlift led to a longer-term decline in fertility outcomes among non-home owning women.

These results are in stark contrast to the fertility changes experienced by women living in owned homes. Column (2) of Table 7 (Panel A) shows the average fertility impact of the Mariel Boatlift for home-owning women in Miami. As with the case of non-owners, only the result where the selection of the comparison group is based on the synthetic control method and where the full set of controls (a) to (i) has been included is displayed[29]. The estimate indicates that the fertility for home-owning women in Miami increased by approximately 1.1%, on average, after the treatment. However, the estimated impact is not statistically different from zero. If anything, the point estimate suggests that the Mariel Boatlift may have had a modest positive impact on the childbearing outcomes of home-owning women in Miami (although its associated t-statistic would suggest that there is no evidence that there was any fertility impact on this group of women in the post-treatment period).

Column (2) of Table 7 (Panel B) displays estimates of the separate-year fertility effects for each of the post-treatment years 1982-1988. On their own, the point estimates suggest that the Mariel Boatlift may have had varying impacts on the fertility outcomes of home-owning women throughout the post-treatment years. The impacts were negative in 1983, 1985 and 1986 but positive in 1982, 1984, 1987 and 1988. This said, none of the estimated impacts were statistically significant at the conventional levels. There is therefore little evidence to suggest that the Mariel Boatlift affected the fertility outcomes of home-owning women in Miami in any considerable way.

We put the conclusions arising from the above analyses to a further test by re-specifying our difference-in-differences model to account for potentially confounding trends which may be driving the observed negative fertility effect for home-renting women relative to home-owning women. The new specification is:

$$FERT = \delta_0 + \delta_1 RENTER + \delta_2 MIAMI.RENTER + \delta_3 AFTER.MIAMI + \delta_4 AFTER.RENTER + \delta_5 AFTER.MIAMI.RENTER + \boldsymbol{Z}'\theta + u \qquad (5)$$

Where "$RENTER$" is a dummy variable coded 1 if the observation lives in a rented home and coded 0 otherwise. As before, "$MIAMI$" is a dummy variable coded 1 if the observation lives in Miami and 0

[29] Here, the synthetic control is formed by a weighted average of 3 metropolitan areas: New York, Philadelphia, 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 should match (see Panel C of Table 3). Note that the results derived with the use of all other metropolitan areas forming the comparison group are very similar to those presented here.

otherwise. Also, "$AFTER$" represents a dummy variable for the post-treatment period. It is equal to 1 if the observation is from the years 1982-1988 and 0 otherwise. "$MIAMI.RENTER$" is obtained by interacting the "$MIAMI$" and "$RENTER$" dummies. "$AFTER.RENTER$" is obtained by interacting the "$AFTER$" and "$RENTER$" dummies. "$AFTER.MIAMI.RENTER$" is a dummy variable obtained by interacting the three dummy variables: "$AFTER$", "$MIAMI$", and "$RENTER$". Observations from Miami in the post-treatment period that live in rented homes receive a 1 for this triple interaction term. All other observations receive a 0 for this variable. $\boldsymbol{Z}$ is the vector of covariates as defined before. Our coefficient of interest is now the one on the triple interaction term "$AFTER.MIAMI.RENTER$", δ_5. δ_5 measures the impact of the Mariel Boatlift on the fertility outcomes of Miami women living in rented homes after netting out changes in the fertility of home-renting women across MSAs (which are not due to the immigration shock) as well as changes in the fertility of all women living in Miami (whether renters or homeowners – which are possibly due to changes in county-level policies or economic conditions that affect the fertility of all women in Miami). Hence, the estimates resulting from this difference-in-difference-in-differences specification provides a more robust estimate of the fertility impact of the Mariel Boatlift on Miami women living in rented homes than the one provided by the earlier analysis.

Column (3) of Table 7 (Panel A) displays the fertility impact estimate for home-renting women using the triple-differences specification. The MSAs constituting the control group are chosen based on the synthetic control method where the reference population for analysing fertility outcomes are women age 15-44 (see Panel A of Table 2). The impact estimate – derived by running specification (5) with the full set of controls (i.e. covariates (a) to (i)) included in the regression – indicates that the fertility of home-renting women fell by approximately 6.7% as a result of the immigration shock. The estimated impact is statistically significant at the 10% level and is only slightly larger than the one derived earlier using only the sample of home-renters.

