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# Motherhood and Women's Agency in Egypt:

## Evidence from a Staggered Difference-in-Differences Approach

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# Motherhood and Women's Agency in Egypt: Evidence from a Staggered Difference-in-Differences Approach

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**Abstract**

This paper examines the causal effect of first-time motherhood on women's agency in Egypt, a setting where high fertility coexists with low female labor force participation. Using four waves of the Egyptian Labor Market Panel Survey (2006, 2012, 2018, 2023) and a staggered difference-in-differences design, we estimate the impact of a first birth on a composite Women's Agency Index covering household decision-making, financial autonomy, and mobility. We find that first childbirth raises agency by roughly 0.10 index points, with gains concentrated in decision-making and financial autonomy while smaller effects on mobility.

**Keywords:** Agency, Fertility, Decision-making, Gender, Event study, Staggered difference-in-differences

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# 1 Introduction

Women's agency, defined as the ability to make strategic life choices in the face of opposition (Kabeer, 1999; Moghadam, 2003), shapes both individual well-being and broader development outcomes. Agency is not a single trait. It includes decision-making power within the household, freedom of movement, control over money, and the gender norms a woman lives under (Samari, 2015, 2019; Friedrich, 2023). In Egypt, as in much of the MENA region, patriarchal norms tie women's agency closely to fertility and reproductive choices (Friedrich, 2023). The global pattern is reasonably clear: women with more agency tend to have fewer children and greater reproductive autonomy (Upadhyay et al., 2014). The Egyptian pattern is less structured. Samari (2015) shows that agency can both help women control their fertility and, in the same population, push them toward the high fertility their communities expect of them. Whether motherhood expands a woman's authority or contracts it is therefore an empirical question, not a settled one. The setting makes this question urgent. Egypt's total fertility rate rose from 2.8 births per woman in 2008 to a peak of 3.5 in 2014 before easing back to roughly 3.1 by 2018 (Friedrich, 2023). Female labor force participation has stayed at 15 to 18 percent for years, and women carry most of the country's unpaid care work (Assaad and Krafft, 2020; UNDP and UN Women, 2023). Political representation has improved, with women now holding about 28 percent of parliament seats, and the National Strategy for the Empowerment of Egyptian Women 2030 sets ambitious targets. These gains sit uneasily next to the persistent economic constraints. If first birth shifts a woman's bargaining position inside her household, that shift matters for how Egypt's demographic and labor market trajectories interact. Existing research on this question has two limitations. First, most studies measure agency one dimension at a time, looking at decision-making, mobility, or financial autonomy in isolation rather than as parts of a single construct (Samari, 2017; Friedrich, 2023). Second, the empirical work is largely cross-sectional or relies on associative relationships that cannot separate the effect of motherhood from selection into motherhood. We know motherhood and agency move together according to Egyptian data. We do not know which causes which, or under what conditions.

This paper addresses both limitations. We construct a Women's Agency Index (WAI) that combines decision-making, financial autonomy, and mobility into one measure following the UNDP and UN Women (2023) methodology. We then estimate the effect of first childbirth on this index using four waves of the Egyptian Labor Market Panel Survey (ELMPS, 2006 to 2023), applying the Callaway and Sant'Anna (2021) group-time difference-in-differences estimator as our primary specification and the Sun and Abraham (2021) interaction-weighted estimator as a complementary check. Both use never-treated women as the comparison

group.

Three findings stand out. The aggregate effect of first birth on the WAI is positive and persists for roughly a decade, with the index rising by about 0.10 points on average, or 0.34 standard deviations. The aggregate masks a sharp dimensional split: decision-making and financial autonomy rise substantially after first birth, while mobility does not move. The effect is larger for women with below-secondary education and for women in the upper wealth quintiles than for their counterparts, consistent with motherhood serving as an alternative pathway to authority for women whose schooling channel was limited but whose households can mobilize resources to translate that authority into observable autonomy. Read together, these results suggest that motherhood functions as a route to household authority that stays inside the home rather than translating into greater public freedom.

The paper contributes to the literature in three ways. It is the first study to apply a staggered difference-in-differences design to the fertility-agency question in the MENA region, addressing the bias problems that affect conventional two-way fixed effects estimators when treatment timing varies (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). It introduces a unified, multidimensional agency index built for longitudinal analysis. And it documents a heterogeneity pattern by pre-birth education and household wealth that earlier work had not isolated, with implications for how policymakers think about supporting women through the fertility transition. The remainder of the paper is organized as follows. Section 2 reviews the literature on fertility and agency and the methods used to study their relationship. Section 3 describes the data, the construction of the WAI, and the empirical strategy. Section 4 presents the main results, dimensional decomposition, and heterogeneity analysis. Section 5 reports robustness checks. Section 6 concludes.

# 2 Literature Review

The literature on fertility and women's agency has grown substantially over the past two decades, but most of it sits in two regions: South Asia and sub-Saharan Africa. Work on the MENA region, and on Egypt specifically, is thinner, more recent, and split between studies that find motherhood empowering and studies that find it constraining. This section organizes that work around two questions. What does theory predict about how motherhood should affect agency in a patriarchal context? And what has the empirical literature actually shown, with what limitations?

## 2.1 Motherhood as compensating authority

The starting point for thinking about motherhood and agency in patriarchal settings is the observation that women's pathways to household authority are limited. Where labor markets exclude women, the social roles available to women narrow to a small set compared to men, and motherhood is the most socially legitimate of them. Becoming a mother changes a woman's status inside her household in ways that becoming a wife alone does not. She gains a recognized claim over child-related decisions, a stake in long-run household resource allocation, and, in many cases, the social standing that comes with producing the next generation (Kabeer, 1999; Moghadam, 2003; Friedrich, 2023). This logic generates a specific prediction. The agency gain from motherhood should be largest for women whose alternative routes to authority are weakest. A woman with no labor market role, no independent income, and no formal occupational identity has the most to gain from the institutionalized authority that motherhood confers. A woman who already has economic standing has less marginal authority to gain from becoming a mother and more to lose, because motherhood adds caregiving demands without removing market obligations. Friedrich (2023) develops this argument explicitly for Egypt, framing motherhood as a compensating source of authority for women excluded from market-based power. The same framework predicts a "double burden" for employed women, who face the time and energy costs of motherhood without the same authority dividend. The theory also predicts asymmetry across dimensions of agency. Authority granted through the maternal role is authority over the domestic sphere. There is no parallel mechanism by which motherhood should expand a woman's ability to move freely in public space, since motherhood typically increases caregiving responsibilities that anchor women to the home. We should expect motherhood to raise decision-making and financial autonomy more than mobility, even within the same population. These predictions are testable, and they shape how we read the empirical evidence below.

## 2.2 Empirical evidence and its limitations

The Egyptian and MENA evidence on fertility and agency is mixed. Samari (2015, 2017, 2019) finds that women with children have higher decision-making power and greater mobility than women without, though financial autonomy moves in the opposite direction. Yount et al. (2016) confirm that decision-making, mobility, and financial control load on a common agency factor in Egyptian data, supporting a multidimensional treatment of the construct. Friedrich (2023), using three waves of ELMPS, reports that the agency gains from childbirth are larger for rural and less-educated women, which is consistent with the compensating-authority story. Krafft (2018), in Tunisia, finds the opposite pattern: higher

fertility goes with lower autonomy. The discrepancy across studies tracks differences in how agency is measured, which populations are studied, and which empirical strategy is used. Three limitations run through this body of work. First, most studies measure agency one dimension at a time. Decision-making, mobility, and financial autonomy are typically reported as separate outcomes, and conclusions about the overall direction of motherhood's effect depend on which dimension the reader weighs most. The few studies that combine dimensions, such as Yount et al. (2016) and Jayachandran et al. (2021), do so for measurement validation or for short-form survey design rather than for causal analysis. No study in the Egyptian context has tracked a unified, longitudinally consistent agency index across the full ELMPS panel. Second, the empirical strategies used to date cannot separate the effect of motherhood from selection into motherhood. Cross-sectional regressions, logistic models, and even multilevel approaches (Samari, 2015, 2017, 2019) compare mothers to non-mothers without addressing the fact that women who become mothers earlier, later, or not at all differ on traits that also predict agency. Friedrich (2023) advances this by using fixed-effects regression on three ELMPS waves, which removes time-invariant heterogeneity, but a static fixed-effects model still pools all post-birth observations into a single coefficient and cannot trace how the effect of motherhood evolves over time. Recent econometric work has shown that even well-specified two-way fixed effects estimators produce biased coefficients in staggered settings when treatment effects vary across cohorts (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). None of the prior Egyptian studies addresses this. Third, the literature has not systematically tested whether the effect of motherhood depends on a woman's prior economic position, even though the compensating-authority framework predicts exactly such heterogeneity. Friedrich (2023) gestures toward this in the discussion but does not estimate it. The result is a theoretical prediction that has been articulated but not directly tested in the Egyptian data.

