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Unilateral divorce rights, domestic violence and women's agency: Evidence from the Egyptian *Khul* reform☆

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ABSTRACT

We investigate whether the introduction of the right to unilateral, no-fault, divorce for women has an impact on domestic abuse, investments in children's human capital, women's labor force participation, and other proxies of women's agency in the context of the Egyptian *Khul* reform of 2000. We employ a difference in differences design, comparing mothers of children older than the age cutoffs used to assign the children's custody to the mother, to mothers of younger children, before and after the reform. The first group of women is less affected by the legislative change in terms of being able to make credible divorce threats because it faces higher divorce costs, including the loss of alimony and the marital house. Results suggest that the introduction of *Khul* decreased domestic abuse and increased investments into children's education while we do not find significant effects on labor force participation.

1. Introduction

Despite international efforts in promoting gender equality and women's empowerment, gender gaps have not narrowed at the same pace everywhere and in every domain (World Bank, 2012). The Middle East and Northern Africa (MENA) region is where the gap in gender rights, agency and opportunities has been the most visible and severe (World Economic Forum, 2018). Domestic violence is one among the challenges women face at high rates in MENA countries: the WHO estimates that 37% of women in this region face intimate partner violence (IPV) at least once in their lifetime, the worst statistic in the world together with South-East Asia (World Health Organization, 2013).

Past research in developed countries has shown that divorce laws have consequences on the balance of bargaining power between spouses in intra-household decision-making and on domestic violence more specifically (see, among others, Lundberg and Rose (1999), Chiappori et al. (2002), Stevenson and Wolfers (2006), Voena (2015) and Brassiolo (2016)). This paper studies the effect of the introduction of the right to file for unilateral, no-fault, divorce for women on domestic violence, wife's decision making power within the household and women's labor force participation in Egypt.

Egypt introduced unilateral no-fault divorce for women, also called *Khul*, in 2000. To identify the causal effect of *Khul* divorce we use a difference in differences strategy, comparing women more or less affected by the reform, before and after its introduction. Women more affected by the reform are those for whom divorcing is less costly, constituting our "treated" subjects. In this setting, women who face a high divorce cost are mothers of children older than the age thresholds

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used to assign the custody to fathers, who would lose the custody of the children above these age cutoffs, the right to alimony and the right to stay in the marital home. For these women divorcing is arguably harder and costly even after the introduction of no-fault divorce, hence we would expect the reform to have a limited impact on them. In the main specification, we use the age and the sex of the youngest child to assign women to the treatment group (women facing lower divorce costs), or to the control group (women facing higher divorce costs).

Our results suggest that the reform significantly decreased domestic abuse, and that it increased the educational attainment of treated women's children, consistently with the view that men and women have different preferences over investments in children's human capital (Duflo, 2003; Rangel, 2006; Heggeness, 2020). Both of these results are in line with an increase in the bargaining power of the wife within the couple. Our preferred specification indicates that the divorce reform decreased IPV by about 7 percentage points, corresponding to a 58 percent decrease with respect to the pre-reform sample mean. In line with these results, we find weakly positive impacts on other measures of women's agency, such as the share of women reporting to have the final say on issues related to household budget spending and visits to relatives and friends. The *Khul* reform appears to have increased school enrollment of children born to treated mothers by about 6 percentage points, or a 12% increase with respect to the pre-period average, and to have delayed their entry into the labor force and into marriage. Lastly, we find no robust effect on women's employment. Some estimates weakly point towards a reduction in overall labor force participation, even though effects seem to kick-in only several years after the reform.

The introduction of unilateral divorce entails a redistribution of the gains to marriage and of bargaining power within the couple in favor of the spouse who wants out and to the detriment of the spouse most interested in preserving the marriage, as in models of intra-household decision making with heterogeneous preferences (McElroy and Horney, 1981; Manser and Brown, 1980). The previous literature has focused on changes in divorce regimes within western countries, where the right to unilaterally file for divorce had been granted simultaneously and symmetrically to both spouses. These contexts do not allow, a priori, to unequivocally identify in favor of which spouse the redistribution of power within the couple has occurred, as in some couples it could have been the husband who wished to opt out and in some others the wife.

The 2000 Egyptian *Khul* divorce reform, on the contrary, introduces the right to unilateral divorce for women only. *Khul* divorce was introduced during the 1990s and the 2000s in many countries with a Muslim majority to allow women to file for divorce without the need of proving their husband's fault (Welchman, 2007). Before then, wife-initiated divorce was only allowed on the basis of the husband's fault in very restrictive situations. To the best of our knowledge, we are the first to focus on a divorce reform that clearly redistributes the bargaining power in favor of the wife, allowing her to make credible exit (divorce) threats. For this reason, we believe this context is particularly well suited to investigate how an increase in the woman's bargaining power affects domestic violence, women's agency and labor force participation.

This paper aims to contribute to two strands of the literature. The first studies how the introduction of unilateral divorce affects couples' decisions and women's empowerment within the household, focusing on outcomes such as spousal homicides and well-being (Dee, 2003; Stevenson and Wolfers, 2006; Brassiolo, 2016); female labor supply (Chiappori et al., 2002; Genadek et al., 2007; Gray, 1998; Olivetti and Rotz, 2017; Parkman, 1992; Stevenson, 2008; Voena, 2015); marriage-specific investments (Stevenson, 2007); fertility (Drewianka, 2008; Alesina and Giuliano, 2007); or children's education and well-being (Lundberg and Rose, 1999; Heggeness, 2020). The second includes studies testing the unitary versus collective model of the household. This literature often finds that an increase in bargaining power of mothers, often linked to a relative increase in their financial resources, improves children's human capital investments and increases the share of household spending on children's goods (Duflo, 2003; Quisumbing and Maluccio, 2003; Rubalcava et al., 2009; Schady and Rosero, 2008). In the context of family law, Rangel (2006) finds that the extension of alimony rights to cohabiting couples in Brazil increases hours worked by female adults and investments in the education of children, while Nunley and Seals (2011) shows how in American states adopting joint-custody laws children's private school attendance declines.

Most of these studies use European and U.S. data. In particular, no research has been conducted up to date on developing countries to understand the relationship between domestic violence and divorce. Heggeness (2020) is the first to provide evidence on the impact of access to divorce on the welfare of children in a middle income country, finding a positive effect on children's education. To the best of our knowledge, the study by Hassani-Nezhad and Sjögren (2014) is instead the only one that investigates the relationship between introduction of unilateral divorce and female labor supply in developing countries. Using cross-country variation in the timing of Khul reform between 1980 and 2008 in eighteen MENA countries and data aggregated at the country-sex level, they show that the right to petition for divorce, which was unilaterally given to women, increased the labor force participation of women relative to men.

We believe our study contributes to the literature in three ways. First, to the best of our knowledge, we are the first to investigate quantitatively the relationship between easier access to divorce, women's empowerment, and development indicators, like education, using individual level data in a developing country. Second, this paper is the first to study a unilateral ad asymmetric shift in women's bargaining power due to a divorce reform. Finally, social norms and culture matter, especially when studying domestic violence. Very few studies focus on the MENA region, despite its high incidence of domestic abuse and the wide disparities in the treatment of men and women in family law (Breslin and Kelly, 2010). This analysis attempts to shed light on an important topic and geographic area that have not been widely studied yet.

The rest of the paper is structured as follows. Section 2 describes the institutional context, Section 3 summarizes the conceptual framework, Section 4 details the empirical strategy, while Section 5 illustrates the data sources and provides descriptive statistics. We present results in Sections 6 and 7, and conclude in Section 8.

2. Institutional context: Egyptian personal status law and the 2000 *Khul* reform

We study the effects of the divorce reform passed in Egypt in 2000 that introduced the right to unilaterally file for divorce for women. In Egypt, divorce and matters pertaining to family life are regulated by personal status law, which is predominantly based on traditional Islamic law (*Shari'a*) and remains highly unchanged since the 1920s (Deif, 2004). Family laws do not often treat men and women the same.[1] In particular, the laws regulating divorce delineate two completely different gender-based systems for divorce. Men have a unilateral and unconditional right to divorce (*talaq*). It is sufficient for them to repudiate their wife verbally and register the announcement at a religious notary office within 30 days, with no need to enter a courtroom. On the contrary, both before and after the reform, women always have to petition a court (Deif, 2004).

The 2000 *Khul* reform changed the reasons why women could ask for divorce and made filing for divorce easier. Before the reform,

[1] For instance, while the evidentiary value of female witnesses is considered to be equal to that of men in most parts of the legislation, for family law women's testimony is worth half to a man's one. Egyptian jurisprudence applies the witness testimony rules of the Hanafi school, which requires the testimony of two male witnesses or two females and one male witness. See Amina (1996).

women could only file for a fault-based divorce in a few special cases, by proving their husband's fault on the following grounds: (1) illness and impotence; (2) failure to provide maintenance or financial support; (3) absence or imprisonment; and (4) "injury". The last reason is formulated in general terms and could thus, in principle, encompass cases of domestic violence. In practice, however, some forms of intimate partner violence are not only tolerated but also seen as a husband's religious and legitimate right to discipline his wife, and it is therefore very difficult to obtain a divorce on this ground (Deif, 2004).[2] The option to file for a traditional fault-based divorce remained in place even after the 2000 reform, with the same circumstantial requirements. Even when the circumstances grant women this right on paper, the process to get a fault-based divorce is typically long and burdensome, and its outcome (whether the divorce is granted) remains uncertain. A number of government officials have to assess whether the woman deserves a divorce and has the right reason to seek for one (Deif, 2004).[3] In particular, a major barrier women face to obtain a fault-based divorce is the substantial burden of proof required on their part. To file for divorce on grounds of physical violence, a woman needs to provide courts not only with a medical certificate, but also with two male witnesses (or one male and two female or four female), preferably not related to her, who saw the abuse occur. As most episodes of abuse happen within the walls of the family's house, it is evident that this requirement constitutes a huge hurdle in escaping from an abusive relationship by means of a fault-based divorce (Al-Sharmani, 2007).[4]

On January 29, 2000, President Mubarak, responding to requests by women's rights activists, signed law no. 1 of 2000, granting Egyptian women the right to file for *Khul* divorce. Under this new procedure, women do not have to provide reasons why they want a divorce, do not need men's consent and men do not have the right to appeal the divorce to a higher court. In addition, the requisite arbitration period is shorter and arbitrators, as opposed to mediation sessions prescribed for a fault-based divorce, do not have to assess the woman's need and worth of a divorce. Finally, the entire *Khul* process should take no more than six months to complete, contrary to the time that it takes to get fault-based divorces, which often amounts to several years.[5] In summary, the 2000 reform introduced unilateral divorce for the first time for all those women who could not opt for fault-based divorce or who lacked the evidence to prove fault, including victims of domestic violence. It also made the divorce process easier, faster and certain for all of them. As a result, the Cairo court alone, in the two months following the reform, received over 3000 applications (An-Na'im, 2002).

In exchange for this quicker divorce option, women must agree to forfeit significant financial rights to which they would be entitled under *talaq* divorce or under fault-based divorce. In particular, they have to give up their rights to alimony, to the deferred part of the dower, as well as repay the part of the dower already paid by the man.[6]

The laws regulating the custody of children after a divorce are instead independent of the type of divorce and of the spouse who has asked for it. Children's custody is given to the mother only below certain age thresholds, that were fixed at 10 years of age for sons and at 12 for daughters until 2005, although, on a case by case evaluation, the judge could extend the custody until the child turned 15 years old (Deif, 2004; Welchman, 2007). In 2005, the child's age custody has been raised to 15 years old for both sons and daughters.[7] The custody of children older than these age thresholds is usually automatically assigned to the father.[8] The enforcement of the age-custody thresholds has been rigorous enough to induce legislators, society and the media to discuss new solutions to mitigate custody conflicts in recent years. For instance, following cases of children under maternal custody kidnapped by fathers, Egypt pondered to introduce a travel ban for children of divorced parents younger than 15 (see for instance Osama (2019) and El-Din (2016)). Also, in a recent attempt to reform the law, conservative parties suggested to lower women's custody at age nine for both girls and boys, or to increase visitation periods for the non-custodial parent (Osama, 2019). These proposals have, however, been strongly opposed by women's rights organizations, and a campaign decrying the draft's approach to women's legal guardianship swept social media and the press.

Importantly, when mothers no longer have the custody of children they also lose the right to live in the marital house and to housing support as divorced women never obtain a share of, or a legal title to, the marital house. They are recognized the right to adequate housing post-divorce only during the period in which they hold the physical custody of at least one child. As a consequence, childless women and those who no longer retain the custody of their children are exposed to the risk of homelessness if they do not have somebody who could take care of their accommodation (Deif, 2004).[9] For these reasons, filing for divorce seems much costlier for mothers of older children than it is for mothers of children younger than the custody cutoffs. This aspect has been documented by several organization advocating in favors of women's rights and media outlets (Mada Masr, 2021), noting that divorcing is harder for several categories of women even after the introduction of *Khul*. We make use of this specificity of the law to motivate our empirical strategy.

3. Conceptual framework

Facilitating women's access to divorce can affect domestic violence, measures of wife's agency and other household outcomes through different channels. First, a direct mechanism operates through a change in divorce rates: easier access to divorce could increase divorce rates

[2] Since *Shari'a* is often deemed to permit the "disciplining" of "disobedient" women, domestic violence has often been excused under this article as long as the good faith is maintained, that is, the beating is not severe, not directed at the face and not aimed at vulnerable "fatal blow areas" (Deif, 2004). Moreover, judges have considerable discretion in deciding which degree of injury a married woman should endure. As reported by Breslin and Kelly (2010) and by interviews conducted among judges by Deif (2004), judges may discriminate women of different economic classes, based on prejudices and beliefs about what women of different backgrounds can tolerate. Deif (2004) writes that judges often assume that physical abuse or polygyny are a natural part of the existence of women coming from the lowest classes and as such they do not necessarily warrant a divorce.

[3] Government officials have a say in the process, including judges, public prosecutors and arbitrators involved in compulsory mediation between the couple.

[4] Al-Sharmani's analysis of the Egyptian divorce processes (Al-Sharmani, 2007) and Ihab Nagy, attorney for the Egyptian Organization for Women's rights (interviewed in Deif, 2004), confirmed that most cases fail because of lack of witnesses.

[5] The main reason for the difference in duration is that *Khul* eliminated the husband's right to appeal the divorce to higher courts (Deif, 2004).

[6] In Egypt the dower is paid by the husband or his family and it is composed of two parts: the first part is paid promptly at the conclusion of wedding and the second part is paid by the husband's family to the wife in case of termination of the marriage by death or by *talaq* or by fault-based divorce.

[7] Law 4 (2005).

[8] The reasoning behind these rules, that with some minor changes are widespread throughout the whole MENA region, lies in the different roles assigned to the parents in the Arab culture: the mother is thought to be the "custodian", while the father the "guardian". The mother's duties are linked to the physical care and nurturing of young children and her function is somehow considered exhausted when the children reach an age in which they might be more in need of a guardian, that is when boys "need to learn the ways of men", and girls "need the guardian's physical protection, as they come of an age to be married or to be the target of predatory male interest" (Welchman, 2007).

[9] The online newspaper Mada Masr reports that "Khula for women who have not had children or whose children have surpassed the age of custody means eviction from their homes" (Mada Masr, 2021).

and directly allow women to escape abusive relationship (Gray, 1998; Gruber, 2004; Allen, 1992; Friedberg, 1997; Gonzalez and Viitanen, 2006; Kneip et al., 2014; Wolfers, 2006). In our analysis, we focus on currently married women due to how our survey data was designed, as we will more carefully explain in Section 5. Since divorced women are not part of our sample, differential selection out of marriage could theoretically be one of the mechanisms behind our results.

A second mechanism can operate through changes in the spouses' matching on the marriage market: easier access to divorce reduces the commitment cost of marriage and it might lead some mismatched couples to "try out" marriage (Alesina and Giuliano, 2007). All the women in our analysis got married before the introduction of the reform so we mechanically shut down this marriage market mechanism.

The main theoretical channel that inspires our empirical analysis is instead the reallocation of bargaining power between husband and wife. In particular, the introduction of *Khul* could have redistributed the bargaining power in favor of the wife, by allowing her to make credible divorce threats. The models of Manser and Brown (1980) and McElroy and Horney (1981) can be directly applied in this context to describe household decision-making. In both models husband and wife have different preferences and the bargaining over within-couple distribution of resources depends on each spouse's threat point, that is interpreted as the utility of remaining single or of getting divorced. The introduction of unilateral divorce for women can be seen as an exogenous increase in their outside option, which could affect intra-family bargaining outcomes even if the option of divorce is not actually being exercised. [10] Consequently, we could observe a decrease in domestic violence, as well as an increase in indicators of women's bargaining and decision making power even within couples who remain married (Lundberg and Rose, 1999; Voena, 2015; Fernandez and Wong, 2014).

Other channels might however offset the positive outcomes associated to an improvement in the woman's relative standing within the couple. Increased bargaining, for instance by means of threatening separation, might increase tensions and conflict within the household and it might come at the cost of higher domestic violence (Anderson and Genicot, 2015). Moreover, increased opportunities for women might challenge traditional gender norms and also spur more conflict. These are the predictions of the backlash theory, from the sociology literature, that argues that an increase in wives' independence might represent a challenge to a culturally prescribed norm of male dominance and female dependence. When men feel that their masculinity and traditional dominant role within the couple is being threatened, violence may be a means of reinstating their authority over their wives (Hornung et al., 1981; Macmillan and Gartner, 1999).

There is mixed empirical support for these predictions. Koenig et al. (2003) find that in conservative areas of rural Bangladesh higher women's autonomy and membership in savings and credit groups were both associated with significantly elevated risks of violence. Similarly, Macmillan and Gartner (1999) find that women's labor force participation substantially increases risks when their male partners are not employed while Bertrand et al. (2015) report that couples where the wife earns more than the husband are less satisfied with their marriage and are more likely to divorce. In line with the hypothesis that increased opportunities for women might accentuate marital discord within households, Anderson and Genicot (2015) find that increased property rights for women increased the incidence of wife beating and suicides among both men and women in India.

