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## Women's Political Participation and Intrahousehold Empowerment: Evidence from the Egyptian Arab Spring

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## ABSTRACT

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# Women's Political Participation and Intrahousehold Empowerment: Evidence from the Egyptian Arab Spring\*

Egyptian women have played an unprecedented role in the Arab Spring democratic movement, possibly changing women's perception about their own rights and role. We question whether these events have translated into better outcomes within Egyptian households. We conjecture that potential changes must have been heterogeneous and depended on the local intensity of protests and women's participation over 2011-13. We exploit the geographical heterogeneity along these two margins to conduct a double difference analysis using data surrounding the period. We find a significant improvement in women's final say regarding decisions on health, socialization and household expenditure, as well as a decline in the acceptance of domestic violence and girls' circumcision, in the regions most affected by the protests. This effect is not due to particular regional patterns or pre-existing trends in empowerment. It is also robust to alternative treatment definitions and confirmed by triple difference estimations. We confront our main interpretation to alternative mechanisms that could have explained this effect.

**JEL Classification:** J12, J16, D74, I14

**Keywords:** Arab Spring, revolutions, gender, empowerment, Egypt

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# 1 Introduction

The “Arab Spring” democratic movement born in Tunisia quickly spread to Egypt. Demonstrations against Mubarak’s autocratic regime started in January 2011. They were accompanied by a strong repression causing the death of many demonstrators and feeding popular outrage all over the country. From the very beginning, Egyptian women have played an unprecedented role in these events, marching with men during the uprisings or lobbying actively in the social media (Shalaby, 2016). For both men and women, “bread, liberty and social justice” were the main grievances underlying the 2011 protests, not women’s rights specifically (Costello et al., 2015). Yet, the visibility of women amidst the revolutionaries started to challenge the historical stereotypes of a male-dominated public space (Khamis, 2011). The fall of Mubarak in February 2011 was followed by military rule until June 2012, when the Islamist Mohammed Morsi became president. Massive demonstrations through June and July 2013 saw the high mobilization of women against Muslim Brotherhood’s rules. This time, the fight was not only for civic and political freedom but also to defend what had just been gained in terms of gender equality. A new gender discourse had began to emerge in Egypt, used by El-Sissi during his rise to power and leading to political actions in favor of women (Zaki, 2015). Over this period, women have challenged stereotypes and altered perceptions about their traditional roles in the public sphere (Shalaby, 2016).

The present paper questions whether the Egyptian Arab Spring has also led to a change in women’s situation within Egyptian homes. We conjecture that these changes in the private sphere, if any, must have been heterogeneous and depended on the intensity of the movement locally. We exploit geographical heterogeneity in protest intensity and women’s involvement to conduct a difference-in-difference analysis on various measures of women’s empowerment. We draw from the 2008 and 2014 Egyptian Demographic and Health Surveys (DHS). These datasets contain direct empowerment measures, notably women’s say regarding decisions on household expenditure, health and socialization.<sup>1</sup> We focus on these outcomes, as well as on a composite index of power and on questions regarding women’s tolerance towards domestic violence and the intention to circumcise daughters. We combine the DHS with information on protest intensity at governorate level, proxied by the local proportion of fatalities, injuries and arrests over the period. Our baseline estimation captures the relative time variation in

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<sup>1</sup>Similar direct measures of intrahousehold bargaining power, often drawn from DHS surveys, have been used to elicit the key determinants of women’s empowerment (see references in section 2) or its impact on child health (Lépine and Strobl, 2013), maternal health care (Bloom et al., 2001) or child labor (Reggio, 2011).

empowerment measures in governorates experiencing a high level of protest intensity. We also suggest estimations using demonstration intensity at a more spatially disaggregated level and a treatment variable directly pertaining to women’s participation in the protests.

We find a significant increase in women’s final say in the regions most affected by the Arab Spring events. Empowerment increases by a magnitude of 12 – 18% depending on modelling choices. We also point to a decline in women’s acceptance of domestic violence and a large reduction in the intention to excise their daughters. We explore the sensitivity of our results to alternative sets of controls and specifications, accounting for possibly changing structures of the population, propensity score reweighting and fixed effects in a pseudo-panel approach. We show that the intensity of protests was not correlated with the initial empowerment levels nor with the trend in empowerment prior to the events. Triple difference estimations based on couples’ age reinforce these results. Finally, using discrete groups of increasing intensities of treatment conforms to the intuition of a monotonic relationship between the empowerment effect and the degree of exposure to the protests.

Our favorite interpretation recognizes that women’s visibility in the street protests had a strong impact on those who took part or witnessed them in the locality, possibly pervading the private sphere by changing women’s (and men’s) perception about their role and rights. We suggest an extensive discussion of alternative interpretations of our findings. Even though women’s labor market participation is strongly associated with household agency, we show that its variation during the event was at best a minor mediating effect. Policy reforms, including the 2014 constitutional changes, took place after the period under study while their enforcement remains in question. Migration and sex ratios do not seem to constitute plausible channels. We end the paper by discussing the implications of our results, their limits and possible extensions for future research.

## 2 Literature and Contribution

This paper relates to recent studies on the way women’s political representation can weaken stereotypes about gender roles. Mechanisms leading to these changes may pertain to an increasing number of women entering male-dominated areas, hence reducing statistical discrimination against women – or to just a few iconic women playing a role model that may change behavior. This literature shows that increased political representation of women improves voters’ opinion about female leaders ([Beaman et al., 2009](#)), aspirations regarding

girls' education and participation to household chores (Beaman et al., 2012), inspiration for women entrepreneurs (Ghani et al., 2014) or women's confidence to report sex crimes (Iyer et al., 2012). Noticeably, the bulk of this literature focuses on electoral quotas (reserved seats in parliament or more decentralized councils), which may be specific and sometimes ineffective.<sup>2</sup> In contrast, we exploit the increased visibility of Egyptian women following an unprecedented surge in political participation at every stages of the Arab Spring revolution (we will show that geographical variation in women's activism is not driven by omitted variables that would explain an otherwise change in intrahousehold empowerment).

This literature on female political participation rarely considers the impact of political activism on the domestic sphere. Noteworthy, a parallel literature focuses on how 'imported norms' and counterstereotypes affect intrahousehold decisions. Jensen and Oster (2009) shows for example that the exposure to strong women characters on television increases Indian women's decision power and intolerance for domestic violence. La Ferrara et al. (2012) also demonstrate that exposure to soap operas, where the majority of female characters have no or one child, have influenced fertility choice in Brazil. Our contribution lies at the interface between these literatures, as we document how the sudden participation of women to a male-dominated public space affects gender norms in the household. Few quantitative studies have examined the relationship between women's socioeconomic or household empowerment and political outcomes. They point to a positive association - and a likely mutually reinforcing process - between women's political participation and various measures of empowerment like the ability to leave the home and to socialize with friends (Chhibber, 2002), the access to economic networks (Prillaman, 2016) and household agency (Bleck and Michelitch, 2018).

More generally, this paper contributes to the understanding of women's autonomy and its determinants in the context of developing countries (Duflo, 2012) and more particularly of MENA countries (OECD, 2017; Suad and Slyomovics, 2001). Using direct empowerment measures as we do, several studies have shed light on key determinants of women's position in the household, including women's labor market status (Anderson and Eswaran, 2009), gender norms (Mabsout and van Staveren, 2010), household structure (Debnath, 2015) or asset ownership (Allendorf, 2007; Mishra and Sam, 2016). Particularly in the case of Egypt, recent

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<sup>2</sup>Among exceptions, Bhalotra et al. (2017) study the impact of women's candidacy following a woman's electoral victory. These authors indicate that affirmative action may have disruptive effects (Deininger et al., 2015), may backlash against women (Gagliarducci and Paserman, 2011), or may just not eliminate negative stereotypes (Coate and Loury, 1993).

studies have focused on the impact of educational programs on women in conservative regions (Sieverding and Elbadawy, 2016; Elsayed and Roushdy, 2017). Sadania (2016) connects women’s labor market participation and their say within Egyptian households, showing that the empowering effect of employment depends on the type of occupation. A recent study is especially relevant to us: El-Mallakh et al. (2018) point to the effect of the Arab Spring on women’s employment using variation in protest intensity. We will liaise with their results hereafter. We are not aware of any study that would explicitly address the potential effect of Arab Spring events and female activism on gender norms and inequality within households: this observation motivates the present paper.

