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Women's Political Participation and Intrahousehold Empowerment: Evidence from the Egyptian Arab Spring[+]

Olivier Bargain [a c] , Delphine Boutin [a c] and Hugues Champeaux [b]

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Abstract

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 acceptation 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.

Keywords: Arab Spring, Revolutions, Political Activism, Gender, Empowerment, Egypt.

JEL: J12, J16, D74, I14

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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 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 (Zaki, 2015).

The present paper questions whether the exposure to the Arab Spring events has also led to a change in women's situation within Egyptian homes. Women's participation to the protest may have altered perceptions about their traditional roles, not only in the public sphere but also within households. We conjecture that these changes, 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.[1] 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.

[1]Similar direct measures of intrahousehold bargaining power, often drawn from DHS surveys, have been used to elicit the key determinants of women's empowerment or its impact on child health (Lépine and Strobl, 2013), maternal health care (Bloom et al., 2001) or child labor (Reggio, 2011). Several studies have used them in the case of Egypt, for instance Sadania (2016).

Our baseline estimation captures the relative time variation in 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 has increased by a magnitude of $12\% - 19\%$ (depending on modelling choices). We also point to a decline in women's acceptance of domestic violence (by around $6\% - 12\%$) and a reduction in the intention to excise their daughters (by around $8\% - 12\%$). 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 marriage duration 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. Nonetheless, we also consider alternative interpretations of our findings that pertain to potential changes in labor market participation, to policy reforms and to demographic changes (migration, sex ratios). We suggest that they are, at best, minor mediating effects. 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,[2] while few studies look at the impact of women's electoral victory on women's subsequent political participation (Bhalotra et al., 2017). In the present paper, we exploit the increased visibility of Egyptian women following an unprecedent 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

[2]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).

ownership (Allendorf, 2007; Mishra and Sam, 2016). Particularly in the case of Egypt, recent 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.

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).[3] 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. For our main analysis, we exclude households from border governorates (Red Sea, Matrouh, New Valley, North Sinai and South Sinai). Our final sample is composed of $27,783$ women over 2008 and 2014. In order to check the parallel 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 ($21,897$ observations after applying the same

[3]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.

selection criteria).[4]

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.[5] 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 a composite index including women's work as a fourth dimension, as well as each dimension separately (ternary or binary versions of the indices). Statistics for all these power measures are reported in Table A1 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.[6] 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 review the four phases of the Egyptian Arab Spring. The first one corresponds to the 18

[4]EDHS is also available in 2005 but we use 2000-2008 to obtain longer past trends. Note also that 2005 was a singular year because of the presidential election, social unrest and Mubarak's repression against the Kefaya movement (Sika, 2012) (which included specific violence against women in public places in order to intimidate women from participating in the protests). The year 2000 was stable from a political and societal point of view, hence the choice of this non-event year for parallel trend verifications.

[5]In the light of the Egyptian patriarchy context, these dimensions seem relevant and have been used in previous studies (see for instance Sadania 2016). This is less the case of other aspects such as decisions upon daily purchase and cooking, which do not really capture women's autonomy as they could simply reflect delegation of responsibility on these specific tasks. Women's control over their own earnings, available in some years, is of limited information given the low labor market participation rate. Instead, we simply include women's work directly as a fourth dimension in our composite index in robustness checks.

[6]Unfortunately, the 2008 DHS wave does not contain any information regarding the actual experience of domestic violence.

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 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.[7] 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).[8] We will check the sensitivity of our results to the timing, focusing alternatively on incidents occurring only during the first phases of the revolution. The total number of casualties (injuries, arrests) between 2011 and 2014 reaches 5,221 (44,453, 45,885) cases.

[7]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).

[8]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). In a recent paper, Giesing and Musić (2019) also exploit the same database to measure the impact of protests intensity on education, health expenditures and savings.

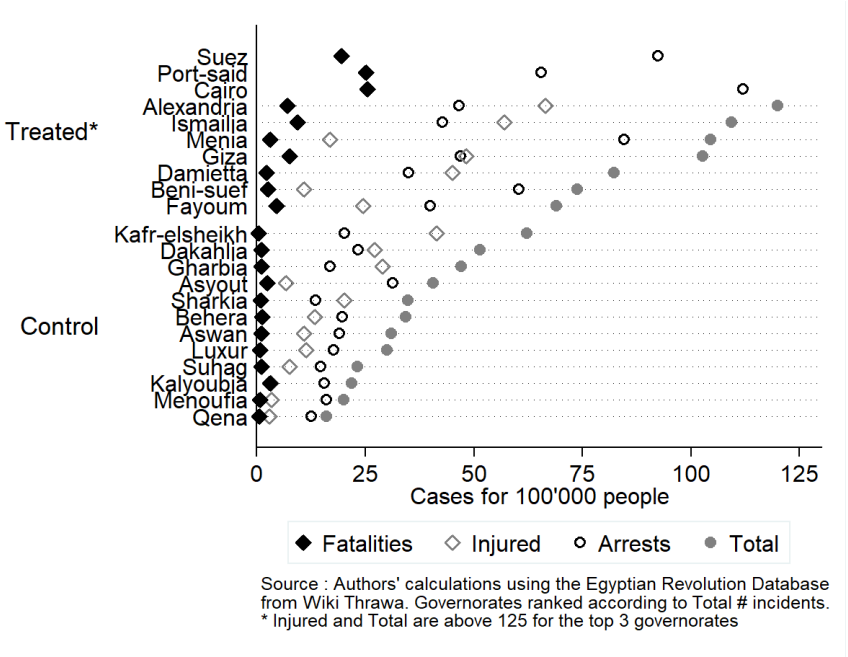
Treatment based on Incidents. We sum these incidents at governorate levels (baseline) and divide the aggregate by the population size of the governorate using Census information to obtain a measure of incidents per inhabitant. This incidents proportion can be seen as a proxy for the local conflict intensity and for the proportion of people, notably women, involved in the local demonstrations – recall that the participation of women was historically high at every stage of the process.[9] 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 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 demonstrate in their district. This is especially the case for the citizens of large cities (like those of Cairo, who converged to Tahrir 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.[10]

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 who participated in at least one of the relevant political activities (provision of support to protesters and participation in any kind of protest or strikes) 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

[9] Simple calculations based on the SYPE data (described hereafter) show that 1.5% of interviewed women have participated to the protest versus 13.2% of the men, i.e. one woman for 9 men on average. This may seem modest but this is far more than in previous eras during which women were never (or very marginally) seen in demonstrations.

[10] 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).

Figure 1: Protest Intensity: Incidents Rates by Governorate



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.[11] 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

[11] There could be reverse causality between self-reported protest participation and final say if we directly used this micro data. This is less of a problem here since we aggregate this information to construct and impute governorate-level treatment intensity in the DHS.

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.