This paper addresses the three limitations above. It builds a unified Women's Agency Index from three dimensions that the prior literature has validated as the core of the construct in Egypt (Yount et al., 2016), and it tracks this index across four ELMPS waves spanning 2006 to 2023. It applies the Callaway and Sant'Anna (2021) group-time difference-in-differences estimator as the primary specification, with the Sun and Abraham (2021) interaction-weighted estimator as a complementary check, to recover dynamic, cohort-robust effects of first childbirth on agency, using never-treated women as the comparison group. And it tests the compensating-authority prediction directly by estimating how the effect of motherhood differs by pre-birth employment status. The approach allows us to ask whether the mixed findings in the prior literature reflect a genuine ambiguity in motherhood's effect or a heterogeneity that earlier studies were not designed to detect.

# 3 Data, Descriptive Statistics, and Methodology

## 3.1 Data

The analysis utilizes data from the Egyptian Labor Market Panel Survey (ELMPS) for 2006, 2012, 2018, and 2023. The ELMPS is a nationally representative panel dataset of Egyptian households, collected by the Central Agency for Public Mobilization and Statistics (CAPMAS) and the Economic Research Forum (ERF). The data spans 5 waves (1998, 2006, 2012, 2018, and 2023); this study uses the latter 4 waves as the 1998 wave does not include data on women's fertility and empowerment. The paper focuses on decision-making, access to financial resources, and women's mobility as the dimensions of the WAI, and uses the year of first childbirth as the treatment event.

The analytical sample is built from the full ELMPS panel of women aged 15–49 across the four waves (2006, 2012, 2018, 2023), comprising 46,541 person-wave observations on 15,699 unique women. We then impose three restrictions designed to satisfy the requirements of the staggered event-study design. *First,* we apply internal-consistency restrictions on the fertility variables: any reported first or second birth that would imply an age at birth below 12 or above 50 is set to missing. This step corrects 216 first-birth records and 67 second-birth records, all of which exhibit inconsistencies between reported birth years and respondent ages. *Second,* we drop women whose first birth occurred before the first observed survey wave (first child birth year $\leq 2006$), since the event-study design requires at least one pre-treatment observation per treated unit. This removes 13,721 person-wave observations belonging to 4,401 "always-treated" women. *Third,* we drop person-wave observations with missing values on the Women's Agency Index (WAI), which removes an additional 16,571 observations. The remaining analytical sample contains 16,249 person-wave observations on 7,290 unique women. Table 1 reports the full sample flow.

Treatment cohorts are defined in wave units, aligned with the ELMPS survey structure: $g = 2$ corresponds to a first birth in 2007–2012 (treated by wave 2), $g = 3$ to a first birth in 2013–2018 (treated by wave 3), and $g = 4$ to a first birth in 2019–2023 (treated by wave 4). The remaining women, who have no recorded first birth, form the never-treated comparison group ($g = 0$). When we report "with-covariates" specifications, we include three baseline time-varying controls in the regression-adjustment step: secondary education or above, urban residence, and wealth quintile (1–5). These three variables are chosen because they are defined for all women in the sample (mothers and never-mothers alike) and because the cohort descriptives in Section 3.1 show them to differ across cohorts at baseline.[1]

[1]Because the ELMPS is a longitudinal survey with refresher samples and non-trivial attrition, maintaining a strictly balanced panel over the 17-year period would severely restrict the sample. We therefore use the full

Table 1: Sample Flow

| Step | Person-wave obs. | Unique women |
|---|---|---|
| Raw ELMPS panel (women aged 15–49) | 46,541 | 15,699 |
| After fertility-age validation (12–50)[a] | 46,541 | 15,699 |
| After dropping always-treated (*1st child birth year* $\leq$ 2006)[b] | 32,820 | 11,298 |
| After dropping missing WAI | 16,249 | 7,290 |
| *Final analytical sample, by cohort:* | | |
| $g = 0$ Never-treated (childless) | 2,749 | 1,704 |
| $g = 2$ First birth in 2007–2012 | 6,822 | 2,397 |
| $g = 3$ First birth in 2013–2018 | 5,468 | 2,375 |
| $g = 4$ First birth in 2019–2023 (late-treated) | 1,210 | 814 |
| Balanced sub-panel (women observed in all 4 waves) | 10,280 | 2,570 |

*Notes:* The table traces the sample flow from the raw ELMPS panel to the final analytical sample. The unbalanced panel is used for all main specifications; the balanced sub-panel is reported only for robustness.
[a] The fertility-age validation step sets implausibly-timed birth records to missing (age at birth $< 12$ or $> 50$) without dropping the underlying observations. This step corrected 216 first-birth and 67 second-birth records.
[b] Always-treated women are dropped because the staggered event-study design requires at least one pre-treatment observation per unit, which is not available for women whose first birth predates the panel.

Panel attrition in the ELMPS arises from household moves, refusals, and the refresher sample design (which adds new households in later waves). Importantly, attrition is not systematically correlated with treatment status: among women present in 2006, approximately 65% are re-interviewed in 2012, 58% in 2018, and 52% in 2023, with similar rates for eventual mothers and childless women. We present a formal attrition analysis in the robustness section comparing characteristics of attriters and non-attriters, and show that results are robust to restricting the sample to the balanced sub-panel of 2,570 women observed in all four waves.

## 3.2 Methodology

### 3.2.1 Women's Agency Index

We construct a composite index capturing three dimensions of women's agency: participation in household decision-making, financial autonomy, and mobility (Malhotra et al., 2002; Samari, 2019; Friedrich, 2023). The Women's Agency Index (WAI) follows the UNDP and UN Women (2023) methodology used for the Women's Empowerment Index.

unbalanced panel for our main specifications and report balanced-panel results in the Robustness section.

All binary and ordinal indicators within each dimension are first normalized to take values between 0 and 1. Each dimension is then computed as the unweighted arithmetic mean of its constituent normalized indicators. **Decision-Making** ($I_{DM}$) is built from 9 indicators capturing household decision-making authority: decisions about own clothing purchases, own medical care, daily household purchases, large household purchases, visits to family or friends, cooking, children's clothing, children's schooling, and children's medical care.[2] **Financial Autonomy** ($I_{Fin}$) is based on 2 indicators, whether the woman has access to household money and whether she has personal savings or assets (land, property, gold, etc.). **Mobility** ($I_{Mob}$) is based on 4 indicators reflecting a woman's ability to go to the local market, to a doctor, to visit family, friends, or neighbors, and to take children to the local health center, without requiring permission from a household member.

Two indicators in the decision-making dimension (children's schooling, children's medical care) and one in the mobility dimension (taking children to a health center) are logically conditional on having children. For women who have never had children, these items are coded based on the ELMPS questionnaire routing: the survey asks these questions of all women in the relevant age group regardless of parity, with respondents indicating their hypothetical or general household decision-making authority.[3]

The three dimensions are then combined using the geometric mean:

$$WAI = (I_{DM} \cdot I_{Fin} \cdot I_{Mob})^{1/3}, \tag{1}$$

which penalizes extreme inequality across dimensions: a woman scoring zero on any single dimension receives a WAI of zero regardless of her scores on the other two. To handle exact zeros (which would make the geometric mean undefined), we add a small constant $\epsilon = 0.0001$ to any dimension score equal to zero before computing the log transformation.[4] Following the UNDP cutoffs, we classify the resulting index into four categories: less than 0.600 (low), 0.600–0.699 (lower-middle), 0.700–0.799 (upper-middle), and 0.800 or greater (high women's agency).