Other studies, however, reached opposite conclusions on the association between violence and women's empowerment. Aizer (2010), for instance, finds that decreases in the male–female wage gap reduce violence against women in the U.S., consistent with a bargaining effect. Also Chin (2012) finds that female employment reduces the incidence of spousal violence in India, although he claims the effect to be a result of the reduction in the time women are exposed to their violent husbands. Whether improvements in women's bargaining power improve their well-being and reduce domestic violence seems ultimately an empirical question.

Finally, it is possible that the *Khul* reform might have indirectly increased husbands' legal accountability and thus deterred domestic violence. Both before and after the reform, severe episodes of domestic violence could be used in court to ask for a fault-based divorce, which would be a better financial option for women seeking a way out of their marriage. One potential cost associated with fault-based divorce, however, is that women who are not granted it might face backlash from their husbands, with no way out of their relationship. The introduction of a certain option out of marriage through no-fault divorce might have indirectly lowered the cost of applying for fault-based divorce. This, in turn, might increase the chances that an incident of domestic violence is used in a court and thus husbands' legal accountability. Similarly, even if women only threatened to ask for a no-fault divorce, it is possible that men might fear that other issues, such as IPV, could incidentally come to the ear of the judge (or other family members) and be punished. Finally, it is possible that the reform might have signaled that social norms and judicial views were getting more protective of women's rights. As a consequence it might have shifted collective beliefs that domestic violence would be punished more if the issue reached court.

4. Empirical strategy

4.1. Women's outcomes

While before 2000 women-initiated divorce was not a possibility for most women, the introduction of *Khul* made asking for a divorce feasible and easier for all of them. Yet, even after the reform, divorcing was arguably more costly for mothers of children older than the age thresholds used to assign custody to the father, than it was for mothers of younger children.

This setting motivates the use of a difference-in-differences strategy: we compare the change in the outcomes of interest for mothers of children younger than the custody age cutoffs, before and after the reform, to the change in the same outcomes for mothers of older children, who should be less affected by the legal change. In our main specification, we assign women to the control group if, in case of divorce, they would lose the custody of all their children. To do that, we look at the age of the woman's youngest child: we assign women to the control condition if their youngest child's age is above the age cutoff after which custody passes to the father. The cutoff is 10 years of age for boys and 12 years for girls. Women whose youngest child is a boy aged 10 or 11 but who also have a daughter in the same age range are assigned to the treatment group, since they would keep the custody of at least one child, the daughter, in case of a divorce.

Our main specification is:

$$Y_{it} = \alpha + \beta\, Post_t + \gamma\, Treated_i + \delta\, Post_t \cdot Treated_i + X_{it}'\,\lambda + \epsilon_{it} \quad (1)$$

where Y_{it} is a dependent variable for woman i, such as reported domestic violence, in year t; $Treated_i$ is an indicator of the treatment group and $Post_t$ is a binary indicator for the post reform period. The parameter of interest is δ. X_{it} is a vector of control variables, always including the woman's and her husband's years of education, a dummy for urban households, indicators of household wealth-index quintiles, and age group fixed effects for combinations of the woman and her

[10] As argued by Becker (1981), this setting can simply be seen as an application of the Coase theorem. If compensations between spouses are feasible and costless, efficient bargaining within the couple ensures a redistribution of the gains to marriage in such a way that divorce only occurs when the joint benefits exceed the joint costs.

husband's ages.[11] In some specifications we also add controls for age-group time trends and age-at-marriage time trends, by interacting the age group dummies or the age at marriage quartiles dummies with the $Post_t$ indicator. Depending on the data we use, we estimate this equation using survey rounds conducted in 1995 and 2005, thus respectively 5 years before and 5 years after the reform, or in 1998, 2006, and 2012, thus 2 years before and 6 and 12 years after.

There are several reasons to prefer the assignment to treatment based on the age of the youngest child as opposed to using the oldest child's age or another criterion. First, women who lose the custody of the last child also lose the right to the marital house and the right to housing support, as opposed to women who maintain the custody of all or some of the children. These women would be able to make less credible divorce threats if they had no means to support themselves financially and risked homelessness. Second, the youngest child's age is less strongly correlated than the oldest one's with the mother's age. This allows us to have enough variation in women's age within each treatment group to control for mothers' age and age time-trends. In some specifications we also leverage variation in the age of the oldest child, as explained more in detail in the results section.

While for most outcomes we only use data from one period before and one period after the introduction of the reform, for a subset of our dependent variables we also estimate event study regressions of the form:

$$Y_{it} = \mu_t + \gamma\, Treated_i + \sum_{k=-L}^{k=+U} \delta_k \cdot \mu_k \cdot Treated_i + X'_{it}\,\lambda + \epsilon_{it} \tag{2}$$

where μ_t represents year fixed effects and the rest of the notation remains the same.

The regression sample is made of currently married women between 18 and 49 years old, whose youngest child's age is within a ±5 years window around the custody cutoffs.[12] We use this last restriction to make the treatment and control group of women more comparable in terms of demographics, such as age and education, and other unobservables, so that they would mainly differ by the arguably almost random age of their youngest child.

One potential concern with our empirical strategy could be changes in the composition of the group of women with younger children after the reform due to endogenous fertility choices. That is, after the reform, some women may strategically decide to have more children, or to space out births more, precisely because it provides a credible outside option through divorce. In particular, our focus on the 5 years window around the custody cutoff only keeps in the sample women who stopped having children before the reform.

We carry out two exercises to convince the reader that strategic fertility decisions do not seem to be a concern. First, we consider a larger window around the cutoff (10 years), thereby including in the sample essentially all women that had a child after 2000, the year of the reform. While this does not solve the issue, as women who might have strategically chosen to have an additional child after the reform are now self-selecting into treatment rather than outside of our sample, it might still provide some intuition regarding the sign of the possible bias.

Second, we check if the propensity to have an additional child changes around the year of the reform, differently for women whose youngest child's age is close to or above the cutoff, relative to women whose youngest child is still quite young. The incentives to have an additional child after the reform should in fact be strongest for women whose youngest child is approaching the custody cutoff age, or is already above it.

We construct a birth events panel using all women with at least one child at the time of the survey interview and we estimate the following equation:

$$Has\_birth_{it} = \sum_{k=1990}^{2010} \mu_k + \sum_{k=1990}^{2010} \delta_k \cdot D_{ik} \cdot \mu_k + X'_{it}\beta + \epsilon_{it}, \tag{3}$$

where μ_t indicates year dummies and D_{it} is an indicator for women whose youngest child in year t is older than a certain age threshold. We consider two different age thresholds: the custody cutoff and 3 years before the custody cutoff.[13] The coefficient δ_k measures the difference in the propensity to have an additional birth in year k between women whose youngest child is (close to or) above the age cutoff and mothers of younger children. If fertility choices were affected by the divorce reform, we would expect the coefficient δ_k to jump in $k = 2000$, or to be significantly higher for $k \geq 2000$, as incentives to have another child are higher for women whose child is approaching the cutoff after the reform.

We plot the coefficients δ_k in Fig. A.2. The darker dots are from a regression that uses the custody cutoffs to define D_{it}, while for the lighter series we define D_{it} using children "close" to the cutoff (3 years younger or older). In the same figure, we also plot the raw mean preceding birth spacing by birth year. Overall the figure suggests that birth spacing is increasing over time. Similarly, while the propensity to have a child is lower for women with children above or close to the cutoff than for women with younger children (the coefficients are negative), this difference seems to shrink over time. However, these patterns appear the result of a secular trend: we do not observe a discontinuity nor a significant change in the time trend slope in 2000 in birth spacing or the relative propensity to have a child on the basis of previous children's ages.

4.2. Children's outcomes

Previous literature has shown that improving women's position within the household, relative to men's, is associated with larger investments in children's health and education (Duflo, 2003; Rangel, 2006; Heggeness, 2020). We study how an improvement in the woman's bargaining power due to *Khul* affects children's outcomes using an alternative difference-in-differences strategy.

We define treatment at the household level: all children from households in which the youngest child is below the custody cutoff are treated. Since the earliest children's outcomes available in the post-reform period are from 2006, we set the custody cutoff to 15 years old for both girls and boys, as the custody cutoffs were extended by law in 2005. We then restrict the estimation sample to all children between 15 years old, the custody cut-off, and 25 years old, the age at which formal education is expected to end, born to women included in our women analysis sample. These are children that in case of divorce would not be assigned to the mother, but that are treated under the new definition if some of their siblings are below 15 years old. We cannot estimate the impact of the reform on children younger than 15 years old, since all households with at least one child below this age would be automatically treated and thus we would not have within-child's age variation in the treatment status.

Rather than comparing children below and above the custody cutoff, this second empirical strategy allows us to compare same-age children living in treated and control households, which is important as education outcomes are strictly related to the child's age. Moreover, the effects of increases in mother's relative position within the household

[11] We group the woman's age into 6 categories: 18–25, 26–30, 31–35, 36–40, 41–45, 46–49. We similarly use 6 categories to group the husband's ages, which are however more dispersed: 18–30, 31–40, 41–45, 46–50, 51–60, 61–90. We include a full set of indicators for each possible combination of the woman's age and her husband's age.

[12] We restrict to currently married women since the questions about the domestic violence experienced in the previous 12 months and decision making power within the couple are meaningful only for this group of women.

[13] Similar results hold when using different age thresholds.

need not only benefit the children below the cut-off themselves but it is plausible that they would spur to all the off-spring, even to those that in case of divorce would end up living with the father. If anything, they might impact more older children, who are at the margin for dropping out of school and entering the workforce.

We estimate the following difference-in-differences model with age fixed effects, comparing the change in the outcomes of interest for children older than the custody age cutoff with siblings younger than the age cutoff (treated) to that of same-age children with siblings older than the age cutoff (control), before and after the reform:

$$Y_{ict} = \alpha_c + \beta\, Post_t + \gamma\, Treated\_hh_i + \delta\, Post_t \cdot Treated\_hh_i + X'_{ict}\,\lambda + \epsilon_{ict} \quad (4)$$

where Y_{ict} is the dependent variable of interest, such as enrollment, for children i of age c, in year t; α_c are children's age fixed-effect, $Treated\_hh_i$ is an indicator for children of treated households and $Post_t$ is a binary indicator for the post reform period. The parameter of interest is δ. We augment the set of covariates, X_{ict}, used in Eq. (1) with a set of dummies indicating family size and a control for children's gender.[14] As before, in some specifications we include controls for age-group time trends and age-at-marriage time trends, and we further account for trends in the number of children within the household.[15] We estimate this equation using survey rounds conducted in 1998, 2006, and 2012.

The coefficient δ provides an estimate of the effect of the *Khul* reform on children's outcomes. Our identifying assumption is that, in the absence of the divorce law reform and conditional on time-varying controls, the change in education and working outcomes would not have been systematically different across treated and control children of the same age.

5. Data and descriptive statistics

5.1. Demographic and health surveys

We use two sources of data. The first is the Demographic and Health Surveys (DHS), nationally representative surveys of ever married women aged 15 to 49. Our main analysis uses repeated cross sections collected in Egypt in 1995 and 2005, respectively 5 years before and 5 years after the reform.

Table 1 contains descriptive statistics on the DHS sample of women used in the main analyses. It includes mothers whose youngest child's age is within a ±5 years window from the custody cutoff.[16] Panel A reports marital status statistics. The rest of the table restricts the sample to currently married women, as in the regressions.

We construct our outcomes of interest using answers to questions on self-reported domestic violence, working status in past 12 months and on women's decision making power within the household.[17] Our main measure of domestic violence is an indicator that takes the value 1 for women reporting to have experienced physical violence perpetuated by the husband in the last 12 months, and 0 otherwise. The wording of the questions changes slightly from one survey round to the other. In the 1995 round, women were asked who had beaten them in the last 12 months and how often. In subsequent rounds, the survey asked separately about specific types of husband violent behavior, such as punches, slaps, strangling etc.

We consider three ways of constructing the domestic violence indicator, which we label as *Strict*, *Preferred* and *Comprehensive*. All three take value 1 if the woman reported being beaten at least once by her husband in the last 12 months in the 1995 survey. As for the 2005 survey, the *Strict* definition restricts the types of violence that could be considered akin to the wording "beating" to the cases of punching/hitting, kicking/dragging or to attempts to strangle/burn the respondent. The second way (*Preferred* definition) considers as being "beaten" also women who had been slapped or whose arm had been twisted or who were threatened or attacked with a knife or other weapon. Finally, the third way (*Comprehensive* definition) also includes women answering affirmatively to the question "Did your husband push you, shake you, or throw something at you in the last 12 months?". We think the second way of defining the violence dummy includes the types of violence that are most similar to the wording "beaten" included in the 1995 survey, so we will adopt this definition in most of the analyses.[18] In the data appendix we show that non-response rates to IPV questions are extremely low (less than 2% on each question) and that differential attrition by treatment status due to missing IPV data is not a concern.

To construct proxies for women's agency within the couple, we use questions asking about their weight in household decisions. We construct two dummies, *Visits* and *Budget* taking value 1 if the woman reports that she only, or both her husband and she jointly, have the final word respectively over decisions to visit the woman's family and relatives and over how to spend the household budget. They take value 0 if the woman reports that her husband only is the one having the final say over such matters. As a robustness check, in the appendix we also consider slightly different questions to define the variable *Budget*. We refer to the data appendix for a more detailed description of the variable construction and the wording of the survey questions.

The statistics reported in Table 1 show that domestic violence is quite common in Egypt. Depending on the survey year and definition of violence we adopt, between 7% and 15% of women report episodes of intimate partner violence in the last 12 months.[19] A little over half of the respondents have a say over how to spend the household budget, while the share of women who can decide over visits to family and friends doubled, from 38% to 76%, over the time frame considered.

Both when looking at domestic violence and measures of decision making power, women in the treatment group seem to fare slightly worse than women in the control group. The two groups of women look nevertheless quite comparable in terms of observable characteristics such as education, age at first marriage, urban status, or whether they moved to a different town or place of residence after marriage (variable *Moved*). Women in the control group and their husbands are a few years older on average than women in the treatment group and their husbands. This is expected, as the age of the youngest child is positively correlated with mother's age, although less so than the age of the first born. To avoid confounding the effect of the reform with age-specific secular trends, we will control for age and age-specific time trends in our analysis.

[14] In this equation we group the woman's age into 10 categories, since the age range is larger: 16–25, 26–30, 31–35, 36–40, 41–45, 46–50, 51–55, 56–60, 61–65, 66–70. We use 7 categories to group the husband's ages, which are however more dispersed: 18–30, 31–40, 41–45, 46–50, 51–60, 61–90, 91–100.

[15] We control for a time trend for each family size, that is, for binary indicators for families with 1 child, 2 children, 3 children etc. Results are robust to using coarser ways of aggregating family size.

[16] This is women whose youngest child is a daughter between 7 and 17 years old or a boy between 5 and 15 years old.

[17] Domestic violence questions were asked only to the subsample of women selected for this special survey module. In the data appendix we show that women selected for the domestic violence module are similar to those not selected and we explain more in detail the criteria for inclusion in each survey round.

[18] In particular, while the first definition might miss some less severe forms of IPV that are however similar to the wording "beaten", we see the wording "push you, shake you or throw something at you" as having a quite different meaning from the word "beaten". In fact, while we would expect domestic violence to be lower in 2005 than in 1995 due to secular trends, when adopting this definition of domestic violence the share of women reporting IPV in the last 12 months is actually higher in 2005 than in 1995, as shown in the summary statistics in Table 1.

[19] The 1995 questionnaire also asked whether women had ever been beaten by their husbands, and this figure is higher than 30%.

Table 1
Summary statistics - DHS.
Source: Authors calculations. Demographic and Health Survey (DHS): Egypt 1995 (DHS-III), Egypt 2005 (DHS-V).

	Mean (1)	Pre-reform (2)	Post-reform (3)	Control (4)	Treated (5)
Panel A: Marital status					
Married	0.88	0.87	0.89	1	1
Divorced/Separated	0.03	0.02	0.04	0	0
Widowed	0.09	0.11	0.08	0	0
Panel B: Demographics					
Respondent's age	39.91	39.75	40.02	42.83	38.31
Respondent's years of education	5.1	4.3	5.67	4.86	5.23
Husband's age	47.32	47.76	47.01	50.46	45.61
Husband's years of education	6.76	6.06	7.26	6.41	6.95
Number of children	4.03	4.25	3.87	3.98	4.06
Age at first marriage	18.67	18.32	18.92	18.46	18.78
Moved after marriage	0.31	0.39	0.26	0.29	0.32
Treated	0.65	0.7	0.61	0	1
Urban	0.49	0.52	0.47	0.51	0.47
Number of household members	6.57	6.81	6.39	6.21	6.76
Panel C: Dependent variables					
Beaten by husband last year - Strict	0.1	0.13	0.07	0.09	0.11
Beaten by husband last year - Preferred	0.12	0.13	0.12	0.11	0.13
Beaten by husband last year - Comprehensive	0.14	0.13	0.15	0.12	0.14
Visits - Has dmp over visits	0.67	0.38	0.76	0.69	0.65
Budget - Has dmp over budget	0.57	0.54	0.58	0.57	0.57
Worked last year	0.25	0.2	0.28	0.25	0.25
N	8595	3573	5022	3038	5557
N (domestic violence module)	3139	1591	1548	1073	2066

5.2. Egypt Labor Market Panel Surveys

To carry out our analysis on children's education and on additional labor force participation outcomes, we rely on the Egypt Labor Market Panel Survey (ELMPS) using the 1998, 2006, and 2012 rounds. Although the ELMPS can be used as longitudinal data for a subset of the respondents, in our analysis we employ them as repeated cross sections in order to retain the maximum sample size.

This nationally-representative data provides information on labor market and human development outcomes, tracking all individuals of the household at all ages and their relationship to each other. This allows us to identify children's enrollment status and education level for each woman, which is not possible in the DHS data.