Arguably, there is a correlation between protest intensity and other dimensions including urbanization and education. Yet it does not necessarily contradict the lack of correlation with pre-event empowerment. First, being urban is not a marker of women's empowerment in the Egyptian context. Urbanization has led to the existence of huge urban or suburban areas populated by low-educated households, which are not necessarily different from rural households in terms of gender norms.[12] Moreover, only 60% of the treated are urban. The rest lives in rural zones close to the urban areas where the action was taking place - they have possibly taken part to local protests in nearby cities or been influenced by them.[13] Second, the educational gap between treated and control groups appears to be relevant only at the extreme education levels ('no education' and 'higher education' levels, cf. Table A1). This means that higher schooling in treated regions coincides with (i) a higher rate of people escaping from illiteracy, which is not per se a guarantee of significant moves toward gender balance, and (ii) more people holding a higher degree, which is anyway a too small group to embody the effect that we characterize in our estimations.[14] Finally, and more generally, 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).

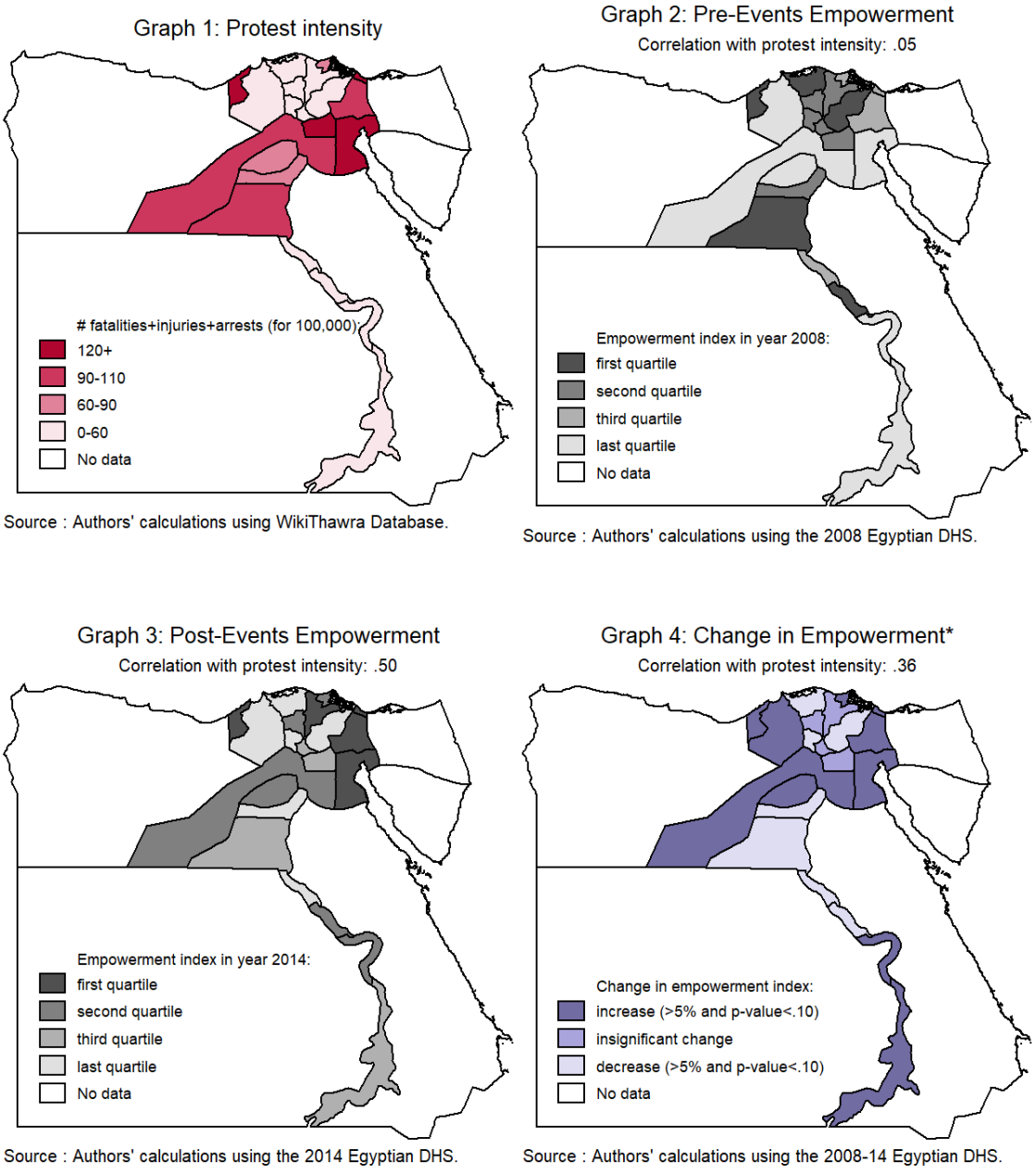
The third graph of Figure 2 depicts the situation in 2014, i.e. after the events. The correlation between protest intensity and empowerment shifts goes up to .50. The last graph shows the change in empowerment over the period. The improvement in women's bargaining position following the Arab Spring was not generalized: the change in empowerment is

[12]The coefficient of a urban dummy is not significant in our empowerment estimations, whatever the specification.

[13]Heterogeneous effects will actually show a significant impact of the protests for both urban and rural households.

[14]The coefficient of higher education in empowerment estimations multiplied by the gap in higher education rates (Table A1) gives a contribution of only 0.5% of the average empowerment index in the pre-Arab Spring period.

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



insignificant overall (p-value of .69) and shows a significant increase in only 10 of the governorates (Alexandria, Aswan, Behera, Damietta, Fayoum, Giza, Ismailia, Post Said, Qena and Suez). We observe a .36 correlation between protest intensity (graph 1) and empower-

