



Gender, Institutions and Politics

Quentin Lippmann

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Gender, Institutions and Politics

Soutenue par

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Summary

This thesis studies the link between institutions, gender and politics. Three questions are studied: can institutions undo gender norms? Would institutions be more gender-egalitarian if they were headed by women? Why are women absent from positions of power?

The first chapter of this thesis, co-written with Alexandre Georgieff and Claudia Senik, tests whether institutions can undo gender. In particular, we study the consequences of institutions on the perpetuation of gender norms. We study the norm according to which a woman should earn less than her husband. Using the German division as a natural experiment, we show that East German institutions have undone gender. East German women can earn more than their husband without increasing their number of housework hours, put their marriage at risk, or withdraw from the labor market. By contrast, the norm of higher male income and its consequences are still prevalent in the West.

The second chapter studies whether institutions would be more gender-egalitarian if more women were heading them. In particular, I test whether female politicians have the same priorities than their male counterparts. The context studied is the French Parliament from 2001 to 2017. Using text analysis and quasi-experimental variations to randomize legislators' gender, this chapter shows that women are twice more likely to initiate women-related amendments in the Lower House. Women's issues constitute the key topic on which women are more active, followed by health and childhood issues whereas men are more active on military issues. I provide supporting evidence that these results are driven by the individual interest of legislators. Finally, I replicate these results in the Upper House by exploiting the introduction of a gender quota.

The third chapter studies the reasons behind the underrepresentation of women in positions of power. I investigate whether the persistence of incumbents hinders female access to political positions when incumbents are predominantly men. I exploit regression discontinuity from close electoral races in French municipalities to randomize the eligibility of

incumbent mayors for reelection. Despite a context increasingly favorable to the election of women, I find that the persistence of incumbents does not block female access to the position of mayor. I investigate the mechanisms and show that it is more difficult for a woman to replace a female incumbent than a male one.

Discipline : Sciences économiques

Mots-clés : Gender, Institutions, Politics, Public Policies

Résumé

Cette thèse vise à étudier le lien entre institutions, genre et politique. Elle cherche à répondre à trois questions: les institutions peuvent-elles défaire les normes de genre ? Les institutions seraient-elles plus égalitaires si elles étaient dirigées par des femmes ? Pourquoi les femmes sont-elles absentes des positions de pouvoir ?

Le premier chapitre de cette thèse vise à étudier le rôle des institutions dans la création des normes de genre. La norme étudiée est celle selon laquelle une femme doit gagner moins que son mari. En utilisant, la division de l'Allemagne comme une expérience naturelle, nous montrons que les institutions égalitaires est-allemandes ont défait le genre. Après la réunification, une femme est-allemande peut gagner plus que son mari sans augmenter son nombre d'heures de travail domestique, risquer de divorcer ou de se retirer du marché du travail. A l'opposé, en Allemagne de l'Ouest, ces comportements sont toujours observables.

Le deuxième chapitre étudie si les institutions seraient plus égalitaires avec des femmes à leur tête. En particulier, nous cherchons à déterminer si les femmes politiciennes ont les mêmes priorités que leurs collègues masculins. Le contexte étudié est celui du Parlement Français durant la période 2001-2017. En combinant des méthodes d'analyse de texte avec des variations exogènes dans le sexe des politiciens, ce chapitre montre que, relativement à leurs collègues masculins, les femmes politiciennes à l'Assemblée Nationale défendent plus les intérêts des femmes dans la population. Le thème où les différences sexuées d'activité parlementaire sont les plus marquées est précisément celui de l'égalité femmes-hommes, suivi des thématiques liées à l'enfance et à la santé. Les hommes sont plus actifs sur les thématiques militaires. Nous montrons que ces différences proviennent de l'intérêt individuel des législateurs. Enfin, nous répliquons ces résultats au Sénat en exploitant l'introduction d'une réforme qui a imposé la parité.

Le troisième chapitre s'intéresse aux raisons derrière la sous-représentation des femmes dans les positions de pouvoir. Il cherche à déterminer si dans un contexte où les politiciens sont majoritairement des hommes, la "prime aux sortants" lors d'élections réduit le

nombre de femmes élues. Le contexte étudié est celui des municipalités de moins de 1000 habitants en France. Nous montrons que contrairement à ce qu'on peut s'attendre, lorsque les politiciens ne sont pas éligibles à leur réélection, la part de femmes élus n'augmente pas. C'est parce qu'il est plus difficile pour une femme de remplacer une femme que de remplacer un homme.

Discipline : Sciences économiques

Mots-clés : Genre, Institutions, Politique, Politiques Publiques

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Main Introduction

In most developed countries, women lag behind men on the labor market. They have a lower wage, devote more time to domestic chores and are underrepresented in positions of power. During the second half of the 20th century, the magnitude and the list of these inequalities have unambiguously diminished (Goldin, 2006). Yet, they remain a reality whose disappearance which seemed conceivable in the second half of the 20th century, seems to be receding as we move into the 21st century. It is well established that the reduction of these inequalities has slowed down over the past twenty years (Blau and Kahn, 2006).

In parallel with these developments, economic research on the determinants of gender inequality has largely evolved towards the inclusion of psychological and social factors (Bertrand, 2011). In addition to arguments based on discrimination and human capital accumulation on the labour market (Altonji and Blank, 1999), comparative advantages (Becker, 1973) and bargaining power within the household (Chiappori, 1988), there are now also arguments based on preferences and norms to explain the persistence of gender roles. The notion of norm is closely linked to that of identity developed by Akerlof and Kranton (Akerlof and Kranton, 2000). Norms would lock individuals into gender-specific areas, both on the labour market and within the household, by dictating the type of behaviour to be followed. Changes in norms are considered to be one of the main factors influencing the trajectory of gender inequality (Eckstein and Lifshitz, 2011), through the transformation of gender roles. The measurement of norms involved both objective variables, such as behaviour, and subjective variables, such as attitudes and expectations towards each sex.

Research has identified some determinants of gender norms, including technological advances (Goldin and Katz, 2002, (Goldin, 2006)), parental influence (Fernández *et al.*, 2004), culture (Fernández and Fogli, 2009), information (Fogli and Veldkamp, 2011) or historical factors (Alesina *et al.*, 2013). Yet, there remains one factor on which little is understood: institutions. As the definition of institutions can be ambiguous, it is worth

explaining clearly what this term designates throughout this thesis. We follow the definition of institutions proposed by Alesina and Giuliano (2015). Institutions designate the formal constraints (rules, laws, constitutions) that structure human interactions. Thus, institutions can be considered as what we can change in the organization of societies. An obvious example is public policies.

This thesis is structured around three questions encompassing the relationship between institutions and gender equality. The first question asks whether institutions can undo gender and transform gender norms deeply rooted in human societies. The second question asks whether introducing more women at positions of power could make institutions more gender-egalitarian. Finally, the third question tests one concrete hypothesis according to which women are underrepresented at positions of power. The answers to these three questions constitute the three chapters of my thesis.

The first chapter of this thesis analyzes whether institutions have the potential to undo gender and transform gender norms which are deeply rooted in human societies. Few studies have sought to develop a causal analysis of the impact of institutions on gender attitudes and norms. Such an analysis faces endogeneity problems, since institutions facilitating gender equality are more likely to emerge in environments where attitudes are already favourable to them. Recent studies have rather highlighted correlations between institutions and labour market behaviour or have focused on specific public policies (Thévenon and Solaz, 2013), such as parental leave, to measure their effectiveness.

To develop a causal test of the impact of institutions on gender norms, we use the 41-year division of Germany as a natural experiment. We show that the GDR's gender-equal institutions created a culture that has undone the male breadwinner norm and its consequences. Since reunification, East Germany still differs from West Germany not only by a higher female contribution to household income, but also because East German women can earn more than their husbands without having to increase their number of housework hours, put their marriage at risk or withdraw from the labor market. By contrast, the norm of higher male income, and its consequences, are still prevalent in West Germany.

The second chapter of this thesis tests a concrete hypothesis to transform institutions and make them more gender-friendly. This hypothesis consists in introducing more women at positions of power. Formal institutions, i.e. politics, are overwhelmingly ruled by men and it is possible that introducing more women at their head would lead to policies better representing the interests of women in the society.

To test this hypothesis, I combine quasi-experimental variations to randomize legisla-

tors' gender and text analysis to identify women-related policies among more than 300,000 amendments discussed in the French Parliament during the 2001-2017 period. I exploit mixed-gender close races in the Lower House to show that female legislators are twice more likely to initiate women-related amendments. As compared to other topics, I establish that women's issues constitute the key topic on which women are more active relatively to men, followed by health and child issues. At the other end of the spectrum, men are more involved in military issues. I investigate the mechanisms and provide evidence that the activity of female legislators cannot be entirely explained by constituents' preferences or political parties' strategies but stems from individual interest of legislators. Finally, using a difference-in-differences strategy that directly exploits the introduction of a gender quota in the Upper House, I replicate these findings.

Finally, the last chapter of this thesis studies why women are underrepresented from positions of power. In particular, it tests whether the persistence of incumbents hinders the election of female politicians. It is often assumed that in a context where politicians are predominantly men and where they enjoy an electoral advantage for reelection, the entry of new candidates, including women, is limited. This argument has important implications as it could serve as a motivation to introduce term limits in politics. Yet, there is little research that has investigated its validity.

I exploit regression discontinuity from close electoral races in French municipalities to randomize the eligibility of incumbent mayors for reelection. Despite a context increasingly favorable to the election of women, I find that the persistence of incumbents does not block female access to the position of mayor. I investigate the mechanisms and show that it is more difficult for a woman to replace a female incumbent than a male one. This is consistent with a backlash or stereotype threat effect penalizing women after a female incumbent.

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Chapter 1: Undoing Gender with Institutions. Lessons from the German Division and Reunification

This chapter is co-written with Alexandre Georgieff and Claudia Senik.

Abstract

Using the 41-year division of Germany as a natural experiment, we show that the GDR's gender-equal institutions created a culture that has undone the male breadwinner norm and its consequences. Since reunification, East Germany still differs from West Germany not only by a higher female contribution to household income, but also because East German women can earn more than their husbands without having to increase their number of housework hours, put their marriage at risk or withdraw from the labor market. By contrast, the norm of higher male income, and its consequences, are still prevalent in West Germany.

1 Introduction

Men spend more time in paid work, and women more time in housework. In spite of the rise in female participation in the labor market and the feminist struggle for greater symmetry, this gender-wise specialization within couples remains a quasi-universal norm. As a consequence, gender gaps in labor force participation and earnings have not subsided (Bertrand *et al.*, 2015; Blau and Kahn, 2017) and the male breadwinner model remains prevalent.

What is the rationale for the stability of this pattern? Household economists have proposed various explanations based on the notion of comparative advantage (Becker, 1973, 1974), with or without bargaining between spouses (Chiappori, 1988, 1992; Weiss, 1997). These comparative advantages can be seen in turn as partly natural or as being dictated by the nature of economic activity and job characteristics at each stage of a society's development (Alesina *et al.*, 2013; Autor *et al.*, 2003; Beaudry and Lewis, 2014; Black and Spitz-Oener, 2010). Institutions also certainly play a role in designing the architecture of choices for men and women, and providing incentives for more or less specialization (Esping-Andersen, 2009).

On top of these potential determinants, social scientists have pointed out the superimposition of norms (Akerlof and Kranton, 2000, 2013), postulating that people may attach some value to the roles they endorse per se, such as gender roles. An entire set of stylized facts that cannot be rationalized within standard economic models comes in support of this idea. One of the most striking of these observations is the reaction of households when the male breadwinner norm is violated. While any economic model of decision-making within the family would predict that a spouse should decrease her number of housework hours as her personal contribution to the household income increases, empirical evidence shows that things are not so simple. Beyond spouses' income equality threshold, when a woman becomes the primary breadwinner, she starts *doing gender* (West and Zimmerman, 1987) by increasing her number of housework hours, and sometimes withdrawing from the labor force. Such marriages also become more unstable.¹ It would be difficult to make sense of such behaviors without recourse to the notion of gender identity. Existing studies have mostly contributed to establishing the existence of this gender-unequal norm, but little is known about its origin and the part played by public policy in constructing or

¹See Greenstein (2000), Akerlof and Kranton (2000), Bittman *et al.* (2003), Evertsson and Neramo (2004) and Schneider (2011) on housework, Heckert *et al.* (1998); Jalovaara (2003) and Liu and Vikat (2007), on the risk of divorce, and Bertrand *et al.* (2015) on the three types of consequences.

deconstructing it.

This paper brings the first causal evidence that the male breadwinner norm is cultural and can be undone by institutions. Developing a causal test of the role of institutions is a major empirical challenge. Specifically, gender-equalizing institutions are much more likely to emerge in an environment where mentalities have already become more gender friendly. To overcome this empirical hurdle, we focus on Germany and exploit the natural experiment constituted by the 41-year division of the country. Before World War II, prior to the division, gender norms, including female labor force participation, were essentially similar in Eastern and Western regions. During the division, East Germany adopted gender-equalizing policies, in line with the universal "right" (and obligation) to work. Work-family balance programs, kindergarten and other childcare facilities were put in place (Bauernschuster and Rainer, 2012). In the meantime, a traditional family policy prevailed in West Germany. The institutions and policies implemented in the two regions radically diverged and so did gender roles. As a result, in 1989, women's labor force participation in the German Democratic Republic (GDR) had reached about 89%, one of the highest in the world, against 56% in West Germany (Rosenfeld *et al.*, 2004). After reunification, the government of the former Federal Republic of Germany (FRG) took over East Germany and rapidly dissolved its institutions and structures and absorbed them into those of West Germany, which remained unchanged.

Using data from the German Socio-Economic Panel from 1991 to 2012, we establish that, since reunification, the male breadwinner norm has been prevalent in West Germany but not in the East. First, we show that women who earn more than their husbands "compensate" by increasing their number of housework hours in West Germany. But this is not the case in East Germany, where women monotonically keep decreasing the time they spend on housework as their contribution to the household finances rises. Consistently, in West Germany, the risk of divorce increases for couples where the wife switches from earning less to earning more than her husband, whereas this is not the case in East Germany. Finally, we show that when a woman's potential income is higher than that of her husband, she is more likely to withdraw from the labor market, but only in West Germany. Likewise, when a West German woman actually starts earning more than her husband, she is more likely to withdraw from the labor market in the following year. These behaviors are mirrored by self-reported preferences, as East German women attach almost as much value to paid work as men, in contrast with West German women.

To demonstrate the robustness of our results, we run systematic placebo exercises to

establish that it is the focal point of equal incomes that triggers these reactions, and not any other alternative cut-off point. Similarly, we show that it is the former Berlin wall that constitutes the dividing line, and not any other division of Germany. We also provide substantive evidence showing that our results do not stem from pre-existing or current differences between Eastern and Western regions. First, we show that before the division, Eastern and Western regions had similar industrial, employment and social structures: we show this using first-hand statistical sources pertaining to the year 1933, as well as Prussian data from 1886 and 1849. We also illustrate the diverging trends in terms of female labor market participation. Second, we rule out the suspicion that East-West differences in household behavior could be due to other historical differences in unobservables or persisting structural differences, such as economic conditions. To do so, we focus on areas where it is likely that people face the same structural conditions. We show that among couples who currently live in the West, i.e. in the same environment, those who migrated from the East after 1990 display much more egalitarian behavior in terms of female labor market participation, wage earnings and housework time. In the same spirit, we use another survey (GoGold) to show that among women who currently live in East or West Berlin, those who were born in a former socialist country, or whose mother was born in a former socialist country, are much more likely to be working full time (*ceteris paribus*) than those who were not. We also focus on couples who live near the former East-West border and document the sharp spatial discontinuity of the female contribution to household income at the border and re-establish our main results on this subsample. Finally, we exploit the heterogeneity within West Germany to rule out the suspicion that our findings may be driven by wage structure differentials.

These results shed light on the sources of gender inequality. After decades of progress, female labor force participation has recently plateaued (Blau and Kahn, 2017) and researchers have started to investigate the determinants of social norms influencing gender equality. These include technological changes (Goldin and Katz, 2002), the influence of ancestors (Fernández *et al.*, 2004; Fernandez and Fogli, 2009), information (Alessandra and Laura, 2011) and past technology (Alesina *et al.*, 2013). We add to this literature by considering the role of institutions in sustaining the male breadwinner norm.

Our results are directly related to the literature on social norms and preferences, beyond gender issues. Experiments in behavioral economics have shown that, in dictator games for instance, the situation of equal earnings is a focal point that powerfully influences decisions (Rabin, 1993; Charness and Rabin, 2002). Cultural economics has also shed light

on the evolution of social norms (Bisin and Verdier, 2001, 2011; Alesina and Giuliano, 2015; Fernández, 2013). Here, we illustrate the influence of institutions on social norms and identity.

This paper is in the line of Alesina and Fuchs-Schündeln (2007) who exploited the same episode of socialism in Germany to study the lasting (and progressively withering) effect of socialist institutions on mentalities. Regarding gender attitudes, several studies, such as Breen and Cooke (2005), Bauernschuster and Rainer (2012) or Beblo and Gorges (2018) have illustrated the smaller gender gap in East Germany, as compared with West Germany, in terms of self-reported attachment to work. Campa and Serafinelli (2016) show that this appears to be a hallmark of socialist states. Besides work attitudes, Lippmann and Senik (2018) have also shown that the gender gap in mathematics is smaller in East Germany and in former socialist countries..

The rest of this work is organized as follows. Section 2 recalls the institutional background of East and West Germany. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 studies the impact of higher female earnings on housework hours, the risk of divorce, and female labor market participation. Section 6 provides robustness checks. Section 7 illustrates the differences in work attitudes among Western and Eastern couples. Section 8 concludes.

2 The German Division as a Natural Experiment

2.1 Before the Division

The division of Germany was drawn by a postwar agreement between the Allies, on the basis of the zones occupied by the Soviet Union and Western countries. In 1949, five Länder formed the GDR and the remaining eleven constituted the FRG. The line of division was thus arguably unrelated to pre-existing differences between the two regions.

To provide evidence on this matter, we collected data from German statistical yearbooks before the division. Table 1.1 describes the situation in 1933.² Columns 1 and 2 provide descriptive statistics for the Eastern and Western regions of Germany. Column 3 computes the differences between the two regions. In 1933, the employment structure was similar in the two regions. About 45% of East Germans worked in industry against 40% in the West.

²In the Appendix Table 1.B1, we replicate the same exercise using data from the 19th century in 1886 and 1849. Similarly, we find that there was very little differences between East and West Germany in the 19th century.

As regards gender behavior, the table shows that the female share of employment was 2.8 percentage points higher in the East, and the birth rate (per thousand) 1.95 points higher in the West.

How did the small East/West differences compare with the average regional differences? To investigate this question, we performed a permutation test that follows the logic of a Fisher exact test, and compared, for each measure, the East-West difference with the picture that would emerge from any random partition of Germany (excluding Berlin) into two groups of respectively 15 and 5 regions (column 5). It turns out that in 1933, the structural dissimilarities between the two regions that would later become East Germany and West Germany (excluding Berlin) were not any different from what would stem from any random division of the 20 regions into two groups of 5 + 15 regions. This is attested by the p-value (column 5). This result supports the idea that the division of Germany was not influenced by pre-existing regional differences.

2.2 Diverging Trends during the Division

After the division, between 1949 and 1990, the GDR rapidly set up institutions in favor of gender equality. Beyond its constitution ensuring full equality between men and women, the *Mother and Child Care* and *Women's Rights* Acts, adopted in 1950, aimed at '[establishing] a range of social services in support of full female employment, including a network of public childcare centers, kindergartens and facilities for free school meals' (Cooke, 2007, p. 935), as well as paid maternity leave. By 1972, additional policies expanded childcare facilities and extended paid maternity leave to 18 weeks. A final set of reforms implemented between 1972 and 1989 improved childcare facilities, extended parental leave to 20 weeks and allowed fathers as well as grandmothers to take this leave (Cooke, 2007). In summary, these policies were targeted at making participation in the labor force compatible with maternity (see Goldstein and Kreyenfeld, 2011 about fertility trends in both regions).

In the meantime, the FRG's policies strengthened the traditional family model. Irregular school schedules and scarce childcare facilities inhibited female employment. The tax system favored single earner families, as non-employed spouses and children could get public health insurance at no extra cost. Until 1977, the Marriage and Family law stated that: '*The wife is responsible for running the household. She has the right to be employed as far as this is compatible with her marriage and family duties*' (Civil Code on the *Effects*

of Marriage in General, title Five, Section 1356).³ Subsequent policies then alternated more or less conservative incentives for female participation in the labor market.

As a result of these very different policies, the female labor market participation rate started to diverge after the division. To illustrate the impact of the two different sets of policies during the division, we collected data from statistical yearbooks from 1959 to 1987. Figure 1.1 displays the diverging trends of women's share in total employment. In the FRG, the share of employed women, as a percentage of the total female population remains steadily around 30%, whereas in the GDR, it rises from approximately the same level to 50% between 1959 and 1987 (the years for which these statistics are available).⁴

2.3 Persisting Differences after Reunification

After reunification, the government of the former Federal Republic of Germany (FRG) took over East Germany and rapidly dissolved its institutions and structures, absorbing them into those of West Germany, which remained unchanged. Yet, persisting differences between the two regions were still observable ten years later in 2000. Labor force participation was still approximately the same across gender in the former GDR (around 80%), whereas the gap remained wide in West Germany, with 65% of women in the labor force against 81% of men (Schenk, 2003). As we will show, these objective differences are supported by opinions regarding gender roles. In terms of paid work time, in 2000, East German workers generally worked longer hours than West Germans: 35 hours for women and 42 hours for men in the former GDR against respectively 29 and 40 hours in the former FRG. This is probably a legacy of the different labor laws that prevailed during the division.⁵ The status of part-time employment also differed considerably across regions. In West Germany, part-time workers, most of whom were women, often worked less than 20 hours, and were not eligible for the same social benefits as full-time workers (Rosenfeld *et al.*, 2004). In East Germany, part-time workers had longer hours, received identical social benefits and used these contracts primarily as a transition to retirement.

This does not mean that there are no gender differences at all in East Germany. For instance, Rosenfeld *et al.* (2004), document the existence of gender wage gaps and occu-

³See also Rheinstein and Glendon (1978).

⁴These diverging trends happened notwithstanding migration. Cornelius and Tsuda (2004) reports that 730,000 Germans moved from the Soviet zone to the other zones in the late 1940s, and another 3.8 million moved from East Germany to West Germany between 1949 and the building of the Berlin Wall in August 1961; then only 600,000 Germans moved West between 1961 and 1988.

⁵The standard regulatory full-time number of work hours per week was 43.75 in the GDR against 36 to 39 in the FRG (Rosenfeld *et al.*, 2004.)

pational segregation. Additionally, within the household, although men participate more in housework in the East than in the West, Eastern women still take on a greater share of housework (Cooke, 2007).

3 Data

3.1 Source

We use the *German Socio-Economic Panel*, a longitudinal survey run by the German Institute for Economic Research (DIW, Berlin).⁶ This survey was started in 1984 in West Germany and was extended to East Germany in 1990. In 1998, 2000, 2002, 2006, 2009, 2011 and 2012, additional German households were added to the initial sample. We use 22 waves, from 1991 to 2012.

3.2 Main Explanatory Variables

East versus West. Our exercise consists in contrasting the behavior of East versus West Germans. We exploit the biographical information contained in the dataset. The questionnaire asked all individuals: "*Where did you live before 1989?*". We define an *East* dummy variable that takes the value 1 (0) if both spouses lived in GDR (FRG) before 1989, independently of where they live at the time of the survey.⁷

Relative Income. Our main explanatory variable is a dummy that equals 1 if the wife earns more than her husband and 0 otherwise (hereafter: *WifeEarnsMore*), where income measures monthly labor earnings.

3.3 Main Outcomes

Housework. The time spent on housework is measured using the following question: "*What is a typical weekday like for you? How many hours per normal workday do you spend on housework (washing, cooking, cleaning)?*". The definition of housework, i.e. the list of tasks included in the survey, follows the general usage in the literature. In particular, it does not include the time parents spend with children. The norm concerning childcare

⁶Socio-Economic Panel (SOEP), data for years 1984-2012, version 29, SOEP, 2013. For more details, see Goebel *et al.*, 2018.

⁷This definition excludes mixed couples where one spouse originates from GDR and the other from FRG. For robustness, we also included this type of couple and define an *East* dummy variable that takes the value 1 (0) if only one spouse lived in GDR (FRG) before 1989. We performed this robustness check using successively the origin of the wife and of the husband. The results remained similar.

has changed since the 1970s: a new norm of intensive parenting has diffused, whereby the time spent with children is more and more considered as leisure. This is particularly true of more educated parents and wealthier families (Sullivan, 2010). For this reason, we leave this aspect of couples' time-use aside from our main housework measure.

Divorce. In Section 5.2, we look at the impact of female relative income on the risk of divorce. We consider the sample of married working couples, aged 18 to 65 years old, and estimate the likelihood of divorce within the coming years, according to their relative income. We use the marital status reported by both spouses at each wave, as well as the biographic data file. As divorce takes time, our main variable of interest is the risk of divorce in a 5-year horizon.

Labor Market Participation. In Section 5.3, we look at the impact of female relative income on her participation in the labor market. We estimate the likelihood of withdrawing from the labor market in a one-year horizon ($T+1$). Consequently, the variable of interest is a dummy that codes 1 if the individual has no labor earnings in $T+1$ (out of the labor market) and 0 otherwise.

Attitudinal Variables. We use subjective attitudes elicited in the SOEP survey, namely: *How important is success at work for satisfaction?* *How important is marriage for satisfaction?* *How important is work for satisfaction?* *How important is a successful career for satisfaction?* *How important is family for your satisfaction?* The first two questions were asked in 1992, 1995, 2004, 2008 and 2012. The remaining three questions were asked in 1991, 1994, 1998 and 1999. Given the distribution of preferences (see Figure 1.F1 to Figure 1.F5 in the Appendix), we define dummy variables that equal 1 if the respondent has declared the matter to be very important and 0 otherwise.

3.4 Descriptive Statistics

Figure 1.2 depicts the entire distribution of female relative income in dual-earner married couples, aged 18 to 65 years old according to where they lived during the division (East or West Germany). The distribution is extremely skewed to the left in the sample of West German couples, where the mode is the point where the wife earns about 20% of the total family earnings. By contrast, in the East German sample, the distribution is much more symmetric, with a mode around equal earnings.⁸ Nevertheless, in both regions, there are significantly fewer couples in a situation where the wife earns more than her husband

⁸Additionally, Figure 1.C1 shows that these patterns display a slight convergence across cohorts and Figure 1.C2 describes the limited evolution over time.

than the opposite and the two distributions seem very similar beyond the point where the wife earns about 70% of the total income.

Table 1.2 presents descriptive statistics on our main sample, using housework hours as the outcome. There are 6,104 couples for whom we know whether both members lived in East or West Germany before reunification: 1,976 are from East Germany and 4,129 from West Germany. On average, these couples are present in the sample for 5.6 years, which makes a total of 34,205 observations over 22 years (22,091 from West Germany and 12,114 from East Germany).

Households differ in several dimensions across the two samples. On average, West German households are richer, and more often childless. Men's level of income and contribution to household finance is higher in Western couples than in Eastern ones.⁹ The opposite holds for women. There are more Eastern couples where women earn more than their spouse (29% versus 11% for Western couples). East German men spend a slightly higher number of hours in housework than West German men, and the reverse is true for women.

In the West, we observe 698 transitions to a situation where the wife earns more and 513 in the opposite direction. These numbers are respectively 719 and 602 in the East. There are 16,208 observations where the wife always earns less than her husband in the West (resp. 6,190 in the East) and 1,423 where the wife earns more in all periods (resp. 2,316 in the East).

In the Appendix, Tables 1.C1 and 1.C2, we present descriptive statistics of the sample for the study of divorce and labor market participation. These samples are very similar to that described in Table 1.2. West German couples exhibit a higher divorce rate (within a 5-year horizon) than East German ones (9% versus 7%). Additionally, West German women withdraw more often from the labor market than East German ones (2% versus 1%).

4 Econometric Specification

We use the German division as a natural experiment and argue that, absent this division, similar preferences would prevail in East and West Germany. In a sense, this setting is similar to a difference-in-differences strategy where we assume a similar starting point

⁹Note that household income includes all elements of income, including transfers and return on financial assets, whereas we use net labor income to construct our measure of women's relative contribution to household finance.

and a common trend assumption between the two regions. To illustrate the existence of the male breadwinner norm, we follow Bertrand *et al.* (2015) and focus on the specific point of equal incomes of spouses.¹⁰ Three outcomes are considered: the number of housework hours, the risk of divorce and labor market participation.

Formally, we estimate the following equation:

$$Y_{it} = \gamma_1 WifeEarnsMore_{it} + \gamma_2 WifeEarnsMore_{it} * East_i + \beta X_{it} + \mu_i + \epsilon_{it} \quad (1)$$

Where i is the subscript for the individual and t for time. $WifeEarnMore_{it}$ is a dummy that equals 1 if the wife i earns more than her husband at time t . If the norm is prevalent among West Germans but not among East Germans, we expect γ_1 to be positive and significant, whereas γ_2 should be negative. If this norm does not exist among East Germans, γ_2 should totally offset γ_1 .

The controls included in X_{it} are the log of household income, respondent and partner's age and age squared, respondent and partner's education level (4 categories), a dummy controlling for the presence of children, year fixed-effects and regional fixed-effects (at the Land level, 16 categories). Depending on the outcome considered, we also control for the degree of specialization within the couple, namely relative income, as this variable is likely to be correlated with the division of housework or the probability of withdrawing from the labor market.¹¹

Our preferred specification contains individual fixed-effects μ_i . This is important because it is likely that spouses match on unobserved characteristics, such as their preferences in terms of household income structure, which produces self-sorting of spouses into different types of families. For instance, without individual fixed-effects, the results could be driven by overachieving wives who earn more than their husbands and also spend more time on housework, or alternatively underachieving husbands with a strong preference for idleness. Introducing fixed-effects alleviates this selection issue.

¹⁰Although this paper does not build on a structural model, we discuss in the Appendix Section A how our findings can be incorporated in the framework of a structural model of household behavior. In this framework, transgressing the male breadwinner norm produces a decrease in the value of the marriage which increases the risk of divorce or leads the wife to restore this value by shifting the division of tasks toward a more traditional arrangement.

¹¹As pointed by Bertrand *et al.* (2015), controlling for relative income is important when standard Beckerian forces influence the outcome. In particular, they will lead the wife to do less housework when her relative income increases. For instance, couples where the wife earns 10% of the household income are probably couples where the wife has specialized in unpaid work activities. She is thus mechanically more likely to perform housework than a woman earning 40% of household income.

One crucial assumption in this setting is the exogeneity of the treatment. If an individual could move from East to West Germany, or vice-versa, because of her preferences for some gender-relevant aspect of institutions, this would bias our analysis. To overcome this issue, as explained in section 3.2, we define an *East* dummy variable that takes the value 1 if both spouses lived in GDR before reunification, independently of where they live at the time of the survey. Given that migration between the two regions was forbidden during the division and that there were no stark differences between East and West Germany before the division, exposure to the institutions can be considered as random.¹²

Unlike Bertrand *et al.* (2015), we do not test for the existence of a discontinuity in the distribution of household income. This is because, in our dataset, the number of observations at the point of equal earnings is too high, and is higher in the East (2.75%) than in the West (1.37%). This makes it impossible to run a McCrary test for the discontinuity of the distribution function at the point of equal earnings.¹³ However, in order to test for the relevance of the point of equal income, we run a robustness exercise consisting in estimating all of our regression equations with alternative thresholds, i.e. alternative focal points, as explained in section 6.3.1. We establish that the only relevant threshold is that of equal earnings.

The sample used in our main specification contains dual-earner married couples, aged 18 to 65 years old. The reason for not including households with unemployed adults is that this situation is most likely transitory and might not be reflected by the division of housework between spouses. We do not include couples where one spouse is out of the labor force, as the contribution of the latter to household finance is in most cases nil, and the allocation of her time into paid-work versus housework is trivially skewed. Moreover, couples where the wife changes from being out of the labor force to earning more than her husband are likely to be atypical. We select married couples rather than all couples because the former are generally more "stable" than simply cohabiting couples. However, in order to alleviate concerns about selection biases, we replicate all of our results using a larger sample including non-married and one-earner couples (See Appendix).

¹²In the Appendix, we replicate all our results with a geographical definition based on where households currently live. They remain essentially unchanged.

¹³We attribute this to the approximation of self-declared income by respondents in the SOEP survey, but we have no means to correct these figures and we are reluctant to -artificially- simply drop these observations.

5 Results - Consequences of Violating the Male Breadwinner Norm

We study whether there is a difference across Western versus Eastern German couples in terms of the consequences of a wife earning more than her husband. Our outcomes of interest are: housework hours, the risk of divorce and female participation in the labor market.

5.1 Housework. Couples of the Former GDR are Not "Doing Gender"

We start with non-parametric visual evidence about the supply of housework hours by women, according to whether the spouses lived in the former GDR or not. Figure 1.3 displays the number of female housework hours according to the contribution of female earnings to the total earnings of the couple.

The left-hand-side panel shows that among West German couples, women decrease their number of housework hours as their relative earnings rise, until they reach the vicinity of equal earning. Beyond that point, their number of housework hours stops decreasing and remains at about 2 hours per day. It even seems to increase again at about 75%. By contrast, the right hand-side panel shows that East German women monotonically reduce the time they devote to housework as their relative contribution to household finances increases. One can suspect an inflection in the curve at the point where women earn more than 80% of the couple's income, but due to the small number of couples who are in this situation, it is not possible to draw more than one dot.

Figure 1.4 represents the relationship between men's housework time and their wives' relative income. Men monotonically increase their number of housework hours, with a small plateau around the point of equal incomes.

Next, we estimate Equation 13 with and without individual fixed-effects using housework hours as an outcome. Following Gupta (2007) who argues that individual income is a decisive determinant of the number of housework hours, as it reflects women's degree of autonomy, we augment the baseline specification and control for individual labor incomes (in the Appendix, we also test without controlling for individual incomes or introducing hourly wages). This is not totally collinear with household income as the latter includes non labor income.

The results are displayed in Table 1.3. Columns 1 to 3 display cross-sectional OLS

estimates and columns 4 to 6 estimations including individual fixed-effects. Panel A is restricted to the sample of women, panel B to their husbands and panel C studies the time gap between the spouses.

Columns 3 and 6 of panel A show that, as expected, the number of housework hours performed by West German women increases when they earn more than their husband. In column 6, we see that West German women increase their time spent on housework by about 0.18 hours per day when they start earning more than their husbands. This is not the case in East Germany. The interaction between the two variables, *East* and *WifeEarnsMore* is negative, and its order of magnitude is sufficient to offset exactly the positive coefficient of *WifeEarnsMore*, so that there is no effect left for East German couples. Accordingly, the coefficient on *WifeEarnsMore* is statistically significant when the regression is run on the subsample of West German women (in columns 1 and 4) but it is not when the subsample includes only East German women (columns 2 and 5). These results hold in cross-sectional as well as in fixed-effects specifications. Additionally, the East dummy variable (column 3) attracts a negative coefficient, indicating that East German women spend less time on housework than West German women.

Because *doing gender* is about within-couple interactions, it is of interest to observe the difference in the time spent on housework by each spouse. Panel C presents estimates of the time gap on the usual controls. Again, for West German couples, the time gap increases when the wife earns more than her husband all other things being equal (column 1 and 4), but this is not the case for East German couples (columns 2 and 5). Likewise, the coefficients on *WifeEarnsMore* and *WifeEarnsMore*East* totally compensate each other, so that no effect is left in East Germany (columns 3 and 6).

