IZA DP No. 18734

June 2026

Housing Policy and Fertility Behavior in Egypt: Evidence from the 1996 Rental Liberalization

Cecilia D'Agostini
University of Florence

Lucia Ferrone
University of Florence

Gianna Claudia Giannelli
University of Florence
and IZA@LISER

IZA DP No. 18734 June 2026

Housing Policy and Fertility Behavior in Egypt: Evidence from the 1996 Rental Liberalization*

Abstract

Egypt's demographic trajectory has shifted markedly over recent decades, with fertility declines in the 1980s and 1990s followed by a reversal that peaked at 3.5 children per woman in 2014. While previous research has examined determinants such as women's employment, contraceptive use, and migration, the role of housing policy remains largely unexplored. This study investigates the impact of Egypt's 1996 rental market liberalization on fertility. Using four waves of the Egypt Labor Market Panel Survey (ELMPS) combined with district-level housing data from the 2006 Census, we exploit geographic and temporal variation in exposure to the reform. We examine whether women of marriageable age in 1996 living in districts with greater availability of liberalized rental units experienced different fertility outcomes. Results show that exposure to the reform increased the likelihood of childbearing and positively affected other fertility measures, including age at first birth and parity at ages 25, 30, and 35. Evidence suggests that earlier marriage is the main mechanism, consistent with the reform lowering housing constraints for young couples.

JEL classification

J13, J16, O18, R21

Keywords

Egypt, fertility, housing, liberalization

Corresponding author

Gianna Claudia Giannelli
giannaclaudia.giannelli@unifi.it

* We are grateful to Ibrahim Ezzeldin, Leonardo Menchini, and Luca Tiberti for their useful comments and suggestions.

1 Introduction

This research examines the nexus between housing policy and fertility behavior in Egypt. Egypt provides a compelling case study due to a persistent, decades-long housing shortage, particularly within urban centers. Crucially, this shortage was not a product of physical scarcity, but rather a structural misalignment; a significant portion of the housing stock remained vacant as landlords withdrew properties from the rental market in response to unfavorable regulations (World Bank, 2007). This supply-demand mismatch stemmed from a combination of institutional frictions, including rigid rent controls dating back to the 1960s, the absence of functional credit markets, and prohibitively high entry costs (Assaad et al., 2017). These distortions incentivized an oversupply of luxury real estate for the elite, while forcing low- and middle-income households into the informal sector (World Bank, 2007). A critical demographic consequence of this market failure was the systematic delay of marriage (McCall, 1988b). In a context where the groom is culturally mandated to provide a house for the new couple, these escalated costs and search difficulties necessitated prolonged periods of capital accumulation, forcing couples to postpone marriage (Ramadan and Assaad, 2008). A pivotal shift in the Egyptian housing market occurred with the enactment of Law 4 of 1996, commonly referred to as the 'New Rent Law.' This legislation effectively liberalized the rental sector by allowing new contracts to be negotiated at market rates and granting landlords the right to non-renewal—features strictly prohibited under the previous rent-control regime. Although the liberalization applied exclusively to contracts signed after January 1996, it introduced a newfound dynamism into the market. A key demographic consequence of this reform was a shift in marriage timing; recent evidence suggests that the resulting increase in housing availability allowed couples to marry at a significantly earlier age (Assaad et al., 2021). By easing the primary structural barrier to family formation, this 'marriage anticipation' may have accelerated subsequent life-cycle events, like childbearing. This study seeks to evaluate whether—and to what extent—fertility behavior is associated to this rental liberalization.

To investigate this relationship, we use individual retrospective data from four waves of the Egypt Labor Market Panel Survey (ELMPS), combined with district-level information on the share of rental units governed by the New Rental Law, derived from the 1996 and 2006 Censuses. We employ a difference-in-differences approach to estimate the association between the district-level prevalence of new rental units and various fertility outcomes. Our results indicate that women exposed to the law—specifically, those aged at least 21 at the time of its introduction and residing in districts with a higher share of new rental contracts—exhibit higher fertility outcomes. These outcomes include a greater probability of giving birth in a given year, lower age at the first birth, and higher parity at ages between 25 and 35 years old. To address potential endogeneity in the spatial distribution of rental units, we implement a series of robustness checks accounting for urbanization patterns, migration, and different definitions of treatment cohort. Mechanism analysis suggests that the primary channel operates through earlier marriage, consistent with the findings of Assaad et al. (2021). These findings also carry important policy implications. They highlight the extent to which housing market policies

can shape demographic behaviors, suggesting that interventions designed to improve housing access may have unintended effects on fertility. To mitigate such outcomes, housing policies should be complemented by greater investment in family planning initiatives and educational programs that promote more equitable gender roles. These results also highlight the resilience of traditional fertility norms and their role in demographic transition, underlining the importance of considering culture and social norms when designing public policies.

This study offers a new perspective on the institutional factors influencing reproductive behavior and it situates itself at the intersection of two strands of literature. First, it contributes to the body of work on the determinants of fertility stalls. Fertility stalls, defined as interruptions of declining fertility trends, have been documented in several developing countries undergoing the demographic transition, and are particularly informative for understanding fertility behavior associated with policy changes. Fertility stalls in sub-Saharan Africa were first documented in Ghana and Kenya in the early 2000s (Bongaarts, 2006; Bongaarts and Casterline, 2013), and were later systematically identified across many other countries in the region (Schoumaker, 2019). In this literature, fertility outcomes are determined by proximate factors, particularly contraceptive use and marriage patterns. In Egypt, following a steady decline from the 1970s, the total fertility rate (TFR) plateaued in the early 2000s before reversing, rising from 3.0 in 2008 to 3.5 by 2014 (Zalak and Goujon, 2017). Various factors have been proposed to explain this reversal. Eltigani (2003) suggests that the shifting reproductive behavior of middle- and high-income households played a central role in halting the decline. Exploring the labor market, Krafft (2020) shows that this trend reflects a genuine increase in fertility rather than a mere shift in population structure, noting that the contraction of female public-sector employment is associated with rising fertility—though likely not as the sole driver. Other research has highlighted the role of cultural remittances; Bertoli and Marchetta (2015) find that return migration from traditional oil-producing countries is correlated with higher fertility outcomes. More recently, the socio-political climate has been examined: Ferhat et al. (2022) find that women in rural areas heavily affected by the Arab Spring tended to marry and begin childbearing earlier. Finally, Ali and Gurmu (2018), exploiting the exogeneity of a school policy that reduced the number of years of elementary school, find that education decreases fertility by delaying marriage. Our research complements these findings by examining the housing market as a critical, yet under-explored, structural factor.

Second, our analysis contributes to the literature on how housing market structures shape gendered outcomes, particularly fertility, by illustrating how institutional reforms interact with individual life-cycle decisions. Recent empirical studies provide consistent evidence of the link between housing affordability and reproductive behavior. The negative relationship between housing costs and fertility is well-documented in developed economies (see Li, 2024 for multiple countries; Clark (2012) and Dettling and Kearney (2014) for the United States, and Flynn (2017) for Europe). Although the literature on developing contexts remains more limited, recent evidence from markets such as China (Liu et al., 2020) suggests that similar price-fertility trade-offs are an increasingly global phenomenon. Van Doornik et al. (2024) identify a positive

causal effect of improved housing access on fertility in Brazil, with the most pronounced impacts observed among low-income households in disadvantaged neighborhoods. This is complemented by evidence from South Africa, where rising rental prices—and the subsequent decrease in affordability—resulted in significant declines in fertility rates (Simo-Kengne and Bonga-Bonga, 2020).

The remainder of this paper is organised as follows. Section 2 introduces the socio-cultural context and housing conditions in Egypt and presents the conceptual framework. Section 3 describes the data and outlines the empirical strategy. Section 4 reports the results, including robustness checks and an analysis of the transmission channels. Finally, Section 5 concludes with policy implications and directions for future research.

2 Context and Conceptual Framework

Egyptian context

To understand how fertility and the rent liberalisation policy may interact, it is necessary to spend a few words on the marriage customs and the housing market in Egypt.

Marriage remains a near-universal institution in Egypt; women have a nearly 100% probability of marrying at least once by age 50 (Salem, 2015). This universality is sustained by deeply rooted social norms that define the structure of Egyptian society. First, marriage provides the only socially and legally sanctioned context for sexual relations and, consequently, family formation (Hoodfar, 1997). Second, it serves as the primary institutional framework through which women access financial support and social protection, which are often socially guaranteed only within their roles as wives and mothers (Sholkamy, 2008). Finally, marriage is the fundamental marker of the transition to adulthood; it enables young couples to establish residential independence and form autonomous household units (Assaad and Krafft, 2014).
In this context, marriage is a formal contract that entails significantly more than the commencement of a new family. The entry into marriage is a socially and legally mediated process; Egyptian women generally require the permission of a male guardian (wali), typically the father, who represents the woman's interests during the contractual negotiations (Hendy, 2024). These negotiations determine various aspects of marital life and the legal rights of both parties. A fundamental element of this contract is the dower (mahr), a mandatory payment from the groom to the bride. The dower consists of two components: the prompt dower (muqaddam), paid at the time of marriage to provide the bride with a degree of initial financial autonomy, and the deferred dower (mu'akhar), which is payable upon divorce or the husband's death, intended to serve as a financial deterrent against arbitrary divorce by the husband (Hoodfar, 1997). The legal landscape regarding the dissolution of these contracts shifted significantly in 2000 with the introduction of Khul' (unilateral divorce for women). Prior to this reform, wife-initiated divorce was restricted to specific "fault-based" grounds, such as the husband's failure to provide

maintenance, which were often difficult to prove in court (Welchman, 2007). The introduction of Khul' has been associated with increased female bargaining power within the household, a reduction in domestic violence, and improved educational outcomes for children (Corradini and Buccione, 2023). Beyond divorce, marital negotiations cover the woman's right to employment after marriage or childbirth, the level of financial maintenance provided by the husband, the division of household contributions (such as furniture and appliances), and provisions regarding polygyny, which remains legal for up to four wives (Hoodfar, 1997).
The financial burden of these customs falls disproportionately on the groom and his family, as they are traditionally responsible for providing the marital home (El Feki, 2013). To mitigate these rising costs, families often resort to kin marriage, as lower dowries and reduced transaction costs are generally expected when marrying a relative. However, the most prevalent consequence of high marriage costs has been the systematic postponement of marriage. This delay allows the groom time to accumulate the necessary funds—often through labor migration to oil-producing states—to afford a residence (Singerman, 2007). Consequently, the difficulty of securing affordable housing is frequently cited by engaged couples as the primary obstacle to finalizing their marriage (Hendy, 2024).

