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Refugee Immigration and Natives' Fertility

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Abstract

Research and policy debates on population aging typically assume that immigration matters only through immigrants' own fertility and its impact on total population size, ignoring effects on natives' fertility. We study this margin in the context of the large inflow of Syrian refugees into Türkiye after 2011. Using an instrumental-variables strategy, we provide evidence that greater local exposure to refugees increases native fertility. Drawing on complete birth histories from the Demographic and Health Surveys, we find that the response operates primarily through earlier entry into motherhood and is concentrated among younger women. The result holds across three independent datasets, including administrative birth registers, and is mirrored in women's stated ideal number of children. Examining potential mechanisms, we find the evidence most consistent with improved household economic security and find no support for channels operating through housing costs or the use of paid childcare.

JEL classification

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Keywords

forced migration, fertility, refugees, social interactions

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1. Introduction

Demographic change is transforming the economic and social foundations of modern societies (Burgalassi and Matsumoto 2024; UNFPA 2025). Population ageing and declining fertility are placing pressure on pension systems, reshaping labour markets, and weakening long-run growth in both advanced and emerging economies (Stanley 2024). Much of the policy debate has focused on these consequences and on relatively narrow responses, such as pronatalist transfers.[1] Immigration has also become central to this debate because immigrants tend to be younger and have higher fertility rates, potentially easing labour shortages and fiscal pressures.[2]

Immigration is not, however, a simple or permanent solution to demographic decline. The scale required to offset ageing in many countries would be politically and socially difficult to sustain (Espenshade 2001; United Nations Population Division 2001), while immigrant fertility tends to converge towards native fertility within a generation (Adsera and Ferrer 2015). As Dustmann et al. (2017) emphasises, immigration can therefore form only one part of a broader response. Moreover, existing research and demographic projections generally treat native fertility as fixed. This motivates our main question: does large-scale immigration affect both realized fertility and fertility preferences among natives?

Two recent studies have examined the impact of immigration on fertility rates, though not on fertility preferences, in early 20th-century settings. Daudin et al. (2019) show that internal migration in France between 1861 and 1911 contributed to fertility convergence across regions through the diffusion of economic and cultural norms. Similarly, Carlana and Tabellini (2025) find that immigration into U.S. cities between 1910 and 1930 increased male employment, which in turn accelerated marriage, fertility, and household formation.[3] Building on these studies, we provide evidence from a recent episode of forced migration, Syrian refugees into Türkiye following the 2011 civil war, to examine whether and how such inflows influence natives' fertility outcomes, including childbirth, birth parity, total number of children, and ideal number of children.

The empirical challenge, of course, lies in identification. Refugees do not settle randomly, and areas with high refugee concentrations may differ in unobserved ways that also affect fertility. To address this concern, we employ an instrumental variables (IV) approach following Del Carpio and Wagner (2015) and subsequent extensions (Aksoy and Tumen 2021; Aksu et al. 2022). The instrument exploits variation in distances from Syrian governorates to Turkish provinces, which (conditional on province characteristics and fixed effects) is plausibly orthogonal to unobserved

[1] Reviews find that cash transfers on their own raise births only modestly and temporarily, with much of the response reflecting the timing of childbearing rather than completed family size. Broader packages that help parents combine work and family life through childcare, parental leave, and flexible working arrangements are associated with somewhat larger and more persistent effects (EBRD 2025).

[2] See, for example, the IMF's *World Economic Outlook* (April 2018, p. 26), whose policy message, "more migrants needed to offset ageing population," was covered by *The Guardian* on 9 April 2018: https://www.theguardian.com/business/2018/apr/09/get-more-migrant-workers-to-offset-strain-of-ageing-population-warns-imf.

[3] In sociology, see Alba and Nee (2003) on how immigration may change natives' norms.

shocks to native fertility.

Using three distinct datasets, the Demographic and Health Surveys (DHS), administrative population registers, and the National Survey on Domestic Violence Against Women (NSDVW), together with multiple fertility outcomes, we document consistent evidence that forced migration increases both objective (births, total number of children) and subjective (ideal number of children) fertility measures among native Turkish women. Our main analysis draws on the DHS: its complete birth histories allow us to construct a woman-year panel spanning 2009–2018 and to include individual fixed effects, so that fertility responses are identified from within-person variation, while its detailed information on birth parity and its subjective fertility measures let us characterize the response along both dimensions. The administrative population registers provide aggregate-level validation using official birth records. The NSDVW offers a large sample, captures recent fertility behaviors, and contains detailed information on household economic structure; we report the corroborating evidence from it in the appendix.

Our estimates point to a modest but demographically meaningful fertility response. A 10-percentage-point increase in refugee exposure raises the total number of children by 0.101, equivalent to 5.8 percent of the sample mean of 1.752. The response operates mainly through entry into motherhood: the annual probability of a first birth rises by 1.6 percentage points, while second- and higher-order births remain essentially unchanged. The effect is quantitatively larger among younger women and peaks at ages 25–29. The estimated effects are comparable in magnitude to those of generous parental-leave policies and universal early-childcare programs (Lalive and Zweimüller 2009; Mörk et al. 2013; Olivetti and Petrongolo 2016).

We explore four potential channels. The strongest evidence points to household income and economic security. Refugee exposure increases husbands' formal, social-security-covered employment, without affecting whether they work, while native women's own employment falls. The NSDVW also shows an increase in household asset income. These patterns are consistent with the literature, which finds that refugee inflows displaced some native workers from informal jobs while shifting native men into formal jobs and higher-skill occupations (Del Carpio and Wagner 2015; Akgündüz and Torun 2020; Aksu et al. 2022).

A second, more tentative possibility is that social interaction affected fertility preferences. Refugee exposure increases native women's reported ideal number of children, while Syrians have higher fertility and report larger ideal family sizes than native Turks. These differences create scope for greater contact with refugees to influence native fertility preferences. The available survey evidence is consistent with this possibility, but it is not conclusive. Our design cannot distinguish norm diffusion from social comparison, changing expectations, perceived competition, or other responses to immigration (Bisin and Verdier 2000; Bernardi 2003; Fernández and Fogli 2009). By contrast, we find no evidence that the fertility response operates through changes in house prices or childcare costs.

The remainder of the paper is organized as follows. Section 2 provides background on

Syrian refugee migration to Türkiye. Section 3 describes the data sources, and Section 4 outlines the empirical strategy. Section 5 presents the main results from the DHS birth-history panel, Section 6 reports evidence from administrative birth registers, Section 7 examines mechanisms, Section 8 discusses magnitude and demographic interpretation, and Section 9 concludes.

2. Syrian Refugees: Background and Characteristics

A brutal civil war broke out in Syria in March 2011, killing thousands and displacing millions. By April, Syrians had begun seeking refuge in Türkiye and neighboring countries such as Jordan and Lebanon. In response, Türkiye adopted a generous open-door policy, granting temporary protection to all Syrians entering the country (Ferris and Kirişci 2016). Although initially referred to as "guests" rather than asylum seekers, a dedicated protection framework was soon established, allowing Syrians fleeing the conflict to enter Türkiye and guaranteeing they would not be forcibly returned. While permitted to remain indefinitely, they were not granted formal work rights.

Between the onset of the war and mid-2012, the Turkish government constructed more than 20 large refugee camps in several provinces near the Syrian border. Initially, relatively few Syrians entered, only around 8,000 by December 2011. However, as the conflict deepened, the number of Syrians fleeing rose sharply, particularly from northern border areas of Syria to southeastern provinces of Türkiye. By the end of 2012, the refugee population had surged to nearly half a million. As camp capacity diminished, many Syrians began settling in nearby towns and provinces along the Türkiye–Syria border. The temporary protection policy extended access to education, healthcare, and other services in the province where refugees registered, which limited internal mobility despite formal freedom of movement (especially in the early years).[4] Today, Türkiye hosts one of the world's largest refugee populations, comprising more than half of all Syrian refugees globally.[5]

Data on Syrians in Türkiye come from the Turkish Disaster and Emergency Management Authority (AFAD), which has released annual province-level data since 2011. These show that variation in Syrian settlement density along the border is closely linked to proximity to border gates and crossing points (AFAD 2013). Appendix Figure A.1 illustrates the location of the main Türkiye–Syria border crossing points, which served as the primary entry routes in the early years of the conflict. Consistent with this geography, Appendix Figure A.2 documents a highly uneven spatial distribution of Syrians across Turkish provinces from 2013 onward. In

[4] Previous studies have typically treated mass refugee migration as exogenous, asserting that the timing of migration and the characteristics of refugees are not shaped by local conditions in destination areas (Borjas and Monras 2017; Clemens and Hunt 2019; Aksoy et al. 2023).

[5] According to the Disaster and Emergency Management Authority (AFAD), about 90% of Syrians who entered Türkiye came from six governorates near the Turkish border in northern Syria: Aleppo (36%), Idlib (21%), Raqqa (11%), Latakia (9%), Hama (7.5%), and Hasakah (5.4%) (AFAD 2013).

particular, refugee-to-population ratios are persistently higher in provinces adjacent to major border gates, while other regions received substantially lower shares relative to their native populations. Large metropolitan provinces such as İstanbul, Ankara, İzmir, Antalya, Konya, and Mersin also attracted sizable absolute numbers of Syrians over time, but their refugee-to-population ratios remained considerably smaller than those in the southeastern border belt. Taken together, this variation is central to our empirical strategy, as refugee settlement patterns were shaped primarily by the location of entry gates and the distance from origin governorates, which we discuss in detail in Section 4.

To better understand the characteristics of Syrian migrants and the nature of the treatment, we use Gallup World Poll data and compare pre-war Syrians residing in the border governorates with Turkish natives living just across the border. Appendix Table A.1 shows important differences in family structure and fertility-related outcomes prior to the conflict. Syrians lived in much larger households, had more children, and reported substantially higher ideal family sizes. They also had lower educational attainment, although average household income levels were broadly similar across the two groups. Taken together, these patterns point to distinct fertility norms and demographic profiles on either side of the border before the war.

3. Data Sources

Demographic and Health Surveys (DHS). Our main analysis uses the DHS waves of 2003, 2008, 2013, and 2018. The 2013 and 2018 waves provide the retrospective birth histories from which we build the woman-year panel; the 2008, 2013, and 2018 waves are pooled as cross-sections for outcomes observed only at the interview date; and the 2003 and 2008 waves, both of which precede the inflow, are used for the pre-migration placebo checks in Appendix Figure A.3, described in Section 4.

The DHS is a nationally representative survey that covers a broad range of topics, including fertility, maternal and child health, family planning, and socioeconomic characteristics. We use its objective fertility measures for our main results and its subjective measures to examine shifts in fertility preferences associated with forced migration. From the DHS we construct four reported fertility outcomes, namely the woman's cumulative number of children, first births, second-or-higher-order births, and her stated ideal number of children; an indicator for whether the woman gave birth in the calendar year is used for the pre-inflow placebo of Section 5.4 rather than as a reported outcome; four outcomes used in the mechanism analysis, namely whether the woman held a job during the year, whether her husband worked in the previous week, whether his job provides social-security coverage, and the type of childcare used by working mothers; and indicators for being currently married and for being divorced or separated.

The complete retrospective birth histories collected in the 2013 and 2018 waves allow us to construct a woman-year pseudo-panel. Although the DHS is not a true panel survey, it records complete birth histories for each respondent, so we can reconstruct annual fertility outcomes for the years before each interview. The resulting dataset covers native women aged 15–49 over 2009–2018 and contains 109,747 woman-year observations across all 81 provinces. Appendix Table A.2 reports descriptive statistics for this panel.

This setup treats each year of a woman's birth history as a separate person-year observation, so each respondent appears multiple times in the data. It allows us to include respondent (woman) fixed effects, meaning we compare each woman to herself over time. By doing so, we remove all time-invariant individual characteristics (such as baseline fertility preferences, personality traits, or family upbringing) and can isolate how changes in local refugee exposure relate to changes in her own fertility behavior. The panel spans 2009–2018 and therefore captures medium- to longer-run fertility responses, complementing the short-run evidence from the NSDVW described below.

National Survey on Domestic Violence Against Women (NSDVW). We use the NSDVW as independent corroborating evidence. It is a nationally representative individual-level survey conducted in 2008 and 2014 by the Turkish Statistical Institute, covering women aged 15–59 using a weighted, stratified, multilayered cluster sample; the pooled analysis file covers 78 provinces, 51 of them in both waves. The NSDVW does not include Syrian refugee women in either wave, as it samples only Turkish citizens living in private households, and refugees were not part of this sampling frame in 2008 or 2014.

Unlike many other surveys by the Turkish Statistical Institute, the NSDVW is released at the province level, allowing for much more precise geographic information (rather than only observing respondents at the NUTS-2 sub-region level).[6] All interviews were conducted face-to-face. While the NSDVW is not a panel, the two waves (fielded before and after the onset of the Syrian war) form pooled cross-sections that allow for clear pre- and post-exposure comparisons.

The survey provides rich information on fertility and labor market outcomes, as well as detailed demographic characteristics of respondents and their partners, including ethnic background (mother tongue) and indicators of household wealth (e.g., house or vehicle ownership). For our analysis, we restrict the sample to ever-married women of childbearing age (15–49 years), yielding approximately 12,000 respondents, of whom 11,285 have complete covariates and enter the estimation samples.[7]

Our main NSDVW outcome captures *short-run* fertility responses; we also use the respondent's total number of children as a stock measure of cumulative fertility. *Gave birth last year*

[6] Türkiye follows the EU NUTS classification: NUTS-1 consists of 12 large regions, NUTS-2 of 26 subregions, and NUTS-3 of 81 provinces. We use NUTS-2 and NUTS-3 units to measure regional and province-level variation, depending on data availability.

[7] Appendix Table A.3 reports summary statistics. The average woman in the sample is 34 years old and has completed about seven years of schooling. 18% were employed in the previous week. Husbands have an average of 8.4 years of schooling, with 81% employed and 68% working formally. The average number of children is 2.16, while 8.8% of women gave birth in the last year and 6.7% were pregnant at the time of the survey.

equals 1 if a child aged 0, identified as the respondent's son or daughter, appears in the household roster, and 0 otherwise. Because the 2014 wave was conducted in April–May, this outcome captures births occurring between roughly April 2013 and the interview date in April or May 2014, implying conceptions between roughly July 2012 and August 2013.

Other Datasets. We further validate our results using province-level administrative birth records for Turkish natives from 2009 to 2018, which allow us to compute total and age-specific fertility rates by province and to examine fertility timing at the aggregate level.

In addition, we use two auxiliary surveys. The Gallup World Poll is fielded annually in more than 120 countries on roughly 1,000 adults each; we use the 2008 to 2011 waves on either side of the border, with Gallup sampling weights, to compare the fertility and family size of Syrians in the border governorates with those of Turkish natives living just across the border. The KONDA Barometer is a nationally representative face-to-face survey run monthly since 2010 by the polling company KONDA; we use the February 2016 round, whose theme module covers attitudes toward Syrian refugees, surveying 2,649 adults aged 18 and above in 27 provinces, to examine whether reported contact with Syrians is related to family outcomes. We also use a province panel of median residential sale prices per square meter published by the Central Bank of the Republic of Türkiye (TCMB/EVDS) for 2010–2018, and province population, unemployment, higher-education, and total foreign trade series from the Turkish Statistical Institute.

Finally, we use annual province-level data on registered Syrian refugees from the Disaster and Emergency Management Authority (AFAD), which cover both camp and non-camp residents and allow us to construct our instrumental variable.[8] We measure natives' exposure to the refugee inflow by the ratio of registered Syrian refugees to their province's native population.

4. Empirical Strategy

We exploit differences across provinces in refugee exposure and changes over time. Our main specification uses the DHS birth-history panel. For outcomes observed only at the survey date, and for the NSDVW analysis, we use the corresponding cross-sectional specification described

[8]Our exposure measure, based on registered Syrians, may undercount some refugees, especially in later years. Two factors mitigate this concern. First, registration under Temporary Protection was effectively mandatory during the initial inflow, yielding very high early coverage. Second, any later under-registration would mainly add measurement error that weakens the first stage and biases estimates toward zero, making our results conservative.

below.[9]

$$Y_{ipt} = \beta_1 \widetilde{R}_{pt} + \mu_i + \vartheta_t + \phi_{a(i,t)} + \sum_{s \neq 2009} \mathbb{1}\{t = s\} \left(\pi_s^U U_p^{2010} + \pi_s^H H_p^{2010} + \pi_s^T \ln T_p^{2010}\right) + \varepsilon_{ipt} \quad (1)$$

where Y_{ipt} denotes the fertility outcome for woman i in province p in calendar year t. We examine four outcomes: the woman's cumulative number of children, first births, second-or-higher-order births, and the ideal number of children. The birth-parity measures allow us to distinguish entry into motherhood from subsequent increases in family size, while ideal family size captures fertility preferences.

Our variable of interest, $\widetilde{R}_{pt}$, is the ratio of registered Syrian refugees to the province's native population, expressed in units of 10 percentage points, so that β_1 measures the effect of a 10-percentage-point increase in the refugee-to-population ratio. Coefficients are reported on this scale throughout, except where a table note states otherwise.

μ_i are woman fixed effects, which absorb the province and every time-invariant characteristic of the woman, so time-invariant individual controls such as schooling, rural residence, and mother tongue do not enter separately. ϑ_t are calendar-year fixed effects and $\phi_{a(i,t)}$ are single-year-of-age fixed effects. U_p^{2010}, H_p^{2010}, and $\ln T_p^{2010}$ are the province's pre-inflow (2010) unemployment rate, higher-education index, and log total trade volume, each interacted with calendar-year indicators (2009 is the omitted base year). These interactions allow provinces with different pre-inflow economic conditions to follow different paths over time; because the trade control is held at its pre-inflow value, it cannot itself respond to the refugee inflow. The panel is estimated on woman-year observations and identification comes from within-respondent variation over time. ε_{ipt} is the error term.

Outcomes reported once, at interview, cannot be studied with woman fixed effects. For these, and for the NSDVW, we estimate a pooled cross-sectional version of the design: the same treatment and instrument, with province and survey-round fixed effects, individual controls (age, age squared, years of schooling, rural residence, and mother tongue), and log trade volume, weighted by the survey sampling weights.[10]

[9] We use the t subscript throughout for notational consistency across datasets. The NSDVW consists of two repeated cross-sections (2008 and 2014). Because there were essentially no Syrians in Türkiye in 2008, treatment is zero in the pre-period, and the IV is identified entirely from cross-provincial differences in refugee stocks in 2014. This cross-sectional variation is substantial (see Appendix Figure A.2). The instrument combines pre-war Syrian population shares with fixed distances to Turkish provinces and therefore predicts this uneven provincial allocation. The 2008 wave supplies the pre-inflow baseline within the pooled estimation; all identifying variation in exposure comes from the 2014 wave.