To probe the robustness of the results, we also replicate our results with a definition of housework that includes childcare (Table 1.D1). Results are similar but the magnitude is much stronger when childcare is included. We also use the female share of total housework as the outcome variable instead of the time gap (Table 1.D2). Results point in the same direction but are less precisely estimated.

5.2 Marriage Instability

Where gender norms are compelling, transgressing them should put one's marriage at risk. To enquire, we look at the association between women's relative income and marital instability.

Table 1.4 displays the probability of divorce within the next five years according to

whether the wife earns more than her husband.¹⁴ The coefficient on *WifeEarnsMore* is positive and statistically significant in estimates that include individual fixed-effects (columns 4 and 6), but not in the cross-sectional estimates (column 1 to 3). Hence, it is a change in the situation of a couple that triggers divorce, rather than the difference between couples. This is consistent with the idea of self-selection of spouses into different types of couples.

Looking at column 4, we see that, among West German couples, when a wife starts earning more than her husband, the risk of divorce in the next 5 years increases by about 3 percentage points. Column 5 shows that nothing happens for East German couples and consequently, when pooling both types of couples (column 6), the coefficient on *WifeEarnsMore* is offset by the one on *WifeEarnsMore * East*. This is consistent with the results concerning the number of housework hours.

5.3 Labor Market Participation

Bertrand *et al.* (2015) show that, in order to abide by the male breadwinner model, American women avoid earning more than their husband. This drives some of them, when their earning capacity is greater than that of their husband, to simply withdraw from the labor market. We ask whether this traditional behavior is also adopted by Eastern and Western German couples.

To do so, we estimate Equation 13 with and without fixed-effects, where the outcome variable is a dummy variable indicating whether an individual leaves the labor market within a year (in $T+1$). Table 1.5 displays the results. Columns 1 to 3 display cross-sectional OLS estimates and columns 4 to 6 estimates that include individual fixed-effects. Panel A is restricted to the sample of women and panel B to their husbands.

The results are consistent with the previous outcomes. Columns 3 and 6 of panel A show that, as expected, the probability of a West German woman withdrawing from the labor market in $T+1$ increases when she earns more than her husband. The interaction between the two variables, *East* and *WifeEarnsMore* is negative, and its order of magnitude is sufficient to offset exactly the positive coefficient of *WifeEarnsMore*, so that there is no effect left for East German couples. In column 6, although the coefficient related to *WifeEarnsMore * East* is not significant, it is close to standard significant levels (p-value of 0.129). Consistent with the previous outcomes, panel B shows that men do not withdraw

¹⁴It would be unrealistic to imagine that couples divorce immediately as soon as the wife starts earning more than her spouse. Accordingly, Table 1.D7 displays the estimates of a linear probability model of the risk of divorce at different time-horizons.

from the labor market when their wife earns less than them. On the contrary, this situation reinforces the probability that they will remain in the labor market, especially in the West.

We now look at the labor market participation of women according to their potential income. We first predict individuals' potential income based on a Heckman selection model that includes a dummy variable for the presence of children in the selection equation.¹⁵ We then estimate potential wages for the entire sample of women based on the following characteristics: education (4 categories), age, age squared, year and Land fixed-effects. Next, we build a dummy that equals 1 if the potential wage of the individual is higher than the actual wage of his/her spouse (*WifeEarnsMorePotentially* and *ManEarnsMorePotentially*). About 21% of couples in our sample are in a situation where the potential earnings of the wife are higher than the actual earnings of her husband.¹⁶ Finally, we run a linear probability model in order to predict the likelihood of an individual participating in the labor market, according to the dummy *WifeEarnsMorePotentially* for the sample of women and *ManEarnsMorePotentially* for the sample of men. Since potential income can only change because of age, this specification does not include individual fixed-effects.

It turns out that when the potential wage of a woman is higher than the actual wage of her husband, the probability that she is out of the labor force increases by about 3 percentage points for West German women, whereas this pattern is reversed for East German women (Table 1.6). When they earn potentially more than their husband, the probability of their withdrawal from the labor market *diminishes* by about 3 percentage points. Turning to the analysis of male behavior, we find a consistent pattern with the previous table. West German men are more likely to work when they earn potentially more than their wife, whereas East German men remain insensitive to this possibility.

6 Robustness Checks

The norm of higher male earnings seems to be influential among West German couples, but not among former East German couples. All of the modalities of the typical *doing gender* behavior that have been documented in the literature seem to be at work among West German couples, i.e. higher female earnings are associated with longer housework hours, greater marriage instability and less participation in the labor market. This section tests the robustness of these results. We first test alternative samples and specifications.

¹⁵In the Appendix Table 1.D8, we reproduce these estimates including a married dummy in the selection equation. We obtain similar results.

¹⁶In terms of actual incomes, 19% of couples in our sample are in that situation.

Then, we examine alternative mechanisms that could drive the results. Finally, we run several placebo tests for different focal points and other possible divisions of Germany.

6.1 Alternative Samples and Specifications

To ascertain our results, we run several robustness tests on the definition of the *East* dummy, the choice of the sample, specifications and period. The related Tables and Figures are in the Appendix.

First, we change the definition of the *East* dummy. The measure used previously required that both spouses lived in the former GDR or the former FRG before 1989. We now relax this assumption and show how the results vary depending on whether it is the wife or the husband who originates from either region. We find similar results using only the information about the husband or the wife (Table 1.E3). We also consider the case of mixed-couples. Although it is not possible to perform similar regressions on this sample because of its limited size, we display descriptive statistics in Table 1.E1 and show that they appear to behave in between purely Western and Eastern couples. Moreover, we replicate all of our main results using an alternative, purely geographic, definition of the East-West divide, i.e. classifying couples according to whether they currently live in a Land of the former GDR or of the former FRG at the time of the survey. Descriptive statistics are in Table 1.E4 and the results are presented in Tables 1.E5 and 1.E6. They are similar to those obtained using the biographic definition (i.e. whether couples lived in East or West Germany before reunification).

Second, we run the same regressions on each of our three outcomes (housework hours, divorce and labor market participation) using alternative samples and specifications. We enlarge the sample to include single-earner couples and, alternatively, unmarried couples (Table 1.E7): the results are identical. We also check the robustness of our results to the exclusion of outliers (Table 1.E8). We use alternative specifications without controlling for individual incomes or household income, including quadratic terms in wages and introducing hourly wages (Table 1.E9 for housework, Table 1.E10 for divorce and Table 1.E11 for labor market participation). The results remain essentially unchanged. We also use larger clusters to compute the standard errors. In our setting, part of the treatment is at the regional level but clustering at this level would lead to a number of clusters that is arguably too small (only two). We could also cluster at smaller geographical units but doing so would fail to capture the serial-correlation for households who moved across the territory. For this reason, we chose to cluster at the household level (which coincides with

the individual level) in the main specification and repeat our main analysis with the relevant alternative options (district and Land level). The results do not change (see Tables 1.E12 and 1.E13).

Third, as market work and housework decisions can be made simultaneously, we check that our results are robust even for couples whose market work time does not vary. This is important, as it provides supporting evidence that our findings are not driven by a reallocation of time between paid-work and housework time, but instead by a variation in hourly wages. To do so, we replicate the main analysis related to housework and restrict the sample to couples whose female absolute and relative paid-work time remains constant or varies marginally. The results are displayed in Figure 1.E1 and Figure 1.E2. They show that the main results on housework remain similar. This supports an interpretation in terms of shock, where the variation in income is due to a change in hourly wages.

Finally, we drop the first years of reunification one by one, progressively, and re-run our analysis of female housework. This is because these years may have been experienced as exceptionally chaotic and uncertain, thus triggering certain behavior that is not generalizable. We display the results on housework as this is the outcome with the highest number of observations and statistical power. Table 1.E14 shows that the results are unchanged even once these years have been dropped.

6.2 Alternative Mechanisms

6.2.1 Historical Differences in Unobservables: Evidence from the East/West Border

We have established that the differences between the Eastern and Western parts of Germany were not larger than the average interregional differences in 1933, 1886 and 1849 but there may remain concerns about differences in unobservables characteristics related to gender norms. If there had been some regional differences before 1949, following an East-West axis, it is likely that the legacy of these historical differences in unobservables would be weaker in the vicinity of the East/West border. Therefore, we would expect more homogenous behavior as we move closer to the border.

To enquire, we exploit geolocated data and examine the behaviors of couples according to which side of the East/West border they currently live. For each household, we calculate the relative distance from its district to the East-West border.¹⁷ Figure 1.5 below

¹⁷The geographical unit refers to the so-called *Kreis*. This corresponds to a level of administration that is intermediate between the German States (Länder) and the municipalities. There are 404 districts in the

displays the wife's contribution to household income at different points of space across the East-West border. We see that the wife's contribution to household finance is sharply discontinuous at the border, i.e. ten kilometers East and West from the former political border. More generally, the relative share of female income slightly increases as one moves from the Western part of Germany to the Eastern border, following parallel trends. This spatial discontinuity within an otherwise homogenous region is confirmed by the regressions displayed in Table 1.E15. Even within a distance of 50 kilometers, the wife's share of income is 10 percentage points higher in couples who live at the East of the former border, than in couples who live on the West side (the difference is statistically significant at 1%). This asymmetry in the structure of household income is not due to the fact that Eastern couples are poorer, hence need to rely more on female income. Indeed, the coefficient barely changes after controlling for household income in addition to spouses' age, age squared, a dummy controlling for the presence of children and education level (Model C of Table 1.E15).

We also replicate the main results related to housework around the East/West border (we focus on housework because it is the most robust result and the least likely to be undermined by a smaller sample size). The results are available in Figure 1.E3 and Table 1.E16. Although the precision of the estimates is weaker closer to the border, we see that the coefficients remain essentially stable in magnitude across the territory, whereas historical and persisting differences in unobservables are expected to grow as we move away from the East/West border. This suggests that our findings are not driven by historical differences in unobservables but rather by the lasting consequences of the division of Germany.

6.2.2 Is the Difference Due to the Wage Structure?

Although Eastern and Western Länder are now part of the same country, they still differ in several dimensions and in particular regarding the wage structure. We explore the role of three factors: lower household income, lower male hourly wage and lower wage dispersion in East Germany. These three factors could potentially explain our findings. For instance, in a context of low household income, individuals may not engage in *doing gender* behaviors because they simply cannot afford to divorce, to spend more time on housework or to withdraw from the labor market. Also, in a context of low male hourly wage, women could have a higher bargaining power within the household which could explain the absence of *doing gender* behaviors. Similarly, in a context of low dispersion of

database.

wages, the focal point of 50% could lose its meaningfulness.

To rule out these alternative explanations, we investigate the possible relationship between the wage structure and *doing gender* behaviors. We exploit the heterogeneity within West Germany (culturally homogeneous) and contrast the behavior of couples whose household income is high (similarly where male hourly wages and the dispersion of wages at the Land level are high) to couples whose household income is low (similarly where male hourly wages and the dispersion of wages at the Land level are low). Provided that there is sufficient heterogeneity within West Germany, this exercise will indicate whether our results may be due to these three confounding factors.

To quantify the level of heterogeneity, we use two types of measures. First, regional measures based on the average level of household income, male hourly wage and wage dispersion within a Land (macro definition). Second, individual measures based on the relative position of a household in West Germany in terms of household income and male hourly wages (micro definition). For each of these measures, we classify couples in two groups according to their position relative to the median value of the measure.

Descriptive statistics about the heterogeneity within West Germany are described in Table 1.E17 for the household income level, Table 1.E19 for the male hourly wage level and Table 1.E21 for the dispersion of wages. Considering these three factors, we see that East German couples are often close to West German couples who stand below the median.

For each of these factors, we observe similar *doing gender* behaviors for households above or below the median. Women keep increasing their number of housework hours when they earn more than their husbands in West Germany, whether they are in a low or a high income household (Table 1.E18). Similarly, the level of male hourly wages does not seem to alter the results (Table 1.E20). Finally, measuring the dispersion of income using a Gini coefficient, we observe similar *doing gender* behaviors in Länder where the dispersion is below or above the West German median (Tables 1.E22, 1.E23, 1.E24). This suggests that the East-West difference in household behavior is not due to the different structure of earnings in these regions.

6.2.3 Other Institutional Differences

Some of the East/West differences could be due to current objective structural disparities that might exist across German Länder. For example, unemployment is more pervasive in the East. It is also likely that the eastern part of Germany is still better equipped with childcare facilities than the western part, which could obviously influence women's work

behavior. We extend the arguments developed in the previous section to focus on areas where Eastern and Western households live together.

The Case of Migrants. We first focus on Western Länder and contrast the behavior of West German "natives" to that of East German "immigrants" (i.e. those who lived in East Germany before 1990). In order to rule out the possibility that movers, as such, have idiosyncratic features that happen to be more gender-equal, we distinguish couples who always lived in the same Western Land (since they entered the survey) from those who moved from one Land to another within West Germany. Figure 1.6 displays the entire distribution of female relative income. In each of the three groups, the distribution has two hikes. Among non-mover Western couples, the principal mode is around 20%, and we can see a minor one around 45% for "native" Western couples. But for former Eastern couples, the pattern is reversed. Finally, for movers from one Western Land to another, the two modes are approximately of equal importance. Hence, among couples who currently live in the West, those who immigrated from the East after 1990 still follow a much more gender-equal script. We also provide descriptive evidence in Table 1.E25 about the characteristics of these couples. Considering the gender gap in housework hours and relative income, Eastern couples seem to be more gender equal than couples who have moved from one Western Land to another or than non-mover Western couples.

Focusing on Berlin. Finally, to rule out the possibility that persisting differences in the availability of childcare institutions explain the observed differences in terms of household behavior, we profit from a survey that was run in Berlin in 2011 (see Dolan *et al.*, 2016). We use this additional database because it offers the unique feature of surveying a large sample of individuals located in a single city, with a distinction between dwellers in West Berlin and East Berlin. This gives us the possibility of observing people who live in a small, economically and spatially integrated and homogenous area. The survey asked respondents (all living in Berlin) where they used to live before 1990, and, if they were too young, where their mother used to live before 1990. People can choose between the following answers: East Berlin (within the former GDR), West Berlin (within the former FRG), GDR excluding Berlin, FRG excluding Berlin, other country part of the socialist bloc, or other country. The survey includes over 6,000 respondents, of whom 3,147 women, with 1,666 living in West Berlin and 1,481 living in East Berlin. We look at women's participation in the labor market, arguably the first outcome affected by the availability of childcare institutions, according to whether they (or their mother) lived in the FRG or the

GDR during the division. Figure 1.7 displays the main results. It turns out that the effect of having lived in the East is of similar magnitude whether women currently live in East or West Berlin as of 2011: the proportion of women working part-time or full-time is about 12 percentage points higher, i.e. approximately one third, among women who used to live in the East before 1990 as compared to women who used to live in the West. The results displayed in Figure 1.7 hold in Probit estimates that control for age, age squared and the presence of children under 16 in the household, and these differences hold only for women and not for men. Hence, even within a small geographic area, where childcare facilities are identically accessible to every household, the attachment to work is higher for women with an "East German" culture. In Figure 1.E4, we also show that a similar pattern holds among young women.

6.3 Placebo Tests

6.3.1 Other Possible Focal Points

We have followed the literature in testing for the influence of the focal point of equal earnings between spouses. But could the threshold be lower or higher? To enquire, we run our preferred specification, following Equation 13, with respectively the number of female housework hours, the risk of divorce and the probability of being out of the labor force as independent variables, and we successively replace the dummy *WifeEarnsMore* by a dummy which equals 1 if the income of the wife is greater than 10%, 30%, 50%, 70% or 90%, and 0 otherwise. The coefficient on the dummy variable starts being statistically significant only when the share of the wife's income reaches 50%. This is true for the estimates of female housework hours (Table 1.E27), marital instability (Table 1.E28) and labor market participation (Table 1.E29). The 50% threshold is also the only point at which the results are significant for the three studied outcomes. Beyond this point, most of the coefficients turn statistically insignificant because many of the couples who are *doing gender* are on the other side of the cut-off point and because of the low number of observations.

6.3.2 Other Possible Divisions of Germany

The paper's main claim is that the difference between East and West Germany stems from the different institutions that prevailed during the division. In order to challenge this interpretation, we run a systematic placebo exercise, which consists in testing the relevance of all of the possible divisions of the 15 German regions (excluding Berlin) into two groups of respectively 5 (Group 1) and 10 (Group 2) regions. This mimics the division of Germany

into the GDR (5 Länder) and the FRG (10 Länder), excluding Berlin (by definition, this exercise uses a geographical definition of the East dummy variable). We run our preferred specification, following Equation 13 (with the number of female housework hours as the independent variable, as it provides the highest statistical power) and look at whether our coefficients of interest, i.e. those associated with the dummy *WifeEarnsMore*, and the interaction of *WifeEarnsMore* * *Group1*, are statistically significant, and at which level.

It turns out that out of 3,003 combinations, there are only 101 cases, i.e. 3.36% cases, where both coefficients are statistically significant at the 1% level. Table 1.E30 displays a synthetic analysis of the results, i.e. estimates of the probability that both coefficients of interest are statistically significant, according to the composition of Group 1 and Group 2, i.e. to how many Eastern Länder are included in Group 1. Column 1 displays the probability that the coefficients of interest are significant at the 10% level; column 2 at the 5% level and column 3 at the 1% level. The table shows that as more Eastern Länder are included in Group 1, the coefficients become more statistically significant. The difference between Eastern and Western Länder thus does not seem to be hiding another more relevant divide.

7 Direct Evidence on the East/West Divide in Gender Preferences

We interpret the contrasted behavior of German couples, according to their origin, as the mark left by four decades of socialism on gender identity norms. In order to sustain this interpretation, we present some evidence of self-stated preferences and attitudes. This complements the information produced by Bauernschuster and Rainer (2012) and Beblo and Gorges (2018), who used the German General Social Survey (ALLBUS). Table 1.7 presents the marginal effect of the following equation:

$$Preference_{it} = \gamma_1 East_i + \gamma_2 Female_i + \gamma_3 Female_i * East_i + \beta X_{it} + \epsilon_{it} \quad (2)$$

where the preferences of individual *i* in year *t* depend on her gender and whether she has lived in East Germany or not before 1989, controlling for the usual socio-demographic variables (household income, age, age squared, presence of children in the household, education of both spouses, year and Land fixed-effects).

As expected, column 1 of Table 1.7 shows that the probability of considering work as

being important is 18.9 percentage points lower for a West German woman than for a West German man, but only 4.8 percentage points ($-0.189 + 0.141$) lower for an East German woman than for an East German man. Other outcomes, such as the importance of success at work (column 2) and a successful career (column 3) follow the same pattern. For these three columns, the negative sign of γ_2 and the positive sign of γ_3 suggest that the gender gap in work-related preferences is narrower in East Germany than in West Germany.

One may wonder whether in East Germany, work values have crowded out family values, but this is not the case. Columns 4 and 5 respectively display estimates of the self-reported importance of marriage and family. The coefficients of interests are not statistically significant for column 4 and positive for column 5, which suggests that there was no shift in preferences concerning marriage or family in East Germany.

Additionally, in Figure 1.F6 and Table 1.F1, we investigate cohort patterns and find supporting evidence that the gender gap in preferences is particularly small among cohorts who lived in the GDR and were aged between 20 to 40 at the time of reunification. For older and younger cohorts, who were less exposed to socialist institutions, the gender gap seems closer to what is observed among West Germans.

8 Conclusions

During the four decades of the socialist episode in East Germany, institutions provided strong incentives for women's participation in the labor market, which, in turn, instilled more equal gender norms concerning the division of tasks between spouses. We show that these institutions created a culture that has undone the male breadwinner norm and its consequences. In particular, East German women can earn more than their husband without having to increase their number of housework hours, put their marriage at risk or withdraw from the labor market. By contrast the traditional norm of higher male earnings, and its consequences, are still prevalent in West Germany. These attitudes are mirrored by subjective preferences, as East German women attach almost as much value to paid work as men, contrarily to West German women.

We showed that these results are directly attributable to the cultural legacy created by the different institutions that prevailed during the division. To rule out alternative explanations, we first showed that the East-West differences did not preexist before the 1949 division, using first-hand statistical data pertaining to 1933, and exploiting Prussian data from the 19th century. We also provided evidence of the different behavior of women currently living in Berlin, according to which side of the iron curtain they used to live before

reunification, and showed a similar pattern among East German couples who migrated to West Germany. Additionally, we replicated our results in a small geographical area surrounding the East/West border in order to rule out the role of historical differences in unobservables. We also analyzed the potential role of wage structure differentials and ran placebo exercises of alternative divides. Finally, we established that it is the focal point of equal incomes that triggers *doing gender* behaviors, as opposed to any other cut-off point.

Admittedly, this paper uses a reduced-form model in order to distinguish the cultural drivers of the East-West difference from other potential structural factors, such as local labor markets, marriage markets, income distribution, infrastructure, or other contextual features. The aforementioned robustness tests attempt to rule out the role of such alternative mechanisms. However, they do not measure the respective weight of these different factors. Future research using structural models of household behavior could fill that gap.

From a public policy perspective, one can wonder which of the many institutional differences that opposed the East and the West had the largest influence on gender norms. It would be difficult not to recognize that the root of the change was the full-time employment norm that became prevalent in the East during the Socialist era. This, in turn, was made possible by the work-family balance policy that was implemented in East Germany. In Western countries, the male breadwinner norm is likely to progressively vanish with the current extension of female education and labor market participation. How much time it will take is an open question but what this paper shows is that a large part of the answer is in the hands of policy-makers.

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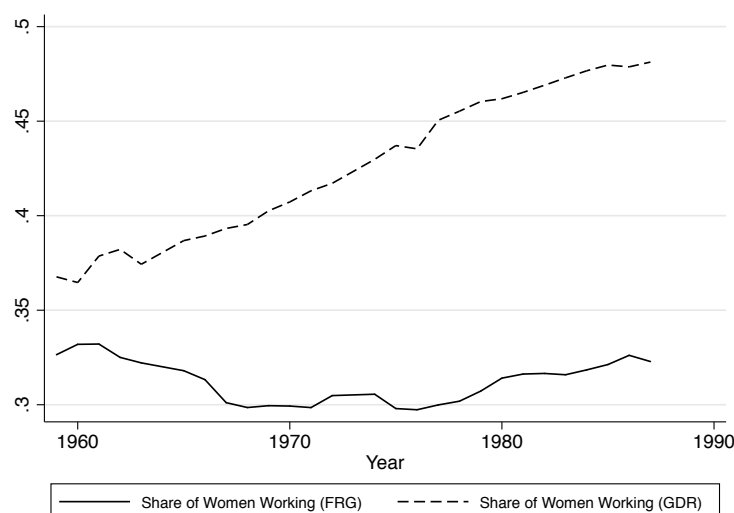
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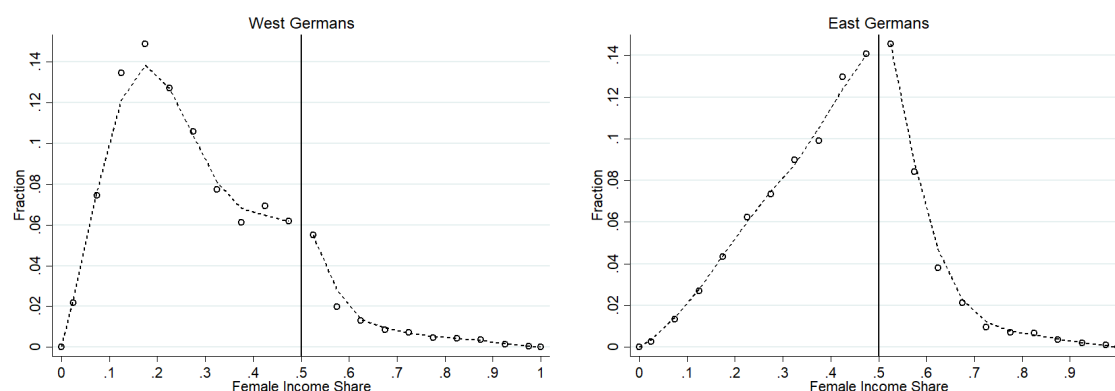
9 Figures

Figure 1.1: Evolution of Women's Participation in the Labor Market



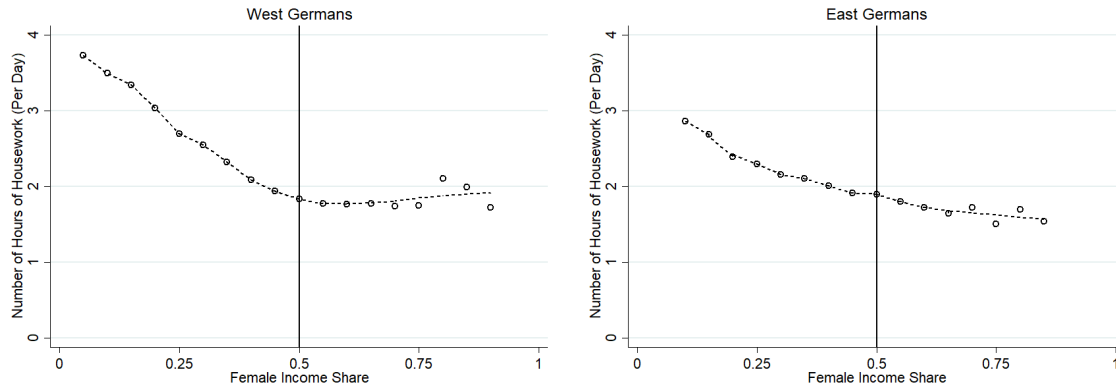
Notes: Authors' computation using the statistical yearbooks of FRG and GDR from 1959 to 1987. The shares of working women are computed by dividing the number of working women by the total number of women in the population. We do not report data for years prior to 1959 because self-employed starts being included in the number of workers from 1959 only, making longer time series inconsistent.

Figure 1.2: Female Income Share among Western and Eastern German Couples



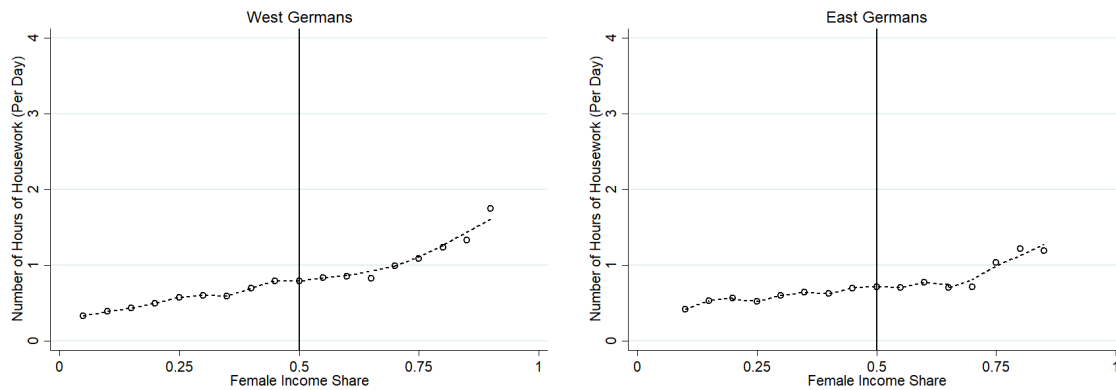
Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Sample: dual-earner married couples between 18 and 65 years of age. Female Income Share is defined as $\text{Female Income} / (\text{Female Income} + \text{Male Income})$. The vertical black line corresponds to Female Income Share = 0.5. Each dot represents the fraction of couples in a 0.05 relative income bin. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989.

Figure 1.3: Women's Housework Time Depending on their Share of Income



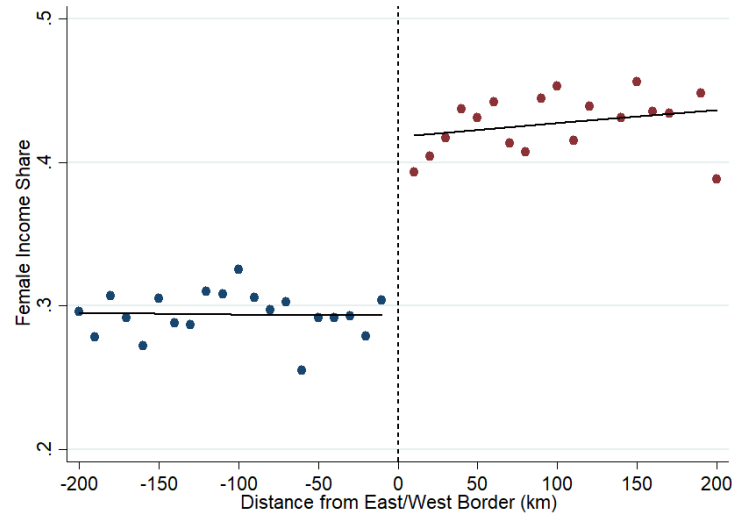
Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Sample: dual-earner married couples between 18 and 65 years of age. Female Income Share is defined as $\text{Female Income} / (\text{Female Income} + \text{Male Income})$. Each dot represents at least 50 observations in a 0.05 relative income bin (99.5% of the total sample). The vertical black line corresponds to Female Income Share = 0.5. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989.

Figure 1.4: Male Housework Time Depending on Female's Share of Income



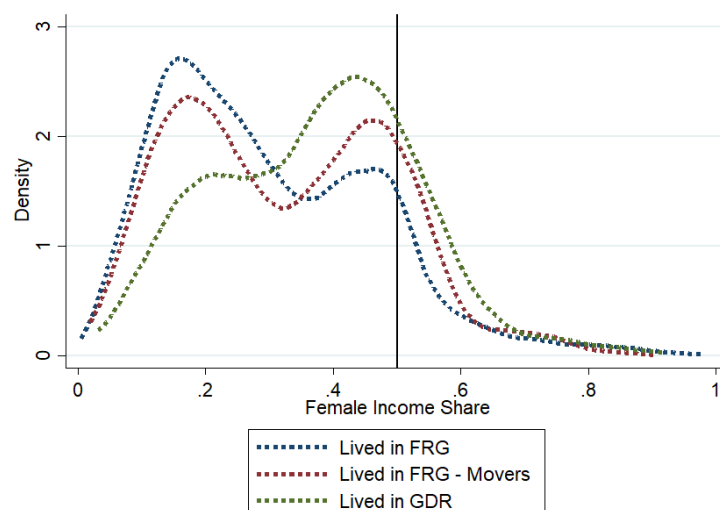
Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Sample: dual-earner married couples between 18 and 65 years of age. Female Income Share is defined as $\text{Female Income} / (\text{Female Income} + \text{Male Income})$. Each dot represents at least 50 observations in a 0.05 relative income bin (99.5% of the total sample). The vertical black line corresponds to Female Income Share = 0.5. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989.

Figure 1.5: Female Share in Household Income Depending on the Distance to the East/West Border



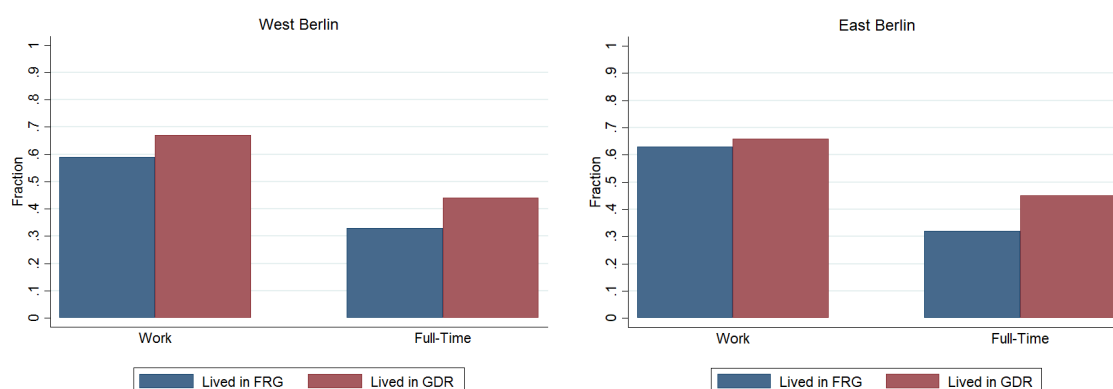
Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Sample: dual-earner married couples between 18 and 65 years of age without movers from East to West (or the opposite). The relative distance is defined as the euclidean distance in kilometers from the centroid of the district to the East/West border. A positive (negative) distance means that the couple is geographically located in the East (West). The vertical dashed line corresponds to the border. Each dot represents the average value of the female income share in total household income, in a 10 km bin.

Figure 1.6: Distribution of Female Relative Income in Western Länder Depending on Couples' Former Location



Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Sample: dual-earner married couples between 18 and 65 years of age living in Western Länder. Female Income Share is defined as $\text{Female Income} / (\text{Female Income} + \text{Male Income})$. The vertical black line corresponds to Female Income Share = 0.5. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989.

Figure 1.7: Female Participation in the Labor Market in Berlin



Notes: The left graph is restricted to women living in West Berlin. The graph on the right is restricted to women living in East Berlin.

10 Tables

Table 1.1: East and West Germany in 1933. Descriptive Statistics

	<i>East</i>	<i>West</i>	$ East - West $	Average Regional Differences	P-value
	(1)	(2)	(3)	(4)	(5)
Industry and Handcraft %	44.64	40.08	4.55	4.69 (2.92)	0.47
Retail and Transport %	16.32	16.65	0.33	2.05 (1.77)	0.89
Agriculture %	16.01	21.21	5.20	5.79 (3.66)	0.51
Services%	9.10	9.60	0.51	0.49 (0.46)	0.36
Free occ. / Self-employed %	13.95	12.98	0.97	0.66 (0.49)	0.25
Female share of employees** %	35.92	33.12	2.80	2.94 (1.91)	0.43
Female share of high school students%	31.61	34.87	3.26	2.32 (1.57)	0.28
Marriages per 1000 inhabitants	9.87	9.45	0.42	0.52 (0.34)	0.57
Births per 1000 inhabitants	13.02	14.97	1.95	1.02 (0.78)	0.15
Population (in Millions)	11.43	35.44			

Notes: Own calculations based on Statistisches Reichsamt (1936:27, 37, 1935:297) for 1933. We use the regions of the former German Empire that coincide with the later boundaries of GDR and FRG, excluding Berlin. East: Anhalt, Mecklenburg, Prov. Sachsen, Sachsen, Thüringen. West: Baden, Bayern, Bremen, Hamburg, Hessen, Hohenzollerische Lande, Lippe, Lübeck, Oldenburg, Prov. Hannover, Prov. Hessen-Nassau, Prov. Westfalen, Rheinprovinz, Schaumburg-Lippe, Württemberg. ** For these statistics, the divide is based on the State Employment Office Districts, i.e. for GDR: Mitteldeutschland, Sachsen, and for FRG: Bayern, Hessen, Niedersachsen, Rheinland, Sudwestdeutschland, Westfalen. Column (4) displays the average absolute value of differences between regions in all possible regional partitions of 20 regions into 5 + 15, as well as the standard error of these averages in parenthesis. Column (5) displays the probability that these differences are higher than the East/West difference displayed in column (3), which corresponds to the share of these differences that is higher than the East/West difference. In row 6 (female share of employees), the calculation was made on the basis on the 8 regions available in official statistics (instead of 20). In row 7 (female share of high school students), data are missing for Schaumburg-Lippe, and the resulting number of available Länder is 19.