We also allow for a specification where the treatment effects for home-renters are able to vary across the post-treatment years. This is done by replacing the variables "$AFTER.MIAMI$" and "$AFTER.MIAMI.RENTER$" in equation (5) respectively with separate post-treatment Year–Miami interaction variables and separate post-treatment Year–Miami–Renter interaction variables. The estimated fertility impacts for each of the post-treatment years are given by the coefficients on the post treatment Year–Miami–Renter interaction terms.

The year-specific fertility impact estimates for home-renting women in Miami are displayed in column (3) of Table 7 (Panel B). Again, the impact point estimates across all post-treatment years are consistently negative and they suggest that the negative fertility effects of the Mariel Boatlift for home-renters were largest during the years 1983 (where the estimated fertility impact is -12.1%) and 1984 (where the estimated fertility impact is -13.9%). Both these year treatment effect estimates are statistically significant at least at the 5% level. Whilst the rest of the year treatment effect estimates are negative, they are not statistically different from zero.

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 amongst renters, no such effect is produced for homeowners. These observations are consistent with the postulates by Dettling and Kearney (2011), which were earlier discussed in Section 2.1.

All in all, the conclusions arising from the traditional difference-in-differences technique match quite well those that were found using the synthetic control approach. This reinforces the robustness of our earlier findings and conclusions.

6. CONCLUSION

This paper represents the first attempt to identify a causal relationship between immigration flows and native childbearing outcomes. The findings arising from this research will provide us with a deeper understanding of the fertility consequences brought about by immigration. It identifies possible channels through which individual childbearing decisions and outcomes may be influenced by an immigration influx. This will be 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. The research findings will also be 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 manpower augmenting strategies are indeed appropriate and the likely consequences for the childbearing outcomes of host country natives.

This study uses a natural experiment, exploiting the large, unexpected and localised immigration of Cuban nationals to the Miami area in the United States in 1980 in order to examine the fertility consequences for natives. The impact of the immigration shock is estimated by comparing the evolution of fertility outcomes for women living in Miami, after the shock, to those for women living in other areas of the United States unaffected by the Cuban immigration. This is done in 2 ways: Firstly, by applying the synthetic control estimator developed by Abadie et al. (2010) and assessing the significance of the impact estimates using placebo tests. Secondly, by applying the traditional difference-in-differences estimator and using inference techniques based on actual person-level data to assess the significance of the impact estimates. Both methods lead to the same conclusion: The immigration shock led to short-term declines in native childbearing in Miami during the years 1983 and 1986. The negative fertility impacts in both years were economically meaningful and statistically significant. An analysis of the childbearing intentions among Miami women before-and-after the immigration shock reveal the observed declines in fertility to be planned, with the proportion of women in Miami expecting to have additional children declining in the years just preceding the actual fertility reductions. These results suggest that immigration shocks have short-term implications for the fertility decisions and outcomes of natives.

In addition, fertility effects are found to vary by residential tenure: While the immigration shock had a considerable negative impact on the fertility of women living in rented homes, it had practically no effect on those 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 those living in rented homes.

REFERENCES

Abadie, Alberto and Gardeazabal, Javier. (2003) The Economic Costs of Conflict: A Case Study of the Basque Country. *American Economic Review*, 93 (1), 113-132.

Abadie, Alberto, Diamond Alexis, and Hainmueller, Jens. (2010) Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association*, 105 (490), 493-505.

Abadie, Alberto, Diamond Alexis, and Hainmueller, Jens. (2011) SYNTH: Stata Module to Implement Synthetic Control Methods for Comparative Case Studies. [Online] Available from: http://ideas.repec.org/c/boc/bocode/s457334.html.

Abadie, Alberto, Diamond Alexis, and Hainmueller, Jens. (2011) Synth: An R Package for Synthetic Control Methods in Comparative Case Studies. *Journal of Statistical Software*, 42 (13), 1-17.

Adsera, Alicia. (2004) Changing Fertility in Developed Countries. The Impact of Labor Market Institutions. *Journal of Population Economics*, 17 (1), 17-43.