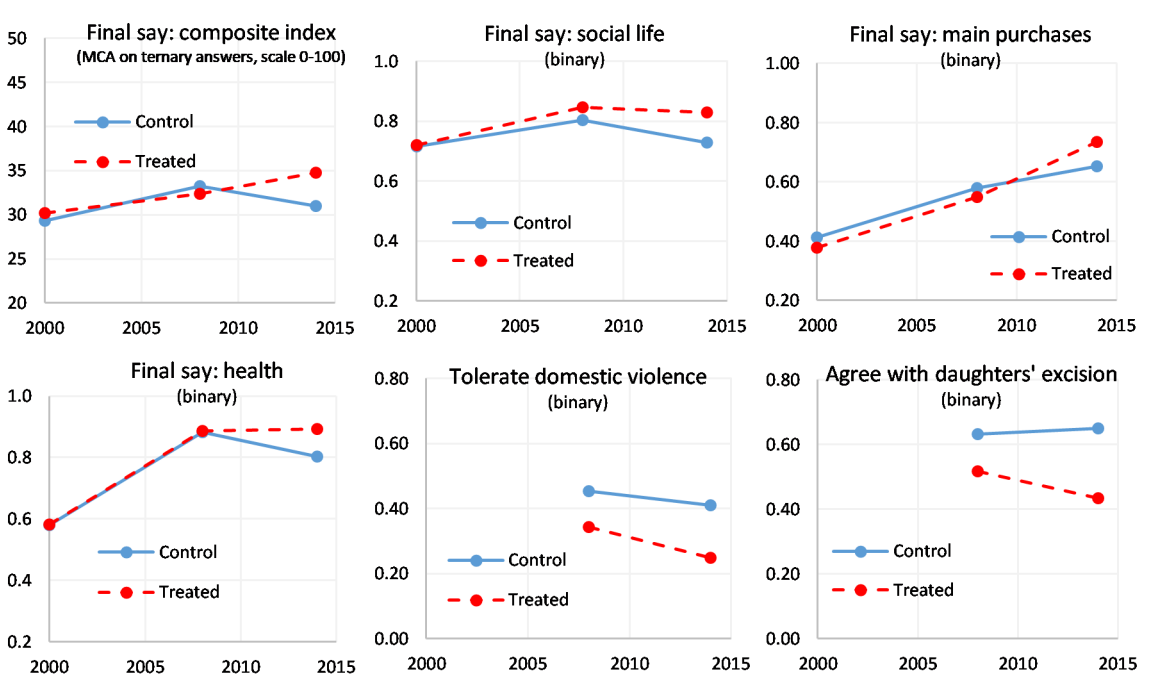
ment variation (graph 4). There is no one-to-one correspondence in ranks between treated regions and regions experiencing improvements in women's empowerment, but out of the 10 governorates with a high-protest intensity, 8 were subject to an increase in empowerment (7 of them with a rise larger than $> 5\%$ 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 provide a few additional descriptive elements. As we saw, empowerment has remained stable on average. Using our main composite index, we find a score of 32.86 in 2008 and 32.71 in 2014 (a change of -0.4%, not statistically different from zero). Using protest intensity, we see that empowerment has dramatically increased in the treated group (from 32.39 to 34.75, i.e. +7.3%) while it has decreased in the control group (from 33.22 to 31.00, i.e. -6.7%). These trends by treatment group are reported in the first graph of Figure 3. The empowerment decline in the untreated population could reflect the regressive influence of conservative forces which reemerged over the period and implied a backlash against women (notably during the period of Islamist leadership, cf. Manea (2014)). The raw difference-in-difference calculation yields an increase of 4.59 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 most exposed governorates have seen women's empowerment increase by around 14% 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.

The rest of Figure 3 shows empowerment trends for other measures including the specific 'final say' questions (health, social life, main household purchases) and women's views about domestic violence and girls' excision. All are binary outcomes for the ease of interpretation (final say outcomes are coded as 1 if the woman decides alone or with her husband, 0 if the husband decides alone). A first observation is that the overall decline in empowerment for the control group is observed in some measures, typically the final say indices with a high initial level (health, social life), but not all. In particular, decision power regarding household purchases started at a lower level in 2008 and has increased in both treated and untreated regions, yet faster among the former (+34% versus +13%). Tolerance for domestic violence has actually declined in both groups but the decline was more pronounced among the treated (-27% versus -10%). Hence, the pattern of what has happened overall during the Arab Spring

era – as captured by the control group – is relatively composite and it is difficult to conclude about an overall regression in gender rights for those not exposed to the protests. Most importantly, Figure 3 shows that all the indices go in the same direction in terms of *relative* trends between treated and control groups. Compared to the gap between groups in 2008 – which was quasi null for final say indices and at the advantage of the treated for violence and excision outcomes – the situation in 2014 corresponds to a relative gain of around 10 percentage points for the treated. Given different initial outcome levels, the relative gain has been larger for the final say over purchases (+19%) than for health or socialization (+10% and +7%). Double difference estimations shall refine these raw measures.

Figure 3: Trends in Empowerment Measures



These graphs are based on empowerment measures drawn from the Egyptian DHS for the years 2000, 2008 and 2014. Data on domestic violence and excision are not available for 2000. The binary `final say' questions are coded as 1 if the woman decides or both spouses decide, 0 if the man decides alone.

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:

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

The coefficient β on the interaction term is the difference-in-difference estimator, representing the effect of living in a highly exposed governorate after the events. The coefficient γ 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 δ 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, husband employment status) as well as household information (wealth, urban dummy, number of children and religion, i.e. a dummy equal to 1 for Christian, 0 for Muslim) . 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 A1 in the Appendix indeed shows that the most exposed governorates are significantly richer, more urban and more educated than other governorates. However, we have explained above that initial conditions do little to explain pre-existing differences in women's conditions. Moreover, we control for structural differences with the set of variables X_i and, 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 additionally 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 completely rule out the possibility of counfounding geographical heterogeneity 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 drivers of female empowerment during the events. We assemble four 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 levels were particularly high (i.e. places with a higher capacity of change) or low (places with a higher margin of improvement). Previous discussions convey that observed households in treated regions are not necessarily the young, urban and educated couples that might have triggered the protests. Second, a successful test of the parallel trends will indicate that the treated regions were not necessarily having different empowerment trajectories compared to more other regions in the years prior to the events. Third, we will carry out a triple-difference analysis using the duration in marriage to define an extra control group. Finally, we provide an extensive sensitivity analysis showing that the effect is stable when using alternative spatial variation or when focusing on the early events, i.e. those for which the protests were not specifically directed towards women's rights.

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 composite index of empowerment based on ternary final say questions.[15] The upper panel shows baseline estimates of the treatment effect β, i.e. the coefficient of $Post \times Treat$. We obtain a very significant effect of around 4-5 points across the different models presented in columns 1-7. This is a substantial effect. In relative terms (indicated as 'relative effect'), it represents an increase of 12%-15% in empowerment for women in the exposed governorates relatively to other regions prior to the Arab Spring. Another way to interpret this magnitude is to compare it to the estimate of other relevant correlates of empowerment. In particular, exposure to the protest has an effect of the same

[15]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/older birth cohorts also have positive effects as well as the husband employment status.

order as holding a higher education degree (relative to having no education) or 1.5 times the effect of having secondary education (relative to no education).