[10] The pooled NSDVW estimating equation is $Y_{ipw} = \alpha + \beta \widetilde{R}_{pw} + X'_{ipw}\gamma + \theta \ln T_{pw} + \delta_p + \vartheta_w + \varepsilon_{ipw}$, where w indexes the 2008 and 2014 waves, X_{ipw} contains age, age squared, years of schooling, rural residence, and mother tongue, δ_p are province indicators, and ϑ_w is a wave indicator. The sample is ever-married women aged 15–49 (11,285 observations with complete covariates), estimates are weighted by the women's sampling weights, and coefficients are again reported per 10 percentage points. The pooled DHS cross-sections use survey-year fixed effects in place of ϑ_w and the DHS sampling weights.

In all individual-level and province-level models, we cluster standard errors at the province level (NUTS-3) to account for within-province correlation in errors. Results are robust to corrections for spatial correlation (Conley 1999) and to excluding influential provinces: dropping the highest-exposure province moves the cumulative-fertility estimate from 0.101 to 0.110, and dropping each of the 81 provinces in turn keeps it between 0.080 and 0.141 (Appendix Table A.4).

IV Strategy. A key concern with the OLS specification is potential endogeneity arising from non-random refugee settlement patterns. Refugees may have self-selected into provinces with better economic opportunities, stronger social networks, or greater availability of humanitarian assistance, factors that could also influence native fertility independently of refugee arrivals. To address this issue, we employ an instrumental variable for province-level Syrian refugee concentration, constructed from straight-line distances between Syrian governorate capitals and Turkish provincial capitals. This approach, introduced by Del Carpio and Wagner (2015), has been widely adopted in recent studies (e.g., Aksoy and Tumen 2021; Erten and Keskin 2021; Aksu et al. 2022; Akgündüz et al. 2023).

The distance-based instrument assumes that distance is a key determinant of refugee settlement and affects outcomes only through its impact on refugee concentration. This assumption is particularly relevant in our setting. As discussed in the background section and illustrated in Appendix Figures A.1 and A.2, early refugee entry was channeled through a limited number of border gates and crossing points, and the Turkish government rapidly established camps in nearby border provinces. Both the authorities and refugees initially viewed displacement as temporary; when camps quickly exceeded capacity, many Syrians moved to adjacent towns and cities along the border. Over time, the temporary protection regime encouraged a gradual diffusion of refugees beyond the border belt, while refugee-to-population ratios remained much higher in provinces closest to the main entry points.[11]

The instrument is calculated as follows:

$$IV_{pt} = \frac{1}{S}\sum_{s=1}^{S}\left(\frac{\Pi_s}{T_{sp}}\right)R_t, \tag{2}$$

where T_{sp} denotes the distance between Syrian governorate s and Turkish province p, Π_s is the share of the Syrian population residing in governorate s in 2010, the last full pre-war year, $S = 13$ is the number of origin governorates, and R_t is the cumulative number of registered Syrian refugees in Türkiye in year t.[12]

Intuitively, the instrument predicts how many refugees province p would host in year t if refugee arrivals were allocated according to (i) the pre-war distribution of Syrians across origin

[11]For instance, Aksu et al. (2022) show that the distance-based instrument strongly predicts the refugee-to-population ratio, even after controlling for numerous location-specific factors and fixed effects by 2015.

[12]Data from the Syrian Central Bureau of Statistics provide the share of the Syrian population across governorates in 2010.

governorates and (ii) geographic frictions that make closer Turkish provinces more likely to receive displaced populations. Provinces closer to large origin governorates therefore receive higher predicted stocks, while more distant provinces receive lower predicted stocks. The instrument is built from 1,053 origin–destination pairs (13 Syrian governorates × 81 Turkish provinces). We use refugee *stocks* rather than flows because the inclusion of year fixed effects absorbs all nationwide changes in refugee inflows, removing the time-series component of inflows and leaving identification to come solely from cross-province differences in accumulated refugee presence within each year.

Identification. Before turning to the results, we briefly discuss the key identification assumptions (relevance and excludability) underlying our IV strategy.

Relevance requires that the distance-based instrument be strongly correlated with the refugee-to-population ratio. As mentioned above, this condition is plausible in the Turkish context: the government initially established refugee camps along the border, and once these reached capacity, Syrians moved mainly to nearby provinces. Consequently, proximity to the Syrian border was a key determinant of early refugee settlement. Across all IV tables, the first-stage coefficients have the expected sign (shorter distances predict larger accumulated refugee stocks), and the corresponding F-statistics exceed the conventional threshold of 10 in all but one reported specification.[13]

Excludability requires that the distance-based instrument affects native fertility only through its impact on the refugee-to-population ratio, not through other factors. A key implication is that, before the refugee inflow, predicted exposure should be unrelated to underlying fertility dynamics. Appendix Figure A.3 provides direct visual placebo tests of this implication. It plots pre-migration changes in fertility and in fertility preferences against the province's average distance to the Syrian governorates, the distance measure that enters the instrument. Panel A relates the change in DHS births between the 2003 and 2008 waves to this distance, Panel B the 2009 to 2010 change in the province-level total fertility rate, and Panel C the 2003 to 2008 change in the stated ideal number of children. In all three panels the relationship is essentially flat, indicating no systematic pre-migration divergence in fertility trends, or in stated preferences, between provinces closer to Syria and those more distant.[14]

We complement these visual checks with formal pre-2011 placebo regressions (reported in Section 5.4). In these analyses, we restrict the DHS pseudo-panel to 2008–2010, assign each province its 2013 refugee-to-population ratio and its 2013 instrument value to the placebo year 2010, and re-estimate the model with woman and calendar-year fixed effects, controlling

[13] The exception is the year-fixed-effects-only house-price row of the housing-channel analysis in Section 7, where the F-statistic is 6.7; it is reported alongside its province-and-year-fixed-effects counterpart, whose F-statistic is 126.2, and the two give the same null result.

[14] Note that the choice of time intervals is dictated by data availability in the DHS and the population registers. For the pre-refugee period (i.e., before 2011), we use the 2003 and 2008 DHS waves. For the population registers, our data begin in 2009.

for age, age squared, and log provincial trade volume, both by OLS and instrumenting the placebo ratio with distance-based predicted exposure. As elsewhere, the woman fixed effects absorb rural residence, years of schooling, and mother tongue, which are recorded once per woman. Conceptually, a post-inflow exposure pattern is applied as a placebo treatment in years when actual refugee presence was negligible. The coefficients on this placebo treatment are consistently close to zero and statistically insignificant, ruling out differential pre-migration trends by predicted exposure. Taken together, evidence suggests that distance to the Syrian border is unlikely to capture underlying fertility trends. This pattern is consistent with the interpretation that the instrument influences native fertility mainly through its association with the refugee-to-population ratio.

A further concern is that provinces nearer the Syrian border may have had stronger pre-existing trade links with Syria. If so, conflict-induced changes in trade could confound the estimated effects. To assess this possibility, we regress exports and imports on the instrument. Columns 1 and 2 of Appendix Table A.5 show no statistically or economically meaningful relationship between distance-based predicted exposure and trade flows.

Another potential threat is that the Syrian conflict generated broader economic shocks that disproportionately affected border provinces. In that case, part of the estimated fertility response could simply reflect shifts in local economic conditions. Using nighttime light density as a proxy for economic activity, Column 3 of Appendix Table A.5 shows a coefficient effectively equal to zero, indicating no systematic association between the instrument and local economic conditions. Taken together, these null results support the validity of the exclusion restriction.

Finally, one might worry that provinces closer to Syria differ systematically in institutional characteristics that could influence fertility. This is unlikely. In the DHS birth-history panel, woman fixed effects absorb every time-invariant characteristic of the woman and of the province in which she is observed; in the pooled cross-sections, province fixed effects do the same; and in the province-level analysis, NUTS-1 region-by-year fixed effects additionally absorb any shock common to provinces in the same broad region and year.

5. Results

5.1 Total Number of Children and Parity Transitions

Table 1 reports our main results, estimated on the DHS birth-history pseudo-panel of native women aged 15–49 observed over 2009–2018 (109,747 woman-year observations across all 81 provinces). Panel A reports OLS estimates of Equation 1, and Panel B reports instrumental-variables estimates.

Column 1 shows that refugee exposure raises the total number of children. In our IV specification, a 10-percentage-point increase in the refugee-to-population ratio increases the total number of children by 0.101, statistically significant at the 1 percent level. The OLS estimate of 0.065 has the same sign. The first-stage F-statistic of 88 confirms a strong first-stage relationship.

To gauge the magnitude, the IV estimate corresponds to an increase of about 5.8 percent relative to the sample mean of 1.752 children per woman. Because an increase of that size is far outside the experience of most provinces, a more realistic benchmark is a one standard deviation increase in exposure, equal to 4.08 percentage points, which raises the total number of children by 0.041, about 2.3 percent of the mean. The response is therefore modest at the individual level but demographically meaningful.

Columns 2 and 3 locate the response in the fertility sequence. For each woman-year, we observe whether the woman is childless at the start of the year or already has at least one child, and we estimate annual parity transitions using these beginning-of-year risk sets: the first-birth outcome is defined on woman-years in which the woman is childless, and the second-or-higher-birth outcome on woman-years in which she already has a child. Among initially childless women, a 10-percentage-point increase in exposure raises the annual probability of a first birth by 0.016, a 1.6-percentage-point increase relative to a mean annual first-birth probability of 6.3 percent (Column 2). By contrast, the estimate for second-or-higher-order births is small, negative, and statistically insignificant (Column 3).

The DHS panel of complete birth histories points to entry into motherhood as the operative margin, and the administrative records in Section 6 show that exposure also lowers the mean age at first birth, consistent with childbearing being brought forward. We next examine which women drive the response.

5.2 Heterogeneity by Age

Table 2 reports the cumulative-fertility response by the woman's age, estimating the main specification separately within each age band. The response is concentrated at younger ages and peaks at ages 25–29, where a 10-percentage-point increase in exposure raises the total number of children by 0.159. The estimate is 0.046 at ages 15–19, positive but imprecise at ages 20–24 (0.082), and 0.071 at ages 30–34. It is essentially zero at the oldest ages: 0.003 at ages 40–44 and −0.002 at ages 45–49. Women at the upper end of the age range therefore contribute almost nothing to the estimated effect.

Grouping the sample into broad bands (Panel B), the estimate is 0.185 for women aged 20–29 and 0.064 for women aged 30–39. The difference of 0.121 is not statistically significant ($p = 0.100$), so we describe the response as concentrated at younger ages rather than as a sharp age gradient. Restricting the sample to the prime childbearing ages strengthens the cumulative-fertility estimate, which rises from 0.101 for ages 15–49 to 0.112 for ages 20–44, 0.132 for ages 20–40, and 0.168 for ages 20–34, each significant at the 1 percent level (Appendix Table A.6). The administrative age-specific birth rates, from an entirely separate data source, also peak in the same 25–29 band (Appendix Table A.7).

5.3 Heterogeneity by Education, Wealth and Employment

Table 3 splits the sample by the woman's own education, comparing women with a university degree to all others. The cumulative-fertility estimate is 0.096 for university-educated women and 0.093 for the rest, both significant at the 1 percent level, and a formal test does not reject equality ($p = 0.946$). Equality is likewise not rejected for first births ($p = 0.436$) or for second-or-higher births ($p = 0.121$). We therefore find no evidence that the fertility response differs by education.

The response also does not vary by baseline household wealth. Splitting women by the DHS wealth index, the estimate is 0.073 for women in the top two wealth quintiles and 0.086 for women in the bottom three, and equality is not rejected ($p = 0.688$; Appendix Table A.8). This is consistent with a household-income channel that operated broadly rather than only among wealthier households, a point we return to in Section 7.

Finally, the cumulative-fertility response is almost identical by prior employment status. We classify women according to whether they were employed in the previous calendar year rather than at the interview date, since a recent birth may itself affect current employment. The IV estimates are 0.077 for women employed in year $t - 1$ and 0.088 for women not employed (Appendix Table A.9). Second-or-higher births remain close to zero in both groups. For first births, the estimate is 0.015 among women not previously employed and 0.008 among those previously employed. The first-birth risk set is less than half as large for previously employed women, with 9,101 compared with 19,928 woman-years, so that estimate is correspondingly less precise. The equality tests at the foot of the table settle the comparison directly: they do not reject for any of the three outcomes, with $p = 0.789$ for the total number of children, $p = 0.823$ for first births and $p = 0.297$ for second-or-higher births. The cumulative-fertility response is therefore not confined to women who were outside employment before the birth. We consider couples' relative education separately in Section 7, where the outcomes of interest, such as husbands' employment and household resources, are measured at the couple level.

5.4 Robustness Checks for the Individual-Level Analyses

Evidence from the NSDVW. The pooled NSDVW cross-sections provide independent evidence from a different survey. A 10-percentage-point increase in refugee exposure raises the total number of children by 0.187 and the probability of having given birth in the past year by 0.032 (Appendix Table A.10). Despite the shorter time horizon and different sample, both estimates are positive and consistent with the DHS results.

Robustness Across Specifications. Appendix Figure A.4 shows how the three main DHS outcomes respond to a wide range of changes in the sample and specification. We first vary the estimation window, restricting the panel to 2012–2018 or 2011–2018. We then test whether the results are driven by particular parts of the country by excluding, in separate specifications, the southeastern provinces, Istanbul, Ankara, Izmir, Bursa, and the five largest provinces together. In a separate set of checks, we add interactions between calendar-year indicators and the province's pre-inflow fertility rate, employment rate, or female unemployment rate, and we re-estimate the model without the baseline trade control. Across these exercises, our results remain robust.

Placebo Tests. To check whether our results are driven by the timing of the treatment, we run a placebo test that assigns a placebo treatment year before the refugee inflow. On the DHS pseudo-panel of births for 2008–2010, assigning 2010 as the placebo year yields statistically insignificant estimates (Appendix Table A.11).

Selective Native Migration. One natural concern is that native women may have responded to the refugee inflow by moving across provinces. This could change the composition of women observed in more exposed provinces if those who moved differed systematically in their fertility. It could also create measurement error because the DHS records a woman's province at the time of the survey and applies it retrospectively to her birth history. We address both concerns in two ways.

First, Appendix Figure A.5 follows native cohorts across the 2008 and 2014 NSDVW waves. We define province cells by age band in 2008, alone and interacted with education, age the cohorts forward by six years, and compare their survey-weighted size with the corresponding cohorts observed in 2014. The figure reports 48 pre-specified versions of this exercise, varying the age bands, education groups, outcome transformation, weighting, and treatment of small cells. None of the estimates is statistically different from zero. A joint test also fails to reject. Stacking the three cell definitions within a single grid variant, five-year age bands, the log-ratio outcome, weighted by 2008 cell size, and no minimum-respondents rule, and testing that predicted exposure has no effect in any of the three gives $F(3, 50) = 0.29$, $p = 0.835$. This statistic tests those three coefficients only; it is not a joint test of all 48 plotted estimates, which are not jointly testable because they are overlapping re-estimations on the same data.

Second, we use the residential histories available in the 2013 DHS to restrict the fertility analysis to women who were already settled in their province before the refugee inflow. The cumulative-fertility estimate is 0.095 in the full 2013 sample, 0.084 after excluding women who moved in 2011 or later, and 0.087 after excluding those who moved in 2009 or later (Appendix Table A.12). The estimates are therefore very similar when recent movers are removed. Together, these results provide no evidence that selective native migration accounts for the fertility response.

6. Aggregate-Level Analyses

6.1 Province-Level Data and Fertility Measures

We next turn to province-level birth records from the Central Population Administration System (MERNIS), obtained from the Turkish Statistical Institute (TurkStat). Türkiye has 81 provinces, which we observe annually from 2009 to 2018.

These data complement the individual-level analysis in two ways. First, they cover the universe of registered births and therefore allow us to assess whether the DHS findings are also visible in official population records. Second, they report births by the mother's province of usual residence and five-year age group, allowing us to examine the age profile of the response.[15] Births are classified by the citizenship of the mother and of the father, so births from mixed Syrian–Turkish marriages are recorded separately.[16]

We construct province-year fertility measures using midyear female population estimates. For each five-year age group, 15–19, 20–24, 25–29, 30–34, 35–39, 40–44, and 45–49, we calculate the age-specific fertility rate by dividing the number of births by the corresponding female population in that province, age group, and year. We calculate the total fertility rate by summing these age-specific fertility rates and multiplying by five to account for the width of the age intervals. We also use administrative data on the mean age of women at first birth, available for 2014–2018, to examine changes in the timing of entry into motherhood. We combine these fertility measures with province-level economic and demographic data from TurkStat.[17]

6.2 Empirical Strategy

For the province-level analysis, we estimate:

$$Y_{pt} = \beta \mathit{RefugeePopulation}_{pt} + \delta_p + \lambda_{r(p)t} + \sum_k \sum_{\tau \neq 2010} \gamma_{k\tau} \left(X^k_{p,2010} \times \mathbb{1}\{t = \tau\} \right) + \varepsilon_{pt}, \tag{3}$$

where Y_{pt} is a fertility outcome in province p and year t, and $\mathit{RefugeePopulation}_{pt}$ is the ratio of refugees to the native population. Province fixed effects, δ_p, absorb all time-invariant differences across provinces, including geography and other fixed provincial characteristics. NUTS-1 region-by-year fixed effects, $\lambda_{r(p)t}$, absorb all shocks shared by provinces within the same broad region in a given year, including nationwide shocks and region-specific changes in economic conditions

[15] Parents are required to register a birth with the local population directorate within one month of delivery.

[16] TurkStat compiles birth statistics from the Central Population Administration System and records the citizenship of each parent. Births to two Syrian parents under temporary protection are registered in the foreigners' register and are outside these statistics. Registered marriages between Syrian nationals and Turkish citizens were rare in the early years of the inflow and grew only later in our sample period.

[17] Appendix Table A.13 reports summary statistics for the province-level data.

or policy. Separate calendar-year fixed effects are redundant because they are already contained within the NUTS-1 region-by-year fixed effects.

The coefficient β is therefore identified by differences in the evolution of refugee exposure across provinces within the same NUTS-1 region, after accounting for permanent differences across provinces and shocks shared by provinces in the same region and year.

We also interact each province's pre-inflow unemployment rate, higher-education share, and log trade volume with calendar-year indicators. These interactions allow provinces with different initial characteristics to follow different paths over time. The regressions are weighted by each province's fixed 2010 population, giving less weight to fertility rates from small provinces that are based on relatively few births.[18] Using pre-inflow population weights ensures that the weights cannot respond to subsequent refugee settlement, fertility changes, or internal migration. As in the individual-level analysis, we instrument the refugee-to-population ratio using distance-based predicted exposure and cluster standard errors at the province level.[19] Because the refugee-to-population ratio enters the province-level regressions as a raw proportion, the tables report the coefficient and standard error multiplied by 0.1, so that estimates are per 10-percentage-point increase, the same scale as elsewhere in the paper.