[2]The 2006 and 2012 ELMPS include an additional item ("taking children to the local health center") that is not present in later waves. To maintain strict comparability across waves, we use the 9 common indicators in all waves.

[3]In practice, the ELMPS modules on decision-making are administered to all ever-married women and, in later waves, to a broader sample. Childless respondents answer about household decisions in general terms (e.g., "who in this household decides about children's medical visits?"). This coding ensures that the pre-treatment WAI values for the control group reflect genuine household authority rather than structural zeros. Importantly, because we include individual fixed effects in all specifications, any time-invariant differences in the applicability of these items across groups are absorbed. The identifying variation comes from *within-person changes* around the event, not cross-sectional level differences.

[4]We show in the robustness section that results are insensitive to the choice of $\epsilon$ across values ranging from 0.00001 to 0.01.

Internal reliability of the WAI is evaluated using Cronbach's alpha within each dimension. The decision-making dimension shows excellent coherence ($\alpha = 0.93$), and mobility exhibits strong internal consistency ($\alpha = 0.85$). The financial autonomy dimension, being a two-item construct, has a pairwise correlation of $\rho = 0.16$, suggesting the two indicators capture distinct aspects of financial independence. Exploratory factor analysis confirms that decision-making, financial autonomy, and mobility load on a common behavioral agency factor, supporting the theoretical coherence of the three-dimensional WAI.

### 3.2.2 Identification Strategy

The central empirical challenge in estimating the impact of motherhood on women's agency is the staggered nature of the treatment. Because the transition to parenthood occurs at different stages of the life cycle for different women, traditional linear estimators are susceptible to well-documented biases. Specifically, in the presence of heterogeneous treatment effects, the standard Two-Way Fixed Effects (TWFE) model may produce inconsistent estimates by utilizing previously treated units as effective controls for those treated later (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfœuille, 2020).

To ensure the integrity of our causal estimates, we depart from traditional TWFE in favor of two complementary, robust estimators: the Group-Time Average Treatment Effect approach of (Callaway and Sant'Anna, 2021) and the Interaction-Weighted (IW) estimator of Sun and Abraham (2021).

Our primary analysis utilizes the Callaway and Sant'Anna (2021) framework, which identifies the Average Treatment Effect on the Treated ($ATT$) by disaggregating the sample into group-time cells. This approach is particularly well-suited to our unbalanced panel, as it explicitly restricts the comparison group to "never-treated" or "not-yet-treated" units, thereby isolating the treatment effect from the influence of prior treatment shocks.To corroborate these findings, we implement the Sun and Abraham (2021) IW estimator. This method involves a fully-interacted specification that allows for maximum flexibility across treatment cohorts:

$$WAI_{it} = \sum_{e \in \mathcal{E}} \sum_{k \neq -1} \delta_{e,k} \left( \mathbb{1} E_i = e \cdot D_{i,t}^k \right) + \alpha_i + \lambda_t + \varepsilon_{it} \tag{2}$$

where $E_i$ represents the birth cohort and $D_{i,t}^k$ signifies the relative time to treatment. The IW estimator then re-aggregates these cohort-specific coefficients ($\delta_{e,k}$) using weights proportional to the sample share of each cohort. By utilizing both frameworks, we demonstrate that our results are not merely artifacts of a particular weighting scheme, but represent a robust empirical phenomenon.

**Identification and Sensitivity** The validity of our approach rests on the assumption of parallel trends, the premise that, absent childbirth, the agency of mothers and non-mothers would have evolved along identical trajectories. We assess this assumption by inspecting the pre-event "lead" coefficients and performing joint significance tests. Furthermore, we subject our results to the "Honest DiD" sensitivity framework developed by Rambachan and Roth (2023). This allows us to quantify the extent to which our conclusions are robust to potential, unobserved deviations from the parallel trends assumption. Together, these methods provide a foundation for interpreting the subsequent shift in women's agency as a direct consequence of the transition into motherhood.

We define the event-time indicators relative to the year of first childbirth. Given the temporal structure of the ELMPS and our sample restrictions, we identify individual-year leads for $k \in \{-3, -2\}$ and lags for $k \in \{0, 1, 2\}$, with the period immediately preceding treatment ($k = -1$) serving as the omitted reference category. This window allows us to capture the immediate transition into motherhood while maintaining sufficient statistical power across cohorts.

Given the ELMPS consists of four waves spaced approximately 6–7 years apart (2006, 2012, 2018, and 2023), individual women are observed at most four times. Consequently, our event-time coefficients are identified through cross-cohort variation. For instance, the coefficient at $k = 1$ is identified by comparing women who had their first child one year prior to a given survey wave against the control group within that same wave. As is standard in event-study designs applied to spaced panels (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021), adjacent coefficients are identified from distinct cohort-wave combinations rather than consecutive annual observations of the same individual. Our estimates therefore reflect cohort-averaged effects at each relative time interval, rather than individual-level year-over-year dynamics. To account for potential serial correlation in the error term within individuals over time, we cluster all standard errors at the individual level.

## 3.3 Results

### 3.3.1 Descriptive Statistics

The Women's Agency Index (WAI) and its three constituent dimensions are scaled from 0 (no agency) to 1 (full agency). The following subsections trace the evolution of these measures across the four ELMPS waves.

**1. Decision-Making.** The mean of the decision-making dimension rises from approximately 0.48 in 2006 to about 0.78 in 2018 and 2023 (Figure 1). The increase is driven almost

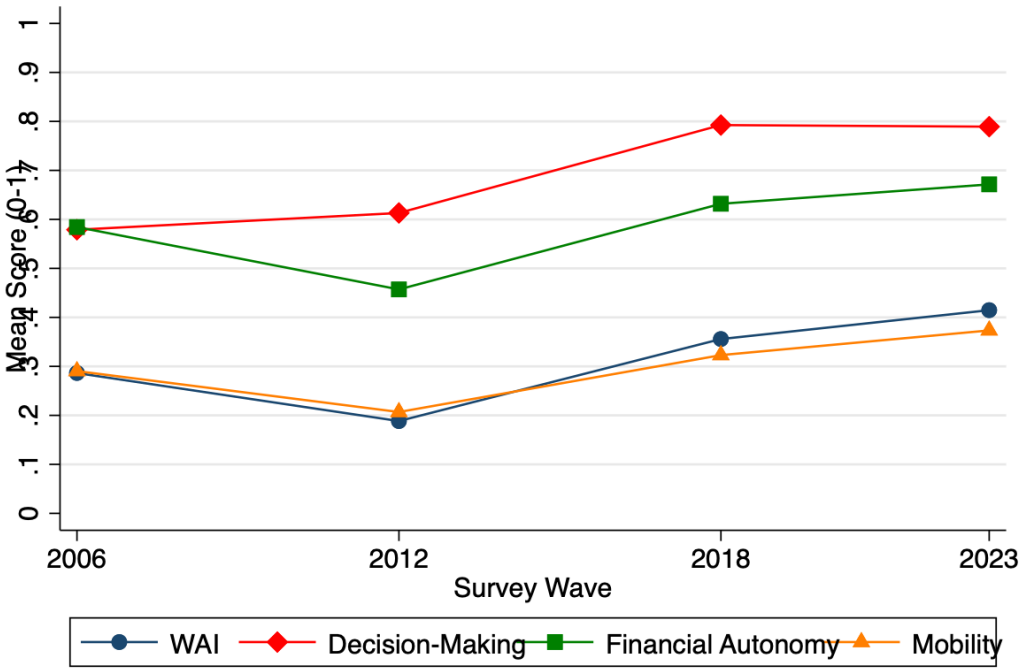
entirely by indicators relating to children's matters; the most striking gain is in mothers' authority over schooling decisions, which rises from 0.2 in 2006 to nearly 0.8 by 2018. The pattern is similar in urban and rural areas, though rural areas catch up more quickly. By marital status, never-married women report the lowest decision-making scores in every wave, while divorced women's scores more than double between 2006 and 2023 (0.3 to 0.7).