An important observation of Table 1 is that 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 municipality 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 the size of the effect.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Period of interest: 2008-2014							
$Post \times Treat$	4.578***	4.550***	4.947***	4.181***	4.208***	3.999***	4.181**
	(0.997)	(0.996)	(1.034)	(1.058)	(0.995)	(0.988)	(1.989)
Relative effect[a]	**0.138**	**0.137**	**0.149**	**0.126**	**0.127**	**0.120**	**0.126**
Observations	27,783	27,782	27,782	27,782	27,782	27,782	27,782
R-squared	0.016	0.017	0.050	0.051	0.052	0.053	0.051
Placebo: 2000-2008							
$Post \times Treat$	-1.015	-1.014	-1.237	-0.333	-0.788	-0.437	-0.333
	(0.985)	(0.985)	(1.019)	(0.997)	(0.998)	(1.006)	(1.930)
Relative effect[a]	0.035	0.035	0.042	0.011	0.027	0.015	0.011
Observations	21,897	21,897	21,897	21,897	21,897	21,897	21,897
R-squared	0.022	0.022	0.080	0.083	0.082	0.085	0.083
Individual Controls X_i	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth cohorts	No	Yes	Yes	Yes	Yes	Yes	Yes
Municipalities	No	No	Yes	Yes	Yes	Yes	Yes
$POST_i \times X_i$	No	No	No	Yes	No	Yes	Yes
PS reweighting	No	No	No	No	Yes	Yes	No
Cluster	Municip.	Municip.	Municip.	Municip.	Municip.	Municip.	Govern.

Linear estimations based on 2000, 2008 and 2014 DHS. Estimation of woman's empowerment (ternary composite index) on the treatment, i.e. above-median protest intensity (based on the Egyptian Revolution database and defined as the governorate-level proportion of fatalities, injuries and arrests). We use different specifications: 1- baseline covariates including wealth, urban, education of the wife, age of wife and husband, husband in work, religion; 2-adding birth cohorts; 3-adding municipality dummies; 4-adding interactions between POST and the controls; 5 & 6- using propensity score reweighting. Standard errors in parentheses are clustered at municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels respectively.

[a] Effect relative to the pre-event control group mean empowerment (33.2 in 2008, 29.3 in 2000), in %.

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 weights $\frac{1}{1-p_i}$ and $\frac{1}{p_i}$ for non-treated and treated observations respectively.[16] 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.

Regional Effects and Standard Error Clustering. In models 1-6, we cluster standard errors at the municipality level. In model 7, we alternatively cluster at governorate level using a 'wild bootstrap' approach. The standard error increases as expected but the effect is still significant at the 5% level. More sensitivity checks are provided in Table B1 in the appendix. We compare our baseline (column 1) to a similar regression including governorate fixed effect (column 2): the effect decreases a little (-10%) but is still significant at the 1% level. Then, we cluster at governorate level directly (column 3) or, because the small number of governorates (22) is likely to create a high variability in the estimates, using bootstrapped standard errors. Results show little sensitivity to the number of replications, namely 800 or 1000 replications (columns 4 and 5). The main check based on 'wild bootstrap' is suggested by Cameron et al. (2008) and relies on the implementation procedure by Roodman et al. 2018.

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 2008 DHS waves, as explained in the data section.[17] Thus, $POST_i$ takes a value 1 for women observed in 2008 and 0 for those of 2000. The lower panel in Table 1 report the placebo

[16] 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.

[17] Note that religion is not included in this estimation since it was not available in the 2000 DHS.

estimates of β for all the specifications. None of them are statistically different from zero: living in the areas that would later be highly exposed to the Arab Spring protests did not imply a specific trend in terms of woman's intra-household bargaining power.

4.2 Sensitivity Analyses

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. 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 only 10% smaller compared to our baseline selection. Results in Table 2 indicate that the magnitude of the effect (column 1) is similar to the main estimation, 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 2). Combining both adjustments again leads to similar results (column 3). Our conclusion are also unaffected by outliers related to age, as shown by the estimate obtained when trimming the age selection (i.e. selecting women aged 20-40, column 4).

Pseudo-Panel. We also suggest a pseudo-panel approach following Devereux (2004). To some extent, this approach averages up individual unobservables that may act as confounders. We define cells based on 22 governorates, 3 birth cohorts and 2 locations (ur-

ban/rural).[18] We replicate our estimations using the mean values of all the variables in the model, weighting estimations by cell size and including a cell fixed effect. The pseudo-panel difference-in-difference estimate is close to the baseline, slightly larger but not significantly so (column 5).

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 interviewed in the presence of their husbands. The estimate hardly changes compared to the baseline (column 6).[19] We also exclude women interviewed in the presence of any relative: the empowerment effect increases a little but not significantly (column 7).[20]

Border governorates. We have excluded border governorates from our baseline regressions for several reasons. First, we aimed to focus on a relatively homogenous population (border regions are inhabited by Bedouins tribes with nomadic traditions, whose reactions to the Arab Spring events may be very different from that of the population around the Nile Valley). Second, these regions are not densely populated (2% of the Egyptian population lived in these areas according to the last Census). Third, given the geographical size of these regions, and its small nomadic population, we have some doubts on the representativeness of the DHS surveys for these governorates. Finally, these governorates are close to others types of conflicts (for instance the Sinai Peninsula and the nearby Gaza Strip conflict). Nonetheless, we perform some sensitivity analyses with the inclusion of frontier governorates. The relative contribution of these governorates seems marginal as the average effect is barely

[18] We impose a minimum number of observations (at least 50 per cell) which leads us to discard some cells, i.e. we retain 114 out of the 136 initial cells.

[19] In additional estimations, we find that the presence of the husband during interviews is negatively correlated with the degree of autonomy of the wife, especially regarding the final say question on social life. Further work should attempt to disentangle the underlying mechanisms, namely self-censorship and household heterogeneity. Note also that double difference estimations using the presence of husbands as the outcome show no significant effect of the treatment.

[20] 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.

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

	Same cohort[b]	No divorce[c]	Same cohort & No divorce[b,c]	20-40 years[d]	Pseudo-panel[e]	Interview: no Husband[f]	Interview: alone[g]	Including border governorates[h]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Post \times Treat$	4.513***	3.895***	4.234***	4.660***	4.965***	4.179***	4.468***	4.652***
	(1.055)	(1.080)	(1.082)	(1.092)	(1.209)	(1.085)	(1.158)	(1.039)
Relative effect[a]	**0.136**	**0.117**	**0.127**	**0.146**	**0.149**	**0.126**	**0.133**	**0.145**
Observations	25,095	24,261	22,157	20,311	228	26,018	20,909	29,509
R-squared	0.050	0.049	0.049	0.056	0.208	0.052	0.061	0.058

Linear estimations based on 2008 and 2014 DHS. Estimation of woman's empowerment (ternary composite index) on the treatment, i.e. above-median protest intensity (based on the Egyptian Revolution database and defined as the governorate-level proportion of fatalities, injuries and arrests). We use the specification of model 4 in Table 1 (controls include wealth, urban, education of the wife, age of wife and husband, husband in work, religion, birth cohort, municipality dummies, interactions between POST and the controls). Standard errors in parentheses are clustered at the municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels respectively.

[a] Effect relative to the control group pre-period average empowerment, in %.

[b] Same cohort: women aged 15-43 in 2008 and aged 21-49 in 2014.