Adsera, Alicia and Menendez, Alicia. (2011) Fertility Changes in Latin America in Periods of Economic Uncertainty. *Population Studies*, 65 (1), 37-56.

Bhaumik, Sumon K. and Nugent Jeffrey B. (2011) Real Options and Demographic Decisions: Evidence from East and West Germany. *Applied Economics*, 43(21), 2739-2749.

Bohn, Sarah, Lofstrom, Magnus, and Raphael, Steven. (2011). Did the 2007 Legal Arizona Workers Act Reduce the State's Unauthorized Immigrant Population?. IZA Discussion Paper No. 5682, December.

Card, David. (1990) The Impact of the Mariel Boatlift on the Miami Labor Market. *Industrial and Labor Relations Review*, 43(2), 245-257.

Cortes, Patricia. (2008) The Effect of Low-Skilled Immigration on U.S. Prices: Evidence from CPI Data. *Journal of Political Economy*, 116(3), 381-422.

Cortes, Patricia and Tessada, Jose. (2011) Low-Skilled Immigration and the Labor Supply of Highly Skilled Women. *American Economic Journal*, 3(3), 88-123.

Daniels, Joseph P. and Von der Ruhr, Marc. (2003) The Determinants of Immigration-Policy Preferences in Advanced Economies: A Cross Country Study. *Atlantic Economic Journal*, 31(2), 146-158.

Dettling, Lisa J. and Kearney, Melissa S. (2011) House Prices and Birth Rates: The Impact of the Real Estate Market on the Decision to have a Baby. NBER Working Paper 17485, October.

Dustmann, Christian and Preston, Ian. (2000) Racial and Economic Factors in Attitudes to Immigration. IZA Discussion Paper No. 190, August.

Furtado, Delia and Hock, Heinrich. (2010) Low Skilled Immigration and Work-Fertility Tradeoffs Among High Skilled US Natives. *American Economic Review*, 100(2), 224-228.

Gonzalez, Libertad and Ortega, Francesc. (2009) Immigration and Housing Booms: Evidence from Spain. IZA Discussion Paper No. 4333, July.

Hondroyiannis, George. (2010) Fertility Determinants and Economic Uncertainty: An Assessment Using European Panel Data. *Journal of Family and Economic Issues*, 31(1), 33-50.

Lach, Saul. (2007) Immigration and Prices. *Journal of Political Economy*, 115(4), 548-587.

Masud Piloto, Felix. (1996) *From Welcome Exiles to Illegal Immigrants: Cuban Migration to the U.S., 1959-1995*. United States, Rowman and Littlefield.

Mayda, Anna M. (2006) Who is Against Immigration? A Cross-Country Investigation of Individual Attitudes towards Immigrants. *Review of Economics and Statistics*, 88(3), 510-530.

Mulder, Clara H. (2006) Population and Housing: A Two-Sided Relationship. *Demographic Research*, 15(13), 401-412.

Namboodiri, Krishnan. (1996) *A Primer of Population Dynamics*. New York and London, Plenum Press.

O'Donoghue, Cathal and O'Shea, Eamon. (2006) Fertility Decline in Ireland. In: Gustafsson, S. and Kalwij, A. (eds.) *Education and Postponement of Maternity: Economic Analyses for Industrialized Countries*. Netherlands, Kluwer Academic Publishers, pp. 87-112.

Ottaviano, Gianmarco and Peri Giovanni. (2007) The Effects of Immigration on U.S. Wages and Rents: A General Equilibrium Approach. Centre for Research and Analysis of Migration Discussion Paper 13/07, September.

Ranjan, Priya. (1999) Fertility Behaviour under Income Uncertainty. *European Journal of Population*, 15(1), 25-43.

Saiz, Albert. (2003) Room in the Kitchen for the Melting Pot: Immigration and Rental Prices. *Review of Economics and Statistics*, 85(3), 502-521.

Saiz, Albert. (2007) Immigration and Housing Rents in American Cities. *Journal of Urban Economics*, 61(2), 345-371.

Scheve, Kenneth F. and Slaughter Matthew J. (2001) Labor Market Competition and Individual Preferences over Immigration Policy. *Review of Economics and Statistics*, 83(1), 133-145.