Another relevant difference with respect to how we use the DHS sample is related to the fact that the first post-reform year is 2006, while in the DHS we use questionnaires made in 2005. In 2005 the custody cutoff was raised to 15 years old for both sons and daughters (Law 4 of 2005). While this does not affect the 2005 wave of the DHS data, which asks questions retrospectively about the previous 12 months, it changes how we should define treated women after 2006. However, it also allows us to retain in the analysis the ELMPS year 2012 since no other changes to the law occurred between 2006 and 2012. In this way, we are able to observe individuals 2 years prior to the reform, and 6 and 12 years after it.

Our main outcome measure for women's labor force participation is the dummy *Currently Working*, that takes value of 1 if the woman declared to be employed during the 3 months previous to the interview, and 0 otherwise.[20] Alternatively, we use the average number of days worked over a week, conditional on being employed (variable *Days Worked*) as an intensive margin measure.

In Table 2, panel A and B, we show descriptive statistics on the ELMPS sample of women. As for the DHS data, we consider the sample of currently married mothers whose youngest child's age is within a ±5 years window from the custody cutoff.[21] Female employment is relatively low at 48%. It increased in the first two years of interest and then contracted in 2012. Treated women are slightly more likely to be employed. Overall, there does not seem to be a sharp difference in observable characteristics across the two groups of women. As in the DHS sample, the main difference is the age of women and their husbands, treated couples being few years younger than controls.

To study investments in children's human capital, we construct the following outcomes for each child born to women in our analysis sample: the dummy *Enrolled*, equal to 1 if the child was enrolled in school on the date of the interview, the dummy *Ever Worked*, equal to 1 if the child reported to have ever worked and the dummy *Ever Married*, equal to 1 if the child was married at the time of the survey.[22]

We measure these outcomes on the subsample of children between 15 and 25 years old, as detailed in Section 4. Summary statistics for the children sample can be found in panels C and D of Table 2. As expected, treatment children are on average younger than control ones, since they have younger siblings, and are thus more likely to be enrolled in school. Also, treated children have more brothers and sisters on average, since they belong to families where there is more variation in children's age. However, we will directly account for these age-driven differences by always including age fixed-effects, age-specific time trends, and further controlling for parents' age and trends in the number of children within the household. No differences can be noted on other variables that are less likely to be correlated with the age of the youngest child, such as mother's age at first marriage, parents' education, and urban status.

6. Effect of *Khul* on women's empowerment

In this section we present evidence that the reform reduced domestic violence and seems to have weakly increased women's agency

[20] We include both unemployed and women out of the labor force in the residual category, since in Egypt the bulk of women not working is represented by housewives (56.09%) who are categorized as individuals out of the labor force.

[21] This means that we are considering women whose youngest child is between 10 and 20 years old.

[22] We prefer to use a variable asking if the child has ever worked as opposed to a variable reporting the current working status since youngsters tend to have informal jobs with high turnover.

Table 2
Summary statistics - ELMPS.
Source: Authors calculations. Egypt Labor Market Panel Surveys (ELMPS): 1998, 2006, 2012.

	Mean	Pre-reform (1998)	Post-reform (2006)	Post-reform (2012)	Control	Treated
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: demographics- women's sample						
Married	0.92	0.93	0.92	0.90	1	1
Divorced	0.01	0.01	0.01	0.01	0	0
Widowed	0.07	0.06	0.07	0.09	0	0
Age	45.45	44.63	45.71	45.75	49.81	42.44
Education	5.64	5.16	5.37	6.20	5.07	6.04
Husband's age	52.31	52.17	52.47	52.26	56.49	49.42
Husband's education	2.96	2.97	2.90	3.02	2.85	3.04
Age at first marriage	20.16	19.54	20.16	20.55	20.28	20.08
Number of children	2.81	3.02	2.88	2.61	2.49	3.03
Treated	0.59	0.64	0.56	0.58	0	1
Urban	1.44	1.31	1.44	1.53	1.41	1.46
Number of household members	5.67	5.99	5.82	5.32	5.31	5.92
Panel B: dependent variables- women's sample						
Currently working	0.48	0.47	0.54	0.43	0.45	0.49
N. Days worked	5.89	5.90	6.04	5.74	5.86	5.91
N	5381.00	1381.00	1863.00	2137.00	2197.00	3184.00
Panel C: demographics- children's sample						
Age	19.33	18.88	19.39	19.54	19.71	19.02
Sex	0.43	0.41	0.44	0.44	0.41	0.45
Treated	0.55	0.60	0.53	0.54	0	1
Education level	2.27	2.13	2.41	2.22	2.48	2.11
Mother's age	46.39	45.92	46.40	46.66	49.36	43.96
Mother's education	4.84	4.39	4.46	5.50	4.82	4.86
Father's age	53.28	53.50	53.17	53.25	56.01	51.05
Father's education	2.77	2.79	2.65	2.86	2.78	2.76
Mother's age at first marriage	19.72	19.12	19.66	20.16	20.16	19.37
N. children in the household	3.86	4.08	4.08	3.51	3.28	4.34
Urban	1.47	1.34	1.47	1.56	1.44	1.50
Number of household members	6.25	6.61	6.39	5.91	5.69	6.72
Panel D: dependent variables- children's sample						
Enrolled	0.51	0.50	0.48	0.54	0.43	0.57
Ever worked	0.30	0.29	0.31	0.29	0.34	0.26
Ever married	0.09	0.02	0.09	0.14	0.08	0.10
N	13959	3659	5108	5192	5905	8054

over households decisions. We also discuss the effects of the reform on women's labor force participation.

Validating the parallel trend assumption Unfortunately we cannot plot pre-trends of our agency and domestic violence outcomes since we only have one data-point (1995) before the reform. We can, however, use similar questions from the 1988 and 1992 surveys asking about attitudes towards women's decision-making (rather than realized women's decision-making) to visually inspect pre-trends on the variables *Visits* and *Budget*.[23] Fig. 1 plots the coefficients δ_t of Eq. (2) using as dependent variables the agency measures "extended" to harmonize the questions across survey rounds. That is, we use attitudes towards women's decision-making in 1988 and 1992, when measures of actual women's decision-making are missing.[24] Pre-trends are remarkably parallel. Yet, because the agency questions do not seem directly comparable before 1995, we do not include earlier years in the main analysis.

Main estimates of effect on domestic violence, agency and labor force participation We start by looking at the effect of the introduction of *Khul* on outcomes reflecting women's standing within the couple: reported domestic violence in the previous year, women's decision making power and working status. Table 3 reports estimates of Eq. (1) on the three alternative definition of the domestic violence indicator that we described in Section 5, the *Visits*, and *Budget* dummies and an indicator for women who worked in the past 12 months.

The set of controls we use throughout includes the respondent's and her husband's education, the household urban status and wealth quintile, and a set of wife and husband's age group pair fixed-effects. In the second column of each dependent variable we add controls for age-at-first-marriage time-trends, assuming that age-at-first-marriage is a proxy for how conservative the values of the household are.[25] The reason for including these trends is that, among women of the same age, those with older children have become pregnant at a younger age, which might signal that the couple holds more conservative attitudes

[23] The earlier surveys asked the respondent questions like *"Who should have the final say over matter X? The husband, the wife or both jointly?"*, rather than directly asking *"Who has the final say over matter X in your household?"*, as done in more recent years. These questions about attitudes were asked to both the husband and the wife separately in the 1992 survey and only to women in 1988.

[24] We assume that husband's attitudes are good proxy for the amount of decision making power the woman actually holds in the years in which realized outcomes are missing. That is, for the 1992 survey the dummies *Visits* and *Budget* reflect who the husband thinks should have a final say over those issues, while for the 1988 survey we use the wives' attitudes, lacking any better measure of actual behavior. We refer to the data appendix for more details on the construction of these variables.

[25] We assume that women who got married at a younger age belong to more conservative and traditionalist families. All the women in the sample got married before the reform, so age of marriage is not a bad control.

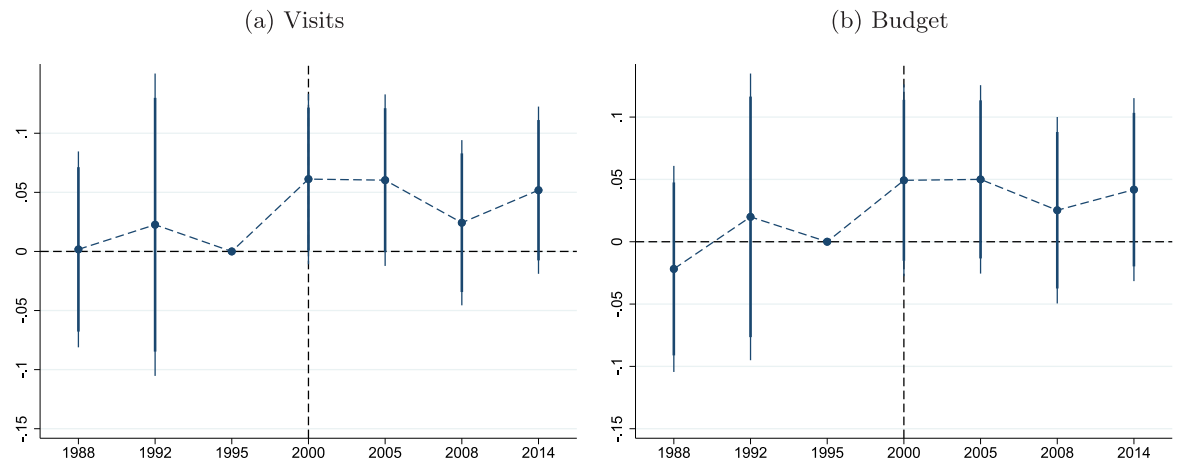

Fig. 1. Women's decision-making event study. **Notes:** Figure plots the event-study coefficients δ_k in Eq. (2) and 95 percent confidence intervals, from regressions using as dependent variables women's agency over visits (Panel A) and household budget (Panel B).
Sources: 1988, 1992, 1995, 2000, 2005, 2008 and 2014 DHS, Egypt.

Table 3
Effect of Khul on domestic violence and women's agency.
Sources: Data sources: 1995 and 2005 DHS, Egypt.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Domestic violence	Violence								
	Strict			Preferred			Comprehensive		
Post*Treated	−0.077***	−0.072***	−0.070**	−0.072**	−0.069**	−0.077**	−0.054	−0.049	−0.056
	(0.028)	(0.028)	(0.030)	(0.032)	(0.032)	(0.034)	(0.033)	(0.033)	(0.035)
Post	0.032	0.065**	0.010	0.096***	0.173***	0.193	0.107***	0.176***	0.168
	(0.022)	(0.031)	(0.138)	(0.026)	(0.039)	(0.189)	(0.026)	(0.039)	(0.188)
Treated	0.041**	0.038*	0.035*	0.036*	0.030	0.033	0.036*	0.030	0.032
	(0.020)	(0.020)	(0.021)	(0.020)	(0.020)	(0.021)	(0.020)	(0.020)	(0.021)
N	3101	3101	3101	3098	3098	3098	3099	3099	3099
Mean of Dep. Var.	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes
Panel B: Women's agency									
	Visits			Budget			Worked (last year)		
Post*Treated	0.059	0.062*	0.053	0.064*	0.068*	0.067*	−0.029	−0.030	0.014
	(0.036)	(0.036)	(0.038)	(0.035)	(0.035)	(0.037)	(0.022)	(0.022)	(0.023)
Post	−0.062**	−0.042	−0.015	0.287***	0.283***	0.376***	0.027	0.154***	0.165**
	(0.030)	(0.043)	(0.182)	(0.031)	(0.042)	(0.133)	(0.019)	(0.028)	(0.073)
Treated	−0.047	−0.051	−0.043	−0.063*	−0.064**	−0.062*	0.018	0.009	−0.013
	(0.032)	(0.032)	(0.034)	(0.032)	(0.032)	(0.033)	(0.017)	(0.017)	(0.018)
N	6471	6471	6471	6542	6542	6542	8582	8582	8582
Mean of Dep. Var.	0.56	0.56	0.56	0.40	0.40	0.40	0.18	0.18	0.18
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The dependent variables are: the strict, preferred and comprehensive definitions of the dummy *Violence*, that is, binary indicators for domestic violence experienced in the year prior to the interview as defined in Section 5 (Panel A); *Visits* (columns (1)–(3), Panel B) and *Budget* (columns (4)–(6), Panel B), which are dummies indicating respectively if the woman has final say on decisions to visit relatives and spend household budget and *Worked (last year)*, a dummy indicating women who worked in the prior 12 months. *Post* is a dummy indicating years after the reform, *Treated* is a dummy indicating mothers of sons younger than 10 years old, or of daughters younger than 12 years old. Controls always include: respondent's and her husband's education, dummies for the household urban status, wealth quintiles, and for a set of wife and husband's age group pairs. Columns (2), (5) and (8) add age at marriage time trends. Columns (3), (6) and (9) add respondent's age time trends. The estimation sample restricts to married women whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

and values as regard gender roles. Once we control for the respondent's age, the age of the youngest child (and assignment to treatment) could thus be correlated with traditionalism. If more traditional attitudes are slower to change, the outcomes of interest might be on diverging trend across women in the control and treatment group. Controlling for age-at-marriage time trends attenuates this concern.

Finally, since women in the control group are on average 4 years older than women in the treatment group, we might be worried that our diff-in-diff estimates are capturing differences in the speed of secularization across cohorts. That is, the outcomes of women 4 years apart might be more aligned in 2005 than in 1995 only as a result of secularization. To address this threat to identification, in the third column of each outcome we introduce time trends varying by the respondents' age group.

Overall, the signs of the difference-in-differences coefficients suggest that domestic violence declined and women's agency increased for the treatment group in comparison with the control group after the *Khul* reform.

The effect of the reform on domestic abuse is large, negative and statistically significant when adopting the first two definitions of IPV. When looking at our preferred definition, women in the treated group experience a decline in domestic violence of about 7 percentage points

relative to women in the control group. This effect is very large, corresponding to a reduction of domestic violence of about 58% with respect to the pre-period average (12%). The estimates also show that treated women at baseline were on average more likely to suffer from domestic abuse. This is in line with previous studies about domestic violence, documenting how the presence of young children within the household is often correlated with tensions in the couple and a larger incidence of partner abuse (Fantuzzo et al., 1997). Results are however somewhat sensitive to the definition of violence we adopt: when using the *Comprehensive* dummy, results are negative and of slightly lower magnitude, but they are no longer statistically significant.

The results on our proxies of women's bargaining power point in the same direction but statistical significance is weak. The coefficient of the interaction term is always positive, at around 6 percentage points, both when looking at decisions related to visits and to the household budget.[26] In Table A.1 we consider slightly different ways of defining the variable *Budget*, which include answers to questions asking about decision making power over purchases for daily needs, and the results are strikingly similar.[27]

Finally, Table 3 shows a null effect on women's labor force participation. In the first three columns of Table A.2, we run the same analysis but we restrict the sample to women selected for the domestic violence module, showing that even on this subsample we find no effect on labor force participation.

The rest of Table A.2 examines the effect of the reform on women's labor market outcomes using the ELMPS data. We find negative but marginally significant or insignificant effects on an indicator for being currently employed. However, conditional on being employed, the reform seems to have increased the number of days of work per week (variable *Days Worked (week)*). This corresponds to a 6 percent increase in the number of days worked with respect to the pre-reform mean in the sample of working women.

Overall, there does not seem to be a clear effect on women's labor force participation. The coefficients are usually insignificant and they change in sign and magnitudes across the different samples. If anything, results for the ELMPS sample seem to weakly suggest that the introduction of unilateral divorce for women decreased their labor force participation, while increasing work effort for the subsample of working women. These findings seem more aligned to studies showing that the adoption of divorce laws deemed favorable to women reduces wives' labor supply in a household-bargaining model (Chiappori et al., 2002; Voena, 2015). In Egypt, El-Enbaby et al. (2019) find that a large conditional cash transfer reduced women's paid employment. This might consistently reflect preferences of women in Egypt, where women face a social cost from working and prefer not to work outside the home (United States Agency for International Development and National Council for Women, 2009). For instance, sexual harassment on the work place is a widespread practice and a major impediment to women joining the labor force, especially in the private sector (Constant et al., 2020). The fact that we do not observe a reduction in the number of hours worked by working women, but the opposite, might also suggest that, being an extremely selected subsample, working behaviors in this case follow the general pattern detected in most of the studies on legalization of divorce and female labor supply in Western countries (Gray, 1998; Kneip et al., 2014).

Finally, inspecting the event study in Fig. A.3, it seems that labor market outcomes start diverging only 12 years after the *Khul* reform was passed. For this reason, we are skeptical about interpreting our results on labor force participation as being driven by the Khul reform. In this context, possible confounding factors could be given by other labor market shocks, such as the Arab Spring of 2011, to which women in the treatment and the control group might have responded in different ways, as mothers' working decisions often tend to depend on their children's ages.[28] For these reasons, in the rest of the paper we will focus on domestic violence and the other proxies of women's agency.

Heterogeneity by treatment intensity Our main empirical strategy assigns women to the treatment group if they would keep the custody of at least one of their children in case of divorce. However, within this group, some mothers would keep the custody of all their children while others only of some of them if they asked for divorce. The latter are potentially less affected by the reform than women who would keep the custody of all their children, if losing the custody of even just one child represents a high cost for a mother.

We split the treatment group into two subgroups according to whether the woman would have kept the custody of all her children (high intensity treatment group) or only of some of them (low intensity treatment group) in case of divorce and estimate a modified version of Eq. (1):

$$Y_{it} = \alpha + \beta\ Post_t + \gamma_1\ Keep\_all_i + \gamma_2\ Keep\_some_i + \\ + \delta_1\ Post_t \cdot Keep\_all_i + \delta_2\ post_t \cdot Keep\_some_i + X'_{it}\lambda + \epsilon_{it} \quad (5)$$

where, *Keep_all* takes on value 1 if the woman would be able to keep all of her children after a divorce (i.e. her oldest child's age is below the custody cutoff) and 0 otherwise, while *Keep_some* takes on value 1 if the woman's oldest child's age is above the custody cutoff but the youngest child's age is below it, so that she would be able to keep the custody of only some of her children if she divorced. Our prediction is that the coefficient δ_1 should be larger in absolute terms than δ_2, as bargaining power should increase the most for women in the *Keep_all* group as a consequence of the reform.