**2. Financial Autonomy.** Financial autonomy improves from 0.60 in 2006 to about 0.67 in 2023 (Figure 1). The gain is concentrated in access to household money (from 0.5 to 0.7), partially offset by a decline in personal savings and asset ownership. The urban premium present in 2006 and 2012 has fully closed by 2023, with rural women slightly overtaking their urban counterparts (0.70 vs. 0.68). Widowed women record the highest scores throughout (close to 0.9); never-married women record the lowest, although their scores grow rapidly from 0.07 in 2012 to 0.29 in 2023.

**3. Mobility.** Mobility follows a U-shaped trajectory: 0.30 in 2006, 0.20 in 2012, and 0.40 in 2023 (Figure 1). The largest absolute freedom is the ability to visit the local market, which peaks at 0.45 in 2023. Mobility is consistently higher in urban areas, and widowed women enjoy the greatest freedom of movement while never-married women report the lowest scores.

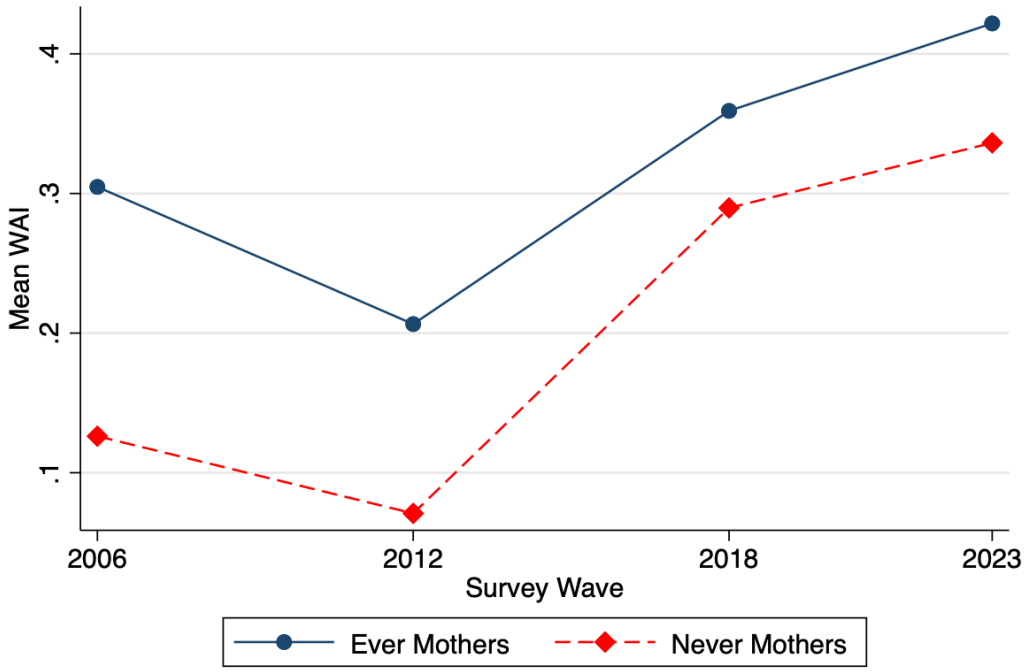
**4. Women's Agency Index (WAI).** The composite WAI displays low overall levels, particularly in 2012 (Figure 1). Regional differences are small. Marital status matters: widowed women record the highest WAI in every wave and never-married women the lowest. Across waves, financial autonomy and decision-making make the largest contributions to the composite; mobility contributes the least.

Figure 1: WAI and Sub-Index Trends by Survey Wave




*Source:* Authors' calculations based on ELMPS 2006–2023.

Figure 2: WAI Trends by Treatment Status




*Source:* Authors' calculations based on ELMPS 2006–2023.

### 3.3.2 Main Results

Table 2 reports the headline ATTs from three complementary estimators: the Callaway and Sant'Anna (2021) difference-in-differences estimator, the Sun and Abraham (2021) interaction-weighted estimator, and a two-way fixed-effects benchmark (TWFE). Each estimator is reported with and without baseline covariates.

**Convergence across estimators.** Our preferred estimate, column (2) of Table 2, indicates that first childbirth raises the WAI by 0.098 index points. The estimate corresponds to approximately one-quarter of the within-sample standard deviation of WAI and is statistically and economically meaningful. Removing covariates (column 1) yields 0.120, a slightly larger estimate that reflects the lifecycle composition of the never-treated group. The Sun and Abraham (2021) estimator returns an average post-treatment effect of 0.117 without covariates and 0.085 with covariates; both are significant at the 1% level (columns 3–4). TWFE produces 0.099 (static binary, column 5) and 0.091 (with covariates, column 6). The convergence of all six estimates into the 0.085–0.120 range, despite different identification strategies and weighting schemes, provides confidence in the headline magnitude.

Table 2: Effect of First Childbirth on the Women's Agency Index (WAI)

| | CS | | IW | | TWFE | |
|---|---|---|---|---|---|---|
| | (1) No cov. | (2) With cov. | (3) No cov. | (4) With cov. | (5) Static | (6) Stat.+cov. |
| ATT (overall) | 0.120*** | 0.098*** | 0.117*** | 0.085*** | 0.099*** | 0.091*** |
| | (0.021) | (0.021) | (0.024) | (0.019) | (0.012) | (0.013) |
| Doubly robust | 0.120*** | 0.098*** | | | | |
| | (0.021) | (0.020) | | | | |
| Covariates | No | Yes | No | Yes | No | Yes |
| $N$ | 6,813 | 6,806 | 14,681 | 14,663 | 14,681 | 14,663 |

*Notes:* Dependent variable: Women's Agency Index (0–1). Sample: 7,290 women (ELMPS 2006–2023), excluding first births before 2007. CS: Callaway and Sant'Anna (2021) with never-treated controls. S&A: Sun and Abraham (2021) IW estimator; ATT is the mean of coefficients $k \in \{0, 1, 2\}$ with $\text{SE}(\bar{\beta}) = \sqrt{(1/9)\mathbf{w}'V_{\text{iw}}\mathbf{w}}$. TWFE: individual and wave fixed effects. Covariates: baseline education, urban residence, and wealth. Standard errors (in parentheses) clustered at the woman level. $^{*}\,p < 0.10$,$^{**}\,p < 0.05$,$^{***}\,p < 0.01$.

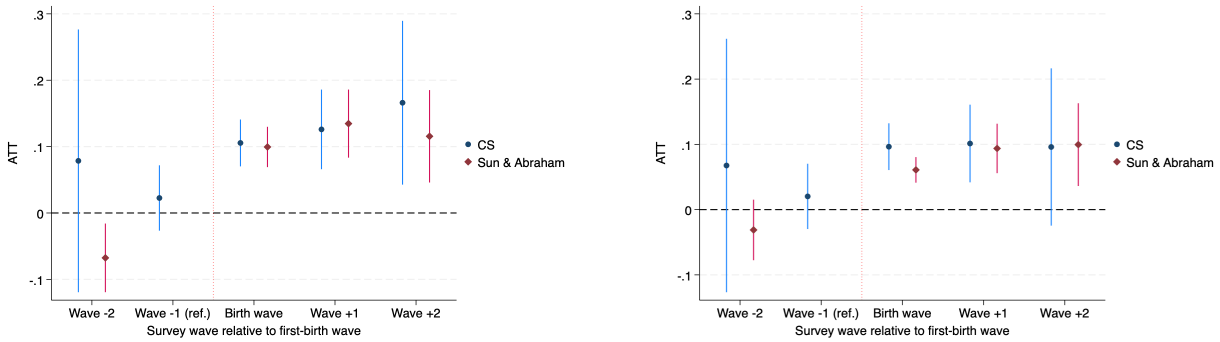
Figure 3 shows the CS and S&A event-study coefficients side by side, with and without baseline covariates. The two estimators track each other closely throughout. At $k = -2$, the pre-treatment coefficient is small and not statistically different from zero in either specification, which is what we'd want to see for parallel trends to hold in the run-up to treatment. Things start to shift at $k = 0$, the year of the first birth, and the effects keep building over the next two periods. Without covariates, the S&A coefficients climb to 0.135 at $k = +1$ and 0.116 at $k = +2$, and CS traces out almost the same path. Adding covariates flattens the trajectory somewhat, but the post-treatment estimates still sit well above zero.