[c] No Divorce: women married for at least 4 years to avoid divorces or new marriages during 2011-14 .

[d] Sub-sample: woman aged between 20 and 40 years old.

[e] Pseudo-panel: estimations on 114 cells x 2 year (3 cohort x 22 governorate x 2 urban, excluding quasi-empty cells).

[f] Interview without men being present, including the husband.

[g] Interview without anyone else being present.

[h] Including border governorates (Red Sea, Matrouh, New Valley, North Sinai and South Sinai).

affected (column 8).[21]

Time Variation in Treatment. We 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 the incidents that occurred under Mansour's regime (i.e. starting July 2013), as a majority corresponds to the repression against islamists.[22] Results are also consistent if we exclude the protests of June 2013 against Islamist President Morsi, as this period is the beginning of explicit claims in favor of women's right. That is, estimates on the earlier period focus on political movements that, even if correlated with geographical confounders, are not simultaneously related to deep

[21] This is consistent with results found in El-Mallakh et al. (2018) and Giesing and Musić (2019), who found little variation due to the exclusion of these governorates.

[22] As a matter of fact, these events are equally interesting as they show 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.

changes in terms of gender equality. As recalled in the introduction, the main grievances at the onset of the Arab Spring were about living standards, civil/political freedom and social justice, not women's rights specifically.

Spatial Variation in Treatment. We consider spatial variation in protest intensity. First, we compare our baseline results to an estimation where we classify regions within governorates as urban or rural (while Cairo and Alexandria are completely urban, for instance, other governorates contain both urban and rural regions). For obvious reasons of visibility and coordination, people have gathered in cities or large towns to demonstrate, yet, as previously discussed, only 60%of the treated in our data are urban. In Table 3, heterogeneous effects are compared to the baseline (column 1). These effects are very similar for both urban households (column 2) and rural households (column 3), which indicates that the empowering influence of protests is not a pure urban phenomenon. Next, we suggest a more disaggregated perimeter. 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. The above definitions of urban (or rural) parts of governorates was a first step in this direction. We suggest a more disaggregated spatial variation in treatment by considering a person's municipalitiy (so-called Markaz/Kism) and her relevant border municipalities.[23] The treatment group is defined as households living in extended municipalities that experience above-median protest intensity. Table 3 points to a significant effect, yet smaller in magnitude (column 4). The relative effect, i.e. the response relative to control group municipalities prior to the events, is around 8%. Consistently with the analysis at governorate level, it is slightly smaller if we focus on urban households, i.e. 7% (colum 5).

4.3 Women's Political Engagement and Empowerment in the Home

Before considering alternative channels, we develop here the discussion about the most likely mechanism at work: the intensity of protests is associated 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.

[23]Precisely, to exclude low-populated subdivisions corresponding to vast desert areas, we do not include border Markaz/Kism 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

	Governorate level[b], baseline	Governorate level[b], urban	Governorate level[b], rural	Extended municipality level[c]	Extended municipality level[c], urban
	(1)	(2)	(3)	(4)	(5)
$Post \times Treat$	4.181***	4.064***	4.363***	2.656**	2.276**
	(1.058)	(1.074)	(1.544)	(1.202)	(1.151)
Relative effect [a]	**0.126**	**0.123**	**0.131**	**0.080**	**0.069**
Observations	27,782	11,920	15,888	27,782	27,782
R-squared	0.051	0.060	0.044	0.050	0.049

Linear estimations based on 2008 and 2014 DHS. Estimation of woman's empowerment (ternary composite index) on the treatment, i.e. above-median protest intensity. This intensity is based on the Egyptian Revolution database and defined as the governorate-level (columns 1-3) or municipality-level (column 4) proportion of incidents (fatalities, injuries and arrests). We use the specification of model 4 in Table 1 (controls include wealth, urban, education of the wife, age of wife and husband, husband in work, religion, birth cohort, municipality dummies, interactions between POST and the controls). Standard errors in parentheses are clustered at the municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels respectively.

[a] Effect relative to the pre-event control group average empowerment, in %.

[b] 22 governorates of Egypt, i.e. excluding border governorates.

[c] Local municipality, i.e. markaz (for rural areas) or kism/aqsam (for urban areas), and relevant border municipalities.

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.[24] 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 for the outcomes under study. Acemoglu et al. (2014) point to the fact that other platforms

[24] Exposure to conflicts may also increase cooperative behavior (Beekman et al., 2017), egalitarianism (Bauer et al., 2014) or altruism (Voors et al., 2012).

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.

Heterogeneity and Triple Differences. We have checked that exposed regions did not exhibit specific empowerment levels or trends prior to the events. Still, we cannot completely exclude the role of some unobserved geographical heterogeneity in the upwards empowerment trends of the treated regions (Besley and Case, 2000). We suggest an additional check based on triple difference estimations. While there is no perfect control group for that purpose, we suggest using couples' duration as a gradual filter in terms of exposure to the protests.[25] Older couples comprise women of 49 years old at most, hence they are not necessarily of an older generation that would be insensitive to cultural changes. We actually check that empowerment is not fixed in this group.[26] This means that these couples could well be affected by unobserved geographical factors that are not controlled for and that could change empowerment during the Arab Spring period. At the same time, we argue that these couples may be less directly involved â or exposed to â local street protests because of familiy duties, a lower internet usage and/or a weaker identification to young female leaders. Thus, the double difference coefficient should capture the role of the region-specific confounding factors while the triple difference should grasp the specific effect of protest exposure. We denote Z a dummy for younger couples. Results are reported in Table 4 using alternative thresholds at 20 and 25 years of marriage for sensitivity checks (columns 2 and 4 respectively). $POST \times TREAT$ captures the treatment effect for those less exposed to the protests (older couples Z=0). It is insignificant in both cases. $POST \times TREAT \times Z$ captures the additional treatment effect on those most exposed (younger couples Z=1): we find a significant effect of a similar magnitude as our baseline double-difference estimate.

We provide two additional checks. First, we ask whether we do not reject the validity test for a triple difference, i.e. an insignificant coefficient on $POST \times TREAT$, because of

[25] Other, possibly more exogenous variation might be used, such as prominent cases of violence against women or of internet interventions of female activists/bloggers. Yet, using precise time variation requires more specific data, and knowledge of the underlying, possibly complex dynamics. Moreover, cyberactivism has been very specific in Egypt, often described as a coordination device for street mobilizations more than a driver of change per se (e.g. Acemoglu et al., 2017).