Schoen, Robert, Astone, Nan Marie, Kim, Young J., Nathanson, Constance A, and Fields, Jason M. (1999) Do Fertility Intentions Affect Fertility Behavior?. *Journal of Marriage and Family*, 61(3), 790-799.

Simon, Curtis J. and Tamura, Robert. (2009) Do Higher Rents Discourage Fertility? Evidence from U.S. Cities, 1940-2000. *Regional Science and Urban Economics*, 39(1), 33-42.

Sleebos, Joelle. (2003) Low Fertility Rates in OECD Countries: Facts and Policy Responses. OECD Labour Market and Social Policy Occasional Paper No. 15, October.

Sobotka, Tomas, Skirbekk, Vegard and Philipov, Dimiter. (2011) Economic Recession and Fertility in the Developed World. *Population and Development Review*, 37(2), 267-306.

United States Current Population Survey Marriage and Fertility June Supplement Series 1973-1988. (2011) [DVD] California, Unicon Research Corporation

United States Current Population Survey Annual Social and Economic March Supplement Series 1973-1988. (2011) [DVD] California, Unicon Research Corporation

Willis, Robert J. (1999) A Theory of Out-of-Wedlock Childbearing. *Journal of Political Economy*, 107(6), S33-64.

Yi, JunJian and Zhang, Junsen. (2010) The Effect of House Price on Fertility: Evidence from Hong Kong. *Economic Inquiry*, 48(3), 635-650.

Figure 1 (Panel A): Evolution of Fertility Rates for Women age 15-44 (1973-1988): Miami vs Synthetic Control Area

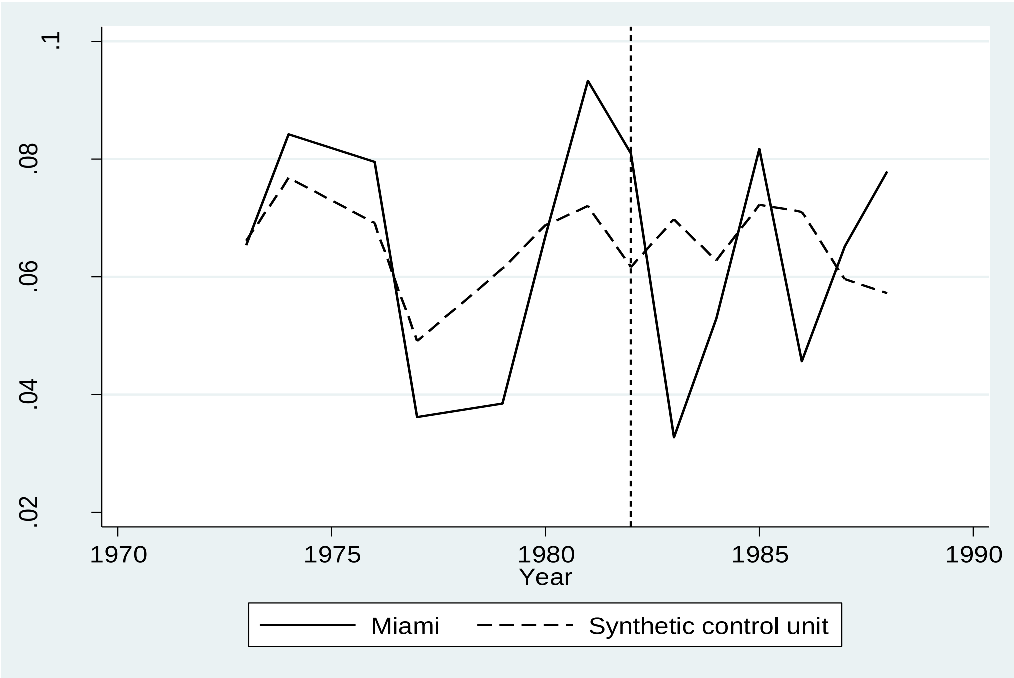

Figure 1 (Panel B): Placebo Test: Estimated Fertility Impacts for Miami and Placebo Fertility Impacts for 27 Comparison MSAs. Women age 15-44.