### 3.3.3 Sub-Index Decomposition

We decompose the aggregate WAI effect into its three constituent dimensions in Table 3. The CS estimates show that the gains from first childbirth operate primarily through the domestic-bargaining channel rather than through freedom of movement.

The magnitudes here stand out. Decision-making goes up by 0.202 index points and financial autonomy by 0.261. Mobility, by contrast, moves by just 0.062, an order of magnitude smaller than the other two channels. The pattern fits a story in which a first birth raises a woman's standing inside her household, giving her more say over decisions about children and resources, without loosening the external norms that limit where she can physically go. Figure 4 shows the corresponding event-study dynamics.

Figure 3: CS and S&A Event-Study Estimates (Main Specification)

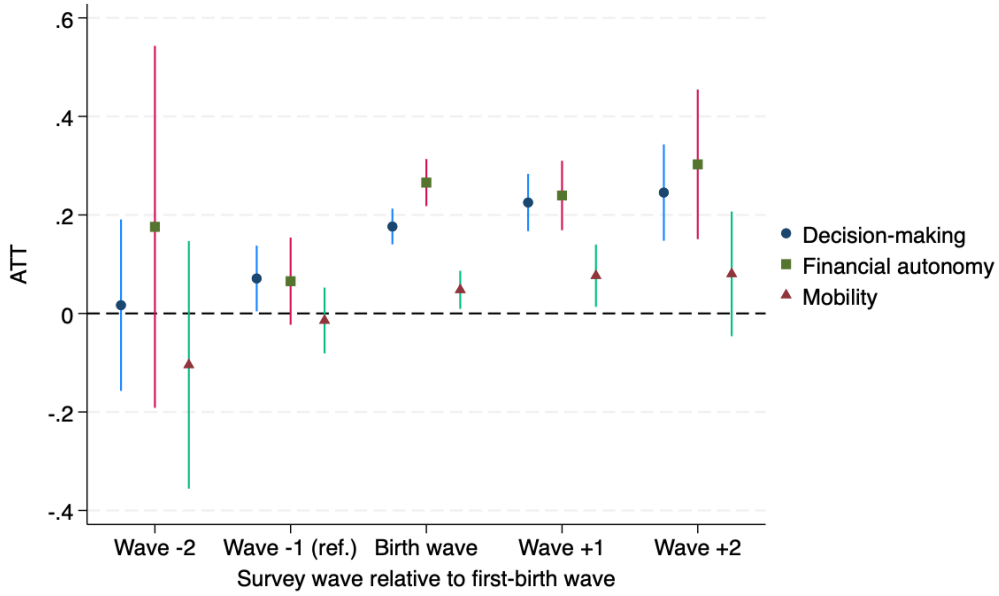



(a) No covariates

(b) With covariates

*Notes:* Each panel overlays the CS and S&A event-study coefficients for the effect of first childbirth on the Women's Agency Index. Both estimators use never-treated women as the comparison group. The horizontal axis is event time in waves relative to first birth; the omitted reference period is $k = -1$. Using 95% confidence intervals with standard errors clustered at the woman level. Covariates in panel (b) are baseline secondary education, urban residence, and wealth quintile.

Figure 4: CS Event Studies for the Three WAI Sub-Indices




### 3.3.4 Heterogeneous Effects

We assess heterogeneity along six margins: the sex of the first-born child, urban vs. rural residence at baseline, secondary education at baseline, employment status at baseline, baseline wealth quintile, and whether the mother went on to have a second child within the

Table 3: First-Birth Effects on the Three Sub-Indices of WAI

| | Decision-making (1) | Financial autonomy (2) | Mobility (3) |
|---|---|---|---|
| ATT (overall) | 0.202*** | 0.261*** | 0.062*** |
| | (0.021) | (0.027) | (0.022) |
| $N$ | 6,813 | 6,813 | 6,813 |

*Notes:* CS estimates with never-treated women as the comparison group and regression adjustment for the outcome model. The dependent variables are the three components of the WAI, each scaled 0–1. Standard errors clustered at the woman level. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$.

panel period. For each margin we re-estimate the CS specification on the corresponding split sample. Table 4 reports the overall ATT for each subgroup; Figure **??** presents the six event-study panels as a single composite figure.

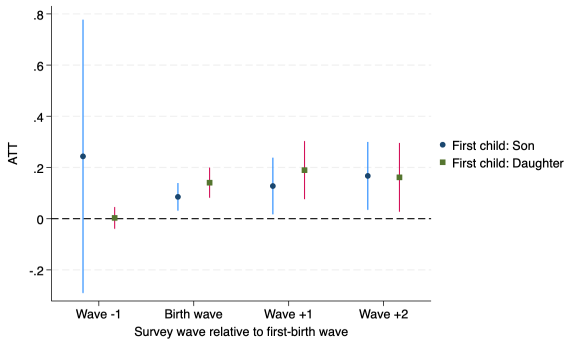
Table 4: Heterogeneous Treatment Effects on the WAI

| Subgroup | ATT |
|---|---|
| *Sex of first child* | |
| First-born son | 0.120*** |
| | (0.035) |
| First-born daughter | 0.162*** |
| | (0.037) |
| *Residence (baseline)* | |
| Urban | 0.116*** |
| | (0.036) |
| Rural | 0.118*** |
| | (0.026) |
| *Education (baseline)* | |
| Secondary or above | 0.045 |
| | (0.032) |
| Below secondary | 0.161*** |
| | (0.027) |
| *Employment (baseline)* | |
| Employed | 0.155*** |
| | (0.051) |
| Not employed | 0.106*** |
| | (0.024) |
| *Wealth (baseline)* | |
| Bottom 40% (q1–q2) | 0.091*** |
| | (0.031) |
| Top 60% (q3–q5) | 0.147*** |
| | (0.030) |
| *Second-child status* | |
| Two or more children | 0.120*** |
| | (0.023) |
| Only one child | 0.121*** |
| | (0.026) |

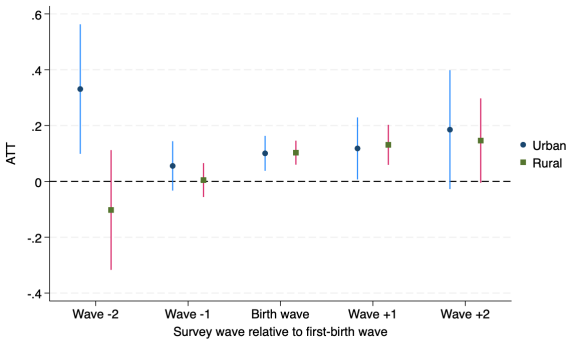
*Notes:* Each row reports the CS-DiD overall ATT from a split-sample regression with never-treated women as the comparison group. "Baseline" moderators are measured at wave 1 (2006); women not observed in 2006 are assigned the value from their first observed wave (this affects mostly women in the late-treated cohort). Second-child status splits the ever-mothers by whether the year when the second child was born is observed; never-mothers are retained as controls in both rows. Standard errors clustered at the woman level. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Three patterns are worth flagging. **First**, the effect is a bit larger for women whose first child is a daughter (0.162) than for those whose first is a son (0.120). The gap is not huge, but it lines up with the idea that mothers of daughters lean into agency more actively, perhaps to make up for not having produced a son in a patrilineal setting. However, it is worth noting that the difference between the effect of having a son and having a daughter is not statistically significant. **Second**, the gains are concentrated among women with below-secondary education (0.161); for women with secondary schooling or more, the estimate shrinks to 0.045 and is not statistically significant. That fits a story where motherhood serves as an *alternative route* to agency for women shut out of the education channel. **Third**, women in the top three wealth quintiles see larger gains (0.147) than those in the bottom two (0.091).