This lack of housing availability was the main reason for the introduction of rental liberalization. The New Rent Law was, however, enacted as part of a broader suite of structural adjustment programs, which included the privatization of state-owned enterprises, trade liberalization, and wide-reaching economic deregulation (Yousef, 2004). Before 1996, the rental market was governed by the "Old Rent Law", a series of successive legislative acts dating back to the first post-war period. These regulations froze rental rates at fixed, historically low levels, prohibited tenants' eviction and permitted the intergenerational inheritance of tenancy contracts (McCall, 1988a). McCall (1988b) identifies several market distortions resulting from these controls, most notably a sharp decline in private investment in rental housing and the rise of "key money", an illegal, informal upfront payment demanded by landlords to grant access to a contract. These frictions caused an expansion of the informal rental market and created significant horizontal and vertical inequalities among residents. The liberalization introduced by the New Rent Law aimed to mitigate these market failures by restoring price signals and property rights. Table 1 summarizes the main distinctions between the two rental regimes highlighting the changes introduced in 1996. In the decade following the reform, Shawkat (2018) reports a significant surge in the availability of rental units (1996–2006). However, this trend was not sustained over the long term; between 2006 and 2017, the rental sector contracted by 17% as the market shifted toward homeownership. Despite these formal legislative shifts, informal housing remains a defining feature of the Egyptian landscape, particularly in urban centers. Emerging in the mid-1960s, informal settlements expanded rapidly during the 1970s when formal urban development stalled due to regional conflicts. Arandel and El Batran (1997) describe informal urbanisation as the result of rapid and uneven urban development and high population densities combined with the inability of the formal sector to provide land or housing as needed by the urban population. By 1996, informal housing accommodated approximately 62% of the population in Greater Cairo—an agglomeration comprising the urban districts of

the Cairo, Giza, and Qalyubia governorates (Sims, 2012).

Table 1: *Comparison of Rental Law Provisions in Egypt*

Policy	Old Rent	New Rent (Law 4/1996)
Duration	*Indefinite:* Contracts were essentially perpetual.	*Definite:* Fixed-term contracts (e.g., 1–5 years).
Renewal	Automatic; renters had a legal right to remain.	At the landlord's discretion; no automatic right to renew.
Eviction	Nearly impossible; required extreme legal circumstances.	Permitted upon contract expiration without court proceedings.
Rent Setting	*Fixed*: Set by government formulas based on construction costs.	*Market-based*: Determined by supply and demand; subject to change upon renewal.
Applicability	Contracts signed before 1996 (still holds for those units).	All new contracts and buildings from 1996 onwards.

Source: Adapted from Assaad et al. (2021), McCall (1988a).

Conceptual Framework

The New Rent Law has created a dual system in the Egyptian rent market, causing two main effects: i) the increase in housing supply, since many landlords have decided to put their properties on the market given the potentiality of higher revenues; ii) a sharp increase in rent fees for new contracts, due to the liberalization of the rent market. We hypothesize that the 1996 New Rent Law influenced fertility through three primary, and potentially countervailing, channels. The first channel operates through the timing of marriage. Under the legacy rent-control regime, the chronic physical scarcity of available units acted as a structural barrier to family formation; young men, particularly those with precarious employment prospects, were often unable to secure the required marital home (Krafft and Assaad, 2020). By transitioning the housing market from a state of rationing to one of availability, the New Rent Law has been shown to reduce the age at first marriage (Assaad et al., 2021). This shift in marital timing can influence fertility through two distinct mechanisms: (i) a biological effect, where earlier marriage extends the total reproductive window spent within a union, and (ii) a "tempo effect," where shifts in the timing of marriage trigger corresponding changes in the timing of first births, creating a temporary fluctuation in period fertility rates (Bongaarts, 2006). While tempo effects may not necessarily alter completed fertility, they carry significant socioeconomic consequences in the Egyptian context. Given that many Egyptian women exit the labor force upon the birth of their first child, earlier childbearing may result in an accelerated departure from the job market, potentially diminishing long-term financial autonomy and intra-household bargaining power (Hendy, 2015).

Secondly, a psychological and income-based mechanism may operate. Following the East-

erlin hypothesis (Easterlin, 1975), fertility is not merely a function of absolute income, but of a couple's relative economic standing compared to their own upbringing. The chronic housing shortage of the old regime often forced "waithood" or lower-quality cohabitation, depressing the perceived status of young cohorts. Greater housing availability and the ability to secure residential independence allow newly formed households to meet critical status markers more effectively than their predecessors. This improvement in the relative ease of establishing an independent household raises aspirations and perceived resource stability, conditions that typically drive higher fertility rates. This theoretical channel aligns with empirical evidence from Choudhury (1977), who finds that relative income significantly influences the spacing of births rather than cumulative fertility, particularly among couples with high levels of education and professional status. In this light, the easing of housing constraints may not only facilitate the entry into marriage but also accelerate the subsequent pace of family formation by improving the perceived economic security of the household.

Finally, as mentioned in the introduction, a classical price effect may exert downward pressure on fertility. Unlike the "Old Rent" (pre-1996) regime—characterized by nominally low, fixed contracts—the liberalization of the rental market resulted in a sharp convergence toward market-clearing prices. For newly formed households, these heightened housing outlays represent a substantial burden on the disposable budget, potentially crowding out the expenditures required for child-rearing. Within this framework, the increased cost of the "housing-childbearing" bundle may induce couples to postpone or limit fertility as they adjust to tighter budgetary constraints.

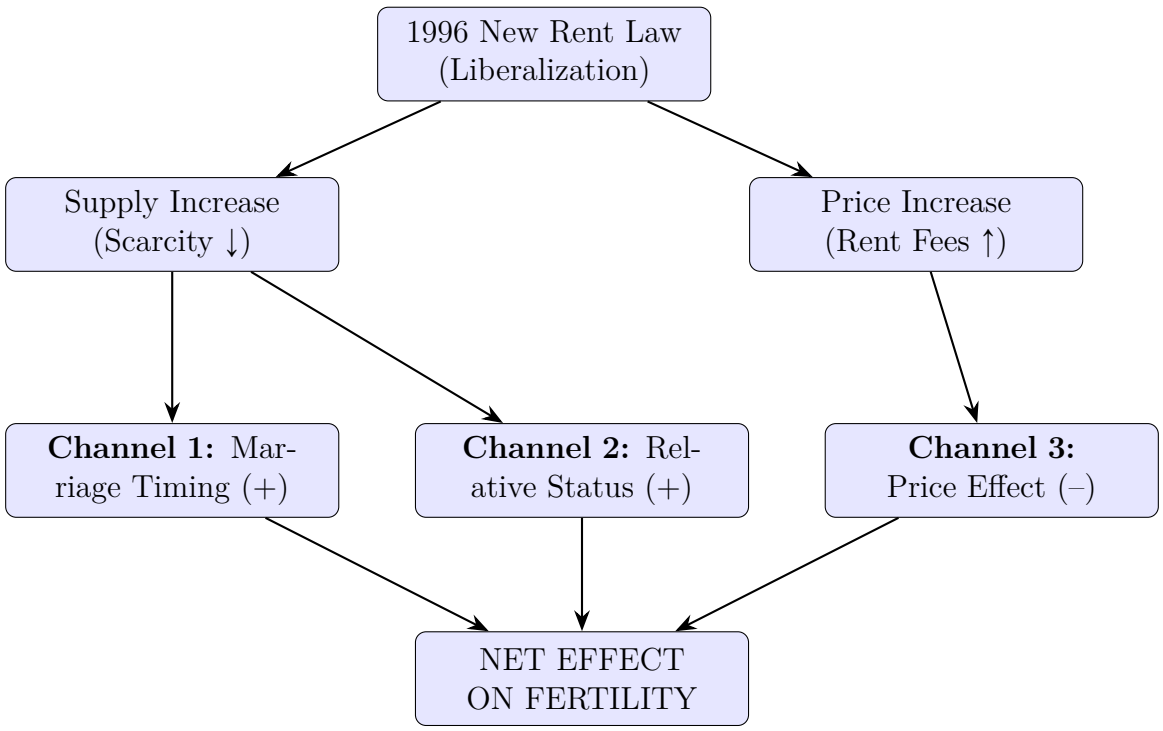


Figure 1: *Conceptual Framework of the New Rent Law's Impact on Fertility*

The net impact of the New Rent Law on fertility is ultimately an empirical question. It represents a trade-off between the positive supply-side shocks (Timing and Relative Status)

and the negative demand-side shock (Price Effect).