Table 1.2: Descriptive Statistics of the East/West Samples

	<i>West Germans</i>				<i>East Germans</i>			
	Mean	S.D.	Min	Max	Mean	S.D.	Min	Max
Relative Income	0.29	0.17	0	1	0.42	0.15	0	1
WifeEarnsMore	0.11	0.31	0	1	0.29	0.45	0	1
Woman's Housework Time	2.61	1.50	0	20	1.98	1.09	0	12
Man's Housework Time	0.61	0.72	0	16	0.68	0.73	0	10
Hswk Woman - Hswk Man	1.99	1.73	-15	19	1.30	1.32	-9	12
Paid Work Time Woman	27.03	13.29	1	80	37.72	10.40	1	80
Paid Work Time Man	44.60	9.81	1	80	45.70	9.77	1	80
Income HH (Euros)	3679.14	2405.09	10	200000	2660.94	1234.34	102	16259
Income Woman (Euros)	973.45	835.55	2	30170	1056.71	640.34	17	15000
Income Man (Euros)	2456.87	1703.53	46	99999	1477.82	913.59	25	15000
Hourly Wage Woman	8.62	5.43	0	129	6.79	4.11	0	96
Hourly Wage Man	13.14	8.62	0	392	7.84	4.99	0	138
Woman's Age	43.64	8.46	20	65	42.90	8.49	19	65
Man's Age	46.26	8.73	22	65	45.23	8.68	21	65
Kids in HH (1=YES)	0.68	0.47	0	1	0.72	0.45	0	1
Observations	22091				12114			

Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Descriptive statistics are based on the main sample: married couples with positive income. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989. Income measures are based on 2010 constant euros. Number of housework or paid-work hours per day.

Table 1.3: Housework Time and Relative Income

	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
<i>Panel A: Women</i> <i>Dep Var: Housework Time (hours per day)</i>						
WifeEarnsMore	0.10** (0.05)	-0.02 (0.04)	0.12** (0.05)	0.17*** (0.04)	0.01 (0.03)	0.18*** (0.04)
WifeEarnsMore*East			-0.14** (0.06)			-0.18*** (0.05)
East			-0.67*** (0.09)			
Observations	22091	12114	34205	22091	12114	34205
<i>Panel B: Men</i> <i>Dep Var: Housework Time (hours per day)</i>						
WifeEarnsMore	-0.07** (0.03)	-0.04 (0.03)	-0.08** (0.03)	-0.04 (0.03)	-0.01 (0.02)	-0.04 (0.03)
WifeEarnsMore*East			0.04 (0.04)			0.03 (0.04)
East			0.17*** (0.06)			
Observations	22091	12114	34205	22091	12114	34205
<i>Panel C: Couple</i> <i>Dep Var: Housework Time Gap (Woman's - Man's)</i>						
WifeEarnsMore	0.17*** (0.06)	0.03 (0.05)	0.19*** (0.06)	0.21*** (0.05)	0.02 (0.04)	0.23*** (0.05)
WifeEarnsMore*East			-0.18** (0.08)			-0.21*** (0.06)
East			-0.84*** (0.11)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4128	1976	6104	4128	1976	6104
Observations	22091	12114	34205	22091	12114	34205

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

Table 1.4: Risk of Divorce and Relative Income

<i>Dependent variable: Divorced within a 5-year time horizon (1=Yes)</i>						
	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
WifeEarnsMore	0.01 (0.02)	0.00 (0.01)	0.01 (0.02)	0.03** (0.02)	-0.01* (0.01)	0.03** (0.02)
WifeEarnsMore*East			-0.01 (0.02)			-0.05*** (0.02)
East			-0.01 (0.03)			
Couple fixed-effects	No	No	No	Yes	Yes	Yes
Couples	1897	882	2779	1897	882	2779
Observations	9037	4906	13943	9037	4906	13943

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The dependent variable is a dummy that equals 1 if the couple has divorced within a 5-year time horizon. The sample contains only dual earner married couples. Standard errors clustered at the couple level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: log of household income, man and woman's log of individual income, man and woman's age and age squared, man and woman's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and couple fixed-effects (columns 4,5 and 6).

Table 1.5: Labor Market Participation and Relative Income

	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
<i>Panel A: Women</i>	<i>Dep. Var.: Out of the labor market in T+1 (1=Yes)</i>					
WifeEarnsMore	0.02*** (0.00)	0.00 (0.00)	0.02*** (0.00)	0.01* (0.00)	-0.00 (0.00)	0.01* (0.00)
WifeEarnsMore*East			-0.02*** (0.00)			-0.01 (0.01)
East			-0.03*** (0.01)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	3255	1510	4765	3255	1510	4765
Observations	22159	12115	34274	22159	12115	34274
<i>Panel B: Men</i>	<i>Dep. Var.: Out of the labor market in T+1 (1=Yes)</i>					
HusbandEarnsMore	-0.01** (0.00)	-0.00 (0.00)	-0.01* (0.00)	-0.01* (0.00)	-0.00 (0.00)	-0.01* (0.00)
HusbandEarnsMore*East			0.01 (0.00)			0.01 (0.01)
East			-0.01* (0.01)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	3255	1510	4765	3255	1510	4765
Observations	22159	12115	34274	22159	12115	34274

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample is restricted to dual earner married couples in T. Standard errors clustered at the individual level are given in parentheses. The dependent variable is a dummy that equals 1 if the individual is not in the labor market in T+1 (1 year horizon). East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls include relative income, log of household income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

Table 1.6: Labor Market Participation and Potential Relative Income

	<i>Women</i>			<i>Men</i>		
	<i>Out of the labor force in T</i>			<i>Out of the labor force in T</i>		
	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
WifeEarnsMore Potentially	0.03** (0.01)	-0.03*** (0.01)	0.02* (0.01)			
WifeEarnsMore Potentially*East			-0.05*** (0.02)			
ManEarnsMore Potentially				-0.04*** (0.02)	-0.01 (0.01)	-0.04*** (0.02)
ManEarnsMore Potentially*East						0.04** (0.02)
East			-0.14** (0.05)			-0.11** (0.05)
Observations	35848	14290	50138	32104	14647	43783

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls include log of husband income, log of household income, respondent and partners' age and age squared, respondent and partner's education level along with year fixed-effects and Land fixed-effects.

Table 1.7: Attitudes to Work of East versus West Germans

<i>Dependent Variable: How Important is ... for your satisfaction ?</i>					
	(1) Work	(2) Success At Work	(3) A Successful Career	(4) Marriage	(5) Family
East	0.128*** (0.019)	0.026** (0.01)	0.028* (0.016)	0.007 (0.011)	0.012 (0.011)
Female	-0.189*** (0.015)	-0.121*** (0.008)	-0.144*** (0.014)	0.037*** (0.008)	0.032*** (0.008)
Female*East	0.141*** (0.02)	0.106*** (0.015)	0.098*** (0.019)	-0.0148 (0.012)	0.023** (0.011)
Observations	11147	20362	11098	20611	11271

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Probit marginal effects. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: log of household income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, a dummy representing whether the woman is working, a dummy representing whether the man is working and their interaction with the East dummy, year fixed-effects and Land fixed-effects.

11 Appendix

A How to Think about "Doing Gender" in the Framework of a Structural Model?

Consider that couples (or husbands) have a preference for situations where the man earns more than his spouse ($W_f < W_m$, where W_f is the female wage and W_m the male wage). Respecting this norm creates a positive psychological value for the marriage (a positive contribution to welfare), by sustaining gender identity; conversely, violating the norm generates a negative value. This preference for gender identity, in turn, produces a certain type of "traditional" allocation of tasks within the household.

In this paper, we study the consequences of an increase in the relative wage of the wife that leads to a violation of the norm, i.e. to a situation where $W_f > W_m$. Let us first consider that the rise in the relative wage of the wife is due to a shock. We will then examine the case where this is the outcome of a decision.

Whether in a unitary or a collective model, the transgression of the norm may reduce the value of the marriage. In this case the wife can restore the degree of gender identity of the couple by accomplishing a greater share of housework, i.e. shifting the division of tasks toward a more traditional arrangement. In the case of a collective model à la Chiappori (1992), if the violation of the norm only hurts the husband, because spouses have different preferences, this may decrease the value of the marriage for the husband, hence reducing the bargaining power of the wife. In this case, the increase in the number of housework hours on behalf of the wife can be interpreted as a transfer of utility to the husband (due to a shift in distribution factors). Naturally, a shock that decreases the value of the marriage, in the eyes of the husband or both spouses, automatically increases the likelihood of divorce (Weiss and Willis, 1997). Finally, if a shock brings the wife's potential earnings higher than her husband's, and this is threatening the gender identity of the couple, hence the strength of the marriage, she (or they) might choose not to take the risk, hence to let the wife withdraw from the labor market.

Now, in case the rise in the female relative income is not due to a shock, it is possible to rationalize our findings as the outcomes of a joint decision. First, the couple can "decide" ex ante to let the wage of the wife rise (by working more hours or accepting a wage rise or a promotion) as long as her number of housework increases too, in order to maintain the degree of gender intensity in the couple. They can also agree, ex ante, on the non-participation of the wife to the labor market, because of the potential violation of the norm that it would create and the associated loss of value for the marriage. Finally, spouses may also be shortsighted or may wrongly expect to be able to transfer utility, or to restore the gender identity of the couple, which leads them to take labor market decisions that fragilize their couple.

We interpret our results as the outcome of a shock. This is because our main findings are robust to the inclusion of individual fixed-effects and also because they are essentially

unchanged when we restrict the sample to couples where female absolute and relative paid work-time remain constant or vary marginally. This supports an interpretation in terms of shock, where the variation in income is due to a change in hourly wages.

B Pre-Division Differences in the 19th Century

Table 1.B1 provides descriptive statistics on differences between East and West Germany in the 19th century. To compute this table, we have used Prussian data collected by Becker *et al.* (2014). We are able to replicate, to some extent, the same calculations as in Table 1.1 about the employment structure. Unfortunately, there is little data on gender differences except the share of girls enrolled in primary schooling.

Table 1.B1: East and West Germany in the 19th Century. Descriptive Statistics

	<i>East</i>	<i>West</i>	$ East - West $	Average Regional Differences	P-value
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: 1882/1886</i>					
Industry %	0.383	0.38	0.003	0.04 (0.03)	0.95
Agriculture %	0.497	0.484	0.013	0.04 (0.03)	0.82
Retail and Transport %	0.054	0.046	0.008	0.006 (0.004)	0.28
Services %	0.08	0.078	0.002	0.008 (0.006)	0.89
Share of Girls in Primary School %	0.501	0.497	0.002	0.002 (0.002)	0.11
<i>Panel B: 1849</i>					
Industry %	0.114	0.138	0.024	0.044 (0.031)	0.67
Agriculture %	0.845	0.831	0.014	0.046 (0.033)	0.82
Services %	.04	.03	.01	0.007 (0.004)	0.23
Share of Girls in Primary School %	0.492	0.488	0.004	0.003 (0.002)	0.32

Notes: Own calculations based on Prussian data sets (collected by Becker *et al.*, 2014). Panel A is restricted to data from 1882 and 1886. Panel B is restricted to data from 1849. We include only Prussian districts that will be part of FRG or GDR, excluding Berlin. The divide is based on German districts (*Regierungsbezirk*). In 1849, East: Erfurt, Frankfurt, Magdeburg, Merseburg, Potsdam, Stettin and Stralsund. West: Aachen, Arnsberg, Dusseldörf, Koblenz, Köln, Minden, Munster and Trier. In 1882/1886, additional data is available for the following Western districts: Aurich, Hannover, Hildesheim, Kassel, Lüneburg, Osnabruck, Schleswig, Sigmaringen, Stade and Wiesbaden. Column (4) displays the average absolute value of differences between regions in all possible regional partitions, as well as the standard error of these averages in parenthesis. Column (5) displays the probability that these differences are higher than the East/West difference displayed in column (3), which corresponds to the share of these differences that is higher than the East/West difference.

C Additional Descriptive Statistics

C.1 Marriage Instability Sample

Table 1.C1: Descriptive Statistics of the East/West Samples for the Analysis of Divorce

	<i>West Germany</i>				<i>East Germany</i>			
	Mean	S.D.	Min	Max	Mean	S.D.	Min	Max
Divorce at 5 Years	0.09	0.29	0	1	0.07	0.25	0	1
Relative Income	0.28	0.16	0	1	0.43	0.14	0	1
WifeEarnsMore (1=Yes)	0.09	0.29	0	1	0.31	0.46	0	1
Woman's Age	42.17	7.27	21	63	41.47	7.24	21	60
Man's Age	44.75	7.51	22	63	43.69	7.39	23	61
Household Income	3514.32	1896.05	400	55000	2645.10	1123.42	460	16000
Income Woman	905.55	715.13	2	15000	1075.80	597.69	37	9715
Income Man	2379.90	1865.43	75	99999	1452.16	811.59	102	13000
Kids in HH (1=Yes)	0.74	0.44	0	1	0.81	0.39	0	1
Observations	9054				4919			

Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Descriptive statistics are based on the main sample: married couples with positive income. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989.

C.2 Labor Market Participation Sample

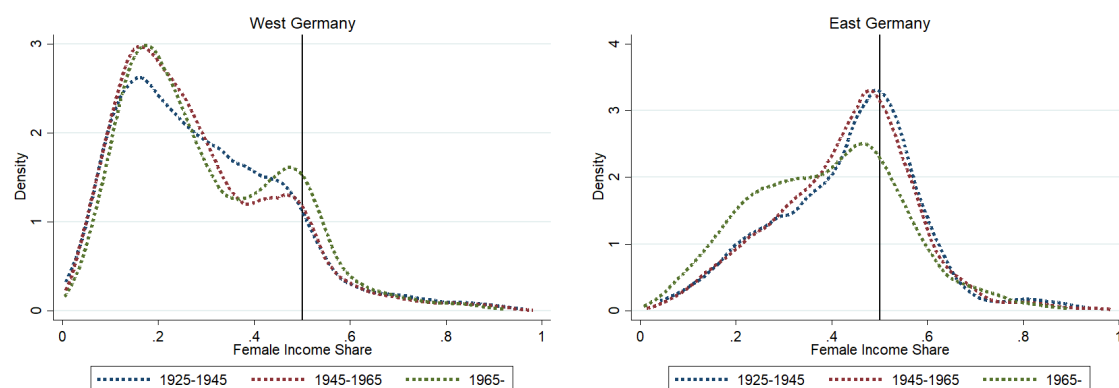
Table 1.C2: Descriptive statistics of the East/West Samples for the Analysis of Labor Market Participation

	<i>West Germany</i>				<i>East Germany</i>			
	Mean	S.D.	Min	Max	Mean	S.D.	Min	Max
WomanOutLaborMarket T+1	0.02	0.14	0	1	0.01	0.08	0	1
Woman's Share of Income	0.28	0.16	0	1	0.42	0.15	0	1
WifeEarnsMore	0.10	0.30	0	1	0.29	0.45	0	1
Paid Work Time Woman	26.81	13.22	1	80	37.75	10.35	1	80
Paid Work Time Man	44.69	9.74	1	80	45.88	9.74	2	80
Income HH (Euros)	3680.29	2350.14	358	200000	2691.83	1215.62	409	16259
Income Woman (Euros)	970.00	831.21	2	30170	1067.38	626.38	17	15000
Income Man (Euros)	2462.87	1692.49	50	99999	1499.65	906.88	25	15000
Hourly Wage Woman	8.61	5.62	0	199	6.80	4.05	0	96
Hourly Wage Man	13.14	8.68	0	392	7.89	5.01	0	157
Woman's Age	43.55	8.39	20	65	42.94	8.41	19	65
Man's Age	46.17	8.67	22	65	45.27	8.60	21	65
Kids in HH (1=YES)	0.68	0.47	0	1	0.72	0.45	0	1
Observations	21047				11560			

Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Descriptive statistics are based on the main sample: married couples with positive income in T and information on T and T+1. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989. Number of housework or paid-work hours per day.

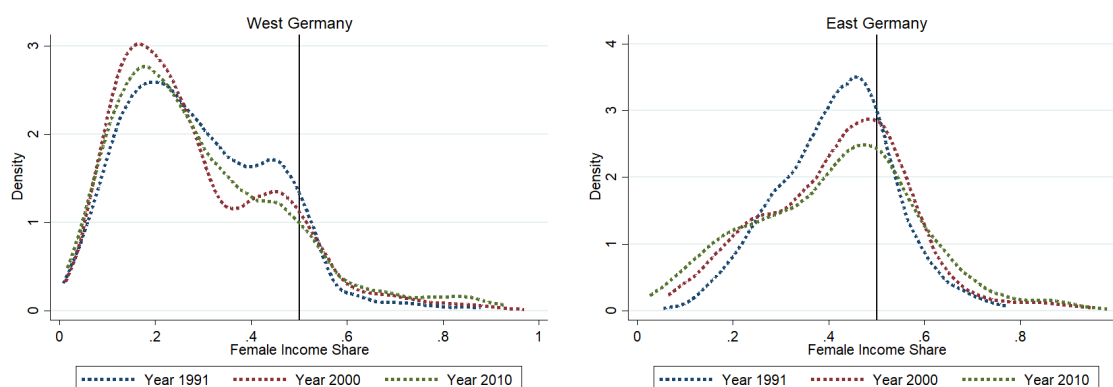
C.3 Relative Income in East and West Germany

Figure 1.C1: Female Income Share in West and East Germany per Cohort



Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Generations are based on the year of birth. Sample: dual-earner married couples between 18 and 65 years of age. Female Income Share is defined as $\text{Female Income} / (\text{Female Income} + \text{Male Income})$. The vertical black line corresponds to Female Income Share = 0.5. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989.

Figure 1.C2: Female Income Share in West and East Germany in Different Years



Notes: The data comes from the German Socio-Economic Panel waves in 1991, 2000 and 2010. Sample: dual-earner married couples between 18 and 65 years of age. Female Income Share is defined as $\text{Female Income} / (\text{Female Income} + \text{Male Income})$. The vertical black line corresponds to Female Income Share = 0.5. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989.

D Additional Results

D.1 Housework - Adding Childcare

Table 1.D1: Woman's Housework+Childcare Time and Relative Income. Adding Childcare

<i>Dependent variable: Woman's Housework+Childcare Time (hours per day)</i>						
	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
WifeEarnsMore	0.54*** (0.16)	0.12 (0.11)	0.64*** (0.16)	0.53*** (0.14)	0.18** (0.08)	0.62*** (0.14)
WifeEarnsMore*East			-0.53*** (0.19)			-0.44*** (0.16)
East			-2.08*** (0.29)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4082	1958	6040	4082	1958	6040
Observations	21292	11592	32884	21292	11592	32884

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls include relative income between spouses, log of household income, log of woman's income and log of man's income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

D.2 Housework - Analyzing the Share of Housework

This section considers alternative outcome measures for housework. Although we could use the share of female housework instead of the time gap, we believe that the latter is more suited to an econometric analysis. We hereafter explain why.

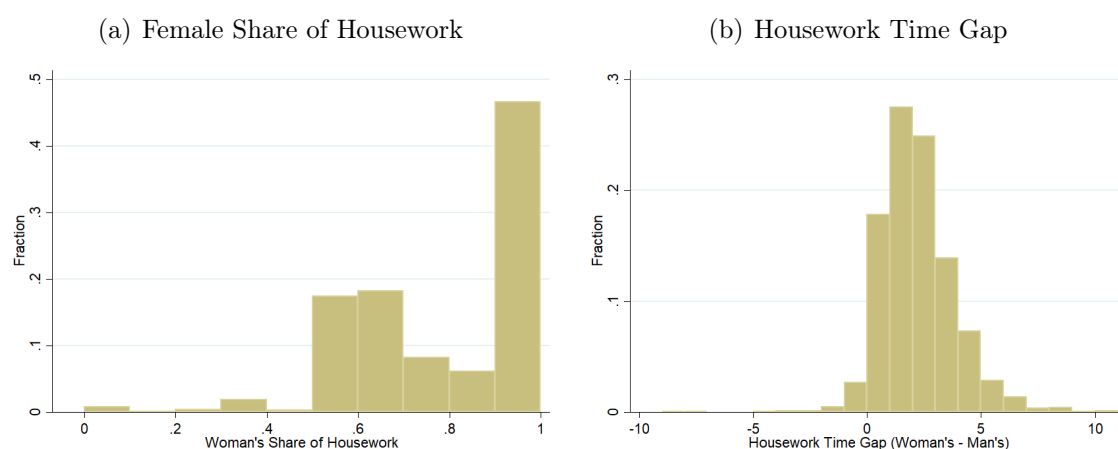
In Figure 1.D1, we plot the distribution of the female share of housework and of the time gap. We see that the distribution of the female share of housework is highly skewed towards 1. This is because the time spent on housework is measured using the following question: "*What is a typical weekday like for you? How many hours per normal workday do you spend on housework (washing, cooking, cleaning)?*". Answers are given in the form of integers 0,1,2,... and as such rounded to zero if the time is closer to zero than from 1.

In Table 1.D2, we display the results using the share of housework. Dependent variables are: for Panel A the share of female housework measured as a continuous variable, and for Panel B a dummy that equals 1 if the female share is equal to 100%. The two panels deliver results that are similar to those obtained using the time gap measure, although they are less robust and less precisely estimated. In particular, columns 4 to 6 show that when a woman earns more than her husband, her share of housework time increases by about 2 p.p. in the West (column 4) but not in the East (column 5), but the interaction in column 6 is not statistically significant at conventional thresholds (the coefficient associated to *WifeEarnsMore*East* has a p-value equals to 0.105 in Panel A and to 0.185 in Panel B). Yet, these results are less precisely estimated, in part because of the skewness of the distribution of this new outcome.

Another potential factor explaining the results based on the time gap measure is the weight of outliers. Using the time gap necessarily gives more weight to the outliers, which could thus drive the results. To alleviate this concern, Table 1.D3 runs the main specification excluding outliers. The results are robust to excluding progressively the top and bottom 1% (column 1), 5% (column 2) and 10% (column 3).

Additionally, using the time gap could give more weight to households who spend a greater total number of housework hours because as this total increases, the time gap is likely to increase too. In Table 1.D4, we replicate the main results controlling for the total number of housework hours. We see that the cross-sectional estimates are slightly less precise (column 1 to 3) but that the fixed-effects specifications remain essentially unchanged.

Figure 1.D1: Distribution of Housework Measures



Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Sample: dual-earner married couples between 18 and 65 years of age. Figure (a) represents the distribution of the female share of housework ($\text{Female Housework Time} / (\text{Female} + \text{Male Housework Time})$). Figure (b) represents the distribution of the housework time gap between spouses ($\text{Female Housework Time} - \text{Male Housework Time}$).

Table 1.D2: Housework Time and Relative Income. Analyzing the Share of Housework Time

	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
<i>Panel A: Continuous Measure</i>	<i>Dep Var: Woman's Share of Housework</i>					
WifeEarnsMore	0.02 (0.01)	0.00 (0.01)	0.02 (0.01)	0.02** (0.01)	-0.00 (0.01)	0.02** (0.01)
WifeEarnsMore*East			-0.01 (0.01)			-0.02 (0.01)
East			-0.07*** (0.02)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4132	1969	6101	4132	1969	6101
Observations	21992	12024	34016	21992	12024	34016
<i>Panel B: Dummy</i>	<i>Dep Var: Woman's Share of Housework = 100% (1=Yes)</i>					
WifeEarnsMore	0.02 (0.01)	0.00 (0.01)	0.02 (0.01)	0.02** (0.01)	-0.00 (0.01)	0.02** (0.01)
WifeEarnsMore*East			-0.01 (0.01)			-0.02 (0.01)
East			-0.07*** (0.02)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4132	1969	6101	4132	1969	6101
Observations	21992	12024	34016	21992	12024	34016

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

Table 1.D3: Housework Time and Relative Income. Removing Outliers

<i>Dep Var: Housework Time Gap (Woman's - Man's)</i>			
	(1)	(2)	(3)
WifeEarnsMore	0.21*** (0.05)	0.17*** (0.05)	0.16*** (0.05)
WifeEarnsMore*East	-0.20*** (0.06)	-0.15** (0.06)	-0.11* (0.06)
Sample Restriction	1%	5%	10%
Individual fixed-effects	Yes	Yes	Yes
Individuals	6020	5537	5464
Observations	32680	26123	25131

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

Table 1.D4: Housework Time and Relative Income. Controlling for Total Housework Hours.

	<i>Dep Var: Housework Time Gap (Woman's - Man's)</i>					
	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
WifeEarnsMore	0.16*** (0.06)	0.05 (0.05)	0.17*** (0.06)	0.15*** (0.05)	0.02 (0.04)	0.16*** (0.05)
WifeEarnsMore*East			-0.13* (0.07)			-0.14** (0.06)
East			-0.60*** (0.10)			
Total Household Housework Hours	0.55*** (0.02)	0.31*** (0.02)	0.48*** (0.01)	0.51*** (0.02)	0.27*** (0.02)	0.44*** (0.02)
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4128	1976	6104	4128	1976	6104
Observations	22091	12114	34205	22091	12114	34205

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: relative income between spouses, log of household income, total housework hours, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

D.3 Housework - Controlling for Paid Work Time

Table 1.D5: Housework Time and Relative Income - Controlling for Paid-Work Time and Including Log Individual Incomes

<i>Dependent variable: Woman's Housework Time (hours per day)</i>						
	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
<i>Panel A: Women Dep Var: Housework Time (hours per day)</i>						
WifeEarnsMore	0.03 (0.05)	-0.05 (0.04)	0.05 (0.05)	0.13*** (0.04)	-0.01 (0.03)	0.15*** (0.04)
WifeEarnsMore*East			-0.11* (0.06)			-0.18*** (0.05)
East			-0.47*** (0.10)			
Observations	20836	11469	32305	20836	11469	32305
<i>Panel B: Men Dep Var: Housework Time (hours per day)</i>						
WifeEarnsMore	-0.04 (0.03)	-0.03 (0.03)	-0.05 (0.03)	-0.04 (0.03)	-0.00 (0.03)	-0.05 (0.03)
WifeEarnsMore*East			0.03 (0.04)			0.05 (0.04)
East			0.15*** (0.05)			
Observations	20836	11469	32305	20836	11469	32305
<i>Panel C: Couple Dep Var: Housework Time Gap (Woman's - Man's)</i>						
WifeEarnsMore	0.07 (0.06)	-0.02 (0.05)	0.10* (0.06)	0.17*** (0.05)	-0.01 (0.04)	0.20*** (0.05)
WifeEarnsMore*East			-0.15* (0.08)			-0.23*** (0.06)
East			-0.62*** (0.11)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4043	1902	5945	4043	1902	5945
Observations	20836	11469	32305	20836	11469	32305

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

Table 1.D6: Housework Time and Relative Income - Controlling for Paid-Work Time and Excluding Log Individual Incomes

<i>Dependent variable: Woman's Housework Time (hours per day)</i>						
	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
<i>Panel A: Women Dep Var: Housework Time (hours per day)</i>						
WifeEarnsMore	0.25*** (0.05)	0.00 (0.04)	0.26*** (0.05)	0.20*** (0.04)	0.01 (0.03)	0.22*** (0.04)
WifeEarnsMore*East			-0.27*** (0.06)			-0.22*** (0.05)
East			-0.56*** (0.10)			
Observations	20836	11469	32305	20836	11469	32305
<i>Panel B: Men Dep Var: Housework Time (hours per day)</i>						
WifeEarnsMore	-0.02 (0.03)	-0.01 (0.03)	-0.02 (0.03)	-0.03 (0.03)	0.00 (0.02)	-0.03 (0.03)
WifeEarnsMore*East			0.01 (0.04)			0.04 (0.04)
East			0.14** (0.05)			
Observations	20836	11469	32305	20836	11469	32305
<i>Panel C: Couple Dep Var: Housework Time Gap (Woman's - Man's)</i>						
WifeEarnsMore	0.27*** (0.05)	0.01 (0.05)	0.29*** (0.05)	0.24*** (0.05)	0.01 (0.04)	0.25*** (0.05)
WifeEarnsMore*East			-0.28*** (0.08)			-0.26*** (0.06)
East			-0.69*** (0.11)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4043	1902	5945	4043	1902	5945
Observations	20836	11469	32305	20836	11469	32305

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: relative income between spouses, log of household income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

D.4 Marriage Instability - Additional Results

Table 1.D7: Relative Income and the Risk of Divorce at Different Horizons

<i>Dependent variable: Divorced at horizon</i>					
	1 year	2 years	3 years	4 years	5 years
	(1)	(2)	(3)	(4)	(5)
WifeEarnsMore	0.00 (0.01)	0.01 (0.01)	0.02* (0.01)	0.02** (0.01)	0.03** (0.02)
WifeEarnsMore*East	0.00 (0.01)	-0.01 (0.01)	-0.02 (0.01)	-0.03** (0.01)	-0.05*** (0.02)
Probability of Divorce	0.01	0.03	0.05	0.07	0.08
Couple fixed-effects	Yes	Yes	Yes	Yes	Yes
Couples	4409	3867	3413	3032	2782
Observations	21551	18681	16414	14567	13973

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the couple level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: log of household income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and couple fixed-effects (columns 4,5 and 6).

D.5 Labor Market Participation - Additional Results

Table 1.D8: Labor Market Participation and Potential Relative Income of Women - Including Marital Status in Heckman Selection

	<i>Women</i>			<i>Men</i>		
	<i>Out of the labor force in T</i>			<i>Out of the labor force in T</i>		
	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
WifeEarnsMore Potentially	0.05*** (0.01)	-0.02** (0.01)	0.04*** (0.01)			
WifeEarnsMore Potentially*East			-0.06*** (0.02)			
ManEarnsMore Potentially				-0.04*** (0.02)	-0.01 (0.01)	-0.04*** (0.02)
ManEarnsMore Potentially*East						0.03** (0.02)
East			-0.14** (0.05)			-0.11** (0.05)
Observations	35848	14290	50138	32104	14647	43783

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Standard errors clustered at the individual level are given in parentheses. Controls include log of husband income, log of household income, respondent and partners' age and age squared, respondent and partner's education level along with year fixed-effects and Land fixed-effects.

E Robustness

E.1 Alternative Definitions of East vs West Couples- Does it Matter which Spouse is from the East/West?

In this section, we consider whether our results are sensitive to the origin of the husband and the wife before 1989. In Table 1.E1, we first display descriptive statistics on "purely West", "purely East" and mixed couples. We see that mixed couples are somehow in between the two other types of couples. For instance, in 18% of mixed couples, the wife earns more than her husband whereas this figure is respectively of 10% and 29% for purely West and purely East couples. We observe a similar pattern for the other variables related to gender norms such as relative income, housework time or paid work time. Looking only at mixed couples in Table 1.E2, we observe little difference depending on whether it is the wife or the husband who used to live in the GDR. Overall, relative income and the probability that WifeEarnsMore seem slightly higher in couples where the wife comes from the East. Yet, the small number of observations makes it difficult to draw definitive conclusions.

In Table 1.E3, we replicate the main analysis, using female housework time as the outcome. We use this specification because it appears to be the most precisely estimated on the full sample and consequently the most likely to present interesting heterogeneous pattern. We perform this analysis for three groups: "pure couples", only husbands from the East/West and only wives from the East/West. We cannot run this analysis directly on mixed couples because of the small number of observations (726 for mixed couples against respectively 12,114 and 22,091 for Eastern and Western couples). We do not observe different patterns in Panel A ("pure" couples), Panel B (only husbands from East/West) or Panel C (only wives from East/West). In the three types of couples, we see that when a wife earns more than her husband, she increases the time she spends on housework when she lives in the West but not in the East.

Table 1.E1: Descriptive Statistics of Couples Depending on Where they Lived Before 1989.

	<i>Both Lived only in FRG</i>		<i>Both Lived only in GDR</i>		<i>Mixed Couple</i>	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Woman From East	0.00	0.00	1.00	0.00	0.72	0.45
Man From East	0.00	0.00	1.00	0.00	0.28	0.45
Relative Income	0.29	0.17	0.42	0.15	0.35	0.19
WifeEarnsMore	0.11	0.31	0.29	0.45	0.18	0.38
Woman's Housework Time	2.61	1.50	1.98	1.09	2.09	1.22
Man's Housework Time	0.61	0.72	0.68	0.73	0.75	0.98
Hswk Woman - Hswk Man	1.99	1.73	1.30	1.32	1.34	1.61
Actual Weekly Work Time	27.03	13.29	37.72	10.40	31.59	13.40
Actual Weekly Work Time	44.60	9.81	45.70	9.77	43.64	9.98
Income HH	3679.14	2405.09	2660.94	1234.34	3216.06	1210.66
Income Woman	973.45	835.55	1056.71	640.34	1067.98	739.85
Income Man	2456.87	1703.53	1477.82	913.59	2016.15	975.10
Hourly Wage Woman	8.62	5.43	6.79	4.11	8.16	4.59
Hourly Wage Man	13.14	8.62	7.84	4.99	11.32	5.75
Woman's Age	43.64	8.46	42.90	8.49	38.86	8.62
Man's Age	46.26	8.73	45.23	8.68	42.83	8.52
Kids in HH (1=YES)	0.68	0.47	0.72	0.45	0.62	0.49
Observations	22091		12114		726	

Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Descriptive statistics are based on the main sample: married couples with positive income. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989. Number of housework or paid-work hours per day.

Table 1.E2: Descriptive Statistics of Mixed Couples Depending on Who is from the East.

	<i>Woman from East</i>		<i>Man from East</i>	
	Mean	S.D.	Mean	S.D.
Woman From East	1.00	0.00	0.00	0.00
Man From East	0.00	0.00	1.00	0.00
Relative Income	0.35	0.19	0.34	0.18
WifeEarnsMore	0.18	0.39	0.17	0.38
Woman's Housework Time	2.11	1.28	2.04	1.06
Man's Housework Time	0.73	0.95	0.79	1.07
Hswk Woman - Hswk Man	1.38	1.64	1.25	1.54
Actual Weekly Work Time	32.04	13.33	30.43	13.56
Actual Weekly Work Time	43.19	10.45	44.82	8.57
Income HH	3158.21	1174.68	3368.22	1291.27
Income Woman	1054.47	757.74	1103.54	691.14
Income Man	1958.54	909.68	2167.66	1117.61
Hourly Wage Woman	7.81	4.35	9.06	5.07
Hourly Wage Man	11.26	5.85	11.49	5.51
Woman's Age	39.05	8.37	38.38	9.24
Man's Age	43.33	8.60	41.49	8.17
Kids in HH (1=YES)	0.61	0.49	0.64	0.48
Observations	526		200	

Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Descriptive statistics are based on the main sample: married couples with positive income. Eastern (Western) couples are those in which both spouses lived in GDR (FRG) before 1989. Number of housework or paid-work hours per day.

Table 1.E3: Housework Time and Relative Income Depending on Whether Husband or Wife Has Lived in GDR or FRG

<i>Dependent variable: Woman's Housework Time (hours per day)</i>						
	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
<i>Panel A: East = 1 if Both from East</i>						
WifeEarnsMore	0.10** (0.05)	-0.02 (0.04)	0.12** (0.05)	0.17*** (0.04)	0.01 (0.03)	0.18*** (0.04)
WifeEarnsMore*East			-0.14** (0.06)			-0.18*** (0.05)
East			-0.67*** (0.09)			
Observations	22091	12114	34205	22091	12114	34205
<i>Panel B: East = 1 if Man from East</i>						
WifeEarnsMore	0.11** (0.05)	-0.01 (0.04)	0.13*** (0.05)	0.18*** (0.04)	0.01 (0.03)	0.19*** (0.04)
WifeEarnsMore*East			-0.15** (0.06)			-0.19*** (0.05)
East			-0.64*** (0.09)			
Observations	22617	12314	34931	22617	12314	34931
<i>Panel C: East = 1 if Woman from East</i>						
WifeEarnsMore	0.11** (0.05)	-0.01 (0.04)	0.13*** (0.05)	0.18*** (0.04)	0.01 (0.03)	0.19*** (0.04)
WifeEarnsMore*East			-0.15** (0.06)			-0.19*** (0.05)
East			-0.64*** (0.09)			
Observations	22617	12314	34931	22617	12314	34931

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses (Panel A), the man (Panel B) or the woman (Panel C) lived in the former GDR before 1989. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

E.2 Replication of Results Based on the Geographical Divide Between East and West Länder

This section replicates the results with a definition of *East* based on the geographical divide between Eastern and Western Länder. Here, the dummy *East* equals 1 if the individual/couple currently lives in the former regions of the GDR (and 0 otherwise).