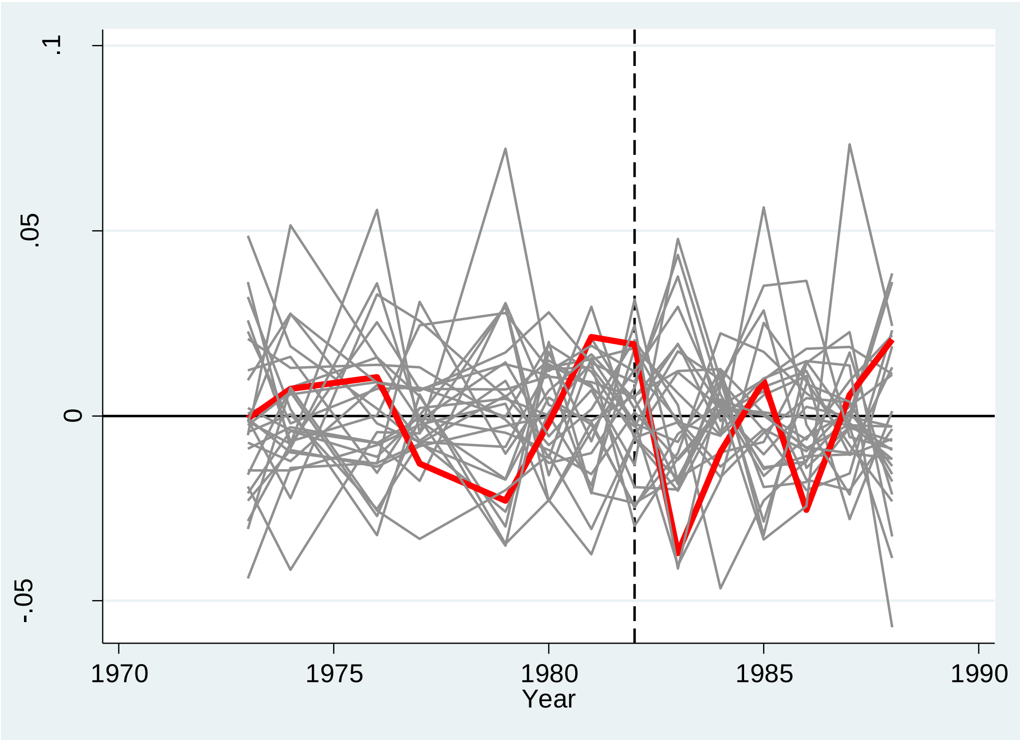

Table 1: Evolution of Fertility Rates in Miami (1973-1988)

Year	Females age 15-44		Married Females age 15-44		Females age 18-34		Married Females age 18-34	
1973	0.065	(0.019)	0.114	(0.033)	0.096	(0.029)	0.170	(0.049)
1974	0.084	(0.022)	0.139	(0.036)	0.108	(0.031)	0.175	(0.048)
1976	0.080	(0.024)	0.100	(0.036)	0.124	(0.036)	0.145	(0.051)
1977	0.036	(0.015)	0.053	(0.026)	0.059	(0.024)	0.085	(0.041)
1979	0.038	(0.016)	0.061	(0.030)	0.062	(0.025)	0.090	(0.044)
1980	0.067	(0.020)	0.105	(0.036)	0.100	(0.029)	0.138	(0.047)
1981	0.093	(0.024)	0.116	(0.039)	0.117	(0.032)	0.153	(0.054)
1982	0.081	(0.023)	0.134	(0.042)	0.107	(0.031)	0.173	(0.056)
1983	0.033	(0.015)	0.054	(0.027)	0.050	(0.022)	0.088	(0.043)
1984	0.053	(0.018)	0.107	(0.037)	0.081	(0.028)	0.161	(0.057)
1985	0.082	(0.020)	0.119	(0.036)	0.123	(0.030)	0.162	(0.050)
1986	0.046	(0.011)	0.062	(0.018)	0.071	(0.018)	0.093	(0.028)
1987	0.065	(0.014)	0.102	(0.024)	0.082	(0.020)	0.125	(0.034)
1988	0.078	(0.014)	0.107	(0.025)	0.111	(0.021)	0.169	(0.039)

Notes: Samples exclude all women of Cuban origin. Standard errors in parentheses. Sample weights used in all computations. Data were unavailable for years 1975 and 1978.