3 Data and Methodology

Data

Our analysis draws on individual characteristics and fertility outcomes from four waves of the Egypt Labor Market Panel Survey (ELMPS), spanning 2006 to 2023. These data were collected by the Economic Research Forum (ERF) in collaboration with the Central Agency for Public Mobilization and Statistics (CAPMAS). We utilize the 2006, 2012, 2018, and 2023 waves (OAMDI, 2016, 2020, 2024a,b) to maximize sample size and ensure the representativeness of various birth cohorts. Although the ELMPS is longitudinal, we prioritize the most recent observation for each respondent to capture the most complete retrospective history while avoiding duplication. We then restructure these data into a woman-year panel dataset, tracking individuals through their reproductive lifespan—typically ages 15 to 49, though we include pregnancies occurring as early as age 10 to minimize selection bias. These individual-level data are then merged with district-level data from the 10 per cent public-use samples of the 1996 Egyptian Population and Housing Census (used for baseline district characteristics) and the 2006 census, accessed through the Integrated Public Use Microdata Series (Minnesota Population Center, 2020). The census provides detailed information on dwelling ownership and rental type, distinguishing between units under the old and new rent laws.

Fertility variables. Our main outcome of interest is $gave\ birth_{i,d,t}$, a binary variable equal to 1 if woman i, living in district d, gave birth in year t. Given that the 1996 reform is hypothesized to influence fertility primarily through the channel of earlier marriage, our identification strategy follows Assaad et al. (2021) in defining the treated cohort based on the timing of entry into the marriage market. Specifically, we classify women who reached marriageable age in 1998—two years after the law's promulgation—as treated. Accounting for an average age at first marriage of approximately 21 in 1998, this treatment definition encompasses women born in 1977 or later. The analysis is restricted to the 1985–2010 period to capture the most relevant window of impact. The sample is capped at 2010 to ensure that estimates are not confounded by the socio-political instability and demographic shifts following the Arab Spring uprising in early 2011. On the lower bound, the sample begins in 1985 to establish a symmetrical 13-year window prior to 1998, facilitating a balanced comparison between the pre- and post-law periods. We integrate our analysis with other measures of fertility for a better comprehension of the change in behavior of family formation. These additional outcomes allow us to disentangle whether the observed effects represent a net increase in fertility or purely an anticipation of childbirth (tempo effects).

District-level rent variables. To capture housing availability resulting from the New Rent Law, we follow the methodology proposed by Assaad et al. (2021) and construct a variable

measuring the share of rental units subject to the new law relative to all rental units in a woman's district of residence. In the analysis, we also control for the overall share of rental units at the district level, in order to disentangle the effect of general rental availability from the specific impact attributable to the introduction of the New Rent Law. The share of rental units under the new law is set to zero for all years prior to 1998 and is held constant at its 2006 Census level for all subsequent years. In contrast, the total share of rental units is assigned its 1996 Census value for observations before 1998 and its 2006 Census value for all years thereafter. We acknowledge that the share of rentals under the new law in 2006 may not perfectly reflect housing availability immediately following the law's introduction; however, the 2006 Census remains the only source with this level of disaggregated housing information. We also include a set of other district-level variables for the year 1996 so that we can control for other characteristics of the districts at the moment of the law implementation. In particular, we use the population density, female average education level, female employment rate, number of cars per household and average rent, all at the district level for the year 1996.

Descriptive Statistics

Table A1 in the Appendix presents the weighted descriptive statistics disaggregated by treatment cohort. The analytical sample comprises 101,579 treated woman-year observations and 91,445 observations in the control group. By construction, the treated cohort is significantly younger; however, the two groups also exhibit divergence across several other demographic dimensions. Regarding fertility outcomes, while the treated cohort shows lower cumulative fertility values, they exhibit a significantly younger age at first birth. Socioeconomic trends are also evident: treated women show higher educational attainment and originate from more educated households, consistent with the secular trend of increasing education in Egypt. Furthermore, the treated group is more likely to establish independent nuclear households post-marriage and shows a lower propensity for migration. Finally, analysis of district-level baseline variables reveals that while differences are statistically significant, their economic magnitude is negligible. This suggests that the baseline characteristics of the districts were substantively similar in 1996, providing a comparable foundation for the econometric analysis.

To visualize the spatial distribution of rental units subject to the new law, we provide maps (Fig. 2) at both the national level (left) and for Greater Cairo (right). The national map reveals significant heterogeneity in the adoption of liberalized contracts across districts. However, a distinct spatial clustering is evident in Greater Cairo, where central urban areas exhibit a notably lower exposure to new rental units compared to the rest of the country.
Several factors likely contribute to this Cairo-specific pattern. First, because the share of new rent is calculated relative to the total rental stock, the results are subject to a "dilution effect" in older urban centres. As Cairo was the primary epicentre of Egypt's urban expansion between the 1950s and 1990s, a massive proportion of its formal housing stock remains locked in "Old Rent" contracts, making the share of total rent particularly high in these districts (see Fig. A1

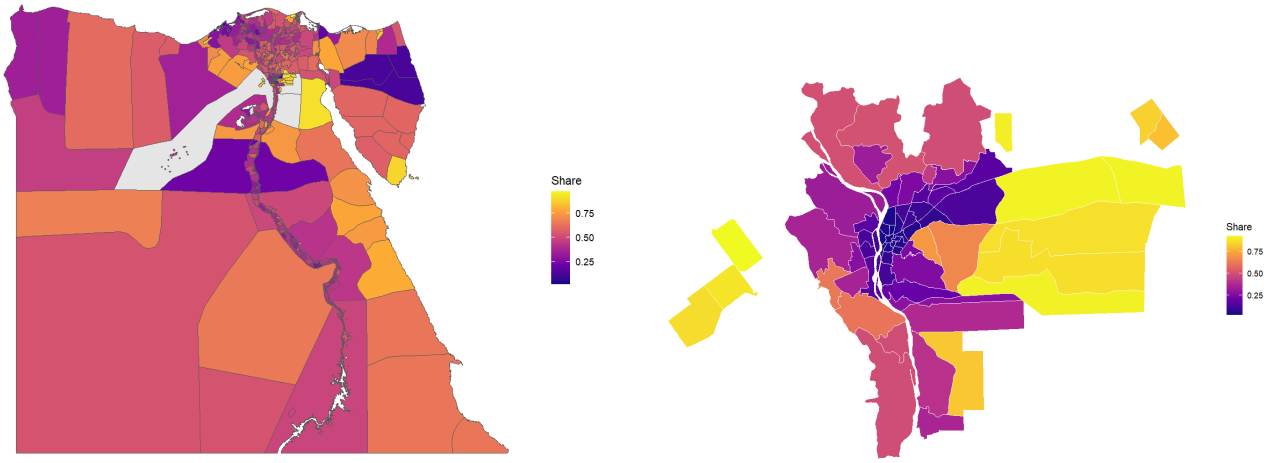


Figure 2: *Distribution of rents under the new law: National overview (left) and Greater Cairo focus (right).*

Source: Authors' elaboration using data from 2006 Census.

in the Appendix). The high density of these rent contracts mechanically dilutes the share of new rental contracts on the total rental market. Second, the prevalence of informality in the capital's housing market complicates tenure reporting. While Sims (2012) notes that liberalized rent is the most frequent tenure type within informal markets, these contracts are seldom registered. Both tenants and landlords in informal units often under-report these arrangements to evade administrative controls and tax liabilities. Finally, the socio-economic leading position of Greater Cairo distinguishes it from the rest of the nation. The region hosted approximately 22% of the national population, 43% of all public sector jobs, and 57% of national manufacturing production in 2006 (Sims, 2012). This unique economic concentration suggests that Greater Cairo operates under a different set of market dynamics than rural or secondary urban districts. As we will discuss in the methodology section, these distinct spatial and structural patterns necessitate a robust empirical strategy to address potential endogeneity.

Empirical Strategy

Following Assaad et al. (2021), we use a difference-in-differences approach to estimate the relationship between the introduction of the New Rent Law and fertility. The linear probability model is specified as follows:

$$\begin{aligned} \mathrm{y}_{i,t,d} = \beta_0 &+ \beta_1 Share\_NewRent_{d,t} + \beta_2 \mathrm{Treated}_i + \beta_3 \big(Share\_NewRent_{d,t} \times \mathrm{Treated}_i\big) \\ &+ \beta_4 Share\_Rented_{d,t} + \beta_5 \big(Share\_Rented_{d,t} \times \mathrm{Treated}_i\big) \\ &+ \gamma X_{i,t} + \delta Z_d * Post\_Law_t + \mu_y + \mu_g + \mu_t + \epsilon_{i,t,d} \end{aligned} \tag{1}$$

where $y_{i,d,t}$ equals 1 if woman i, living in district d, gives birth in year t. The variable $Share\_NewRent_{d,t}$ denotes the share of rental units under the new law in district d and year t, which varies depending on whether t falls before or after 1998, while $Share\_Rented_{d,t}$ represents the share of total rental units in district d in year t. The primary coefficient of interest,

β_3, captures the change in the probability of giving birth in a certain year for a woman born in 1977 or later, who lives in an area with higher new rent housing. This coefficient represents our difference-in-differences estimate, capturing the differential effect of living in districts with higher versus lower shares of new rental units on fertility outcomes across treatment cohorts. The indicator $Treated_i$ captures the cohort effect instead, as we assume that only the women of marriageable age in 1998 are exposed to the policy. The vector $X_{i,t}$ includes individual-level controls, such as age and its square, urban residence, educational attainment, father's and mother's education, cumulative births up to $t-1$, years of marriage, whether the woman has ever migrated, whether she has migrated to a district with a higher share of new rental units and the whether after marriage the couple has started residing alone, with the woman's family or with the spouse's . The vector Z_d captures baseline district-level characteristics measured in 1996, including population density, average rent levels, male and female employment rates, female educational attainment, and the number of vehicles per household. To account for the possibility that districts with different initial conditions might follow divergent fertility trends, we interact these baseline measures with a post-1996 indicator, $Post\_Law_t$. This flexible specification ensures that our results are not confounded by time-varying shocks correlated with the districts' socio-economic profile at the time of the law's enactment. Finally, we include wave μ_y, governorate μ_g and year fixed effects μ_t. In the robustness checks, we also propose a specification using district fixed-effects, which are more restrictive, but allow to control for district-specific trends. Standard errors $\epsilon_{i,d,t}$ are clustered at the district of residence level.