### 3 Data and Empirical Approach

#### 3.1 Individual Data and Empowerment

**The Egyptian DHS.** Our main empirical analysis is performed using the Egyptian Demographic and Health Surveys (DHS).<sup>3</sup> DHS are cross-sectional surveys containing a wealth of information about household characteristics, health and living conditions as well as specific questions about decision-making in the family. We focus on the 2008 and 2014 waves, two years surrounding the Arab Spring events. In 2008, and up until 2011, the political situation in Egypt was stable and no major event or change in the social or political context could have affected women’s empowerment. For the end period, the DHS was collected in April-May 2014, which corresponds to the end of the Arab Spring (election of El-Sissi in May 2014).

**Selection.** We restrict our sample to married women aged 15 to 49 years old in monogamous households. We exclude households from border governorates because of their proximity with strong terrorism movements (Sinai Peninsula) or with the Libyan conflict. This is not a strong restriction given that these governorates are not densely populated.<sup>4</sup> Our final sample is composed of 28,069 women over 2008 and 2014. In order to check the parallel

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<sup>3</sup>Note that we will also rely on several other datasets for the sake of analyzing the robustness of our results or the underlying mechanisms: the Survey of Young People in Egypt (SYPE), the Egypt Labor Market Panel Survey (ELMPS), the Egyptian Labor Force Survey (LFS) and the Egypt Household International Migration Survey (Egypt-HIMS). They will be described as we go through.

<sup>4</sup>According to the last Egyptian Census, only 2% of the total Egyptian population lived in these areas in 2006.



trend assumption of our difference-in-difference approach, we will make use of the 2000 wave: placebo estimations will rely on the pooled 2000 and 2008 waves (21,143 observations after applying the same selection criteria).

**Empowerment.** Our main outcome consists of direct empowerment measures drawn from the DHS. We select variables about intrahousehold decision-making regarding the woman’s health, her ability to visit relatives and important household expenditure. After normalization to positive values, each index gives a ternary outcome: 2 if the woman alone makes the decision, 1 if the decision is joint and 0 if the husband alone has the final say. We focus on a summary index of empowerment based on these questions. To aggregate the three dimensions, we rely on a Burt Multiple Correspondence Analysis (MCA), in a similar way as in [Lépine and Strobl \(2013\)](#). This index is normalized on a scale from 0 (no decision power) to 100 (full decision power). In robustness checks, we will consider each dimension separately as well as binary versions of these measures (taking value of 0 if both or only the husband make the decision and 1 otherwise). Statistics for all these indices are reported in Table A.1 in the Appendix. We also consider two additional outcomes related to women’s attitude towards gender roles, autonomy and well-being. The first one is drawn from a question on whether a husband is right to hit or beat his wife in diverse situations.<sup>5</sup> We construct a dummy equal to 1 if the women’s attitude shows tolerance towards at least one of these situations and 0 otherwise. The other is a dummy taking the value 1 if the wife intends to circumcise her daughters and 0 otherwise.

## 3.2 Arab Spring Exposure: Data and Treatment

**Summary of the Events.** To locate the following discussion on protest intensity, we first recall the four phases of the Egyptian Arab Spring. The first one corresponds to the 18 days of the revolutionary movement that overthrew Hosni Mubarak (January 25 2011 to February 11 2011) but also caused many casualties. This event followed the demonstrations in Tunisia in December 2010, which created an unpredictable shock wave across MENA countries. Mubarak’s fall was followed by a period of military rule: the Supreme Council of the Armed Forces was supposed to ensure the democratic transition but was also responsible for many misdemeanors on protesters. In June 30 2012, the candidate of the islamist Muslim

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<sup>5</sup>Unfortunately, the 2008 DHS wave does not contain any information regarding the actual experience of domestic violence.

Brotherhood movement, Mohammed Morsi, was elected president. Governmental actions were complicated by the international situation and electors urged government to resolve economic and security problems. At the end of June 2013, a massive movement called for Morsi's resignation, leading to his arrest and to the violent repression of islamists and Morsi's partisans under the second military regime, led by Interim President Adly Mansour. The militaries stayed in power until the election of president El-Sissi in June 2014, followed by a period of economic and social stability until today.

**Incidents Data.** Our main source of variation is the intensity of protests at local levels, proxied by the proportion of incidents during the Arab Spring events. We draw this measure from the Egyptian Revolution Database, collected by the Egyptian Center for Economic and Social Rights over the period January 2011 - December 2014. This dataset gathers information on the number of people arrested, injured and killed over the period.<sup>6</sup> We assume that our potential effect is driven by the conflicts occurring over the whole period. This includes the 18 days of the revolution (January 25 2011 to February 11 2011), the first military council (February 11 2011 to June 30 2012), the islamist regime of Morsi (June 30 2012 to July 3 2013) and the second military regime (4 July 2013 to El-Sissi's election).<sup>7</sup> We will check the sensitivity of our results to the timing, focusing alternatively on incidents occurring only during the first phases of the revolution. Note that the total number of casualties (injuries, arrests) between 2011 and 2014 reaches 5,221 (44,453, 45,885) cases.

**Treatment based on Incidents.** We sum these incidents at governorate levels (baseline) and divide the aggregate by the population size of the governorate using Census information to obtain a measure of incidents per inhabitant. This incidents proportion can be seen as a proxy for the local conflict intensity and for the proportion of people, notably women, involved in the local demonstrations (recall that the participation rate of women was high at every stage of the process). Using the ranking of governorates along this measure, we define our treatment group as the set of governorates with above-median protest intensity. We will

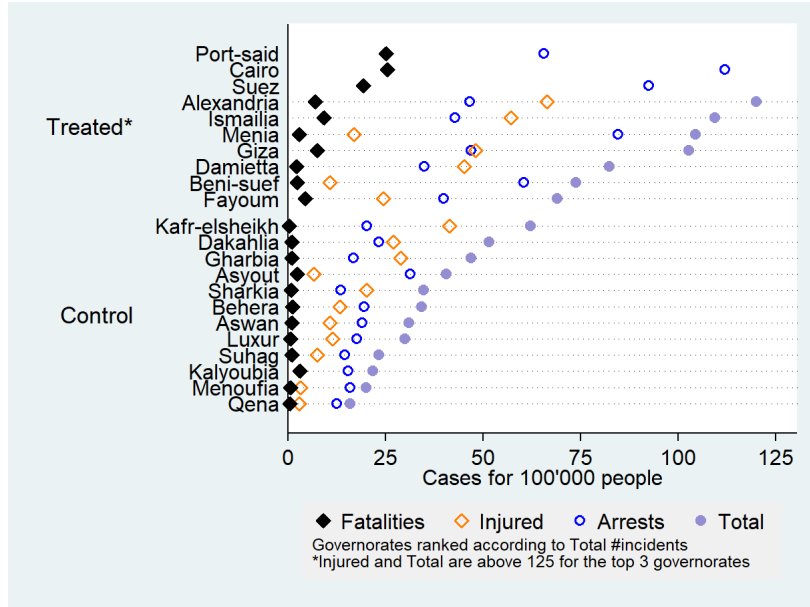
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<sup>6</sup>It was recorded as comprehensively as possible on the basis of press releases, human rights sources, the Student Observatory and the Freedom of Thought and Expression Foundation. It can be retrieved from Wiki Thawra, an independent website dedicated to documenting all the incidents since the onset of the Egyptian Arab Spring (see <https://wikithawra.wordpress.com>, in Arabic).

<sup>7</sup>In a similar vein, [El-Mallakh et al. \(2018\)](#) use the number of fatalities as a proxy for protest intensity, yet they focus on the first phase of the revolution including the early demonstrations and the first military regime (2011-12).

address the sensitivity of our results to alternative assumptions. The choice of governorates as the baseline perimeter is motivated by the fact that people did not necessarily demonstrated in their district. This is especially the case for the citizens of large cities (like those of Cairo, who converged to Tahir square). However, we will provide sensitivity analysis using more disaggregated perimeters. Note that aggregating incidents as different as fatalities, injuries and arrests in one score seems an arbitrary way to grasp the extent of the demonstrations. In fact, Figure 1 actually shows that using each measure separately leads to a very similar grouping of treated and controls.<sup>8</sup>

Figure 1: Protest Intensity: Incidents Rates by Governorate



**Alternative Treatment based on Women’s Participation.** Our measure of protest intensity is based on a comprehensive recording of all incidents. Yet, this is only a proxy for the magnitude of the protests in general and of women’s participation in particular. As an alternative treatment, we construct a governorate-level measure of women’s participation in the Arab Spring protests. We make use of the 2014 Survey of Young People in Egypt (SYPE), which contains information about 10,000 individuals aged 18-35 and living in non-border governorates. Individuals were retrospectively asked about their personal experience of taking part in the Arab Spring events. We construct a variable equal to 1 for women

<sup>8</sup>Arrests alone lead exactly to the same group of treated governorates, while this is almost the case for fatalities and injuries (reranking concerns only one and two governorates respectively).

who participated in at least one of the relevant political activities (provision of support to protesters, participation in any kind of protest demonstration, participation in strikes and in election campaigns) and 0 otherwise. We use answers by female respondents and the respondent's mother in order to account for a broader population than the youth (note however that the 18-35 represent 70% of the total population according to Census data). We calculate an average score by governorate, deflated by the governorate population, and take the above-median threshold to defined treated regions. This approach has its own drawbacks, notably the fact that it relies on survey data (which is less representative) and possibly suffers from recall bias. Nonetheless, this is an interesting alternative measure to check the sensitivity of our result. The most important fact is that it correlates much with our incidents measure: the treated regions overlap at 70% across the two rankings.