Table 1.E4 provides descriptive statistics of the sample. Similarly to the sample used in the "biographic" definition, we see that women living in East Germany earn a higher share of the household income (42% vs 15%) and are more likely to earn more than their husband (30% vs 11%). These households are also poorer on average and of similar age.

Table 1.E5 displays the results obtained with the geographical definition. They are very similar to those obtained with the biographical definition. When a woman earns more than her husband, she is more likely to increase the time spent on housework (Panel A), the couple is more likely to divorce (Panel B) and she is more likely to withdraw from the labor market (Panel C), in the West but not in the East. In column 6, using individual fixed-effects specification, we see that the coefficient *WifeEarnsMore* and *WifeEarnsMore * East* always offset each other.

Table 1.E4: Descriptive Statistics of the East/West Samples: Geographical Divide

	<i>West Germany</i>				<i>East Germany</i>			
	Mean	S.D.	Min	Max	Mean	S.D.	Min	Max
Woman's Share of Income	0.29	0.17	0	1	0.42	0.15	0	1
WifeEarnsMore	0.11	0.31	0	1	0.30	0.46	0	1
Woman's Housework Time	2.58	1.48	0	14	1.99	1.08	0	12
Man's Housework Time	0.62	0.73	0	16	0.66	0.73	0	10
Hswk Woman - Hswk Man	1.96	1.72	-15	13	1.33	1.32	-9	12
Paid Work Time Woman	27.28	13.26	1	80	38.01	10.25	1	80
Paid Work Time Man	44.56	9.80	1	80	45.79	9.83	1	80
Income HH (Euros)	3608.42	2336.48	10	200000	2653.50	1284.20	102	16259
Income Woman (Euros)	964.69	808.57	2	30170	1055.89	658.23	17	15000
Income Man (Euros)	2403.01	1656.72	46	99999	1461.53	941.68	25	20452
Hourly Wage Woman	8.49	5.31	0	129	6.74	4.18	0	96
Hourly Wage Man	12.87	8.41	0	392	7.74	5.11	0	138
Woman's Age	43.13	8.64	20	65	42.97	8.49	19	65
Man's Age	45.77	8.87	20	65	45.29	8.64	22	65
Kids in HH (1=YES)	0.67	0.47	0	1	0.73	0.45	0	1
Observations	23662				10897			

Notes: The data comes from the German Socio-Economic Panel, using all the waves from 1991 until 2012. Descriptive statistics are based on the main sample: married couples with positive income. People are in the East (resp. West) sample if they currently live in Länder that used to be part of the former GDR (resp. FRG) and have the German nationality.

Table 1.E5: The Consequences of Violating the Male Breadwinner norm in Eastern versus Western Länder. Geographical Divide

	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
<i>Panel A</i>	<i>Dep. Var.: Female Housework Time (Hours per day)</i>					
WifeEarnsMore	0.14*** (0.05)	-0.04 (0.04)	0.16*** (0.05)	0.17*** (0.04)	0.02 (0.03)	0.19*** (0.04)
WifeEarnsMore*EastGeo			-0.19*** (0.06)			-0.18*** (0.05)
EastGeo			-0.90*** (0.10)			-0.49 (0.32)
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4642	1888	6479	4642	1888	6479
Observations	23662	10897	34559	23662	10897	34559
<i>Panel B</i>	<i>Dep. Var.: Divorced within a 5-year time horizon (1=Yes)</i>					
WifeEarnsMore	0.02 (0.02)	0.02* (0.01)	0.02 (0.02)	0.03** (0.01)	-0.01 (0.01)	0.03** (0.01)
WifeEarnsMore*EastGeo			0.01 (0.02)			-0.03** (0.01)
EastGeo			-0.03 (0.02)			
Couple fixed-effects	No	No	No	Yes	Yes	Yes
Couples	1798	676	2460	1798	676	2460
Observations	9946	4219	14165	9946	4219	14165
<i>Panel C</i>	<i>Dep. Var.: Woman out of the labor market in T+1 (1=Yes)</i>					
WifeEarnsMore	0.02*** (0.00)	0.00 (0.00)	0.02*** (0.00)	0.01** (0.00)	-0.00 (0.00)	0.01** (0.00)
WifeEarnsMore*EastGeo			-0.02*** (0.00)			-0.01** (0.01)
EastGeo			-0.03*** (0.01)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4132	1647	5728	4132	1647	5728
Observations	24574	11259	35833	24574	11259	35833

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 (resp. 0) if the individual currently lives in a Länder that used to be part of the former GDR (resp. FRG) and have the German nationality. Controls include log of household income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (column 4,5 and 6). Panel A and C further control for relative income and Panel A and B for respondent and partner's log of individual income.

Table 1.E6: Attitudes to Work in Eastern Versus Western Länder. Geographical Divide

<i>Dependent Variable: How Important is ... for your satisfaction ?</i>					
	(1) Work	(2) Success at Work	(3) A Successful Career	(4) Marriage	(5) Family
EastGeo	0.106*** (0.018)	0.026** (0.011)	0.026* (0.015)	0.009 (0.011)	0.012 (0.010)
Female	-0.178*** (0.014)	-0.112*** (0.008)	-0.123*** (0.012)	0.038*** (0.007)	0.038*** (0.002)
Female*EastGeo	0.115*** (0.019)	0.085*** (0.014)	0.061*** (0.018)	-0.014 (0.012)	0.017* (0.010)
Observations	12,762	22,906	12,710	23,185	12,896

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 (resp. 0) if the individual currently lives in Länder that used to be part of the former GDR (resp. FRG) and has the German nationality. Controls: respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, a dummy representing whether the woman is working, a dummy representing whether the man is working and their interaction with the East (Geographical) dummy, year fixed-effects and Land fixed-effects.

E.3 Alternative Samples - Unmarried and Single-Earner Couples

Table 1.E7: Consequences of Violating the Male Breadwinner Norm. Alternative Samples

	(1)	(2)	(3)	(4)
<i>Dep Var: Woman's Housework Time (hours per day)</i>				
WifeEarnsMore	0.18*** (0.04)	0.14*** (0.04)	0.18*** (0.04)	0.16*** (0.04)
WifeEarnsMore*East	-0.18*** (0.05)	-0.15*** (0.05)	-0.18*** (0.05)	-0.17*** (0.05)
Marital Status	Only Married	Only Married	All couples	All couples
Only Dual Earner	Yes	No	Yes	No
Individual fixed-effects	Yes	Yes	Yes	Yes
Individuals	6104	7490	7228	8660
Observations	34205	44834	40226	51233
<i>Dep Var: Divorced within a 5-year time horizon (1=Yes)</i>				
WifeEarnsMore	0.03** (0.02)	0.04** (0.02)		
WifeEarnsMore*East	-0.05*** (0.02)	-0.05*** (0.02)		
Marital Status	Only Married	Only Married		
Only Dual Earner	Yes	No		
Individual fixed-effects	Yes	Yes		
Individuals	2782	3284		
Observations	13973	16221		
<i>Dep Var: Woman's Out of the Labor Market T+1 (1=Yes)</i>				
WifeEarnsMore	0.01* (0.00)	0.01 (0.01)	0.01** (0.00)	0.01** (0.00)
WifeEarnsMore*East	-0.01 (0.01)	-0.00 (0.01)	-0.01* (0.00)	-0.01 (0.01)
Marital Status	Only Married	Only Married	All couples	All couples
Only Dual Earner	Yes	No	Yes	No
Individual fixed-effects	Yes	Yes	Yes	Yes
Individuals	4765	5507	5335	6073
Observations	34274	44793	39722	50562

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Income controls include log of household income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, a dummy controlling for whether only the woman is working and a dummy controlling for whether only the man is working, both alone and interacted with East, year fixed-effects, Land fixed-effects and individual fixed-effects. Panel A and C further control for relative income and Panel A and B for respondent and partner's log of individual income.

Table 1.E8: Consequences of Violating the Male Breadwinner Norm. Alternative Samples - Sensitivity to Excluding Outliers

	(1)	(2)	(3)
<i>Dep Var: Woman's Housework Time (hours per day)</i>			
WifeEarnsMore	0.17*** (0.04)	0.19*** (0.04)	0.17*** (0.04)
WifeEarnsMore*East	-0.15*** (0.05)	-0.19*** (0.05)	-0.16*** (0.05)
Outlier's Excluded Sex	Woman	Man	Both
Individual fixed-effects	Yes	Yes	Yes
Individuals	6063	6048	6008
Observations	33604	33584	33029
<i>Dep Var: Divorced within a 5-year time horizon (1=Yes)</i>			
WifeEarnsMore	0.03** (0.02)	0.03* (0.02)	0.03** (0.02)
WifeEarnsMore*East	-0.05*** (0.02)	-0.05*** (0.02)	-0.05*** (0.02)
Outlier's Excluded Sex	Woman	Man	Both
Individual fixed-effects	Yes	Yes	Yes
Individuals	2759	2761	2739
Observations	13766	13754	13567
<i>Dep Var: Woman's Out of the Labor Market T+1 (1=Yes)</i>			
WifeEarnsMore	0.01* (0.00)	0.01* (0.00)	0.01* (0.00)
WifeEarnsMore*East	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Outlier's Excluded Sex	Woman	Man	Both
Individual fixed-effects	Yes	Yes	Yes
Individuals	4746	4750	4730
Observations	33664	33669	33107

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample excludes couples where the woman (column 1), the man (column 2) and at least one of them (column 3) has an hourly wage in the top or the bottom 1% of the distribution. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Income controls include log of household income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, a dummy controlling for whether only the woman is working and a dummy controlling for whether only the man is working, both alone and interacted with East, year fixed-effects, Land fixed-effects and individual fixed-effects. Panel A and C further control for relative income and Panel A and B for respondent and partner's log of individual income.

E.4 Alternative Specifications - Housework

Table 1.E9: Woman's Housework Time and Relative Income. Alternative Specifications

	<i>Dependent variable: Woman's Housework Time (hours per day)</i>						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
WifeEarnsMore	0.32*** (0.04)	0.32*** (0.04)	0.18*** (0.04)	0.18*** (0.04)	0.19*** (0.04)	0.32*** (0.04)	0.33*** (0.04)
WifeEarnsMore*East	-0.26*** (0.05)	-0.26*** (0.05)	-0.17*** (0.05)	-0.18*** (0.05)	-0.18*** (0.05)	-0.27*** (0.05)	-0.28*** (0.05)
LogIncomeHH	-0.24*** (0.03)	-0.47 (0.29)		-0.03 (0.04)	-0.04 (0.04)	-0.26*** (0.04)	-0.28*** (0.04)
LogIncomeHH_sq		0.01 (0.02)					
LogIncomeW			-0.31*** (0.07)	-0.36*** (0.07)	-0.61** (0.25)		
LogIncomeM			-0.16** (0.08)	-0.12 (0.08)	-0.42* (0.24)		
LogIncomeW_sq					0.02 (0.02)		
LogIncomeM_sq					0.02 (0.02)		
HourlyWageW						0.02*** (0.00)	0.05*** (0.01)
HourlyWageM						-0.00*** (0.00)	-0.01*** (0.00)
HourlyWageW_sq							-0.00*** (0.00)
HourlyWageM_sq							0.00* (0.00)
Individual fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individuals	6104	6104	6274	6104	6104	5945	5945
Observations	34205	34205	36020	34205	34205	32305	32305

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls include respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

E.5 Alternative Specifications - Marriage Instability

Table 1.E10: Risk of Divorce and Relative Income. Alternative Specifications

	<i>Dependent variable: Divorced within a 5-year time horizon (1=Yes)</i>						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
WifeEarnsMore	0.04*** (0.01)	0.04*** (0.01)	0.03** (0.02)	0.03** (0.02)	0.03* (0.02)	0.04** (0.02)	0.04** (0.02)
WifeEarnsMore*East	-0.05*** (0.02)	-0.05*** (0.02)	-0.05*** (0.02)	-0.05*** (0.02)	-0.04** (0.02)	-0.05*** (0.02)	-0.05*** (0.02)
LogIncomeHH	-0.02* (0.01)	-0.22** (0.09)		-0.02** (0.01)	-0.02*** (0.01)	-0.01 (0.01)	-0.01 (0.01)
LogIncomeHH_sq		0.01** (0.01)					
LogIncomeW			0.01 (0.01)	0.02* (0.01)	-0.05 (0.06)		
LogIncomeM			-0.01 (0.01)	-0.00 (0.01)	-0.06 (0.06)		
LogIncomeW_sq					0.01 (0.00)		
LogIncomeM_sq					0.00 (0.00)		
HourlyWageW						0.00 (0.00)	0.00 (0.00)
HourlyWageM						0.00 (0.00)	0.00 (0.00)
HourlyWageW_sq							-0.00 (0.00)
HourlyWageM_sq							-0.00 (0.00)
Individual fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individuals	2782	2782	2782	2782	2782	2727	2727
Observations	13973	13973	13973	13973	13973	13215	13215

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the couple level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls include respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and couple fixed-effects.

E.6 Alternative Specifications - Labor Force Participation

Table 1.E11: Labor Force Participation and Relative Income. Alternative Specifications

	<i>Dep Var: Woman out of the labor force in T+1</i>				
	(1)	(2)	(3)	(4)	(5)
WifeEarnsMore	0.01* (0.00)	0.01* (0.00)	0.01** (0.00)	0.01* (0.00)	0.01* (0.00)
WifeEarnsMore*East	-0.01 (0.01)	-0.01 (0.01)	-0.01* (0.01)	-0.01 (0.01)	-0.01 (0.01)
LogIncomeHH	-0.01*** (0.00)	-0.02 (0.03)		-0.01*** (0.00)	-0.01*** (0.00)
LogIncomeHH_sq		0.00 (0.00)			
HourlyWageW			0.00** (0.00)	0.00* (0.00)	-0.00 (0.00)
HourlyWageM			-0.00*** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)
HourlyWageW_sq					0.00 (0.00)
HourlyWageM_sq					0.00** (0.00)
Individual fixed-effects	Yes	Yes	Yes	Yes	Yes
Individuals	5566	5566	5587	5459	5459
Observations	35688	35688	35306	33589	33589

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls include respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

E.7 Alternative Clusters

The main results display estimations which cluster standard errors at the household level. This section tests the robustness of the main results by using larger clusters at the regional level. Table 1.E12 displays the results obtained by clustering the standard errors at the Land level ($n=16$). Given the small number of Länder, we also present results obtained by clustering the standard errors at the district level ($n=404$) in Table 1.E13. In both tables, the precision of the estimation is generally similar to the results obtained by clustering standard errors at the household level. In Table 1.E13, which is our preferred estimation given the number of clusters available, we actually obtain smaller standard errors for the outcomes related to housework and labor market participation. Only for divorce, the results are slightly less precise with a p-value of 0.069 for the coefficient related to *WifeEarnsMore* in column 3.

Table 1.E12: Consequences of Violating the Male Breadwinner Norm. Clustering at the Land Level

	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
<i>Panel A</i>	<i>Dep. Var.: Female Housework Time (Hours per day)</i>					
WifeEarnsMore	0.10** (0.04)	-0.02 (0.03)	0.12*** (0.04)	0.17*** (0.05)	0.01 (0.02)	0.18*** (0.06)
WifeEarnsMore*East			-0.14*** (0.04)			-0.18*** (0.05)
East			-0.67*** (0.07)			
Individual fixed-effects Individuals	No	No	No	Yes 4128	Yes 1976	Yes 6104
Observations	22091	12114	34205	22091	12114	34205
<i>Panel B</i>	<i>Dep. Var.: Divorced within a 5-year time horizon (1=Yes)</i>					
WifeEarnsMore	0.01 (0.02)	0.00 (0.01)	0.02 (0.02)	0.03* (0.02)	-0.01** (0.01)	0.03* (0.02)
WifeEarnsMore*East			-0.01 (0.03)			-0.05** (0.02)
East			-0.01 (0.04)			
Couple fixed-effects Couples	No	No	No	Yes 1900	Yes 882	Yes 2782
Observations	9054	4919	13973	9054	4919	13973
<i>Panel C</i>	<i>Dep. Var.: Woman Out of the labor market in T+1 (1=Yes)</i>					
WifeEarnsMore	0.02*** (0.00)	0.00 (0.00)	0.02*** (0.00)	0.01 (0.00)	-0.00 (0.00)	0.01 (0.00)
WifeEarnsMore*East			-0.02*** (0.00)			-0.01 (0.01)
East			-0.03*** (0.01)			
Individual fixed-effects Individuals	No	No	No	Yes 3255	Yes 1510	Yes 4765
Observations	22159	12115	34274	22159	12115	34274

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the Land level ($n = 16$) level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls include log of household income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6). Panel A and C further control for relative income and Panel A and B for respondent and partner's log of individual income.

Table 1.E13: Consequences of Violating the Male Breadwinner Norm. Clustering at the District Level

	West (1)	East (2)	All (3)	West (4)	East (5)	All (6)
<i>Panel A</i>	<i>Dep. Var.: Female Housework Time (Hours per day)</i>					
Wife Earns More	0.1* (0.05)	-0.02 (0.04)	0.12** (0.05)	0.17*** (0.04)	0.01 (0.03)	0.18*** (0.04)
WifeEarnsMore*East			-0.14** (0.06)			-0.18*** (0.05)
East			-0.67*** (0.10)			
Individual fixed-effects Individuals	No	No	No	Yes 4128	Yes 1976	Yes 6104
Observations	22091	12114	34559	22091	12114	34205
<i>Panel B</i>	<i>Dep. Var.: Divorced within a 5-year time horizon (1=Yes)</i>					
WifeEarnsMore	0.01 (0.03)	0.00 (0.01)	0.02 (0.02)	0.03* (0.02)	-0.01 (0.01)	0.03* (0.02)
WifeEarnsMore*East			-0.01 (0.02)			-0.05** (0.02)
East			-0.01 (0.03)			
Couple fixed-effects Couples	No	No	No	Yes 1900	Yes 882	Yes 2782
Observations	9054	4919	13973	9054	4919	13973
<i>Panel C</i>	<i>Dep. Var.: Woman Out of the labor market in T+1 (1=Yes)</i>					
WifeEarnsMore	0.02*** (0.00)	0.00 (0.00)	0.02*** (0.00)	0.01* (0.00)	-0.00 (0.00)	0.01* (0.00)
WifeEarnsMore*East			-0.02*** (0.00)			-0.01 (0.01)
East			-0.03*** (0.01)			
Individual fixed-effects Individuals	No	No	No	Yes 3255	Yes 1510	Yes 4765
Observations	22159	12115	34274	22159	12115	34274

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the district level ($n = 404$) are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls include log of household income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6). Panel A and C further control for relative income and Panel A and B for respondent and partner's log of individual income.

E.8 Accounting for the Variation in Paid-Work Time

This section investigates the role of paid-work time variations on *doing gender* behaviors. It shows that our results hold even when the female relative and absolute paid-work time remain stable or vary marginally.

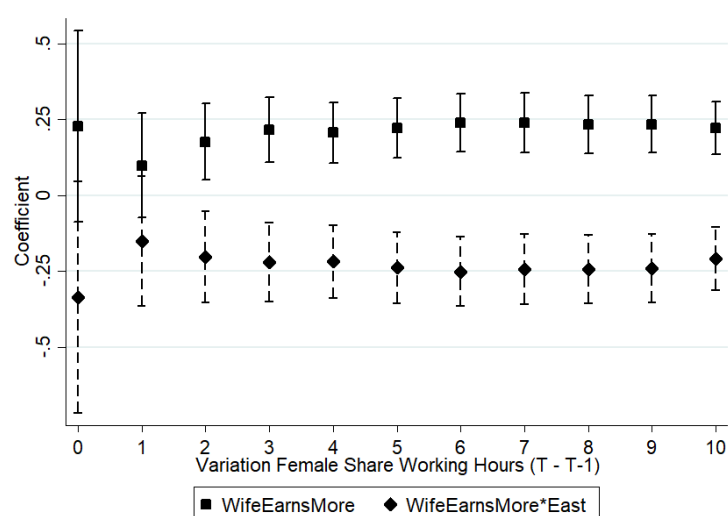
Figure 1.E1 presents the main estimates of the relationship between women's housework time and relative income restricting the sample to couples where the variation of relative female paid-work time between two successive periods is within the bandwidths on the x-axis (0, 1 p.p., 2 p.p.,...).¹⁸ As we can see, the coefficient related to *WifeEarnsMore* is essentially stable at around 0.2-0.25 and always offset by the one associated with *WifeEarnsMore * East*, which is negative. In terms of precision, the results become statistically significant at the 5% level from a 2 p.p. variation of female relative paid-work time. Below this threshold, the coefficients are less precisely estimated but similar in terms of magnitude.

Figure 1.E2 replicates the same exercise using the variation of absolute female paid-work time. It thus restricts the sample to couples where the number of paid-work time of the wife is within a set of bandwidth (stable, 1 hour, 2 hours, ...).¹⁹ As we can see, the results hold across the entire set of bandwidths with a magnitude of about 0.25 for the coefficient related to *WifeEarnsMore*, always offset by the one on *WifeEarnsMore * East*.

¹⁸For instance, if $x = 2$, it means that the difference of female relative paid-work time ($\text{Female Paid-WorkTime} / \text{Female} + \text{Male PaidWorkTime}$) between 2 successive periods must be within the $[-0.02; 0.02]$ interval. Assuming that the share was initially at 50%, it means that the sample is restricted to couples where the wife's relative paid-work time will remain between 48% and 52%.

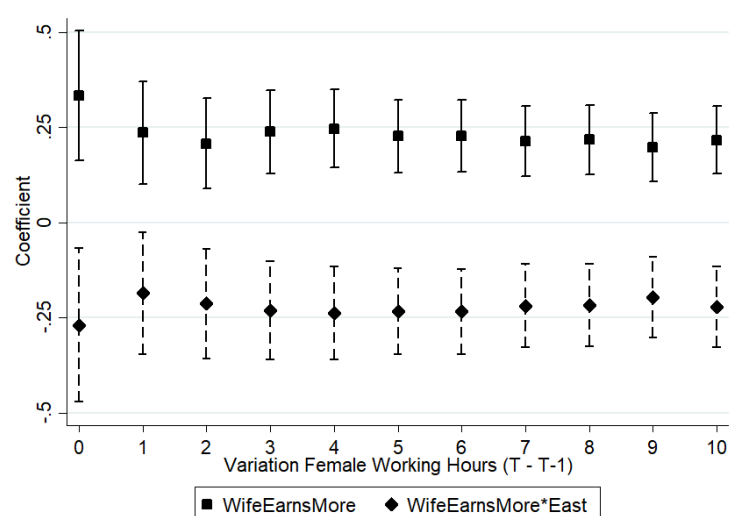
¹⁹For instance, if $x = 2$, it means that the difference of female paid work time between 2 successive periods must be within the $[-2; 2]$ interval. Assuming that the wife was initially working 40 hours per week, it means that the sample is restricted to couples where the wife's absolute paid-work time will remain between 38 and 42 hours.

Figure 1.E1: Woman's Housework Time and Relative Income depending on the Variation of Relative Female Paid-Work Time.



Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Each dot represents the coefficient from the main regression using female housework time as an outcome variable. The x-axis represents the bandwidth of the variation of the relative female paid-work time within 2 successive years. Square dots represent the coefficient related to *WifeEarnsMore* (solid confidence intervals are represented at the 95% level) and diamond dots represent the coefficient related to *WifeEarnsMore * East* (dashed confidence intervals are represented at the 95% level). Standard errors clustered at the individual level are given in parentheses. Controls include relative income between spouses, log of household income, log of woman's income and log of man's income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

Figure 1.E2: Woman's Housework Time and Relative Income depending on the Variation of Absolute Female Paid-Work Time.



Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Each dot represents the coefficient from the main regression using female housework time as an outcome variable. The x-axis represents the bandwidth of the variation of the absolute female paid-work time within 2 successive years. Square dots represent the coefficient related to *WifeEarnsMore* (solid confidence intervals are represented at the 95% level) and diamond dots represent the coefficient related to *WifeEarnsMore * East* (dashed confidence intervals are represented at the 95% level). Standard errors clustered at the individual level are given in parentheses. Controls include relative income between spouses, log of household income, log of woman's income and log of man's income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

E.9 Dropping the First Years Post-Reunification

Table 1.E14: Woman's Housework Time and Relative Income. Dropping the First Years Post-Reunification

	<i>Dependent Variable: Woman's Housework Time</i>				
	1992- (1)	1993- (2)	1994- (3)	1995- (4)	1996- (5)
WifeEarnsMore	0.19*** (0.04)	0.19*** (0.04)	0.19*** (0.04)	0.18*** (0.04)	0.17*** (0.04)
WifeEarnsMore*East	-0.20*** (0.05)	-0.20*** (0.05)	-0.19*** (0.05)	-0.18*** (0.06)	-0.16*** (0.06)
Individual fixed-effects	Yes	Yes	Yes	Yes	Yes
Individuals	5911	5744	5605	5504	5393
Observations	32843	31605	30401	29265	28130

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls include relative income between spouses, log of household income, log of woman's income and log of man's income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

E.10 Alternative Mechanisms - Historical Differences in Unobservables

Table 1.E15 shows that there is a sharp jump in the female share of household income at the border between East and West Germany.

Table 1.E15: Regression discontinuity of the Female Income Share in Household Income depending on the Distance to the East/West Border

	50 km (1)	100 km (2)	150 km (3)	200 km (4)
<i>Model A: Local linear polynomial</i>				
East	0.104*** (0.022)	0.115*** (0.016)	0.122*** (0.013)	0.117*** (0.012)
Observations	7684	15122	25185	35406
<i>Model B: Third order polynomial</i>				
East	0.102*** (0.037)	0.101*** (0.02)	0.119*** (0.017)	0.124*** (0.015)
Observations	7684	15122	25185	35406
<i>Model C: Local linear polynomial with controls</i>				
East	0.1*** (0.025)	0.101*** (0.016)	0.108*** (0.014)	0.099*** (0.011)
Observations	7684	15122	25185	35406

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the household level are given in parentheses. East is a dummy equals to 1 when the household is located in East Germany. Model A includes a local linear polynomial in distance from the border and model B includes a third order polynomial in distance from the border. Model C includes the following controls: log of the household income, respondent's and partner's age, age squared and educational level and a dummy controlling for the presence of children. Observations are weighted by proximity to the border using a triangular kernel.

We also replicated our main result concerning the relationship between women's housework time and WifeEarnsMore in the vicinity of the East/West Border. This provides supporting evidence that our results are not driven by historical differences in unobservables or by other structural differences.

Table 1.E16 displays regression estimates using four different bandwidths. *Doing gender* is taking place on the western side of the border. West German women increase the time spent on housework by 0.23-0.31 hours per day while East German women do not. As we move towards the border, the sample size and the precision of the estimations diminish. At the 50 km threshold, the p-value is 0.11 for the coefficient related to *WifeEarnsMore * East* and its magnitude is in line with the coefficients at different bandwidths.

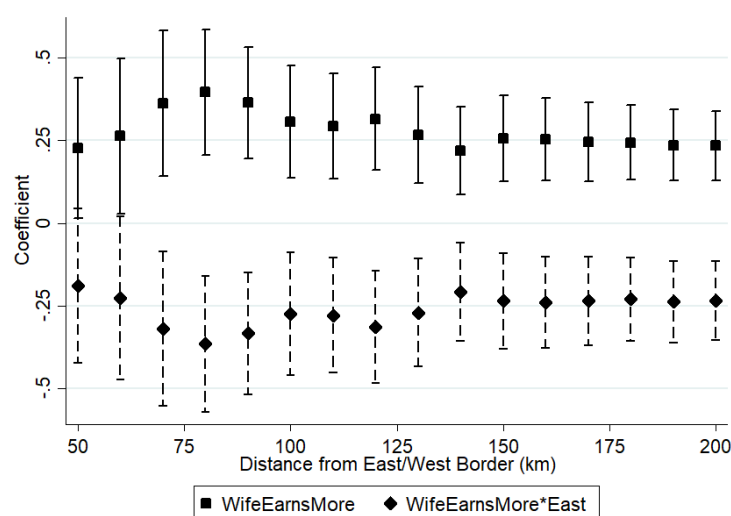
In the same vein, Figure 1.E3 displays the estimates at every 10km bin from 50 to 200 km around the border. Importantly, the coefficients remain stable in magnitude across the territory while historical and persisting differences are expected to grow as we move away from the East/West border. This suggests that our results are driven by the lasting cultural consequences of the division of Germany, rather than by some structural features of these territories.

Table 1.E16: Woman's Housework Time and Relative Income. Restricting the Sample Around the East/West Border

<i>Dependent Variable: Woman's Housework Time</i>				
	50 km (1)	100 km (2)	150 km (3)	200 km (4)
WifeEarnsMore	0.23** (0.11)	0.31*** (0.09)	0.26*** (0.07)	0.23*** (0.05)
WifeEarnsMore*East	-0.19 (0.12)	-0.27*** (0.1)	-0.24*** (0.07)	-0.23*** (0.06)
Individual fixed-effects	Yes	Yes	Yes	Yes
Individuals	1093	2054	3373	4646
Observations	6222	11824	19612	26378

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 if the household lives in East Germany. Controls include relative income between spouses, log of household income, log of woman's income and log of man's income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

Figure 1.E3: Woman's Housework Time and Relative Income around the East/West Border. Different Bandwidths



Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Square dots represent the coefficient related to *WifeEarningsMore* (solid confidence intervals are represented at the 95% level) and diamond dots represent the coefficient related to *WifeEarningsMore * East* (dashed confidence intervals are represented at the 95% level). Standard errors clustered at the individual level are given in parentheses. Controls include relative income between spouses, log of household income, log of woman's income and log of man's income, respondent and partners' age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

E.11 Alternative Mechanisms- Do the East/West Income Level Differences Matter?

This section investigates the potential role of household income level in explaining the occurrence of *doing gender* behaviors. The suspicion is that East German couples are obviously poorer, which could explain why they do not engage in *doing gender* behaviors. To investigate, we exploit the heterogeneity within West Germany to contrast the behavior of households depending on their income. We use two measures: one regional measure based on the average household income of the Land (macro definition) and one individual measure based on the relative position of a household in the distribution of household income across the entire West Germany (micro definition).

Table 1.E17 provides descriptive statistics on the income of households in the three categories: within West Germany above the median, within West Germany below the median and in East Germany. In Panel A, using a micro measure, we see that the bottom half of the distribution of West German couples earn on average 2418 euros, whereas the upper half earns 4879 euros. East German households earn on average 2661 euros. Thus, East German households are on average in between the average of below and above the median household income within West Germany. In Panel B, using a regional (macro) measure of household income, we see that East German couples lag behind the couples living in the poorest West German Länder and we obtain less heterogeneity within West Germany.

Table 1.E18 replicates the main result contrasting the behavior of West German households living in poor households in columns 1 to 3 (and alternatively poor Länder in columns 4 to 6) against those living in households (Länder) whose income is above the median. We see that in both groups, West German women keep increasing their housework time when they earn more than their husband. In column 1 to 3, which uses an individual definition displaying larger levels of heterogeneity, there is very little difference among the two groups.

Table 1.E17: Descriptive Statistics on the Income Level

	<i>West Germany</i>		<i>East Germany</i>
	Above Median (1)	Below Median (2)	(3)
<i>Panel A: Individual (Micro) Level</i>			
Household Income	4879 (2874)	2418 (490)	2661 (1234)
<i>Panel B: Regional (Macro) Level</i>			
Household Income	3625 (214)	3232 (150)	2568 (97)

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. This table displays the mean and standard deviations (in parentheses) of household incomes based on the definitions at the individual (micro) level or at the regional (macro) level.

Table 1.E18: Housework Time and Relative Income Depending on the Household Income Level

<i>Dependent variable: Woman's Housework Time (hours per day)</i>						
	<i>Reference: Individual Level</i>			<i>Reference: Regional Level</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
WifeEarnsMore	0.18*** (0.06)	0.19*** (0.07)	0.18*** (0.05)	0.18*** (0.06)	0.16** (0.07)	0.16*** (0.06)
WifeEarnsMore*Below Median			-0.02 (0.07)			
WifeEarnsMore*Below Median						0.04 (0.09)
Sample	Above Median	Below Median	All	Above Median	Below Median	All
Couples	2583	2643	4010	2235	1807	4010
Observations	10807	10652	21459	11804	9655	21459

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only West German dual earner married couples. The sample is restricted to households above the median in columns 1 and 4, below the median in columns 2 and 5 and includes all the households in columns 3 and 6. Standard errors clustered at the individual level are given in parentheses. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

E.12 Alternative Mechanisms - Do the East/West Male Hourly Wages Differences Matter?

In this section, we replicate the same exercise as in the previous section using the male hourly wage. The suspicion is that in a context of low male hourly wage, women may have a larger household bargaining power and refuse to engage in *doing gender* behaviors. This could potentially explain why these behaviors are not observed in East Germany. To investigate this channel, we exploit the heterogeneity within West Germany to contrast the behavior of households depending on the male hourly wage. We use two measures: one regional measure based on the average male hourly wage of the Land (macro definition) and one individual measure based on the relative position of one household in the distribution of male hourly wages across the entire West Germany (micro definition).

Table 1.E19 provides descriptive statistics about male and female hourly wages as well

as the gender gap in hourly wages (male - female) of households in the three categories: within West Germany above the median, within West Germany below the median, and in East Germany. In Panel A, we see that within the bottom half of the distribution of West Germans, the male hourly wage is about 8.4 euros, whereas in the upper half, it is about 17.7 euros. East German men are close but below the bottom half of West German men, as they earn on average 7.84 euros per hour. In terms of gender gap between men and women, in the upper half of West German couples, we observe a gender gap of about 8.1 euros per hour whereas it is about 0.8 euros for the bottom half. East German households are in between with a gender gap of about 1.05 euros per hour. In Panel B, using a regional (macro) measure of male hourly wage, we obtain much less variation. West German men living in Länder where the male hourly wage is above the median earn on average 13.45 euros per hour, whereas those living in the bottom half earn about 12.17 euros per hour.

Table 1.E20 replicates the main result contrasting the behavior of West Germans living in households where the male hourly wage is below the Western median against those living in households (Länder) whose male hourly wages is above the median. We see that in both groups, West German women spend more time on housework when they earn more than their husband. In column 1 to 3, which uses the micro definition, with a higher level of heterogeneity, there is little difference between the two types of households. Point estimates are slightly lower in households whose male hourly wage is below the median but the estimation coefficients remain unambiguously positive and significant at the 1% level. In columns 4 to 6, which use the macro definition, the pattern is similar.

Table 1.E19: Descriptive Statistics on Hourly Wages

	<i>West Germany</i>		<i>East Germany</i>
	Above Median (1)	Below Median (2)	(3)
<i>Panel A: Individual (Micro) Level</i>			
Male Hourly Wage	17.7 (10.09)	8.41 (2.09)	7.84 (4.99)
Female Hourly Wage	9.6 (6.27)	7.6 (5.2)	6.79 (4.11)
Gender Gap Hourly Wage	8.1 (11.1)	.81 (5.63)	1.05 (5.52)
<i>Panel B: Regional (Macro) Level</i>			
Male Hourly Wage	13.45 (1.04)	12.17 (0.37)	7.72 (0.45)
Female Hourly Wage	8.97 (0.82)	8.21 (0.33)	6.76 (0.32)
Gender Gap Hourly Wage	4.36 (2.07)	3.73 (0.19)	0.97 (0.22)

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. This table displays the mean and standard deviations (in parentheses) of household incomes based on the definitions at the individual level or at the regional level.