Identification Issues. While our Difference-in-Differences approach provides a robust framework, the validity of our estimates relies on several identification assumptions. Here, we discuss potential threats to our identification strategy—specifically regarding the exogeneity of the treatment intensity and the definition of the treatment cohort—and outline the strategies used to mitigate these concerns. A primary concern is the potential endogeneity of our treatment intensity measure: the district-level share of rental units governed by the New Rent Law. Variation in this share might be correlated with unobserved district-level factors that also influence fertility. For instance, rapid urbanization and population growth might simultaneously drive the supply of new-law rental units and affect reproductive behavior. To disentangle the impact of the New Rent Law from these broader urban trends, we augment the baseline model with a series of interaction terms. Specifically, we interact the treatment cohort indicator with population density in 1996 and 2006, as well as with the population growth recorded between 1996 and 2006. By controlling for these density trajectories, we ensure that the identified fertility response is a structural consequence of housing liberalization rather than a byproduct of general urbanization or regional demographic momentum. Secondly, the definition of the treated cohort itself may be endogenous. Assigning treatment based on the "average age at marriage" is potentially problematic, as the New Rent Law has been shown to directly influence matrimonial timing (Assaad et al., 2021). If the policy delays or accelerates marriage, using marriage age as an assignment variable could bias our estimates. To ensure our results are not driven by this behavioral response, we perform a robustness check using the statutory legal age of marriage (16 years during the sample period) as an exogenous threshold for treatment assignment. By

shifting the focus from an empirical outcome to a legal requirement, we isolate the effect of the housing reform from changes in marriage patterns. A final potential source of endogeneity is residential sorting. We use the district of residence rather than the district of birth, arguing that for a mechanism centered on housing availability, the current residence is the most relevant geographic unit. We are not overly concerned about migration-driven bias for two main reasons. First, internal migration in Egypt has been historically low and declining over the last 50 years (David et al., 2019), limiting the scope for systematic sorting. Second, our main analysis explicitly controls for whether a woman has ever moved and whether she relocated to a district with a higher rental share. Despite these reassurances, we still proceed with additional tests in the robustness section to ensure the stability of our estimates. Specifically, we replicate our analysis by splitting the sample between "movers" and "stayers" and by using the district of birth as an alternative, purely exogenous geographic assignment.

4 Results

Main results

Table 2 presents the results of the baseline specifications. All three columns include the full set of individual-level and district-level controls. The sample of column (1) is the entire country, column (2) comprises only the districts of Greater Cairo, while column (3) is the rest of the country.

Table 2: *Impact of Housing Reform on Fertility (Gave Birth)*

	(1) Full Sample	(2) Greater Cairo	(3) Rest of the Country
Share New Rent	-0.055***	0.023	-0.073***
	(0.017)	(0.043)	(0.018)
Treated Cohort	-0.013**	0.022	-0.008
	(0.006)	(0.029)	(0.006)
Share New Rent × Treated Cohort	0.090***	0.024	0.096***
	(0.013)	(0.041)	(0.012)
Adjusted R^2	0.050	0.051	0.051
Observations (N)	193,024	25,964	167,060
Clusters	275	53	222

Note: Standard errors clustered at district level in parentheses. All specifications include individual-level and district-level controls, governorate FE, wave FE, and year FE. Period of observation: 1985-2010. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The coefficient of the interaction term—the central focus of this study—is positive and statistically significant in both the full sample (Column 1) and the specification excluding Greater Cairo (Column 3). These results suggest that for the treated cohort, the liberalization

of the rental market is positively associated with fertility. In the national and non-Cairo models, the baseline coefficient for the new rental share is negative and significant. This implies that for the control cohort, residing in a district with higher rental liberalization is negatively correlated with the probability of birth, likely reflecting unobserved urban characteristics or the higher cost of living inherent to those areas. Furthermore, the treatment cohort dummy is negative and significant, indicating that women born after 1977 residing in districts with a 'zero-share' of new rentals exhibit a lower baseline fertility compared to the control group. Crucially, the positive and significant interaction term in Columns 1 and 3 demonstrates that the housing reform has effectively counteracted these negative baseline trends. For women in the treated age group, increased access to new rental contracts is significantly associated with a higher probability of childbirth relative to their peers in less liberalized districts. This relationship, however, loses statistical significance within the Greater Cairo subsample (Column 2). As previously discussed, Cairo's districts represent a socioeconomic outlier, characterized by unique demographic pressures and an artificially suppressed share of new rental units. Notably, in this subsample, both the baseline rental share and the treatment cohort dummy also fail to reach significance. The exclusion of Greater Cairo from the analysis further increases the magnitude and precision of the estimated relationship between rental liberalization and the probability of giving birth, confirming that the reform's impact is particularly robust in the rest of the country.

Table A2 in the Appendix reports the average marginal effects of the treatment for varying thresholds of the 'new rental share' to quantify the magnitude of the policy impact. The estimates reveal a distinct non-linear relationship: at low levels of liberalization—specifically when new rental units constitute only 20% of the market—the marginal effect for the treated cohort is positive but statistically insignificant in the full sample (0.005). In contrast, this effect is positive and significant in the sample excluding Greater Cairo (0.012^{***}), while it is negative and significant within Greater Cairo itself (-0.021^{**}), where the mean share of new rental units is much lower than the rest of the country. However, as the share of new rental units increases, we observe a clear increasing pattern. Beyond the 40% threshold, the point estimates for the full sample become positive and grow substantially in magnitude, suggesting that the policy's effectiveness is conditional on the depth of the market transition. In particular, treated women residing in districts where the new rental share ranges from 40% to 75% exhibit a 2.3 to 5.5 percentage point higher probability of giving birth compared to the control group. Notably, the mean district-level share of new rental units is 41.3% in the full sample; our results indicate that the reform's impact becomes positive and significant even below this average. These findings suggest a threshold effect, whereby rental liberalization only begins to significantly offset the baseline fertility decline once a critical mass of housing supply is available in the local market.

Alternative Measures of Fertility

In this section, we expand the analysis using alternative fertility measures to better characterize the nature of the behavioral shift. While our baseline results demonstrate that a higher share of new rental units is associated to a higher annual probability of childbirth for treated women, they do not distinguish between changes in the timing of births (tempo effects) and changes in completed fertility (quantum effects). To address this, we examine the age at first birth and the parity at 25, 30, 35 and 40 years old. The results are presented in Table A3.

Age at first birth. To investigate how the rental reform influenced the transition to motherhood, we implement a discrete-time duration analysis. The sample is restricted to women aged 15 and older, following them until the year of their first birth or the year of the interview, whichever occurs first. Column (1) reports these estimates. The coefficient on the interaction term is positive and statistically significant, suggesting that the liberalization of the rental market accelerated the transition to motherhood. Specifically, treated women in districts with higher rental availability are more likely to have their first child earlier than their counterparts in the control cohorts.

Parity at different ages. A central question of this article is whether the rent policy has only lead to an anticipation of births (tempo effect) or whether it has also driven a genuine increase in total parity (quantum effect). To distinguish between these dynamics, we analyze the relationship between the share of new rental units and the cumulative parity reached at specific age thresholds: 25, 30, 35, and 40 years. For this exercise, the dataset is collapsed to a single observation per woman. To capture exposure to housing market conditions at the time of household formation, we link each individual to the rental share of the district where she resided one year after marriage. The sample is restricted to women who got married between 1985 and 2010, ensuring a timeline that covers both control and treated cohorts. We use parity at ages 35 and 40 as proxies for completed fertility; while reproductive activity may continue beyond these points, these thresholds capture the vast majority of total births while maintaining a representative sample of the treated cohorts. The results in Columns (2)–(5) of Table A3 reveal a compelling empirical pattern. The interaction term is positive and statistically significant for all age thresholds between 25 and 35. Notably, the effect peaks at age 30 (coeff. 1.180***), suggesting that rental availability acts as a crucial catalyst during the primary childbearing years. At the 40-year threshold (Column 5), the interaction remains positive but loses statistical significance. This result is expected due to data censoring: the majority of women in the treated cohorts (born after 1977) had not yet reached age 40 at the time of the survey, reducing the sample size and the statistical power for this specific group. However, the persistence of significant, positive coefficients up to age 35 indicates that the fertility response to rental liberalization may not merely be a temporal shift in childbearing, but it represents a cumulative increase in the number of children for women exposed to a more dynamic housing market.