**The Geography of Protests and Empowerment.** We use regional variation in protest intensity as a potential variation in the exposure to women's involvement in the revolution. For this reason, it is interesting to provide descriptive statistics in the form of the geographical dispersion of the treatment intensity, as can be found in the first graph of Figure 2. We distinguish four groups: three different intensities of treatment among the 10 most exposed governorates (60-90, 90-120 and 120+) and the low-incident group (0-60). The second graph shows the spatial dispersion in our main empowerment measure prior to the Arab Spring events (the composite index based on decision power regarding health, socialization and purchases, drawn from the DHS and averaged at governorate level). We check whether strong protests took place in the most advanced locations in terms of gender equity. We actually do not observe any particular association of that sort: the cross-governorate correlation between protest intensity (graph 1) and the 2008 empowerment score (graph 2) is only .05. Hence, a rise in women's empowerment in regions most affected by the events may not be interpreted just as reflecting a fundamental heterogeneity in the initial position of women across regions of Egypt.<sup>9</sup>

The third graph of Figure 2 depicts the situation in 2014 while the last graph shows the

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<sup>9</sup>Note that there is a great diversity of norms even within conservative regions of Upper Egypt (Ibrahim and Wassef, 2001). Several studies explain that concerns for democracy and gender equity are relatively uncorrelated from one another (Kostenko et al., 2016). However, it is true that there is a correlation between protest intensity and other dimensions, notably due to the fact that events mainly took place in urban areas - hence the high correlation with the proportion of urban households (.78) and, subsequently, a positive correlation with education levels (.44) and a negative correlation with family size (-.43).

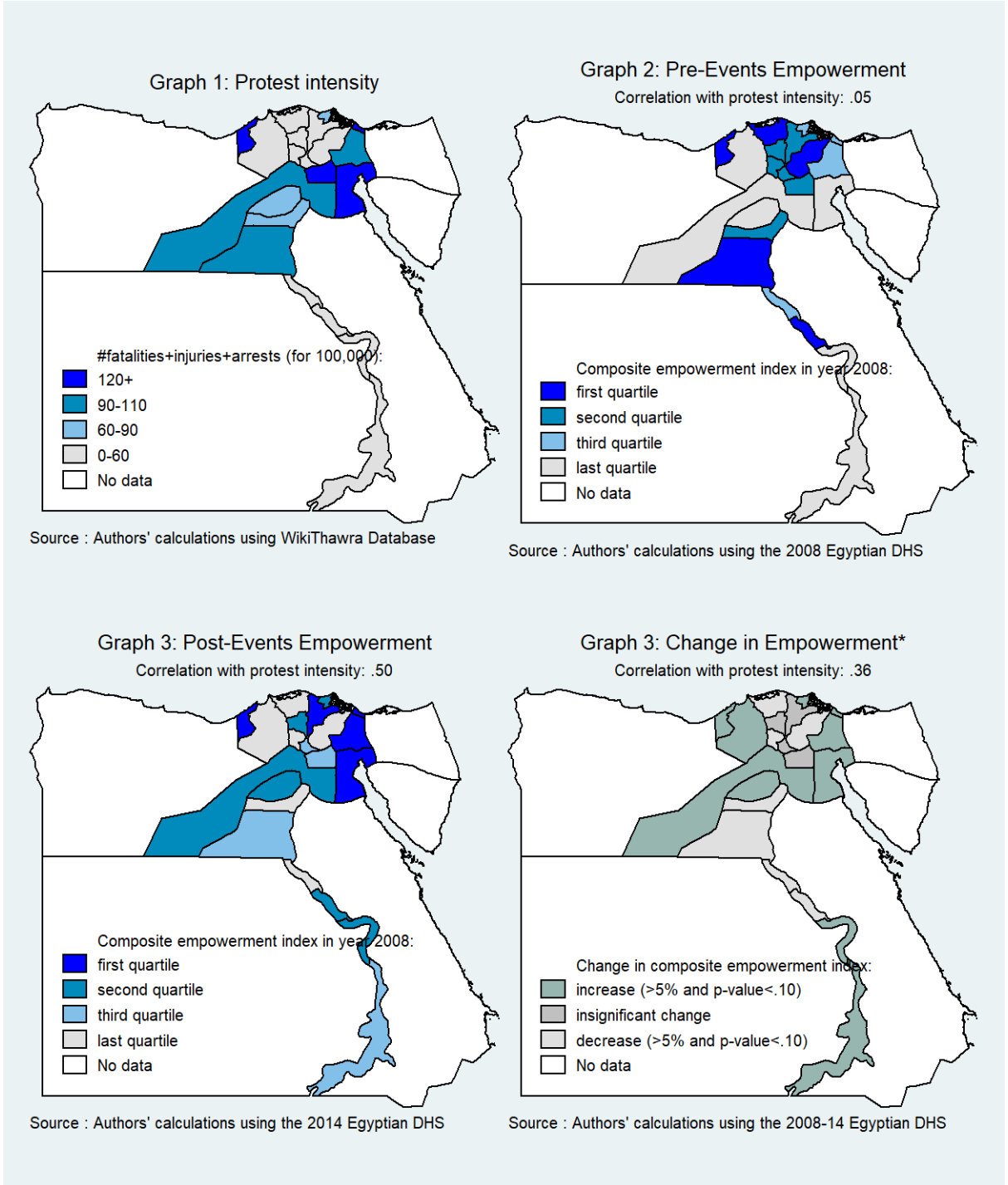
change in empowerment over the period. The improvement in women’s bargaining position following the Arab Spring was not generalized: the change in empowerment is insignificant overall (p-value of .69) while a significant increase is found in only 10 of the governorates (Alexandria, Aswan, Behera, Damietta, Fayoum, Giza, Ismailia, Post Said, Qena and Suez). Yet, improvements in female empowerment are not random, nor just correlated with the initial conditions of women, as we just saw. They seem highly correlated with conflict intensity. Comparing these graphs hence give a sketch of our main result: the correlation between protest intensity and empowerment shifts from .05 prior to the events (graph 2) to .50 in 2014 (graph 3). This yields a .36 correlation between protest intensity (graph 1) and empowerment variation (graph 4). There is no one-to-one correspondence in ranks, but out of the 10 governorates with high-protest intensity (treatment group), 8 were subject to an increase in empowerment (7 of them with a rise larger than  $> 5\%$  and significant according to a t-test). The rest of the paper will attempt to check if this result holds when controlling for households’ characteristics in micro regressions.

**Raw Difference-in-Difference.** Before moving to the empirical approach, we can provide a raw difference-in-difference calculation based on average empowerment measures from the DHS in treated and control groups at base and end periods. Table A.1 indicates that for the main empowerment index, the double difference yields an estimate of around +4.8 points. We obtain a consistently similar estimate when using our alternative treatment variable based on women’s participation to protests. If we compare these effects to the base-period control group average, we conclude that households located in the ten most exposed governorates have seen women’s empowerment increase by around 16% relatively to the other regions before the Arab Spring. We will see that this magnitude is very close to what we obtain using micro regressions.