[26] We find the same upward trends in empowerment as in shorter marriages prior to the events. Note also that no difference in empowerment trends across marriage durations prior to the Arab Spring is a placebo check for the triple difference approach.

imprecise estimates. In column 1, we report a triple-difference estimation using a pseudo-control group defined as couples married for more than 15 years. This group is partly treated so that the validity check is rejected, but the important observation is that the $POST \times TREAT$ coefficient is not much more precisely estimated than when using the 20-year threshold. Hence, not rejecting the validity test in the latter case is mainly due to a smaller $POST \times TREAT$ coefficient rather than imprecise estimations. Second, we suggest a triple difference estimation where the additional control and treated groups are more balanced (columns 3 and 5). We simply select the younger Z=1 in a proportion very similar to that of the older couples Z=0 (the balance is not perfect due to age indivisibility). The estimated effect is stronger (as expected given the more contrasted groups) and the precision slightly lower (given the smaller sample), but the precision relative to total sample size is higher due to more balanced groups, and these estimations confirm previous results.

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. 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 (+10.4%) compared to that based on protest intensity (+12.6%). 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. The effect hardly changes due to the presence of male relatives during the interview (relative effect of +10.2%).

Alternative Treatment Variable: Ranks and Discrete Treatment Intensity. We pursue this comparison for other types of treatment variables. In the baseline, the threshold

Table 4: Triple Differences of exposure to Arab Spring (T) and younger couples (Z) on women empowerment

Stronger exposure to treatment Z=1 defined as being a couple of:	Less than 15 years of marriage	Less than 20 years of marriage		Less than 25 years of marriage	
	(1)	(2)	(3)	(4)	(5)
$Post \times Treat \times Z$	3.429***	3.917***	4.715***	3.728**	4.803**
	(0.946)	(1.109)	(1.318)	(1.510)	(1.891)
$Post \times Treat$	2.956**	2.096	1.846	1.751	1.383
	(1.295)	(1.400)	(1.410)	(1.674)	(1.698)
Relative effect [a]	0.103	0.118	0.144	0.112	0.150
% of the sample with Z=0:	0.41	0.26	0.49	0.13	0.47
# obs. with Z=0:	11,507	7,284	7,284	3,659	3,658
Observations	27,782	27,782	14,939	27,782	7,831
R-squared	0.051	0.051	0.060	0.051	0.073

Linear estimations based on 2008 and 2014 DHS. Estimation of woman's empowerment (ternary composite index) on the treatment, i.e. above-median protest intensity (based on the Egyptian Revolution database and defined as the governorate-level proportion of fatalities, injuries and arrests). We use the specification of model 4 in Table 1 (controls include wealth, urban, education of the wife, age of wife and husband, husband in work, religion, birth cohort, municipality dummies, interactions between POST and the controls). Z is a dummy variable for younger couple, as indicated. The triple difference estimator $Post \times Treat \times Z$ is the specific effect for those more sensitive to treatment in exposed governorates. Post x Treat is the effect for those less sensitive to treatment in exposed governorates. Standard errors in parentheses are clustered at the municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels respectively.

[a] Triple diff. effect relative to the control group pre-period average empowerment, in %.

(1): Pseudo assignment group, because 15 years is to low to characterize only older, less exposed couples among the Z=0.

(3): Similar to (2) but more balanced sample between Z=1 (marriage duration < 6 years) and Z=0 (>20 years).

(5): Similar to (4) but more balanced sample between Z=1 (marriage duration < 3 years) and Z=0 (>25 years).

applied to determine treated governorates was arbitrarily set to the median. In columns 3-4 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 5-6, 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

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

	Binary Treatment		Rank[b]		Discrete Treatment[c]		Combined
	Protest intensity	Women's participation	Protest intensity	Women's participation	Protest intensity	Women's participation	Treatment[d]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × *Treat*	4.181***	3.498***	0.171**	0.204***			
	(1.058)	(0.965)	(0.0735)	(0.0740)			
Post× *High Treat*					5.772***	5.903***	
					(1.139)	(1.064)	
Post× *Interm. Treat*					4.005***	3.283**	
					(1.283)	(1.316)	
Post×*Low Treat*					3.125*	1.347	
					(1.741)	(1.089)	
Post×*Treated (Protest Intensity)* × *Treated (Women participation)*							6.019***
							(0.913)
Post×*Treated (Protest Intensity)* × *Untreated (Women participation)*							1.250
							(1.584)
Relative effect (a)	0.126	0.104	n.a.	n.a.	0.131	0.107	0.111
Observations	27,782	27,782	27,782	27,782	27,782	27,782	27,782
R-squared	0.052	0.052	0.049	0.050	0.051	0.051	0.052

Linear estimations based on 2008 and 2014 DHS. Estimation of woman's empowerment (ternary composite index) on the treatment, i.e. above-median protest intensity (based on the Egyptian Revolution database and defined as the governorate-level proportion of fatalities, injuries and arrests) or above-median women's participation (based on the 2014 SYPE survey and defined as the proportion of women engaged in Arab Spring demonstrations). We use the specification of model 4 in Table 1 (controls include wealth, urban, education of the wife, age of wife and husband, husband in work, religion, birth cohort, municipality dummies, interactions between POST and the controls). Standard errors in parentheses are clustered at the municipality level. *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level.

[a] Effect relative to prior control group average empowerment in %. With discrete treatment: average effect over the 3 groups. With combined treatment: average effect over the 2 groups.

[b] Rank from the scores of protest intensity / women's participation (coefficients not comparable to binary/discrete treatment).

[c] Discrete treatment groups: governorates ranked 1-3 (high), 4-6 (interm.), 7-10 (low), the reference being the control group.

[d] Discrete groups combining both treatment variables: Post × Protest intensity above median × Women participation above the median and Post × Protest intensity above the median × Women participation below median.

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. Note however that the three effects are not significantly different.[27] The pattern

[27] F-tests cannot reject the null hypothesis of equality of the effects (the p-value is 0.21 for all three effect and 0.14 when testing the equality of the top and low intensity effects). If we cluster standard errors

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) and high and medium groups show increasing responses. In both columns 5 and 6, the relative effect calculated as the mean impact over the three treated groups yields a response that is close to what we obtained with binary treatments (13.1% and 10.7% respectively).

Finally, we can combine both sources of variation (protest intensity and women participation). A simple way of doing so consists in introducing heterogeneity within our main treatment variable (protest intensity) between governorates with high versus low women participation. Results in column 7 show that our main effect is driven by regions combining above-median protest intensity and above-median women participation. This is simply explained by the fact that among exposed governorates, women have been the most active in those with the highest protest intensity. The magnitude of the effect is actually similar to the estimate obtained in the three governorates with the highest protest intensity (the 'high treatment' of column 5).

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.[28] Notwithstanding, our results align to El-Mallakh et al. (2018)'s when we

at governorate rather than municipality level, top and medium intensity effects are highly significant but again the equality of the different effect is not rejected.

[28] 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

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.[29]

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.[30] Thus, it seems that female employment is not a strong explanatory factor overall.[31] 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

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.

[29] Results are reported with other outcomes, which are discussed later, in Appendix Tables C.1 and C.3. We distinguish overall work and paid work (last two columns of the lower panels in these tables).