6.3 Results

Table 4 presents the province-level estimates. Column 1 reports the total fertility rate, Columns 2 and 3 report birth rates for women aged 20–29 and 30–39, and Column 4 reports the mean age at first birth. The top panel presents the OLS estimates, while the bottom panel presents the IV estimates.

In the IV specification, a 10-percentage-point increase in refugee exposure raises the total fertility rate by 0.073 (Column 1), significant at the 1 percent level. This is of the same order of magnitude as the DHS estimate, although the two outcomes are measured differently. The estimated birth-rate response is positive in both broad age groups: 3.07 additional births per 1,000 women aged 20–29 and 2.38 additional births per 1,000 women aged 30–39. The finer five-year age profile, reported in Appendix Table A.7, peaks at ages 25–29, which is also the peak of the DHS age profile in Table 2.

Column 4 provides direct evidence on the timing of childbearing. A 10-percentage-point increase in exposure reduces the mean age at first birth by 0.115 years, or approximately 1.4 months. Together with the increase in first-birth transitions in the DHS panel, this result indicates that at least part of the fertility response reflects earlier entry into motherhood.

[18] This weight is implied rather than taken from a published population series: we recover it from the province's 2010 births and its 2010 crude birth rate, the two quantities the administrative extract contains. It is a single number per province, held fixed across all years.

[19] Istanbul is the only province in its NUTS-1 region and is therefore fully absorbed by the NUTS-1 region-by-year fixed effects. The estimation sample consequently contains 80 provinces rather than 81.

The panel structure of the administrative data also allows us to trace the response over time. Figure 1 plots year-specific reduced-form coefficients for a one-standard-deviation increase in predicted exposure. These coefficients are not directly comparable in magnitude with the IV estimates in Table 4, which are reported per 10-percentage-point increase in realized exposure.

Because the administrative series begins in 2009 and 2010 is the reference year, the pre-inflow test is necessarily limited to the 2009 coefficient. This estimate is close to zero at -0.004 and statistically insignificant ($p = 0.53$). The coefficient for 2011, the first year of refugee arrivals, is also small and insignificant. The coefficient becomes positive in 2012, reaches 0.063 in 2014, and remains between 0.05 and 0.07 through 2018. The post-inflow coefficients are jointly significant ($F = 28.6$, $p < 0.001$). The results therefore show that the fertility response emerged after refugees began settling more widely and remained visible through the end of the sample.

As a further check, Appendix Figure A.6 reports a complementary event study based on a high-versus-low-exposure comparison and the Callaway and Sant'Anna (2021) estimator. Because this analysis uses a binary measure of exposure, its coefficients are on a different scale from those in Figure 1. Nevertheless, it shows the same broad pattern: the estimated fertility gap is not statistically distinguishable from zero before 2012, turns significantly positive in 2012, and remains positive, and significant in most years, through 2018.

Overall, the aggregate analysis supports the individual-level findings using complete administrative birth records. Refugee exposure raises native fertility, the age profile peaks at ages 25–29, and the decline in the mean age at first birth is consistent with an earlier transition into motherhood.

7. Mechanisms

7.1 Labor Markets and Household Economic Security

We first consider whether changes in labor-market conditions can help explain the fertility response. Refugee inflows could reduce fertility if they displace native workers, lower earnings, or increase employment uncertainty. Previous research generally finds that job loss and weaker labor-market prospects delay or reduce childbearing, although the effects of unemployment may depend on the relative strength of income and substitution effects (Hotz et al. 1997; Kravdal 2002; Adserà 2004; Lindo 2010; Del Bono et al. 2012; Currie and Schwandt 2014; Huttunen and Kellokumpu 2016). By contrast, more stable employment and greater household resources may make it easier for couples to marry and have children.

The Turkish setting contains both possibilities. Syrian refugees entered mainly informal and lower-skilled employment. Earlier studies find that this displaced some native workers from informal jobs, particularly less-skilled workers, but also shifted some native men toward formal employment and higher-skill occupations (Del Carpio and Wagner 2015; Tumen 2016; Ceritoglu et al. 2017; Akgündüz and Torun 2020; Aksu et al. 2022). The implications for fertility are therefore ambiguous. They depend on whether the refugee inflow reduced household employment and income or instead improved the quality and security of native workers' jobs.

We assess three margins in turn. First, we ask whether native women's employment declined, and if so for whom. Second, we study whether husbands became more or less likely to work and whether their jobs became more formal. Third, we use the NSDVW to assess whether income from household assets increased. These outcomes are drawn from different samples because no single dataset contains all three measures in a comparable form.[20]

Table 5 reports the effect of refugee exposure on whether a woman held a job at any point during the year, against a mean of 0.306. Column 1 uses the full sample and gives an IV estimate of -0.036, with a standard error of 0.010; the point estimate is about 12 percent of the mean. Columns 2 and 3 divide women by university education: the estimate is -0.012 for university graduates and -0.038 for women without a university degree. Columns 4 and 5 use a broader education threshold: the estimate is -0.031 for women with at least a high-school education and -0.037 for those with less than a high-school education. The point estimates are larger in absolute value for the less educated, but the equality tests do not reject that the effect is the same across education groups, with $p = 0.135$ for the university contrast and $p = 0.741$ for the high-school contrast. Neither test could detect a difference smaller than 0.049, which is larger than the pooled estimate itself, so they rule out a gradient much bigger than the effect but cannot establish a smaller one. We therefore read the decline as broad-based, with point estimates largest where native women compete most directly with refugee labor in informal and low-skilled work, a direction consistent with the evidence that the inflow displaced less-skilled natives from informal jobs (Del Carpio and Wagner 2015; Tumen 2016; Ceritoglu et al. 2017). Because the measure counts any job held during the year, including seasonal and daily work and unpaid work in a family business, the fall in women's own employment lowers the opportunity cost of time spent on childbearing and therefore works in the same direction as the fertility response, rather than against it.

Since fertility decisions depend on household resources, we next ask whether refugee exposure improved husbands' employment conditions. This is particularly relevant in the Turkish setting, where previous studies find that some native men moved from informal into formal jobs following the refugee inflow. Table 6 therefore reports husbands' labor-market outcomes for currently married women, the group for whom these outcomes are observed. Panel A reports two separate margins. The first row asks whether refugee exposure changes the probability that the husband worked in the previous week. The estimate is -0.031, with a standard error of 0.034,

[20] Women's annual employment is reconstructed from the DHS retrospective job histories and is analyzed in the woman-year panel used for the main fertility estimates. Husbands' employment outcomes are observed only at the survey date and are therefore analyzed using the pooled DHS cross-sections. The household asset-income measure is available only in the NSDVW. The latter two analyses should therefore be viewed as complementary evidence rather than extensions of the DHS birth-history panel.

indicating no statistically significant change in husbands' overall employment. The second row asks whether the husband's current job provides social-security coverage. Here, a 10-percentage-point increase in refugee exposure raises the probability of coverage by 0.033, significant at the 10 percent level. In Türkiye, social-security coverage is a standard indicator of formal, contract-based employment. The results therefore suggest that the main adjustment occurs in the type of job husbands hold, rather than in whether they work at all.

Panel B studies whether this increase in job formality differs across households. We classify couples using the education of both spouses because the outcome is the husband's job and the potential mechanism operates through household resources. Hypergamous couples are those in which the husband has more completed years of schooling than the wife; high-skill homogamous couples are those in which both spouses have at least eight completed years of schooling, the compulsory basic level; and low-skill homogamous couples are those in which neither spouse does. Because hypergamy is defined by the within-couple gap, a hypergamous couple can also fall in either homogamous group, so the three groups overlap. The estimated increase in formal employment is positive for all three couple types, at 0.044 for hypergamous couples, 0.036 for high-skill homogamous couples, and 0.024 for low-skill homogamous couples. Only the hypergamous estimate is individually significant, at the 5 percent level, but the equality tests do not reject equality across the three, either pairwise or jointly. We therefore read the result as a broad increase in husbands' job formality rather than one specific to a particular couple type.

Overall, formal employment provides social-security coverage and greater job stability, offering a plausible way for refugee exposure to improve household economic security without increasing husbands' overall employment.

Appendix Table A.14 provides complementary evidence on asset-based income using the pooled 2008 and 2014 NSDVW waves. The outcome is an index constructed from six standardized indicators of whether the woman receives income from assets she owns: land, a house, a company, a vehicle, a bank account, or other assets. It therefore measures access to income-generating assets, rather than the value of those assets or total household income.

A 10-percentage-point increase in refugee exposure raises the index by 0.147 points in the pooled sample, about 0.27 of the index's standard deviation. The corresponding estimates are 0.186 for hypergamous couples and 0.217 for high-skill homogamous couples, significant at the 1 and 5 percent levels, respectively. The estimate for low-skill homogamous couples is positive, at 0.095, but statistically insignificant. These results provide separate evidence of an improvement in household economic resources.

Taken together, the results show a decline in native women's own employment and no change in husbands' overall probability of working. Instead, husbands become more likely to hold formal jobs, while the NSDVW shows an increase in income from household assets. This distinction is particularly important in Türkiye, where informal employment remains widespread and many workers lack the social insurance and employment protections associated with registered work.[21] The evidence therefore points to improved job quality and greater household economic security. More stable, formally registered work may reduce the economic uncertainty surrounding marriage and entry into motherhood. We interpret these findings as evidence consistent with a household-income channel.

7.2 Fertility Preferences and Social Interaction

We next consider whether social interaction may have contributed to the fertility response. Demographic research shows that family-size preferences can be shaped by relatives, friends, co-workers, and other social contacts (U.S. National Research Council 2001; Bernardi 2003). Exposure to the fertility behavior of others may affect both desired family size and the timing of childbearing (Durlauf and Walker 2001; Balbo and Barban 2014; Daudin et al. 2019). Related work suggests that perceived cultural or demographic competition may also influence fertility choices (Parsons 2000; Morland 2016). More broadly, Blanc and Wacziarg (2025) show that migration can transmit demographic behavior across populations rather than affecting aggregate fertility only through population composition.

Our data do not allow us to identify any of these processes directly. Social contact is not randomly assigned, and changes in fertility preferences may reflect social comparison, changing expectations, perceived competition, economic conditions, or other responses to immigration. We therefore present the following evidence as suggestive rather than as a test of a specific mechanism.

The clearest result comes from the DHS measure of fertility preferences. Table 7 applies the same instrument, in a pooled cross-sectional version of the design, to women's stated ideal number of children. A 10-percentage-point increase in refugee exposure raises the ideal number of children by 0.176, with a standard error of 0.059, relative to a sample mean of 2.749. Because the ideal number of children is reported once, at interview, this specification uses province and survey-year fixed effects with individual controls rather than woman fixed effects. The response is therefore visible in stated preferences as well as in realized fertility.

Appendix Table A.15 provides context for this result. The 2018 DHS surveyed Syrian and Turkish women using the same questionnaire. Conditional on age, education, and region, Syrian women report an ideal family size about 0.8 children larger and have approximately one more child than Turkish women. These comparisons are descriptive, but they show that the two groups differed substantially in both desired and realized fertility.

We also use the February 2016 round of the KONDA Barometer to study whether reported contact with Syrians is related to family outcomes among natives. Appendix Table A.16 shows that respondents who report more frequent contact tend to live in larger households. Appendix

[21] See the World Bank's *Türkiye Country Economic Memorandum: Jobs for Prosperity*, which identifies high informality as a central challenge in Türkiye's labor market.

Table A.17 reports summary statistics for this sample. This association is measured within a single cross-section and should be interpreted cautiously, as household size is only an indirect measure of fertility, and contact with Syrians may be related to location, occupation, family structure, or other unobserved characteristics.

Overall, the evidence is consistent with fertility preferences responding alongside realized fertility, but it does not establish a direct social-interaction or norm transmission channel. We therefore treat social interaction as a possible complementary explanation rather than an identified mechanism.

7.3 Housing Costs

Refugee inflows may also affect fertility through local housing markets. Rising house prices can increase the wealth of homeowners, while making housing less affordable for renters and prospective buyers (Lovenheim and Mumford 2013; Dettling and Kearney 2014). In the United States, part of the homeowner response may operate through mortgage refinancing or borrowing against accumulated housing equity. This channel is likely to be weaker in Türkiye. The mortgage market is relatively small, and average mortgage maturities are shorter than in most OECD countries, limiting households' ability to convert increases in property values into immediately available resources.[22] Homeownership may therefore provide less of a liquid wealth channel than in the United States. At the same time, higher rents could reduce disposable income among non-homeowners. We assess these possibilities through the three linked tests reported in Table 8.

Panel A first asks whether refugee exposure affected provincial house prices. The outcome is the log median sale price per square meter. Across three reduced-form and two IV specifications, all five point estimates are negative and statistically insignificant. We therefore find no evidence that refugee exposure increased provincial sale prices. These data measure raw median transaction prices rather than quality-adjusted prices and do not include rents. They may therefore miss changes in the composition of houses sold or effects concentrated in the rental market.

Panel B asks whether controlling for sale prices changes the estimated fertility response. On the housing-price sample, the estimated effect on the total fertility rate is 0.305, with a standard error of 0.265, when house prices are omitted. The estimate is 0.320, with a standard error of 0.289, when house prices are included. Both estimates are imprecise and are reported per unit of the refugee-to-population ratio, so their magnitudes are not directly comparable with the main DHS coefficients. The relevant comparison is between the two rows: adding house prices leaves the fertility estimate virtually unchanged.

[22] The OECD's review of mortgage finance identifies Türkiye as one of the few OECD countries where average mortgage maturities are below 20 years. The IMF's 2025 Article IV report also describes Türkiye's mortgage market as particularly small relative to comparable countries.

Panel C provides a separate test using the DHS. If housing wealth were an important channel, rising property values should benefit homeowners but not non-homeowners. Conversely, higher housing costs should place greater pressure on non-homeowners. We therefore estimate the cumulative-fertility response separately for the two groups. The estimates are almost identical: 0.101 for homeowners and 0.097 for non-homeowners. A formal test strongly fails to reject equality ($p = 0.960$). The same is true for first births ($p = 0.589$) and second-or-higher births ($p = 0.730$). Homeownership is recorded only in the 2013 DHS wave, giving a sample of 45,339 woman-years. The minimum detectable difference is 0.241, approximately 13 percent of the outcome mean, so the test can rule out large differences by ownership status but not smaller ones.

Taken together, these results provide no evidence that changes in house prices or housing wealth account for the fertility response.

7.4 Childcare Costs

A final possibility is that refugee inflows reduced the cost of childcare. Furtado (2016) shows that low-skilled immigration in the United States increased the supply of childcare workers, lowered the cost of market care, and raised fertility among highly educated native women. The key prediction is that, where immigrants expand the supply of childcare, native mothers should become more likely to use paid care.

This channel is less likely to be at play in Türkiye. Childcare is still provided mainly by relatives, female labor-force participation is low, and the market for paid care is small and largely informal (İlkkaracan 2012). Syrian women's employment rates were also very low during our study period, and limited Turkish-language skills would have made it difficult for many to enter childcare work, where communication and trust are essential. The inflow was therefore unlikely to generate a large increase in the supply of paid childcare.

The DHS does not report childcare prices, so we cannot test this channel directly. Instead, we use a question asked of working mothers about who looks after their youngest child while they work. We distinguish any market-based childcare from care provided by the mother, husband, another child, or another relative. Market care is then divided into babysitters and kindergartens. This provides an indirect test: if refugee exposure substantially reduced the cost of paid childcare, its use should increase in more exposed provinces.[23]

Table 9 reports the results. Column 1 uses an indicator for any market-based childcare, Column 2 considers babysitters separately, and Column 3 considers kindergartens. Panel A reports OLS estimates, while Panel B instruments refugee exposure using distance-based predicted exposure.

[23] The analysis is limited to women who worked after the birth of their youngest child and therefore speaks only to childcare arrangements among working mothers. It does not capture childcare use among non-working mothers or measure the price or quality of care directly.

Across both panels, none of the estimates is statistically significant, and five of the six point estimates are negative, so they do not suggest greater use of paid childcare in more exposed provinces. Overall, the results provide no support for the type of childcare-supply channel documented in the United States, which would require paid childcare use to increase with refugee exposure.

7.5 Marriage and Divorce

Appendix Table A.18 reports the effects of refugee exposure on marriage and divorce using the DHS and NSDVW. These outcomes are closely linked to fertility in Türkiye, where births outside marriage are rare. The two surveys produce similar estimates. A 10-percentage-point increase in exposure raises the probability of being currently married by 0.0200 in the DHS and 0.0244 in the NSDVW. It also lowers the probability of being divorced or separated by 0.0128 in the DHS and 0.0140 in the NSDVW, although the latter estimate is less precise.

These results indicate that stronger marital attachment accompanied the fertility response. One possible interpretation is that improved household economic security, reflected in husbands' formal employment and higher asset income, made marriage and family formation easier. This interpretation is also consistent with the finding that the fertility response operates mainly through entry into motherhood. We nevertheless treat the marriage and divorce results as accompanying patterns rather than evidence of a separate causal mechanism.[24]

8. Magnitude and Demographic Interpretation

The results show that refugee exposure raises native fertility, but their demographic interpretation depends on three issues: the scale of exposure, the distinction between cumulative and annual fertility measures, and whether the response reflects births brought forward or additional completed births. This section brings these points together, with Figure 2 summarising the implied magnitudes and the range of longer-run effects consistent with the data.

In the DHS panel, a 10-percentage-point increase in refugee exposure raises the total number of children by 0.101, with a 95 percent confidence interval of 0.039 to 0.163, relative to a sample mean of 1.752. The administrative records provide independent evidence of a similar order of magnitude: the total fertility rate rises by 0.073 per 10-percentage-point increase in exposure (Table 4). As discussed in Section 6.3, the outcomes are not directly comparable. The

[24] Changes in the pool of potential spouses are unlikely to explain these results. Intermarriage between Syrians and Turks remained uncommon, so the increase in native marriage and fertility is unlikely to reflect cross-group unions. In addition, about half of the Syrian refugee population was aged 18 or under in mid-2013 (AFAD 2013), which limits the immediate effect of the inflow on the marriage-age population. We therefore find little indication that changes in the local marriage market account for the fertility response.

DHS measures the cumulative number of children observed per woman, whereas the administrative outcome is a synthetic annual fertility rate constructed from age-specific birth rates.

A 10-percentage-point increase means ten refugees per 100 natives, a level reached only in the most exposed parts of Türkiye. In 2018, the median province had 0.67 refugees per 100 natives, compared with 2.86 at the 75th percentile and 7.63 at the 90th percentile. Thirty-three of the 81 provinces had more than one refugee per 100 natives. A one-standard-deviation increase in the woman-year distribution is 4.08 refugees per 100 natives and was exceeded by only 13 provinces in 2018. It therefore represents a substantially exposed province rather than the typical province.[25]

Panel A of Figure 2 translates the DHS coefficient into additional children per 100 women at several observed exposure levels. At one standard deviation, the estimate corresponds to 4.1 additional children per 100 women. At the mean exposure, it corresponds to 8.4 additional children per 100 women. At the median, the implied response is 0.7 additional children per 100 women.