### 3.3 Empirical Approach

**Difference-in-Difference Estimations.** We denote  $Y_i$  the main outcome, namely the index of empowerment for a woman in household  $i$ ,  $TREAT_i$  the treatment variable equal to 1 if the household is located in a highly exposed governorate and 0 otherwise,  $POST_i$  the time dummy equal to 1 for the post-Arab Spring period (year 2014) and 0 for the base period (2008). Pooling the 2008 and 2014 waves of the Egyptian DHS, we estimate our main difference-in-difference model as follows:

Figure 2: Geography of Arab Spring Protests and Women's Empowerment in Egypt



$$Y_i = \alpha + \beta POST_i \cdot TREAT_i + \gamma POST_i + \delta TREAT_i + \eta X_i + \phi X_i \cdot POST_i + \epsilon_i.$$

The coefficient  $\beta$  on the interaction term is the difference-in-difference estimator, represent-

ing the effect of living in a highly exposed governorate after the events. The coefficient  $\gamma$  on  $POST_i$  captures the time trend in the outcome, including the overall effect of the Arab Spring that is common to all the governorates, as identified on the non-treated. The coefficient  $\delta$  on the treatment variable alone picks the average (time-invariant) difference between the two groups of governorates. Covariates  $X_i$  may improve the precision of the model but also control for the difference in observables between treated and control groups. They include individual characteristics (women’s and men’s age and education) as well as household information: wealth, urban dummy, number of children, religion (a dummy equal to 1 for Christian, 0 for Muslim) and marriage duration. In sensitivity checks, we add birth cohorts and municipality dummies, as well as interactions of  $POST_i$  and  $X_i$ .

**Identification Issues.** In the context of a double difference analysis, treated and control groups are not randomly chosen, and may be very different. Table A.1 in the Appendix indeed shows that the most exposed governorates are significantly richer, younger, more urban and educated than other governorates. The long-term differences are controlled for in  $X_i$ . Since controlling for these characteristics in a linear way may be too restrictive, we also suggest augmenting estimations with propensity score reweighting. To guarantee that the control group represents a valid counterfactual for the trend in empowerment over the few years under study, we must also control for group-specific trends in observables. Indeed, diverging trends in empowerment between groups in the absence of treatment may be due to different trends in observables characteristics, which we account for with the interaction  $POST_i \times X_i$ .

We must also assume that there is no time-varying unobservables that would affect the outcome trends of the two groups differently. A minimum requirement in this respect will consist in checking whether the outcomes of the two groups show parallel trend prior to the period under study, namely between 2000 and 2008. This point also relates to the central question of a potential endogeneity of the treatment ([Bertrand et al., 2004](#); [Besley and Case, 2000](#)). In our case, it would be on account of omitted variables that codeterminate a high intensity of protest and a rise in women’s empowerment. While we cannot rule out the possibility of regional unobservables of that sort, we will provide numerous checks that tend to indicate that the intensity of protest is relatively random - or at least uncorrelated with unobservable determinants of female empowerment. We assemble three pieces of suggestive evidence. First, when comparing the graphs 1 and 2 of Figure 2, we have concluded that fierce conflicts did not specifically occur in places where initial empowerment was particu-

larly high (i.e. places with a higher capacity of change) or low (places with a higher margin of improvement). Second, a successful test of the parallel trends will indicate that these regions were not necessarily having different empowerment trajectories than more conservative regions in the years prior to the events. Finally, we will carry out a triple-difference analysis using the duration in marriage to define an extra control group.

## 4 Results

### 4.1 Main Results

**Main Estimations.** In Table 1, we present our main results based on the difference-in-difference estimator using the ternary composite index of empowerment.<sup>10</sup> The upper panel shows baseline estimates of the treatment effect  $\beta$ . We obtain a very significant effect of around 4-5 points across the different models presented in columns 1-6. It represents an increase of between 14% and 18% in empowerment for women in the exposed governorates relatively to other regions prior to the Arab Spring. Estimates are remarkably stable throughout the different models. Model 1 is a minimalist version controlling for the basic set of variables  $X_i$  only. Model 2 adds birth cohorts, which would capture specific cohort effects in addition to age. Results are hardly changed. Model 3 controls for municipalities fixed effects, which leads to slightly (but not significantly) larger estimates. In model 4, we introduce time trends in observables characteristics: again, we do not see much variation in our treatment effect.<sup>11</sup>

We have accounted for observed differences between more or less exposed governorates using a linear specification of characteristics  $X$ . It is nonetheless possible to use matching techniques to relax the linearity assumption and to verify (or impose) common support. With a binary treatment variable, the simplest approach consists in estimating the propensity of being treated and using the inverse propensity score (PS) to reweight the data (Hirano et al., 2003). Denoting  $p_i = P(TREAT_i = 1)$  as the estimated probability of being in a region of high protest intensity for observation  $i$ , based on the set of variables  $X$ , we use the

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<sup>10</sup>Detailed estimation results are available from the authors. The main significant effects go as follows: education has a clear positive effect on empowerment, increasing with education categories; age and the intermediate and older birth cohorts also have positive effects as well as the intermediate group of marriage duration.

<sup>11</sup>In all models, we cluster standard errors at the municipality level. Inference does not change much if we opt for clusters at municipality x year level: standard errors decrease by around a quarter and all our conclusions are preserved.



Table 1: Effect of Arab Spring Events on Women's Empowerment: Baseline

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Period of interest: 2008-2014</i>						
Post x Treat	4.776*** (1.050)	4.759*** (1.048)	5.261*** (1.092)	4.469*** (1.111)	4.520*** (1.048)	4.281*** (1.042)
Relative effect <sup>a</sup>	0.159	0.159	0.176	0.149	0.151	0.143
Observations	27,341	27,330	27,330	27,330	27,330	27,330
R-squared	0.017	0.018	0.050	0.051	0.052	0.053
<i>Placebo: 2000-2008</i>						
Post x Treat	-0.758 (1.046)	-0.758 (1.046)	-0.924 (1.101)	-0.136 (1.063)	-0.424 (1.083)	-0.228 (1.065)
Relative effect <sup>a</sup>	-0.029	-0.029	-0.035	-0.005	-0.016	-0.009
Observations	20,544	20,544	20,544	20,544	20,544	20,544
R-squared	0.021	0.022	0.082	0.085	0.085	0.088
Individual Controls $X_i$	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohorts	No	Yes	Yes	Yes	Yes	Yes
Municipalities	No	No	Yes	Yes	Yes	Yes
$POST_i \times X_i$	No	No	No	Yes	No	Yes
PS reweighting	No	No	No	No	Yes	Yes

Linear estimations based on 2000, 2008 and 2014 DHS. Treatment: above-median protest intensity based on the Egyptian Revolution database (governorate-level proportion of fatalities, injuries and arrests).

Standard errors in parentheses are clustered at municipality level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

<sup>a</sup> Effect relative to the control group pre-period average empowerment, in %.

weights  $\frac{1}{1-p_i}$  and  $\frac{1}{p_i}$  for non-treated and treated observations respectively.<sup>12</sup> According to model 5, results appears to be robust to the addition of these weights. The same is true in model 6 when time trends in  $X_i$  are also included. This means that a linear form of  $X_i$  already managed to control relatively well for differences in characteristics between treated and control groups.

**Placebo Checks.** Our placebo estimations aim at checking whether the parallel trend assumption holds, i.e. whether treated and control governorates had similar empowerment trends before the Arab Spring. We do so by replicating our estimations on the 2000 and

<sup>12</sup>We have checked that the mean of each covariate in  $X$ , as well as the mean propensity score, is approximately equal across the treatment and control groups once these weights are used.

2008 DHS waves, as explained in the data section.<sup>13</sup> Thus,  $POST_i$  takes a value 1 for women observed in 2008 and 0 for those of 2000. The lower panel in Table 1 shows the placebo estimates of  $\beta$  for the specifications 1-6. None of them are statistically different from zero: living in the areas that would later be highly exposed to the Arab Spring did not imply a specific trend in terms of woman’s intra-household bargaining power.

## 4.2 Sensitivity Analysis

We now proceed with a series of robustness checks for our difference-in-difference estimations. They are based on the most complete linear model (4th model of Table 1). Yet, the following results barely change when using other specifications including PS reweighting.