Table 4 presents the results of this analysis, showing that women in the "high intensity" treatment group are those experiencing the largest decrease in domestic violence, consistently with the view that they are able to make more credible divorce threats. Results are robust across the three definitions of the *Violence* outcome. Table A.3 shows that results are robust to the exclusion of controls and age time trends. The results on the decision making variables are less clear cut: as before, the estimates of the interaction terms are quite imprecise and the effect of the reform on these variables does not seem significantly larger for women with the lowest divorce costs. Finally, these results weakly suggest that the reform might have decreased labor force participation for women in the high intensity treatment group, consistently with the results using ELMPS data in Table A.2.

Heterogeneous effects by women and household characteristics Most of the effects we observe are on our measures of domestic violence. We zoom-in on this outcome and explore whether the reform had heterogeneous effects according to some covariates of interest. Fig. 2 plots the coefficient of the difference-in-difference interaction term estimated on different subsamples.[29]

The introduction of *Khul* seems to increase more the bargaining power of women whose youngest child is a boy. We interpret this result

[26] The specific measures used to proxy for female agency might not accurately reflect household decision-making, thus weakening our findings. This is even more relevant in male-dominated contexts with entrenched gender norms such as rural Egypt, where male decision-making is the socially accepted norm, and responses to questions on intra-household decision making may suffer from a high degree of desirability bias. Indeed, in mixed-method evaluations comparing qualitative and quantitative research, qualitative methods often identifies changes in household decision-making and perception of household roles that are not picked up by more standard quantitative survey tools (Bonilla et al., 2017).

[27] We refer to the data appendix for a detailed description of variable definitions.

[28] For instance, Hendy (2015) argues that between 1988 and 2012 female labor force participation in Egypt decreased, primarily in response to the contraction of public sector hiring that occurred over the 1990s. She contends that Egyptian women, especially married ones, tend to prefer inactivity rather than private sector jobs. She finds indeed that married women continue to

Table 4
Heterogeneity of effect of Khul by treatment intensity.
Sources: Data sources: 1995 and 2005 DHS, Egypt.

	Violence			Visits	Budget	Worked (last year)
	Strict	Preferred	Comprehensive			
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Keep_all	−0.195***	−0.195***	−0.190***	0.040	0.088	−0.089*
	(0.066)	(0.071)	(0.073)	(0.078)	(0.075)	(0.050)
Post*Keep_some	−0.062**	−0.070**	−0.048	0.068*	0.051	0.020
	(0.030)	(0.035)	(0.036)	(0.037)	(0.039)	(0.023)
Post	0.152	0.325	0.321	0.407***	−0.047	0.278***
	(0.151)	(0.201)	(0.201)	(0.150)	(0.193)	(0.086)
Keep_all	0.079	0.079	0.080	−0.064	−0.084	0.004
	(0.052)	(0.052)	(0.052)	(0.071)	(0.064)	(0.038)
Keep_some	0.033	0.030	0.029	−0.062*	−0.040	−0.014
	(0.021)	(0.021)	(0.021)	(0.034)	(0.034)	(0.017)
N	3101	3098	3099	6542	6471	8582
mean depvar	0.10	0.10	0.10	0.40	0.56	0.18
Woman's age of marr. time trend	Yes	Yes	Yes	Yes	Yes	Yes
Woman's age time trend	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variables are the same as the ones used in the previous table. *Post* is a dummy indicating the years after the reform, *Keep_all* is a dummy indicating if the woman would keep all of her children after a divorce (i.e. her oldest child's age is below the custody cutoff), and *Keep_some* is a dummy indicating if the woman would keep only some of her children after a divorce (i.e. her youngest child's age is below the custody cutoff but her oldest child's age is not). Controls include respondent's and her husband's education, dummies for the household urban status, wealth quintiles, and for a set of wife and husband's age group pairs, age at marriage time trends and respondent's age time trends. The estimation sample restricts to married women whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

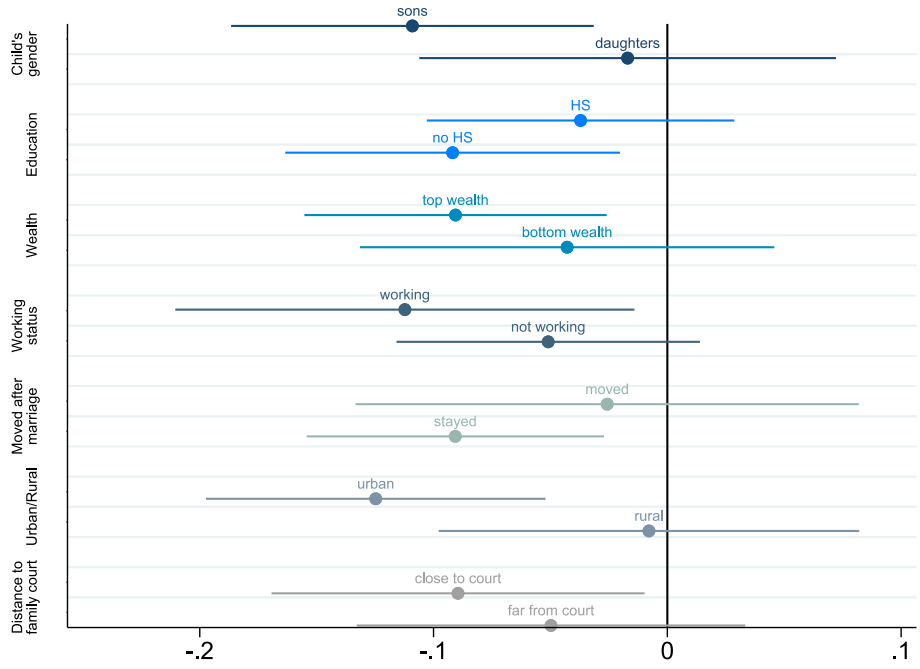

Fig. 2. Heterogenous effects on domestic violence. **Notes:** The figure plots the coefficients and relative 95 confidence intervals for the regression of domestic violence on the difference-in-difference interaction term estimated in Eq. (1) on a series of sub-samples split by the following characteristics: gender of the youngest child (female or male); education (below or above high school education); wealth (below or above the top two quintiles of wealth); working status (employed or unemployed); arrangement after marriage (if woman moved or not after marriage); urbanization (rural or urban); distance from family courts (further or closer than 5 km from nearest family court).
Sources: 1995, and 2005 DHS, Egypt.

in view of widely documented son preferences, often held by fathers, among parents in developing countries (Behrman and Deolalikar, 1990; Lafortune and Lee, 2014; Carranza, 2014; Thomas, 1990; Duflo, 2003). Son preferences may manifests through a stopping rule, so that families with a daughter as a last born may be less likely to exhibit son bias and also be more progressive in other dimensions. In addition, if parents care more about sons than daughters, the threat of losing the custody of a girl should matter less than the possibility of losing a boy.

We find that violence decreases more for women with less than a high school education, and for women in the top two quintiles of the wealth distribution, although neither of these differences is statistically significant. The heterogeneous effect by wealth supports the view that only wealthier women are able to make credible divorce threats, as *Khul* comes at the cost of repaying the dower and forfeiting all financial

work after marriage if they are employed in the public sector, while they quit at the time of marriage if they work in the private sector.

[29] Column 3 of Table A.4 reports the estimates of a triple difference-in-difference regression in which the third dimension used in the interactions is the characteristic of interest (such as wealth, urban status etc.). The other two columns are the diff-in-diff estimates computed on the separate samples shown in Fig. 2.

rights, including alimony, the marital house and the deferred part of the dower.[30] Similarly, the reform seems to have decreased domestic violence more for working women, with the caveat that working status is potentially a bad control.

We also find that women who have moved away from the place where they grew up after marriage are less affected by the divorce reform.[31] This suggests that the possibility of relying on strong social networks is important to be able to make credible divorce threats.

Finally, we find that the introduction of *Khul* had a significantly larger impact on domestic abuse in urban households. The triple interaction term of the difference of the reform effect on rural vs. urban household is marginally significant despite the precision issues (see Table A.4). Part of this difference might be explained by access to family courts: controlling for urban status, treated women living less than 5 km away from family courts see a larger decrease in domestic violence than women living further away, even though the difference is not statistically significant.[32]

Heterogeneous effects by the youngest child's age Women whose youngest child's age is very close to the custody cutoff might be less affected by the reform. The introduction of *Khul* should in fact affect mothers of very young children the most as, in case of divorce, they would retain the custody of children and the right to live in the marital house for a longer time, making their divorce threats more credible.

We first check whether results are robust to the exclusion of families with children very close to the cutoff (±1 year from the cutoff), and report results in Table A.5. As expected, results show larger effects on violence and decision making over the household budget, including the comprehensive definition of violence. Effects on *Visits* and labor force participation are similarly small and non significant.

Lastly, we explore whether the impact of the reform on domestic violence differs according to the youngest child's age, when the child is younger than the custody cutoff. The impact of the reform could indeed be decreasing in the age of the youngest child, up to the age at which custody is transferred to the father, while divorce threats of women with children above the cutoff should be equally little credible. To explore this hypothesis we estimate the following equation:

$$Violence_{it} = \alpha + \beta\ Post_t + \sum_{k=-5}^{5} \gamma_k\ D_k + \sum_{k=-5}^{5} \delta_k\ Post_t \cdot D_k + X'_{it}\lambda + \epsilon_{it} \quad (6)$$

where D_k is a dummy equal to 1 if the woman's youngest child's age is distant k years from the custody cutoff, e.g. D_{-3} identifies women whose youngest child is 3 years younger than the custody cutoff; $k = 0$ is the last year in which custody remains to the mother. Our hypothesis is that the coefficients δ_k should be increasing in k for $k \in [-5, 0]$ and be constant thereafter, as mothers of children older than the custody threshold are not differentially affected by the reform.

[30] We note that, ex-ante, we might have expected results to go in the opposite direction for two reasons: first, wealth refers to the household wealth, rather than to the woman's wealth. Divorce might be more costly for women living in wealthier households as they have more to give up (a larger house, a larger deferred dower, a richer lifestyle). Second, we might expect a smaller difference in outcomes between treatment and control group among wealthier mothers, as the financial implications of losing house and alimony once children surpass the age cutoffs are less dramatic, and they might be able to make credible divorce threats regardless of children's custody rights.

[31] We use age at marriage and a question asking how long the woman has been living in her current town/city of residence to identify women who have moved from the place of residence of their childhood after marriage.

[32] To compute distance to the nearest family court we use the GPS respondent's location collected by the DHS and we manually coded the coordinates of family courts in Egypt. We found the list of Egyptian family courts on a website curated by the UNDP Egypt that can be accessed here: http://ladsegypt.org. We then used Google maps to manually look for and record the geographic coordinates.

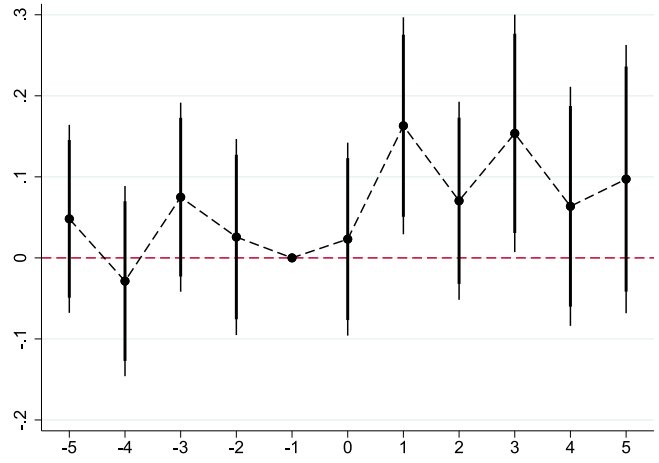

Fig. 3. Effect of the reform on domestic violence by age of the youngest child. **Notes:** The x axis shows the year difference between the youngest child's age and the age cutoff used to assign custody to the father, while the y axis shows the coefficients of the interaction terms estimated in Eq. (6) with their confidence intervals. The thinnest line corresponds to a 5 percent confidence interval and the thickest one to a 10 percent confidence interval. We take as reference the last year when the custody of the child remains to the mother.
Sources: 1995, and 2005 DHS, Egypt.

Fig. 3 only partially confirms this hypothesis: the coefficients δ_k are lower for $k \in [-5, 0]$, that is for mothers of children younger than the custody cutoffs, than they are for $k \in [1, 5]$, that is for mothers of children older than the custody cutoff. However, there does not seem to be much of an increasing trend up to $k = 1$, but rather the effect seems uniform for all women in the treatment group, regardless of the age of the youngest child. Moreover, power is low and confidence intervals are large, which makes these effects almost never statistically significant from zero, despite the magnitude of the point estimates.

Additional robustness and placebo checks In Table A.6 we look at difference-in-differences effects on women's *attitudes* towards domestic violence, to rule out the concern that the observed reduction in domestic violence is driven by diverging trends in social norms that might affect self-reporting differently for women in the treated and control group. In theory, we should expect that the divorce reform did not change social norms and attitudes differently for treated and control women. In both the 1995 and the 2005 surveys, women were asked whether they agreed that husbands are justified beating their wives when they burn food, when they neglect children or when they refuse to have sex. The dependent variables used in this table are dummies indicating agreement with such statements. Attitudes do not change over time differently for treated and control women, except for the marginally significant coefficient on the dummy "burns food" in the third column of panel B.

Next, we run a placebo test on women in the control group, who should not be affected (or should be equally little affected) by the reform, regardless of the age of their last-born. We test whether the trends of our dependent variables are parallel for women with younger or older children, among women with children older than the custody cutoffs. To implement this test, we estimate Eq. (1) on the sample of women whose youngest child is older than the real custody cutoff, but this time we assign respondents to a placebo treatment and control condition on the basis of a higher age cutoff. We set the placebo cutoffs 5 years after the actual cutoffs (15 years old for women whose youngest child is a boy and at 17 for women whose youngest child is a girl) and restrict the estimation sample to women whose youngest child's age is

within an age window of ±5 years from the placebo age cutoff, akin to how we defined the main analysis sample.[33]

Table A.7 presents the results of the placebo check. Sample size is lower and this decreases precision even further. However, the point estimates of the difference-in-differences term for domestic violence are generally lower in absolute value than the ones in Table 3 and they are never statistically different from zero, while our main results point towards a large, negative, and significant effect on domestic violence. If anything, the placebo estimates on domestic violence are positive, suggesting perhaps that, in the absence of the reform, domestic violence was trending up faster for women with younger children, which would run counter finding a negative impact of the divorce reform on domestic violence.

While the null placebo results on *Budget* are in line with what we expected to find, the results on *Visits*, are similar in sign and magnitude to the main results, although not significantly different from zero. This calls for a word of caution in interpreting our main results as it might suggest that the decision making power of mothers with relatively younger children may have been trending up faster over that period of time relative to mothers of older children.

Finally, Table A.8 shows that results are robust to the inclusion of women whose last born is within a ±10 years window around the custody cutoff, rather than restricting to a 5 years window as in the rest of our analyses. For each outcome we report results from our preferred specification.[34] The sign and magnitude of the coefficients is essentially unchanged by the expansion of the sample. This is important in light of the concerns around potential strategic fertility decisions.

In fact, by focusing on the 5 years window around the custody cutoff, we are excluding from the analysis women who had children after the reform. It is possible that a selected group of women might have tried to strategically space-out births or have more children after the reform to continue to have a credible outside option through divorce. Selection out of our sample would affect the most the composition of our treatment group, as fertility is generally higher for younger women, with younger children. Therefore, restricting the sample to women with children close to the cutoff might result in a treatment group that is more positively selected and this could explain why we find a larger reduction in domestic violence in this group after the reform. Re-including these women in our analysis sample, would probably bias the composition of the treatment group in the opposite direction. Yet, results using this extended sample are incredibly robust, suggesting that selection due to strategic fertility choices does not seem to be a concern.

Could results be explained by selection out of marriage? Because our analysis sample is restricted to married women, selection out of marriage could be one of the channels driving our results, if treated women with the worst outcomes exit their marriages at higher rates after the introduction of *Khul*. To explore this possibility, we check whether divorce rates in our survey data change differently for women with children below or above the custody cutoff before and after the reform.

Fig. A.1 plots the average divorce rate among mothers whose youngest child is within a ±5 years window from the custody cutoff. In the DHS data, divorce rates are higher in the control group than in the treatment group. This is plausible since control group women are older on average and because we cannot distinguish between men-initiated and women-initiated divorce in our data. Yet, the treatment group divorce rate seems to catch up in the last years we observe. The corresponding event study, shows a small, but not significant, increase in divorce rates of treated women relative to control ones starting from the year in which the reform was introduced. Divorce rates in the ELMPS data are somehow lower for both treatment and control group and the event study coefficients are small and not significant.

Because of the very small and non significant effects we find on divorce rates, we believe that selection out of marriage is not the leading channel driving our results. Reallocation of bargaining power or violence deterrence due to increased legal accountability within married couples seem more plausible explanations. In Table A.9, we compute a lower bound on the effect that can be attributed to changes happening within married couples in the spirit of Lee (2009). Assuming that married women in the treatment group after the reform are more "positively selected" than married women in the control group (more agency and less domestic violence), we trim the control group in the post-reform period dropping observations with the lowest agency residuals or highest violence residuals and we estimate Eq. (1) on this selected sample.[35] Estimates are very similar to the ones in Table 3, confirming the idea that the effects of the reform cannot be explained by selection out of marriage.

7 Effect of *Khul* on investments in children

Motivated by past research showing that women invest more in children's education and household goods (Quisumbing and Maluccio, 2003; Rubalcava et al., 2009; Rangel, 2006; Schady and Rosero, 2008; Martínez A., 2013; Heggeness, 2020), in this section we study the effects of the reform on children and investments in their human capital.