Taken together, these alternative measures reinforce and extend our primary findings by

demonstrating that the New Rent Law facilitated more than a mere shift in the timing of childbearing. The evidence indicates that the mitigation of housing constraints yielded both a tempo effect—an accelerated transition to the first birth—and a substantial quantum effect, resulting in higher cumulative parity over the life cycle. As shown by the parity levels at the ages of 30 and 35, treated women in more liberalized districts exhibit a significantly higher total number of children compared to the control group. The persistence of these effects across different age thresholds suggests that the reform is associated with a fundamental shift in fertility levels rather than a temporary adjustment in birth spacing or a simple advancement of planned births. By reducing the entry costs into the housing market, the law appears to have permanently expanded the reproductive capacity of treated cohorts, allowing them to achieve larger family sizes within their prime childbearing years.

Sensitivity analysis and robustness checks

In this section, we proceed with some robustness checks to test the validity of our estimates.

Migration history. To address concerns regarding potential endogeneity arising from selective migration, we investigate whether our results are driven by households moving to districts with higher rental availability specifically to facilitate marriage and childbearing. The results of these tests are reported in Table A4. We first disaggregate the sample into "stayers" (those residing in their district of birth) and "movers" in Columns (2) and (3), respectively. The estimates for stayers closely mirror the full-sample baseline presented in Column (1). While the interaction coefficient for movers is slightly smaller in magnitude, it remains statistically significant, suggesting that the policy impact is not confined to a specific subgroup of the population. In Column (4), we further examine the subsample of movers by introducing an interaction between the treatment cohort and a binary indicator for whether the individual moved to a district with a higher share of new rental units. In this specification, the primary interaction term (Treated $\times$ Share New Rent) loses its significance. However, the interaction between the treatment cohort and the "Moved to Higher Rent" indicator is positive and statistically significant. This finding suggests a voluntary residential sorting mechanism: couples may be strategically moving to districts with more liberalized rental markets to secure housing and transition into family formation. Rather than undermining our results, this confirms that the availability of new rental contracts is a decisive factor in life-cycle decisions. Finally, Column (5) provides a more conservative robustness test by using the district of birth as the geographic unit of treatment instead of the district of residence. This approach effectively addresses the potential endogeneity of migration, as the location of birth is exogenous to adult fertility decisions. Even in this specification, the coefficient remains positive and statistically significant (0.088^{***}). Taken together, these analyses alleviate concerns that selective migration or "residential sorting" is biasing our primary findings, confirming that rental liberalization exerts a robust influence on fertility across different geographic and migratory contexts.

Alternative treatment cohort. A potential concern in our baseline specification is that the

average age at marriage might be endogenous to the New Rent Law. As documented by Assaad et al. (2021), rent liberalization is negatively associated with marriage age, suggesting that the reform itself accelerated family formation. To address this potential circularity, we conduct a robustness check by replacing the "average age" threshold with the legal age of marriage, which was 16 at the time of the reform. Under this alternative specification, the treatment is assigned to women born in 1982 or later—individuals who reached the legal marrying age in 1998 or after, ensuring their entire "eligible" matrimonial life occurred under the new regulatory regime. As shown in Column (1) of Table A5, the interaction term $Treated \times Share\_NewRent$ remains positive and highly significant (0.051***). While the magnitude is smaller than in the baseline—consistent with the younger age profile of this subsample—the persistence of the effect confirms that the relationship between rent liberalization and fertility represents a structural response to housing accessibility rather than an artifact of marriage timing endogeneity. In Column (2), we further explore this temporal dimension by interacting the share of new rental units with a continuous variable: age at the time of law implementation (1998). The negative and significant coefficient for this interaction indicates that the older a woman was when the law took effect, the lower the impact of rental liberalization on her annual probability of giving birth. To facilitate the interpretation of this result, Panel B of Table A5 reports the marginal effects of rental share calculated at various age thresholds. The effect is positive and statistically significant for women who were 10 and 15 years old in 1998—cohorts that likely entered the marriage market between 2001 and 2008, when the new rental market was already maturing. For women aged 20 to 25 at the time of the reform, the marginal effect is statistically insignificant, while it becomes negative and significant for those aged 30 and 35. These results do not contradict our primary findings; rather, they highlight that the reform's benefits were concentrated among women who entered the housing market when it was already well-developed. For older cohorts, the lack of a significant effect (or the negative correlation) mirrors the baseline results for the control group, reflecting the fact that their housing and fertility trajectories were largely established under the previous "Old Rent" regime.

Endogeneity of the share. A final concern regarding our empirical specification pertains to the potential endogeneity of the new rental share. Since the intensity of rental liberalization is not randomly assigned across districts, it might be correlated with omitted factors that independently influence fertility. Specifically, urbanization and population density are primary determinants of rental supply; if these factors also shape reproductive choices, our baseline estimates would be confounded. To mitigate this concern, we perform a robustness check by augmenting the baseline model with interactions between the treatment cohort and measures of population density and its growth. Table A6 presents these extended specifications. In column (1), the interactions between the treated cohort and population density (measured in both 1996 and 2006) are negligible in magnitude and statistically insignificant. Crucially, the coefficient for the interaction between the treatment cohort and $Share\_NewRent$ remains positive, significant, and stable in magnitude. Similarly, in column (2), the interaction with population density growth is statistically insignificant, while the primary interaction of interest ($Treat\_cohort \times Share\_NewRent$) maintains its significance and positive sign. The stability

of our estimates after controlling for these alternative demographic drivers provides strong evidence that our findings are not artifacts of broader urbanization trends, but are specifically driven by structural changes in the housing market.

Alternative specifications. To further substantiate the empirical validity of our findings, we subject the baseline results to a battery of robustness checks focusing on alternative specifications, as summarized in Table A7. In the first specification, we re-estimate the primary model while incorporating district-level fixed effects to account for time-invariant unobserved heterogeneity at a highly granular geographic level. The persistence of the interaction term's magnitude and statistical significance in Column (1) confirms that our baseline results are not driven by idiosyncratic district-level characteristics or localized omitted variables. Recognizing the binary nature of the dependent variable, we further evaluate the sensitivity of our results to functional form by employing a Poisson Pseudo-Maximum Likelihood (PPML) estimator in Columns (2) and (3). The PPML framework is particularly advantageous in this context, as it remains robust to heteroskedasticity and provides consistent estimates in the presence of binary outcomes where the conditional variance may be misspecified. While the PPML coefficients represent shifts in the log-odds of a birth rather than direct additive changes in probability, the stability of the positive and highly significant interaction term across both governorate and district-level fixed effects underscores the structural resilience of the relationship between housing liberalization and fertility. Finally, we address potential concerns regarding pre-existing fertility trends or secular demographic shifts through a temporal placebo test reported in Column (4). This identification strategy involves a deliberate temporal displacement where we artificially "mimic" the policy structure exactly ten years prior to its actual implementation. Specifically, we anticipate the treated cohort to women born in 1967 or later and shift the reference for rental liberalization to 1988, while restricting the observation window to the 1975–2000 period. This timeframe predates any potential anticipatory or actual effects of the 1996 reform. The resulting interaction coefficient is negligible in magnitude and statistically indistinguishable from zero, providing stronger evidence that the observed fertility response in our primary analysis is likely tied to the 1996 housing reform rather than reflecting long-standing regional trends or broader demographic transitions. Collectively, these alternative specifications reinforce the strength of our main results and suggest that the identified impact of the New Rent Law is robust to a wide array of econometric and identification challenges.

Heterogeneity Analysis and Mechanisms

While data limitations preclude a direct investigation of the specific roles played by rent prices or household income, we employ a heterogeneity analysis to identify the sub-populations most sensitive to the liberalization of the rental market. Specifically, we examine how the relationship between the share of new rental units and the probability of giving birth varies by socio-economic status. To do so, we introduce a triple interaction term involving the treatment cohort, the local share of new rentals, and three distinct measures of household socio-economic well-being: the

wealth index, the woman's years of schooling, and the husband's years of schooling. The results, presented in Table A8, suggest that socio-economic status acts as a significant moderator. In column (1), the triple interaction with the wealth index—a standardized measure of household assets—is negative and statistically significant. This indicates that higher levels of wealth attenuate the relationship between the availability of new rental units and fertility; conversely, the effect is most pronounced among the least wealthy. Similarly, columns (2) and (3) show that both the woman's and her husband's education levels reduce the magnitude of the estimated effect. Taken together, this heterogeneity analysis suggests that the positive fertility response to rental market liberalization is primarily driven by the most resource-constrained households. This finding aligns with the economic intuition regarding credit and liquidity constraints. Prior to the policy, wealthier or more educated households were likely better equipped to navigate the housing market—either by affording the substantial "key money" (lump-sum deposits) required for old-rent contracts or by having the means to purchase property outright. In contrast, the poorest couples, who could neither afford high upfront costs nor accumulate the necessary savings for a down payment, were effectively "locked out" of the marriage and fertility market. Once the 1996 law liberalized the market and lowered these entry barriers, these constrained households exhibited the strongest and most immediate response.

To further substantiate these findings, we investigate the underlying mechanism of marriage timing by examining the association between the prevalence of new rental units and the transition into union formation. While this relationship was previously documented by Assaad et al. (2021) using the 2012 wave of the ELMPS, we extend their framework by constructing a pseudo-survival model that leverages four distinct ELMPS waves, thereby increasing the longitudinal depth and statistical power of the analysis.The sample is structured as a person-period dataset, tracking each woman annually from age 15 until the year of marriage. For women who remained single at the time of the survey, observations are censored at the date of the interview or at age 49. The dependent variable is a binary indicator equal to one in the year of marriage and zero for all preceding years 'at risk'. Our primary coefficient of interest is the interaction between the $Treated\_Cohort$ indicator and the district-level share of new rental units.Table A9 reports the results disaggregated by region to account for spatial heterogeneity. The coefficient on the interaction term is positive and statistically significant across all specifications, indicating that for women born after 1977, residing in a district with a higher share of new rental units significantly increases the annual probability—or hazard—of entry into first marriage. Notably, this effect remains statistically significant within the Greater Cairo subsample, albeit with a smaller magnitude than in the rest of the country. These findings provide robust empirical support for the hypothesis that rental market liberalization facilitates an accelerated transition into marriage, acting as a crucial precursor to the observed increases in fertility. By lowering the barriers to independent housing, the reform effectively offsets the marriage delays traditionally associated with high-cost urban environments.