**Alternative Sample Selections and Pseudo-Panel.** Ideally, we would like to follow the same women over time to ensure the stability of the sample and account for individual effects. In the absence of panel data, we first suggest an artificial ‘aging’ of the sample aimed to focus on the same cohort distribution. We simply retain married women aged 15-43 years old in 2008 and 21-49 years old in 2014. The final sample is reduced by 9.5% compared to our baseline selection. Results in Table 2 indicate that the magnitude of the effect (column 2) is similar to the main estimation (column 1), only slightly but not significantly larger. In the same vein, a second check explicitly addresses the question of marital separation. It may well be the case that the empowerment effect highlighted in this paper also caused an increase in divorce and separation. Using the Egyptian Labor Force Survey (LFS), we find that marriage rates have decreased regularly over the 2011-14 period, from 64.18% to 63.80%, and more so in highly exposed governorates (a raw difference-in-difference on marriage rates gives an insignificant  $-.78$ ). This change is probably too small to affect our results. Nonetheless, we replicate our estimations on a sample with women married for at least 4 years, so that those who potentially divorced and remarried during the Arab Spring are ignored. The effect is a little smaller but very close to the baseline (column 3). Combining both adjustments again leads to something very close (column 4). Finally, to average up individual unobservables that may still act as confounders, we construct a pseudo-panel using our selected sample, following [Devereux \(2004\)](#). We define cells based on 22 governorates, 3 birth cohorts and 2 locations (urban/rural), hence a total of 132 cell types.<sup>14</sup> We replicate our estimations using the mean values of all the variables in the model, weighting estimations by cell size

<sup>13</sup>Note that religion is not included in this estimation since it was not available in the 2000 DHS.

<sup>14</sup>We impose a minimum number of observations per cell, which leads us to discard 6 cells.

and including a cell fixed effect. The 5th column of Table 2 shows that the pseudo-panel difference-in-difference estimation yields a close estimate to the baseline, slightly larger but not significantly so.

**Interview Conditions.** As noted by [Lépine and Strobl \(2013\)](#), the main issue when using direct proxies for empowerment is the measurement error in women’s bargaining power that could result from an attenuation bias due to the presence of husbands or other men during the interview. We have performed several checks and did not find very large differences in this respect. In particular, we have replicated our estimations on a subsample excluding women who have been interviewed in the presence of their husbands. The estimate hardly changes compared to the baseline (column 6 of Table 2). We also exclude women interviewed in the presence of any relative: the empowerment effect increases a little but not significantly (column 7).<sup>15</sup>

Table 2: Effect of Arab Spring Events on Women’s Empowerment: Alternative Samples

	Baseline	Same cohort <sup>b</sup>	No divorce <sup>c</sup>	Same cohort & No divorce	Pseudo-panel <sup>d</sup>	Interview: no Husband <sup>e</sup>	Interview: alone <sup>f</sup>
Post x Treat	4.469*** (1.111)	4.838*** (1.121)	4.158*** (1.131)	4.552*** (1.147)	5.198*** (1.288)	4.487*** (1.137)	4.893*** (1.221)
Relative effect <sup>a</sup>	0.149	0.162	0.148	0.161	0.156	0.175	0.186
Observations	27,330	24,726	23,836	21,807	264	25,586	20,556
R-squared	0.051	0.051	0.049	0.049	0.243	0.052	0.060

Linear estimations based on 2008 and 2014 DHS. Treatment: above-median protest intensity based on the Egyptian Revolution database (governorate-level proportion of fatalities, injuries and arrests). The model includes individual controls  $X_i$ , birth cohorts, municipality dummies and  $POST_i \times X_i$  (in the model with pseudo-panel, fixed effects absorb birth cohorts). Standard errors in parentheses are clustered at municipality level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

<sup>a</sup> Effect relative to the control group pre-period average empowerment, in %.

<sup>b</sup> Same cohort: women aged 15-43 in 2008 and 21-49 in 2014

<sup>c</sup> No Divorce: women married for at least 4 years to avoid divorces/new marriage over 2011-14

<sup>d</sup> Pseudo-panel: estimations on 126 cells (cohort x governorate x urban) x 2 years

<sup>e</sup> Interview without men being present including the husband

<sup>f</sup> Interview without anyone else being present

<sup>15</sup>Note that the presence of other people during the interview is recorded specifically for the question on domestic violence, which comes just after final say questions.

**Time and Spatial Variation in Treatment.** We finally check the sensitivity of our analysis to the time and spatial definition of the treatment group. We start with alternative timings: results hold when focusing on the treatment generated by the early phases of the revolution. In particular, results are consistently close if we exclude from our aggregated measure of protest intensity the incidents that occurred under Mansour’s regime (i.e. starting July 2013), as a majority of these incidents corresponded to the repression against islamists.<sup>16</sup> Then, we address the spatial variation in protest intensity. We have argued that a person’s strict locality is too narrow to capture her potential participation to the events. While this has motivated the choice of governorates, the latter are sometimes very large. Thus, we suggest two alternative definitions. First, we compare our baseline results to an estimation where we classify regions within governorates as urban and rural (while Cairo and Alexandria are completely urban, for instance, other governorates contain both urban and rural regions). Since women in urban regions were more likely to participate in the protests, we define the treated as urban households experiencing above-median protest intensity. In Table 3, the resulting effect (column 2) is of the same order of magnitude as the baseline (column 1).<sup>17</sup> We also suggest a more disaggregated perimeter based on a person’s municipality (so-called Markaz/Kism) and the relevant border municipalities.<sup>18</sup> The treatment group is defined as households living in extended municipalities that experience high protest intensity, using the same threshold (in terms of incidents rate per inhabitant) as in the baseline. Table 3 points to a smaller estimate (column 3) but a relative effect - i.e. the response relative to control group municipalities prior to the events - of a similar order of magnitude to the baseline. Again, the coefficient is only a little lower when focusing on urban municipalities while the relative effect is similar (column 4).

### 4.3 Women’s Political Engagement and Empowerment in the Home

While we consider several alternative channels in the next subsection, we expand here the discussion about the most likely mechanism at work: the intensity of protests is associated

<sup>16</sup>As a matter of fact, these events were equally interesting as they were showing the intensity of rejection against the Muslim Brotherhood movement, which had never disapproved the violence experienced by female protesters (and beyond) and had even attempted, under Morsi’s regime, to decriminalise female genital mutilation.

<sup>17</sup>The fact that it is slightly smaller may simply reflect some heterogeneity in response between rural and urban households (i.e. a larger effect among treated rural households, as captured in baseline estimates).

<sup>18</sup>To exclude low-populated subdivisions corresponding to vast desert areas, we do not include border Markazs whose centroid is located at more than a 100km.

Table 3: Effect of the Arab Spring Events on Women Empowerment: Alternative Geographic Definition of Treatment

Geogr. Perimeter:	Governorate	Governorate	Municipality (b)	Municipality (b)
Treatment includes:	All (baseline)	Urban only	All	Urban only
Post x Treat	4.469*** (1.111)	4.108*** (1.027)	2.236*** (.859)	1.856*** (.864)
Relative effect <sup>a</sup>	0.149	0.131	0.119	0.120
Observations	27,330	27,330	27,330	27,330
R-squared	0.051	0.050	0.048	0.048

Linear estimations based on 2008 and 2014 DHS. Treatment: above-median protest intensity based on the Egyptian Revolution database (local proportion of fatalities, injuries and arrests). The model includes individual controls  $X_i$ , birth cohorts, municipality dummies and  $POST_i \times X_i$ . Standard errors in parentheses are clustered at municipality level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

<sup>a</sup> Effect relative to the control group pre-period average empowerment, in %.

<sup>b</sup> More disaggregated area including the person's municipality (markaz/aqsam) and relevant border municipalities.

with women's political activism and a subsequent change in women's empowerment in the home. We complete our analysis with a triple difference analysis and attempt to validate our treatment variable against a more specific measure of women's engagement in the democratic movement.

**Interpretation.** The variation in protest intensity, and of women's participation to the events, possibly entails different degrees of awareness about gender rights and their pervasive consequences at home. The effect may not even be driven by women alone. Exposure to members of another group creates empathy that can alter social norms (Boisjoly et al., 2006), which might have happened for men and women standing alongside in the demonstrations.<sup>19</sup> Arguably, not all women were on the street, so it may well be the increased consciousness about women's fight, in more exposed regions, that had been able to trigger some progress at home. This mechanism relates to the social psychology literature, which points to the role of such an emotional climate on social sharing (Rimé et al., 2017) as well as on the idea that repression galvanizes action and increases identification with the movement (Ayanian and Tausch, 2016). It also connects to the literature on role models, as extensively discussed in the section 2. Finally, our results support the view that actual demonstrations were what matters

<sup>19</sup>Exposure to conflicts may also increase cooperative behavior (Beekman et al., 2017), egalitarianism (Bauer et al., 2014) or altruism (Voors et al., 2012).

for the outcomes under study. [Acemoglu et al. \(2014\)](#) point to the fact that other platforms of exchange like the social media served essentially as a mobilization and coordination device of the protests - yet it was street protests that had the strongest impact on those who took part or witnessed them in the locality, as they were concrete materializations of the conflict. We come back to this point in the concluding discussion.