Fig. 4 plots the event-study coefficients δ_t of Eq. (2) for the children dependent variables used in this section, namely an indicator for being enrolled in school at the time of interview, an indicator for having ever worked, and an indicator for being married. Importantly, since children in the control group are older on average, in our regressions we control for children's age and age time-trends, to avoid confounding age effects with the effect of the reform. While pre-trends are parallel for all outcomes, the figure suggests that the probability of being enrolled in school increases, the probability of having worked decreases and the probability of being married decreases for children in the treatment group relative to children in the control group after, relative to before the reform.

Table 5 shows pooled difference-in-differences results from estimating Eq. (4). The first column of each outcome controls for the baseline set of linear controls presented in the previous subsections and time trends in the number of children within the household. We include this additional trend to take into account the significant difference in the size of the offspring across treated and control households due to how we construct the sample and assign treatment status.[36] As done with the analysis of women's outcomes, in the second column, we further introduce age at marriage time trends and the third column adds mother's age time trends.

[33] The resulting placebo treatment group is thus made of women whose youngest child's age is between 10 and 14 if the last-born is a boy and between 12 and 16 if the last-born is a daughter and the placebo control group is made of women whose last born is between 15 and 19 if boy and between 17 and 21 if girl.

[34] Results are virtually unchanged when removing controls and time trends as in other specifications or when using different definitions of the domestic violence variable.

[35] We regress each outcome on all the regressors in Eq. (1) except for the interaction of the treatment dummy with the post-reform indicator. We then drop the observations belonging to the control group in the post-reform period with the most unfavorable residuals (highest in the case of domestic violence and lowest in the case of decision-making power proxies. The percentage of observations belonging to the control group post-reform to drop correspond to the diff-in-diff effect of the reform on the probability of being divorced divided by the share of married women in this group. Since our estimated diff-in-diff effect on the probability of being divorced is 0.009 and 95.57% of control women in 2005 are married (the remaining being divorced or separated), we drop .009/.956 = 0.97% of the control group post-reform.

[36] Due to our definition of treatment, treated children happen to have more, younger, siblings as seen in Table 2 (variable *N. children in the household*)

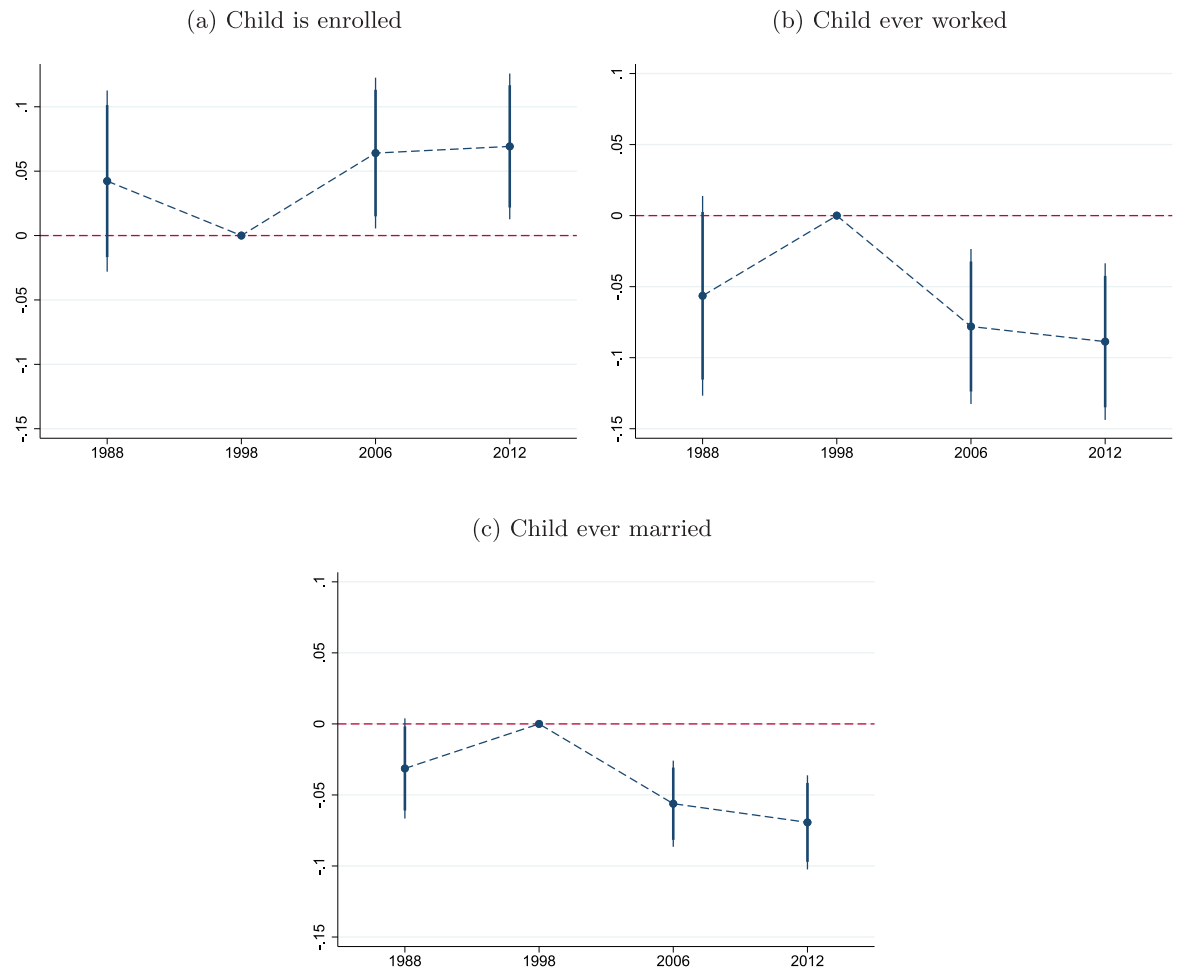

Fig. 4. Children's outcomes event-study. **Notes:** Figure plots the event-study coefficients δ_k in Eq. (2) and 95 percent confidence intervals, from regressions using as dependent variables children's enrollment (Panel A), work experience (Panel B), and probability of being married (Panel C).
Sources: 1998, 2006, and 2012 ELMPS.

Table 5
Effect of Khul on children outcomes.
Sources: Data sources: 1998, 2006 and 2012 ELMPS.

	Enrolled			Ever worked			Ever married		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treated_hh	0.049*	0.052**	0.067**	−0.065***	−0.067***	−0.083***	−0.016	−0.015	−0.045***
	(0.027)	(0.026)	(0.027)	(0.024)	(0.023)	(0.026)	(0.012)	(0.012)	(0.013)
Post	0.095	0.159**	0.154	−0.108	−0.161**	−0.231	0.010	0.028	0.416***
	(0.071)	(0.075)	(0.266)	(0.076)	(0.081)	(0.147)	(0.015)	(0.019)	(0.159)
Treated_hh	−0.022	−0.027	−0.037	0.061***	0.065***	0.076***	0.004	0.003	0.022**
	(0.025)	(0.024)	(0.024)	(0.022)	(0.021)	(0.022)	(0.009)	(0.009)	(0.010)
N	11,574	11,574	11,574	11,575	11,575	11,575	11,577	11,577	11,577
Mean of Dep. Var.	0.48	0.48	0.48	0.28	0.28	0.28	0.02	0.02	0.02
N. children time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The dependent variables are: *Enrolled* (column (1)–(3)), which is a dummy indicating whether the child was enrolled in school at the date of the interview; *Ever Worked* (column (4)–(6)) a dummy indicating whether the child has ever worked by the date of the interview; *Ever married* (column (7)–(9)), a dummy indicating whether the child was married at the date of the interview. *Post* is a dummy indicating the years after the reform, and *Treated_hh* is a dummy indicating whether the child's youngest sibling's age is below 15. All regressions include child's age specific fixed effects. Controls in column (1), (4) and (7) include: child gender, mother's and father's education, dummies for the household urban status, wealth quintiles, and for a set of mother and father's age group pairs. Columns (2), (5) and (8) add age at marriage time trends, and trends in the number of children. Columns (3), (6) and (9) add mother's age time trends. The estimation sample restricts to children between 15 and 24 years old. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

The results suggest that the introduction of unilateral divorce for women increased school enrollment by about 5 or 6 percentage points. This corresponds approximately to a 12% increase in the likelihood of being enrolled, given a pre-period mean of 48 p.p. for the control group. Considering the magnitudes of the various coefficients (not reported in the table), this implies that the *Khul* divorce law lead to a similar increase in the probability of a child being enrolled as a unitary increase in the fathers' education level (i.e. passing from an education level, such as middle school, to the next, such as highschool). The results on children working status depict a consistent story: the reform decreased

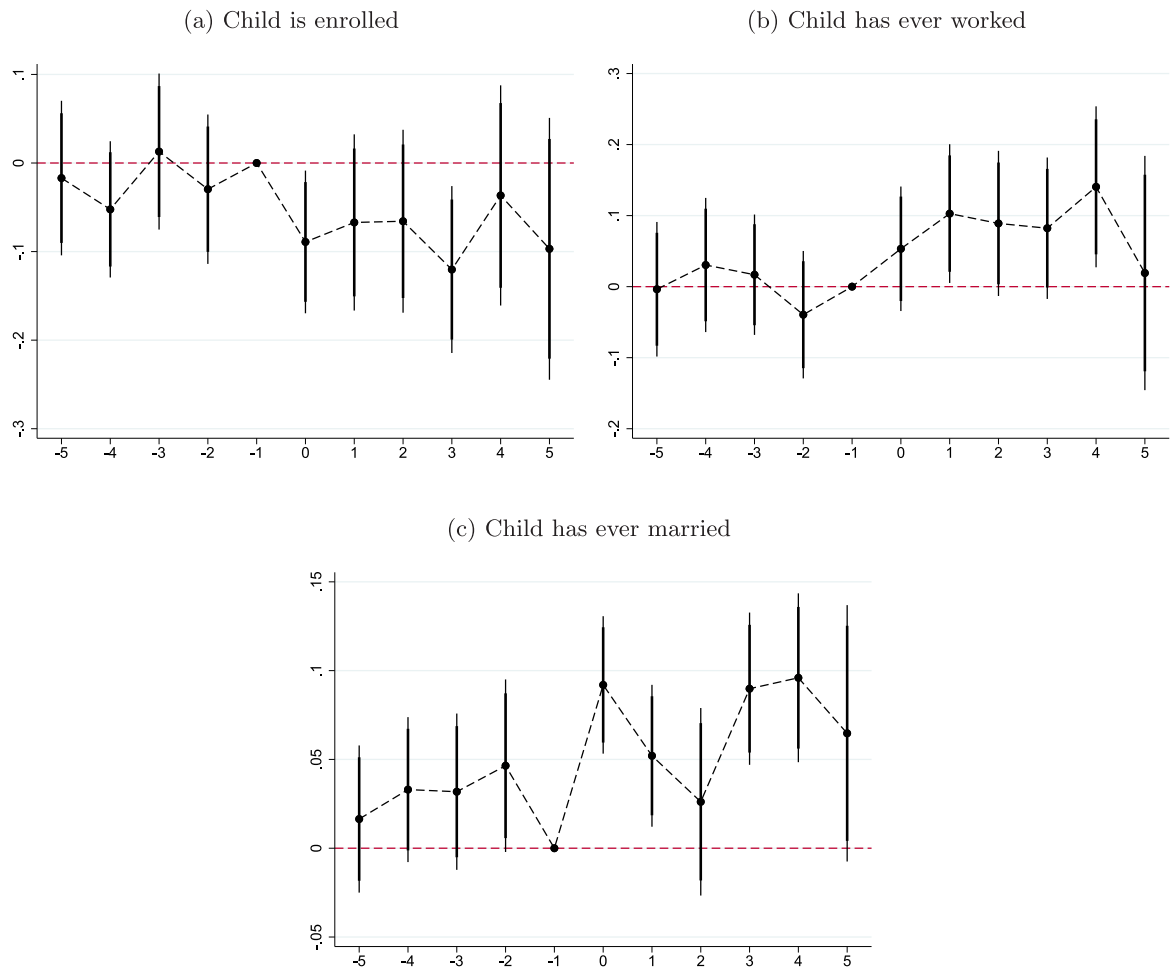

Fig. 5. Effect of the reform on children's outcomes by age of the youngest sibling. **Notes:** the x axis shows the year difference between the youngest sibling's age and the age cutoff used to assign custody to the father, while the y axis shows the coefficients of the interaction terms estimated in Eq. (6) with their confidence intervals. The thinnest line corresponds to a 5 percent confidence interval and the thickest one to a 10 percent confidence interval. We take as reference the last year when the custody of the child remains to the mother. Panel A exhibits results for children enrollment, Panel B exhibits results for children working experience and Panel C shows results for children's probability of being married.
Sources: 1998, 2006, and 2012 ELMPS.

the chances a child ever worked by between 6 and 8 percentage points, approximately a 25% overall decrease.

Finally, we consider whether the divorce reform affected marriage status for older children. The dependent variable in columns (7)-(9) is therefore a dummy equal to 1 if the child is married at the time of the interview. Results weakly suggest that the reform delayed marriage for children living in treated households.

These results are in line with an increase in the bargaining power of women. It seems that the improvement in their relative position within the couple helped them to allocate more resources to keep older children in school and delay their exit from the parental home.[37]

Similarly to what done for women's outcomes, we can decompose the effect of the reform into the part driven by reallocation of bargaining power between husband and wife and the part driven by selection out of marriage, by trimming 1% of the sample of married women in the control group post-reform with the most unfavorable children's outcomes (low enrollment and high labor force participation). At least 80% of the effect of the Khul reform can be attributed to the change in bargaining powers within married couples according to the estimates in Table A.9.

We report heterogeneity analysis for this set of outcomes in Fig. A.4 and Tables A.10–A.12 in Appendix A. We find evidence in favor of sons being the most impacted by the reform, in terms of education investments and delay in marriage, while no significant effects are found for daughters. The triple interaction coefficients, however, are significantly different from zero only when considering marriage. When looking at heterogeneity by household or parents' characteristics, we find larger effects among children born to women with lower educational attainment (as for women's outcomes), although, overall, we do not have enough power to detect significant heterogeneous effects.

Finally, we exploit the exact distance of the youngest sibling's age from the custody cutoff to test whether the impact of the reform differs by sibling's age. We first test the robustness of the results by dropping from the sample households in which the youngest child is in a close window around the cutoff. Table A.5 shows that results are essentially

[37] These results are consistent with other evidence suggesting that mothers have a higher preference for children's education than fathers in Egypt. Using ELMPS data, Namoro and Roushdy (2009) find that mother's contribution to marriage costs, unlike the father's, positively affects child schooling. Roushdy (2004) shows a positive relation between more empowered women's status (such as mobility, decision making in the household, etc.) and investment in children education, while no results are found on nutritional status. These results seem in line with a broader literature on intra-household decision making in middle-income countries. For instance, Heggeness (2020) shows how access to divorce increased school enrollment in Chile.

unchanged, or slightly larger, as predicted. Next, we estimate an equation equivalent to (6) on children's outcomes and augmented by child's age fixed-effects. Fig. 5 displays the coefficients of a full set of dummies δ_k for $k = [-5, 5]$ from Eq. (6). We take as reference the children whose youngest sibling is 14 years old, i.e. one year previous to the mother losing his/her custody in case of divorce, while the year of the event is associated to time 0. Regardless of the outcome we look at, the divorce reform seems to have similarly affected all children in the treatment group, since we do not observe an increasing trend in the coefficients by age of the youngest child, similarly to what we observed in the same analysis on domestic violence.

8 Conclusions

During the 1990s and the 2000s many countries in the MENA region made changes to family law to grant women the right to non-consensual, not fault-based divorce, often called *Khul*. Egyptian women were allowed to file for unilateral divorce in exchange of forfeiting financial support from the husband starting in 2000. The reform was controversial and it was defined as "groundbreaking" by some national media (Reda, 2017).

This paper studies the effects of the introduction of *Khul* on domestic violence, indicators of the wife's decision making power within the household, investments into children's education and women's labor force participation. We find that the introduction of unilateral divorce lead to significantly lower domestic violence, higher children education, and a lower probability that children between 15 and 24 years old work, consistent with an increase in the bargaining power of the wife following the reform. We instead find no clear and robust effect on women's labor force participation and a weakly positive effect on other indicators of wives' decision making power within the household.

To the best of our knowledge, we are the first to analyze the effect of access to divorce on outcomes reflecting decision making within couples by exploiting a divorce law that clearly redistributed the bargaining power in favor of the wife, rather than symmetrically introducing rights for both men and women. Overall, our findings provide the first systematic evidence that easier access to divorce can positively affect women bargaining power within the household and be a deterrent of spousal violence in a developing country. In the context of this study, our results are even more compelling given the incidence of domestic violence and the gendered family laws present in many countries of the MENA region. We believe that more research is needed to understand how these types of institutional and legal changes can be effectively implemented without triggering backlash reactions, and how they interact with local culture to shape gender roles.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The authors do not have permission to share data.

Appendix A. Additional tables and figures

See Figs. A.1–A.4 and Tables A.1–A.12

Appendix B. Data appendix

B.1. Demographic health survey

Sample The DHS surveys ever married women between the ages of 15 to 49. We restrict the sample to currently married women since our main dependent variables apply only for married women: they are constructed from questions asking about decision making within the couple and violence perpetuated by husbands in the previous 12 months. For our main analyses we use questions collected in 1995 and 2005, respectively 5 years before and 5 years after the introduction of the divorce reform. The DHS conducted seven survey waves in Egypt in 1988, 1992, 1995, 2000, 2005, 2008 and 2014. Only the survey waves of 1995, 2005 and 2014 included a domestic violence module. However, since child custody rules changed between 2005 and 2014, we use the surveys conducted in 1995 and 2005 for our main analysis. We further restrict the sample to women whose youngest (living) child is within a 5 years window from the age cutoff used to assign the custody to the father in case of a divorce. The cutoff up to 2005 was set at 10 years of age for boys and 12 years for girls, therefore our main analysis sample is made of currently married mothers whose youngest child's age is between 5 and 15 in case the youngest is a boy and between 7 and 17 in case the youngest is a girl.

Table A.1
Robustness to alternative definition of the outcome *Budget*.
Sources: Data sources: 1995 and 2005 DHS, Egypt.