5 Conclusions

This paper investigated the relationship between rental market liberalization and fertility in Egypt, leveraging the 1996 New Rent Law as a quasi-natural experiment. By comparing the demographic outcomes of women who reached marriageable age before and after the reform, we identify a significant positive association between the availability of new rental units and the probability of childbirth. Our findings show that for women born after 1977, residing in a district with a higher share of new rental contracts is associated with a higher likelihood of giving birth in any given year. Further analysis reveals that the observed relationship is not purely a "tempo" effect, even if there is an anticipation of the first childbirth, but extends to the quantum of fertility. The positive association remains consistent across measures of cumulative fertility—specifically, parity at 25, 30 and 35 years old. Although the treated women have not yet reached the end of their reproductive years—preventing a final calculation of completed fertility— these results suggest that the liberalization of the housing market may have a lasting impact on total parity. These findings proved robust to various specifications, including controls for urbanization trends and migration history, ensuring that the results are not driven by selective residential sorting. The heterogeneity analysis provides a key insight into the mechanism at play: the results are primarily driven by the poorest and least educated households. This suggests that these resource-constrained groups were the most severely marginalized under the previous rent-control regime, which required high upfront "key money" for access. Finally, by confirming that rental availability is correlated with an anticipation of marriage, we substantiate the channel through which housing serves as a primary gatekeeper for both union formation and family expansion in the Egyptian context.

These results offer several critical implications for understanding the intersection of housing policy and demography. First, they underscore the resilience of traditional fertility norms in Egypt. It appears that high fertility rates were previously held back by structural housing scarcity; once the New Rent Law lowered entry barriers to independent residency, fertility behaviors responded upward to align with cultural preferences. This suggests that structural constraints, such as housing availability, can significantly dictate the pace of a country's demographic transition. As argued by Fargues (1997), fertility in the region may be driven more by deep-seated cultural norms and parent preferences than by simple access to reproductive services. Second, our findings highlight a potential "policy paradox." While the 1996 liberalization was a successful market-based solution to a chronic housing shortage, it may have inadvertently created upward pressure on fertility in a country already struggling with rapid population growth. To be effective, urban and housing policies must be complemented by a "socially-attentive lens" that integrates family planning and reproductive health awareness. Such integrated policy design is particularly urgent given that accelerated childbearing in Egypt is frequently correlated with reduced female labor force participation, diminished autonomy, and weakened intra-household bargaining power. Finally, the observation that the most economically vulnerable households are primarily driving this fertility surge points to a potential intensification of the "poverty trap." If rental liberalization facilitates earlier marriage

and higher parity among low-income cohorts, it may exacerbate long-term financial precarity if these households cannot sustain the cumulative costs of child-rearing. Consequently, housing market flexibility must be accompanied by targeted interventions. While housing subsidies are a standard tool for supporting vulnerable families, in the Egyptian context, they must be designed with care to avoid creating a pro-natalist "vicious cycle." To ensure that improved housing access facilitates social mobility rather than demographic pressure, subsidies should be integrated with human capital incentives—such as linking housing support to female education or workforce participation. By shifting the focus from merely "subsidizing a marital home" to "supporting household economic stability," policy can help young families navigate the trade-offs between residential independence and sustainable family planning.

References

Ali, F. R. M. and S. Gurmu (2018). The impact of female education on fertility: a natural experiment from Egypt. *Review of Economics of the Household 16*(3), 681–712.

Arandel, C. and M. El Batran (1997). *The informal housing development process in Egypt*. University College, London, Development Planning Unit Londom, UK.

Assaad, R. and C. Krafft (2014). Youth transitions in Egypt: school, work, and family formation in an era of changing opportunities. Silatech Working Paper 14.

Assaad, R., C. Krafft, and D. J. Rolando (2017). The role of housing markets in the timing of marriage in egypt, jordan, and tunisia. Working Papers 1081, Economic Research Forum.

Assaad, R., C. Krafft, and D. J. Rolando (2021). Evaluating the impact of housing market liberalization on the timing of marriage: Evidence from egypt. *Population studies 75*(3), 343–361.

Bertoli, S. and F. Marchetta (2015). Bringing it all back home–return migration and fertility choices. *World Development 65*, 27–40.

Bongaarts, J. (2006). The causes of stalling fertility transitions. *Studies in family planning 37*(1), 1–16.

Bongaarts, J. and J. Casterline (2013). Fertility transition: is Sub-Saharan Africa different? *Population and development review 38*(Suppl 1), 153.

Choudhury, R. H. (1977). Relative income and fertility. *Demography 14*(2), 179–195.

Clark, W. A. (2012). Do women delay family formation in expensive housing markets? *Demographic research 27*(1), 1.

Corradini, V. and G. Buccione (2023). Unilateral divorce rights, domestic violence and women's agency: Evidence from the Egyptian Khul reform. *Journal of Development Economics 160*, 102947.

David, A., N. El-Mallakh, and J. Wahba (2019). Internal versus International migration in Egypt: Together or Far Apart. Working Paper 1366, Economic Research Forum Working Paper Series.

Dettling, L. J. and M. S. Kearney (2014). House prices and birth rates: The impact of the real estate market on the decision to have a baby. *Journal of Public Economics 110*, 82–100.

Easterlin, R. A. (1975). An economic framework for fertility analysis. *Studies in family planning 6*(3), 54–63.

El Feki, S. (2013). *Sex and the citadel: Intimate life in a changing Arab world*. Random House.

Eltigani, E. E. (2003). Stalled fertility decline in Egypt, why? *Population and Environment 25*(1), 41–59.

Fargues, P. (1997). State policies and the birth rate in egypt: From socialism to liberalism. *Population and Development Review*, 115–138.

Ferhat, S., R. Hotte, and P. Verwimp (2022). Entry into marriage, motherhood and the arab spring: Evidence from egypt. Working Papers ECARES 2022-16, ULB – Universite Libre de Bruxelles.

Flynn, L. (2017). Delayed and depressed: From expensive housing to smaller families. *International Journal of Housing Policy 17*(3), 374–395.

Hendy, R. (2015). Women's participation in the egyptian labor market. *The Egyptian labor market in an era of revolution*, 147–161.

Hendy, R. (2024). The State of Marriage in Egypt. In *The Handbook of Marriage in the Arab World*, pp. 263–305. Springer Nature Singapore.

Hoodfar, H. (1997). *Between marriage and the market: Intimate politics and survival in Cairo*. Univ. of California Press.

Krafft, C. (2020). Why is fertility on the rise in Egypt? The role of women's employment opportunities. *Journal of Population Economics 33*(4), 1173–1218.

Krafft, C. and R. Assaad (2020). Employment's role in enabling and constraining marriage in the Middle East and North Africa. *Demography 57*(6), 2297–2325.

Li, W. (2024). Do surging house prices discourage fertility? Global evidence, 1870–2012. *Labour Economics 90*, 102572.

Liu, J., C. Xing, and Q. Zhang (2020). House price, fertility rates and reproductive intentions. *China Economic Review 62*, 101496.

McCall, B. B. (1988a). The effects of rent control in Egypt: Part I. *Arab Law Quarterly*, 151–166.

McCall, B. B. (1988b). The Effects of Rent Control in Egypt: Part II. *Arab Law Quarterly 3*, 274.

Minnesota Population Center (2020). Integrated Public Use Microdata Series, International: Version 7.3 [Machine-readable database]. Accessed: 2025-10-20.

OAMDI (2016). Egypt - Labor Market Panel Survey (ELMPS 2006). Economic Research Forum (ERF). Version 4.2 of Licensed Data Files.

OAMDI (2020). Egypt - Labor Market Panel Survey (ELMPS 2012). Economic Research Forum (ERF). Version 3.0 of Licensed Data Files.

OAMDI (2024a). Egypt - Labor Market Panel Survey (ELMPS 2018). Central Agency For Public Mobilization Statistics (CAPMAS). Egypt: Economic Research Forum (ERF). Version 3.0 of Licensed Data Files.

OAMDI (2024b). Egypt - Labor Market Panel Survey (ELMPS 2023). Central Agency For Public Mobilization Statistics (CAPMAS). Egypt: Economic Research Forum (ERF). Version 2.0 of Licensed Data Files.

Ramadan, M. and R. Assaad (2008). Did Housing Policy Reforms Curb The Delay In Marriage Among Young Men In Egypt? Working paper, Economics Research Forum (ERF).

Salem, R. (2015). Changes in the institution of marriage in egypt from 1998 to 2012. Working Papers 911, Economic Research Forum.

Schoumaker, B. (2019). Stalls in fertility transitions in sub-saharan africa: Revisiting the evidence. *Studies in family planning 50*(3), 257–278.

Shawkat, Y. (2018). Egypt State of Rent 2017. Technical report, The Built Environment Observatory.

Sholkamy, H. (2008). The Empowerment of Women: Rights and Entitlements in Arab Worlds. In M. Mukhopadhyay and S. Meer (Eds.), *Gender, Rights and Development: A Global Sourcebook*. Amsterdam: Royal Tropical Institute.