[30] 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.

[31] 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 improves women's say 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).

gender discourse and policy reforms in the El-Sissi era can be viewed as a consequence of the 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 border 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).[32] 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.[33] 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.

[32]The proportion of movers among highly exposed governorates was 1.58%, indeed not much larger than among control governorates (1.49%).

[33]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 C.1 (upper panel) reproduces the baseline results using a MCA-composite index based on ternary answers on health, socialization and purchase decisions (column 1). It is compared to a similar composite index adding a fourth dimension, namely whether the woman works (column 2). The effect is in the same order of magnitude. We also isolate each of the three decision-making variables as dependent variables with ternary outcomes (0 if the husband decides, 1 for joint decision and 2 if the wife decides). The effects are all significant (columns 3-5) but vary in magnitude, with larger impacts on decisions pertaining to the wife's health and family purchases. Alternatively, we can construct binary indices by pooling answers. We may argue that some decisions could reflect autonomy when being made alone (health, social life) while other require coordination (large expenditures). More generally, the meaning of joint decision for empowerment is ambiguous (Acosta et al., 2019), so we try both approaches in Table C2: women's autonomy excludes joint decision (upper panel) or includes it (lower panel). We obtain significant effects in most cases. Relative effects are in the same order of magnitude when autonomy includes joint decision (they are slightly lower due to a lower effect on health) and much larger when autonomy excludes joint decision (especially due to a more than doubled effect on health). We have also replicated results with ternary answers when treated governorates are identified using women's protest participation. Empowerment effects reported in Table C.3 (upper panel) are much in line with the results discussed above.

Domestic Violence. We focus on other questions related to women's perception about their integrity and well-being. The first one addresses women's tolerance of domestic violence (a dummy equal to 1 if she disagrees with any justification for the use of violence). Results from a linear probability model are presented in Table C1 (lower panel) and point to a

negative effect of higher protest intensity on the acceptation of violence (column 1), which becomes significant 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. When using women's protest participation to define treated governorates, we find insignificant effects whatever the conditions of interview (Table C.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.[34] We replicate our estimations on a dummy equal to 1 if the mother intends to circumcise her daughters and 0 otherwise. Results in Table C.1 (lower panel) show a significant decrease in the choice to circumcise girls in treated governorates, whether the woman is alone during the interview or not. The relative effect of the protests on FGM is of a similar magnitude as for decision-making (a reduction of around 10%). These results convey that an exposure to the protests may have entailed an improvement in women's 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 C.3, lower panel).

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 a substantial increase in women's autonomy in highly exposed governorates. This effect is robust to alternative modelling strategies and broadly consistent across different

[34] 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.

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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Appendix A. Descriptive statistics

Table A1: Descriptive Statistics: 2000-2008-2014

	Control			Treated			Raw Diff-in-Diff 2008-2014		
	2000	2008	2014	2000	2008	2014	Abs. DD	Relative DD	Signif.
Alternative Dependent Variables (Empowerment)									
Composite index: ternary[a]	29.32	33.22	31.00	30.19	32.39	34.75	4.59	0.14	***
Composite index: ternary + work[b]	28.35	31.29	29.18	29.53	30.65	32.53	3.99	0.13	***
Decide on her health[c]	0.88	1.17	0.95	0.95	1.12	1.09	0.19	0.16	***
Decide on her social life[c]	0.86	0.90	0.84	0.85	0.94	0.97	0.09	0.10	***
Decide on main purchases[c]	0.47	0.63	0.72	0.43	0.60	0.81	0.12	0.19	***
Decide on her health[d]	0.58	0.88	0.80	0.58	0.89	0.89	0.09	0.10	***
Decide on her social life[d]	0.72	0.80	0.73	0.72	0.85	0.83	0.06	0.07	***
Decide on main purchases[d]	0.41	0.58	0.65	0.38	0.55	0.74	0.11	0.19	***
Tolerate domestic violence (0/1)	-	0.45	0.41	-	0.34	0.25	-0.05	-0.11	***
Agree with daughters' excision (0/1)	-	0.63	0.65	-	0.52	0.43	-0.10	-0.16	***
Explanatory Variables									
Wealth[e]	3.33	2.93	2.86	3.70	3.38	3.66	0.357	0.12	***
Urban (0/1)	0.37	0.29	0.29	0.64	0.60	0.60	0.00	-0.01	
# kids	0.97	0.84	0.91	0.89	0.81	0.88	0.00	0.00	
Educ.: None	0.48	0.33	0.23	0.31	0.30	0.20	0.00	0.00	
Educ.: Primary	0.19	0.12	0.10	0.19	0.13	0.09	-0.01	-0.08	
Educ.: Secondary	0.27	0.45	0.54	0.37	0.43	0.53	0.01	0.03	
Educ.: Higher	0.06	0.10	0.13	0.13	0.14	0.17	0.00	-0.02	
Wife's age	34.4	34.0	33.3	34.7	34.3	33.5	-0.19	-0.01	
Husband's age	41.9	41.2	40.0	42.1	41.4	40.0	-0.26	-0.01	
Marital duration (years)	16.3	14.8	13.6	15.6	14.8	13.4	-0.15	-0.01	

Statistics based on 2000, 2008 and 2014 DHS. Treated: households living in governorates with above-median protest intensity (based on the Egyptian Revolution database and defined as governorate-level proportion of fatalities, injuries and arrests). Last columns: Abs. DD = absolute difference-in-difference, Relative DD = DD relative to the pre-event control group outcome, Signif.: t-test of the raw double differences with ***, ** and * indicating significance at 1%, 5% and 10% levels respectively.

[a] MCA procedure over health, socialization and purchase decision questions (ternary answers).

[b] MCA procedure over health, socialization and purchase decision questions (ternary answers) & female labor market participation.

[c] Ternary answers : 0 (husband decides), 1 (joint decision), 2 (wife decides).

[d] Binary answers : 0 (husband decides), 1 (she decides alone or with husbands).

[e] Average over quintiles of wealth.

Appendix B: Sensitivity to Standard Error Clustering

Table B1: Effect of the Arab Spring on Women Empowerment: Alternative Standard Error Clustering

	(1)	(2)	(3)	(4)	(5)	(6)
$Post \times Treat$	4.181***	3.765***	4.181**	4.181**	4.181**	4.181**
	(1.058)	(1.007)	(1.989)	(2.086)	(2.077)	(1.989)
Observations	27,782	27,782	27,782	27,782	27,782	27,782
R-squared	0.051	0.033	0.051	0.051	0.051	0.051
Cluster	Municip.	Municip.	Govern.	Govern.	Govern.	Govern.
Fixed effect	Municip.	Govern.	Municip.	Municip.	Municip.	Municip.
Bootstrap	No	No	No	800 rep.	1000 rep.	Wild BS

Linear estimations based on 2008 and 2014 DHS. Estimation of woman's empowerment (ternary composite index) on the treatment, i.e. above-median protest intensity (based on the Egyptian Revolution database and defined as the governorate-level proportion of fatalities, injuries and arrests). We use a specification including the following controls: wealth, urban, education of the wife, age of wife and husband, husband in work, religion, birth cohort, interactions between POST and the controls. Fixed effects: we additionally control for municipality dummies ('Municip.') or governorate dummies ('Govern.'). Cluster: standard errors in parentheses are clustered at the municipality level ('Municip.') or governorate level ('Govern'). Clustering based on standard bootstrap (800 or 1000 replications) or wild bootstrap. ***, ** and * indicate significance at the 1%, 5% and 10% levels respectively.