	Budget definition - has last word on:											
	Large Purchases only			Daily Needs only			Large purchases or daily needs			Large purchases and daily needs		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post*Treated	0.059	0.062*	0.053	0.056	0.059*	0.068*	0.058*	0.061*	0.068*	0.054	0.058	0.051
	(0.036)	(0.036)	(0.038)	(0.034)	(0.034)	(0.036)	(0.034)	(0.034)	(0.036)	(0.036)	(0.036)	(0.038)
Post	−0.062**	−0.042	−0.015	0.232***	0.251***	0.213	0.250***	0.270***	0.262	−0.081***	−0.061	−0.076
	(0.030)	(0.043)	(0.182)	(0.028)	(0.037)	(0.171)	(0.028)	(0.037)	(0.166)	(0.030)	(0.043)	(0.179)
Treated	−0.047	−0.051	−0.043	−0.049	−0.052	−0.057*	−0.051	−0.053	−0.057*	−0.045	−0.049	−0.043
	(0.032)	(0.032)	(0.034)	(0.033)	(0.033)	(0.034)	(0.033)	(0.032)	(0.034)	(0.032)	(0.032)	(0.034)
N	6471	6471	6471	6420	6420	6420	6456	6456	6456	6435	6435	6435
Mean of Dep. Var.	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The dependent variables are different definitions of the variable *Budget*. Columns (1)–(3) use the definition adopted in the main text and tables. The indicator in columns (4)–(6) is 1 for women with decision-making over purchases for daily needs in 2005; the one in columns (7)–(9) takes value 1 for women deciding on large household purchases or on purchases for daily needs and the one in columns (10)–(12) indicates women deciding over large household purchases and purchases for daily needs in 2005. We refer to the data appendix for more detail. *Post* is a dummy indicating years after the reform, *Treated* is a dummy indicating mothers of sons younger than 10 years old, or of daughters younger than 12 years old. Controls always include: respondent's and her husband's education, dummies for the household urban status, wealth quintiles, and for a set of wife and husband's age group pairs. Columns (2), (5), (8), (11) add age at marriage time trends. Columns (3), (6), (7), (12) add respondent's age time trends. The estimation sample restricts to married women whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

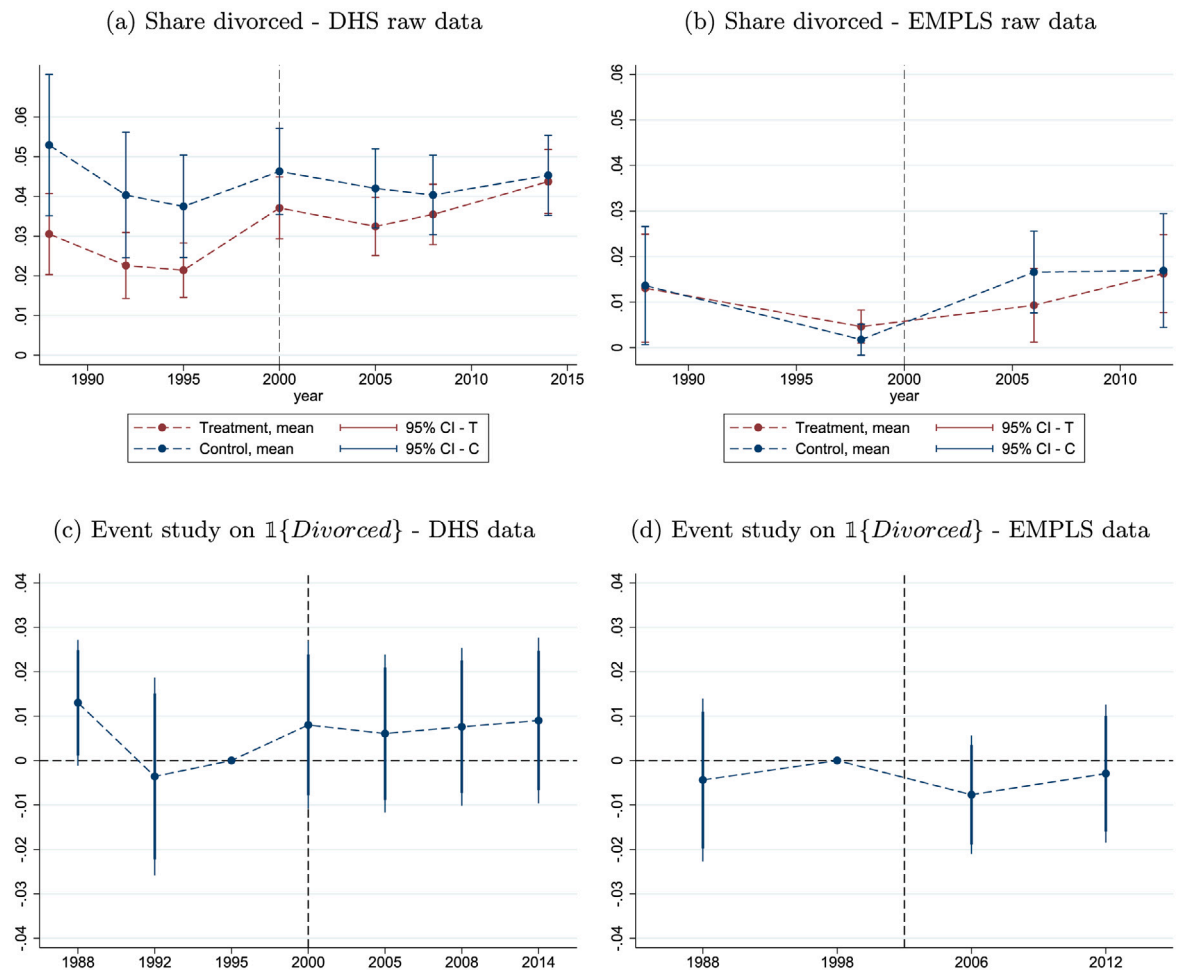

Fig. A.1. Divorce - Raw means and event study. **Notes:** Figures plot the share of divorced women whose last child's age is in a window of ±5 from the custody cutoffs (Panels A and B) and the corresponding event study plots (panel C and D). Treated women are those whose youngest child is younger than the custody cutoff. All panels show 95 percent confidence intervals.
Sources: 1988, 1992, 1995, 2000, 2005, 2008, 2014 DHS; 1998, 2006, and 2012 ELMPS.

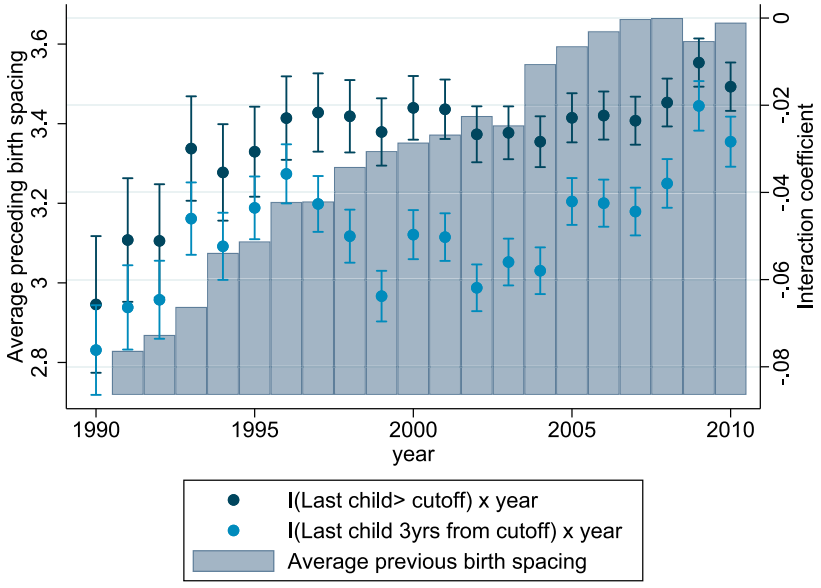

Fig. A.2. Average preceding birth spacing and propensity to have a child by age of the last born. **Notes:** Figure plots the average preceding birth spacing by birth year (histogram) and the coefficients δ_k from Eq. (3) using the two different definitions of D_{it} discussed in the main text, using all women who had at least one child at the time of the DHS interview.
Sources: 1992, 2000, 2005, 2008, 2014 DHS.

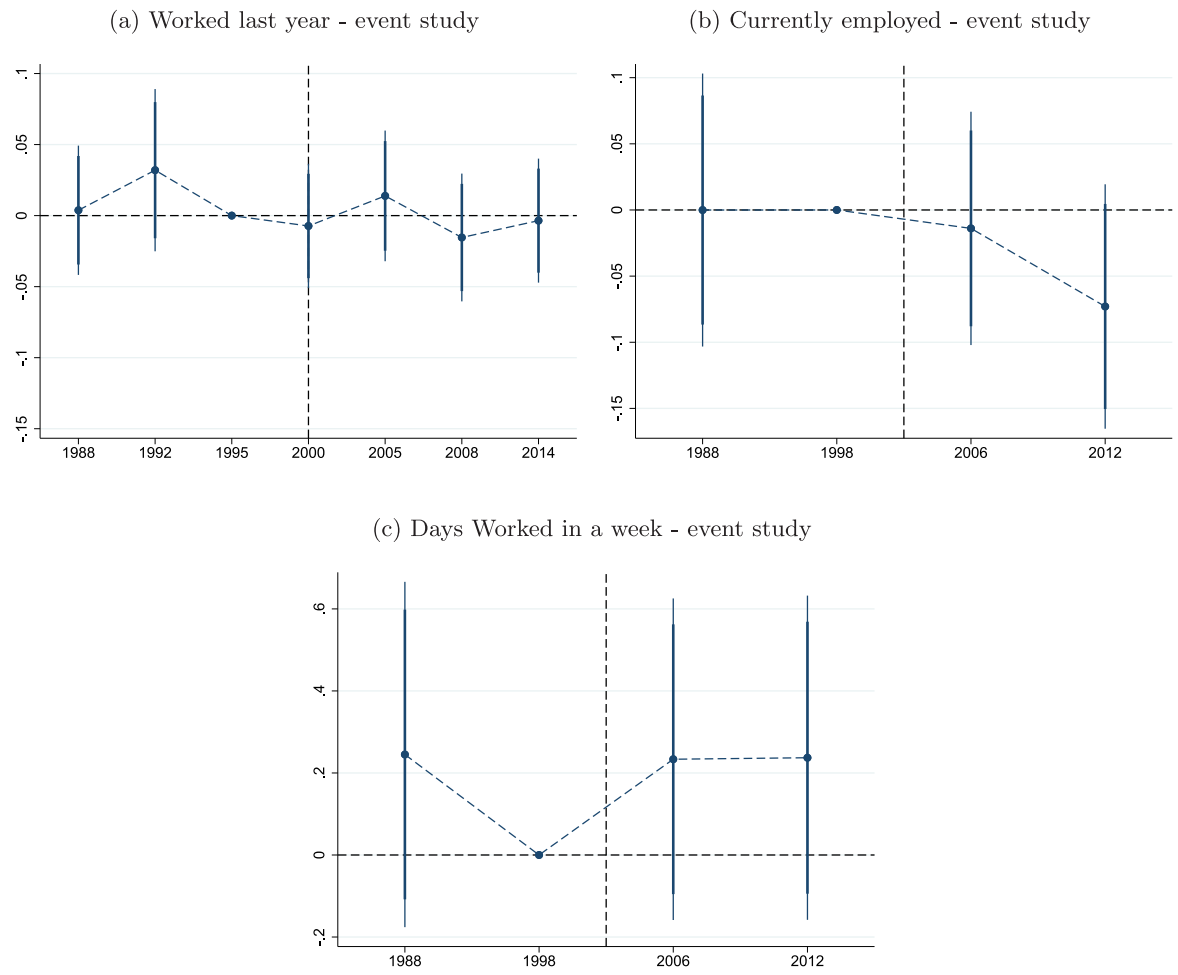

Fig. A.3. Women's employment - event study. **Notes:** The figure plots the event-study coefficients δ_k in Eq. (2) and 95 percent confidence intervals, from regressions using as dependent variables a dummy indicating women who worked in the 12 months preceding the interview (Panel A), women who worked in the 3 months preceding the interview (Panel B) and the number of days a week worked, conditional on being employed (Panel C). Panels A uses DHS data, while the other panels use EMPLS data.
Sources: 1988, 1992, 1995, 2000, 2005, 2008 and 2014 DHS, Egypt and 1998, 2006, and 2012 ELMPS.

Table A.2
Effect of Khul on female labor force participation.
Sources: Data sources: 1995 and 2005, DHS and 1998, 2006 and 2012 ELMPS.

	DHS			ELMPS					
	Worked (last year) - violence sample			Currently working			Days worked (week)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treated	0.007	0.009	0.057	−0.070*	−0.068*	−0.054	0.305**	0.263*	0.356*
	(0.039)	(0.039)	(0.042)	(0.037)	(0.036)	(0.043)	(0.151)	(0.147)	(0.205)
Post	0.019	0.162***	0.078	−0.006	0.125**	−0.987***	−0.256**	−0.309	−0.516
	(0.033)	(0.047)	(0.119)	(0.035)	(0.048)	(0.071)	(0.109)	(0.364)	(0.383)
Treated	−0.006	−0.016	−0.035	0.083**	0.076**	0.067*	−0.289**	−0.245*	−0.318*
	(0.029)	(0.029)	(0.030)	(0.035)	(0.034)	(0.039)	(0.147)	(0.142)	(0.188)
N	3095	3095	3095	5336	5336	5336	1400	1400	1400
Mean of Dep. Var.	0.18	0.18	0.18	0.41	0.41	0.41	5.99	5.99	5.99
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The dependent variables are: *Worked (last year)*, a dummy indicating women who worked in the prior 12 months; *Currently Employed* (column (4)–(6)), a dummy indicating whether the respondent has been employed in the last 3 months; *Days Worked (week)* (column (7)–(9)), which indicates the number of days worked over a week by the respondent. *Post* is a dummy indicating the years after the reform, *Treated* is a dummy indicating women mothers of children younger than 15 years old. Controls always include: respondent's and her husband's education, dummies for the household urban status, wealth quintiles, and for a set of wife and husband's age group pairs. Columns (2), (5) and (8) add age at marriage time trends. Columns (3), (6) and (9) add respondent's age time trends. Columns (1)–(3) use DHS data, restricting the sample to women selected for the domestic violence module. Other columns use ELMPS data. The estimation sample always restricts to married women whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

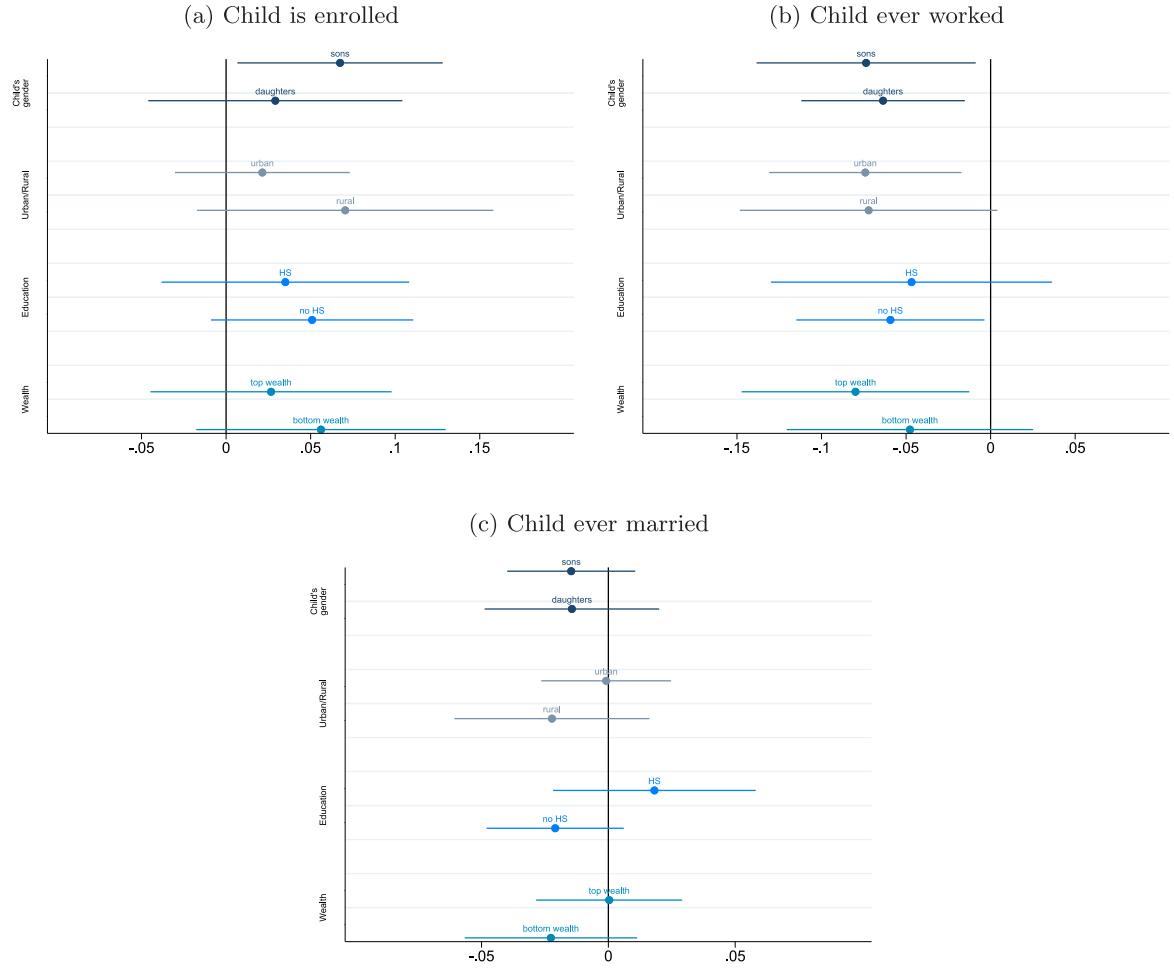

Fig. A.4. Heterogeneity in effects on children's enrollment in school. **Notes:** The figure plots the coefficients and relative 95 confidence intervals from regressions of children's outcomes on the difference-in-difference interaction term in Eq. (4) on a series of sub-samples split by the following characteristics: gender of the child (female or male); urbanization (rural or urban); education (below or above high school education); wealth (below or above the top two quintiles of wealth).
Sources: 1998, 2006, and 2012 ELMPS.