The first-birth estimate can be scaled in the same way, although it measures an annual flow rather than a cumulative stock. A 10-percentage-point increase in exposure raises the annual first-birth probability by 0.016. At one standard deviation, this corresponds to approximately 650 additional first births per 100,000 women who had not previously given birth.[26] The age-specific estimates provide another way to assess the size of the response. In the administrative records, one standard deviation of exposure adds approximately 1.3 births per 1,000 women aged 20–29 and 1.0 birth per 1,000 women aged 30–39. These represent about 1.0 and 1.3 percent of the respective baseline birth rates.

Family-policy reforms provide useful, though imperfect, benchmarks. Lalive and Zweimüller (2009) find that extending job-protected parental leave in Austria from one to two years generated a short-run increase of about 12 births per 100 eligible mothers when the current- and future-child effects are combined. Over ten years, the current-child component declined to approximately three additional births per 100 mothers but remained statistically significant. Mörk et al. (2013) find that anticipation of a large reduction in childcare costs in Sweden increased first and higher-order births, although the increase in second births largely reflected a change in timing. These studies could follow the relevant cohorts beyond the initial response, which allowed them to assess whether the effect later faded. Our data cover at most six years after exposure begins, and the response peaks at ages 25–29, when much of childbearing still lies ahead.

Our data cannot determine whether these earlier births eventually translate into higher

[25] The 4.08 standard deviation is calculated over the woman-year distribution used in the DHS analysis. The unweighted standard deviation across the 81 provinces in 2018 is 10.56 because that calculation gives small provinces such as Kilis the same weight as large provinces such as Istanbul.

[26] This calculation uses the unrounded coefficients. Multiplying 0.015864 by 0.408 gives 0.006478, or 648 additional first births per 100,000 women at risk.

completed fertility. A birth brought forward raises cumulative fertility in every subsequent year even if completed family size remains unchanged. Panel B of Figure 2 makes this uncertainty explicit. At one standard deviation of exposure, the completed-fertility effect could range from zero, if the entire response reflects timing, to 4.1 additional births per 100 women, if the full response represents additional completed births. We cannot identify where within this range the eventual effect lies.

Our estimates therefore describe medium-run cumulative fertility rather than completed family size. This interpretation complements Adsera and Ferrer (2015), who show that immigrant fertility can converge towards native fertility within a generation. We document adjustment in the opposite direction and over a shorter horizon: native fertility responds to the presence of a higher-fertility immigrant population.

9. Conclusion

This paper studies whether large refugee inflows affect the fertility of the native population, using the arrival of Syrian refugees in Türkiye after 2011. We combine complete birth histories from the DHS with administrative birth records and the NSDVW. Across all three sources, greater local refugee exposure is associated with higher native fertility. The DHS results show that the response operates mainly through entry into motherhood rather than second-or-higher births.

The administrative records support both the magnitude and timing of the individual-level results. Fertility begins to rise in 2012, as refugees disperse beyond the border camps, and remains higher through 2018. Exposure also lowers the mean age at first birth. Taken together, these results point to a persistent medium-run response with an important timing component. They do not establish whether women ultimately have more children over their lifetimes or have the same number earlier.

We also study several possible channels. The clearest evidence points to household economic security. Refugee exposure increases husbands' formal, social-security-covered employment without changing their overall probability of working, while native women's own employment falls. The NSDVW also shows an increase in income from household assets. Fertility preferences move in the same direction as realized fertility and women in more exposed provinces report a higher ideal number of children. This is consistent with social interaction playing some role, but the available data cannot distinguish norm diffusion from social comparison, changing expectations, perceived competition, or other responses to immigration. We find no evidence that changes in house prices or the use of paid childcare explain the fertility response.

The broader implication is that immigration can shape fertility in receiving regions not only by adding a younger, higher-fertility population, but also by changing the behavior of natives. Demographic projections that hold native fertility fixed may therefore miss an important part of the adjustment.

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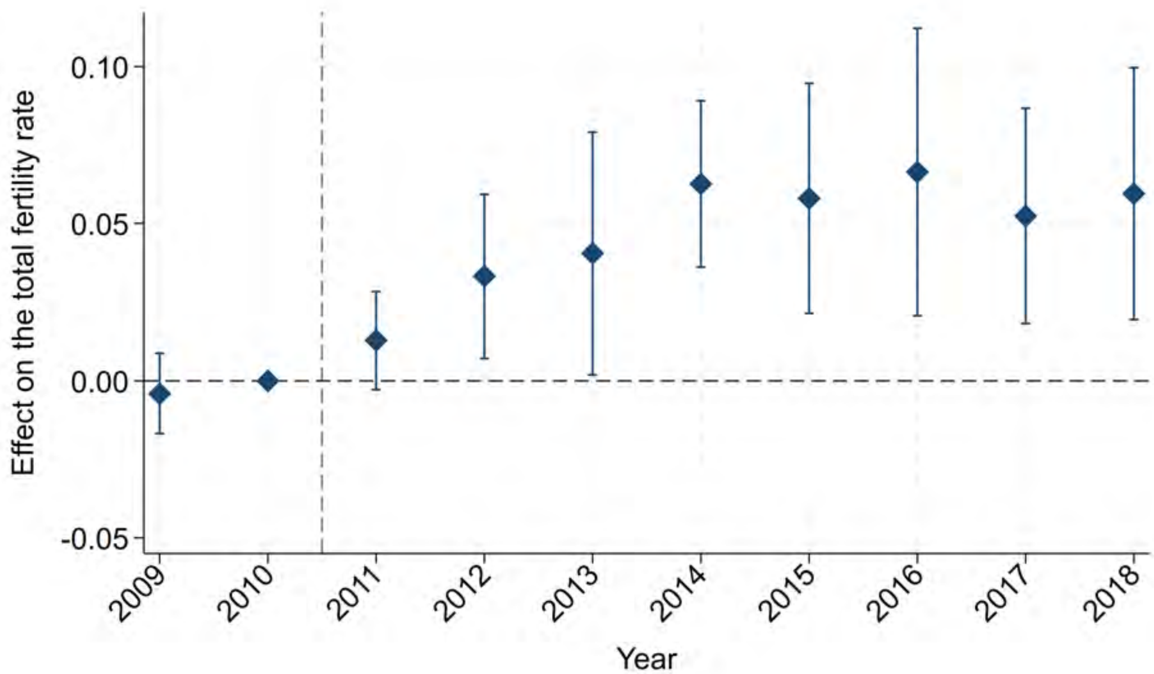
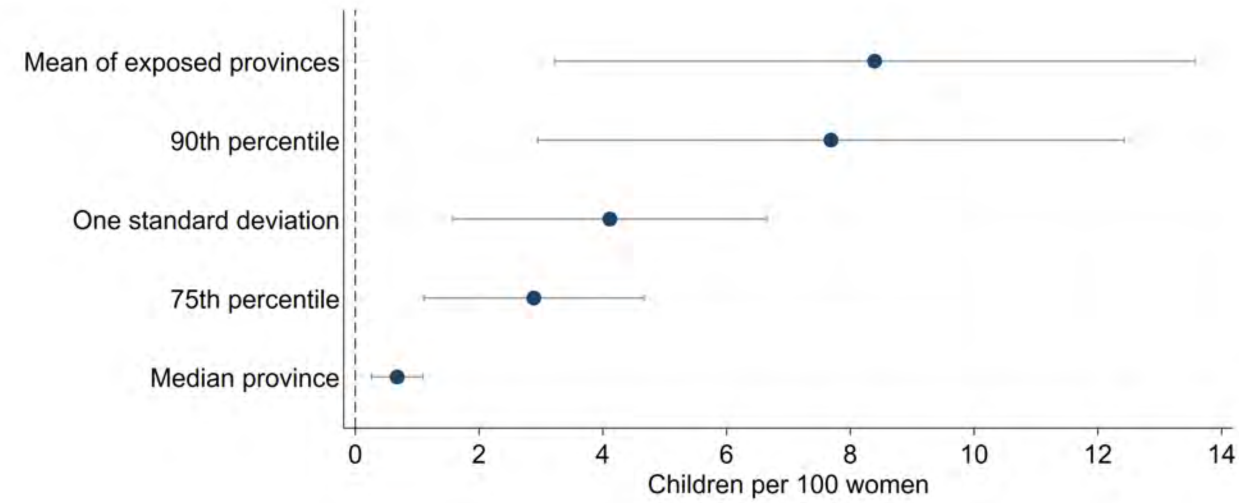
Figure 1. Two-Way Fixed-Effects Event Study of Fertility (Province Registers)



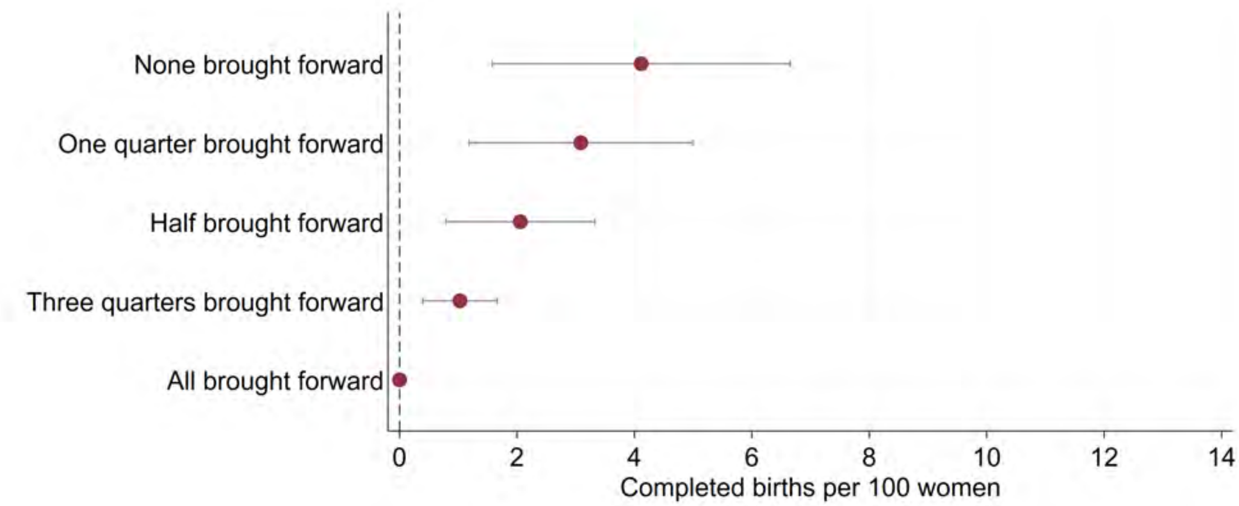
Notes: Province-level administrative birth records for 2009–2018. The figure plots the year-by-year coefficients β_y from $\text{TFR}_{pt} = \alpha_p + \delta_t + \gamma_{r(p),t} + \sum_{y \neq 2010} \beta_y \, (z_p \times \mathbf{1}[t = y]) + \sum_{y \neq 2010} (X_p^{2010} \times \mathbf{1}[t = y])' \lambda_y + \varepsilon_{pt}$, where z_p is standardized, time-invariant predicted refugee exposure, α_p are province fixed effects, δ_t calendar-year fixed effects, and $\gamma_{r(p),t}$ NUTS-1 region by year fixed effects, which absorb region-wide shocks in any year. X_p^{2010} collects the province's pre-inflow (2010) unemployment rate, higher-education index, and log trade volume, each interacted with year indicators, so that provinces may trend differentially by their baseline characteristics. Estimates are weighted by the province's population in 2010, a fixed pre-inflow weight that is the same in every year, so that neither the covariates nor the weights can respond to the inflow. Identification of β_y is therefore within region and year. The omitted reference year is 2010, plotted at zero. The regressor is standardized predicted exposure, so each plotted coefficient is the reduced-form effect of a one-standard-deviation increase in predicted exposure and is not scaled to a 10 percentage-point increase in the refugee-to-population ratio. Vertical bars are 95 percent confidence intervals from province-clustered standard errors. The sample is 800 province-years in 80 province clusters: Türkiye has 81 provinces, but Istanbul is the only province in its NUTS-1 region and is absorbed by the region-by-year fixed effects, so 80 provinces contribute over the ten years. *Source:* Central Population Administration System (MERNIS) province birth records and TurkStat province population statistics, 2009–2018; authors' calculations.

Figure 2. Magnitude of the Fertility Response

A. Effect by exposure level, 95 percent intervals



B. At one standard deviation, if this share of the response is only brought forward



Notes: Panel A converts the two-stage least-squares estimate in column 1 of Table 1, 0.101 additional children per woman per 10 percentage points of exposure, into additional children per 100 women at five exposure levels, carrying its 95 percent confidence interval through each scaling. The refugee-to-population ratio is instrumented by distance-based predicted exposure and standard errors are clustered at the province level. Four of the levels are statistics of the 2018 distribution of that ratio across the 81 provinces, which is highly skewed: the median province stands at 0.67 refugees per 100 natives, the 75th percentile at 2.86 and the 90th at 7.63, and the 33 provinces above one are the exposed provinces, whose mean of 8.33 supplies the top row. The fifth is one standard deviation of exposure in the woman-year panel, 4.08; only 13 provinces exceed it. Kilis, the most exposed at 86.40, is not plotted: extrapolating the coefficient that far reaches well outside the variation identifying it. Panel B asks what the estimate implies for completed fertility. The outcome is cumulative children for women who had not finished childbearing by 2018, so a birth merely brought forward raises it as much as one that would not. Each row assumes a share of the response is only brought forward and so adds nothing to completed fertility, and reports the remainder: the top row assumes none is brought forward, so the completed effect is the measured effect in full, and the bottom row assumes all of it is, so the effect is zero by construction. The share is assumed, not estimated; all sampling uncertainty comes from the measured effect. Both panels share a horizontal scale.
Source: Turkish Demographic and Health Surveys, 2013 and 2018 waves; Disaster and Emergency Management Authority (AFAD) refugee counts and Turkish Statistical Institute province data; authors' calculations.

Table 1. Cumulative Fertility and Parity Transitions, DHS Pseudo-Panel

	(1) Total number of children	(2) First birth	(3) Second-or-higher birth
Panel A. OLS estimates			
Refugee/pop. ratio (per 10 pp)	0.065**	0.010**	−0.007
	(0.025)	(0.004)	(0.005)
Panel B. IV (2SLS) estimates			
Refugee/pop. ratio (per 10 pp)	0.101***	0.016***	−0.003
	(0.032)	(0.006)	(0.005)
First-stage coefficient (×100)	0.182***	0.195***	0.175***
	(0.019)	(0.031)	(0.015)
Mean of outcome	1.752	0.063	0.084
Effect per 1 SD of exposure (4.08 pp)	0.041	0.006	−0.001
Observations	109,747	38,223	70,658
First-stage F (Kleibergen-Paap)	88.0	39.5	141.5

Notes: This table uses the DHS birth-history pseudo-panel from the 2013 and 2018 retrospective birth histories, one observation per woman-year. The estimation sample is native women aged 15–49, observed over 2009–2018. Column 1 is the total number of children, a stock. Column 2 is the annual probability of a first birth, estimated on woman-years in which the woman is childless at the start of the year. Column 3 is the annual probability of a second-or-higher birth, estimated on woman-years in which she already has at least one child. All estimates include woman, calendar-year and single-year age fixed effects, and the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume interacted with calendar-year indicators. Estimates are unweighted. The refugee-to-population ratio is instrumented by distance-based predicted exposure. The mean of the outcome and the number of observations are common to both panels; the effect per standard deviation and the first-stage statistics refer to Panel B. Standard errors clustered at the province level are reported in parentheses. The first-stage coefficient is multiplied by 100 so that it is reported to the same three decimals as the rest of the table. Columns 2 and 3 sum to 108,881 rather than 109,747 because the woman fixed effects drop singleton risk-set groups. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; authors' calculations.

Table 2. Total Number of Children by the Woman's Age (DHS Pseudo-Panel)

	(1)	(2)	(3)	(4)
	Total number of children			
Age band	Refugee/pop. ratio (per 10 pp)	Mean of outcome	Observations	First-stage F (Kleibergen-Paap)
Panel A. Mutually exclusive five-year bands				
Ages 15–19	0.046**	0.069	16,428	41.7
	(0.021)			
Ages 20–24	0.082	0.644	16,575	44.6
	(0.057)			
Ages 25–29	0.159***	1.514	17,544	25.7
	(0.042)			
Ages 30–34	0.071*	2.263	18,045	155.2
	(0.041)			
Ages 35–39	0.042	2.765	16,717	136.9
	(0.045)			
Ages 40–44	0.003	2.920	13,117	144.4
	(0.018)			
Ages 45–49	−0.002	3.125	5,564	68.9
	(0.002)			
Panel B. Mutually exclusive broad bands				
Ages 20–29	0.185***	1.089	35,157	34.4
	(0.048)			
Ages 30–39	0.064	2.508	35,809	99.3
	(0.050)			
Difference (20–29 minus 30–39)	0.121			
	(0.074)			
p-value, equality of the two	0.100			

Notes: This table uses the DHS birth-history pseudo-panel built from the 2013 and 2018 retrospective birth histories, one observation per woman-year. The estimation sample is native women aged 15–49, observed over 2009–2018, cut in each row to the age band named in the stub. The outcome is the total number of children, a stock, in every row. All estimates include woman, calendar-year and single-year age fixed effects, and the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume, each interacted with calendar-year indicators. Estimates are unweighted. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are reported in parentheses. Each row is a separate IV estimate on the main specification of Table 1, restricted to the woman-years in that band. The bands within a panel are mutually exclusive, so no woman-year is counted twice, but they are not exhaustive: Panel B covers ages 20 to 39 only. The seven bands of Panel A together hold 103,990 woman-years against the 109,747 of the main estimation sample; the shortfall is woman-years dropped as singletons once the sample is cut to a single band. Every band's estimation sample covers 80 provinces except ages 40–44, which covers 81: the DHS sampled women in province 62 (Tunceli) only at ages 40–44, so that province enters no other band. The final block reports the difference between the two broad bands with its own cluster-robust standard error, computed from the province-level influence functions of the two estimates. It is a formal test of equality. A difference in significance between the two bands is not on its own evidence that the effects differ. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; authors' calculations.