Table 1.E20: Housework Time and Relative Income Depending on the Male Hourly Wage Level

<i>Dependent variable: Woman's Housework Time (Hours per Day)</i>						
	<i>Reference: Individual Level</i>			<i>Reference: Regional Level</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
WifeEarnsMore	0.25*** (0.08)	0.16*** (0.06)	0.24*** (0.07)	0.19*** (0.07)	0.18*** (0.06)	0.18*** (0.07)
WifeEarnsMore*Below Median			-0.06 (0.08)			
WifeEarnsMore*Below Median						0.02 (0.09)
Sample	Above Median	Below Median	All	Above Median	Below Median	All
Couples	2570	2605	3981	1940	2072	3981
Observations	10514	10335	20849	9824	11025	20849

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only West German dual earner married couples. The sample is restricted to households above the median in columns 1 and 4, below the median in columns 2 and 5 and includes all the households in columns 3 and 6. Standard errors clustered at the individual level are given in parentheses. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

E.13 Alternative Mechanisms- Do the East/West Wage Dispersion Differences Matter?

This section investigates the potential role of the dispersion of wages in explaining the occurrence of *doing gender*. As explained in Section 6.2, we exploit the heterogeneity within West Germany and contrast the behavior of couples in places where the dispersion of wages is high versus low.

Table 1.E21 provides descriptive statistics about income dispersion. We use two of the most commonly used measures: Gini index and variance of logs. The two measures deliver a similar picture. In West German Länder, the Gini index of the female income, male income and household income in Länder where it is above the median is of respectively 0.4, 0.34 and 0.28 whereas it is of 0.37, 0.26 and 0.23 in places where it is below the median. The dispersion level of the male income and household income (but not female income) in East

German Länder is in between the average of the dispersion found in Western Länder above and below the median. In Panel B, using the variance of log as a measure of dispersion, we obtain a similar picture.

In Table 1.E22, 1.E23 and 1.E24, we exploit the heterogeneity in the dispersion level of respectively the household income, the male income and the female income, within West Germany. We contrast the behavior of households living in high dispersion versus low dispersion Western Länder (compared to the median). The three tables yield a similar picture: whether the dispersion level is high or low (in which case it is similar to Eastern Länder), West German woman keep increasing the time spent on housework when they earn more than their husband. As such, this *doing gender* behavior does not seem to be driven by the degree of income dispersion.

Table 1.E21: Descriptive Statistics on the Income Dispersion Level

	<i>West Germany</i>		<i>East Germany</i>
	Above Median (1)	Below Median (2)	(3)
<i>Panel A: Gini Index</i>			
Income Woman	0.4	0.37	0.3
Income Man	0.34	0.26	0.29
Household Income	0.28	0.23	0.24
<i>Panel B: Variance of Log</i>			
Income Woman	0.63	0.5	0.37
Income Man	0.22	0.16	0.19
Household Income	0.38	0.26	0.31

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. This table describes the measures of dispersion based on the indicator used (Gini index for Panel A and Variance of log for Panel B).

Table 1.E22: Housework Time and Relative Income Depending on the Regional Level of Inequality in Household Income

<i>Dependent variable: Woman's Housework Time (hours per day)</i>						
Dispersion Measure	<i>Gini</i>			<i>Variance of Log</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
WifeEarnsMore	0.19*** (0.06)	0.14* (0.07)	0.19*** (0.06)	0.19*** (0.06)	0.14* (0.07)	0.19*** (0.06)
WifeEarnsMore*Below Median						-0.05 (0.09)
WifeEarnsMore*Below Median			-0.05 (0.09)			
Sample	Above Median	Below Median		Above Median	Below Median	
Couples	2112	1922	4010	2112	1922	4010
Observations	11428	10031	21459	11428	10031	21459

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only West German dual earner married couples. The sample is restricted to households above the median in columns 1 and 4, below the median in columns 2 and 5 and includes all the households in columns 3 and 6. Standard errors clustered at the individual level are given in parentheses. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

Table 1.E23: Housework Time and Relative Income Depending on the Regional Level of Inequality in Man's Income

<i>Dependent variable: Woman's Housework Time (hours per day)</i>						
Dispersion Measure	<i>Gini</i>			<i>Variance of Log</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
WifeEarnsMore	0.18*** (0.06)	0.16** (0.07)	0.16*** (0.06)	0.14** (0.06)	0.19*** (0.07)	0.14** (0.06)
WifeEarnsMore*Below Median						0.06 (0.09)
WifeEarnsMore*Below Median			0.04 (0.09)			
Sample	Above Median	Below Median		Above Median	Below Median	
Couples	2235	1807	4010	1764	2271	4010
Observations	11804	9655	21459	9603	11856	21459

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only West German dual earner married couples. The sample is restricted to households above the median in columns 1 and 4, below the median in columns 2 and 5 and includes all the households in columns 3 and 6. Standard errors clustered at the individual level are given in parentheses. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

Table 1.E24: Housework Time and Relative Income Depending on the Regional Level of Inequality in Female Income

<i>Dependent variable: Woman's Housework Time (hours per day)</i>						
Dispersion Measure	<i>Gini</i>			<i>Variance of Log</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
WifeEarnsMore	0.18*** (0.06)	0.16** (0.07)	0.16*** (0.06)	0.18*** (0.06)	0.16** (0.07)	0.16*** (0.06)
WifeEarnsMore*Below Median						0.04 (0.09)
WifeEarnsMore*Below Median			0.04 (0.09)			
Sample	Above Median	Below Median		Above Median	Below Median	
Couples	2235	1807	4010	2235	1807	4010
Observations	11804	9655	21459	11804	9655	21459

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only West German dual earner married couples. The sample is restricted to households above the median in columns 1 and 4, below the median in columns 2 and 5 and includes all the households in columns 3 and 6. Standard errors clustered at the individual level are given in parentheses. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

E.14 Alternative Mechanisms - Other Institutional Differences

Table 1.E25 provides descriptive statistics about Eastern couples who have migrated to the West. It shows that these couples seem more gender egalitarian, i.e. the woman earns a higher share of income and is more likely to earn more than her husband than in "pure" Western couples.

Table 1.E25: Descriptive Statistics of Couples Living in Western Länder: Movers versus Stayers

	<i>Lived in FRG</i>		<i>Lived in FRG Movers</i>		<i>Lived in GDR</i>	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Woman's Share of Income	0.28	0.17	0.30	0.16	0.35	0.16
WifeEarnsMore	0.10	0.31	0.13	0.33	0.16	0.36
Woman's Housework Time	2.64	1.50	2.24	1.36	2.05	1.21
Man's Housework Time	0.61	0.72	0.65	0.65	0.79	0.76
Hswk Woman - Hswk Man	2.03	1.73	1.59	1.53	1.27	1.45
Paid Work Time Woman	26.74	13.21	28.85	13.72	33.17	11.53
Paid Work Time Man	44.55	9.86	44.87	8.45	45.65	9.74
Income HH (Euros)	3660.03	2394.77	3651.71	2868.22	3166.87	1388.25
Income Woman (Euros)	957.33	822.85	1017.75	800.17	1060.04	676.18
Income Man (Euros)	2445.41	1709.47	2490.26	1527.27	1981.45	1063.96
Hourly Wage Woman	8.58	5.39	8.61	5.52	7.51	4.29
Hourly Wage Man	13.09	8.61	13.47	9.76	10.33	5.01
Woman's Age	43.69	8.47	39.80	7.99	41.35	8.96
Man's Age	46.33	8.70	41.96	8.62	43.45	9.25
Kids in HH (1=YES)	0.68	0.47	0.65	0.48	0.61	0.49
Observations	20732		727		896	

Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Descriptive statistics are based on the main sample: married couples with positive income.

Although the sample of East German Migrants is small, it can still be useful to replicate the main analysis on housework and look at the point estimates. We thus replicated the main analysis on housework (which is the most robust result) restricting the sample to couples living in West German Länder and contrasting the behaviour of East German Migrants to West German natives.

The results are displayed in Table 1.E26. Reassuringly, in our preferred specification, which includes individual fixed-effects, the coefficient related to WifeEarnsMore among East migrants (column 5) is negative. Therefore, in the interaction specification (column 6), the two coefficients totally offset each other (the coefficients are respectively 0.17 for WifeEarnsMore and -0.23 for WifeEarnsMore*East). Nevertheless, given the small sample size, the confidence intervals are too large and it is difficult to draw definitive conclusions. Also, the picture is different in the cross-sectional analysis where the coefficient is positive, but again not statistically significant.

Table 1.E26: Housework Time and Relative Income - East German Migrants vs Movers within West Germany

<i>Dependent variable: Woman's Housework Time (hours per day)</i>						
	West	East Migr	All	West	East Migr	All
	(1)	(2)	(3)	(4)	(5)	(6)
WifeEarnsMore	0.10** (0.05)	0.29 (0.19)	0.10** (0.05)	0.17*** (0.04)	-0.11 (0.23)	0.17*** (0.04)
WifeEarnsMore*East Migr			0.02 (0.15)			-0.23 (0.21)
East Migr			-0.54*** (0.20)			
Individual fixed-effects	No	No	No	Yes	Yes	Yes
Individuals	4128	162	4290	4128	162	4290
Observations	22091	896	22987	22091	896	22987

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. EastMigr is a dummy equals to 1 when both spouses lived in the former GDR before 1989 and are now living in West Germany. Controls: relative income between spouses, log of household income, respondent and partner's log of individual income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects (columns 4,5 and 6).

Finally, Figure 1.E4 shows that within Berlin, women who used to live (or whose mother used to live) in the GDR are more likely to be in paid-work than those who used to live in the FRG. The precise formulation of the aforementioned question was: "where have you lived before the German reunification (or, if you were born after, where has your mother lived)?" There are 914 women under 30 years old; 440 live in West Berlin and 474 in East Berlin. Among women over 29 years old, 1226 live in West Berlin and 1007 in East Berlin.

Figure 1.E4: Female Participation in the Labor Market in Berlin



Notes: *The left graph is restricted to women under 30 years old. The right graph is restricted to women above 30 years old.*

E.15 Placebo Tests - Other Possible Focal Points

This section displays placebo tests using alternative focal points rather than the point of equal income at 50%. We use the most restrictive specification with individual fixed-effects for the three main outcomes. As we can see in Table 1.E27, 1.E28 and 1.E29, the only focal point which triggers *doing gender* behaviors is at 50% for the three outcomes.

Table 1.E27: Housework Time and Female Relative Income. Placebo Alternative Thresholds

<i>Dependent variable: Woman's Housework Time (hours per day)</i>					
	(1)	(2)	(3)	(4)	(5)
Wife Earns More than 10%	-0.03 (0.06)				
Wife Earns More than 10%*East	0.05 (0.11)				
Wife Earns More than 30%		-0.05 (0.04)			
Wife Earns More than 30%*East		0.04 (0.06)			
Wife Earns More than 50%			0.18*** (0.04)		
Wife Earns More than 50%*East			-0.18*** (0.05)		
Wife Earns More than 70%				0.24*** (0.09)	
Wife Earns More than 70%*East				-0.09 (0.11)	
Wife Earns More than 90%					-0.09 (0.17)
Wife Earns More than 90%*East					0.16 (0.23)
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individuals	6104	6104	6104	6104	6104
Observations	34205	34205	34205	34205	34205

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: relative income, respondent and partner's log of individual income, log of household income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

Table 1.E28: Marital Instability and Relative Income. Placebo Alternative Thresholds

<i>Dependent Variable: Divorced within a 5-year time horizon</i>					
	(1)	(2)	(3)	(4)	(5)
Wife Earns More than 10%	-0.01 (0.01)				
Wife Earns More than 10%*East	-0.03 (0.02)				
Wife Earns More than 30%		-0.01 (0.01)			
Wife Earns More than 30%*East		0.00 (0.02)			
Wife Earns More than 50%			0.03** (0.02)		
Wife Earns More than 50%*East			-0.05*** (0.02)		
Wife Earns More than 70%				0.06 (0.05)	
Wife Earns More than 70%*East				-0.05 (0.05)	
Wife Earns More than 90%					0.00 (0.02)
Wife Earns More than 90%*East					-0.03 (0.02)
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individuals	2782	2782	2782	2782	2782
Observations	13973	13973	13973	13973	13973

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio- Economic Panel using all the waves from 1991 until 2012. The sample contains only dual earner married couples who divorced during the panel. Standard errors clustered at the couple level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: respondent and partner's log of individual income, log of household income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and couple fixed-effects.

Table 1.E29: Labor Force Participation and Female Relative Income. Placebo Alternative Thresholds

	<i>Woman out of the labor force in $T+1$</i>				
	(1)	(2)	(3)	(4)	(5)
Wife Earns More than 10%	-0.02*** (0.01)				
Wife Earns More than 10%*East	0.04** (0.02)				
Wife Earns More than 30%		0.01 (0.00)			
Wife Earns More than 30%*East		-0.00 (0.01)			
Wife Earns More than 50%			0.01* (0.00)		
Wife Earns More than 50%*East			-0.01 (0.01)		
Wife Earns More than 70%				0.01 (0.01)	
Wife Earns More than 70%*East				-0.00 (0.01)	
Wife Earns More than 90%					0.01 (0.01)
Wife Earns More than 90%*East					-0.00 (0.01)
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individuals	4765	4765	4765	4765	4765
Observations	34274	34274	34274	34274	34274

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: relative income, log of household income, respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, year fixed-effects, Land fixed-effects and individual fixed-effects.

E.16 Placebo Tests - Other Possible Divisions of Germany

Table 1.E30: Placebo Exercise: Alternative Geographical Divides

<i>Dependent variable = 1</i> <i>if the coefficients on WifeEarnsMore and WifeEarnsMore*Group 1</i> <i>are both statistically significant,</i> <i>and 0 otherwise</i>			
	(1) At 10%	(2) At 5%	(3) At 1%
One Eastern Lander in Group 1	0.01 (0.02)	0.00 (0.02)	-0.00 (0.01)
Two Eastern Landers in Group 1	0.13*** (0.02)	0.08*** (0.02)	0.02 (0.02)
Three Eastern Landers in Group 1	0.38*** (0.02)	0.29*** (0.02)	0.12*** (0.02)
Four Eastern Landers in Group 1	0.74*** (0.05)	0.64*** (0.04)	0.38*** (0.04)
Five Eastern Landers in Group 1	1.00*** (0.3)	1.00*** (0.25)	1.00*** (0.17)
Observations	3003	3003	3003

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table tests all of possible divisions of the 15 German regions (excluding Berlin) into two groups of respectively 5 (Group 1) and 10 (Group 2) regions. We estimate equation 13 as in Table 1.3 column 6, with the number of female housework hours as the independent variable, but changing the composition of the "East" dummy into a dummy for belonging in Group 1 rather than Group 2. We then define a dummy that equals 1 if the coefficients associated to WifeEarnsMore and WifeEarnsMore*East are statistically significant at the relevant thresholds. We regress this dummy on the number of Eastern Länder in Group 1 as independent variable using Ordinary Least Squares. The omitted category is 0 Eastern Länder in Group 1. Column 1 displays the probability that the coefficients of interest are significant at the 10% level; column 2 at the 5% level and column 3 at the 1% level. For instance, the cell in the third column and 3rd row shows that with 3 Eastern Länder in Group 1 rather than zero, the probability that the coefficients of interest are statistically significant at the 1% level increases by 12 percentage points.

F Attitudes

F.1 Descriptive Statistics

This section displays descriptive statistics about attitudes with respect to work, success at work, career, marriage and family. The distributions are mostly concentrated on the modalities 1 and 2, representing the categories "very important" and "important". For this reason, the outcome variables in the article are dichotomized and equal 1 if the individual has considered a topic as very important and 0 otherwise.

Figure 1.F1: Importance of Work for Satisfaction

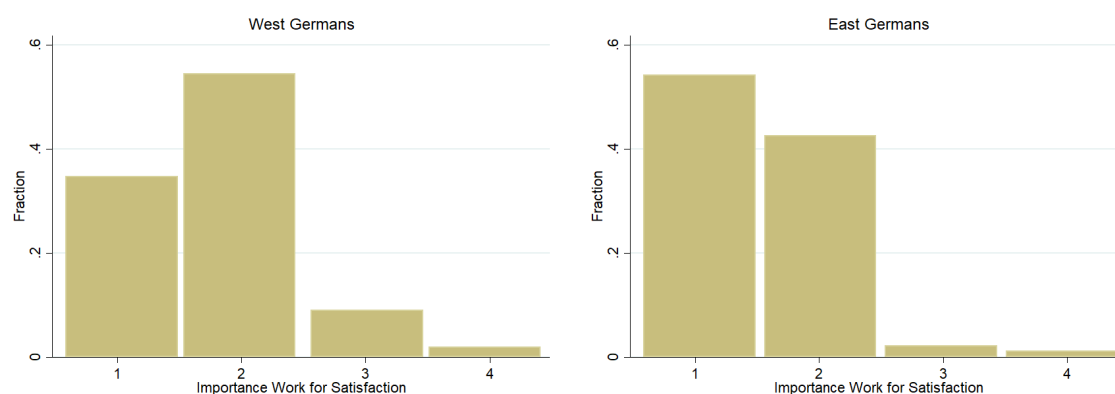


Figure 1.F2: Importance of Success at Work for Satisfaction

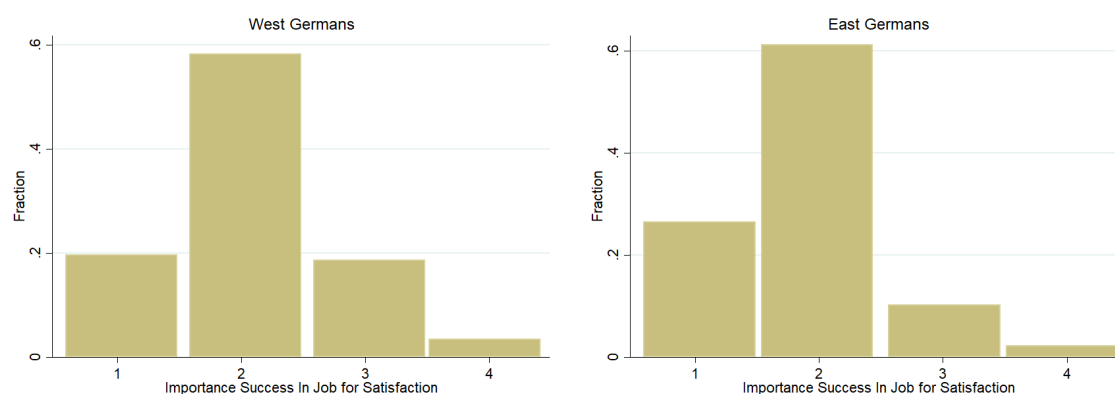


Figure 1.F3: Importance of Career for Satisfaction

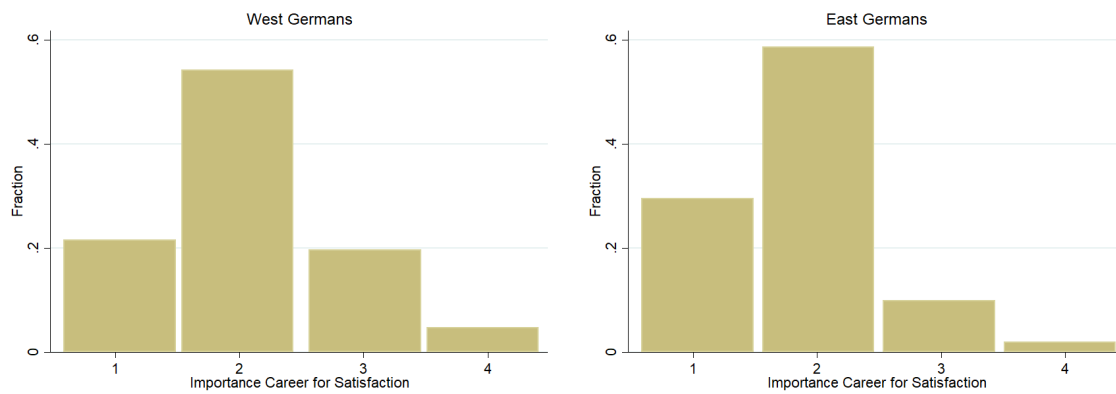


Figure 1.F4: Importance of Marriage for Satisfaction

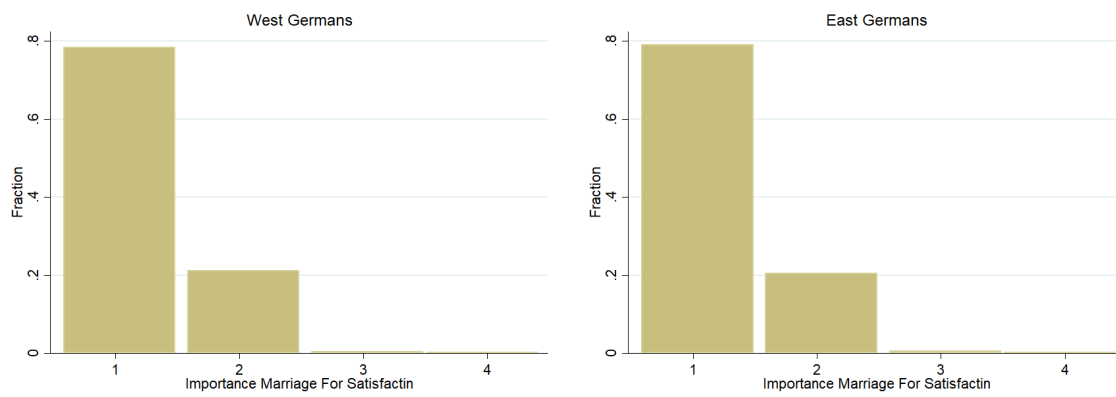
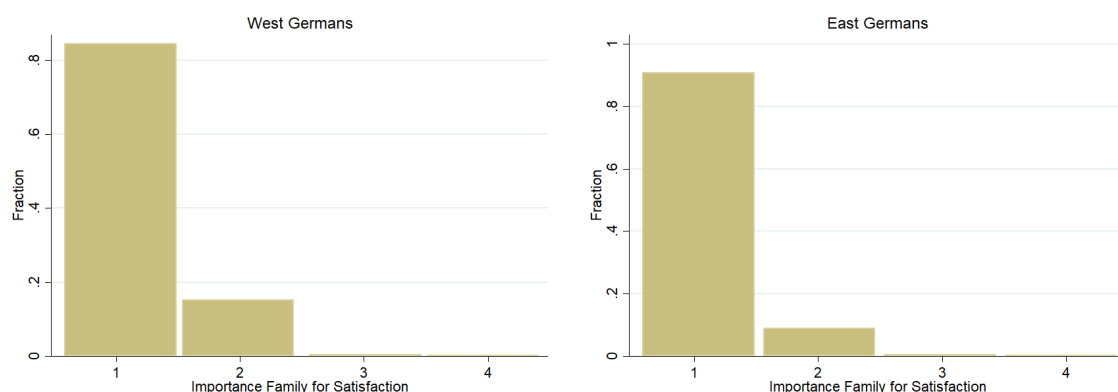


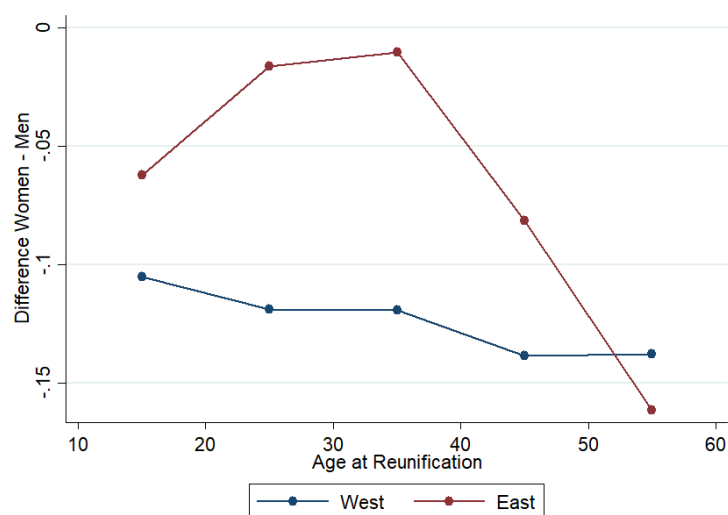
Figure 1.F5: Importance of Family for Satisfaction



Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Descriptive statistics are based on the main sample: married couples with positive income. Eastern couples are those in which both spouses lived in GDR before 1989. Each bar displays the fraction of individuals who gave this answer. The answers are given on a 1-4 scale, 1 being very important, 2 important, 3 unimportant and 4 very unimportant.

F.2 Attitudes Per Cohort

Figure 1.F6: Importance of Success at Work Per Cohort of East versus West Germans



Notes: The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. Sample: dual-earner married couples between 18 and 65 years of age. Eastern couples are those in which both spouses lived in GDR before 1989. Each bin represents the difference between men and women in importance for success at work (0-1 scale) within a window of individuals of similar age at reunification. The age windows are based on 10 years intervals.

Table 1.F1: Attitudes to Success at Work per Cohort of East versus West Germans

<i>Dependent Variable: How Important is Success at Work for your satisfaction ?</i>					
	(1)	(2)	(3)	(4)	(5)
Age at Reunification	10-20	20-30	30-40	40-50	50-
East	0.022 (0.029)	0.015 (0.019)	0.00 (0.016)	0.076*** (0.027)	0.155*** (0.051)
Female	-0.094*** (0.025)	-0.119*** (0.014)	-0.128*** (0.013)	-0.144*** (0.02)	-0.137*** (0.035)
Female*East	0.06 (0.043)	0.121*** (0.028)	0.132*** (0.025)	0.117*** (0.036)	0.0667 (0.075)
Observations	2286	7191	7840	3730	1071

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Probit marginal effects. The data comes from the German Socio-Economic Panel using all the waves from 1991 until 2012. The sample contains only married couples. Standard errors clustered at the individual level are given in parentheses. East is a dummy equals to 1 when both spouses lived in the former GDR before 1989. Controls: respondent and partner's age and age squared, respondent and partner's education level, a dummy controlling for the presence of children, a dummy representing whether the woman is working, a dummy representing whether the man is working and their interaction with the East dummy, year fixed-effects and Land fixed-effects.

Chapter 2: Gender and Lawmaking in Times of Quotas. Evidence from the French Parliament

This chapter is sole-authored.

Abstract

This article investigates whether female legislators defend the interests of women more than their male counterparts. I combine quasi-experimental variations to randomize legislators' gender and text analysis to identify women-related policies among more than 300,000 amendments discussed in the French Parliament during the 2001-2017 period. First, I exploit mixed-gender close races in the Lower House to show that female legislators are twice more likely to initiate women-related amendments. Second, as compared to other topics, I establish that women's issues constitute the key topic on which women are more active relatively to men, followed by health and child issues. At the other end of the spectrum, men are more involved in military issues. Third, I investigate the mechanisms and provide evidence that the activity of female legislators cannot be entirely explained by constituents' preferences or political parties' strategies but stems from individual interest of legislators. Fourth, I replicate these findings in the Upper House using a difference-in-differences strategy that directly exploits the introduction of a gender quota. I obtain similar results.

1 Introduction

Politicians do not resemble the people they represent. This reality is perhaps best illustrated by the underrepresentation of women in politics. While they account for about 50% of the population, women occupy only 24% of parliament seats worldwide²⁰. This imbalance is often criticized on the basis that politicians' identity matters for policymaking. Politicians would tend to advocate on behalf of groups sharing a component of their identity. Female politicians would thus defend the interests of women more than their male counterparts.

This paper investigates the empirical validity of this argument. In doing so, I aim to answer four questions: are female legislators more involved on women's issues? Besides women's issues, do we observe gender differences on other topics? Are these differences due to the identity of legislators or alternative factors? What can we infer for the impact of policies that aim at increasing the share of women in politics such as gender quotas?

To address these questions, the main innovation of this paper is to combine text data to identify women-related policies with quasi-experimental variations to randomize legislators' gender. The text analysis is performed on web scraped data from more than 300,000 amendments discussed within the French Parliament during the period 2001-2017. Amendments constitute the main way to change policies for parliamentarians. Based on the assumption that an amendment related to women's issues will effectively mention women, I classify an amendment as women-related if it contains one of the words "women", "gender" or "sex". Although simple, this measure proves to be highly relevant. As illustration, the most frequent trigram is "equality women men". For quasi-experimental variations, the first part of this article exploits mixed-gender close races in single member constituencies for the Lower House. Since potential confounders, such as constituents' characteristics, are similar whether a man or a woman wins, this strategy randomizes legislators' gender and allows to identify the effect of legislator's identity.

Using this methodology, the first set of results establishes that female parliamentarians are unambiguously and largely more involved on women's issues. As compared to their male counterparts, they are twice more likely to author at least one women-related amendment and they also co-sponsor twice more women-related amendments. These results are established for both accepted and rejected amendments and remain relatively stable across

²⁰<http://data.worldbank.org/indicator/SG.GEN.PARL.ZS>

age, party affiliation and incumbency status of legislators. They are robust to several alternative specifications using the entire sample of parliamentarians, including fixed-effects at the constituency level or controlling for individual and constituencies' characteristics. These findings also hold when using a restrictive dictionary of women's issues including only the word "women", far more common than "sex" and "gender" and are also almost impossible to replicate drawing random samples of amendments, suggesting that they are deeply related to the topic of women's issues.

Second, I extend the analysis to the study of other topics. I manually classify the 10,000 most recurring words into 27 non-mutually exclusive topics corresponding to the usual government ministries and use this dictionary to retrieve the topics of each amendment. I find that, among the 27 topics, women's issues constitute the topic with the largest differences in involvement between male and female legislators. As for other topics, female legislators are more active on child and health issues where they are about 50% more likely to initiate at least one amendment. At the other extreme, men are more active on military issues. Surprisingly, there is only weak or no evidence of gender differences on topics traditionally considered as more feminine such as family and education or more masculine such as business, fiscality and agriculture.

Third, I investigate the mechanisms behind these differences by focusing on the main result related to women's issues. The mixed-gender close races provides information relevant to the debate on the impact of constituents' preferences and legislator's identity on policymaking. In such races, whether a man or a woman wins, constituencies have similar characteristics. Yet, female legislators contribute largely more to women's issues. This suggests that constituents' preferences do not fully determine legislators' actions and brings empirical support to identity-based policymaking. But what lies behind identity? It could be that women are intrinsically more interested in women's issues but it could also be that political parties anticipate that women-related policies supported by women are more credible and therefore strategically force female legislators to produce women-related amendments. To disentangle between individual interest and parties' strategy, I first restrict the sample to cases where the amendment cannot stem from political parties' will. Such cases are more likely to arise when amendments are sole-authored and when legislators from the majority see their amendments rejected by their majority. We observe that, as we move to cases where the political party influence declines, female legislators are increasingly more likely to author women-related amendments than their male counterparts,

suggesting that the results cannot be entirely explained by political parties' influence but must stem from individual interest. To confort this interpretation, I also exploit an institutional feature of the 2012-2017 term where all the legislators were given discretionary funds that they could use as they wanted. Using this data, I show that female legislators are about two to three times more likely to fund associations or projects related to women's issues.

Finally, two shortcomings of the Lower House setting are that, although the share of female legislators has increased from 10 to 27% from 2002 to 2012 following the introduction of a gender quota in the Lower House, it is difficult to infer the causal effect of this policy on the prevalence of women's issues because the design of this quota does not allow to distinguish women who have benefited from it from those that would have been elected either way. Additionally, since the analysis is done at the individual level, it is questionable whether the micro findings translate into a macro effect where more women in politics would lead to more amendments on women's issues. There could exist a substitution effect where amendments initiated by newly elected women would otherwise have been defended by male legislators. To provide evidence on these questions, the last part of this article takes advantage of a gender quota introduced in the Upper House. The work of senators is the same as legislators from the Lower House but the election system for the Upper House is different as it relies on proportional representation in multi-member constituencies. From 2001 onwards, candidate lists in constituencies that elect more than 4 senators were forced to nominate 50% of women. I implement a difference-in-differences strategy comparing the parliamentary activity of constituencies targeted by the quota to those that are not. Consistent with the Lower House findings, I find that the election of one additional woman per constituency led to a doubling of the number and the share of women-related amendments initiated. This suggests that women who enter in politics with a quota do contribute more to women's issues and that this increase leads to a larger prevalence of women-related amendments at the macro level.

The Upper House setting also allows me to quantify the prevalence of women's issues in an hypothetical Parliament with 50% of female politicians. A simple back-of-the-envelope calculation suggests that the share of women-related amendments would be of 8%. Because this exercise implicitly assumes that constituencies work independently from each other, this figure would tend to be an upper-bound of the prevalence of women's issues in a Parliament where men and women would be equally represented.

These results make three contributions to the literature. First, they contribute to the debate on the impact of women as policymakers. An early literature in political science has studied the role played by female legislators in the Parliament, mostly in the US (see for instance Thomas, 1991, Thomas and Welch, 1991 or Bratton and Haynie, 1999). While this literature often finds that gender matters, the results are difficult to interpret as they do not disentangle between constituents' preferences and politicians' identity. Since women are more likely to be elected in more gender-friendly places which are also more demanding on women's issues, the results could be driven by constituents' demands. Therefore, starting from Chattopadhyay and Duflo (2004), the literature has focused on delivering causal evidence by disentangling the impact of politicians' gender from constituents' preferences. Several studies focusing on the experience of developing countries have shown that women deliver different types of public policy when in power (Chattopadhyay and Duflo, 2004, Clots-Figueras, 2011, Bhalotra and Clots-Figueras, 2014, Brollo and Troiano, 2016 or Kalsi, 2017). Yet, on the other hand, evidence from developed countries has been less conclusive (Ferreira and Gyourko, 2014, Bagues and Campa, 2017). On top of these conflicting findings, a common issue in this literature is the difficulty to identify women-related policies. Because of data limitations, empirical work mostly relies on pre-defined categories based on spending or public goods data which rarely include women-related policies and often differ depending on the context. This paper aims to address the weaknesses of each literature: the difficulty to interpret the results as stemming from identity in the early literature and the difficulty to relate the outcomes to specific areas of interest in the causal literature. Moreover, by exploiting the introduction of a gender quota in the Upper House, this paper also provides evidence on the consequences of such policies which have been introduced in over 100 countries worldwide (Krook, 2010).

Second, beyond gender, this paper contributes to the larger debate on the impact of politicians' identity. Understanding the extent to which identity determines policymaking is fundamental to understand how democracy works. In the classical median voter model (Downs, 1957), politicians' identity does not matter as policies should converge towards the preferences of the median voter. But in later-developed frameworks enriching the Downsian model (Alesina, 1988, Osborne and Slivinski, 1996 or Besley and Coate, 1997), policymakers' identity do play a role and influence which policies are implemented. The findings of this paper bring empirical support to these political economy models. Moreover, the methods used in this paper could easily be extended to understand the influence of

alternative dimensions of identity. Examples of these dimensions studied in the literature include caste (Pande, 2003), family (Washington, 2008), ethnicity (Broockman, 2013), religion (Meyersson, 2014, Bhalotra *et al.*, 2014 or Chaudhary and Rubin, 2016) and their interaction (Cassan and Vandewalle, 2017).

Finally, methodologically, this paper contributes to the growing literature using text analysis methods (see Grimmer and Steward, 2013 or Gentzkow *et al.*, 2017). Examples of recent studies include the analysis of economic uncertainty (Baker *et al.*, 2016), central bank's deliberation (Hansen *et al.*, 2018) or political discourse (Gentzkow *et al.*, 2016). This study presents an application of these tools combined with quasi-experimental variations.

This paper is organized as follows. Section 2 describes the institutional setting. The data is introduced in Section 3. The empirical strategy is explained in Section 4. The main results are displayed in Section 5 along with robustness checks. Section 6 dives into the mechanisms underlying the results. Section 7 investigates the impact of a gender quota in the Upper House to derive implications for public policy. Finally, Section 8 concludes.

2 Institutional Context: Legislative Work in the French Parliament

2.1 Parliamentary Procedure

The work of parliamentarians consists mainly in producing and voting the law. Before becoming effective, a law follows the subsequent path. First, a bill can be initiated either by the government or parliamentarians. Once initiated, a bill is examined by one of the Houses. Representatives have then the possibility to modify the bill by producing amendments. Once all the amendments have been examined, parliamentarians have to vote for the bill. If the bill is accepted, it is then passed to the other House which performs the same exercise. This procedure stops when a text is accepted in identical terms in the two Houses.