Simo-Kengne, B. D. and L. Bonga-Bonga (2020). House prices and fertility in South Africa: A spatial econometric analysis. *Economics Bulletin*, 3193–3210.

Sims, D. (2012). *Understanding Cairo: The logic of a city out of control*. Oxford University Press.

Singerman, D. (2007). The economic imperatives of marriage: Emerging practices and identities among youth in the Middle East. Working Paper 6, Middle East Youth Initiative.

Van Doornik, B., D. Fazio, T. Ramadorai, and J. Skrastins (2024). Housing and fertility. Working Paper 612, Banco Central do Brasil.

Welchman, L. (2007). *Women and Muslim family laws in Arab states: A comparative overview of textual development and advocacy*. Amsterdam University Press.

World Bank (2007). Arab Republic of Egypt: Analysis of Housing Supply Mechanisms, Final Note. Technical report, World Bank. License: CC BY 3.0 IGO.

Yousef, T. M. (2004). Development, growth and policy reform in the Middle East and North Africa since 1950. *Journal of Economic Perspectives 18*(3), 91–116.

Zalak, Z. A. and A. Goujon (2017). Exploring the fertility trend in Egypt. *Demographic Research 37*, 995–1030.

Appendix A: Variable Definitions

I. Fertility Outcomes

Gave birth in year t: Binary variable equal to 1 if woman i gave birth in year t. Constructed from retrospective birth histories. *Source: ELMPS.*

Children within 5, 10, and 15 years: Cumulative count of children born within 5, 10, or 15 years post-marriage. *Source: ELMPS.*

Age at first child: A continuous variable indicating the mother's age (in years) at the time of her first live birth. *Source: ELMPS.*

II. Main Treatment Variables

Treated Cohort: A dummy variable equal to 1 if the woman was born in 1977 or later. *Source: Authors' elaboration from ELMPS.*

New Rental Share: A continuous variable representing the intensity of the reform at the district level. It is calculated as the ratio of housing units under "New Rent" contracts to the total number of rental units in the district. It takes the value 0 for years before 1998. *Source: 2006 Census.*

III. Individual and Household Controls

Years of schooling: The total number of years of formal education completed by the woman at the time of the survey. *Source: ELMPS.*

Urban residence: A dummy variable indicating whether the individual resides in an urban area. *Source: ELMPS*

Marriage duration: The number of years elapsed between the year of first marriage and the observation year t. *Source: ELMPS.*

Cumulative births: Number of births before year t. It captures whether the woman has already given birth and how many times. *Source: ELMPS*

Parents' education: Categorical variable indicating the level of education of the respondent's parents, ranging between 1 and 7, used to control for socio-economic background and intergenerational preferences. *Source: ELMPS.*

Age and Age at marriage: Continuous variables representing the woman's current age and her age at the time of her first marriage. *Source: ELMPS.*

Residence after marriage (Alone, Own family, Spouse's family): A set of mutually exclusive dummy variables indicating the living arrangement immediately following marriage. *Alone* indicates a nuclear household; *Own family* and *Spouse's family* indicate matrilocal or patrilocal extended household arrangements. *Source: ELMPS.*

Ever moved: A binary variable equal to 1 if the woman currently resides in a district different from her place of birth. *Source: ELMPS.*

Moved to higher rent: A binary variable equal to 1 if the woman has moved to a district with a higher share of new rental units with respect to the district of birth. *Source: ELMPS and 2006 Census*

IV. District-Level Controls

Share rent (1996 and 2006): The total share of rental units at the district level, changing if the year of observation is before or after 1996. *Source: 1996 and 2006 Census.*

Population Density (1996): The total number of inhabitants per km^2 in the district in 1996. This variable is used in logarithmic form to control for urbanization and local crowding. *Source: 1996 Census.*

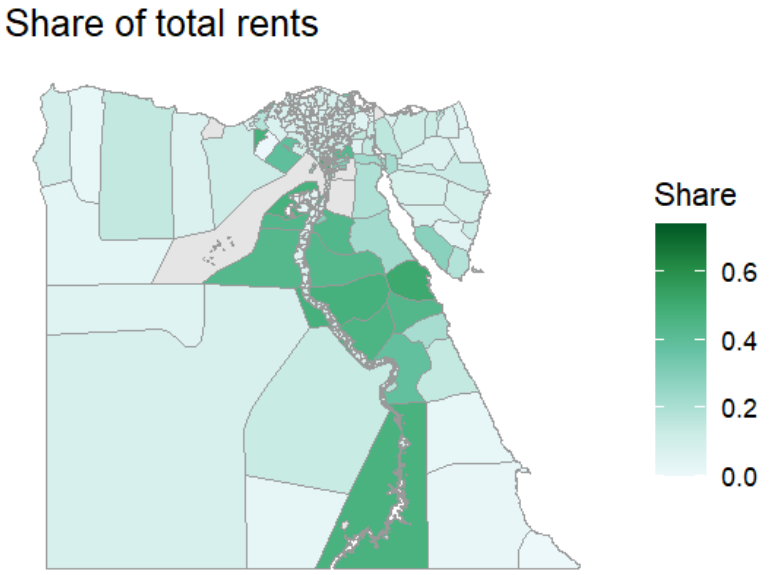
Avg. Monthly Rent (1996): The average district-level monthly rent price recorded prior to the full implementation of the reform. *Source: 1996 Census.*

Employment rates (1996): The share of the male and female population aged 15-64 who were employed at the district level in 1996. *Source: 1996 Census.*

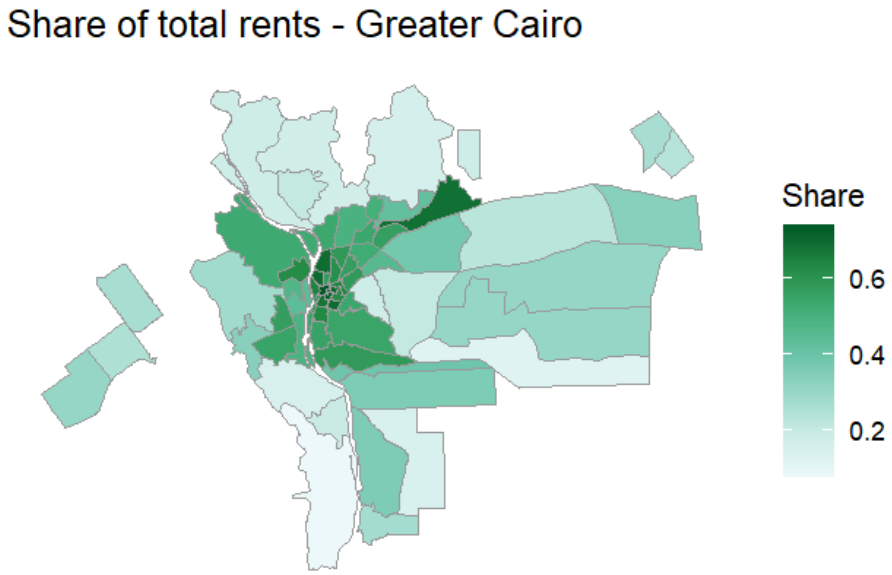
Share owning car (1996): The proportion of households in the district owning at least one private vehicle, serving as a proxy for the baseline wealth and economic development of the area. *Source: 1996 Census.*

Post Law: A binary variable equal to 1 for years equal to or after 1998.

Appendix B: Additional Tables and Figures



(a) National overview



(b) Greater Cairo focus

Figure A1: *Distribution of total rent.*
Source: Authors' elaboration using data from 2006 Census

Table A1: *Descriptive statistics by treatment status*

Variable	(1) Control	(2) Treated	(3) Difference
Fertility Outcomes			
Gave birth in year t	0.126	0.114	-0.012***
	(0.332)	(0.318)	(0.002)
Parity at 25 y.o.	1.304	1.178	-0.126***
	(1.38)	(1.10)	(0.030)
Parity at 30 y.o.	2.165	2.081	-0.084**
	(1.68)	(1.34)	(0.041)
Parity at 35 y.o.	2.746	2.652	-0.094*
	(1.87)	(1.48)	(0.055)
Parity at 40 y.o.	3.041	2.872	-0.168**
	(1.99)	(1.59)	(0.077)
Age at first child	23.698	23.090	-0.607***
	(5.07)	(4.14)	(0.029)
Individual Characteristics			
Years of schooling	7.218	9.345	2.127***
	(6.31)	(5.41)	(0.037)
Rural residence	0.465	0.529	0.064***
	(0.498)	(0.499)	(0.003)
Marriage duration	8.066	1.909	-6.157***
	(7.70)	(3.192)	(0.038)
Cumulative births	1.721	0.490	-1.231
	(1.79)	(0.92)	(0.009)
Mother's education	1.334	1.683	0.349***
	(0.907)	(1.299)	(0.009)
Father's education	1.832	2.190	0.358***
	(1.345)	(1.577)	(0.009)
Age	26.393	20.809	-7.585***
	(7.69)	(4.39)	(0.041)
Age at marriage	21.674	21.700	0.026***
	(5.39)	(4.33)	(0.032)
Residence after marriage:			
Alone	0.655	0.675	0.020***
	(0.476)	(0.468)	(0.003)
Own family	0.062	0.062	-0.0002
	(0.242)	(0.241)	(0.001)
Spouse's family	0.280	0.261	-0.019***
	(0.449)	(0.439)	(0.003)
Ever moved	0.212	0.162	0.050***
	(0.409)	(0.369)	(0.002)
Moved to district with higher share	0.043	0.019	-0.024***
	(0.204)	(0.137)	(0.001)
District level controls			
Share new rent (2006)	0.194	0.348	0.155***
	(0.223)	(0.193)	(0.001)
Share rented (2006)	0.2551	0.219	-0.032***
	(0.238)	(0.216)	(0.002)
Pop. density (1996)	12,478	10,178.52	-2,299.48***
	(20,895)	(18,969)	(207.71)
Avg. monthly rent (1996)	36.938	37.927	0.989***
	(14.09)	(13.08)	(0.088)
Female employment rate (1996)	0.084	0.080	-0.003***
	(0.051)	(0.051)	(0.000)
Male employment rate (1996)	0.529	0.530	0.002***
	(0.036)	(0.038)	(0.000)
Share owning car (1996)	0.051	0.046	-0.005***
	(0.087)	(0.083)	(0.001)
Observations	91,445	101,579	193,024

Notes: Column (1) and (2) report weighted means. Column (3) reports the mean difference between the 2 groups. * p < 0.10, ** p < 0.05, *** p <0.01. Standard devations in parenthesis.