Table 3. Fertility Responses by the Woman's Education (DHS Pseudo-Panel)

	(1)	(2)	(3)	(4)	(5)	(6)
	Total number of children		First birth		Second-or-higher birth	
	University degree	No degree	University degree	No degree	University degree	No degree
Refugee/pop. ratio (per 10 pp)	0.096***	0.093***	0.007	0.021**	0.013	−0.004
	(0.031)	(0.035)	(0.011)	(0.010)	(0.010)	(0.005)
Mean of outcome	0.553	2.032	0.040	0.078	0.073	0.085
Observations	20,795	88,952	14,331	23,892	6,285	64,371
First-stage F (Kleibergen-Paap)	15.0	173.7	15.5	113.8	13.1	196.1
Difference (university minus no degree)	0.003		−0.013		0.017	
	(0.048)		(0.017)		(0.011)	
p-value, equality of the two	0.946		0.436		0.121	

Notes: This table uses the DHS birth-history pseudo-panel from the 2013 and 2018 retrospective birth histories, one observation per woman-year. The estimation sample is native women aged 15–49 over 2009–2018, split by their own education: the more educated group is women with a university degree, the less educated group is all other women. Columns 1 and 2 are the total number of children, a stock. Columns 3 and 4 are the annual probability of a first birth, estimated on woman-years in which the woman is childless at the start of the year. Columns 5 and 6 are the annual probability of a second-or-higher birth, estimated on woman-years in which she already has at least one child. All estimates include woman, calendar-year and single-year age fixed effects, and the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume, each interacted with calendar-year indicators. Estimates are unweighted. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are reported in parentheses. Every estimate is a separate IV regression on the main specification of Table 1, restricted to the education group named in the column head. Groups are defined by the surveys' categorical attainment variables, not a years-of-schooling threshold, which is unusable here because the Secondary category extends to 13 years while Higher begins at 11; education is recorded once per woman, so the groups partition women, not woman-years. The difference is the university group minus the rest, with a cluster-robust standard error built from the province-level influence functions of the two estimates, which reproduce each subgroup's own standard error to within 0.6 percent. A difference in significance between two subgroup estimates is not evidence that the effects differ. A pooled interacted IV is not reported: the education indicator is time-invariant given woman fixed effects, its conditional first-stage F runs from 6.9 to 36.8, and its implied interaction contradicts the subgroup difference in sign. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; authors' calculations.

Table 4. Province Fertility, Age Profile and Timing (Administrative Records)

	(1) Total fertility rate	(2) Birth rate, ages 20–29	(3) Birth rate, ages 30–39	(4) Mean age at first birth
Panel A. OLS estimates				
Refugee/pop. ratio (per 10 pp)	0.036*	2.00	0.93	−0.028
	(0.021)	(1.55)	(0.67)	(0.021)
Panel B. IV (2SLS) estimates				
Refugee/pop. ratio (per 10 pp)	0.073***	3.07**	2.38*	−0.115**
	(0.024)	(1.50)	(1.25)	(0.047)
Mean of outcome	2.17	119.70	74.96	25.36
Observations	800	800	800	400
First-stage F (Kleibergen-Paap)	41.9	41.9	41.9	43.7

Notes: This table uses province-level administrative birth records for 2009–2018, one observation per province-year. The estimation sample is drawn from all 81 provinces observed over 2009–2018. Column 1 is the total fertility rate; columns 2 and 3 are births to women in the stated age band divided by the female population in that band, per 1,000 women, built from counts and the implied population rather than as averages of the underlying five-year rates; column 4 is the mean age of mother at first birth in years, published for 2014–2018 only, hence its smaller sample. These two broad age bands aggregate over the finer age profile in Table A.7. Both panels use the main specification: province and calendar-year fixed effects, NUTS-1 region by year fixed effects, pre-inflow (2010) unemployment, higher education and log trade volume each interacted with year, and are weighted by the province's population in 2010, a fixed pre-inflow weight that is the same in every year and cannot respond to the inflow. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are reported in parentheses. Istanbul is the sole province in its NUTS-1 region and is therefore absorbed entirely by the region-by-year fixed effects, so 80 of the 81 provinces contribute to the estimates, which is why the sample contains 800 rather than 810 province-years (400 rather than 405 in column 4). Means of the outcome are computed over all 81 provinces; the estimates use the 80 provinces that contribute. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Central Population Administration System (MERNIS) province birth records and TurkStat province population statistics, 2009–2018; authors' calculations.

Table 5. Women's Own Employment (DHS Pseudo-Panel)

	(1)	(2)	(3)	(4)	(5)
	Employed during the year				
	All women	University	No university	High school or above	Below high school
Panel A. OLS estimates					
Refugee/pop. ratio (per 10 pp)	−0.013	−0.005	−0.016**	−0.004	−0.017***
	(0.009)	(0.011)	(0.007)	(0.016)	(0.006)
Panel B. IV (2SLS) estimates					
Refugee/pop. ratio (per 10 pp)	−0.036***	−0.012	−0.038***	−0.031*	−0.037***
	(0.010)	(0.017)	(0.010)	(0.017)	(0.009)
Mean of outcome	0.306	0.458	0.270	0.356	0.272
Observations	109,747	20,795	88,952	44,278	65,469
First-stage F (Kleibergen-Paap)	88.0	15.0	173.7	35.3	164.4
Difference (more minus less)		0.026		0.006	
		(0.018)		(0.018)	
p-value, equality of the two		0.135		0.741	

Notes: This table uses the DHS birth-history pseudo-panel built from the retrospective birth histories of the 2013 and 2018 waves, one observation per woman-year. The estimation sample is native women, aged 15–49, observed over 2009–2018. The outcome in every column is an indicator that the woman held at least one job at some point during the calendar year. It is constructed from the surveys' retrospective job histories, which record the start and end dates of every job the woman reports, including self-employment, seasonal and daily work, and unpaid work in a family business. A job still held at the interview date is treated as running to the interview, and a job whose end month is not recalled is dated by its reported end year. Because a woman who worked in any month of the year is coded one, the annual rate is not the same quantity as the share of women working at the interview date. All estimates include woman, calendar-year and single-year age fixed effects, together with the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume, each interacted with calendar-year indicators. Estimates are unweighted. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are reported in parentheses. The mean of the outcome and the number of observations are common to both panels and are reported once, at the foot of the table; the first-stage statistics refer to Panel B. Means of the outcome are computed over the subgroup, before singleton observations are dropped from the estimation sample. Column 1 is the pooled sample. Columns 2 and 3 split it by university attainment and columns 4 and 5 by completion of high school or above, both taken from the surveys' categorical attainment variables (the same variables Table 3 uses for its university split). Each pair sums exactly to the pooled sample, but the two splits are not independent: every woman with a university degree also has high school or above. The difference at the foot of the table is the more educated group minus the less educated group within each pair, columns 2 minus 3 and columns 4 minus 5, with a cluster-robust standard error built from the province-level influence functions of the two estimates, which reproduce each subgroup's own standard error to within 0.4 percent. The smallest difference either test could detect at the 5 percent level with 80 percent power is 0.049, larger than the pooled estimate itself. A difference in significance between two subgroup estimates is not evidence that the effects differ. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; authors' calculations.

Table 6. Husbands' Employment and Job Formality (DHS Cross-Sections)

	Estimate	Mean of outcome	Observations	First-stage F
Panel A. All currently-married women				
Husband worked in the past week	−0.031	0.86	19,006	64.0
	(0.034)			
Husband's job carries social security	0.033*	0.68	18,963	63.7
	(0.017)			
Panel B. Husband's job carries social security, by couple type				
Hypergamous couples (wife less schooled)	0.044**	0.67	8,985	64.2
	(0.020)			
High-skill homogamous couples	0.036	0.85	5,554	54.5
	(0.029)			
Low-skill homogamous couples	0.024	0.53	7,979	96.4
	(0.024)			
Equality of the Panel B estimates (p-values)				
Hypergamous vs. high-skill homogamous	0.793			
Hypergamous vs. low-skill homogamous	0.303			
High-skill vs. low-skill homogamous	0.748			
All three jointly	0.571			

Notes: This table asks whether refugee exposure changes the quality of native husbands' jobs. It pools the DHS cross-sections of 2008, 2013 and 2018, one observation per currently-married woman aged 15–49, so 2008 is a pre-inflow period at zero exposure and each woman is observed once, which makes these estimates cross-sectional. The outcomes come from the survey's Turkish husband-employment module: whether the husband worked in the past week, and whether his current job carries social-security coverage (SSK, Emekli Sandıgı, Bağkur, SGK or a private scheme), the standard Turkish marker of formal, contract-covered employment. Every estimate is a separate two-stage least-squares regression of the outcome on the refugee-to-population ratio, instrumented by distance-based predicted exposure, with rural residence, mother tongue, the woman's schooling, age and its square, log trade volume, and province and survey-year fixed effects, weighted by the survey weights and clustered at the province level. The three couple types are not mutually exclusive, so the equality tests at the foot of the table are not independent-samples comparisons: they are Wald tests computed from a single stacked two-stage least-squares regression in which the three subsamples are appended and the refugee-to-population ratio, the instrument and every control, including the province and survey-year fixed effects, are interacted with the couple-type indicator, so that the three coefficients are estimated jointly with a common cluster-robust covariance matrix; the stacked coefficients and standard errors reproduce the separate regressions in the rows above. The joint test is of equality of all three estimates. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Panel A reports the two husband-employment outcomes for all currently-married women; Panel B reports the social-security-coverage outcome separately by couple type: hypergamous couples are those in which the husband has more completed years of schooling than the wife; high-skill homogamous couples are those in which both spouses have at least eight years of schooling, the completed compulsory basic level; low-skill homogamous couples are those in which neither does. The types overlap: a hypergamous couple can also fall in either homogamous group, so the three subsample sizes sum to more than the Panel A sample; the two homogamous groups are mutually exclusive. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2008, 2013 and 2018 waves; authors' calculations.

Table 7. Ideal Number of Children (Pooled DHS Cross-Sections)

	(1)	(2)
	Ideal number of children	
	Baseline controls	Extended controls
Panel A. OLS estimates		
Refugee/pop. ratio (per 10 pp)	0.111**	0.111**
	(0.043)	(0.045)
Panel B. IV (2SLS) estimates		
Refugee/pop. ratio (per 10 pp)	0.176***	0.174***
	(0.059)	(0.061)
Mean of outcome	2.749	2.749
Observations	19,868	19,691
First-stage F (Kleibergen-Paap)	66.3	66.0

Notes: This table uses the pooled 2008, 2013 and 2018 DHS cross-sections, one observation per interviewed woman. The estimation sample is ever-married women aged 15–49. The outcome in both columns is the ideal number of children, the woman's stated ideal, reported at interview. All estimates include province and survey-year fixed effects, age, age squared, years of schooling, rural residence, mother tongue and log trade volume, and are weighted by the DHS individual sampling weights; column 2 adds the household wealth index and the husband's education. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are in parentheses. The two columns differ only in the control set, so the estimates are read as a robustness pair rather than as two separate results. Column 2 loses 177 observations because the household wealth index and the husband's education are missing for some women. The outcome is measured at interview and cannot use woman fixed effects, so it is complementary to, and not a substitute for, the birth-history evidence in Table 1. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2008, 2013, and 2018 waves; authors' calculations.

Table 8. The Housing Channel, Tested at Three Links

	Estimate	Observations	First-stage F
Panel A. Effect of refugee exposure on log province house prices			
Reduced form, year FE	−0.096	581	
	(0.072)		
Reduced form, province and year FE	−0.063	581	
	(0.050)		
+ baseline house-price trends	−0.026	531	
	(0.062)		
2SLS, province and year FE	−0.384	581	126.2
	(0.303)		
2SLS, year FE	−0.311	581	6.7
	(0.304)		
Panel B. Effect on the total fertility rate, with and without a house-price control			
Without house-price control	0.305	581	22.5
	(0.265)		
With house-price control	0.320	581	21.3
	(0.289)		
Panel C. DHS fertility by homeownership (2013 wave)			
Homeowners, total number of children	0.101	8,311	
	(0.062)		
Non-homeowners, total number of children	0.097*	37,028	
	(0.052)		
Equality test, total number of children (p)	0.960		
Equality test, first birth (p)	0.589		
Equality test, second-or-higher birth (p)	0.730		
Min. detectable differential (80% power)	0.241		
Mean of outcome, total number of children	1.81		

Notes: This table collects the three tests of the housing channel. Panel A asks whether refugee exposure raises house prices, on a province panel of median sale prices per square meter (TCMB/EVDS), up to 581 province-years over 2010–2018. Rows 1–3 regress the log price on scaled distance-based predicted exposure with, in turn, year fixed effects; province and year fixed effects; and those plus baseline (2010) log-price trends. Rows 4–5 are the corresponding two-stage least-squares estimates per unit of the refugee-to-population ratio. Panel B adds the contemporaneous log house price to a province-year fertility regression on the same price panel and re-estimates the two-stage least-squares effect on the total fertility rate with and without the price control, both per unit of the ratio, not per 10 percentage points, so not on the scale of Panel C or Table 4. Panels A and B are unweighted and absorb province and year fixed effects; they are therefore not the specification of Table 4, which is weighted by 2010 population and uses NUTS-1-region-by-year fixed effects with pre-inflow characteristics interacted with year. Panel C splits the DHS birth-history pseudo-panel by homeownership, recorded in the 2013 wave only: 45,339 woman-years with a valid outcome (8,311 owners, 37,028 non-owners) enter the estimation. It reports the effect of a 10 percentage-point increase in the ratio on the total number of children by ownership, and the smallest differential detectable at 80 percent power. Cluster-robust equality tests are reported for all three fertility outcomes; the untabulated coefficients behind the two birth-transition tests are 0.030 (0.094) and 0.076 (0.043) on first births and 0.001 (0.047) and −0.021 (0.022) on second-or-higher births. Standard errors, clustered at the province level, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Central Population Administration System (MERNIS) and TurkStat province statistics; TCMB/EVDS province house-price series; Turkish Demographic and Health Surveys, 2013 wave; authors' calculations.

Table 9. Market Childcare Use Among Working Mothers (Pooled DHS Cross-Sections)

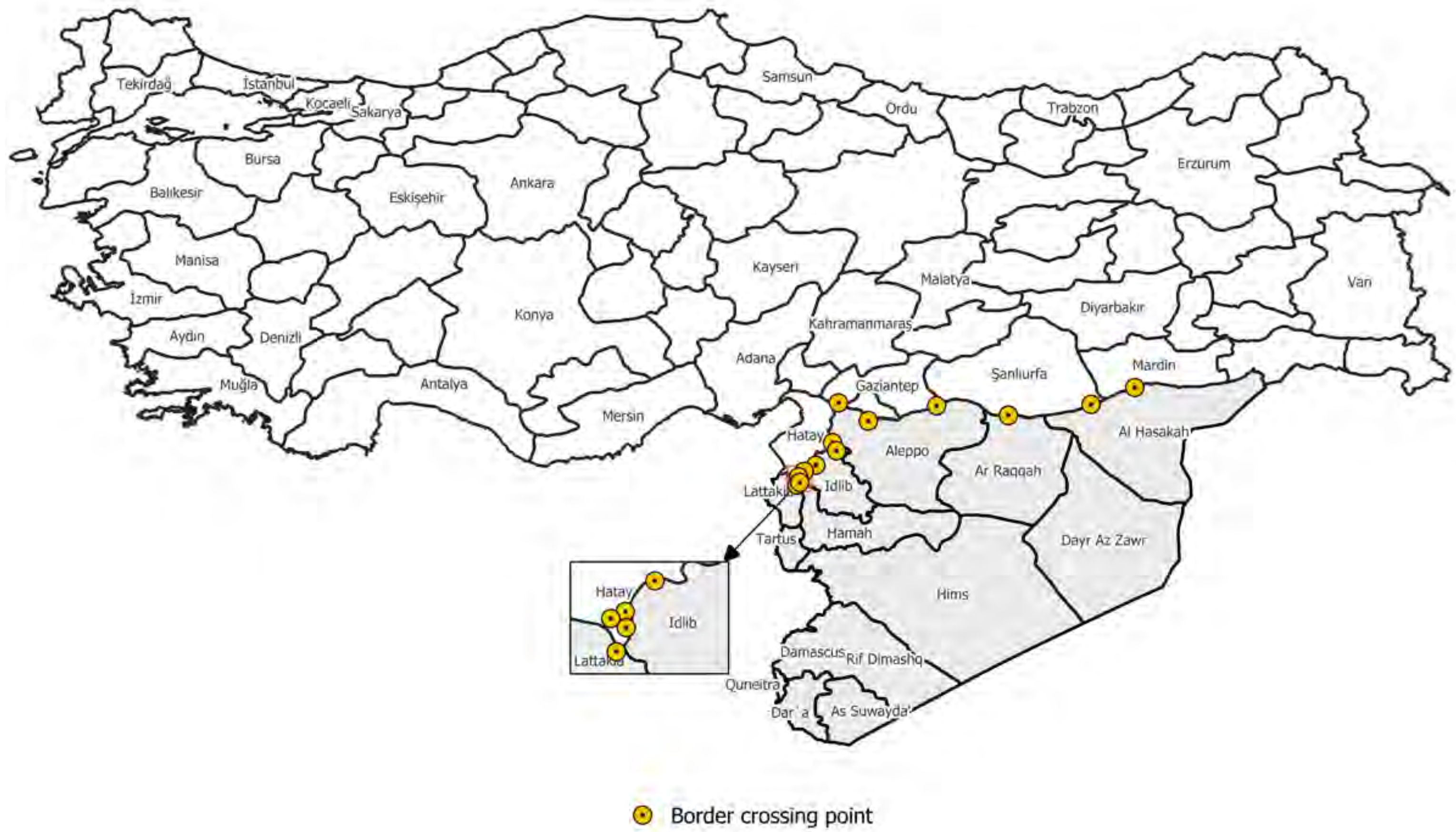
Outcome →	(1) Any market childcare	(2) Babysitter	(3) Kindergarten
Panel A. OLS estimates			
Refugee/pop. ratio (per 10 pp)	−0.016	0.004	−0.020
	(0.038)	(0.022)	(0.035)
Panel B. IV (2SLS) estimates			
Refugee/pop. ratio (per 10 pp)	−0.048	−0.026	−0.022
	(0.044)	(0.026)	(0.034)
First-stage F	31.1	31.1	31.1
Mean of outcome	0.188	0.071	0.117
Observations	1,960	1,960	1,960

Notes: Standard errors clustered at the province level are in parentheses. The sample pools the 2008, 2013 and 2018 waves of the Turkish Demographic and Health Surveys, each of which asks a working mother (women aged 15–49) who looks after her youngest child while she works; there is one observation per woman, so the estimates are cross-sectional. Pooling the three waves covers all 81 provinces. The 2003 wave does not carry the question. Women who report not having worked since the last birth are excluded, because they are not at risk of using childcare. The outcome in column 1 equals one if the youngest child is looked after by a babysitter or by a kindergarten, and zero if the child is looked after by the mother herself, her husband, her own children, her mother, her mother-in-law or another relative. Columns 2 and 3 take the two market categories separately. Panel A reports ordinary least squares estimates. Panel B instruments the refugee-to-population ratio with the distance-based measure of predicted exposure, and the reported F statistic refers to the excluded instrument in the first stage. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio; the per-unit source estimates and standard errors were divided by ten, an exact transformation. All columns include province and year fixed effects, the woman's age and its square, her education category and an urban indicator, and are weighted by DHS sampling weights. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2008, 2013 and 2018 waves; authors' calculations.