Table 2: Metropolitan Areas Contributing to Synthetic Controls

Panel A: MSA weights in synthetic Miami when Fertility is measured with respect to women age 15-44	
MSA	*Weight*
St. Louis	0.066
Cleveland	0.218
Houston	0.334
San Diego	0.004
Tampa St. Petersburg	0.237
Portland	0.140
Panel B: MSA weights in synthetic Miami when Fertility is measured with respect to women living in rented homes	
MSA	*Weight*
New York	0.354
Los Angeles	0.014
Baltimore	0.008
Houston	0.370
Dallas	0.065
Indianapolis	0.114
New Orleans	0.075
Panel C: MSA weights in synthetic Miami when Fertility is measured with respect to women living in owned homes	
MSA	*Weight*
New York	0.293
Philadelphia	0.407
Tampa St. Petersburg	0.300

Table 3: Characteristics Relevant to Predicting Fertility Rates

Variables	Panel A Women age 15-44 Miami	Panel A Women age 15-44 Synthetic	Panel B Women in Rented Homes Miami	Panel B Women in Rented Homes Synthetic	Panel C Women in Owned Homes Miami	Panel C Women in Owned Homes Synthetic
Married	0.488	0.536	0.362	0.444	0.543	0.507
In the Labour Force	0.613	0.604	0.587	0.605	0.620	0.615
Non-Hispanic White	0.492	0.774	0.307	0.504	0.483	0.768
Non-Hispanic African American	0.372	0.157	0.465	0.268	0.367	0.163
Other Race	0.016	0.012	0.012	0.022	0.042	0.021
Hispanic	0.121	0.057	0.217	0.206	0.108	0.048
Family income below US$5,000	0.165	0.104	0.258	0.153	0.037	0.028
Family income US$5,000-US$7,499	0.091	0.096	0.119	0.126	0.017	0.057
Family income US$7,500-US$9,999	0.120	0.098	0.167	0.092	0.047	0.047
Family income US$10,000-US$14,999	0.228	0.218	0.221	0.255	0.161	0.138
Family income US$15,000-US$24,999	0.261	0.270	0.188	0.261	0.369	0.330
Family income above US$25,000	0.136	0.212	0.048	0.114	0.369	0.400
Current Female Unemployment Rate	0.074	0.083	0.068	0.082	0.068	0.085
Current Male Unemployment Rate	0.058	0.054	0.049	0.061	0.049	0.069
Female Unemployment Rate 1 Year ago	0.078	0.085	0.084	0.084	0.084	0.087
Male Unemployment Rate 1 Year ago	0.053	0.053	0.043	0.061	0.043	0.064
Age 15-19	0.190	0.210	0.138	0.141	0.213	0.242
Age 20-24	0.207	0.208	0.238	0.263	0.182	0.161
Age 25-29	0.195	0.192	0.252	0.250	0.117	0.128
Age 30-34	0.165	0.158	0.173	0.174	0.212	0.172
Age 35-39	0.132	0.117	0.125	0.095	0.138	0.150
Age 40-44	0.111	0.113	0.074	0.077	0.138	0.148
8 or Less Years of Completed Education	0.075	0.073	0.067	0.137	0.060	0.040
9 Years of Completed Education	0.064	0.062	0.077	0.071	0.064	0.048
10 Years of Completed Education	0.086	0.080	0.093	0.091	0.066	0.088
11 Years of Completed Education	0.078	0.079	0.079	0.078	0.067	0.073
12 Years of Completed Education	0.385	0.407	0.388	0.371	0.428	0.417
13 Years of Completed Education	0.083	0.083	0.112	0.057	0.086	0.086
14 Years of Completed Education	0.085	0.065	0.070	0.057	0.093	0.081
15 Years of Completed Education	0.025	0.026	0.010	0.031	0.030	0.022
16 Years of Completed Education	0.088	0.090	0.074	0.077	0.069	0.094
17 Years of Completed Education	0.013	0.013	0.013	0.009	0.016	0.019
18 Years of Completed Education	0.018	0.020	0.018	0.020	0.021	0.032

Note: The values of the predictor variables are averaged over the entire pre-treatment period, 1973-1981. Apart from unemployment rates, all figures are proportions.