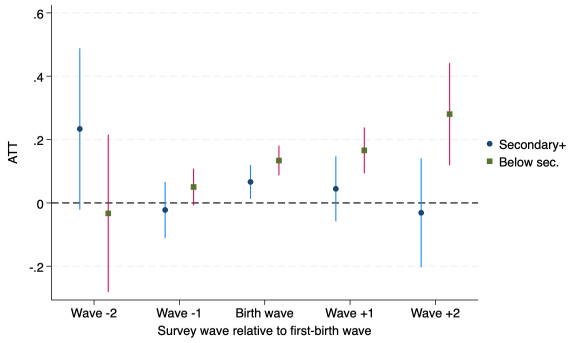
Read together, the wealth and education patterns point to the same thing: turning the status bump from motherhood into observable autonomy seems to take either some material resources to work with, or enough baseline disadvantage that there's room to move. Two non-findings are telling in their own right. Urban and rural women gain almost exactly the same amount (0.116 vs. 0.118), so the WAI effect is not really an urbanization story. And mothers who go on to have two or more children gain the same as mothers who stop at one (0.120 vs. 0.121), the agency gain looks like a discrete jump tied to the first birth, not a dose-response that scales with parity.



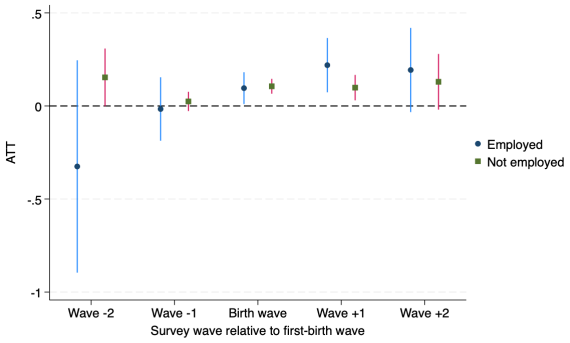

(a) First-child gender



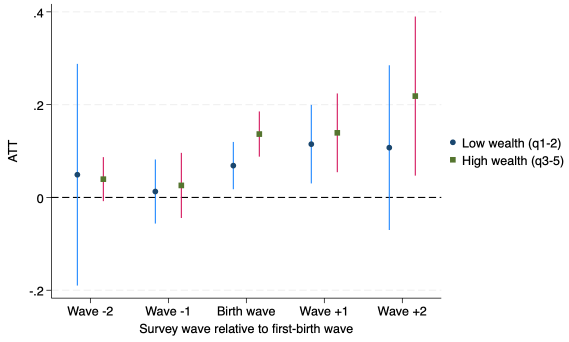

(b) Residence (baseline)



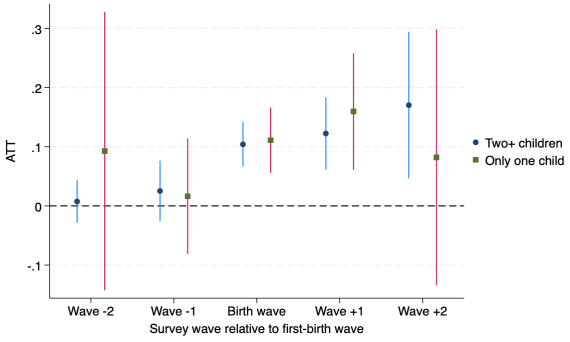

(c) Education (baseline)




(d) Employment (baseline)




(e) Wealth quintile (baseline)




(f) Second-child status

*Notes:* Each panel shows split-sample CS event-study coefficients with never-treated women as the comparison group. Using 95% confidence intervals; the omitted reference period is $k = -1$. Baseline moderators are measured at the earliest observed wave for each woman.

### 3.3.5 Second Birth as a Separate Treatment

The heterogeneity result on second-child status raises a natural question: does a second birth deliver any additional agency gain? To address this, we re-estimate the CS specification using the second-birth year as the treatment-timing variable, on a sample that excludes women whose second birth occurred before 2007. Table 5 reports two specifications. Column (1)

uses never-treated women as the control group (the analogue of our main specification). Column (2) restricts the sample to women who eventually have a second birth and uses late-second-birth women ($\geq$ 2019) as the not-yet-treated control.

Table 5: Effect of Second Childbirth on the WAI

| | (1) Never-treated control | (2) Late-2nd-birth control |
|---|---|---|
| ATT (overall) | 0.010 | $-0.070^{**}$ |
| | (0.015) | (0.029) |
| $N$ | 10,408 | 5,909 |

*Notes:* The treatment is the year of second birth. Column (1) uses CS with never-mothers as the comparison group and regression adjustment. Column (2) restricts the sample to ever-second-birth women with women whose second birth occurred in 2019–2023 serving as the not-yet-treated control. Standard errors clustered at the woman level. $^{*}p < 0.10$,$^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

The two columns tell a coherent story. The never-treated specification gives a precisely-estimated zero (0.010) conditional on already being a mother, having a second child adds nothing to agency relative to staying childless. The cleaner comparison in column (2), early vs. late second-birth mothers, all of whom eventually have two kids, actually flips sign and turns up a statistically significant negative effect (0.070). So the marginal effect of additional children on agency is at best zero, and possibly slightly erodes the first-birth gain. That's what one would expect if the second birth mainly piles on caregiving time without delivering any further bump in social standing.

### 3.3.6 Sensitivity Analyses

We run four sensitivity checks on the main result.

**Alternative control group:** One reasonable worry about the never-treated comparison is that childless women in our sample are systematically younger, less often married, and more urban, all things that might correlate with baseline agency. To get around this, we re-estimate the CS using late-treated mothers (first child birth year $\geq$ 2019) as the not-yet-treated control. The point estimates come out larger (0.078 without covariates, 0.196 with covariates) but noisier, since the effective sample shrinks to roughly 4,772 observations. The combined CS and S&A event studies for this specification are in Figure **??**.

**Doubly-robust estimation:** Running the doubly-robust version of CS gives point estimates almost identical to the regression-adjustment specification: 0.120 without covariates and 0.098 with. That the two methods land in the same place is reassuring that the result is not being driven by misspecification of either the outcome or the propensity model.

**HonestDiD bounds:** We apply the sensitivity analysis of Rambachan and Roth (2023) to assess how robust the result is to violations of parallel trends, reporting two restriction classes. The relative-magnitudes restriction $\Delta^{RM}(\bar{M})$ bounds post-treatment violations at $\bar{M}$ times the largest observed pre-treatment violation. The smoothness restriction $\Delta^{SD}(M)$ bounds the magnitude of second differences in the underlying differential trend by $M$ per period.

Under the smoothness restriction, the result holds up well. At $M = 0$, which imposes exact parallel trends, the 95% confidence interval for the average post-treatment effect is $[0.055, 0.288]$. The breakdown value of $M$ where the lower bound first crosses zero sits between 0.04 and 0.06. In other words, post-treatment trends can drift away from linear extrapolation by up to roughly 0.04 index points per period before the result becomes statistically indistinguishable from zero.

The relative-magnitudes restriction is less forgiving. The original interval [-0.058, 0.146] is consistent with non-zero treatment effects, but already at $\bar{M} = 0.5$ the bounds widen enough to cover zero. We read this fragility as a feature of the pre-treatment data rather than a strike against the causal interpretation: with only one effective pre-period per cohort (the reference period $k = -1$) and one earlier pre-period for cohort $g_4$ alone, there simply isn't enough variation in the pre-treatment data to bound post-treatment violations of arbitrary functional form.