Domestic violence module The DHS surveys conducted in 1995, 2005 and 2014 included a domestic violence (DV) module, also called women's status module in 1995, which was administered to a subsample of respondents. In 1995, the women's status module (including DV questions) was administered to eligible women in a systematic one-third of the households selected for the DHS sample in 24 of the 26 governorates. In the Assuit and Souhag governatorates (which were targeted for a special panel study) all eligible women in the DHS households were administered the women's status module. In 2005 and 2014, the DV section was administered to eligible (ever-married, age 15–49) women in the subsample of households selected for the anemia-testing component of the survey, which constituted one-third of all DHS households. To ensure confidentiality, only one eligible woman was randomly selected from each of the households in the subsample to be asked the DV section.

Table B.1 provides summary statistics by DV module status and by survey year. The first row provides the number of women selected and not selected for this module in the entire DHS sample while the second row shows counts for the subset satisfying the restrictions to be included in our main analysis sample (currently married, with at least one child alive, whose youngest child's age is within a 5 years window from the custody cutoff).

The bottom part of the table shows average characteristics of women selected and not selected for the DV module in our main analysis sample. Women selected in the DV module in 1995 are slightly more likely to come from urban areas (probably due to the oversampling of women from the governatorates of Assuit and Souhag) and have 0.36 fewer years of education on average. All other means are not significantly different between the two groups. On the contrary, women selected for the DV module in 2005 have on average 0.57 years of schooling and are more likely to live in an urban area.

Even if women selected for the DV module might be different from the rest of the DHS women on some dimension, the DHS sampling weights are designed to allow calculations based on the women's status subsample to yield nationally representative estimates.

Whenever we restrict our sample to women selected for the DV module, we use the special weights provided by the DHS data to analyze questions of this module in the regressions. When using questions coming from the regular DHS questionnaire, instead, we use the

Table A.3
Heterogeneity of effect of Khul by treatment intensity - all specifications.
Sources: Data sources: 1995 and 2005 DHS, Egypt.

Panel A: Domestic violence									
	Violence								
	Strict			Preferred			Comprehensive		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*keep_all	−0.140***	−0.129***	−0.195***	−0.119**	−0.110**	−0.195***	−0.113**	−0.104*	−0.190***
	(0.049)	(0.049)	(0.066)	(0.055)	(0.054)	(0.071)	(0.056)	(0.055)	(0.073)
Post*keep_some	−0.065**	−0.061**	−0.062**	−0.062*	−0.060*	−0.070**	−0.042	−0.038	−0.048
	(0.030)	(0.030)	(0.030)	(0.035)	(0.034)	(0.035)	(0.036)	(0.035)	(0.036)
Post	0.033	0.070**	0.152	0.096***	0.176***	0.325	0.107***	0.181***	0.321
	(0.022)	(0.031)	(0.151)	(0.026)	(0.040)	(0.201)	(0.026)	(0.041)	(0.201)
Keep_all	0.073	0.055	0.079	0.074	0.047	0.079	0.074	0.047	0.080
	(0.048)	(0.048)	(0.052)	(0.048)	(0.049)	(0.052)	(0.048)	(0.049)	(0.052)
Keep_some	0.036*	0.034	0.033	0.031	0.027	0.030	0.031	0.026	0.029
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
N	3101	3101	3101	3098	3098	3098	3099	3099	3099
Mean of Dep. Var.	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes
Panel B: Women's agency									
	Visits			Budget			Worked (last year)		
Post*keep_all	0.000	0.016	0.040	0.058	0.060	0.088	−0.144***	−0.146***	−0.089*
	(0.064)	(0.064)	(0.078)	(0.060)	(0.061)	(0.075)	(0.039)	(0.039)	(0.050)
Post*keep_some	0.075**	0.077**	0.068*	0.059	0.063*	0.051	−0.007	−0.007	0.020
	(0.036)	(0.036)	(0.037)	(0.037)	(0.037)	(0.039)	(0.022)	(0.022)	(0.023)
Post	0.287***	0.289***	0.407***	−0.062**	−0.039	−0.047	0.027	0.166***	0.278***
	(0.031)	(0.043)	(0.150)	(0.030)	(0.044)	(0.193)	(0.019)	(0.028)	(0.086)
Keep_all	−0.041	−0.049	−0.064	−0.055	−0.064	−0.084	0.077**	0.032	0.004
	(0.061)	(0.062)	(0.071)	(0.056)	(0.057)	(0.064)	(0.034)	(0.035)	(0.038)
Keep_some	−0.069**	−0.070**	−0.062*	−0.047	−0.050	−0.040	0.007	−0.001	−0.014
	(0.033)	(0.033)	(0.034)	(0.033)	(0.033)	(0.034)	(0.017)	(0.017)	(0.017)
N	6542	6542	6542	6471	6471	6471	8582	8582	8582
Mean of Dep. Var.	0.40	0.40	0.40	0.56	0.56	0.56	0.18	0.18	0.18
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The dependent variables are: the strict, preferred and comprehensive definitions of the dummy *Violence*, that is, binary indicators for domestic violence experienced in the year prior to the interview as defined in Section 5 (Panel A); *Visits* (columns (1)–(3), Panel B) and *Budget* (columns (4)–(6), Panel B), which are dummies indicating respectively if the woman has final say on decisions to visit relatives and spend household budget and *Worked (last year)*, a dummy indicating women who worked in the prior 12 months. *Post* is a dummy indicating the years after the reform, *Keep_all* is a dummy indicating if the woman keep all of her children after a divorce (i.e. her oldest child's age is below the custody cutoff), and *Keep_some* is a dummy indicating if the woman keep some of her children after a divorce (i.e. at least her youngest child's age is below the custody cutoff). Controls always include: respondent's and her husband's education, dummies for the household urban status, wealth quintiles, and for a set of wife and husband's age group pairs. Columns (2), (5) and (8) add age at marriage time trends. Columns (3), (6) and (9) add respondent's age time trends. The estimation sample restricts to married women whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

regular DHS weights and we use the full sample of women to maximize precision.

Variables construction We construct the following main dependent variables from the DHS: the dummy *Visits*, the dummy *Budget*, the dummy *Worked (last year)*, and three definitions of the dummy *Violence*, which we label in the text as "Strict", "Preferred" and "Comprehensive". Our three measures of domestic violence are constructed using answers to questions of the domestic violence module of 1995 and 2005. The wording of the question changed from 1995 and 2005. In 1995 the questionnaire asked: *"From the time you were married has anyone ever beaten you? If yes, can you tell me who has done this to you since you were married? Approximately, how many times were you beaten in the past year?"*. For all definitions, the domestic violence dummy is switched on in 1995 if the respondent answers that her husband was the one beating her and if she reported being beaten at least once in the previous 12 months. In 2005 the domestic violence module was more detailed and contained separate questions for a list of specific forms of domestic violence. For each item on the list it asked: *"Does your husband ever .. ? How often did this happen during the last 12 months: often, only sometimes, or not at all?"*. The items of the list were: *(a) push you, shake you, or throw something at you? (b) slap you or twist your arm? (c) punch you with his fist or with something that could hurt you? (d) kick you or drag you? (e) try to strangle you or burn you? (f) threaten you with a knife, gun, or other type of weapon? (g) attack you with a knife, gun, or other type of weapon? (h) physically force you to have sexual intercourse with him when you did not want to?*. In the "Strict" definition of violence, we assign value 1 to the *Violence* dummy in 2005 if the woman reported that her husband at least once in the previous 12 months did something under items (c), (d), or (e), which seemed to us the circumstances most similar to the wording "beaten" used in 1995. In the "Preferred" definition of violence we also include women who report experiencing physical abuse of the forms under items (b), (f) or (g) in addition to (c), (d), or (e). Finally, we extend the preferred definition to construct

Table A.4
Heterogeneity of effects on domestic violence.
Sources: Data sources: 1995 and 2005 DHS, Egypt.

	(1)	(2)	(3)
Panel A: by gender of last child			
	Boy	Girl	Triple interaction
Post*Treated	−0.109***	−0.017	−0.083
	(0.040)	(0.045)	(0.062)
N	1957	1136	3101
Panel B: by place of residence			
	Urban	Rural	Triple interaction
Post*Treated	−0.125***	−0.008	−0.109*
	(0.037)	(0.046)	(0.059)
N	1485	1611	3101
Panel C: by distance to family court			
	Closer than 5 km	Further than 5 km	Triple interaction
Post*Treated	−0.090**	−0.050	−0.043
	(0.041)	(0.042)	(0.058)
N	1658	1360	3022
Panel D: by educational attainment			
	More than high school	Less than high school	Triple interaction
post_treat2	−0.037	−0.092**	0.038
	(0.033)	(0.036)	(0.055)
N	769	2328	3101
Panel E: by wealth quintile			
	Top 40% wealth	Bottom 60% wealth	Triple interaction
Post*Treated	−0.091***	−0.043	−0.049
	(0.033)	(0.045)	(0.056)
N	1420	1677	3101
Panel F: by arrangement after marriage			
	Moved after marriage	Did not move	Triple interaction
Post*Treated	−0.026	−0.091***	0.051
	(0.055)	(0.032)	(0.065)
N	989	2060	3054

Notes: The dependent variable is the preferred definition of *Violence*, a dummy indicating IPV in the last 12 months. *Post* is a dummy indicating the years after the reform, *Treated* is a dummy indicating women mothers of sons younger than 10 years old, or of daughters younger than 12 years old. Each panel shows coefficients for the interaction term in Eq. (1) estimated for different samples (columns (1) and (2)), as well as a triple interaction coefficient in which the third dimension used in the interaction is the characteristic of interest (column (3)). Sub-samples are split by the following characteristics: gender of the youngest child (female or male) in Panel A; type of place of residence (rural or urban) in Panel B; distance from family courts (further of closer than 5 km from nearest family court) in Panel C; education (below or above highschool education) in Panel D; wealth (below or above the top two quintiles of wealth) in Panel E; arrangement after marriage (if woman moved or not after marriage) in Panel F. All regressions control for: respondent's and her husband's education, age at marriage time trends, dummies for the household urban status, wealth quintiles, and for a set of wife and husband's age group pairs. The estimation sample restricts to married women whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

a "Comprehensive" definition that also includes answers to the question under item (a).

The outcomes *Visits* and *Budget* were instead constructed using questions about the decision making arrangements within the couple. The 1995 and 2000 surveys asked: *"Who has the final say in your family on the following – you, your husband, both you and your husband or someone else?"*, while starting from 2005, the wording changed to *"Who usually makes the following decisions: mainly you, mainly your husband, you and your husband jointly, or someone else?"*. Among the topics "visits to family, friends, or relatives" was asked in all survey years. Our *Visits* dummy takes on values 1 if women responded that they only or jointly with their husband made decisions over visits to family and friends. The 1995 survey included among the decision topics "household budget", while starting from 2000 the question included "major/large household purchases" and "purchases for daily household needs" separately. We use the answers to the "major/large household purchases" item starting from 2000, as this seems most similar to the wording "household budget", to construct our *Budget* dummy in a similar way as the *Visits* dummy.

In a robustness check, we construct different versions of the variable *Budget*, to consider answers to the question about decisions over purchases for daily household needs. In particular, we consider the following changes in the way we construct this outcome for the 2005 survey round:

- Daily needs only - *Budget* takes the value 1 in 2005 if the woman says she (alone or jointly with the husbands) decides on purchases for daily needs and zero otherwise
- Large purchases or daily needs - *Budget* takes the value 1 in 2005 if the woman says she (alone or jointly with the husbands) decides on large household purchases or on purchases for daily needs and zero if she never decides

Table A.5
Robustness to exclusion of children close to cutoff.
Sources: Data sources: 1995 and 2005 DHS, Egypt and 1998, 2006 and 2012 ELMPS.

	Violence			Budget	Visits	Worked	Enrolled	Ever worked	Ever married
	Strict	Preferred	Comprehensive						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treated	−0.093***	−0.102**	−0.086**	0.084*	0.053	−0.000	0.077**	−0.091***	−0.038**
	(0.035)	(0.041)	(0.042)	(0.044)	(0.044)	(0.027)	(0.032)	(0.032)	(0.016)
Post	0.027	0.207	0.201	0.375***	−0.061	0.184**	0.102	−0.168	0.406***
	(0.137)	(0.189)	(0.188)	(0.137)	(0.186)	(0.076)	(0.277)	(0.154)	(0.153)
Treated	0.045**	0.042*	0.040*	−0.069*	−0.033	−0.010	−0.050*	0.082***	0.034***
	(0.023)	(0.023)	(0.023)	(0.039)	(0.039)	(0.020)	(0.029)	(0.028)	(0.013)
N	2552	2549	2550	5393	5329	7061	9531	9532	9534
Mean depvar	0.10	0.10	0.10	0.40	0.56	0.18	0.48	0.28	0.02
N. children time trend	–	–	–	–	–	–	Yes	Yes	Yes
Woman's age of marr. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Woman's age time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The estimation sample in columns (1)–(6) restricts to married women whose youngest child's age is within a ±5 years window around the custody cutoffs, excluding mothers of children within a ±1 years window around the cutoffs. The estimation sample in columns (7)–(9) similarly restricts to children between 15 and 24 years old born to women in this restricted analysis sample. *Post* is a dummy indicating the years after the reform, *Treated* is a dummy indicating women mothers of sons younger than 10 years old, or of daughters younger than 12 years old in columns (1) to (6) and children with at least one sibling younger than 15 years old in columns (7) to (9). Controls include: respondent's and her husband's education, dummies for the household urban status, wealth quintiles, a set of wife and husband's age group pairs, age at marriage time trends, respondent's age time trends. Columns (7) to (9) also include children's age fixed effects, a control for the child's gender, family size and time trends in the number of children. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

Table A.6
Effect on attitudes towards domestic violence.
Sources: Data sources: 1995 and 2005 DHS, Egypt.

	Burns food			Neglects children			Refuses sex		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample									
Post*Treated	−0.023	−0.027	−0.038	0.031	0.027	0.041	0.020	0.018	0.021
	(0.032)	(0.032)	(0.034)	(0.038)	(0.038)	(0.040)	(0.036)	(0.035)	(0.038)
Post	−0.039	−0.085**	−0.089	−0.073**	−0.137***	−0.114	−0.099***	−0.123***	−0.227
	(0.025)	(0.035)	(0.146)	(0.031)	(0.040)	(0.182)	(0.029)	(0.039)	(0.172)
Treated	0.016	0.021	0.029	−0.050	−0.043	−0.054	−0.018	−0.016	−0.018
	(0.029)	(0.029)	(0.031)	(0.035)	(0.035)	(0.036)	(0.033)	(0.033)	(0.034)
N	6602	6602	6602	6602	6602	6602	6602	6602	6602
Mean of Dep. Var.	0.30	0.30	0.30	0.52	0.52	0.52	0.49	0.49	0.49
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes
Panel B: domestic violence sample									
Post*Treated	−0.046	−0.059	−0.079*	0.031	0.019	0.035	0.022	0.013	0.020
	(0.041)	(0.041)	(0.043)	(0.049)	(0.049)	(0.052)	(0.046)	(0.046)	(0.049)
Post	−0.033	−0.066	0.209	−0.058	−0.124**	0.118	−0.099***	−0.100*	−0.363*
	(0.033)	(0.048)	(0.251)	(0.038)	(0.055)	(0.258)	(0.038)	(0.054)	(0.219)
Treated	0.017	0.024	0.033	−0.054	−0.044	−0.049	−0.021	−0.019	−0.021
	(0.032)	(0.033)	(0.033)	(0.039)	(0.039)	(0.040)	(0.036)	(0.036)	(0.037)
N	3096	3096	3096	3096	3096	3096	3096	3096	3096
Mean of Dep. Var.	0.30	0.30	0.30	0.52	0.52	0.52	0.49	0.49	0.49
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The dependent variables are: *Burns Food* (column (1)–(3)), which is a dummy indicating whether the respondent believes husbands are justified to beat their wife if she burns food; *Neglects Children* (column (4)–(6)), which is a dummy indicating whether the respondent believes husbands are justified to beat their wife if she neglects children; *Refuses Sex* (column (7)–(9)), which is a dummy indicating whether the respondent believes husbands are justified to beat their wife if she refuses to have sex. *Post* is a dummy indicating the years after the reform, *Treated* is a dummy indicating women mothers of sons younger than 10 years old, or of daughters younger than 12 years old. Controls in column (1), (4) and (7) include: respondent's and her husband's education, dummies for the household urban status, wealth quintiles, and for a set of wife and husband's age group pairs. Columns (2), (5) and (8) add age at marriage time trends. Columns (3), (6) and (9) add respondent's age time trends. The estimation sample restricts to married women whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

Table A.7
Placebo effect on violence and agency.
Sources: Data sources: 1995 and 2005 DHS, Egypt.

	Violence			Visits	Budget	Worked (last year)
	Strict	Preferred	Comprehensive			
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treat_placebo	0.042	0.062	0.051	−0.075	0.021	−0.015
	(0.043)	(0.048)	(0.050)	(0.062)	(0.062)	(0.035)
Post	0.238	0.272	0.265	−0.190	−0.023	0.100
	(0.174)	(0.181)	(0.182)	(0.266)	(0.250)	(0.140)
Treat_Placebo	−0.018	−0.015	−0.015	0.057	0.017	0.002
	(0.032)	(0.032)	(0.033)	(0.056)	(0.058)	(0.028)
N	1351	1350	1350	3070	3096	3863
Mean of Dep. Var.	0.08	0.08	0.08	0.50	0.39	0.17
Woman's age of marr. time trend	Yes	Yes	Yes	Yes	Yes	Yes
Woman's age time trend	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variables are the same ones used in Table 3. *Post* is a dummy indicating the years after the reform, *Treated_Placebo* is a dummy indicating women mothers of sons younger than 15 years old, or of daughters younger than 17 years old. Controls include: respondent's and her husband's education, dummies for the household urban status, wealth quintiles, and for a set of wife and husband's age group pairs, age at marriage time trends, respondent's age time trends. The estimation sample restricts to married women whose youngest child's is older than the real custody cutoff, and whose age is within a ±5 years window around the placebo custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

Table A.8
Robustness to extending the sample to children in a ±10 years window from the cutoff.
Sources: Data sources: 1995 and 2005 DHS, Egypt and 1998, 2006 and 2012 ELMPS.