Table A2: *Average Marginal Effects of Treatment at Different Rental Share Thresholds*

Threshold Share New Rent	(1) Full Sample	(2) Greater Cairo	(3) Rest of Country
Threshold (20%)	0.005	-0.021**	0.012***
	(0.004)	(0.008)	(0.004)
Threshold (40%)	0.023***	-0.016	0.031***
	(0.005)	(0.014)	(0.005)
Threshold (60%)	0.041***	-0.012	0.050***
	(0.007)	(0.021)	(0.006)
Threshold (75%)	0.055***	-0.008	0.065***
	(0.009)	(0.027)	(0.008)
Observations (N)	193,024	25,964	167,060
Mean Share New Rent	0.413	0.263	0.433

Note: Table reports the average marginal effects (dy/dx) of being in the treated cohort. Standard errors in parentheses. All specifications include individual and 1996 district-level controls interacted with the post-law dummy. Period of observation: 1985-2010. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: *Impact of Housing Reform on Fertility Timing and Parity at Different Ages*

	(1) Prob. First Birth	(2) Parity at Age 25	(3) Parity at Age 30	(4) Parity at Age 35	(5) Parity at Age 40
Share New Rent	-0.021***	-0.901***	-1.091***	-0.672**	-0.056
	(0.008)	(0.229)	(0.283)	(0.330)	(0.385)
Treated Cohort	-0.006*	1.093***	0.852***	0.701***	0.468***
	(0.003)	(0.086)	(0.114)	(0.132)	(0.144)
Share New Rent ×	0.043***	0.808***	1.180***	0.710**	0.301
Treated Cohort	(0.007)	(0.188)	(0.253)	(0.298)	(0.304)
Adjusted R^2	0.022	0.280	0.238	0.204	0.198
Observations (N)	185,490	8,477	7,823	6,656	4,663
Clusters	276	263	263	259	254

Notes: Column (1) reports the probability of having a first child using the full woman-year panel (linear probability model). Columns (2) to (5) use a collapsed dataset with one observation per woman to measure cumulative parity (total number of children) reached at the specified age. All specifications include the full set of individual and district-level controls. Standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: *Robustness Checks: Migration Dynamics and District of Birth*

Gave Birth	(1) Baseline	(2) Stayers	(3) Movers	(4) Movers (Int.)	(5) Birth District
Share New Rent	-0.055***	-0.062***	-0.040	-0.033	-0.045***
	(0.017)	(0.023)	(0.026)	(0.026)	(0.015)
Treated Cohort	-0.013**	-0.015**	-0.003	-0.005	-0.015**
	(0.006)	(0.007)	(0.014)	(0.014)	(0.006)
Share New Rent ×	0.090***	0.096***	0.063**	0.033	0.088***
Treated Cohort	(0.013)	(0.017)	(0.025)	(0.025)	(0.014)
Moved to higher rent ×				0.072***	
Treated Cohort				(0.013)	
Adjusted R^2	0.050	0.049	0.060	0.061	0.050
Observations (N)	193,024	157,586	35,438	35,438	193,024
Clusters	275	256	273	273	274

Notes: Standard errors clustered at the district level in parentheses. Column (1) reports the baseline results. Column (2) restricts the sample to individuals who never moved from their district of residence. Columns (3) and (4) focus on individuals who moved across districts, with Column (4) interacting the treatment with a dummy for moving to a district with higher rental liberalization. In Column (5), the rental share and interaction terms are calculated based on the individual's district of birth to address potential endogenous migration. All specifications include individual controls, district controls, and fixed effects for governorate, wave, and year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: *Robustness: Alternative Treatment Definitions*

Panel A: Coefficients	(1) Gave Birth	(2) Gave Birth
Share New Rent	-0.016	0.114***
	(0.017)	(0.021)
Treated Cohort (1982+)	-0.005	
	(0.009)	
Share New Rent ×	0.051***	
Treated Cohort	(0.018)	
Age at Law		0.061***
		(0.001)
Share New Rent ×		-0.005***
Age at Law		(0.001)
Adjusted R^2	0.050	0.050
Observations	193,024	193,024
Clusters	275	275
Panel B: Marginal Effects		
At Age 10		0.060***
		(0.016)
At Age 15		0.033**
		(0.015)
At Age 20		0.006
		(0.014)
At Age 25		-0.022
		(0.015)
At Age 30		-0.049***
		(0.017)
At Age 35		-0.076***
		(0.020)

Notes: Panel A reports the OLS coefficients. Column (1) defines the treatment based on the 1982 birth cohort. Column (2) uses the continuous age at the time of the law implementation (1996). Panel B reports the marginal effects of the share of new rental units calculated at different age thresholds based on the estimates from Column (2). Standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: *Robustness Check: Population Density and Growth*

Gave Birth	(1)	(2)
Treated Cohort	-0.059***	-0.011
	(0.017)	(0.013)
Share New Rent ×	0.089***	0.090***
Treated Cohort	(0.013)	(0.013)
ln(Pop. Density 1996)	-0.028	
	(0.100)	
ln(Pop. Density 1996) ×	-0.026	
Treated Cohort	(0.057)	
ln(Pop. Density 2006)	0.032	
	(0.101)	
ln(Pop. Density 2006) ×	0.032	
Treated Cohort	(0.057)	
Pop. Density Growth		0.022
		(0.103)
Pop. Density Growth ×		-0.005
Treated Cohort		(0.061)
Adjusted R^2	0.050	0.050
Observations	193,024	193,024
Clusters	275	275

Notes: All specifications include the full set of individual and district-level controls, including the baseline share of new rental units (non-interacted). Standard errors clustered at the district level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: *Alternative Models*

Gave Birth	(1) OLS	(2) PPML	(3) PPML	(4) Placebo
Share New Rent	-0.042**	-0.487***	-0.433**	0.000
	(0.019)	(0.152)	(0.174)	(0.014)
Treated Cohort	-0.016***	-0.284***	-0.363***	-0.002
	(0.006)	(0.074)	(0.070)	(0.010)
Share New Rent ×	0.097***	0.710***	0.878***	0.004
Treated Cohort	(0.013)	(0.133)	(0.131)	(0.017)
Adj. / Pseudo R^2	0.052	0.075	0.078	0.050
Local FE	District	Gov.	District	Gov
Observations (N)	193,024	193,024	192,869	171,516
Clusters	275	275	263	274

Notes: Column (1) reports OLS estimates (Linear Probability Model) with district FE. Columns (2) and (3) report a Poisson Pseudo-Maximum Likelihood model with governorate and district FE. Column (4) contains the results of the placebo test, where policy was "moved" to 1988, and the period of observation is 1975-2000. All specifications include individual-level controls. Standard errors clustered at the district level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: *Heterogeneous Effects: Wealth and Education*

Gave Birth	(1)	(2)	(3)
Share New Rent	-0.060***	-0.156***	-0.094***
	(0.017)	(0.019)	(0.027)
Treated Cohort	-0.018***	-0.036***	-0.028*
	(0.006)	(0.009)	(0.015)
Share New Rent ×	0.100***	0.207***	0.173***
Treated Cohort	(0.012)	(0.020)	(0.032)
Wealth Index	-0.006		
	(0.004)		
Years of Schooling	-0.002***	-0.004***	-0.002***
	(0.000)	(0.001)	(0.000)
Husband's Schooling			0.001
			(0.001)
DiD × Wealth Index	-0.063***		
	(0.010)		
DiD × Years of Schooling		-0.015***	
		(0.002)	
DiD × Husband's Schooling			-0.006**
			(0.003)
Adjusted R^2	0.051	0.052	0.048
Observations (N)	192,986	193,024	115,957
Clusters	275	275	268

Notes: "DiD" refers to the interaction *Treated Cohort* × *Share New Rent*. Column (1) interacts the DiD term with the household wealth index. Column (2) uses years of schooling, and Column (3) uses the husband's years of schooling. All specifications include the full set of individual and district-level controls. Standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: *Share of New Rental Units and Marriage Probability*

Gave Birth	(1) Full Sample	(2) Cairo	(3) Rest of the country
Share New Rent	-0.166***	-0.124*	-0.175***
	(0.023)	(0.065)	(0.024)
Treated Cohort	-0.013**	0.007	-0.019***
	(0.006)	(0.018)	(0.006)
Treated ×	0.214***	0.132**	0.233***
Share New Rent	(0.021)	(0.061)	(0.020)
Adjusted R^2	0.027	0.033	0.025
Observations	121,875	20,398	101,477
Clusters	275	53	222

Notes: Duration model of the probability of getting married. Standard errors in parentheses are clustered at the district level. All specifications include the full set of individual and district-level controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.