We lean primarily on the $\Delta^{SD}$ restriction because smoothness is the substantively appropriate assumption for this setting. Selection into the timing of first childbirth in Egypt is shaped by gradual processes, educational attainment, marriage formation, partner negotiation, family pressure, that evolve over years rather than jumping discontinuously at the moment of birth. The class of confounders that would generate violations beyond the smoothness bound is narrow, and we cannot identify a plausible candidate in this setting.

**Two-way fixed effects benchmark:** The static TWFE estimator gives ATTs of 0.099 (no covariates) and 0.091 (with covariates), close to our preferred CS effects. The small attenuation in TWFE is what one would expect from mild heterogeneity bias when already-treated units end up acting as effective controls (Goodman-Bacon, 2021), but it does not change the substantive picture.

Taken together, these checks back up the headline result: first childbirth raises Egyptian women's agency by roughly 0.10 index points, with the mechanism action concentrated in financial autonomy and household decision-making.

# 4 Robustness

Beyond the sensitivity analyses in Section 3.3.6, we report a few additional checks on the event-study dynamics.

**Alternative WAI specifications:** Re-estimating the main event study using (a) the arithmetic mean of the three sub-indices and (b) a simple additive index gives qualitatively identical results. The geometric mean used in our main specification produces slightly more conservative estimates because it penalizes inequality across dimensions as a woman who scores high on one sub-index but low on another is given less credit than under the arithmetic mean.

**Coefficient stability across control sets:** Following Oster (2019), we progressively layer baseline controls into the CS regression. The point estimate moves from 0.120 with no controls to 0.098 with the full set, a ratio of 0.81. The limited movement suggests the estimate is not particularly sensitive to observable covariates, which in turn implies that selection on unobservables would need to be larger than selection on observables to wipe out the result.

**Balanced vs. unbalanced panel:** Restricting the sample to the balanced sub-panel, women present in all four ELMPS waves, delivers point estimates close to those from the unbalanced panel, with somewhat wider confidence intervals reflecting the smaller sample.

**Alternative clustering:** The results hold up under heteroskedasticity-robust standard errors in place of individual-level clustering; significance levels do not change in any meaningful way.

# 5 Conclusion

This paper provides causal estimates of the effect of first-time motherhood on women's agency in Egypt, using a Women's Agency Index (WAI) built from decision-making, financial autonomy, and mobility sub-indices. Rather than the uniformly constraining picture that

global studies tend to paint, we find that first childbirth raises Egyptian women's agency by roughly 0.10 index points; a discrete jump tied to the first birth, with no further gains from additional children.

The aggregate effect hides what is really going on. Almost all of the action sits in domestic decision-making and financial autonomy; mobility barely moves. Whatever authority a woman gains by becoming a mother stays inside the household. The norms that govern where she can physically go are not loosened by the same event that gives her more say over money and children. The gains are also unevenly distributed. They are concentrated among women with below-secondary education and women in the upper wealth quintiles, while mothers of daughters benefit slightly more than mothers of sons. Together, these patterns suggest that turning the status bump from motherhood into observable agency requires either some material resources to work with or a low enough baseline that there is room to rise, and in a patrilineal setting, having a daughter may push mothers to assert agency more actively than having a son does.

Two policy implications follow naturally. First, programs aimed at women's empowerment should not assume that fertility uniformly undermines agency; in this setting, the first birth is itself an empowering event, and interventions that build on it, particularly around financial inclusion for low-wealth mothers, are likely to gain traction. Second, the gap between domestic and public agency points to the limits of household-level change: gains in decision-making do not spill over into mobility on their own, and closing that gap will require something other than what childbirth provides. What remains open is the mechanism. We can document that the first birth raises status within the household, but we cannot fully separate the symbolic shift of becoming a mother in a society where motherhood confers standing from the practical reality that mothers control more household resources and decisions by necessity. Disentangling these channels is a natural next step.

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# A Appendix

## A.1 WAI Dimensions, Indicators, and Coding

Table 6: WAI Components and Survey Items

| Dimension | Item | ELMPS Question |
|---|---|---|
| Decision-Making | Own clothing | Who decides about your clothing purchases? |
| | Own medical care | Who decides when you need medical care? |
| | Daily HH purchases | Who decides about daily household purchases? |
| | Large HH purchases | Who decides about large household purchases? |
| | Visits to family/friends | Who decides about visits to family? |
| | Cooking | Who decides what to cook? |
| | Children's clothing | Who decides about children's clothing? |
| | Children's schooling | Who decides about sending children to school? |
| | Children's medical care | Who decides about children's medical visits? |
| Financial Autonomy | HH money access | Do you have access to household money? |
| | Personal savings/assets | Do you have savings, property, gold, etc.? |
| Mobility | Local market | Can you go alone to the local market? |
| | Doctor visit | Can you go alone to the doctor? |
| | Family/friends visit | Can you go alone to visit family/friends? |
| | Children's health center | Can you take children to the health center? |