**Heterogeneity and Triple Differences.** An indirect way to check the role of women’s protest participation is to provide an additional degree of heterogeneity. We argue that a couple’s age can be seen as a filter on its ability to be effected by the events. Indeed, older women are less likely to participate in the protests and hence to be exposed to the wind of change.<sup>20</sup> Under this assumption, the double difference estimate on older couples should capture confounding factors that explain region-specific changes in decision-making power not related to the events. Such a triple difference hence completes our placebo checks.<sup>21</sup> We denote  $Z$  a dummy for younger couples. Results are reported in Table 4 using less than 20 years of marriage as the threshold (for sensitivity checks, we use 25 years of marriage: results are very similar).  $POST \times TREAT$  captures the treatment effect for those less exposed to the protests (the older couples). It is insignificant in both cases.  $POST \times TREAT \times Z$  captures the additional treatment effect on those most exposed (the younger couples): we find a significant effect of a similar magnitude as our baseline double-difference estimate. The lower panel of Table 4 also reports placebo estimations: there is no sign of different empowerment trends across marriage durations prior to the events.

**Alternative Treatment Variable: Women’s Protest Participation.** The previous results about a demonstration effect on female empowerment have relied on a measure of protests visibility proxied by the local proportion of dramatic incidents. Yet, it is imperfectly correlated with the proportion of demonstrators and, in particular, with that of women. Alternatively, we must use a more direct measure of women’s engagement in the protests.

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<sup>20</sup>Note that at the same time, older couples are not necessarily more unbalanced regarding household decisions: our estimations show that women’s age affects power positively while marriage duration has a concave effect on empowerment.

<sup>21</sup>We have checked that exposed regions did not exhibit specific empowerment trends prior to the events. Yet, unobservables may still explain upwards trends in empowerment in treated regions even in the absence of the Arab Spring ([Besley and Case, 2000](#)). The triple difference is supposed to capture their effect through the differentiated time change in empowerment among older couples relatively to younger ones, if age filters the protests effect.

Table 4: Effect of the Arab Spring Events: Triple Differences

Sensitivity to Treatment Z:	Less than 20 years of marriage	Less than 25 years of marriage
% of the sample with Z=1:	0.74	0.87
<i>Period of interest: 2008-2014</i>		
Post x Treat x Z	4.196*** (1.183)	3.906** (1.650)
Post x Treat	1.408 (1.500)	1.108 (1.838)
Observations	27,330	27,330
R-squared	0.051	0.051
<i>Placebo: 2000-2008</i>		
Post x Treat x Z	-1.974 (1.327)	-1.363 (1.508)
Post x Treat	1.212 (1.447)	1.006 (1.707)
Observations	20,544	20,544
R-squared	0.085	0.085

Linear estimations based on 2000, 2008 and 2014 DHS. Treatment: above-median protest intensity based on the Egyptian Revolution database (governorate-level proportion of fatalities, injuries and arrests). The model includes individual controls  $X_i$ , birth cohorts, municipality dummies,  $POST_i \times X_i$  as well as  $POST_i \times Z_i$ ,  $TREAT_i \times Z_i$ ,  $POST_i \times TREAT_i$  (the effect for those less sensitive to treatment in exposed governorates) and  $POST_i \times TREAT_i \times Z_i$  (DDD: additional effect for those more sensitive to treatment in exposed governorates). Standard errors in parentheses are clustered at municipality level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

In the data section, we have described another survey (SYPE) and the calculation of a population-adjusted measure of women's protests participation. Based on survey data and individual declaration, this measure is more fragile and less representative than the Revolution Database, which offers a comprehensive account of protest intensity around Egypt. At the same time, the former relates more directly to the mechanism invoked above and hence provides an interesting verification. Table 5 compares our baseline (in column 1) with

using women’s participation in protests as a treatment (in column 2). It turns out that both estimates are of the same order of magnitude. In relative terms, the effect based on women’s participation as treatment points to a slightly smaller increase in empowerment (+13.1%) compared to that based on protest intensity (+14.9%). While both treatment measures have their own caveats, their congruence is reassuring and may indicate that they capture closely related dimensions of the relevant exposure/treatment. Note that the effect hardly changes due to the presence of male relatives during the interview (column 3).

Table 5: Effect of the Arab Spring Events on Women Empowerment: Alternative Treatments

	Protest intensity (baseline)	Binary Treatment		Rank <sup>c</sup>		Discrete Treatment <sup>d</sup>	
		Women’s participation	Women’s participation <sup>b</sup>	Protest intensity	Women’s participation	Protest intensity	Women’s participation
Post x Treat	4.469*** (1.111)	3.622*** (1.027)	3.544*** (1.048)	0.190** (0.0782)	0.221*** (0.0793)		
Post x High Treat.						6.150*** (1.250)	6.351*** (1.177)
Post x Interm. Treat.						4.170*** (1.385)	3.341** (1.407)
Post x Low Treat.						3.436* (1.783)	1.221 (1.138)
Relative effect <sup>a</sup>	0.149	0.131	0.118			0.153	0.131
Observations	27,330	27,330	25,586	27,330	27,330	27,330	27,330
R-squared	0.051	0.050	0.052	0.049	0.051	0.051	0.052

Linear estimations based on 2008 and 2014 DHS; treatment variables: "protest intensity" based on the Egyptian Revolution database (proportion of fatalities, injuries and arrests) and "women’s participation" based on the 2014 SYPE survey (proportion of women engaged in Arab Spring demonstrations). The model includes individual controls  $X_i$ , birth cohorts, municipality dummies and  $POST_i \times X_i$ . Standard errors in parentheses are clustered at municipality level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

<sup>a</sup> Effect relative to the control group pre-period average empowerment, in % (with discrete treatment: average effect over the 3 groups)

<sup>b</sup> Interview without men being present including the husband

<sup>c</sup> Rank from the scores of protest intensity / women’s participation (coefficients not comparable to binary/discrete treatment)

<sup>d</sup> Discrete treatment groups: governorates ranked 1-3 (high), 4-6 (interm.), 7-10 (low), the reference being the control group.

We pursue this comparison for other types of treatment variables. In the baseline, the threshold applied to determine treated governorates was arbitrarily set to the median. In columns 4-5 of Table 5, we now report additional results based on a continuous measure of treatment: it is simply calculated as the governorate ranks in the intensity of treatment.



We obtain again very similar results for both protest intensity and women’s participation: they show a significant and positive effect of the governorate’s ranks for both measures. In columns 6-7, we study possibly nonlinear treatment effects. We split governorates in 4 groups of increasing intensity: high intensity (top 3 governorates), medium (next 3), low (next 4) and the control group. If we suspect that some of the governorates drive our results, then other groups should show insignificant effects. If, on the contrary, our measures really carry some information on a relevant intensity of treatment, we expect the effect to monotonically increase with our measures. This is indeed what we observe in Table 5. With protest intensity, all three groups of treatment show significant effects of gradually increasing magnitudes. The result is similar with women’s participation: only the low-intensity group is insignificant (which means that it could have been classified as part of the control group). In both cases, the relative effect calculated as the mean impact over the three groups yields a response that is close to what we obtained with binary treatments (15% and 13% respectively).

#### 4.4 Alternative Mechanisms

We now consider several alternative interpretations of our central findings. We ask whether protest intensity may capture other channels through which intrahousehold decision making could have been changed. While there is no definitive answer to this question, most of the plausible mediating factors seem unlikely to dominate our hypothesis about women’s participation to the Arab Spring events.

**Labor Markets.** The first interpretation pertains to the increased labor market participation of women in places affected by higher degrees of conflicts and, hence, more damaged local economies. This is related to the ‘added worker’ effect characterized in [El-Mallakh et al. \(2018\)](#). The first stage of this analysis questions whether treated governorates have experienced a relative increase in female labor market participation. With our initial treatment variable - protest intensity - the effect of the Arab Spring events on female labor supply is insignificant.<sup>22</sup> Notwithstanding, our results align to [El-Mallakh et al. \(2018\)](#)’s when we

<sup>22</sup>This result is not necessarily at odd with [El-Mallakh et al. \(2018\)](#) who focus on a different period of treatment (2011-12) and, most importantly, point to an increase in married women’s labor force participation *relative* to their husband’s. Also, they find an employment effect mostly in low-paid informal work. The DHS does not allow us to replicate these results in detail as it does not contain information on husbands’ employment or on women’s employment type.

use women’s protest participation as treatment variable. In this case, we find a significantly positive impact of the Arab Spring on female labor market participation.<sup>23</sup>

Then, the easiest way to check the potential role of labor markets is to introduce the women’s employment dummy in the empowerment estimation. The mediating effect should be measured by the subsequent decrease in our double difference coefficient. When using protest intensity to define treated governorates, the coefficient is hardly affected. This result reflects the aforementioned absence of labor market effects when using protest intensity as the source of variation. When using the variation in women’s protest participation, the coefficient on  $POST \times TREAT$  decreases by around 4% – 5% depending on the specification.<sup>24</sup> Thus, it seems that female employment is not a strong explanatory factor overall.<sup>25</sup> Nonetheless, these results do not preclude women’s employment from playing an important role in general. The female employment dummy has a very significant and relatively large effect on empowerment (for a comparison, its magnitude is about two-third that of the Arab Spring effect).