2.2 Public Policy Impact: Bills, Amendments and Vote

Parliamentarians have three ways to directly impact public policy: bill proposals, amendments and vote.

Bills. Bills can be introduced by the government or by parliamentarians. In practice, during the past 50 years, more than 70% of the adopted bills originated from the government. Since the government sets the agenda of the Houses for two weeks per month (and has different means to set the agenda during the remaining two weeks), the introduction of a bill by parliamentarians does not necessarily lead to its examination. Moreover, when introduced by parliamentarians, bills often result from a collective initiative originating from the political party to which parliamentarians belong to.

Amendments. Amendments consist of deletion, modification or addition of articles included in an existing bill. They can originate from the government or parliamentarians. In practice, nearly 80% of the adopted amendments originate from parliamentarians. Amendments can be both sole authored or co-sponsored by other parliamentarians. Importantly, there is no limit on the number of amendment that can be submitted on a bill, and there is not limit either on the number of co-sponsors an amendment can have or the number of amendments a parliamentarian can produce. On the contrary of bill proposals, all the amendments must be examined and discussed.

Vote. To be adopted, amendments and bills need to obtain a majority of votes. In the vast majority of the cases, the voting system is done with a show of hands. It is the regular procedure and has been adopted because it is much faster than alternative methods. For important bills, the vote can be recorded. In this case, parliamentarians vote from their seat with an electronic device. In case of an absence, it is possible to delegate votes to another representative.

2.3 Amendments as the Main Form of Parliamentary Initiative

In the French Parliament, there is no doubt that the main form of parliamentary initiative lie in amendments²¹. Quantitatively, it is well-established that the government remains the primary source of bills ultimately adopted while amendments mainly originate from parliamentarians. Tables 2.A1 and 2.A2 display descriptive statistics respectively on the origins of bills and amendments ultimately adopted. In the past 50 years, more than 70% of bills originated from the government while the picture is completely reversed

²¹As illustration, the Lower House website states that "*The right to amend is today the main form of expression of the parliamentary initiative*"(<http://www2.assemblee-nationale.fr/decouvrir-l-assemblee/role-et-pouvoirs-de-l-assemblee-nationale/les-fonctions-de-l-assemblee-nationale/les-fonctions-legislatives/l-exercice-du-droit-d-amendement-et-annexe>).

when looking at amendments. During the period 2002-2017, more than 80% of them were initiated by parliamentarians. As for votes, there is a strong party discipline in the French Parliament which leaves little room for individual initiative. This is because parliamentarians risk being excluded from their party if they vote against bills from their own side.

Qualitatively, scholars have acknowledged amendments as the main form of parliamentary initiative (Knapp and Wright, 2006, Avril and Gicquel, 2014). They often argue that this situation stems from the possibility for the government to set a large part of the agenda of both Houses (Rasch and Tsebelis, 2013). As such, bills are not necessarily discussed whereas all the amendments must be examined.

3 Data

3.1 Sources

Parliamentarian activity. Starting in 2002, the activity of legislators is accessible on the official website of the Lower House²². I web scraped this data to build an analyzable dataset containing all the amendments from 2002. It offers the possibility to analyze the activity during the 2002-2007, 2007-2012 and 2012-2017 terms.

For every amendment, this dataset includes three elements used for the analysis. First, it indicates which bill is targeted. Second, it includes the content as well as the oral presentation accompanying it (as they are defended publicly). Third, it specifies the identity of the amendment's initiator and of all the representatives who co-sponsored it²³.

Election Results. The data on the election results comes from the 2002, 2007 and 2012 parliamentary elections. These elections are held in 577 constituencies and the resulting dataset is produced by the French Ministry of Interior.

²²In its raw format, the data can be found from 2002 at <http://www2.assemblee-nationale.fr/recherche/amendements> or from 2007 onwards via an API at <https://www.nosdeputes.fr>

²³See Figure 2.B1 for an example of amendment on the Lower House website

3.2 Identifying Women-Related Amendments

3.2.1 Dictionary-Based Approach

The main challenge is to identify whether the topic of an amendment is related to women's issues. While the topics are not explicitly stated, it is possible to use the information associated to each amendment to retrieve them. In a nutshell, the procedure used in this article applies a dictionary based method to the bills' titles and texts motivating amendments to identify those which are related to women's issues.

Sources of Information. Each amendment is characterized by three elements: its content, a short text motivating its purpose and the bill that the amendment attempts to modify. Reading the content, it is almost impossible to understand what the topic of an amendment is. The content is very short and standardized, stating that a set of words should be removed, replaced or added²⁴.

On the opposite, bills' titles and texts motivating amendments are informative on the topic. For instance, the title "*Bill proposal related to the pay equality between men and women*" leaves no doubt that the topic of the bill is to reduce the wage gap between men and women. As such, an amendment targeting this text would be directly related to women's issues. Additionally, parliamentarians can disseminate and provide dispositions related to women's issues in bills whose primary purpose is unrelated to these issues. In this case, we would find a reference to women in the text motivating the amendment. To encompass these two dimensions, I exploit both the titles of the bills and the texts motivating the amendments to develop a definition of an amendment related to women's issues.

Classifying Amendments. To identify amendments related to women's issues, I build a simple dictionary containing words related to women. If the topic of an amendment is related to women, it is almost certain that the term women or a synonym will appear. I thus use three keywords: "women", "gender" and "sex"²⁵.

Amendments may refer to women's issues without using explicitly the word "women".

²⁴Standardized formulations can be found on the Lower House website (in French) at <http://www2.assemblee-nationale.fr/decouvrir-l-assemblee/role-et-pouvoirs-de-l-assemblee-nationale/les-fonctions-de-l-assemblee-nationale/les-fonctions-legislatives/l-exercice-du-droit-d-amendement-et-annexe>

²⁵In French, these keywords are respectively "*femme*", "*genre*" and "*sexe*". These keywords are stemmed such that the word "women" becomes "wom" to capture the singular and plural forms but also words such as womanly.

For instance, they could only contain the expression "gender equality" or "equality between the sexes". Therefore, introducing the words "gender" and "sex" leads to an *exhaustive* dictionary of women's issues. One drawback of expanding the dictionary is the possibility to capture false matches. This is true for the word "gender" which is sometimes used in the French language as a synonym for "genre" or "kind of". To circumvent this issue, I tagged these false matches and removed them from the classification²⁶ (see Table 2.F3 for the list).

Moreover, for robustness, I also use a *restrictive* dictionary of women's issues including only the word "women". Results remain unchanged.

This methodology is suited to my analysis for three reasons. First, the topic of an amendment is unknown and there does not exist a training dataset containing pre-defined categories for topics. Second, the main interest of this paper is clearly defined, i.e. about topics related to women's issues. Third, it is very unlikely that unsupervised methods would pick the topic related to women's issues because its prevalence is low (about 2%) but also because the total number of topics is unknown²⁷.

3.2.2 Validity of the Dictionary

Most Frequent Expressions. The dictionary-based method has selected 3,744 amendments in the Lower House (1.89%). Table 2.1 displays the 5 most frequent bigrams and trigrams used in these amendments. Looking at Panel A, the most frequent trigram is "equality wom men" which appears 166 times in the sample of amendments and the most frequent bigram is "wom men" which appears 425 times. Most of the expressions are directly related to women's issues containing the word "women" associated with "equality", "professional" or "violence". It strongly suggests that these amendments are used to improve gender equality. The only exception concerns part time work which is associated to women's issues because legislators tend to regulate the use of part time work for women

²⁶The existence of false matches is common in dictionary-based methods (see for instance Gentzkow *et al.*, 2016). Removing them from the exhaustive definition also allows to be consistent with the analysis on the other topics introduced in Section 5.3.

²⁷In Section F.4, I discuss the main advantages and disadvantages of unsupervised methods to perform topic analysis and display the 30 topics found by a latent dirichlet allocation method. In summary, these methods would be more adequate if the research question was "as compared to male legislators, are female legislators working on different topics?" without being interested in the topics involved. But since there exists clearly identified topics on which we wish to test gender differences, dictionary-based methods are more relevant.

since they are significantly more likely to be in this position than men.

Manual Screening. While the most frequent expressions are informative on the relevance of the topic, we cannot conclude on the relevance of the measure. I thus manually screened all the 3,744 amendments in order to determine the share of amendments falsely classified as related to women's issues. I find that 86% of amendments are unambiguously women-related. This rate is high and in line with other studies using dictionary based approach (see for instance Baker *et al.*, 2016). The wrongly classified amendments often refer to jobs mainly occupied by women (such as prostitution). A small share also includes false matches that are difficult to identify. For instance, some amendments refer to citizens as "men and women" in a general statement.

3.3 Main Outcome: Authorship

The main outcome is related to the authorship of an amendment. In the French Parliament, legislators who initiate an amendment are identified as the first author on an amendment. They must defend it orally and are clearly categorized as the largest contributor on an amendment.

Since almost 60% of legislators have never initiated an amendment related to women's issues, the main outcome is a dummy which equals 1 if a legislator has initiated at least one amendment related to women's issues. It stands for the extensive margin of authorship.

3.4 Descriptive Statistics

Table 2.C1 provides descriptive statistics on the Lower House legislators by sex. The sample is restricted to legislators who were elected at the beginning of each term and effectively served as legislators²⁸. The sample contains 1,663 legislators on the 2002-2017 period. About 19% of the legislators are women. They are about 55 years old. They co-sponsor about 212 amendments per year and initiate 25, out of which women's issues represent respectively 3% and 2%.

Regarding the sample of close races used to identify the effect of gender, we see in Figure 2.C5 that they are scattered throughout the territory with a larger concentration in the region surrounding Paris, certainly due to the largest number of representatives in

²⁸After each election, about 20 legislators out of 577 are nominated at high-ranked positions such as minister. Therefore, they never occupy the position of legislator in the Parliament.

this area. Additionally, preferences for women exhibit substantial variations in the sample of close race elections. Figure 2.C6 shows that female vote share ranges from 20 to about 70%. The extent of these variations is similar to other studies exploiting mixed-gender close races (see for instance Bhalotra *et al.*, 2017 in India).

4 Empirical Setting

4.1 Election System for the Lower House

Electoral System. The elections for the Lower House (legislative elections) occur every 5 years in France and aim at electing 577 representatives (*Députés* in French) in 577 constituencies. Parliamentarians are elected by direct universal suffrage. All the French citizens aged more than 18 years old are able to vote.

The election system follows a two-round plurality voting round system. To be elected on the first round, one individual must obtain more than 50% of the votes and 25% of the registered citizens. If these conditions are not met, a second round is organized a week later and the two first-ranked candidates are automatically qualified for it. To be elected on the second round, relative majority is sufficient and the candidate who receives the highest vote share is the winner.

Gender Quotas. Starting in 2002, financial incentives were introduced to force political parties to nominate women. If a political party does not nominate 50% of women, its public funding will be reduced proportionally on the gender gap in nomination. This proportion was gradually increased from 50% in 2002 to 75% in 2007 and reached 150% in 2017²⁹. As a consequence, the share of female legislators increased from 10 to 27% between the 2002 and 2012 elections. Since nearly all the parliamentarians originate from political parties, it is difficult to exploit this policy to distinguish women who have benefited from it and women who would have entered in politics either way.

4.2 Methods

The aim of this article is to provide novel evidence on the role of female parliamentarians on policy decisions. I exploit data at the parliamentary level to analyze how gender

²⁹For instance in 2002, if a political party nominated 100 candidates, composed of 60 men and 40 women, the public financial aid would be reduced by $\frac{(60-40)*0.5}{100} = 10\%$

determines the contribution to women's issues.

To identify the impact of female legislators on policy decisions, I use three complementary specifications. The objective is to obtain both a comprehensive view of what happens in the Lower House but also to disentangle between legislator's and constituents' interests. The first empirical specification is the following:

$$Y_{ict} = \beta Woman_{ict} + \gamma X_{ict} + \epsilon_{ict} \quad (3)$$

Where i is the subscript for the individual level, c for the constituency level and t for the term. Since constituencies elect only one parliamentarian, the observation level is at the legislator-term level. Y_{ict} is the outcome variable which is a dummy equal to 1 if the legislator has initiated at least one amendment on the topic of interest. $Woman_{ict}$ is the main variable of interest. It is a dummy that equals 1 if the parliamentarian is a woman. X_{ict} includes other control variables, namely the age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects.

While this specification is useful to obtain a comprehensive view of gender specialization in the Lower House, it suffers from endogeneity issues undermining the interpretation. In particular, it does not disentangle between what stems from constituents' demand or from parliamentarians' interest. Since we could expect that women are elected in more gender-friendly places which also demand more gender equality, estimates of β from Equation 12 would capture both the effect of the legislator's gender and the effect of constituents' preferences, unobservable in the data.

To disentangle between these two effects, I use two different specifications. First, I add fixed-effects at the constituency level to control for time-invariant changes and obtain results interpretable within constituencies. As unobserved invariant heterogeneity is likely to sort male and female legislators in constituencies according to the interest and attitudes of constituents, fixed-effects improve the identification of the impact of female legislator. The second empirical specification is:

$$Y_{ict} = \beta Woman_{ict} + \gamma X_{ict} + \mu_c + \epsilon_{ict} \quad (4)$$

Where i is the subscript for the individual level, c for the constituency level and t for the term. μ_c is a fixed-effect at the level of a constituency. Yet, factors determining simultaneously the election of a woman and the policy decisions taken by the legislator, such as attitudes towards women's issues, could vary over time undermining the causal interpretation of the fixed-effects estimates.

To alleviate this concern, I implement a close election regression discontinuity design by focusing on mixed-gender close races³⁰. During these elections, women run against men and there are cases where a candidate of either sex wins by a narrow margin. In such cases, victory can be considered as random since unpredictable random events affect the electoral outcomes (Lee, 2008) and therefore provide exogenous variations for the sex of legislators.

Empirically, the running variable is the vote margin between the first woman and the first man³¹. Providing that confounders behave continuously around the victory threshold and that candidates cannot manipulate their score, this strategy causally identifies the impact of female legislators. Formally, the third empirical specification is:

$$Y_{ict} = \alpha + \beta \mathbb{1}\{X_{ict} > 0\} + \gamma f(X_{ict}) + \epsilon_{ict} \quad (5)$$

Where i is the subscript for the individual level and c for the constituency. X_{ict} is the running variable. $\mathbb{1}\{X_{ict} > 0\}$ is a dummy that equals 1 if the running variable is positive, i.e. if a woman won the seat. $f(X_{ict})$ is a polynomial interacted with $\mathbb{1}\{X_{ict} > 0\}$.

This equation is estimated on a narrow margin around the elimination threshold. The reference bandwidth is selected following the approach of Calonico *et al.* (2014) depending on the vote margin. To probe the robustness of the results, I also estimate the equation using the IK bandwidth (Imbens and Kalyanaraman, 2012) which is often larger than the

³⁰This method is regularly used in the literature investigating the impact of female politicians. Examples of studies using this method include Ferreira and Gyourko (2014), Bhalotra and Clots-Figueras (2014), Bhalotra *et al.* (2017) or Brollo and Troiano (2016)

³¹If the woman wins, this variable is thus positive and if the man wins, this variable is negative. For example, if, the first woman obtains 55% of the votes and the first man 45%. Then, the running variable would be equal to 10 percentage points.

reference bandwidth³². In both specifications, observations are weighted with a rectangular kernel (following Imbens and Lemieux, 2008). I also estimate an additional specification on the entire sample which fits a second order polynomial in the running variable X_{ict} .

4.3 Internal Validity Tests

The validity of the regression discontinuity design relies on two assumptions: absence of manipulation of the vote margin and continuity of potential confounders at the cutoff. In this section, I provide evidence supporting these two assumptions.

Figure 2.1 provides evidence on the absence of manipulation in the running variable around the elimination threshold. Visually, we observe that male candidates win more often against female candidates but, importantly for the identification strategy, there is no evidence of a discontinuity in the density of the vote margin. The McCrary test (McCrary, 2008) does not reject the null hypothesis of no manipulation³³. This was expectable as manipulation would require to either commit electoral frauds which are extremely rare in France or predict election results with extreme accuracy which is unlikely because in these constituencies, there are no polls and because unpredictable events (such as weather condition) on the election day could still happen and change the results.

To test the continuity assumption of potential confounders, I estimate Equation 5 using a set of covariates as outcome variables. If the setting is valid, there should not be any discontinuity in these covariates and the coefficient β related to the election of a woman should not be significant. Three sets of covariates are considered: one representing election characteristics (number of candidates, number of registered voters, abstention rate and invalid vote rate, political inclination of the constituency) and demographic characteristics (total population size, total male population, total female population, share of women in the population, share of working women, share of working age people, unemployment rate), preferences for women (female vote share in T and $T-1$). As shown in Table 2.2, the continuity assumption seems verified as there is no significant jump at the cutoff for each of these covariates (the relevant graphs are in Figures 2.C2, 2.C3, and 2.C4). Therefore, there is no evidence that the results are driven by any other characteristic than the gender of the legislator elected.

³²The bandwidths were selected with the Stata packages *rdrobust* and *rdob*

³³I also ran an additional manipulation test based on Cattaneo *et al.* (2018) which confirmed that the drop is not significant (p-value = 0.8208)

5 Results

5.1 No Gender Differences in Parliamentary Activity

We start this analysis with the study of potential gender differences in overall parliamentary activity. This is necessary to interpret the results of the following sections as if women are as active as men, any differential in topic involvement should be interpreted as specialization and, by extension, at the cost of working on other topics.

Table 2.3 provides an answer to this interrogation. The outcome is the number of amendments initiated (first author) in Panel A and a dummy equals to 1 if the legislator has initiated at least one amendment in Panel B. Each column corresponds to a different specification from the least restrictive (column 1) to the most (column 6). There is no clear pattern that emerges except that women seem to be roughly as active as men. In Panel A column 1, we see that women initiate 6 fewer amendments than men. Nevertheless, this result does not hold when constituency fixed-effects are added (column 2) and also disappears in the RDD specification (Column 3,4, 5 and 6) where the point estimates become slightly positive. In Panel B, looking at the extensive margin of amendments' initiation, we see that women seem to be slightly more likely to initiate at least one amendment but the estimates are not significant except in the polynomial specification of column 3. It suggests that there are very limited gender differences, if not none, in amendments' initiation.

5.2 Female Legislators are Twice More Active on Women's Issues

We now turn to the main outcome which is the initiation of women-related amendments. Figure 2.2 displays the main discontinuity graph examining the extensive margin of amendments' initiation. On the right-side of the vertical dashed line, a woman is elected whereas on the left side, it is a man. We see that when a female legislator is elected by a narrow margin, the probability that she introduces at least one women-related amendment jumps by about 20 percentage points, from 20% to more than 40%.

Table 2.4 confirms the visual impression. The five specifications all point towards the fact that women are more likely to initiate an amendment related to women's issues. Looking at column 1, in the pooled OLS specification, women are about 18 p.p. more likely to initiate an amendment. This effect is robust to the inclusion of fixed-effects at the

constituency level (column 2). Similarly, in columns 3, 4, 5 and 6, we see that women elected in mixed-gender close races are about 23 p.p. more likely to initiate such an amendment as compared to their male counterparts. Scaling this effect to the average probability of men to introduce at least one women-related amendment, we find that women are about 75% (column 1) to 120-160% (column 3,4, 5 and 6) more likely to initiate such amendments.

Besides authoring, legislators can contribute to an amendment by co-sponsoring it. Arguably, this requires less effort than authoring but it is still an indicator of interest. Therefore, I replicate the same analysis for co-sponsorship in Figure 2.D1 and Table 2.D1. Consistent with the analysis of authorship, the RDD, fixed-effects and pooled OLS specifications all point towards the fact that female legislators co-sponsor about twice more amendments than their male counterparts.

Additionally, in Section D, I investigate the heterogeneity of these results. The larger involvement of female legislators on women's issues holds for amendments that are ultimately accepted and rejected. It also remains relatively stable across age, incumbency status and political inclination. For the latter, it is interesting to observe that women, whether they are right or left-wing are roughly as likely to author women-related amendments but that this is not true for men. The left-wing ones show a higher involvement on these issues, leading to narrower gender differences on the authoring of women-related amendments among left-wing parliamentarians.

To probe the robustness of these findings, I challenged the dictionary used to identify women's issues by restricting it to only one word: "wom" which is far more common in French language than the two other words "gender" and "sex". Using this alternative dictionary, I replicated the main results on authorship in Table 2.E1. They are essentially similar to those found using the "exhaustive" dictionary. Second, I designed a placebo test that consists in drawing a set of random samples of amendments of equal size to the sample used in the main regression. Using this random sample, I show in Section E.2 that there does not exist any sample of amendments that delivers similar and consistent findings across the five specifications. Third, I checked the robustness of the regression discontinuity specification using the lagged outcome as dependent variable and to alternative bandwidths in respectively Sections E.3 and E.4. Finally, I implemented the so-called local randomization strategy (Cattaneo *et al.*, 2014) for the regression discontinuity in Section E.5 and obtained similar results.

5.3 Extension to Other Topics

This section extends the analysis to the study of other topics besides women's issues. The purpose is twofold: first, to question whether women's issues are the topic where female are the most active; second, to investigate potential gender differences on other topics.

I define a list of 27 non-mutually exclusive topics, which stems from the usual government ministries that existed during the 2002-2017 period. To assign each amendment to a topic, I classify the most frequent 10,000 words into each of the 27 categories and remove false matches. The methodological details are discussed in wider details in Section F.1 and descriptive statistics on the prevalence of each topic are displayed in Figure 2.F1.

Figure 2.3 displays the relative contribution of female legislators to each topic. Each row corresponds to a topic and each dot to the scaled probability that a woman initiates at least one amendment on the given topic as compared to a man. The results are displayed for the least (Pooled OLS, graph a) and the most restrictive specifications (RDD with the CCT bandwidth, graph b).

First, looking at the first row of both graphs, we see that women's issues constitute the key topic on which gender differences in contribution are the most striking. Women are about twice more likely to initiate an amendment on women's issues and there does not exist any other topic where differences are as large.

Second, besides women's issues, two topics seem to emerge as significantly more associated to female legislators: health and child issues, which are displayed in the second and third rows. Female legislators are respectively about 42% and 48% more likely to initiate at least one amendment related to health and child issues in the RDD sample. Coefficients are nonetheless more robust when it comes to health issues (significant at the 5% and 1% level) than child issues (1% and 10%)³⁴. The robustness of women's contribution to health issues is consistent with other results in the literature. For instance, in India, Bhalotra and Clots-Figueras (2014) find a causal link between women's political representation and public health infrastructure.

At the other end of the spectrum, we see that women are almost 50% less likely to initiate at least one amendment related to military issues (significant at the 1% and the

³⁴When using the fixed-effects specification, the results are very similar to the pooled OLS specification. They are displayed in Figure 2.F3.

10% level respectively in the pooled OLS and the RDD specifications). There is also suggestive evidence that female legislators are less involved on elections and european issues although the estimations in the RDD sample are imprecise.

While the between topic analysis provides evidence of the absence of gender differences in involvement on the majority of topics, this finding could hide significant heterogeneity stemming from joint-topic involvement. For instance, female legislators may not work more on security issues as a whole but significantly more to the ones related to women's issues. This joint-topic analysis is all the more important to understand where the larger involvement of female legislators on women's issues comes from. Figure 2.F4 provides an answer to this question (descriptive statistics on the prevalence of each topic can be found in Figure 2.F2). We observe that the larger involvement of female legislators on women's issues come from an increase on amendments related to both women and family, security, europe, health, child, finance, labor, justice, elections and local issues. This strongly suggests that female legislators disseminate women's issues across a wide range of topics including some that are closely related such as family, child, health or labor and others that are less obviously related.

6 Mechanisms: Why Are Female Legislators More Involved on Women's Issues?

Female legislators are more likely to initiate women-related amendments. The effects are large and women's issues consitute the topic on which gender differences are the most striking. This section attempts to dive in the mechanisms behind these facts and consider the influence of constituents' preferences, strategy from political parties and individual interest.

6.1 Accounting for Constituents' Preferences

The first potential mechanism is the influence of constituents' preferences. In a standard median voter model (Downs, 1957), politicians' policies should converge towards the preferences of the median voter. Therefore, if women are elected in constituencies which are more gender-equal and more demanding on women's issues, female legislators should produce more amendments on this topic. This would be explained by voters' preferences

and not because of parliamentarians' own identity and preferences.

While this mechanism is certainly present when we consider the entire division of roles in the Parliament, it is much less influential when the analysis is restricted to constituencies with variations through time in the identity of the legislator and, even less, when we focus on mixed-gender close races. In this case, Section 4.3 provides evidence that constituents' preferences for women and demographic characteristics are similar whether a man or a woman is ultimately elected and yet, we observe large differences on the involvement on women's issues depending on the legislator's gender³⁵. This suggests that constituents' preferences cannot fully explain the results and paves the way for identity-based policy-making.

6.2 What Lies Behind Identity: Individual Interest or Political Parties' Strategy?

Since constituents' preferences cannot fully explain the results, it means that the election of a female legislator leads to a greater contribution on women's issues because of her sex. Two mechanisms could explain this fact. On one hand, female legislators could be intrinsically more interested in women's issues and eager to contribute on this topic. On the other hand, political parties or groups of legislators may behave strategically and anticipate that women-related policies carried by women appear as more credible and therefore more likely to pass. To disentangle individual interest from political parties' strategy, I restrict the analysis to cases where the amendment cannot stem from political parties' will.

First Case: Sole-Authored Amendments. The first case exploits the existence of sole-authored amendments. By definition, the involvement of legislators on such amendments cannot be explained by a desire to add political weight and increase the success rate of an amendment since there is only one author and no co-sponsors. Sole-authored

³⁵Arguably, the narrower the close races, the more alike are constituents' preferences. While in the main regression tables, we report the results using relatively large bandwidths following the CCT approach, one could wonder how the results vary when we move towards the threshold. To answer this interrogation, two strategies are implemented. First, Figure 2.E2 shows that the impact of the sex of legislators is stable as we restrict the sample to narrower close races. Only the precision of the estimates seems to diminish, alleviating concerns on the potential effects of the bandwidth size. Second, Section E.5 implements the so-called randomization strategy and narrows the sample to bandwidths where observables are balanced on both sides of the threshold. This leads to a window within 3 points in which women are still significantly more likely to initiate women-related amendments. The effect is of similar magnitude as the ones found previously.

amendments are also unlikely to originate from political parties' will. Three elements provide supporting evidence for this claim. First, if this assertion is true, we should observe relatively more sole-authored amendments from legislators whose political party is majoritarian in the Parliament. This is because bills would reflect the views of the party and not those of individual legislators (especially because most bills originate from the government constituted of higher-ranked party officials). Quantitative evidence support this claim. During the period 2002-2017, the share of sole-authored amendments was about 60% for legislators from majoritarian parties and went down to 35% in periods of minority. Second, the guidelines indicating how to write an amendment in the Parliament clearly state that the more co-sponsors an amendment has, the more likely it is going to pass³⁶. Therefore, the existence of sole-authored amendments cannot be explained by positive expectations on the success rate. Third, qualitative evidence from interviews with several parliamentary assistants working in the Lower House also confirm this classification³⁷.

Second Case: Rejected Amendments from the Majority. To investigate further the possibility that women are intrinsically more interested in women's issues, the second case exploits the outcome of amendments along with the political compositions of the Parliament. Over the period 2002-2017, two parties have successively been majoritarian: the right-wing from 2002 to 2012 (*UMP*) and the left-wing from 2012 to 2017 (*PS*). Practically, being majoritarian in the Parliament means that the party can pass any bill and amendment. It also means that legislators have less incentives to amend existing bills since they should already reflect parties' interest, as pointed above. Quantitative evidence support this claim as about 27% of majoritarian legislators' amendments are accepted against 7% for those in minority. Legislators in the majority also initiate on average 50% less amendments. Given these low incentives to amend and the high probability that an amendment passes, rejected amendments from majoritarian legislators cannot but stem from their individual interest. Ruling out the possibility of cognitive biases from political parties, it is difficult to conceive cases where a party would both ask a legislator to produce

³⁶They state that "*In the case of the co-sponsorship of an amendment [...] by members of the same political group, the political weight of the amendment will be precisely evaluated by the Lower House and by the government. Sometimes, the amendment will mention that the author carries the amendment in the name of that political group, which obviously add political weight to the amendment*". Source: Rédiger la Loi. Guide de rédaction des propositions de loi et des amendements.

³⁷I interrogated 6 parliamentary assistants who all pointed towards sole-authored amendments as a way to proxy the individual interest of legislators. They also agreed on the fact that amendments backed by the entire group often originate directly from the political party.

an amendment and at the same time ask to vote against it.

Therefore, I investigate gender differences on the samples of women-related amendments that are (i) sole-authored, (ii) from a majoritarian legislator and rejected by the majority and (iii) both sole-authored and from a majoritarian legislator and rejected by the majority. Figure 2.4 displays the results. The vertical axis represents the scaled effect and the horizontal axis represents the three cases described above plus the initial case without restriction on the origin or the outcome of the amendment. Looking at the Pooled OLS specification, we observe that as we move from the initial case to the sole-authored amendments from a majoritarian legislator and ultimately rejected by the majority, female legislators are increasingly likely to initiate women-related amendments when the influence of the party declines from about 100% for all amendments to 300% for sole-authored amendments originating from majoritarian legislators and ultimately rejected by the majority. Turning to the fixed-effects and the RDD specifications, we observe a similar pattern: point estimates increase but, because of the reduced sample sizes, the standard errors also increase and it is more difficult to conclude on the relative size of the effects. Nevertheless, it seems clear that the effects do not decrease in the three specifications, suggesting that the results cannot be entirely explained by political parties' influence but must stem from individual interest.

6.3 Additional Evidence from Legislators' Discretionary Funds

To complement the previous results, we exploit an institutional feature of the 2012-2017 term. During this term, all the legislators were granted a discretionary fund of 130,000 euros per year³⁸. Legislators were free to use the funds as they wanted (except for private gain). In practice, they were used to finance associations and local projects. Therefore, the analysis of the destination of these funds is likely to represent the individual interest of legislators. Another important feature of this data for the analysis is that every expenditure had to be classified according to a pre-defined nomenclature. This nomenclature included, among others, one category entitled equality between men and women, which is likely to be the closest to women's issues.

³⁸During the previous terms, these funds were only available to a subset of legislators and were unknown to most of them and to the public. Following a legal action in 2011, it was decided that clear rules would be defined to regulate the use and the attribution of these funds. In 2017, because the use of these funds was widely criticized, it was decided to end this practice.

I exploit this data to build a dummy variable equal to 1 if the legislator has funded associations or projects related to women's issues. Using this outcome, I replicate the same analysis as before. Figure 2.5 displays the main graph related to legislators elected in a mixed-gender close-race setting. We observe that, when a female legislator is elected by a narrow margin, the probability that she spends money on women's issues jumps starkly by about 30-40 p.p. and reach 50%. As we can see from Table 2.5, this jump is significant at the 1% level. It holds across the entire sample of legislators controlling for individual and constituencies' characteristics (column 1) and in the four usual specifications used for the RDD (column 2,3, 4 and 5). Scaling this jump to the average for male legislators, it represents an increase in the probability to spend money on women's issues that range between 150 and 300%.

It is interesting to compare this scaled effect to the one obtained using amendments. Indeed, with expenditures data from personal funds, we are likely to observe individual interest of legislators. Female legislators seem to be at least 150% more likely than men to spend money on women's issues. This effect is larger than the one obtained on the entire sample of amendments and close to the one obtained on rejected amendments. Therefore, this comforts the interpretation of rejected amendments in terms of individual interest.

7 Extension - Exploiting a Natural Experiment from the Upper House

The Lower House setting provides strong evidence that female legislators contribute more to women's issues because of their sex. This finding raises two questions: does it hold when we consider only women who would have entered in politics with the quota? Does a higher share of female politicians lead to a larger prevalence of women's issues at the macro level? This section brings answers to these two interrogations by exploiting a natural experiment caused by the introduction of a gender quota in the French Upper House.

7.1 Data and Empirical Design

7.1.1 Election System for the Upper House

Electoral System. Since 2008, elections for the Upper House occur every 6 years and aim at electing 348 representatives in 103 constituencies. Half of the Senate is renewed every 3 years. Senators are elected by indirect universal suffrage where only locally elected politicians can vote.

Before 2008, two main changes to the election system occurred: senators were elected for 9 years and the Upper House was renewed every 3 years. Therefore, the 2001 elections were the last ones to elect all the senators for 9 years. In 2004, half of the elected senators were elected for 9 years and the other half for 6 years³⁹. Finally, starting in 2008, all the senators were elected for 6 years. Figure 2.H1 schematizes this election schedule.

Most of the members of the Upper House are elected from multi-member constituencies and depending on the number of representatives elected, two election systems coexist. In constituencies where strictly less than 4 candidates are elected (48% of constituencies), the election system follows a two-round plurality voting round system. It is similar to the election system of the Lower House except that for some constituencies 2 or 3 representatives are elected. In constituencies where strictly more than 3 candidates are elected (52% of constituencies), the election system follows a proportional representation where votes are counted at the level of a list. Seats are attributed following a proportional rule where each candidate has a pre-determined rank of election on a list and when the number of votes for a list increases, so does the number of elected candidates.

Gender Quotas. In 2000, a law was voted which imposed that constituencies where the election system followed proportional representation, i.e. constituencies which elect strictly more than 3 candidates, had to comply with a gender quota. It imposed that, for each list, there had to be a strict alternation between men and women. If the first on a list was a man, the second had to be a woman and the third a man, etc. For constituencies which elect strictly less than 4 candidates, this law made no difference. Furthermore, due to the staggered schedule of the election system of the Upper House, a third of the constituencies had to comply with the quota for the first time in 2001, another third in 2004 and the last third in 2008.

³⁹The divide was assumed to be random

7.1.2 Data

Parliamentarian activity. The data on the amendments from 2001 onwards for the Upper House can be downloaded on the official website of the Senate⁴⁰. Similarly to the Lower House, it contains for each amendment, the bill targeted, the content as well as the oral presentation motivating its adoption and the identity of the initiator and of all the co-sponsors.

Election Results. I complemented this data with the election results for the years 1995, 1998, 2001, 2004, 2008, 2011 and 2014. This supplementary data is used to identify parliamentarians that entered the Senate through regular elections (and not by replacement).

7.1.3 Methods: Difference-In-Differences

I take advantage of a gender quota introduced in the first election after 2000. As a third of the Upper House is renewed every 3 years, three series are treated at different years: 2001, 2004 and 2008. Since the data starts in 2001, I focus on the two series renewed in 2004 and 2008 for which it is possible to have information prior to the introduction of gender quotas.

Using these two series, I implement a difference-in-differences strategy comparing authorship of women-related amendments before and after the implementation of policy between constituencies targeted by the quota and those that are not. I directly relate the introduction of women due to the quota to the production of amendments using an instrumental variable strategy. Formally, the first stage is :

$$NWomen_{ct} = \alpha_0 + \alpha_1 Treatment_c + \alpha_2 Post_t + \delta Treatment_c * Post_t + \epsilon_{ct} \quad (6)$$

The second-stage is:

$$Y_{ct} = \gamma_0 + \beta NWomen_{ct} + \gamma_1 Treatment_c + \gamma_2 Post_t + \epsilon_{ct} \quad (7)$$

where c is the subscript for the constituency level and t for time. $NWomen_{ct}$ corre-

⁴⁰<http://data.senat.fr/donnees/>

sponds to the number of women elected in a constituency. $Treatment_c$ is a dummy that equals 1 if a constituency has to comply with the quota, i.e. elects strictly more than 3 individuals after 2000. $Post_t$ is a dummy that equals 1 if the election year is after the introduction of the quota. β is the key coefficient representing the wald estimates. It measures the change in the prevalence of women's issues due to the election of one additional woman in the constituency.