Table 4: Estimated Impact of Mariel Boatlift on Fertility of Miami Women (Synthetic Control Method)

1983 Treatment Effect (taking 1973-1981 to be the pre-treatment period)	Rank of 1983 Treatment Effect Estimate (Rarity of Magnitude)	P-value of 1983 Treatment Effect Estimate	1986 Treatment Effect (taking 1973-1981 to be the pre-treatment period)	Rank of 1986 Treatment Effect Estimate (Rarity of Magnitude)	P-value of 1986 Treatment Effect Estimate
-0.037	2 of 28	0.071	-0.025	1 of 28	0.036

Note: The p-value from a one-tailed test of the likelihood of observing a treatment effect estimate at least as negative as that for Miami is bounded from below by 0.036 (i.e. 1/28).

Table 5: Estimated Impact of Mariel Boatlift on Fertility of Women age 15-44 in Miami
(Synthetic Control Group as Comparison)

	(1)	(2)	(3)	(4)	(5)
Included Covariates	(a)-(b)	(a)-(f)	(a)-(g)	(a)-(h)	(a)-(i)
Separate-Year Treatment Effects					
1982	0.017	0.005	0.004	0.010	0.010
	(0.026)	(0.029)	(0.029)	(0.028)	(0.028)
1983	-0.034*	-0.046*	-0.048**	-0.050**	-0.051**
	(0.019)	(0.024)	(0.024)	(0.023)	(0.023)
1984	-0.006	-0.017	-0.018	-0.013	-0.012
	(0.021)	(0.029)	(0.029)	(0.028)	(0.028)
1985	0.015	0.009	0.008	0.013	0.014
	(0.023)	(0.025)	(0.024)	(0.024)	(0.024)
1986	-0.026	-0.030	-0.032*	-0.031*	-0.031*
	(0.017)	(0.019)	(0.019)	(0.019)	(0.019)
1987	0.010	-0.009	-0.009	-0.004	-0.005
	(0.018)	(0.022)	(0.022)	(0.022)	(0.022)
1988	0.021	0.011	0.009	0.015	0.016
	(0.018)	(0.021)	(0.021)	(0.020)	(0.020)
Observations	18,146	18,146	18,146	18,146	18,146
R-Squared	0.003	0.036	0.039	0.065	0.068

Notes: Sample consists of women age 15-44. The comparison group encompasses all observations from St. Louis, Cleveland, Houston, San Diego, Tampa St. Petersburg, and Portland. The control variables in column (1) include MSA and year dummies. The control variables in column (2) include MSA, year, age, ethnicity, and family income dummy variables as well as the number of previous births by the individual and MSA-level lagged male and female unemployment rates. The control variables in column (3) include all the control variables in column (2) plus the level of completed education of an individual. The control variables in column (4) include all the control variables in column (3) plus a dummy variable for marital status. The control variables in column (5) include all the control variables in column (4) plus an interaction term for ethnicity and education and an interaction term for ethnicity and marital status. Sample weights are used in all regressions. All specifications are estimated by OLS. Standard errors in parentheses are cluster-robust standard errors that allow for arbitrary correlation in individual error terms within MSA cells. ***denotes a coefficient significant at the 1% level, ** denotes a coefficient significant at the 5% level, * denotes a coefficient significant at the 10% level.

Table 6: Estimated Impact of Mariel Boatlift on Fertility of Women age 15-44 in Miami
(All other MSAs as Comparison)