**El-Sissi’s Political Reforms.** A second determinant could be found in policy changes. In particular, equal rights and protections for women were enshrined in the newest Egyptian constitution of January 2014, with the affirmation of “state’s commitment to protect women against all forms of violence”. Laws forbidding discrimination based on gender were included while women were officially given access to higher judiciary positions for the first time. Quotas were also introduced (‘one quarter of the seats’ for women) in the elected local councils. Yet, the gains made in legal rights are too recent to plausibly empower women in such a short-term period, all the more so as their enforcement remains to be seen. The gender discourse and policy reforms in the El-Sissi era can be viewed as a consequence of the

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<sup>23</sup>Results are reported with other outcomes, which are discussed later, in Appendix Tables A.2 and A.3. We distinguish overall work and paid work (columns 5 and 6 of the lower panels of these tables).

<sup>24</sup>We reach the same conclusions when adding both the work dummy and its interaction with *POST*. Note also that our estimations control for household wealth. Results also hold when imputing regional relative economic opportunities of men and women, as proxied by their relative unemployment rates, taken from the LFS, at the governorate level.

<sup>25</sup>Notice that women’s employment is an important aspect of the gender question, yet not without ambiguity. Indeed, an added worker effect does not necessarily lead to higher women’s empowerment if it simply corresponds to intra-household risk sharing in households with long-term commitment (La Mattina, 2017). Some studies even show that increased female participation among poor households may increase the risk of domestic violence if husbands seek to reassert control after a woman enters the labor force (Heath, 2014).

massive mobilization of women, not something causing the empowerment effect under study. An alternative definition of our treated governorates that excludes the 2014 events leads to the same governorate classification as in the baseline. This means that the few events of the last phase of the revolution did not change the geography of treatment in a way that would have triggered more local enforcement of the new laws.

**Migration.** A third mediating effect could be the role of migration. There are limited chances that more empowered women did self-select into more exposed regions (which would bias our estimate upward). Yet it is possible that out-migration from highly exposed governorates created selective attrition – for instance if those who left were specifically against pro-gender equity ideas. Ideally, we would like to compare baseline results to the estimates obtained from a subsample of geographically stable people. Unfortunately, the DHS is not a panel and has no information on mobility. Hence, we suggest an additional check based on the Egypt Labor Market Panel Survey (ELMPS), a nationally representative panel used by [El-Mallakh et al. \(2018\)](#). We focus on the 2012 data (49,186 observations) and follow our baseline selection (married couples, aged 15-49, excluding boarder governorates). We use retrospective information on the dates of previous geographical moves to define a dummy equal to 1 in case of a move to another governorates or abroad in the years 2011 or 2012. A simple regression of this dummy on standard controls and  $TREAT_i$  yields an insignificant effect (p-value=.532).<sup>26</sup> This check is reassuring but does not cover the last year of our studied period. Hence, we also rely on the Egypt Household International Migration Survey (Egypt-HIMS). This study was conducted in 2013 by the Central Agency for Public Mobilization and Statistics (CAPMAS) as part of the MED-HIMS program, which explicitly focuses on migration in a set of mediterranean countries.<sup>27</sup> The sample (83,269 households) allows calculating the net migration rate by governorate between the previous census (2006) and 2013. We regress it on  $TREAT_i$ , which yields again an insignificant effect (p-value=.329): there is no evidence of differentiated migration patterns that could drive our results.

<sup>26</sup>The proportion of movers among highly exposed governorates was 1.58%, indeed not much larger than among control governorates (1.49%).

<sup>27</sup>We are grateful to Ragui Assaad for information about the data and Ihab Mahmoud Gad and the CAPMAS for access to the Egyptian module of the MED-HIMS. This program is a joint initiative of the European Commission, the World Bank, UNFPA, UNHCR, ILO, IOM and the League of Arab States, see: <http://ec.europa.eu/eurostat/web/european-neighbourhood-policy/enp-south/med-hims>

**Sex Ratio.** Finally, a last channel connects our study to the recent literature on conflicts and gender empowerment. [La Mattina \(2017\)](#) finds increased domestic violence and reduced decision-making power among women who married after the genocide in Rwanda and lived in regions where many men died. The explanatory channel, i.e. a substantial change in the sex ratio in these localities, does not apply to Egypt. Indeed, we have checked that the number of casualties from the Arab Spring events, even if large, cannot have significantly affected the sex ratio in Egypt (both at governorate and municipality levels).

## 4.5 Other Outcomes

**Alternative Empowerment Measures.** We extend our results to variants of the empowerment composite index used in previous estimations. Appendix Table A.2 (upper panel) presents a series of estimates using alternative measures. We first report results based on a MCA combining the ternary answers about health, socialization and purchase decisions plus the woman’s labor market participation (column 1). The effect is around 17% smaller but of similar order of magnitude and still very significant. We also use an alternative MCA calculation based on the three decision-making questions normalized to binary outcomes.<sup>28</sup> This index (column 2) or a variant including labor market participation (column 3) yield results that are relatively larger in terms of relative effect (+22% to +30%) but much less precisely estimated. We also isolate each of the three decision-making variables as dependent variables (presented as normalized ternary outcomes: 0 for husband’s decision, 1 for joint decision and 2 for wife’s decision). The effects are all significant (columns 4-6) but vary in magnitude, from +5.4% for social life to +17.6% for health-related decisions. We have also replicated these results with the treatment measure based on women’s protests participation. Appendix Table A.3 (upper panel) shows empowerment effects very much in line with the results discussed above.

**Domestic Violence.** In the lower panel of Table A.2, we focus on other questions related to women’s perception about their integrity and well-being. The first question addresses women’s tolerance regarding domestic violence (a dummy equal to 1 if she disagrees with any justification for the use of violence). We run the same estimation as before using this dummy in linear probability models. Interestingly, the effect obtained on our selected sample

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<sup>28</sup>That is, 1 if the women makes the decision alone and 0 otherwise: we prefer to categorize joint decision as a sign of low empowerment since these dimensions concern the respondent’s own health privacy and socialization.

is not statistically different from zero overall (column 1) but it becomes significantly negative (a decrease in the acceptance of violence of 11%) when we restrict our sample to those interviewed without the presence of other people listening (column 2). These results somehow temper the improvement in final say found previously, if women are not free to express their opinions in front of their husband or other males. Yet, this may be only a selection effect if women interviewed in the presence of their husband are also less empowered women.<sup>29</sup> Note that when considering women’s participation to define treated governorates, we find insignificant effects whatever the conditions of interview (Table A.3, lower panel).

**Girls’ Circumcision.** Another serious aspect of the discussion about women empowerment in Egypt pertains to female genital mutilation (FGM). This ritual involves the partial or full removal of the external female genitalia. It is rooted in gender inequality and attempts to control women’s sexuality. Despite the severe health and psychological consequences of this practice, it is usually initiated and carried out by women, who see it as a source of honor, and who fear that failing to have their daughters cut will expose them to social exclusion. In Egypt, despite being punishable by the law, FGM remains a common practice among Muslim and Coptic populations. More than 90% of women in our sample have been circumcised. We replicate our estimations on a dummy equal to 1 if the mother intends to circumcise her daughters and 0 otherwise. Results in Table A.2 show a significant decrease in the choice to circumcise girls in treated governorates (column 3), the effect being slightly larger when nobody else listens to the interview (column 4). It is noticeable that for FGM, the relative magnitude is particularly large compared to decision-making variables or domestic violence: relatively to the control group before the Arab Spring, women’s acceptance of FGM on their daughters decreases by 30% in treated governorates. This result may reflect the fact that the shift in social norms following active female participation to the events has particularly entered the private sphere with respect to women’s general perception about their integrity, autonomy and well-being as well as aspirations for their daughters. Note that when using women’s participation for treatment, we also find significant and large effects on the attitude towards daughters’ FGM (Table A.3, lower panel).