7.1.4 Descriptive Statistics: Upper House Activity in Comparison With the Lower House

Compared to the Lower House, the activity in the Upper House is unambiguously lower. On the period 2001-2017, Upper House parliamentarians have produced 109,497 amendments, about twice less than their counterparts from the Lower House. Table 2.H1 provides descriptive statistics at the constituency level. On average, constituencies elect 3 candidates, out of which 0.53 are women (12%) and co-sponsor on average 135 amendments per year. In the Lower House, each parliamentarian co-sponsors about 212 amendments per year, which is nearly 5 times what a single senator does.

Regarding the prevalence of women's issues, the dictionary based method has classified 2,064 Upper House amendments as related to women's issues (1.88%). As for the Lower House sample, the most frequent bigrams and trigrams often refer to gender equality. In Table 2.H2, we see for instance that the second most frequent trigram is "equality women men". At the constituency level, roughly 21 amendments are initiated per year, out of which about 0.3 are related to gender. Therefore, about 60% of constituencies have ever initiated an amendment related to women, and, considering all topics, they have all initiated at least one amendment.

7.2 Results

7.2.1 First Stage - Impact of the Quota on the Election of Women

We begin by analyzing the impact of the gender quota voted in 2000 and introduced in the first election after this year.

Figure 2.6 displays the evolution of the number of women in constituencies targeted and untargeted by the quota. The vertical red-dashed line represents the introduction of the quota. On its right side, treated constituencies have to comply with the quota whereas

on its left side, both types of constituencies are untargeted. Looking at the evolution of the number of women in both types of constituencies, we first see that the number of women elected was stable and following a common trend before the introduction of the quota. It was slightly higher in treated constituencies perhaps because of the proportional election system operating only therein. After the introduction of the quota, the number of women elected in treated constituencies almost triples and is slightly below 2. In the other type of constituencies, there is a slight increase yet nothing comparable as the number of women elected per constituency remains below 0.5.

Table 2.6 confirms this visual impression. The two columns use two different specifications pooling the period after the introduction of the quota. Looking at the coefficient related to $Quota * PostPool$, we see that the quota has increased the number of women elected per constituency by about 1.25. In column 2, we see that this increase remained stable for constituencies where a second election took place within the period studied⁴¹. The F-statistics are respectively of 35.7 and 22.7 for the specifications used in columns 1 and 2, leaving aside concerns about a potential weak instrument.

7.2.2 Gender Quotas Lead to an Increase of the Prevalence of Women's Issues

We turn to the analysis of authorship of women-related amendments. To ease the interpretation and the comparability with the findings obtained in the Lower House, the main explanatory variable is the number of women in a constituency. It allows to interpret the coefficients as the consequence of one additional elected woman.

The impact of the quota is clearly consistent with the findings from the Lower House. In Table 2.7, looking at Panel A, we see that constituencies with one additional elected woman unambiguously produce more amendments related to women's issues. One additional woman leads to an increase of 0.17 amendments per year (column 1), a 1 p.p. increase in the share of women-related amendments (column 2), and a 12 p.p. jump in the probability to produce at least one amendment on this topic (column 3). As opposed to the Lower House results, estimations are more precise when using the share of amendments instead of studying the extensive margin. This is because the Upper House constituencies are held by multiple members. For each of these members, it is more relevant to study the

⁴¹Alternatively, in the Appendix Table 2.H6, alternative measures of the political representation of women are considered (share of women and at least one woman per constituency). The quota had an unambiguous positive impact on all these measures.

extensive margin but for a group, or in this case a constituency, it is preferable to use a continuous measure such as the count or the share of amendments.

Although OLS estimates are informative, they suffer from endogeneity biases which undermine the causal interpretation. For this reason, Panel B displays Wald estimates where the number of women in a constituency is instrumented by the variation in the number of women due to the gender quota introduced in constituencies with more than 4 elected representatives. Results remain similar and slightly larger in terms of magnitude. We observe that a constituency with one additional woman produces 0.39 additional amendment (column 1), increase by 2 p.p. the share of amendments initiated (column 2) and is 28 p.p. more likely to initiate at least one amendment (column 3), the coefficients being significant at the 5% level. Looking at the extensive margin studied in column 3, it is striking that the magnitude is very similar to what was found in the Lower House. Women were on average 23 p.p. more likely to initiate at least one amendment related to women's issues in the Lower House and this figure is about 28 p.p., i.e. indistinguishable from the one found in the Upper House.

Using this setting, it is also possible to extrapolate and obtain an upper bound for the prevalence of women's issues if there were 50% of women in the Upper House. To do so, I estimate equation 7 instrumenting the share of women in the constituency by the variation due to the quota. The results are displayed in Table 2.H9. The point estimates suggest that a 10 p.p. increase of the share of women leads to a 1.59 p.p. increase of the share of amendments related to women's issues. Therefore, in a Parliament with 50% of women, women-related amendments would count for up to 8% of all the amendments. This estimate tends to be an upper bound effect as the calculation implicitly assumes a complete independence of parliamentary work between constituencies. This assumption could be violated in practice if there were some substitution effects between constituencies where a certain share of amendments would originate from external sources and be initiated independently of who is present in the Parliament.

To probe the robustness of the results, I implemented the same robustness tests as for the Lower House findings, namely restricting the dictionary of women-related terms to the word "wom" and implementing placebo tests using random samples of amendments. Results are discussed in Section H.4. The restricted definition of gender-related amendments yields essentially similar findings (see Table 2.H10). As for the placebo tests, results are displayed in Figures 2.H3. In a nutshell, drawing 1000 random samples of amendments, none delivers

similar findings. This provides supporting evidence of the existence of homogeneity and relevance in the sample of women-related amendments selected using the dictionary-based approach.

8 Conclusion

This article has combined quasi-experimental variations and text analysis to investigate the causal impact of female legislators on lawmaking in a context with gender quotas. In the Lower House, using a regression discontinuity restricted to mixed-gender close races, I found that women are twice more likely to initiate amendments related to women's issues than their male counterparts. Classifying amendments into 27 topics, I established that women's issues constitute the topic with the largest gender differences in contribution. Female legislators are also more active on health and child issues whereas men contribute more to military issues. Investigating the mechanisms behind these findings, I showed that constituents' preferences and political parties' strategies cannot entirely explain these facts. Female legislators seem to be intrinsically more interested in contributing to women's issues which brings support for citizen candidate models. To provide evidence on the impact of gender quotas and to extrapolate on the potential macro effect of more women in politics, I used a difference-in-differences strategy exploiting the introduction of a quota in the Upper House only in constituencies that elect more than 4 senators. I found that constituencies which had to comply with the quota doubled the share of amendments related to women's issues they initiate.

Methodologically, the main contribution of this paper is to exploit text data from the Parliament to identify women-related policies along with quasi-experimental variations to randomize legislators' gender. Exploiting text data allows to overcome limitations from spendings or public good data which rarely include women-related categories. It is worth noting that data from Parliaments is often publicly and freely accessible and as such further research could thus extend this methodology to other countries and settings. Text analysis methods could also be used beyond the question of gender to test the relevance of different dimensions of politicians' identity.

From a public policy perspective, these results have two consequences. First, they directly contribute to the discussion on the policy relevance and consequences of gender quotas. They imply that the introduction of gender quotas could lead to a shift in

policymaking taking increasingly into account the interests of women in the population. Second, beyond gender, these findings question more generally the consequences of imbalances between the characteristics of politicians and those of the people they represent. It is questionable whether these imbalances matter for policymaking and whether correcting them would lead to public policies better encompassing the interests of groups of people underrepresented in politics.

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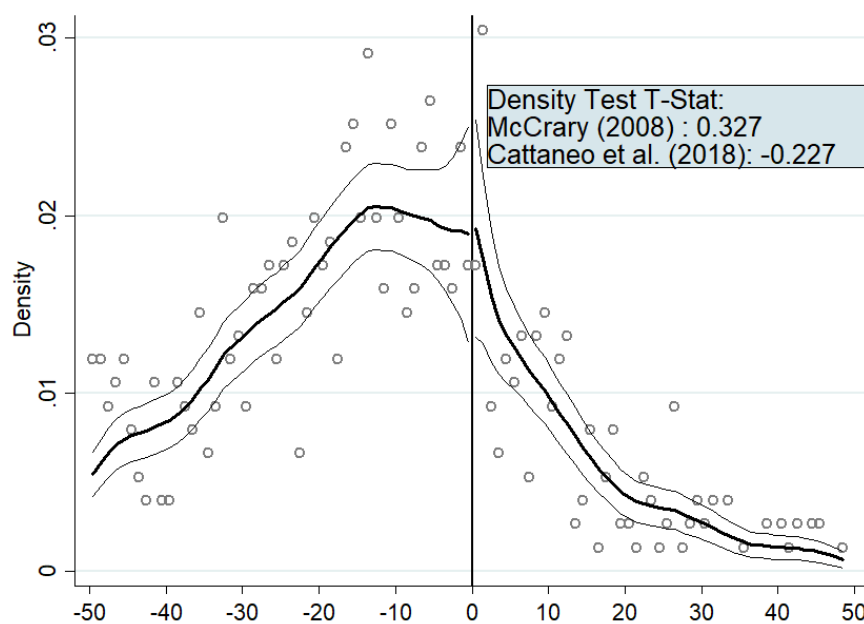
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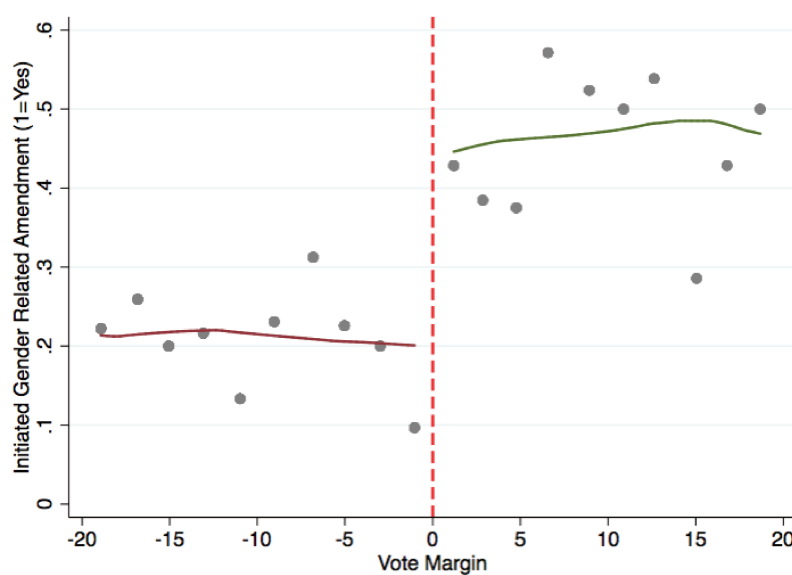
9 Figures

Figure 2.1: Manipulation Test



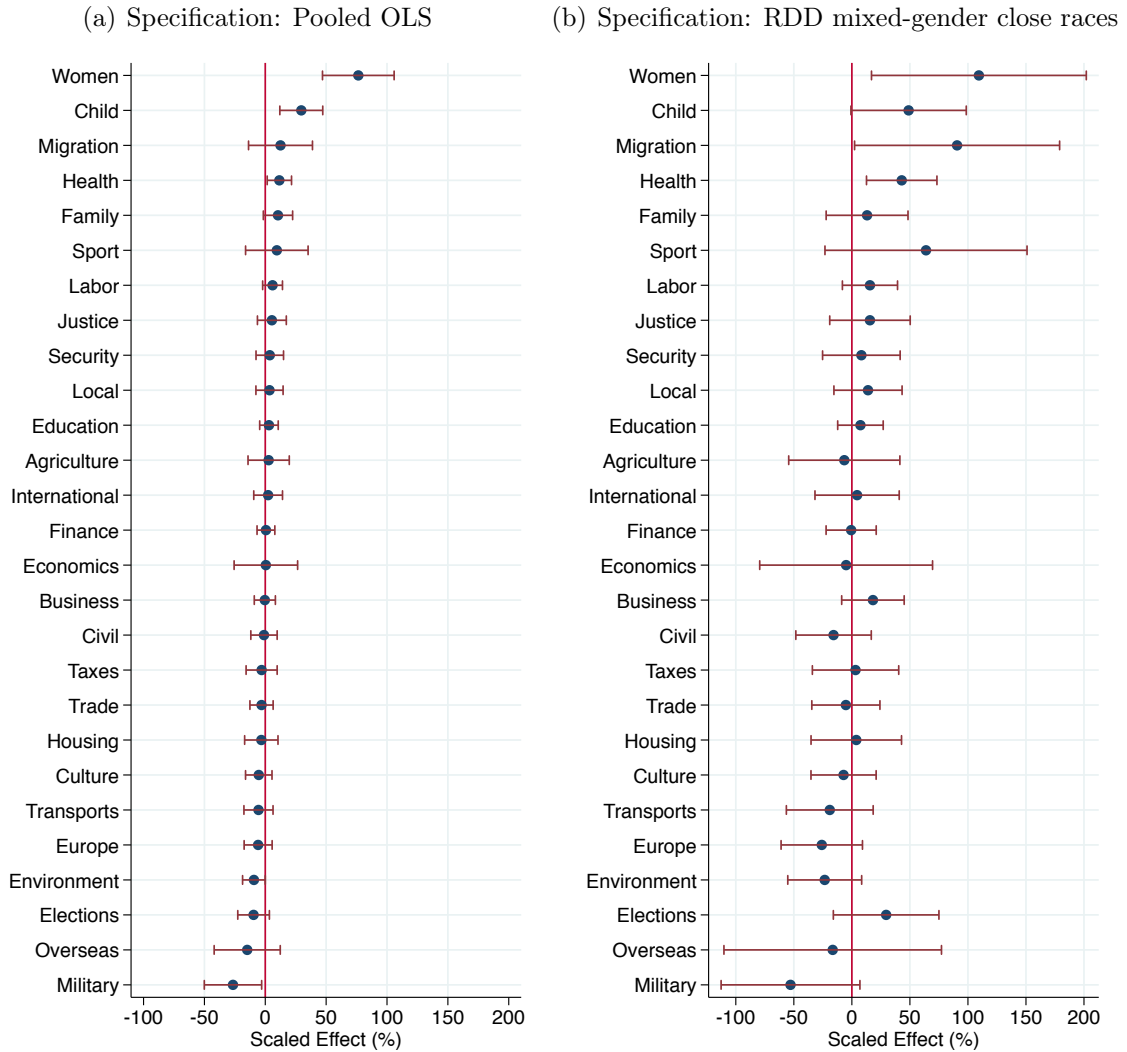
Notes: the data comes from the 2002, 2007 and 2012 elections for the French Lower House.

Figure 2.2: Legislator's Gender and Authorship of Women-related Amendments



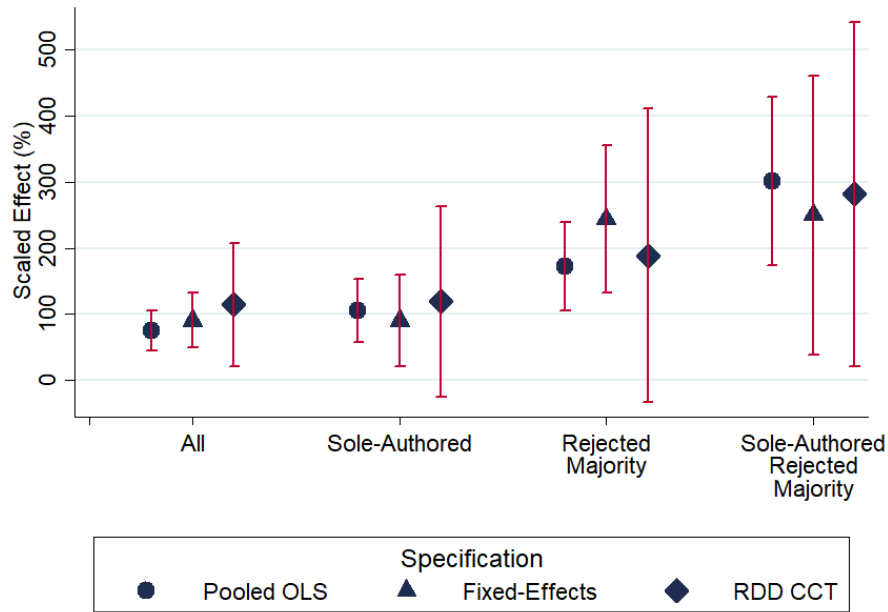
Notes: the data comes from the French Lower House during the period 2002-2017. The outcome is a dummy that equals 1 if the legislator initiates at least one women-related amendment. The x-axis represents the vote margin between the first woman and the first man in a mixed-gender election. On the right side of the vertical dashed line, a woman is elected whereas on the left it is a man. The solid lines correspond to a lowess fit of the bin-averages. There are 10 bins on each side of the cutoff.

Figure 2.3: Extension to Other Topics: Authorship Analysis



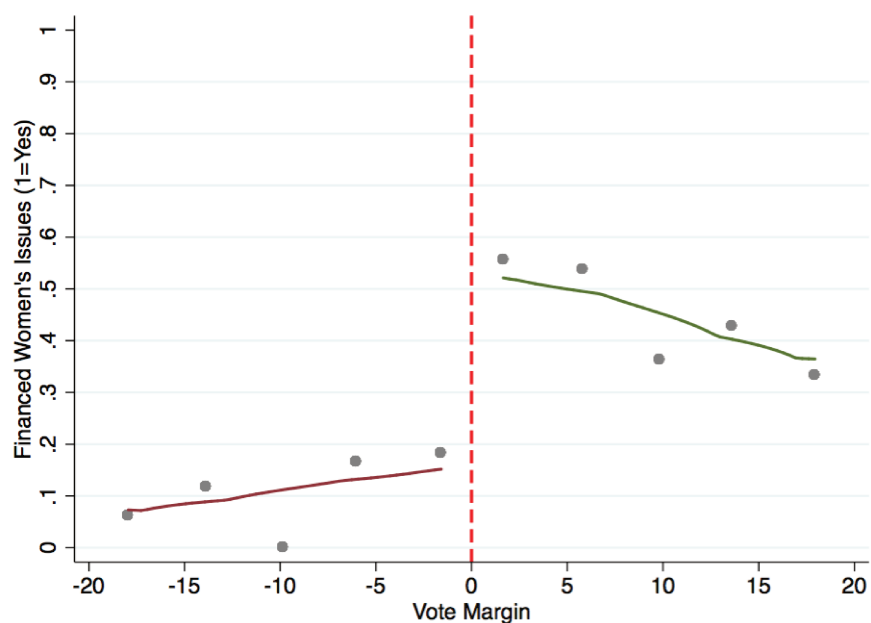
Notes: the data comes from the French Lower House during the period 2002-2017. Each row corresponds to a topic. The outcome is a dummy that equals 1 if the legislator initiates at least one amendment on the topic considered. Each dot represents the coefficient associated to the variable *Woman* divided by the average of male legislators (scaled effect). Confidence intervals are represented at the 95% level. Graph (a) and (b) respectively represent estimates from the pooled OLS specification and the RDD mixed-gender close race with the CCT bandwidth.

Figure 2.4: Investigating the Individual Interest Channel



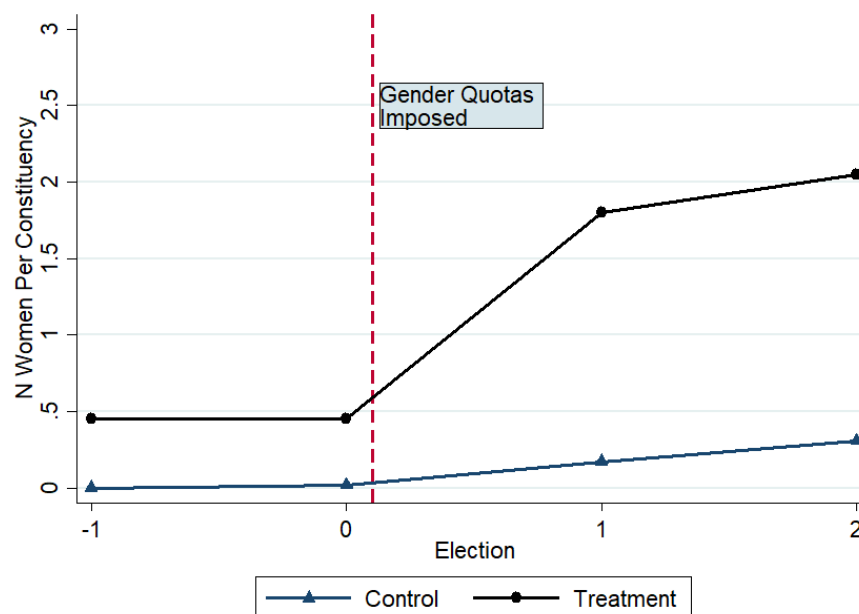
Notes: the data comes from the French Lower House during the period 2002-2017. The y-axis represents the coefficient associated to the variable *Woman* divided by the average of male legislators (scaled effect) in a regression where the outcome is a dummy that equals 1 if the legislator initiates at least one women-related amendment. Confidence intervals are represented at the 95% level. Sole-authored designates the sample restricted to sole-authored amendments. Rejected majority designates the sample restricted to amendments ultimately rejected whose author is from the majority.

Figure 2.5: Legislator's Use of Discretionary Funds for Women's Issues



Notes: the data comes from the 2012 elections for the French Lower House. The outcome is a dummy that equals 1 if the legislator has funded associations or projects related to women's issues. The x-axis represents the vote margin between the first woman and the first man in a mixed-gender election. On the right side of the vertical dashed line, a woman is elected whereas on the left it is a man. The solid lines correspond to a lowess fit of the bin-averages. There are 5 bins on each side of the cutoff.

Figure 2.6: First-Stage - Impact of the Gender Quota on the Number of Female Senators in the Upper House



Notes: the data comes from the 1988-2017 French Upper House. The y-axis represents the average number of women elected per constituency. The x-axis represents the election dates. Circles and triangles respectively designate the average number of women per constituency for those that have to comply with the quota and those that do not need to. The vertical red-dashed line corresponds to the time where gender quotas were introduced.

10 Tables

Table 2.1: Most Frequent Trigrams and Bigrams in the Sample of Amendments Related to Women's Issues

(1)	(2)	(3)	(4)	(5)	(6)
<i>Trigrams</i>			<i>Bigrams</i>		
Rank	N	Keywords	N	Keywords	
1	166	equality wom men	425	wom men	
2	63	violenc done wom	192	men wom	
3	47	worker part time	187	part time	
4	45	high council equality	166	equality wom	
5	36	equality men wom	94	equality professional	

Notes: the data comes from all the amendments produced produced in the Lower House during the period 2002-2017. It is restricted to amendments identified as related to women's issues with a dictionary-based method. The word "wom" is the stem of words such as women or woman.

Table 2.2: Testing the Continuity Assumption

	(1) Discontinuity Estimate	(2) Bandwidth Restriction	(3) N Observations
N Registered Voters	-11.855 (3417.295)	12.91	315
Abstention Rate	-.425 (1.172)	12	292
Invalid Vote Rate	-.082 (.132)	14.61	352
Total Population	230.221 (4800.727)	18.13	414
Population Male	-332.875 (2217.747)	19.13	434
Population Female	33.832 (2534.476)	16.6	393
Share Women Population	.002 (.002)	13.28	319
Share Working Women	-.008 (.013)	20.5	459
Share Working Age Population	.004 (.008)	12.43	299
Unemployment Rate	.004 (.006)	13.54	325
Female Vote Share	1.171 (1.297)	8.61	212
Female Vote Share T-1	-2.258 (7.974)	9.46	230
Left Wing Constituency	-.046 (.114)	9.82	239

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Each line corresponds to one dependent variable. Column 1 displays the discontinuity estimates, column 2 the bandwidth restrictions and column 3 the number of observations. The model fits a local linear regression around the cutoff that allows for a break in the slope at the cutoff. The bandwidth is computed following the CCT approach.

Table 2.3: Are there Gender Differences in Overall Parliamentary Activity - Lower House

Specification	(1) Pooled OLS	(2) Fixed Effects	(3) Poly	(4) <i>Regression</i> LLR IK	(5) <i>Discontinuity</i> LLR CCT	(6) LLR CCT/2
<i>Panel A: N Authored</i>						
Woman (1=Yes)	-4.99 (3.24)	-8.35 (5.67)	6.02 (6.11)	-2.06 (7.77)	0.26 (8.20)	9.10 (10.66)
Bandwidth Restriction			None	22.8	12.1	6.0
Observations	1663	1663	791	484	293	154
Constituencies	597	597	469	328	221	136
<i>Panel B: At Least One Authored</i>						
Woman (1=Yes)	0.01 (0.02)	0.04 (0.04)	0.09** (0.04)	0.08 (0.07)	0.07 (0.08)	0.09 (0.11)
Bandwidth Restriction			None	16.8	11.7	5.8
Observations	1663	1663	791	400	283	147
Constituencies	597	597	469	281	216	129

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. The dependent variable in Panel A is the number of amendments authored and a dummy equals to 1 if the legislator authors at least one amendment in Panel B. Controls in specifications of column 1 and 2 include the age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects. Controls in column 2 also include constituency fixed-effects. Controls in column 3 include a second order polynomial in the running variable. Specifications of columns 4, 5 and 6 fit a local linear regression around the cutoff that allows for a break in the slope at the cutoff using respectively the IK, the CCT and half the CCT bandwidth. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

Table 2.4: Authorship of Women-Related Amendments - Lower House

<i>Dep. Var.: At Least one Amendment Initiated (1=Yes)</i>						
Specification	(1) Pooled OLS	(2) Fixed Effects	(3) Poly	(4) <i>Regression Discontinuity</i> LLR IK	(5) LLR CCT	(6) LLR CCT/2
Woman (1=Yes)	0.17*** (0.03)	0.20*** (0.05)	0.25*** (0.06)	0.25*** (0.08)	0.22** (0.09)	0.32** (0.13)
Control Mean	0.22	0.22	0.19	0.21	0.20	0.19
Scaled Effect	76.4	89.9	128.0	120.0	109.5	166.5
Bandwidth Restriction			None	20.1	14.1	7.1
Observations	1663	1663	791	452	341	183
Constituencies	597	597	469	307	249	156

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Controls in specifications of column 1 and 2 include the age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects. Controls in column 2 also include constituency fixed-effects. Controls in column 3 include a second order polynomial in the running variable. Specifications of columns 4, 5 and 6 fit a local linear regression around the cutoff that allows for a break in the slope at the cutoff using respectively the IK, the CCT and half the CCT bandwidth. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

Table 2.5: Spending on Women's Issues - Lower House

<i>Dep. Var.: Legislator Has Funded Women's Issues (1=Yes)</i>					
Specification	(1) Pooled OLS	(2) Poly	(3) <i>Regression Discontinuity</i> LLR IK	(4) LLR CCT	(5) LLR CCT/2
Woman (1=Yes)	0.29*** (0.05)	0.35*** (0.10)	0.41*** (0.14)	0.39*** (0.15)	0.39* (0.19)
Control Mean	0.18	0.16	0.12	0.13	0.13
Scaled Effect	154.8	215.6	328.7	299.4	297.4
Bandwidth Restriction		None	16.5	15.9	7.9
Observations	540	244	131	126	77
Constituencies	540	244	131	126	77

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2012-2017. Controls in specifications of column 1 include the age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency. Controls in column 2 include a second order polynomial in the running variable. Specifications of columns 3, 4 and 5 fit a local linear regression around the cutoff that allows for a break in the slope at the cutoff using respectively the IK, the CCT and half the CCT bandwidth. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

Table 2.6: First Stage Results: Impact of Gender Quotas on the Number of Female Senators - Upper House

<i>Dep. Var.: N Women Per Constituency</i>		
	(1)	(2)
Quota*Post Pool	1.23*** (0.24)	
Post Pool	0.22*** (0.05)	
Quota*Post 1		1.20*** (0.25)
Quota*Post 2		1.26*** (0.26)
Post 1		0.15*** (0.05)
Post 2		0.29*** (0.07)
Quota	0.48*** (0.15)	0.48*** (0.15)
F-Statistic	35.7	22.7
Observations	216	216
Constituencies	72	72

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2001-2017 French Upper House. Standard errors clustered at the constituency level are given in parentheses.

Table 2.7: Impact of Gender Quotas on Initiation of Women-Related Amendments - OLS and Wald Estimates in the Upper House

Dependent Variable	(1) N	(2) Share	(3) At least One (1=Yes)
<i>Panel A: OLS Estimates</i>			
N Women	0.17* (0.08)	0.01*** (0.00)	0.12*** (0.04)
Observations	216	216	216
Constituencies	72	72	72
<i>Panel B: Wald Estimates</i>			
N Women	0.39** (0.16)	0.02** (0.01)	0.28** (0.13)
Observations	216	216	216
Constituencies	72	72	72

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes French Upper House during the period 2001-2017. Standard errors clustered at the constituency level are given in parentheses. Panel A displays OLS estimates and Panel B Wald estimates.

11 Appendix

A Institutional Setting

Table 2.A1: Number and Share of Bills Adopted by Origin

Term	Total Number	Government	Representatives
1973 - 1978	562	486 (86.47%)	76 (13.53%)
1978 - 1981	259	229 (88.41%)	30 (11.59%)
1981 - 1986	524	499 (95.22%)	25 (4.78%)
1986 - 1988	176	143 (81.03%)	33 (18.97%)
1988 - 1993	428	395 (92.26%)	33 (7.04%)
1993 - 1997	392	343 (87.5%)	49 (12.5%)
1997 - 2002	432	351 (81.25%)	81 (18.75%)
2002 - 2007	290	212 (73.1%)	66 (26.9%)
2007 - 2012	404	328 (81.2%)	76 (18.8%)
2012 - 2017	449	339 (75.5%)	110 (24.5%)

Notes: the data comes from the Lower House during the period 1973-2017.

Table 2.A2: Number and Share of Amendments Adopted by Origin

Term	Total Number	Government	Representatives
2002 - 2007	2692	352 (13.1%)	2340 (86.9%)
2007 - 2012	12719	2528 (19.9%)	10191 (80.1%)
2012 - 2017	38660	2591 (6.7%)	36069 (93.3%)

Notes: the data comes from the Lower House during the period 2002-2017.

B Data

B.1 An Example of Amendment

Figure 2.B1 shows how an amendment is displayed on the Lower House website. The page contains several information such as the title of the bill on the top, the identity of the author and co-sponsors in the middle and the content along with the oral presentation motivating the adoption of the amendment in the bottom.

Figure 2.B1: Example of Amendment on the Lower House website

ART. 18 BIS N°58

ASSEMBLÉE NATIONALE
23 juin 2014

ÉGALITÉ ENTRE LES FEMMES ET LES HOMMES - (N° 2043) ← **Bill's title**

Commission	
Gouvernement	

REJETÉ

AMENDEMENT N°58

présenté par
Mme Nachury, Mme Guégot et Mme Zimmermann ← **Author and Co-sponsors**

ARTICLE 18 BIS ← **Content**

Après l'alinéa 4, insérer les deux alinéas suivants :

« 4° Le deuxième alinéa de l'article L. 5211-10 est complété par une phrase ainsi rédigée :

« L'écart entre le nombre des vice-présidents de chaque sexe des établissements publics de coopération intercommunale à fiscalité propre et des métropoles ne peut être supérieur à un. ».

EXPOSÉ SOMMAIRE ← **Presentation**

L'égalité de représentation entre les femmes et les hommes doit être envisagée pour l'intercommunalité également. C'est aussi un amendement de cohérence avec l'obligation de parité des exécutifs départementaux introduite par la loi du 17 mai 2013. Si l'objectif du présent projet de loi est bien de mettre en oeuvre une politique pour l'égalité entre les femmes et les hommes, il est nécessaire de ne pas « rester au milieu du gué » et de prévoir le respect de ce principe dans et par l'État, les collectivités territoriales, ainsi que leurs établissements publics.

Notes: this figure comes from the Lower House website at <http://www.assemblee-nationale.fr/14/amendements/2043/AN/58.asp>.

This amendment is related to the bill entitled *Equality between Women and Men*. Its content consists in adding the following sentence to the bill: "*the gap between the number of vice-presidents of each sex in a Public Institution of Intercommunal Cooperation*⁴² *cannot be higher than one*".

The motivation is that: *The equality of representation between women and men should be considered at the intercommunal level. This is also an amendment of coherence with the obligation of parity among departmental executives introduced by the law of 17th may 2013. If the objective of the present bill is really to set a public policy in favor of equality between*

⁴²These institutions consist of gathering of small municipalities which coordinate their public policies and set similar level of tax.

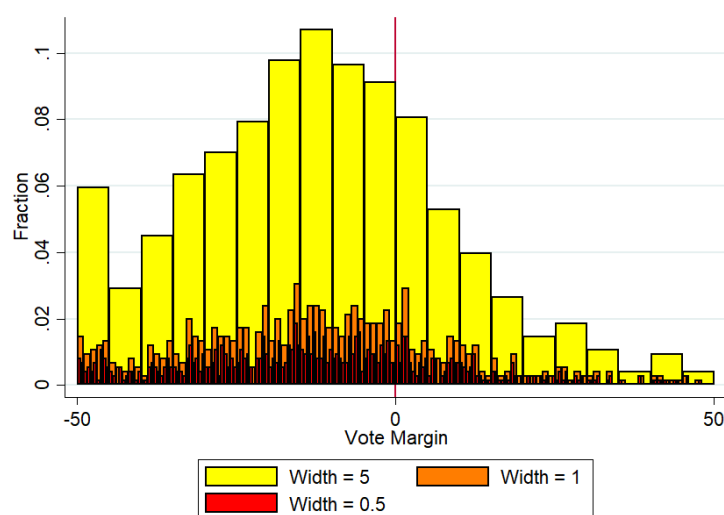
women and men, it is necessary to finish what was started and to provide the respect of this principle in and by the State, the local authorities as well as the public organisms.

C Empirical Strategy

C.1 Internal Validity Tests for the Regression Discontinuity Design

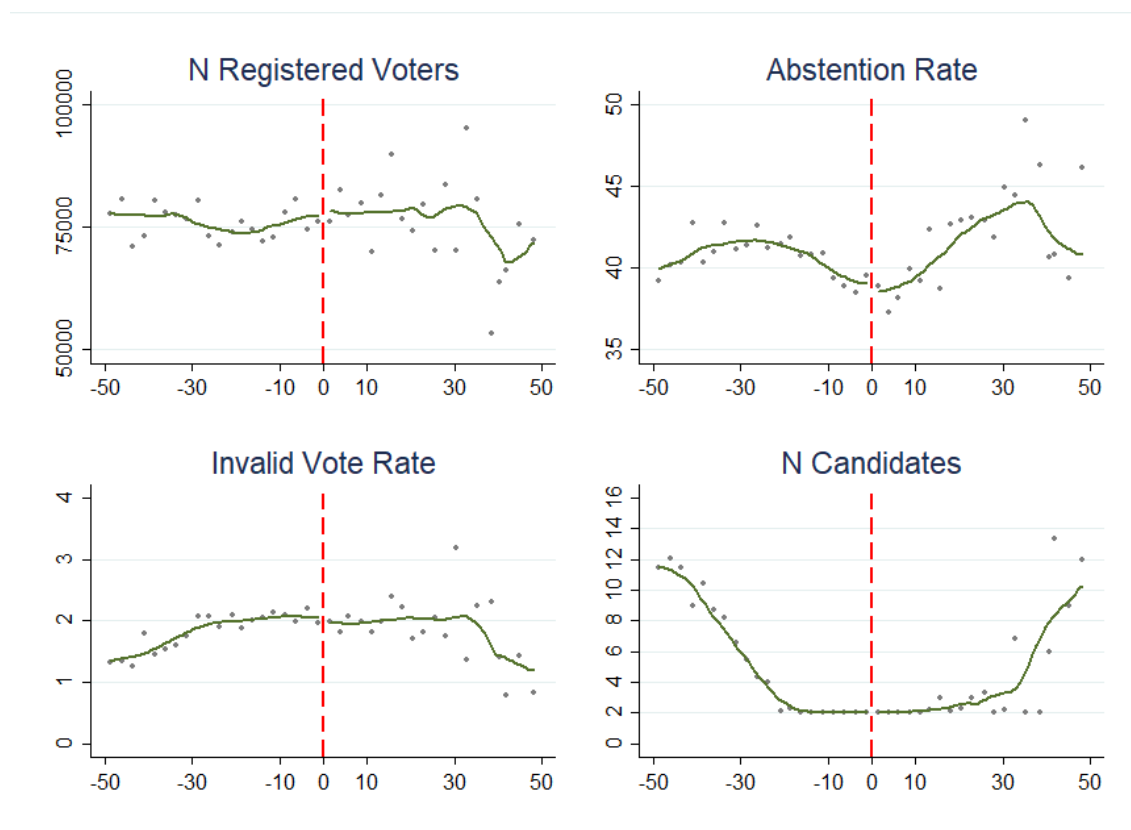
This section provides further internal validity tests for the regression discontinuity design. Figure 2.C1 displays the histogram of the running variable density. Figure 2.C2, 2.C3 and 2.C4 display the graph showing the relationship between potential confounders and the running variable.