Appendix

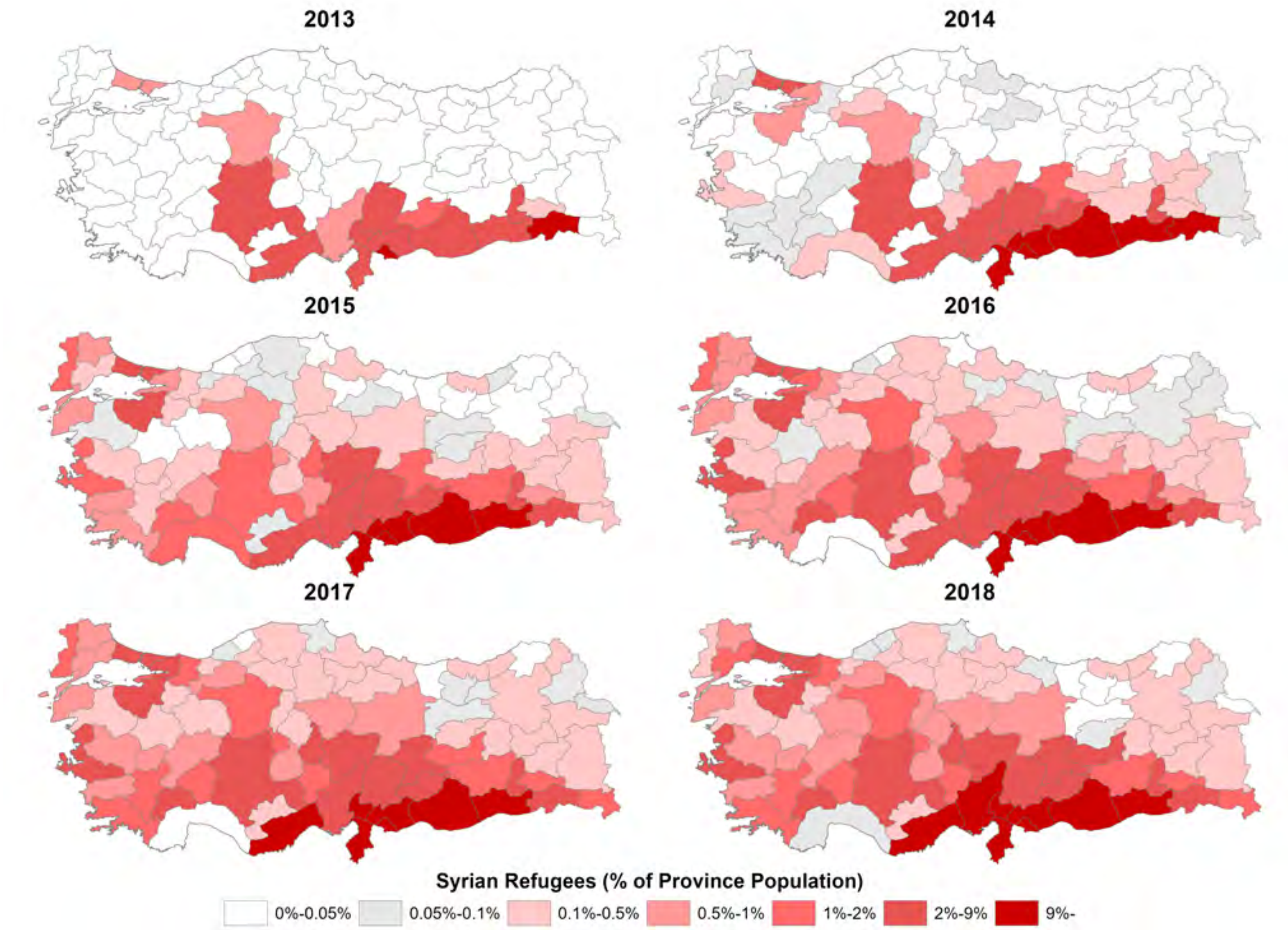
Supplementary Figures and Tables

Figure A.1. Türkiye–Syria Border Crossings



Notes: The map marks the main Türkiye–Syria border crossing points open during the initial phase of Syrian displacement. Boundaries are drawn at the province level for Türkiye and at the governorate level for Syria, and Syrian governorates are shaded. The inset enlarges the area around Hatay, where several crossing points lie close together. *Source:* Crossing points compiled from Turkish Disaster and Emergency Management Authority (AFAD) reporting; province and governorate boundaries from the GADM administrative-areas database; authors' drawing.

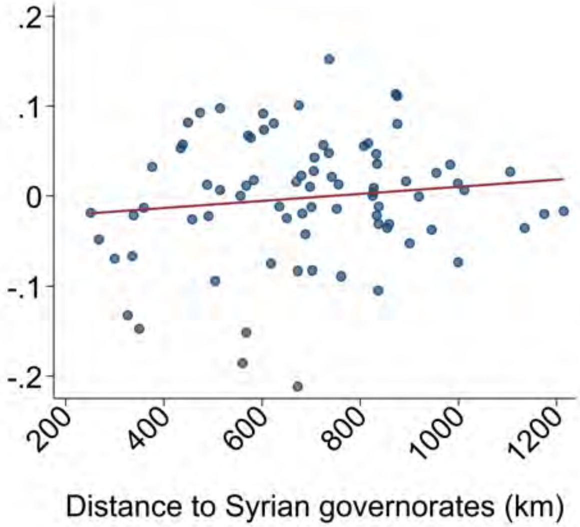
Figure A.2. Spatial Distribution of Syrian Refugees across Turkish Provinces, 2013–2018



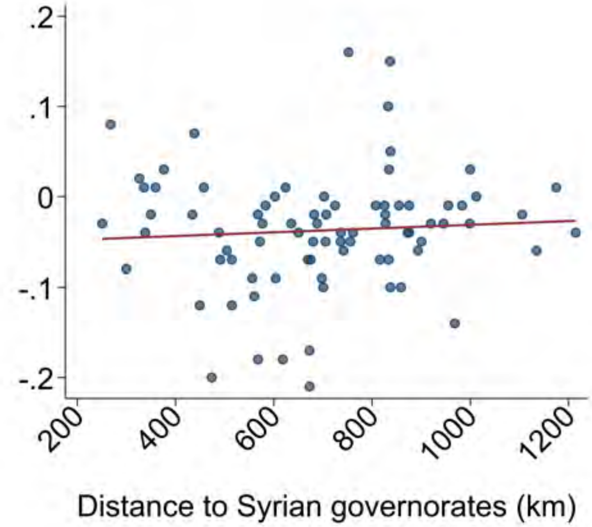
Notes: Each panel maps the refugee-to-population ratio by province for one year from 2013 to 2018. The ratio is registered Syrian refugees in the province divided by the province's native population. *Source:* Turkish Disaster and Emergency Management Authority (AFAD) province refugee counts and TurkStat province population statistics.

Figure A.3. Pre-Migration (pre-2011) Fertility and Fertility Preferences vs. Distance to the Syrian Border

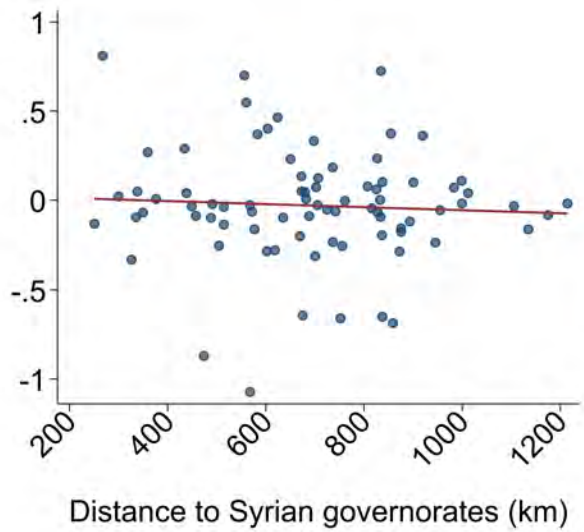
A. Change in births per woman



B. Change in the total fertility rate

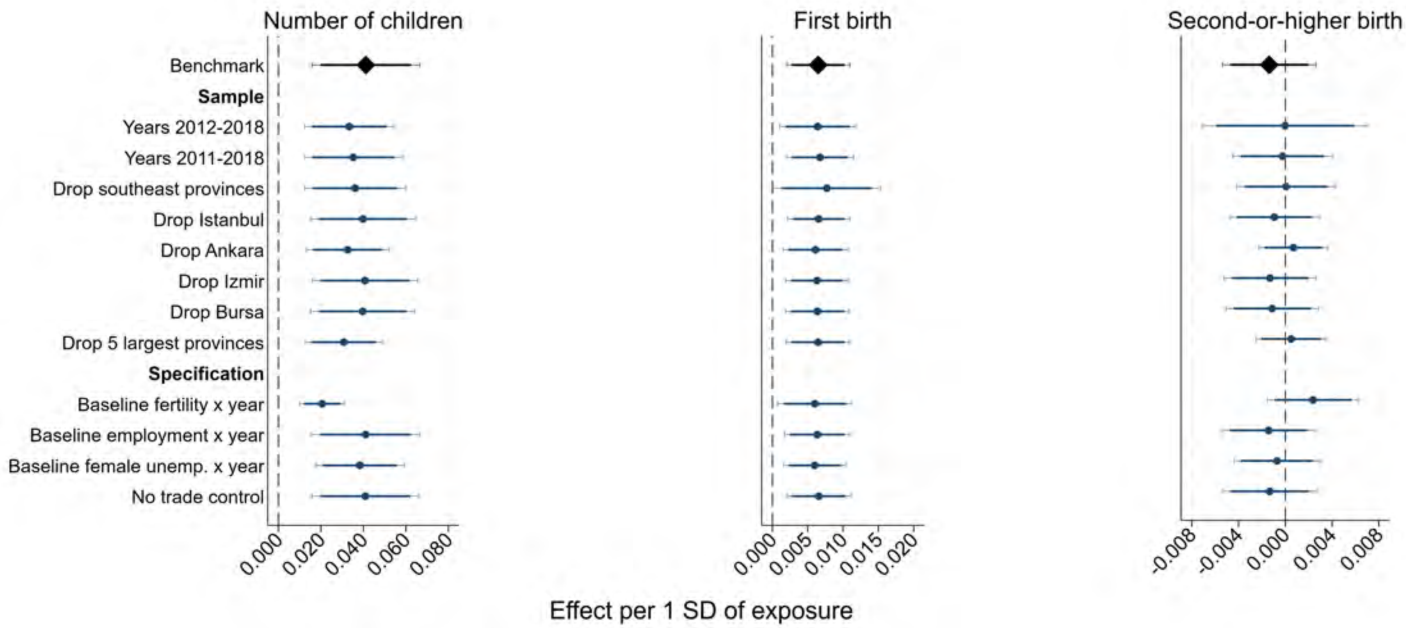


C. Change in the ideal no of children



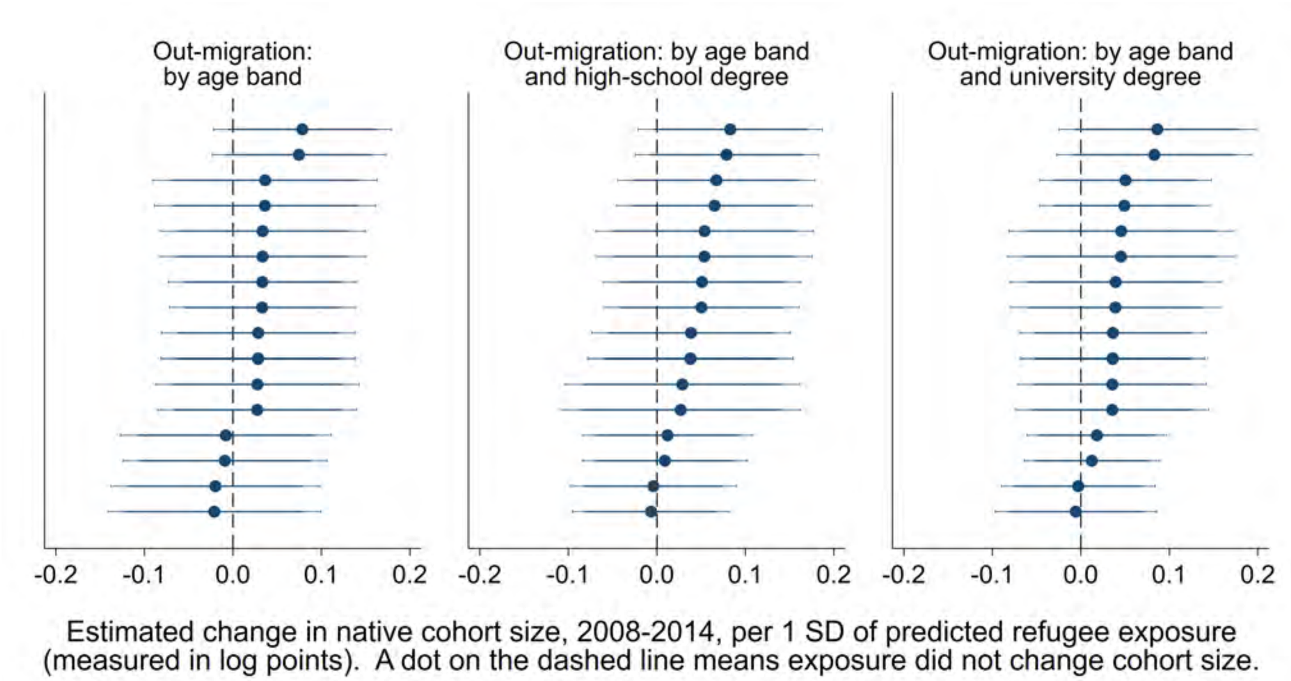
Notes: Each panel plots a pre-inflow change in fertility or fertility preferences against the province's average straight-line distance to the thirteen Syrian governorate capitals, the distance measure that enters the instrument. Panel A plots the province-level change in births between the 2003 wave of the Demographic and Health Surveys, fielded in 2003 and 2004, and the 2008 wave. Panel B plots the province-level change in the total fertility rate between 2009 and 2010 using population-register data. Panel C plots the change in the stated ideal number of children between the same two DHS waves. None of the three relationships is statistically significant: the correlation with distance is 0.13 in Panel A ($p = 0.27$), 0.07 in Panel B ($p = 0.54$), and -0.06 in Panel C ($p = 0.60$). *Source:* Turkish Demographic and Health Surveys, 2003 and 2008 waves (Panels A and C); Central Population Administration System (MERNIS) province birth records and TurkStat province population statistics (Panel B); authors' calculations.

Figure A.4. Robustness of the Fertility Response (DHS Pseudo-Panel)



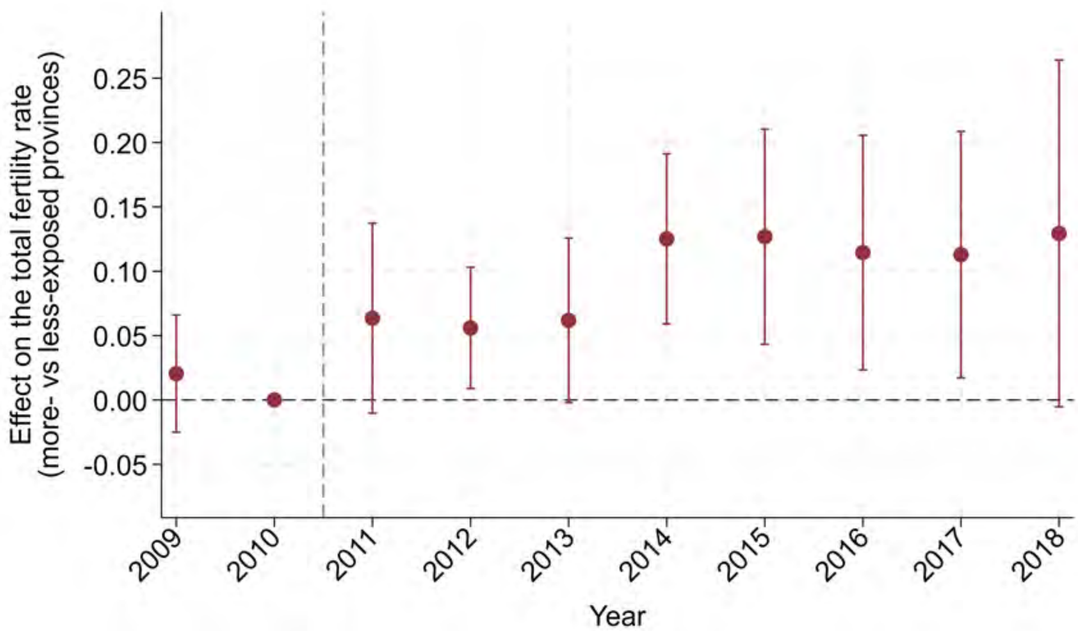
Notes: This figure reports the fertility response across a range of specifications, each a separate two-stage least-squares estimate on the DHS birth-history pseudo-panel built from the 2013 and 2018 retrospective birth histories, one observation per woman-year; the estimation sample is native women aged 15 to 49, observed over 2009 to 2018. The three sub-plots are the three outcomes of Table 1: the total number of children, the first-birth transition, and the second-or-higher-birth transition. Within each sub-plot, every specification absorbs woman, calendar-year and single-year age fixed effects together with the province's pre-inflow characteristics interacted with year, and every point is the effect of a one-standard-deviation increase in refugee exposure, 4.08 percentage points of the refugee-to-population ratio in the woman-year panel, instrumented by distance-based predicted exposure; the thick inner bar is a 90 percent and the thin capped outer bar a 95 percent confidence interval, and the vertical line marks zero. The black diamond is the main specification, and the rows below it are read against it. The Sample rows vary the estimation sample: the post-inflow windows 2012 to 2018 and 2011 to 2018, and dropping in turn the southeastern provinces, Istanbul, Ankara, Izmir, Bursa, and the five largest provinces (Istanbul, Ankara, Izmir, Bursa and Adana) together. The Specification rows vary the controls, adding the province's baseline (2010) total fertility rate, employment rate, or female unemployment rate, each interacted with calendar-year indicators and on top of the baseline unemployment, higher-education and log trade volume controls already included, and, in the last row, omitting the log trade volume control. Row labels are printed once, on the left sub-plot, and apply to all three. Standard errors are clustered at the province level throughout. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; authors' calculations.