	Violence			Budget	Visits	Worked	Enrolled	Ever worked	Ever married
	Strict	Preferred	Comprehensive						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post*Treated	−0.067**	−0.063**	−0.041	0.042	0.043	0.012	0.067***	−0.078***	−0.045***
	(0.026)	(0.031)	(0.032)	(0.034)	(0.036)	(0.022)	(0.025)	(0.025)	(0.013)
Post	−0.034	0.103**	0.094**	0.302***	0.058	0.027	0.106	0.026	0.259***
	(0.039)	(0.046)	(0.047)	(0.043)	(0.047)	(0.027)	(0.223)	(0.137)	(0.081)
Treated	0.042**	0.036*	0.035*	−0.046	−0.042	−0.020	−0.041*	0.064***	0.035***
	(0.018)	(0.018)	(0.018)	(0.030)	(0.032)	(0.016)	(0.023)	(0.021)	(0.010)
N	10,029	10,024	10,026	20,462	19,562	27,006	15,448	15,447	15,451
Mean of Dep. Var.	0.10	0.10	0.10	0.40	0.56	0.18	0.43	0.32	0.03
N. children time trend	–	–	–	–	–	–	Yes	Yes	Yes
Woman's age of marr. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Woman's age time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The estimation sample in columns (1)–(6) restricts to married women whose youngest child's age is within a ±10 years window around the custody cutoffs. The estimation sample in columns (7)–(9) similarly restricts to children between 15 and 24 years old born to women in this extended analysis sample. *Post* is a dummy indicating the years after the reform, *Treated* is a dummy indicating women mothers of sons younger than 10 years old, or of daughters younger than 12 years old in columns (1) to (6) and children with at least one sibling younger than 15 years old in columns (7) to (9). Controls include: respondent's and her husband's education, dummies for the household urban status, wealth quintiles, a set of wife and husband's age group pairs, age at marriage time trends, respondent's age time trends. Columns (7) to (9) also include children's age fixed effects, a control for the child's gender, family size and time trends in the number of children. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

- Large purchases and daily needs - *Budget* takes the value 1 in 2005 if the woman says she (alone or jointly with the husbands) decides on large household purchases and on purchases for daily needs and zero if she never decides or if she decides only on one subject.

Questions about decision making power within the couple were not asked in previous survey rounds, which prevents us from checking pre-trends in a rigorous way. Nevertheless, the 1988 and 1992 rounds include questions on attitudes towards decision making power within the couple that look very similar to the ones about actual, self-reported decision making roles. These questions were asked to the respondents (women) in 1988 and to both respondents and their husbands in 1992. The exact wording of these questions was: *"Who should have the last word on the following–the husband, the wife, both, or someone else?"* and the topics once again included "Visits to friends or relatives" and "Household Budget". Therefore, for the purposes of showing how the *Visits* and *Budget* outcomes evolve over time, we extend these variables to 1988 and 1992 using the women's attitudes in 1988 and the husbands' attitudes in 1992 as a proxy for realized behavior. Similar results hold when we use the women's attitudes also in 1992. We prefer using the husband's attitudes because we implicitly assume that what the husband believes should happen is a better proxy for what

Table A.9
Lower bounds on effect due to changes in bargaining within married couples.
Sources: Data sources: 1995 and 2005 DHS, Egypt and 1998, 2006 and 2012 ELMPS.

	Women's outcomes			Children outcomes		
	Violence (1)	Visits (2)	Budget (3)	Enrolled (4)	Ever worked (5)	Ever married (6)
Post*Treated	−0.073**	0.052	0.043	0.058**	−0.071***	−0.052***
	(0.034)	(0.037)	(0.038)	(0.027)	(0.026)	(0.013)
Post	0.192	0.385***	−0.008	0.189	−0.235	0.418***
	(0.189)	(0.133)	(0.182)	(0.267)	(0.148)	(0.158)
Treated	0.032	−0.060*	−0.042	−0.037	0.076***	0.023**
	(0.021)	(0.033)	(0.034)	(0.024)	(0.022)	(0.010)
N	3093	6522	6451	11,533	11,534	11,537

Notes: This table computes a lower bound on the effect of the divorce reform that can be attributed to the change in bargaining power within married couple (as opposed to selection out of marriage). The estimation sample in columns (1)–(3) restricts to married women whose youngest child's age is within a ±5 years window around the custody cutoffs. We additionally drop 1% of the group of women in the post period with all children above the custody cutoff with the most unfavorable outcomes. The estimation sample in columns (4)–(6) restricts to children between 15 and 24 years old born to women in the analysis sample, similarly dropping children in the control group post reform with the most unfavorable outcomes. *Post* is a dummy indicating the years after the reform, *Treated* is a dummy indicating women mothers of sons younger than 10 years old, or of daughters younger than 12 years old in columns (1) to (3) and children with at least one sibling younger than 15 years old in columns (4) to (6). Controls include: respondent's and her husband's education, dummies for the household urban status, wealth quintiles, a set of wife and husband's age group pairs, age at marriage time trends, respondent's age time trends. Columns (4) to (6) also include children's age fixed effects, a control for the child's gender, family size and time trends in the number of children. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

Table A.10
Heterogeneity of effects on enrollment.
Sources: Data sources: 1998, 2006 and 2012 ELMPS, Egypt.

	(1)	(2)	(3)
Panel A: by gender of child			
	Boy	Girl	Triple interaction
Post*Treated	0.067**	0.029	−0.046
	(0.031)	(0.038)	(0.050)
N	6563	5006	11,574
Panel B: by place of residence			
	Urban	Rural	Triple interaction
Post*Treated	0.021	0.070	0.035
	(0.026)	(0.045)	(0.049)
N	6092	5479	11,574
Panel C: by mother's educational attainment			
	More than high school	Less than high school	Triple interaction
Post*Treated	0.035	0.051*	0.016
	(0.037)	(0.030)	(0.048)
N	2739	8830	11,574
Panel D: by wealth quintile			
	Top 40% wealth	Bottom 60% wealth	Triple interaction
Post*Treated	0.027	0.056	−0.023
	(0.036)	(0.038)	(0.053)
N	5027	6545	11,574

Notes: The dependent variable is *Enrolled*, a dummy indicating if the child is currently enrolled in school. *Post* is a dummy indicating the years after the reform, *Treated* is a dummy indicating children whose youngest sibling is younger than 15. Each panel shows coefficients for the interaction term in Eq. (4) estimated for different samples (columns (1) and (2)), as well as a triple interaction coefficient in which the third dimension used in the interaction is the characteristic of interest (column (3)). Sub-samples are split by the following characteristics: gender of the child (female or male) in Panel A; type of place of residence (rural or urban) in Panel B; education (below or above highschool education) in Panel C; wealth (below or above the top two quintiles of wealth) in Panel D. All regressions include child's age specific fixed effects, mother's and her father's education, dummies for the household urban status, wealth quintiles, and for a set of mother and father's age group pairs. The estimation sample restricts to children between 15 and 24 years old, and whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

Table A.11
Heterogeneity of effects on child's work.
Sources: Data sources: 1998, 2006 and 2012 ELMPS, Egypt.

	(1)	(2)	(3)
Panel A: by gender of child			
	Boy	Girl	Triple interaction
Post*Treated	−0.074**	−0.064***	−0.020
	(0.033)	(0.025)	(0.039)
N	6564	5006	11,575
Panel B: by place of residence			
	Urban	Rural	Triple interaction
Post*Treated	−0.074**	−0.072*	−0.024
	(0.029)	(0.039)	(0.048)
N	6093	5479	11,575
Panel C: by mother's educational attainment			
	More than high school	Less than high school	Triple interaction
Post*Treated	−0.047	−0.059**	0.032
	(0.042)	(0.028)	(0.050)
N	2740	8830	11,575
Panel D: by wealth quintile			
	Top 40% wealth	Bottom 60% wealth	Triple interaction
Post*Treated	−0.080**	−0.048	−0.023
	(0.034)	(0.037)	(0.052)
N	5028	6545	11,575

Notes: The dependent variable is *Ever Worked*, a dummy indicating if the child ever worked. *Post* is a dummy indicating the years after the reform, *Treated* is a dummy indicating children whose youngest sibling is younger than 15. Each panel shows coefficients for the interaction term in Eq. (4) estimated for different samples (columns (1) and (2)), as well as a triple interaction coefficient in which the third dimension used in the interaction is the characteristic of interest (column (3)). Sub-samples are split by the following characteristics: gender of the child (female or male) in Panel A; ; urbanization (rural or urban) in Panel B; education (below or above highschool education) in Panel C; wealth (below or above the top two quintiles of wealth) in Panel D. All regressions include child's age specific fixed effects, mother's and her father's education, dummies for the household urban status, wealth quintiles, and for a set of mother and father's age group pairs. The estimation sample restricts to children between 15 and 24 years old, and whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

Table A.12
Heterogeneity of effects on child's marriage.
Sources: Data sources: 1998, 2006 and 2012 ELMPS, Egypt.

	(1)	(2)	(3)
Panel A: by gender of child			
	Boy	Girl	Triple interaction
Post*Treated	−0.015	−0.014	−0.049**
	(0.013)	(0.018)	(0.020)
N	6565	5007	11,577
Panel B: by place of residence			
	Urban	Rural	Triple interaction
Post*Treated	−0.001	−0.022	−0.001
	(0.013)	(0.020)	(0.021)
N	6095	5479	11,577
Panel C: by mother's educational attainment			
	More than high school	Less than high school	Triple interaction
Post*Treated	0.018	−0.021	0.015
	(0.020)	(0.014)	(0.022)
N	2741	8831	11,577
Panel D: by wealth quintile			
	Top 40% wealth	Bottom 60% wealth	Triple interaction
Post*Treated	0.000	−0.023	−0.001
	(0.015)	(0.017)	(0.022)
N	5029	6546	11,577

Notes: The dependent variable is *Ever Married*, a dummy indicating if the child was ever married. *Post* is a dummy indicating the years after the reform, *Treated* is a dummy indicating children whose youngest sibling is younger than 15. Each panel shows coefficients for the interaction term in Eq. (4) estimated for different samples (columns (1) and (2)), as well as a triple interaction coefficient in which the third dimension used in the interaction is the characteristic of interest (column (3)). Subsamples are split by the following characteristics: gender of the child (female or male) in Panel A; type of place of residence (rural or urban) in Panel B; education (below or above highschool education) in Panel C; wealth (below or above the top two quintiles of wealth) in Panel D. All regressions include child's age specific fixed effects, mother's and her father's education, dummies for the household urban status, wealth quintiles, and for a set of mother and father's age group pairs. The estimation sample restricts to children between 15 and 24 years old, and whose youngest child's age is within a ±5 years window around the custody cutoffs. Standard errors clustered at the psu level. Regressions are weighted by survey weights. *p<0.10,**p<0.05,***p<0.01.

actually happens within the couple and thus for the amount of decision making power the woman actually holds in the years in which realized outcomes are missing. Unfortunately we do not have a way to test this assumption since no survey round asked both questions on attitudes and realized outcomes about women's decision making power.

Finally, we use a few additional outcomes, related to the woman's working status and attitudes towards domestic violence. First, we construct the dummy *Worked (last year)*, indicating women who reported having *"done any work in the previous 12 months even if it was only for a short period of time?"*. As for attitudes towards domestic violence, we use survey rounds conducted in 1995 and 2005 asking *"Sometimes a husband is annoyed or angered by things that his wife does. In your opinion, is a husband justified in hitting or beating his wife in the following situations: ...YES/NO"*. Both years include the situations under *"If she neglects the children?"*, *"If she refuses to have sex with him?"* and *"If she burns the food?"*, which we use to construct three separate dummies. Each dummy takes the value 1 if the respondent agrees that husbands are justified in beating their wives in that occasion.

Missing domestic violence answers Due to the sensitivity of the domestic violence questions, some may worry about the non-response rate and how it might be a source of bias in our estimates. While there is no way to check misreporting and accuracy of the answers, non-response rates are extremely low. We report in Table B.2 the statistics on non-response rate by survey question and year, for women selected for the DV module and for the subset in our analysis sample (mothers whose youngest child is within 5 years from the cutoff). Less than 2% of observations are missing for any given question. This results in approximately 2% of observations in our analysis sample with missing domestic violence outcomes.

The very low non-response rate seems unlikely to cause major selection bias in our causal estimates. Nonetheless, as a further check in Table B.3 we show differences-in-differences estimates on attrition, using as dependent variable a dummy indicating observations with missing violence outcomes. The diff-in-diff coefficient is zero in all specifications and for all violence outcomes, indicating that differential selection out of our sample due to missing answers in the DV module does not seem to be a concern for our empirical strategy.

B.2. Egypt Labor Market Panel Surveys

Sample The ELMPS collects nationally-representative longitudinal data of individuals and households. For our main analyses we use questions collected in 1998, 2006 and 2012, and we use them to create two estimation samples. First, we restrict the sample to currently married women whose youngest (living) child is within a 5 years window from the age cutoff used to assign the custody to the father in case of a divorce. Since the cutoff was raised to 15 years for both boys and girls in 2005, our sample is made of currently married mothers whose youngest child's age is between 10 and 20. In the second sample used to analyze children's outcomes, we exclude children in age-custody, i.e. children younger than 15 years old. We further exclude from the sample individuals who are expected to have completed their education, i.e. individuals older than 25, which is the age at which higher education (university) is supposed to end in Egypt.

Variables construction We construct four main dependent variables from the ELMPS: in the children sample, we build the dummy *Enrolled*, the dummy *Ever worked* and the dummy *Ever married*; in the women sample, our measures of female labor force participation are the dummy *Currently employed*, and the continuous measure *Days worked in a week*. All the four measures are constructed using answers to questions of the 1998, 2006, and 2012 rounds.

Our main measure of education is the dummy *Enrolled*, taking value 1 if the respondents positively answer to the survey question *"Is the individual currently studying?"*. To build a measure of children's labor force participation, we use the survey question *"Has the individual ever worked?"*. Our main variable, *Ever worked*, takes value 1 if the respondent had at least one work experience. Finally, the dummy *Ever married* is 1 if the child was ever married at the time of the survey interview and 0 otherwise.

The outcome *Currently employed* is derived from the work status recorded in the three months prior to the date of the interview. Our dummy takes value 1 if the respondent reports to be employed in the three months prior to the interview, while it takes value 0 if she/he declares to be either unemployed or out of the labor force in the reference period. Lastly, *Days worked in a week* is a continuous measure going from 1 to 7, which records the respondent's self-declared number of days worked over the last week.

Table B.1
Summary statistics by DV module status.

	1995			2005		
	(1) Not in DV module	(2) In DV module	(3) All	(4) Not in DV module	(5) In DV module	(6) All
N	7656	7123	14779	13763	5711	19474
N in regression sample	1982	1591	3573	3474	1548	5022
	Mean	Mean	p-value	Mean	Mean	p-value
Age	39.76	39.76	0.99	40.09	39.91	0.31
Years of education	4.46	4.10	0.05	5.51	6.07	0.00
Age first mar	18.38	18.27	0.44	18.86	19.09	0.08
Urban	0.57	0.47	0.00	0.45	0.49	0.01
Number of children	4.24	4.29	0.41	3.92	3.78	0.01
Treated	0.69	0.71	0.33	0.61	0.61	0.98

Table B.2
Statistics on non-response rate on DV module.

	In estimation sample		In DHS domestic violence module	
	(1) N missing	(2) % missing	(3) N missing	(4) % missing
1995 questions				
Someone ever beaten you? Who?	6	0.38%	39	0.55%
Frequency in last 12 m	0	0.00%	1	0.01%
Missing violence (def. 1)	6	0.38%	39	0.55%
Missing violence (def. 2)	6	0.38%	39	0.55%
Missing violence (def. 3)	6	0.38%	39	0.55%
2005 questions				
Spouse ever pushed/shook?	28	1.81%	99	1.73%
Frequency in last 12 m	0	0.00%	3	0.05%
Spouse ever slapped/twisted?	30	1.94%	101	1.77%
Frequency in last 12 m	1	0.06%	5	0.09%
Spouse ever punched?	28	1.81%	100	1.75%
Frequency in last 12 m	1	0.06%	6	0.11%
Spouse ever kicked/dragged?	28	1.81%	99	1.73%
Frequency in last 12 m	0	0.00%	2	0.04%
Spouse ever strangle/burn?	28	1.81%	100	1.75%
Frequency in last 12 m	0	0.00%	3	0.05%
Spouse ever threatened with weapon?	28	1.81%	102	1.79%
Frequency in last 12 m	0	0.00%	1	0.02%
Spouse ever attacked with weapon?	28	1.81%	103	1.80%
Frequency in last 12 m	0	0.00%	1	0.02%
Missing violence (Strict definition)	29	1.88%	110	1.93%
Missing violence (Preferred definition)	32	2.07%	121	2.12%
Missing violence (Comprehensive definition)	32	2.07%	122	2.14%

Table B.3
Attrition due to non-response in DV module.

	Missing violence outcome								
	Strict			Preferred			Comprehensive		
Post*Treated	0.001	0.001	0.001	0.001	0.001	0.001	−0.001	−0.001	−0.002
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)
Post	−0.001	0.005	0.006	0.000	0.005	0.004	0.001	0.001	0.002
	(0.001)	(0.005)	(0.005)	(0.001)	(0.005)	(0.006)	(0.001)	(0.003)	(0.005)
Treated	0.002	0.002	0.002	0.002	0.001	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
N	3139	3139	3139	3139	3139	3139	3139	3139	3139
Woman's age of marr. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Woman's age time trend	No	No	Yes	No	No	Yes	No	No	Yes

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