## A.2 Additional Descriptive Figures

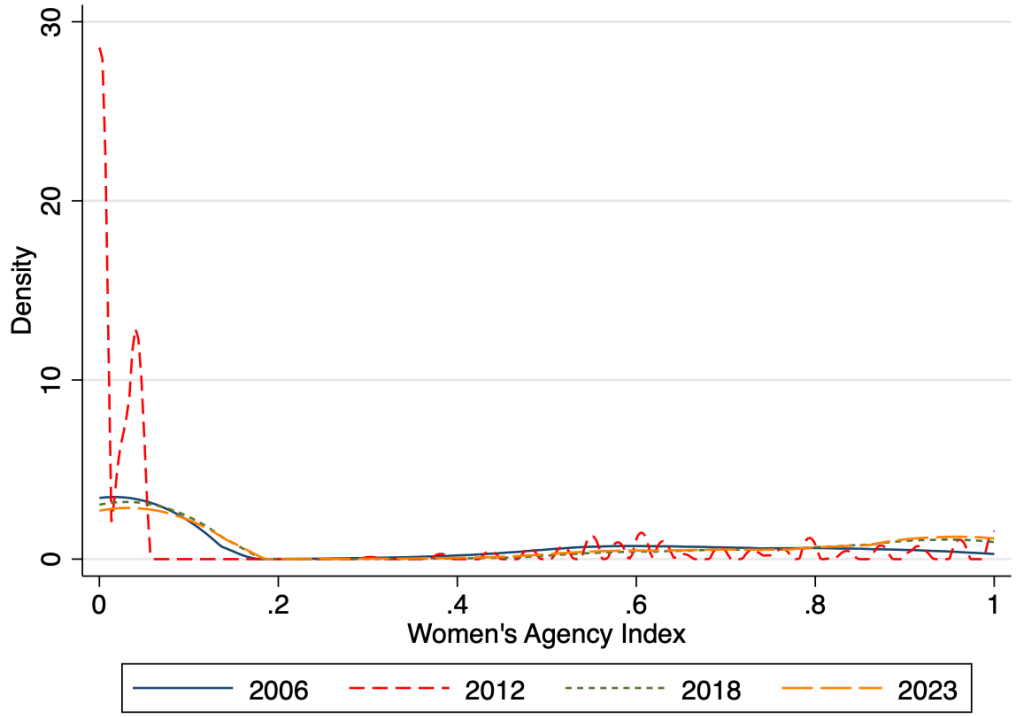


Figure 6: Distribution of WAI by Survey Wave

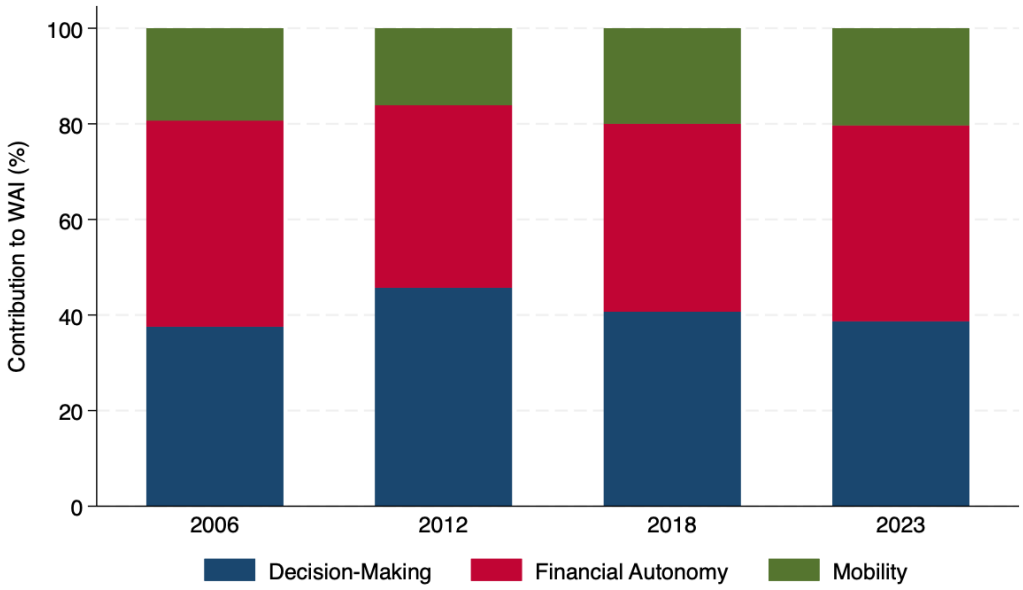


Figure 7: Contribution of Each Dimension to the WAI, by Year

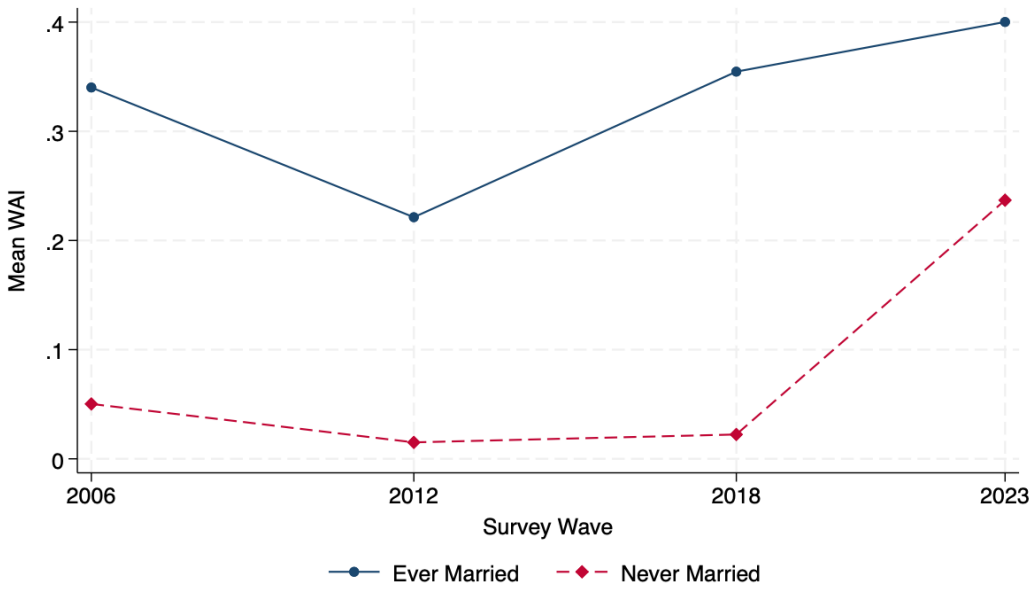


Figure 8: WAI by Marital Status and Wave

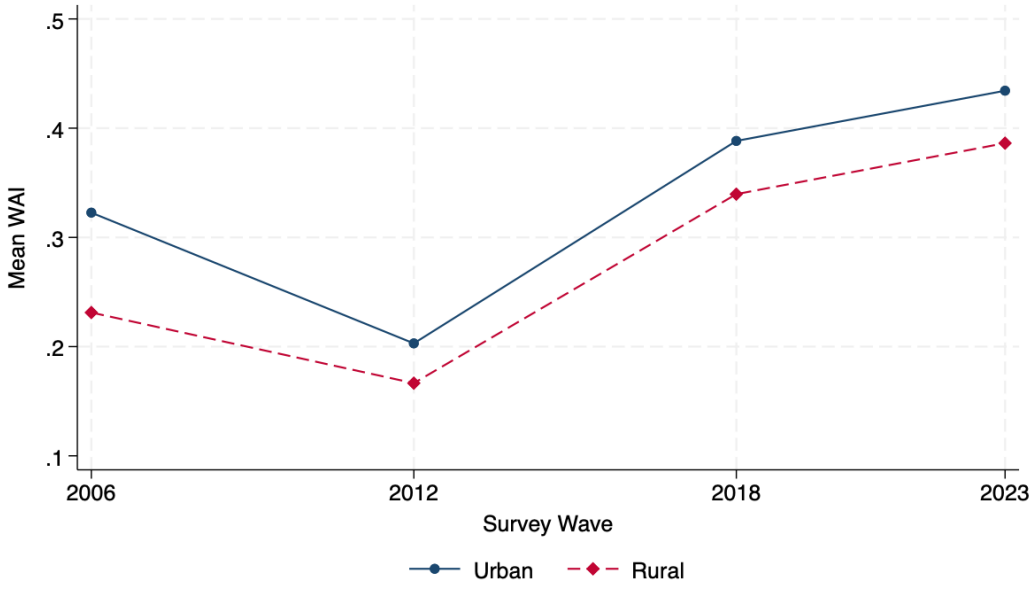


Figure 9: WAI by Urban/Rural Residence and Wave

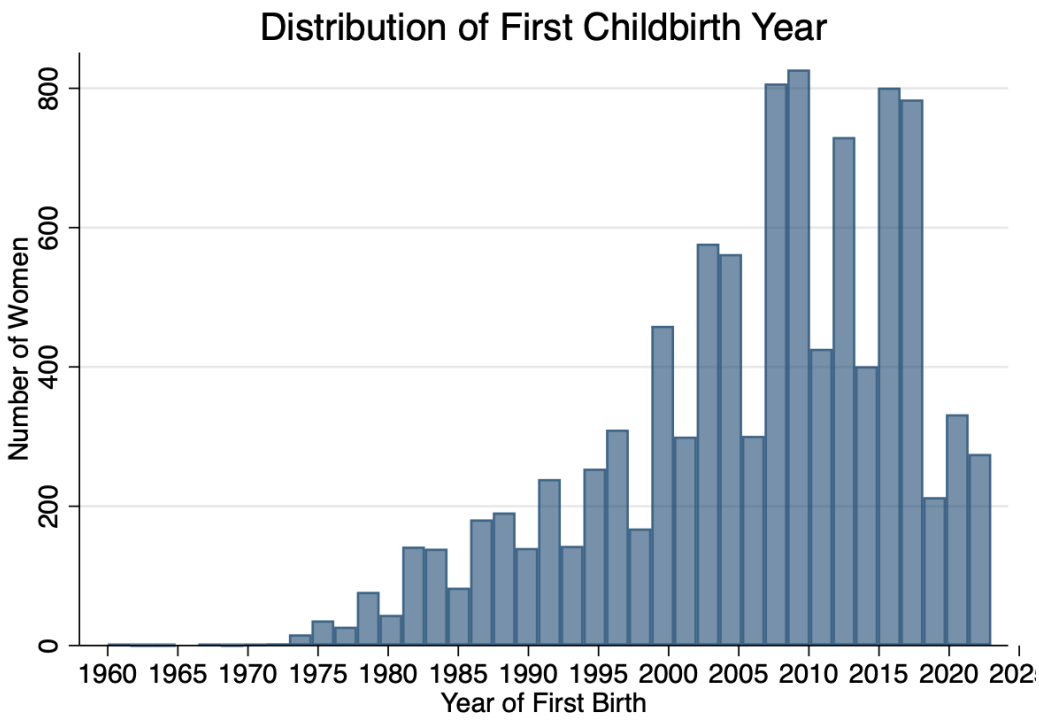


Figure 10: Distribution of First Birth Cohort Year