Figure A.5. Selective Out-Migration: Cohort-Size Estimates (NSDVW)



Notes: This figure tests for selective native out-migration, the concern that if refugee exposure drove natives out of more-exposed provinces the fertility estimates could be biased. It uses the National Survey on Domestic Violence Against Women, pooled 2008 and 2014 waves. Province cells defined by age band in 2008 (and, in two of the three cell definitions, also by schooling) are aged forward six years and matched to the survey-weighted size of the same birth cohorts in 2014, and the 2014-to-2008 change in cell size is regressed across provinces on standardized distance-based predicted exposure. The three panels group the estimates by how the native cohort cells are defined: by age band alone (left), by age interacted with completion of high school or above (middle), and by age interacted with holding a university degree (right). Within each panel the sixteen remaining specification choices are plotted, sorted by the estimate: two age widths (five- and ten-year), two transformations of the outcome (log ratio and inverse hyperbolic sine), weighted and unweighted by 2008 cell size, and with and without a minimum-ten-respondents rule; across the three panels this is the full set of 48 pre-specified specifications, none omitted. The NSDVW does not cover every province, so these regressions use between 43 and 51 provinces depending on the cell definition and the minimum-respondents rule, fewer than the 81 provinces of the administrative panel. Each coefficient is the effect of a one-standard-deviation increase in predicted exposure, in log points of cohort size. Thick bars are 90 percent and thin capped bars 95 percent confidence intervals built from standard errors clustered at the province level; the dashed line marks zero, no differential change in cohort size. The joint test reported in the text stacks the three cell definitions within a single grid variant, five-year age bands, the log-ratio outcome, weighted, no minimum-respondents rule, and tests that predicted exposure has no effect in any of the three ($F(3, 50) = 0.29$, $p = 0.835$); it covers those three coefficients, not all 48 plotted here. The design can detect a differential change in cohort size of roughly 12 to 21 percent (0.109 to 0.194 log points) per standard deviation of exposure at 80 percent power and 5 percent size. The survey weights are normalized rather than population-expansion weights, so this compares survey-weighted cohort sizes across waves, not population counts. A companion DHS-internal check re-estimates the cumulative-fertility response on the women who had not changed residence since before the inflow, identified from the 2013 wave's dated migration history (Table A.12). *Source:* National Survey on Domestic Violence Against Women (NSDVW), 2008 and 2014 waves; Turkish Demographic and Health Surveys; authors' calculations.

Figure A.6. Heterogeneity-Robust Event Study of Fertility (Province Registers)



Notes: This figure re-estimates the administrative event study of Figure 1 with the Callaway and Sant'Anna (2021) difference-in-differences estimator, a method robust to treatment effects that vary across units and over time. Instead of the two-way fixed-effects structure of Figure 1, it builds the counterfactual from an explicit never-treated comparison group. Because refugee exposure reaches every province at essentially the same time, the method needs a binary treatment, so we split provinces at the mean of predicted exposure: the 26 above-average provinces are the treated group from 2012, when the response appears in Figure 1, and the 55 below-average provinces are the never-treated comparison. All 81 provinces contribute here, whereas the two-way estimates use 80, because Istanbul, the only province in its NUTS-1 region, is absorbed by the region-by-year fixed effects. For each year the estimator computes the difference in the total fertility rate between the two groups, adjusting for the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume, weighting by the province's population in 2010, a fixed pre-inflow weight that is the same in every year, and clustering standard errors at the province level. Each plotted point is the fertility gap between more- and less-exposed provinces in that year. This is a high-versus-low contrast, not the per-standard-deviation coefficient of Figure 1, so the two figures' vertical scales are not comparable; only their shape and timing are. Both figures use the same reference year, 2010, the last pre-inflow year: the two-way estimator in Figure 1 omits it, and here we difference every year against it. The Callaway and Sant'Anna command anchors its base on the earliest year in the panel, so we re-base its estimates to 2010, which shifts every point by the same constant and leaves the shape and timing unchanged. 2010 therefore carries no coefficient of its own and is plotted at zero, on the reference year and without a confidence interval. Bars are 95 percent confidence intervals; the dashed line marks the last pre-inflow year, 2010. *Source:* Central Population Administration System (MERNIS) province birth records and TurkStat province population statistics, 2009–2018; authors' calculations.

Table A.1. Demographic Characteristics of Pre-War Syrians and Turkish Natives before 2012

	Syrians	Turkish	Difference
Household characteristics			
Household size	5.95 (2.95)	4.43 (2.10)	1.52^{A}
Number of children age < 15	2.05 (2.16)	1.29 (1.63)	0.76^{A}
Presence of children	0.69 (0.46)	0.55 (0.50)	0.14^{A}
Ideal number of children	3.46 (1.54)	2.85 (1.00)	0.61^{A}
Educational attainment			
Primary school or less	0.55 (0.50)	0.36 (0.48)	0.18^{A}
Secondary	0.38 (0.49)	0.53 (0.50)	-0.15^{A}
Degree level	0.07 (0.26)	0.11 (0.31)	-0.04^{A}
Other characteristics			
Married	0.58 (0.49)	0.63 (0.48)	-0.06^{A}
Urban	0.35 (0.48)	0.64 (0.48)	-0.29^{A}
Real household income	\$4,288.17 (8,281.60)	\$4,449.82 (5,046.49)	−161.64

Notes: Weighted means (standard deviations). The question on "ideal number of children" was asked only in 2008 and 2009. The sample includes roughly 2,500 respondents per country, with variation across variables due to missing data and differences in survey coverage. Sender regions (governorates): Aleppo, Idlib, Raqqa, Latakia, Hasakah, Hama. Receiver regions: Adana, Mersin, Hatay, Kahramanmaraş, Osmaniye, Gaziantep, Adıyaman, Kilis, Şanlıurfa, Diyarbakır, Mardin, Siirt, Batman, Şırnak. Education grouped into three categories: primary or less (up to 8 years of basic education), secondary (9–15 years), and tertiary (college degree or equivalent). Superscript letter A indicates a statistically significant difference ($p \leq 0.01$) between migrants and natives. Household income is adjusted to 2011 prices. *Source:* Gallup World Polls, 2008–2011; authors' calculations.

Table A.2. Descriptive Statistics, DHS Birth-History Pseudo-Panel

	Mean	SD	Min	Max	*N*
Panel A. Fertility outcomes					
Total number of children	1.752	1.785	0	15	109,747
Any birth in the year	0.081	0.272	0	1	109,747
First birth	0.063	0.244	0	1	38,223
Second-or-higher birth	0.084	0.277	0	1	70,658
Panel B. Refugee exposure and instrument					
Refugee/pop. ratio (percentage points)	1.143	4.065	0.000	96.744	109,747
Distance-based predicted exposure	152.222	250.675	0.000	2,001.951	109,747
Panel C. Pre-inflow province characteristics					
2010 unemployment rate	2.876	0.890	0.838	6.786	109,747
2010 higher-education index	6.836	2.723	2.167	14.484	109,747
Pre-inflow log trade volume	15.418	3.736	5.020	25.745	109,747
Panel D. Woman and woman-year characteristics					
Age	30.074	8.956	15	49	109,747
Years of schooling	7.680	4.550	0	22	109,732
Rural residence	0.275	0.446	0	1	109,747
Mother tongue Turkish	0.771	0.420	0	1	109,742
Mother tongue Kurdish	0.186	0.389	0	1	109,742
Mother tongue Arabic	0.024	0.152	0	1	109,742
Household wealth index (1 to 5)	2.943	1.397	1	5	109,747
Childless at the start of the year	0.353	0.478	0	1	109,747
Children ever born at interview	2.022	1.795	0	15	109,747
Age at first cohabitation	20.430	4.310	10	48	89,668
Held a job during the year	0.306	0.461	0	1	109,747
From the 2018 DHS wave	0.587	0.492	0	1	109,747
Woman-years					109,747
Women					16,638
Provinces					81

Notes: The table describes the DHS birth-history pseudo-panel, one observation per woman-year, built from the complete retrospective birth histories of the 2013 and 2018 Turkish Demographic and Health Surveys. The sample is woman-years in which the woman is aged 15–49, drawn from the national samples of those two waves and covering calendar years 2009–2018; it is the estimation sample of column 1 of Table 1, which drops woman-years with missing exposure, instrument or province controls and women observed in a single year, whom the woman fixed effects absorb. The 2013 wave supplies woman-years for 2009–2013 and the 2018 wave for 2009–2018, contributing 45,349 and 64,398 woman-years respectively. Total number of children is cumulative births up to and including the year. First birth is defined on woman-years in which the woman is childless at the start of the year and second-or-higher birth on those in which she already has a child; each is computed on its own estimation sample, hence the smaller counts. The refugee-to-population ratio is registered Syrians divided by province population; the regressions enter it in units of 10 percentage points. Its standard deviation here, 4.065 percentage points, is computed over this estimation sample; the one-standard-deviation scaling used in the text and in Figure 2, 4.08 percentage points, is computed on the same panel before women observed in a single year are dropped, which is the frame the estimation code fixes it on. Distance-based predicted exposure is the instrument of Equation 2, an index with no natural units. The pre-inflow province characteristics are fixed at their 2010 values, are constant within province, and enter interacted with calendar-year indicators. Years of schooling is the DHS single-years variable with the don't-know code, 98, set to missing. The household wealth index is the DHS quintile, 1 poorest to 5 richest. Held a job during the year indicates that any of the woman's recorded job spells overlaps the calendar year, reading every spell in the retrospective job grid and treating a spell still held at interview as ongoing. It counts employment of any kind, including seasonal and daily work and unpaid work in a family business. Age at first cohabitation is recorded only for women who have ever cohabited. Schooling, residence, mother tongue, wealth, children ever born and age at first cohabitation are measured at interview and are constant within woman. All statistics are unweighted; the specification uses no survey weights. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; Disaster and Emergency Management Authority refugee counts and Turkish Statistical Institute province data for the exposure measure, the instrument and the province characteristics; authors' calculations.

Table A.3. Descriptive Characteristics of the NSDVW Sample

	N	Mean	SD	Min	Max
Age	12,043	34.381	7.898	15	49
Years of schooling	11,301	7.041	3.508	0	21
Mother tongue is not Turkish	12,026	0.017	0.129	0	1
Rural	12,043	0.233	0.423	0	1
Woman worked last week	12,041	0.180	0.385	0	1
Partner's years of schooling	11,811	8.424	3.607	0	22
Partner worked last week	11,993	0.810	0.392	0	1
Partner formally employed	12,036	0.683	0.465	0	1
Number of children	12,043	2.156	1.392	0	14
Gave birth last year	12,043	0.088	0.283	0	1
Gave birth in the last two years	12,043	0.169	0.374	0	1
Currently pregnant	11,321	0.067	0.249	0	1

Notes: The sample includes ever-married women aged 15–49 in the pooled 2008 and 2014 waves. Statistics are unweighted. The 11,285 respondents with complete covariates enter the estimation samples reported elsewhere in the appendix. The survey is a pair of repeated cross-sections, so each woman is observed once. *Source:* National Survey on Domestic Violence Against Women (NSDVW), 2008 and 2014 waves; authors' calculations.

Table A.4. Influential Provinces and Spatial-Correlation Robustness (DHS Pseudo-Panel)

	(1) Total number of children	(2) First birth	(3) Second-or-higher birth
Panel A. Influential provinces			
Baseline			
Full sample	0.101***	0.016***	−0.003
	(0.032)	(0.006)	(0.005)
Mean of outcome	1.752	0.063	0.084
Observations	109,747	38,223	70,658
First-stage F (Kleibergen-Paap)	88.0	39.5	141.5
Sample change			
Drop highest-exposure province	0.110***	0.016**	−0.004
	(0.038)	(0.007)	(0.005)
Mean of outcome	1.751	0.063	0.083
Observations	109,561	38,136	70,561
First-stage F (Kleibergen-Paap)	177.5	142.6	192.2
Panel B. Spatial correlation (Conley) across distance cutoffs			
Alternative inference			
Conley standard error, 20 km	(0.031)	(0.006)	(0.005)
Conley standard error, 30 km	(0.031)	(0.006)	(0.005)
Conley standard error, 40 km	(0.031)	(0.006)	(0.005)
Conley standard error, 50 km	(0.031)	(0.006)	(0.005)
Conley standard error, 100 km	(0.031)	(0.007)	(0.005)
Conley standard error, 150 km	(0.032)	(0.009)	(0.005)

Notes: This table uses the DHS birth-history pseudo-panel built from the 2013 and 2018 waves, one observation per woman-year. The estimation sample is native women, aged 15–49, observed over 2009–2018. Column 1 is the total number of children, a stock. Column 2 is the annual probability of a first birth, estimated on woman-years in which the woman is childless at the start of the year, and column 3 is the annual probability of a second-or-higher birth, estimated on woman-years in which she already has at least one child. All estimates use the main specification of Table 1: woman, calendar-year and single-year age fixed effects, plus the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume, each interacted with calendar-year indicators. Estimates are unweighted. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are reported in parentheses. Panel A asks whether any single province drives the result. It drops the highest-exposure province, and re-estimates column 1 while dropping each of the 81 provinces in turn. That leave-one-out exercise was run for column 1 only, and it is not tabulated because it is 81 estimates: they range from 0.080 to 0.141, against a full-sample estimate of 0.101. Panel B reports Conley spatial-HAC standard errors for the Panel A baseline estimates, on the same sample, across Bartlett distance cutoffs from 20 to 150 km, always also allowing arbitrary within-province serial correlation; the estimates are not repeated. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; authors' calculations.

Table A.5. Suggestive Evidence on the Exclusion Restriction, Province-Level Data

Outcome →	(1) **Log of province-level exports**	(2) **Log of province-level imports**	(3) **Log of nighttime light density**
Instrument	0.000	0.000	0.000
	(0.005)	(0.009)	(0.006)
Observations	808	808	808

Notes: Each column regresses one province-year outcome directly on the distance-based instrument, so all three are reduced-form exercises with no first stage. Columns (1) and (2) use province-year trade data with Syria, the log of exports to Syria and the log of imports from Syria. Column (3) uses the log of mean nighttime light density in the province-year, from satellite composites. All three columns include province fixed effects, year fixed effects, and subregion-by-year fixed effects. Standard errors are clustered at the province (NUTS-3) level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* TurkStat province trade statistics and nighttime light density from satellite composites; authors' calculations.

Table A.6. Fertility Response by Age Window (DHS Pseudo-Panel)

Age window	(1) Total number of children	(2) First birth	(3) Second-or-higher birth
Ages 15–49 (main specification)	0.101***	0.016***	−0.003
	(0.032)	(0.006)	(0.005)
Mean of outcome	1.752	0.063	0.084
Observations	109,747	38,223	70,658
First-stage F (Kleibergen-Paap)	88.0	39.5	141.5
Ages 20–44	0.112***	0.018*	−0.005
	(0.038)	(0.010)	(0.006)
Mean of outcome	1.990	0.087	0.090
Observations	86,143	20,940	64,311
First-stage F (Kleibergen-Paap)	73.1	24.5	120.4
Ages 20–40	0.132***	0.022**	−0.003
	(0.039)	(0.010)	(0.007)
Mean of outcome	1.856	0.090	0.105
Observations	75,427	20,199	54,337
First-stage F (Kleibergen-Paap)	64.6	19.9	119.4
Ages 20–34	0.168***	0.022**	0.005
	(0.036)	(0.011)	(0.010)
Mean of outcome	1.497	0.093	0.138
Observations	54,276	18,667	34,726
First-stage F (Kleibergen-Paap)	51.6	15.0	154.9

Notes: This table uses the DHS birth-history pseudo-panel from the 2013 and 2018 retrospective birth histories, one observation per woman-year. The estimation sample is native women observed over 2009–2018, restricted in turn to each of the four age windows shown in the stub. Column 1 is the total number of children, a stock. Column 2 is the annual probability of a first birth, estimated on woman-years in which the woman is childless at the start of the year. Column 3 is the annual probability of a second-or-higher birth, estimated on woman-years in which she already has at least one child. All estimates include woman, calendar-year and single-year age fixed effects, and the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume, each interacted with calendar-year indicators. Estimates are unweighted. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are reported in parentheses. The ages 15–49 window is the main specification and reproduces Table 1 exactly. The observation count falls as the window narrows, because each restriction drops woman-years outside the age band, and within a window the three columns use different risk sets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; authors' calculations.

Table A.7. Fertility Response by the Mother's Age, Five-Year Bands (Administrative Records)

Age band	Refugee/pop. ratio (per 10 pp)	Mean of the age-specific rate
Ages 15–19	1.02	28.66
	(0.62)	
Ages 20–24	1.59	105.34
	(2.25)	
Ages 25–29	3.37***	133.79
	(1.23)	
Ages 30–34	1.46	97.98
	(0.92)	
Ages 35–39	1.96	49.70
	(1.18)	
Ages 40–44	1.64	13.82
	(1.06)	
Ages 45–49	1.12*	2.06
	(0.63)	
Observations	800	
First-stage F (Kleibergen-Paap)	41.9	

Notes: This table uses the province-year administrative birth records for 2009–2018, one observation per province-year. The outcome in each row is the age-specific birth rate for the stated five-year age band, the number of births to women of that age per 1,000 women of that age, the same scale as the broad-band birth rates in Table 4. Each row is a separate two-stage least-squares estimate on the main specification of the administrative analysis (Table 4): province, calendar-year and NUTS-1-region-by-year fixed effects, the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume each interacted with year indicators, and weighted by the province's population in 2010, a fixed pre-inflow weight that is the same in every year and cannot respond to the inflow. The refugee-to-population ratio is instrumented by distance-based predicted exposure; the first-stage Kleibergen-Paap F is 41.9, the same as in Table 4. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio on the age-specific rate; the second column gives the sample mean of that rate. Standard errors clustered at the province level are in parentheses. The estimation sample is 800 province-years in 80 province clusters, as in Table 4: Türkiye has 81 provinces, but Istanbul is the only province in its NUTS-1 region and is absorbed by the region-by-year fixed effects, so 80 provinces contribute. These finer five-year bands can be compared like-for-like with the DHS five-year age profile in Table 2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Central Population Administration System (MERNIS) province birth records and TurkStat province population statistics, 2009–2018; authors' calculations.