	(1)	(2)	(3)	(4)	(5)
Included Covariates	(a)-(b)	(a)-(f)	(a)-(g)	(a)-(h)	(a)-(i)
Separate-Year Treatment Effects					
1982	0.013	0.000	0.000	0.004	0.004
	(0.024)	(0.027)	(0.027)	(0.026)	(0.026)
1983	-0.037**	-0.048**	-0.049*	-0.053**	-0.053**
	(0.017)	(0.019)	(0.019)	(0.019)	(0.019)
1984	-0.007	-0.013	-0.014	-0.011	-0.011
	(0.019)	(0.023)	(0.023)	(0.022)	(0.022)
1985	0.014	0.004	0.005	0.008	0.008
	(0.021)	(0.023)	(0.023)	(0.023)	(0.023)
1986	-0.025*	-0.027	-0.028*	-0.027*	-0.029*
	(0.014)	(0.017)	(0.017)	(0.016)	(0.016)
1987	-0.006	-0.011	-0.011	-0.007	-0.009
	(0.016)	(0.019)	(0.019)	(0.019)	(0.018)
1988	0.007	0.002	0.002	0.007	0.007
	(0.016)	(0.019)	(0.019)	(0.019)	(0.019)
Observations	120,807	120,807	120,807	120,807	120,807
R-Squared	0.001	0.032	0.033	0.067	0.068

Notes: Sample consists of women age 15-44. The comparison group encompasses observations from all identified metropolitan areas excluding Miami. The control variables in column (1) include MSA and year dummies. The control variables in column (2) include MSA, year, age, ethnicity, and family income dummy variables as well as the number of previous births by the individual and MSA-level lagged male and female unemployment rates. The control variables in column (3) include all the control variables in column (2) plus the level of completed education of an individual. The control variables in column (4) include all the control variables in column (3) plus a dummy variable for marital status. The control variables in column (5) include all the control variables in column (4) plus an interaction term for ethnicity and education and an interaction term for ethnicity and marital status. Sample weights are used in all regressions. All specifications are estimated by OLS. Standard errors in parentheses are cluster-robust standard errors that allow for arbitrary correlation in individual error terms within MSA cells. ***denotes a coefficient significant at the 1% level, ** denotes a coefficient significant at the 5% level, * denotes a coefficient significant at the 10% level.

Table 7: Estimated Impacts of Boatlift on Fertility of Women Living in Rented and in Owned Homes

Panel A

	(1)	(2)	(3)
	Renters	Homeowners	Renters (Triple-Differences)
Included Covariates	(a)-(i)	(a)-(i)	(a)-(i)
Average Treatment Effect	-0.051*	0.011	-0.067*
	(0.028)	(0.022)	(0.036)
Observations	19,125	12,562	15,345
R-Squared	0.071	0.084	0.071

Panel B

	Renters	Homeowners	Renters (Triple-Differences)
Included Covariates	(a)-(i)	(a)-(i)	(a)-(i)
Separate-Year Treatment Effects			
1982	-0.024	0.047	-0.069
	(0.045)	(0.038)	(0.060)
1983	-0.125***	-0.014	-0.121**
	(0.032)	(0.034)	(0.048)
1984	-0.105***	0.039	-0.139***
	(0.039)	(0.040)	(0.053)
1985	-0.016	-0.002	-0.003
	(0.042)	(0.029)	(0.053)
1986	-0.063*	-0.011	-0.071
	(0.033)	(0.027)	(0.045)
1987	-0.051	0.015	-0.062
	(0.035)	(0.030)	(0.047)
1988	-0.023	0.007	-0.041
	(0.035)	(0.029)	(0.047)
Observations	19,125	12,562	15,345
R-Squared	0.073	0.085	0.074

Notes: For column (1), sample consists of women age 15-44 living in rented homes. The comparison group encompasses all observations from New York, Los Angeles, Baltimore, Houston, Dallas, Indianapolis, and New Orleans. For column (2), sample consists of women age 15-44 living in owned homes. The comparison group encompasses all observations from New York, Philadelphia, and Tampa St. Petersburg. For column (3), sample consists of women age 15-44. The comparison group encompasses all observations from St. Louis, Cleveland, Houston, San Diego, Tampa St. Petersburg, and Portland. In all regressions, we include as control variables: MSA, year, age, ethnicity, marital status and family income dummy variables as well as the number of previous births by an individual, the MSA-level lagged male and female unemployment rates, the level of completed education of an individual, an interaction term for ethnicity and education and an interaction term for ethnicity and marital status. Sample weights are used in all regressions. All specifications are estimated by OLS. Standard errors in parentheses are cluster-robust standard errors that allow for arbitrary correlation in individual error terms within MSA cells. ***denotes a coefficient significant at the 1% level, ** denotes a coefficient significant at the 5% level, * denotes a coefficient significant at the 10% level.