Appendix C: Alternative Empowerment Measures

Table C1: Effect of the Arab Spring Events: Alternative Outcomes (using Protest Intensity for Treatment Definition)

	Final say: Ternary indices				
	Composite index (ternary), BASELINE	Composite index (ternary + work)	Decide on her health	Decide on her social life	Decide on main purchases
$Post \times Treat$	4.181***	3.667***	0.207***	0.0494*	0.114***
	(1.058)	(1.069)	(0.0353)	(0.0268)	(0.0389)
Relative effect [a]	**0.126**	**0.117**	**0.177**	**0.055**	**0.180**
Observations	27,782	27,782	27,782	27,782	27,782
R-squared	0.051	0.058	0.074	0.057	0.079

	Perception about women's integrity and well-being				Labor market participation	
	Tolerate domestic violence	Tolerate domestic violence [b]	Agree with daughters' excision	Agree with daughters' excision [b]	Overall work	Paid work
$Post \times Treat$	-0.0269	-0.0486*	-0.0647**	-0.0623**	-0.0180	-0.0207
	(0.0266)	(0.0285)	(0.0273)	(0.0292)	(0.0144)	(0.0129)
Relative effect [a]	**-0.059**	**-0.107**	**-0.102**	**-0.099**	**-0.115**	**-0.146**
Observations	27,782	20,909	11,893	8,691	27,781	27,782
R-squared	0.236	0.242	0.384	0.395	0.154	0.171

Linear estimations based on 2008 and 2014 DHS. Estimation of woman's empowerment (ternary composite index) on the treatment, i.e. above-median protest intensity (based on the Egyptian Revolution database and defined as the governorate-level proportion of fatalities, injuries and arrests). We use the specification of model 4 in Table 1 (controls include wealth, urban, education of the wife, age of wife and husband, husband in work, religion, birth cohort, municipality dummies, interactions between POST and the controls). Standard errors in parentheses are clustered at the municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels respectively.

[a] Effect relative to the control group pre-period average empowerment, in %.

[b] Interview without others being present including the husband.

Table C2: Effect of the Arab Spring Events: Alternative Final Say Measures based on Binary Answers (using Protest Intensity for Treatment Definition)

	Final say: binary measure, empowerment = she decides[b]				
	Composite index	Composite index (+ work)	Decide on her health	Decide on her social life	Decide on main purchases
$Post \times Treat$	3.636**	3.321**	0.118***	0.00859	0.0126
	(1.618)	(1.532)	(0.0334)	(0.0178)	(0.0122)
Relative effect[a]	**0.293**	**0.216**	**0.407**	**0.089**	**0.237**
Observations	27,782	27,782	27,782	27,782	27,782
R-squared	0.061	0.061	0.084	0.051	0.038
	Final say: binary measure, empowerment = she decides alone or with husband[c]				
	Composite index	Composite index (+ work)	Decide on her health	Decide on her social life	Decide on main purchases
$Post \times Treat$	7.628***	5.482***	0.0890***	0.0408*	0.101**
	(2.151)	(1.602)	(0.0181)	(0.0210)	(0.0390)
Relative effect[a]	**0.100**	**0.074**	**0.101**	**0.051**	**0.174**
Observations	27,782	27,782	27,782	27,782	27,782
R-squared	0.128	0.144	0.091	0.097	0.110

Linear estimations based on 2008 and 2014 DHS. Estimation of woman's empowerment (ternary composite index) on the treatment, i.e. above-median protest intensity (based on the Egyptian Revolution database and defined as the governorate-level proportion of fatalities, injuries and arrests). We use the specification of model 4 in Table 1 (controls include wealth, urban, education of the wife, age of wife and husband, husband in work, religion, birth cohort, municipality dummies, interactions between POST and the controls). Standard errors in parentheses are clustered at the municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels respectively.

[a] Effect relative to the control group pre-period average empowerment, in %.

[b] Binary variable 1= wife decides alone, 0= joint decision or husband decides alone.

[c] Binary variable 1= wife decides alone or joint decision, 0= husband decides alone.

Table C3: Effect of the Arab Spring Events: Alternative Outcomes (using Women participation for Treatment Definition)

	Final say: Ternary indices				
	Composite index (ternary), BASELINE	Composite index (ternary + work)	Decide on her health	Decide on her social life	Decide on main purchases
$Post \times Treat$	3.498***	3.716***	0.122***	0.0274	0.0443
	(0.965)	(0.943)	(0.0330)	(0.0264)	(0.0406)
Relative effect[a]	**0.104**	**0.123**	**0.106**	**0.031**	**0.077**
Observations	27,782	27,782	27,782	27,782	27,782
R-squared	0.052	0.058	0.070	0.057	0.078

	Perception about women's integrity and well-being				Labor market participation	
	Tolerate domestic violence	Tolerate domestic violence [b]	Agree with daughters' excision	Agree with daughters' excision [b]	Overall work	Paid work
$Post \times Treat$	0.0335	0.0328	-0.0704***	-0.0481*	0.0385***	0.0305**
	(0.0265)	(0.0284)	(0.0256)	(0.0285)	(0.0124)	(0.0124)
Relative effect[a]	**0.067**	**0.069**	**-0.121**	**-0.085**	**0.226**	**0.202**
Observations	20,909	11,893	8,691	27,781	27,781	27,782
R-squared	0.241	0.384	0.395	0.154	0.154	0.171

Linear estimations based on 2008 and 2014 DHS. Estimation of woman's empowerment (ternary composite index) on the treatment, i.e. above-median women's participation intensity based on the 2014 SYPE survey (defined as governorate-level proportion of women engaged in Arab Spring demonstrations). We use the specification of model 4 in Table 1 (controls include wealth, urban, education of the wife, age of wife and husband, husband in work, religion, birth cohort, municipality dummies, interactions between POST and the controls). Standard errors in parentheses are clustered at the municipality level. ***, ** and * indicate significance at the 1%, 5% and 10% levels respectively.

[a] Effect relative to the control group pre-period average empowerment, in %.

[b] Interview without others being present including the husband.