Table A.8. Cumulative Fertility by Baseline Household Wealth (DHS Pseudo-Panel)

	Estimate	Mean of outcome	Observations	First-stage F
Top two wealth quintiles	0.073**	1.36	41,570	10.8
	(0.035)			
Bottom three wealth quintiles	0.086***	1.99	68,177	235.7
	(0.030)			
Equality test (p-value)	0.688			

Notes: This table uses the DHS birth-history pseudo-panel built from the 2013 and 2018 retrospective birth histories, one observation per woman-year. The estimation sample is native women aged 15–49 over 2009–2018, split by baseline household wealth: the top-two-quintiles group is women in wealth quintiles four and five of the DHS wealth index, and the bottom-three group is women in quintiles one to three. Wealth is recorded once per woman, at interview, so the groups partition women rather than woman-years. The outcome is the total number of children, a stock. Every estimate is a separate two-stage least-squares regression on the main specification of Table 1, restricted to the wealth group named in the row: woman, calendar-year and single-year age fixed effects, and the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume, each interacted with calendar-year indicators. Estimates are unweighted. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are reported in parentheses. The equality test reported at the foot of the table is the two-sided p-value for the difference between the two estimates (the top-two-quintile estimate minus the bottom-three), on 81 province clusters; its cluster-robust standard error is built from the province-level influence functions of the two estimates, which reproduce each subgroup's own standard error to within 0.1 percent. A difference in the first-stage F across the two subsamples reflects their different sizes and exposure distributions and is not evidence that the effects differ. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; authors' calculations.

Table A.9. Fertility Response by Lagged Employment (DHS Pseudo-Panel)

	(1)	(2)	(3)	(4)	(5)	(6)
	Employed in year $t-1$			**Not employed in year $t-1$**		
	Total number of children	First birth	Second-or-higher birth	Total number of children	First birth	Second-or-higher birth
Panel A. OLS estimates						
Refugee/pop. ratio (per 10 pp)	0.068*	−0.003	−0.014*	0.051**	0.005	−0.003
	(0.041)	(0.015)	(0.008)	(0.024)	(0.005)	(0.004)
Panel B. IV (2SLS) estimates						
Refugee/pop. ratio (per 10 pp)	0.077	0.008	−0.011	0.088***	0.015*	0.003
	(0.056)	(0.028)	(0.011)	(0.028)	(0.008)	(0.007)
Mean of outcome	1.656	0.091	0.052	1.879	0.073	0.094
Observations	27,400	9,101	17,963	63,861	19,928	43,272
First-stage F (Kleibergen-Paap)	45.1	151.5	39.1	161.8	62.8	205.4
Difference (employed minus not)	−0.010	−0.006	−0.013			
	(0.039)	(0.028)	(0.013)			
p-value, equality of the two	0.789	0.823	0.297			

Notes: This table uses the DHS birth-history pseudo-panel built from the retrospective birth histories of the 2013 and 2018 waves, one observation per woman-year. The estimation sample is native women, aged 15–49, observed over 2009–2018. Columns 1 and 4 are the total number of children, a stock; columns 2 and 5 are the annual probability of a first birth, estimated on woman-years in which the woman is childless at the start of the year; columns 3 and 6 are the annual probability of a second-or-higher birth, estimated on woman-years in which she already has at least one child. All estimates include woman, calendar-year and single-year age fixed effects, together with the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume, each interacted with calendar-year indicators. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are reported in parentheses. Means and observation counts are common to both panels and are reported once at the foot; the first-stage statistics refer to Panel B. Estimates are unweighted. Employment in year $t-1$ is an indicator that the woman held at least one job in the previous calendar year, from the surveys' retrospective job histories, which record every job including self-employment, seasonal and daily work and unpaid family work. It is a sample splitter, not an outcome. A lagged employment status requires the woman to be observed in the previous year, so the two blocks do not sum to the full estimation sample of 109,747 woman-years. The foot of the table reports the difference between the two employment groups for each outcome, columns 1 minus 4, 2 minus 5 and 3 minus 6, with a cluster-robust standard error built from the province-level influence functions of the two estimates. The companion split by the woman's own education, estimated on the same panel and the same specification and using the surveys' categorical attainment variables, is reported in Table 3. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2013 and 2018 waves; authors' calculations.

Table A.10. Cumulative Fertility and Recent Births (NSDVW, Pooled Waves)

	(1) Total number of children	(2) Gave birth in the past year
Panel A. OLS estimates		
Refugee/pop. ratio (per 10 pp)	0.254*	0.034*
	(0.140)	(0.018)
Panel B. IV (2SLS) estimates		
Refugee/pop. ratio (per 10 pp)	0.187**	0.032*
	(0.093)	(0.017)
Mean of outcome	1.964	0.088
Observations	11,285	11,285
First-stage F (Kleibergen-Paap)	128.0	128.0

Notes: National Survey on Domestic Violence Against Women, pooled 2008 and 2014 waves, one observation per interviewed woman; ever-married women aged 15–49. The survey is a pair of repeated cross-sections, so each woman is observed once and the estimates are cross-sectional. Column 1 is the total number of children, a stock; column 2 is an indicator for having given birth in the past year. All estimates include province and survey-wave fixed effects, log trade volume, years of schooling, age and age squared, rural residence and mother tongue, and use survey weights. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio. Standard errors clustered at the province level are reported in parentheses. The survey does not observe parity at the start of the year, so the first-birth and second-or-higher-birth margins cannot be defined here; they are estimated in the DHS birth-history pseudo-panel in Table 1. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* National Survey on Domestic Violence Against Women (NSDVW), 2008 and 2014 waves; authors' calculations.

Table A.11. Placebo Test, Pre-Treatment Fertility Outcomes, DHS Pseudo-Panel

Outcome →	Gave Birth During the Year		Gave Birth During the Year	
	OLS	IV	OLS	IV
Refugee/pop. ratio (per 10 pp)	−0.003	−0.010	−0.001	−0.010
	(0.007)	(0.018)	(0.005)	(0.018)
First-stage F stat	–	20.91	–	15.17
Observations	17,309	17,309	17,309	17,309
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Additional controls	No	No	Yes	Yes

Notes: The sample is ever-married women aged 15–49 observed over 2008–2010. The outcome equals one if the woman gave birth in that calendar year, reconstructed from the retrospective birth history. All columns include woman and calendar-year fixed effects. The time-varying controls are age, age squared, and log trade volume; rural residence, years of education, and mother tongue are recorded once per woman and are therefore absorbed by the woman fixed effects rather than estimated. Columns 3 and 4 additionally control for the province unemployment rate and the higher-education index (the "Additional controls" row). Coefficients are the effect of a 10 percentage-point increase in the refugee-to-population ratio. The IV estimates instrument the concentration of Syrian refugees by the distance instrument (we use 2013 values of refugee-to-population ratio and distance instrument for each province to 2010 data). The first-stage F is the Kleibergen-Paap statistic from this same fixed-effects specification, computed on the 17,010 woman-years that remain once women observed in a single year are dropped, since those women are absorbed by their own fixed effect. Estimates are unweighted. Standard errors are clustered at the province (NUTS-3) level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* The Demographic and Health Surveys (DHS), expanded panel of women with information on children born every year during the period 2008–2010, constructed using round 2013 of the Turkish DHS.

Table A.12. Cumulative Fertility Excluding Recent Movers (DHS)

	Estimate	Mean of outcome	Observations	First-stage F
Full panel, both waves (headline)	0.101***	1.75	109,747	88.0
	(0.032)			
2013 wave (within-wave benchmark)	0.095**	1.82	45,349	15.2
	(0.044)			
excluding movers since 2011	0.084**	1.94	40,157	14.2
	(0.041)			
excluding movers since 2009	0.087**	2.00	37,462	13.8
	(0.040)			

Notes: This table asks whether the cumulative-fertility response could be an artifact of refugee-induced residential mobility, the concern that natives who moved into (or out of) more-exposed provinces after the inflow might differ systematically from those who stayed. The outcome is the total number of children. Every row re-estimates the frozen headline specification of Table 1 on the sample named in the row: woman, calendar-year and single-year age fixed effects and the province's pre-inflow (2010) unemployment rate, higher-education index and log trade volume, each interacted with calendar-year indicators, with the refugee-to-population ratio instrumented by distance-based predicted exposure. Estimates are unweighted. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio, with standard errors clustered at the province level in parentheses. The 2013 DHS wave records each woman's residential history, including the year of every move, so we can identify women who changed residence after the inflow; the 2018 wave does not date moves, which is why the split is made within the 2013 wave. The last two rows drop women whose most recent move was in 2011 or later, and in 2009 or later, restricting the sample to women settled in their province since before the inflow. This DHS-internal check complements the survey cohort-retention test of Figure A.5, which bounds selective out-migration from the population side. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Survey, 2013 and 2018 waves; authors' calculations.

Table A.13. Descriptive Statistics of the Province-Level Data

	Mean	SD	Min	Max
Total Fertility Rate	2.15	0.69	1.34	4.69
Age-Specific Birth Rates (per 100 women)				
Ages 15–19	2.75	1.53	0.42	9.40
Ages 20–24	10.29	3.61	2.88	22.27
Ages 25–29	13.27	2.78	8.80	23.65
Ages 30–34	9.77	2.93	5.34	21.04
Ages 35–39	4.96	2.40	1.74	14.65
Ages 40–44	1.37	1.20	0.27	7.83
Ages 45–49	0.20	0.36	0.00	2.38
Total Trade Volume (million TL)	756	11,300	0	196,000
Unemployment Rate	5.33	2.21	0.84	23.28
Higher Education Index	9.80	3.65	1.90	24.07
Observations	891	891	891	891

Notes: The unit of observation is the province-year. The table describes the full administrative extract, all 81 provinces over 2009–2019 (891 province-years), which runs one year beyond the 2009–2018 window used for estimation. Statistics are unweighted, so each province-year contributes equally. Age-specific birth rates are births per 100 women in the age group; Table 4 and Appendix Table A.7 report the same rates per 1,000 women. Total trade volume is expressed in millions of Turkish Lira (TL). The estimation sample of Table 4 is the 800 province-years over 2009–2018 that remain once Istanbul is absorbed by the NUTS-1-region-by-year fixed effects. *Source:* Central Population Administration System (MERNIS) province birth records and TurkStat province statistics, 2009–2019; authors' calculations.

Table A.14. Household Asset Income (NSDVW, Pooled Waves)

	(1) Household asset-income index
Panel A. Pooled sample	
Refugee/pop. ratio (per 10 pp)	0.147***
	(0.042)
Mean of outcome	0.01
Observations	11,285
First-stage F (Kleibergen-Paap)	128.0
Panel B. Hypergamous couples	
Refugee/pop. ratio (per 10 pp)	0.186***
	(0.050)
Mean of outcome	−0.03
Observations	2,477
First-stage F (Kleibergen-Paap)	97.9
Panel C. High-skill homogamous couples	
Refugee/pop. ratio (per 10 pp)	0.217**
	(0.097)
Mean of outcome	0.24
Observations	2,507
First-stage F (Kleibergen-Paap)	72.3
Panel D. Low-skill homogamous couples	
Refugee/pop. ratio (per 10 pp)	0.095
	(0.064)
Mean of outcome	−0.06
Observations	5,523
First-stage F (Kleibergen-Paap)	157.8

Notes: National Survey on Domestic Violence Against Women, pooled 2008 and 2014 waves, one observation per ever-married woman; ever-married women aged 15–49, interviewed in either wave. The survey is a pair of repeated cross-sections, so each woman is observed once and the estimates are cross-sectional. The outcome is the household asset-income index, the row-mean of six indicators of whether the woman has income from an owned asset, namely land, a house, a company, a vehicle, a bank account or other assets, each component standardized before averaging; the index itself has a standard deviation of about 0.55 in the estimation sample, so coefficients are in units of the index rather than in standard deviations. All estimates include province and wave fixed effects, log trade volume, years of schooling, age and age squared, rural residence and mother tongue, and are weighted by the women's sampling weights. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients are per 10 percentage-point increase in the refugee-to-population ratio, the per-unit estimates divided by ten. Standard errors clustered at the province level are in parentheses. Couple types are defined by an eight-years-of-schooling threshold: hypergamous couples are those in which the wife has at most eight years of schooling and the husband more than eight; high-skill homogamous couples both have more than eight years; low-skill homogamous couples both have at most eight. Panels B and C require the husband's years of schooling to be recorded, and Panel C the wife's as well, so couples with an unrecorded value are excluded from them. The three cells are mutually exclusive and do not exhaust the Panel A sample: couples in which the wife is more schooled than the husband are not shown, and 118 women in the Panel A estimation sample have no recorded husband's schooling and therefore belong to no cell. Household assets are held at the couple level, which is why this is the one exhibit in which the split is defined by the couple's skill composition rather than by the woman's own education. Means of the outcome are computed over the ever-married women aged 15–49 who satisfy the panel's couple-type condition, a slightly larger set than the estimation sample. The item-level estimates behind the index are reported in the replication files and not here. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* National Survey on Domestic Violence Against Women (NSDVW), 2008 and 2014 waves; authors' calculations.

Table A.15. Fertility and Ideal Family Size of Turkish vs. Syrian Women, DHS 2018

	Number of children	Ideal number of children	Number of children	Ideal number of children	Number of children	Ideal number of children
Syrian	0.949***	1.135***	1.137***	1.097***	1.034***	0.817***
	(0.087)	(0.128)	(0.073)	(0.123)	(0.049)	(0.105)
Years of education			−0.130***	−0.062***	−0.114***	−0.045***
			(0.007)	(0.006)	(0.005)	(0.005)
Age			0.102***	0.015***	0.108***	0.021***
			(0.007)	(0.003)	(0.007)	(0.003)
Sub-region FE	No	No	No	No	Yes	Yes
Observations	9,544	9,429	9,544	9,429	9,540	9,425

Notes: The sample pools Turkish and Syrian respondents of the 2018 DHS, a single cross-section. The regressor is an indicator for being a Syrian respondent; the outcomes are the woman's number of children and her stated ideal number of children. Columns 1 and 2 include no controls, columns 3 to 6 control for the woman's years of education and age, and columns 5 and 6 add sub-region fixed effects. Every column is an unweighted ordinary least squares regression. Standard errors in parentheses are clustered on the respondent's recorded place of residence. The 18 respondents with no recorded place of residence also drop out once the estimates are clustered, which is why columns 1 to 4 have 9,544 and 9,429 observations rather than 9,562 and 9,447; columns 5 and 6 already excluded both groups. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish and Syrian samples of the 2018 Demographic and Health Survey (DHS); authors' calculations.

Table A.16. Suggestive Evidence on Social Interaction and Family Size, KONDA Data

Outcome →	Household Size			
Sample →	Full Sample	Gov. Should Not Accept Syrians=0	Syrians Should Only Live in Camps=0	Syrians Hurt Turkish Economy=0
Ref (Never)	–	–	–	–
Rarely	0.339**	0.114	0.277	−0.093
	(0.146)	(0.249)	(0.247)	(0.340)
Frequently	0.452***	0.374	0.397*	0.092
	(0.160)	(0.258)	(0.230)	(0.289)
R^2	0.170	0.185	0.181	0.152
Observations	2,482	938	1,078	716
Sample →	Full Sample	Gov. Should Not Accept Syrians=1	Syrians Should Only Live in Camps=1	Syrians Hurt Turkish Economy=1
Ref (Never)	–	–	–	–
Rarely	0.339**	0.484***	0.377**	0.445**
	(0.146)	(0.169)	(0.180)	(0.178)
Frequently	0.452***	0.544***	0.444**	0.521**
	(0.160)	(0.176)	(0.180)	(0.204)
R^2	0.170	0.189	0.185	0.202
Observations	2,482	1,544	1,404	1,766

Notes: Standard errors clustered at the province level are in parentheses. The outcome is household size. The regressor is the respondent's self-reported frequency of contact with Syrians, grouped into three categories, with Never as the omitted reference category, shown as a dash. The estimates are cross-sectional associations from a single survey round of 2,649 adults aged 18 and above in 27 provinces (2,482 with complete covariates), so the province fixed effects absorb level differences across provinces and no within-province change over time is used. All specifications control for province fixed effects, demographic characteristics, labor market status, and log of household income. Estimates are unweighted. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* KONDA Survey, February 2016; authors' calculations.

Table A.17. Descriptive Characteristics of the KONDA Sample

Variables	Mean (SD)
Age	41.02 (14.68)
Male	0.53 (0.50)
Household size	4.43 (2.23)
Primary school or less	0.44 (0.50)
Secondary	0.33 (0.47)
Degree level	0.16 (0.37)
Sunni Muslim	0.91 (0.29)
Urban	0.79 (0.41)
Unemployed	0.05 (0.22)
Household income (TL)	2,225 (1,675)
Interaction with Syrians	
Frequently	0.67 (0.47)
Rarely	0.20 (0.40)
Never	–
Attitudes toward Syrians	
Government should not accept Syrians anymore	0.62 (0.49)
Syrians should only live in camps	0.56 (0.50)
Syrians hurt the Turkish economy	0.71 (0.45)
Syrians will return after the war	0.51 (0.50)
Can live with Syrians in the same city	0.72 (0.45)
Sample size	2,649

Notes: Unweighted means with standard deviations in parentheses. Variables capture respondents' demographic characteristics, interaction frequency with Syrians, and attitudes toward Syrians. Household income is reported in Turkish Lira (TL). This table describes the full KONDA survey of 2,649 respondents; the estimation sample in that table is the 2,482 respondents with complete covariates. The survey is a single cross-section of adults aged 18 and above in 27 provinces. *Source:* KONDA Survey, February 2016; authors' calculations.

Table A.18. Marriage and Divorce (DHS Cross-Sections and NSDVW)

	(1) Currently married	(2) Divorced or separated
Panel A. Pooled DHS cross-sections		
Refugee/pop. ratio (per 10 pp)	0.0200***	−0.0128**
	(0.0071)	(0.0055)
Mean of outcome	0.942	0.040
Observations	20,082	20,082
First-stage F (Kleibergen-Paap)	64.6	64.6
Panel B. NSDVW		
Refugee/pop. ratio (per 10 pp)	0.0244*	−0.0140
	(0.0129)	(0.0112)
Observations	11,285	11,285
First-stage F (Kleibergen-Paap)	128.0	128.0

Notes: Panel A uses the pooled DHS cross-sections, the 2008, 2013 and 2018 waves; Panel B uses the National Survey on Domestic Violence Against Women, the 2008 and 2014 waves. Both panels have one observation per interviewed woman, so both sets of estimates are cross-sectional. The estimation sample in both panels is ever-married women aged 15–49. Column 1 reports an indicator for being currently married at the interview date; column 2 reports an indicator for being divorced or separated, built in the DHS from the marital-status codes of the individual recode. All estimates include province and survey-year fixed effects, log trade volume, years of schooling, age, age squared, rural residence and mother tongue, and both panels are weighted by their survey's sampling weights. The refugee-to-population ratio is instrumented by distance-based predicted exposure. Coefficients report the effect of a 10 percentage-point increase in the refugee-to-population ratio; the per-unit source estimates and standard errors were divided by ten, an exact transformation. Standard errors clustered at the province level are reported in parentheses. Outcome means are reported for Panel A only; they are not reported for the NSDVW panel in Panel B. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. *Source:* Turkish Demographic and Health Surveys, 2008, 2013 and 2018 waves; National Survey on Domestic Violence Against Women (NSDVW), 2008 and 2014 waves; authors' calculations.