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<sup>29</sup>Additional regressions of the empowerment index on usual controls and detailed presence of others show that the presence of husbands is indeed associated with less power while the presence of other men and women is neutral.

## 5 Conclusion

This paper studies the heterogeneous change in women’s empowerment during the Egyptian Arab Spring. We rely on geographical variation in the intensity of protests during which female demonstrators and women’s cause have gained a huge amount of visibility. Results point to an average increase of 12-18% in terms of household decision power in highly exposed governorates. This effect is robust to alternative modelling strategies and broadly consistent across different empowerment measures based on the final say in health, expenditure and socialization choices or on attitudes regarding domestic violence and girls excision. It was shown not to be mediated by changes in female labor supply, by migration or most recent reforms.

In our double difference estimations, the treatment group was defined as households living in regions experiencing the highest proportions of fatalities, injuries and arrests. These measures based on comprehensive media reports were assumed to proxy the local proportion of demonstrators, including women, as well as the conflict intensity that may have raised awareness about the women’s cause. We have also obtained very similar results when using an alternative definition of treated regions based on women’s rate of protest participation. Future work could nonetheless attempt to elicit more specific information about women’s activism during these events and the way it may have affected the diffusion of social norms, ideas and attitudes regarding gender rights both in the public and private spheres.

Also, while there is a growing research on women’s political representation and on the impact of the media on social changes and family decisions ([Jensen and Oster, 2009](#); [La Ferrara et al., 2012](#)), few studies address the intersection of these literatures. Further research should address how women’s spontaneous activism during the Arab Spring has gained visibility through classic and social media to pervade in turn the private sphere. Our empirical approach has focused on the role of street protests and women’s participation. At the same time, the role of internet activism in the Arab Spring has been highly publicized. Some authors like [Acemoglu et al. \(2014\)](#) argue that while social media helped coordinate street mobilization, the discontent expressed in popular protests was much more influential because demonstrations were a lot more visible and spectacular, providing important information to participants and others in society. Nonetheless, further work should compare these different factors and study the possibly reinforcing effects of demonstrations and e-activism.

Finally, attempts to check whether the empowerment effect elicited in this study is temporary are also expected. The appraisal of the ‘Arab Awakening’ in terms of women’s political

representation is still uncertain - and so is the enforcement of the 2014-15 policy reforms. Moreover, we cannot conclude about how persistent the empowerment effect found in this study could be. The pessimistic view would touch upon the risk that women who witnessed or took part in political movements may be re-subordinated with a return to normalcy, i.e. a patriarchal backlash ([Alison, 2009](#)). A positive tone would see our results as indicating that women's perception about their role in conservative societies is not immutable and that this idea may pass on to the next generation.

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# Appendices

Table A1: Descriptive Statistics

	2008		2014		Raw Diff-in-Diff
	Control	Treated	Control	Treated	
<i>Dependent Variable (Empowerment)</i>					
Composite index: ternary <sup>a</sup> (BASELINE)	29.97	29.11	27.68	31.62	4.794***
Composite index: ternary + work <sup>b</sup>	31.80	31.19	29.77	33.067	3.919***
Composite index: binary <sup>a</sup>	12.32	10.60	10.34	12.40	3.79***
Composite index: binary + work <sup>b</sup>	15.35	13.87	13.48	15.45	3.457***
Decide on her health <sup>c</sup>	1.17	1.12	0.95	1.09	0.191***
Decide on her social life <sup>c</sup>	0.90	0.94	0.84	0.97	0.088***
Decide on main purchases <sup>c</sup>	0.63	0.60	0.72	0.81	0.116***
Tolerate domestic violence (0/1)	0.45	0.35	0.42	0.25	-0.058***
Agree with daughters' excision (0/1)	0.23	0.24	0.28	0.21	-0.079***
<i>Explanatory Variables</i>					
Wealth <sup>d</sup>	2.94	3.37	2.86	3.65	0.357***
Urban (0/1)	0.29	0.60	0.29	0.60	-0.005
# kids	0.85	0.82	0.92	0.88	-0.011
Educ.: None	0.324	0.298	0.234	0.205	-0.002
Educ.: Primary	0.121	0.127	0.1	0.094	-0.012
Educ.: Secondary	0.453	0.43	0.538	0.533	0.018
Educ.: Higher	0.102	0.145	0.129	0.168	-0.004
Wife's age	33.7	34.1	33.1	33.4	-0.08
Husband's age	40.9	41.2	39.8	39.9	-0.164
Marital duration (years)	14.5	14.6	13.4	13.4	-0.087

Statistics based on 2008 and 2014 DHS. Treatment: above-median protest intensity based on the Egyptian Revolution database (governorate-level proportion of fatalities, injuries and arrests). T-test of the raw double differences:

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

<sup>a</sup> MCA procedure over health, socialization and purchase decision questions

<sup>b</sup> MCA procedure over work & health, socialization and purchase decision questions

<sup>c</sup> Ternary answers : 0 (husband decides), 1 (joint decision), 2 (wife decides)

<sup>d</sup> Average over quintiles of wealth.

Table A2: Effect of the Arab Spring Events: Alternative Empowerment Measures (using Protest Intensity for Treatment Definition)

Empowerment indices						
	Composite index (ternary + work)	Composite index (binary)	Composite index (binary + work)	Decide on her health	Decide on her social life	Decide on main purchases
Post x Treat	3.702*** (1.051)	3.656** (1.610)	3.356** (1.525)	0.207*** (0.0352)	0.0483* (0.0268)	0.110*** (0.0388)
Relative effect <sup>a</sup>	0.116	0.297	0.219	0.176	0.054	0.174
Observations	27,330	27,330	27,330	27,842	27,783	27,473
R-squared	0.058	0.061	0.061	0.073	0.057	0.079

Perception about women's integrity and well-being				Labor market participation		
	Tolerate domestic violence	Tolerate domestic violence <sup>b</sup>	Agree with daughters' excision	Agree with daughters' excision <sup>b</sup>	Overall work	Paid work
Post x Treat	-0.0295 -0.0271	-0.0506* (0.0285)	-0.0685*** (0.0132)	-0.0721*** (0.0146)	-0.0179 (0.0144)	-0.0206 (0.0127)
Relative effect <sup>a</sup>	-0.065	-0.112	-0.299	-0.315	-0.116	-0.147
Observations	28,031	21,036	28,031	21,036	28,029	28,031
R-squared	0.238	0.243	0.170	0.175	0.154	0.172

Linear estimations based on 2008 and 2014 DHS. Treatment: above-median protest intensity based on the Egyptian Revolution database (governorate-level proportion of fatalities, injuries and arrests). Standard controls (model 4 of Table 1). Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

<sup>a</sup> Effect relative to the control group pre-period average empowerment, in %.

<sup>b</sup> Interview without others being present including the husband.

Table A3: Effect of the Arab Spring Events: Alternative Empowerment Measures (Using Women's Participation for Treatment Definition)

Empowerment indices						
	Composite index (ternary + work)	Composite index (binary)	Composite index (binary + work)	Decide on her health	Decide on her social life	Decide on main purchases
Post x Treat	3.618*** (0.941)	5.472*** (1.379)	5.243*** (1.300)	0.122*** (0.0332)	0.0453* (0.0253)	0.0695* (0.0389)
Relative effect <sup>a</sup>	0.114	0.444	0.341	0.104	0.050	0.110
Observations	27,330	27,330	27,330	27,842	27,783	27,473
R-squared	0.058	0.061	0.061	0.073	0.057	0.079

Perception about women's integrity and well-being				Labor market participation		
	Tolerate domestic violence	Tolerate domestic violence <sup>b</sup>	Agree with daughters' excision	Agree with daughters' excision <sup>b</sup>	Overall work	Paid work
Post x Treat	0.0329 (0.0262)	0.0339 (0.0283)	-0.0285** (0.0144)	-0.0289** (0.0146)	0.0375*** (0.0126)	0.0293** (0.0122)
Relative effect <sup>a</sup>	0.073	0.075	-0.125	-0.126	0.243	0.209
Observations	28,031	21,036	28,031	21,036	28,029	28,031
R-squared	0.238	0.243	0.170	0.175	0.154	0.172

Linear estimations based on 2008 and 2014 DHS. Treatment: above-median women's participation based on the 2014 SYPE survey (proportion of women engaged in Arab Spring demonstrations). Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

<sup>a</sup> Effect relative to the control group pre-period average empowerment, in %.

<sup>b</sup> Interview without others being present including the husband.