Figure 2.C1: Histogram Density



Notes: The data comes from the 2002, 2007 and 2012 elections for the Lower House. The sample is restricted to mixed-gender close races.

Figure 2.C2: Continuity Assumption - Election Characteristics



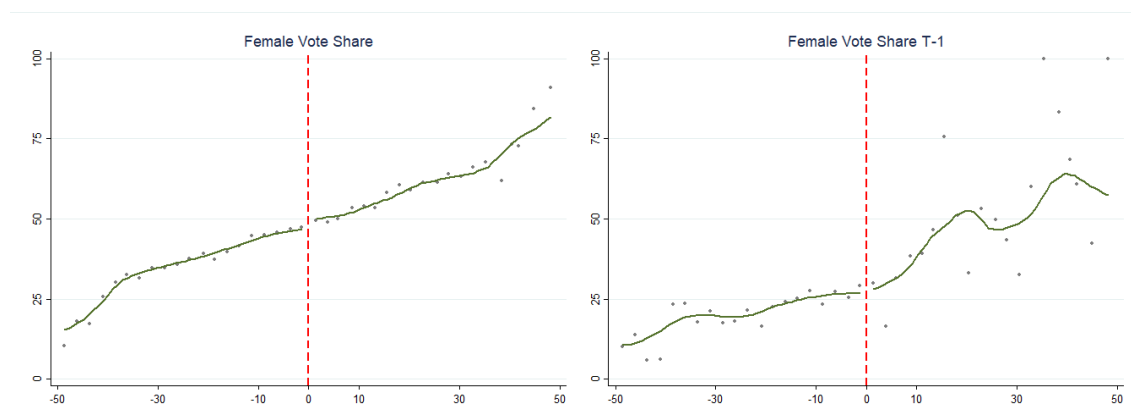
Notes: The data comes from the 2002, 2007 and 2012 elections for the Lower House. The sample is restricted to mixed-gender close races.

Figure 2.C3: Continuity Assumption - Demographics Characteristics



Notes: The data comes from the 2002, 2007 and 2012 elections for the Lower House. The sample is restricted to mixed-gender close races.

Figure 2.C4: Continuity Assumption - Preferences for Women



Notes: The data comes from the 2002, 2007 and 2012 elections for the Lower House. The sample is restricted to mixed-gender close races.

C.2 Descriptive Statistics

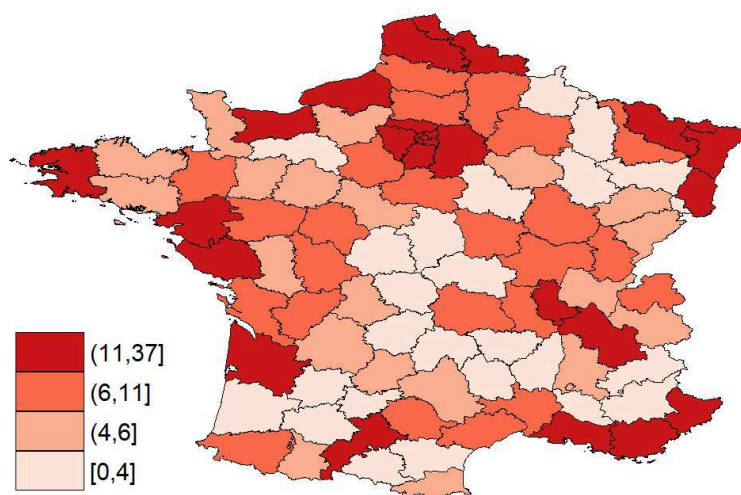
Table 2.C1: Descriptive Statistics on Legislators' Characteristics and their Activity Related to Amendments

	(1)	(2)	(3)	(4)
	Mean	S.D.	Min	Max
Woman (1=Yes)	0.19	0.39	0	1
Age	54.60	8.88	23	81
Left-Wing (1=Yes)	0.43	0.50	0	1
Incumbent (1=Yes)	0.58	0.49	0	1
Victory Margin	16.72	15.10	0	100
N Co-Sponsored Per Year	212.74	295.67	0	2228
N Authored Per Year	25.74	53.84	0	622
Dummy Authored (1=Yes)	0.87	0.34	0	1
N Co-Sponsored Per Year Women-Related	4.61	8.40	0	106
Share Co-Sponsored Women-Related	0.03	0.05	0	1
N Authored Per Year Women-Related	0.39	1.99	0	49
Share Authored Women-Related	0.02	0.08	0	1
Dummy Authored Women-Related (1=Yes)	0.27	0.44	0	1
Observations	1663			

Notes: the data comes from the Lower House during the period 2002-2017. *Women – Related* indicates that the amendment is identified as related to women's issues with a dictionary-based method. Dummy Authored Women-Related is a variable that is equal to 1 if the legislator has authored at least one amendment related to women's issues.

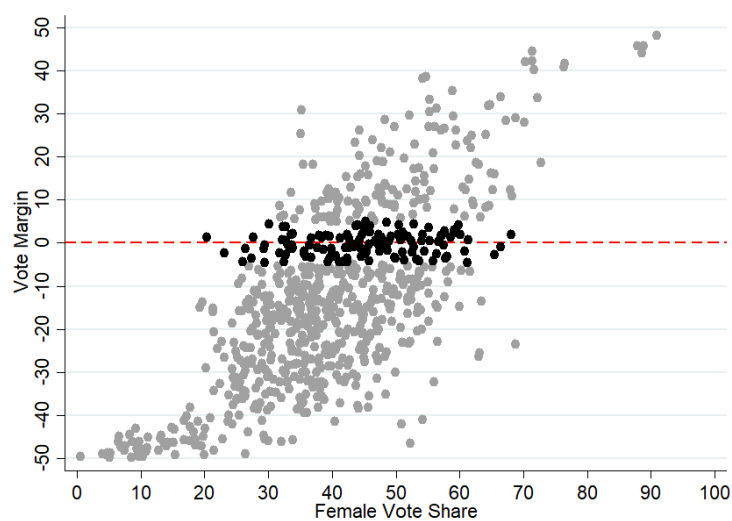
Figure 2.C5 depicts the distribution of the observations on the French territory. Importantly, we see that they are scattered throughout the territory with a larger concentration in the north and east of France.

Figure 2.C5: Where Are the Close-Races?



Notes: the data comes from the 2002, 2007 and 2012 Lower House elections. Each class represents a quantile of the distribution of observations.

Figure 2.C6: Preferences for Women in Close Race Elections



Notes: the data comes from the 2002, 2007 and 2012 Lower House elections. The graph represents the total vote share of women on the x-axis and vote margin (forcing variable) on the y-axis. The black dots are located in a 5 points interval around the elimination threshold.

Table 2.C2: Comparison Between the Samples of mixed gender races and single gender races

	(1) Single-Gender	(2) Mixed-Gender	(3) Difference (1) - (2)
N Registered Voters	75226.440	76054.523	-828.083 (769.419)
Abstention Rate	40.332	40.484	-0.152 (0.385)
Invalid Vote Rate	2.578	1.888	0.690*** (0.064)
Total Population	111019.242	112980.475	-1961.232 (1199.162)
Population Male	53840.456	54719.066	-878.610 (585.647)
Population Female	57178.846	58261.421	-1082.575* (617.269)
Share Women Population	0.515	0.516	-0.001 (0.000)
Share Working Women	0.617	0.637	-0.020*** (0.004)
Share Working Age Population	0.642	0.645	-0.004** (0.002)
Unemployment Rate	0.098	0.092	0.006*** (0.002)
Female Vote Share	22.233	39.289	-17.056*** (0.981)
Left Wing Constituency	0.407	0.369	0.037 (0.024)
Observations	872	791	1663

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Each line corresponds to variable. Column 1 displays the mean for the sample of single gender races, column 2 the mean for the sample of mixed-gender races and column 3 the difference between column 1 and 2.

D Additional Results

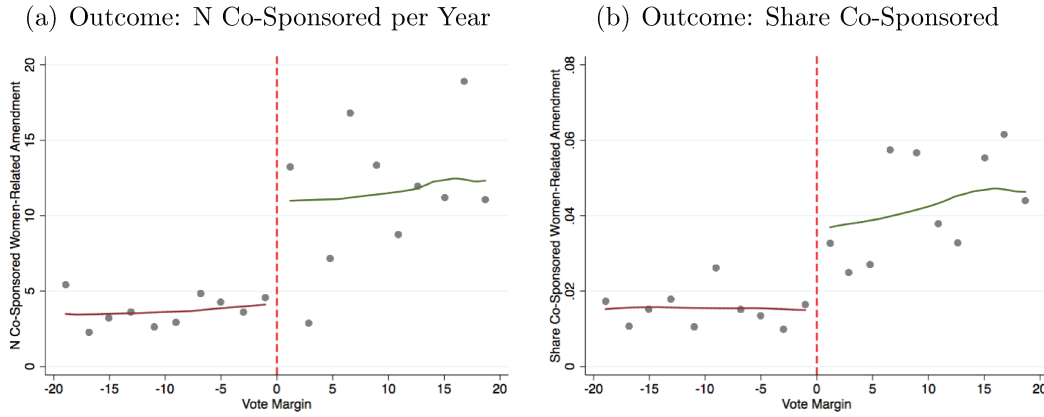
D.1 Co-Sponsorship Activities

In addition to authoring, legislators can contribute to an amendment by co-sponsoring it. Arguably co-sponsoring amendments requires less effort than authoring but it is still a clear indicator of interest which adds political weight to the amendment and increase its chances to pass. In the French Parliament, there is no limit on the number of co-sponsors an amendment can have.

I build two outcomes related to co-sponsorship. The first is the total number of co-sponsorship a legislator makes to women-related amendments. Since some parliamentarians may co-sponsor a large number of amendments on every topic, total numbers may not be informative of a specific interest for women's issues. Therefore, the second outcome is the number of co-sponsors for women-related amendments divided by the total number of co-sponsored amendments. This second outcome informs us on the relative interest a legislator attributes to the topic.

Figure 2.D1 displays the main discontinuity graphs. We observe a sizeable jump in the number (graph a) and the share of co-sponsored women-related amendments (graph b). Table 2.D1 confirms the visual impression. Panel A and Panel B respectively display results using the number and the share of co-sponsored women-related amendments as outcomes. In Panel A, we see that female legislators tend to co-sponsor 5 additional women-related amendments per year. Using the share of amendments as outcome, in Panel B, we see that the larger number of co-sponsored amendments translates into a jump in the share of about 1 to 2 percentage points. Scaling these effects to the average outcome of male legislators, we see that women co-sponsor about 140% (Column 3 Panel A) to 250% (column 5 panel A) additional women-related amendments which translates in an increase of about 100% of the share (from 185% in column 1 to 87% in column 5 of Panel B).

Figure 2.D1: RDD Graph Co-Sponsorship



Notes: the data comes from the French Lower House during the period 2002-2017. The outcomes are respectively the number (a) and the share (b) of women-related amendments co-sponsored by the legislator. The x-axis represents the vote margin between the first woman and the first man in a mixed-gender election. On the right side of the vertical dashed line, a woman is elected whereas on the left it is a man. The solid lines correspond to a lowess fit of the bin-averages. There are 10 bins on each side of the cutoff.

Table 2.D1: Co-Sponsorship of Women-Related Amendments - Lower House

Specification	(1) Pooled OLS	(2) Fixed Ef- fects	(3) Polynomial	(4) <i>Regression Discontinuity</i> LLR IK	(5) LLR CCT
<i>Panel A: N Co-Sponsored</i>					
Woman (1=Yes)	5.25*** (0.75)	6.81*** (1.05)	7.96*** (1.56)	5.53*** (2.10)	5.62** (2.45)
Control Mean	3.46	3.31	3.20	3.91	3.91
Scaled Effect	151.8	205.4	248.8	141.6	143.7
Bandwidth Restriction			None	27.3	11.7
Observations	1663	1663	791	554	286
Constituencies	597	597	469	370	217
<i>Panel B: Share Co-Sponsored</i>					
Woman (1=Yes)	0.03*** (0.01)	0.02*** (0.01)	0.02*** (0.01)	0.02*** (0.01)	0.01* (0.01)
Control Mean	0.02	0.02	0.02	0.02	0.02
Scaled Effect	182.3	124.2	118.1	119.3	88.4
Bandwidth Restriction			None	14.1	11.1
Observations	1663	1663	791	341	274
Constituencies	597	597	469	249	211

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Controls in specifications of column 1 and 2 include the age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects. Controls in column 2 also include constituency fixed-effects. Controls in column 3 include a second order polynomial in the running variable. Specifications of columns 4 and 5 fit a local linear regression around the cutoff that allows for a break in the slope at the cutoff. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

D.2 Heterogeneity Depending on the Outcome of the Amendment

This section investigates the heterogeneity of the main result depending on the outcome of the amendment. Table 2.D2 displays the results. In Panel A, the dependent variable is defined only on the sample of rejected amendments whereas, in Panel B, it is defined

only on the sample of accepted amendments. We see that in both cases, female legislators are significantly more likely to initiate women-related amendments. The scaled-effects are larger for the sample of accepted amendments.

Table 2.D2: Authorship of Women-Related Amendments Depending on the Outcome - Lower House

<i>Dep. Var.: At Least one Amendment Initiated (1=Yes)</i>					
Specification	(1) Pooled OLS	(2) Fixed Ef- fects	(3) Polynomial	(4) LLR IK	(5) LLR CCT
<i>Panel A: Rejected Amendments</i>					
Woman (1=Yes)	0.18*** (0.03)	0.21*** (0.05)	0.23*** (0.06)	0.23*** (0.08)	0.24*** (0.09)
Control Mean	0.20	0.20	0.17	0.20	0.20
Scaled Effect	91.0	106.8	134.9	115.3	121.2
Bandwidth Restriction			None	21.5	16.5
Observations	1663	1663	791	477	394
Constituencies	597	597	469	325	278
<i>Panel B: Accepted Amendments</i>					
Woman (1=Yes)	0.10*** (0.03)	0.12*** (0.04)	0.14*** (0.05)	0.13** (0.06)	0.16** (0.07)
Control Mean	0.07	0.07	0.06	0.06	0.04
Scaled Effect	152.8	174.1	247.6	233.3	408.3
Bandwidth Restriction			None	15.3	10.6
Observations	1663	1663	791	723	258
Constituencies	597	597	469	436	201

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Panel A is restricted to rejected amendments. Panel B is restricted to accepted amendments. Controls in specifications of column 1 and 2 include the age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects. Controls in column 3 include a second order polynomial in the running variable. Specifications of columns 4 and 5 fit a local linear regression around the cutoff that allows for a break in the slope at the cutoff. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

D.3 Heterogeneity Depending on Legislators' Characteristics

This section investigates the heterogeneity of the main result depending on the legislators' characteristics. Three characteristics are considered: the political inclination (left or right-wing), the incumbency status and the age at the beginning of the term. The results are displayed in Tables 2.D3, 2.D4 and 2.D5 for the pooled ols and the fixed-effects specifications.

We observe little heterogeneity depending on these characteristics. Female legislators, whether they are left or right-wing, incumbent or inexperienced or young, are significantly more likely to initiate women-related amendments than their male counterparts. It seems that the gender gap is larger among right-wing legislators than among the leftist ones. This is because the leftist male legislators contribute significantly more to this topic than their male counterparts from the right-wing. Regarding the role of incumbency, the gender gap seems higher among incumbents than among inexperienced legislators.

Table 2.D3: Authorship of Women-related Amendments - Heterogeneity Depending On Political Inclination

Specification	(1) Pooled OLS	(2) Pooled OLS	(3) Pooled OLS	(4) Fixed- Effects	(5) Fixed- Effects	(6) Fixed- Effects
Woman (1=Yes)	0.24*** (0.05)	0.11*** (0.04)	0.25*** (0.05)	0.21** (0.10)	0.27*** (0.07)	0.28*** (0.08)
Left-Wing (1=Yes)			0.05* (0.03)			0.04 (0.05)
Woman*Left-Wing			-0.13** (0.07)			-0.13 (0.10)
Sample Restriction	Right- Wing	Left- Wing	All	Right- Wing	Left- Wing	All
Control Mean	0.19	0.28		0.19	0.28	
Scaled Effect	128.6	41.4		111.2	98.0	
Observations	943	720	1663	943	720	1663
Constituencies	415	367	597	415	367	597

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Controls include the age at the beginning of the term, the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects. Controls in columns 4, 5 and 6 also include constituency fixed-effects. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

Table 2.D4: Authorship of Women-related Amendments - Heterogeneity Depending On Incumbency Status

Specification	(1) Pooled OLS	(2) Pooled OLS	(3) Pooled OLS	(4) Fixed- Effects	(5) Fixed- Effects	(6) Fixed- Effects
Woman (1=Yes)	0.14*** (0.04)	0.20*** (0.05)	0.14*** (0.04)	0.20*** (0.06)	0.47*** (0.12)	0.17*** (0.05)
Incumbent (1=Yes)			-0.03 (0.02)			-0.03 (0.03)
Woman*Incumbent			0.07 (0.06)			0.13** (0.07)
Sample Restriction	New	Incumbent	All	New	Incumbent	All
Control Mean	0.25	0.21		0.25	0.21	
Scaled Effect	56.1	98.2		80.5	224.1	
Observations	698	965	1663	698	965	1663
Constituencies	474	532	597	474	532	597

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Controls include the age at the beginning of the term, the political inclination (left or right-wing), the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects. Controls in columns 4, 5 and 6 also include constituency fixed-effects. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

Table 2.D5: Authorship of Women-related Amendments - Heterogeneity Depending On Age

Specification	(1) Pooled OLS	(2) Pooled OLS	(3) Pooled OLS	(4) Fixed- Effects	(5) Fixed- Effects	(6) Fixed- Effects
Woman (1=Yes)	0.17*** (0.04)	0.17*** (0.05)	0.16*** (0.04)	0.20** (0.08)	0.24** (0.10)	0.20*** (0.05)
AgeAboveMedian (54)			-0.06*** (0.02)			-0.05 (0.03)
Woman*AgeAbMedian			0.01 (0.06)			0.03 (0.07)
Age Above Median	No	Yes	All	No	All	All
Control Mean	0.25	0.20		0.25	0.20	
Scaled Effect	66.0	85.8		79.8	121.9	
Observations	839	824	1663	839	824	1663
Constituencies	472	451	597	472	451	597

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Controls include the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects. Controls in columns 4, 5 and 6 also include constituency fixed-effects. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

E Robustness

E.1 Alternative Dictionary of Women's Issues

To identify amendments on women's issues, the main dictionary contained words whose stemmed version was "wom", "sex" and "gender". This constituted the "exhaustive" definition of women's issues as it contained different words referring to women. Yet, in French language, it is much more common to refer to women's issues by using words starting with "wom" such as woman⁴³. In the sample, "wom" occurs 5,554 times while "sex" and "gender" respectively occur 815 and 560 times.

I thus build a restrictive definition of women's issues by restricting the dictionary to only one word: "wom". This alternative procedure selects 3,285 amendments. Using this measure, I replicate the main results on authorship and co-sponsorship. Table 2.E1 displays the results. They are essentially similar to the ones obtained with the exhaustive definition. In Panel A, we see that female legislators are about twice more likely to initiate at least one amendment on the topic. In Panel B, we observe that female legislators co-sponsor a larger share of women-related amendments but the precision of the estimations is lower than the one obtained with the exhaustive definition.

⁴³For instance the minister responsible for women's issues is officially in charge of *Equality between women and men*. Similarly the public organism working on these issues is the *High Council for Equality between women and men*

Table 2.E1: Authorship of Women-Related Amendments - Restrictive Definition

<i>Dep. Var.: At Least one Amendment Authored (1=Yes)</i>						
Specification	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled OLS	Fixed Effects	<i>Regression Discontinuity</i>			
			Poly	LLR IK	LLR CCT	LLR CCT/2
Woman (1=Yes)	0.16*** (0.03)	0.17*** (0.05)	0.25*** (0.06)	0.26*** (0.08)	0.24** (0.10)	0.31** (0.13)
Control Mean	0.20	0.20	0.17	0.20	0.19	0.17
Scaled Effect	78.8	84.4	142.5	128.5	124.2	175.5
Bandwidth Restriction			10.6	13.8	13.8	6.9
Observations	1663	1663	791	473	336	178
Constituencies	597	597	469	322	246	153

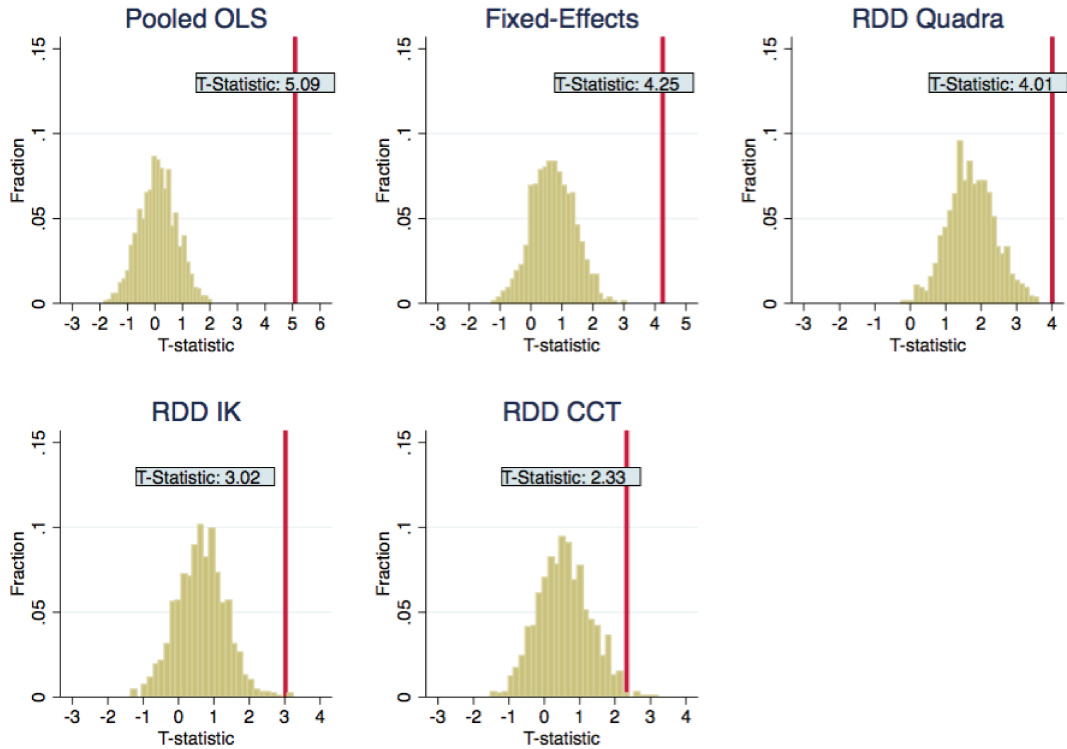
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Outcomes are respectively for Panel A, a dummy that equals 1 if the legislator authored at least one amendment and, for Panel B, the share of women-related amendments co-sponsored. Controls in specifications of column 1 and 2 include the age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects. Controls in column 2 also include constituency fixed-effects. Controls in column 3 include a second order polynomial in the running variable. Specifications of columns 4, 5 and 6 fit a local linear regression around the cutoff that allows for a break in the slope at the cutoff. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

E.2 Placebo - Random Samples

This section designs a placebo test assessing the uniqueness of the results obtained with the sample of women-related amendments. This test attempts to determine the probability to obtain similar findings with random sample of amendments. Specifically, I drew 1,000 samples of 3,744 amendments. I then built a dummy equal to one if a woman initiated one of these amendments. Using this outcome, I used the usual five specifications (pooled ols with and without constituency fixed-effects and three regression discontinuity specifications) and plotted the obtained T-statistics. The results are displayed in Figure 2.E1 for the authorship outcome. In a nutshell, out of the 1,000 random samples of amendments, none delivers consistent findings across the five specifications as the ones observed with the

sample of women-related amendments.

Figure 2.E1: Placebo Test - Random Sample of Amendments Authorship Outcome



Notes: The data comes from the French Lower House during the period 2002-2017. The histograms represent the T-statistic associated to the coefficient *Woman* in the five different specifications used in the paper (pooled ols without constituency fixed-effects, with constituency fixed-effects, mixed-gender close races using a quadratic specification, the IK and the CCT bandwidth). The outcome is a dummy equals to 1 if the legislator has initiated at least one amendment related to the random sample of amendment drawn. There are 1000 samples constituted of 3,744 randomly drawn amendments. The T-statistic box included in each graph displays the t-stat obtained with the sample of amendments classified as women-related..

E.3 Outcome Previous Election

Restricting the sample to narrow mixed-gender close races should provide exogenous variations in the sex of the legislator. Therefore, we should not observe similar results when using lagged outcomes, otherwise this would mean that the activity of legislators persists. Table 2.E2 displays the results using as outcome variable a dummy equals to one if the previous legislator initiated at least one women-related amendment. Reassuringly, coefficients are not significant anymore and largely shrink in magnitude. This suggests

that persistence in the outcome cannot explain the results.

Table 2.E2: Placebo Authorship of Women-Related Amendments

<i>Dep. Var.: At Least one Amendment Initiated in T-1(1=Yes)</i>			
	(1) Polynomial	(2) LLR IK	(3) LLR CCT
Woman in T (1=Yes)	0.06 (0.06)	0.07 (0.08)	0.01 (0.09)
Bandwidth Restriction	None	21.8	13.5
Observations	528	318	222
Constituencies	401	266	195

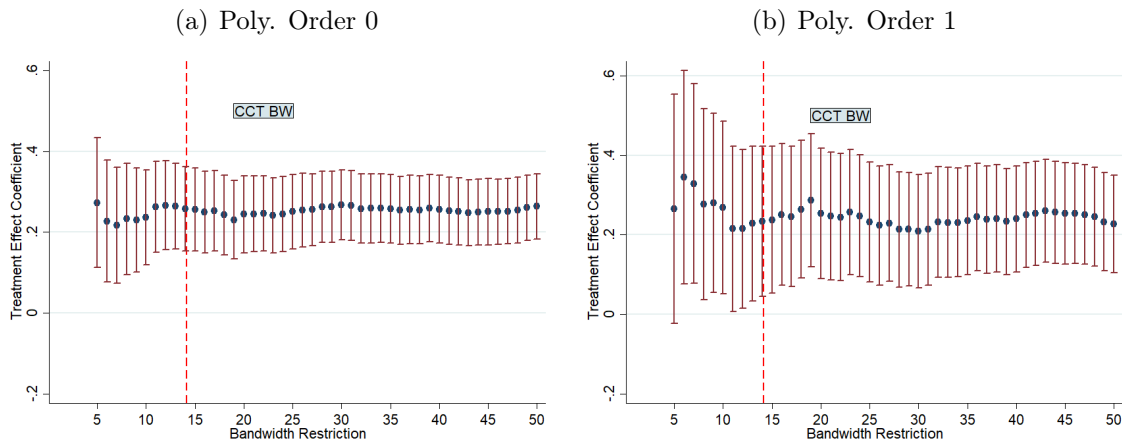
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. The outcome is a dummy that equals 1 if the legislator in T-1 from the same constituency authored at least one women-related amendment. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

E.4 Alternative Bandwidths

The choice of the bandwidth is crucial and while two methods are used (CCT and IK), one could wonder how sensitive the results are to the size of the bandwidth, especially to smaller ones. Figure 2.E2, 2.E3 and 2.E4 provide an answer to this question for the usual outcomes. I replicate the estimation for all the possible windows bounded by two integers including at least 50 observations (the smallest window is [-5;5]).

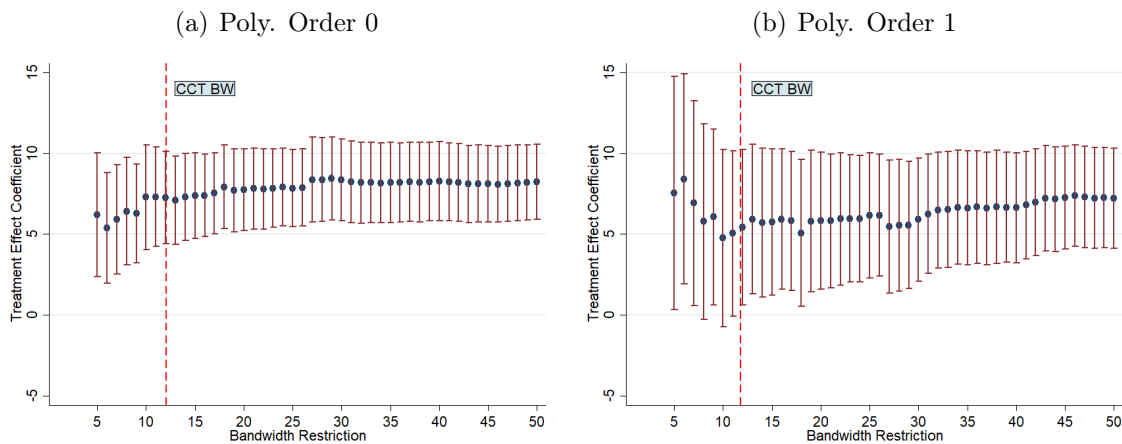
For the three outcomes, the coefficient is always positive and quite stable across the different bandwidths. Only the precision seems to diminish as the bandwidth is narrowed, which is expected since the number of observations also goes down. This is especially true when using the share of co-sponsored amendments as outcome along with a polynomial of degree 1.

Figure 2.E2: RDD Alternative Bandwidth Outcome: At Least One Amendment Initiated



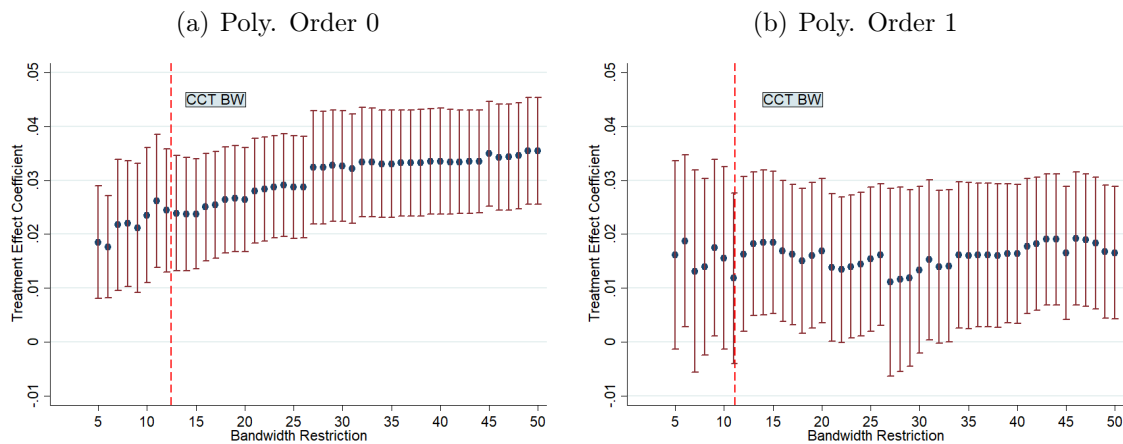
Notes: The data comes from the French Lower House during the period 2002-2017. The vertical axis represents the probability that a woman initiates at least one amendment related to women's issues relatively to a male legislator. Confidence intervals are represented at the 95% level. The vertical red dashed line represents the CCT bandwidth.

Figure 2.E3: RDD Alternative Bandwidth - Outcome: N Amendments Co-sponsored



Notes: The data comes from the French Lower House during the period 2002-2017. The vertical axis represents the additional number of amendments related to women's issues a woman co-sponsors relatively to a male legislator. Confidence intervals are represented at the 95% level. The vertical red dashed line represents the CCT bandwidth.

Figure 2.E4: RDD Alternative Bandwidth - Outcome: Share Amendments Co-Sponsored



Notes: The data comes from the French Lower House during the period 2002-2017. The vertical axis represents the additional share of amendments related to women's issues a woman co-sponsors relatively to a male legislator. Confidence intervals are represented at the 95% level. The vertical red dashed line represents the CCT bandwidth.

E.5 Local Randomization Strategy

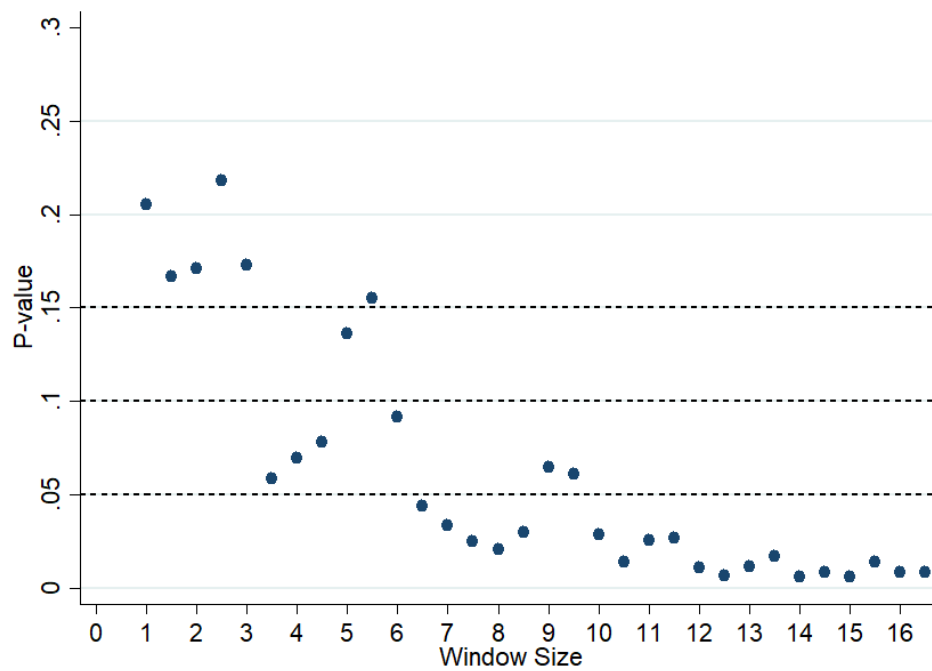
This section implements the so-called local randomization strategy. While the usual regression discontinuity design relies on the continuity of potential confounders around the threshold, this strategy is more stringent. It selects the largest window for which all the covariates are balanced on both sides of the threshold. Using this window, it tests for the difference in the outcome between the two sides of the threshold.

Figure 2.E5 displays the smallest p-value for all the covariates on a wide range of windows. The covariates are the number of candidates, the number of registered voters, the abstention rate, the invalid vote rate, the total population in the constituency, the total male population, the total female population, the share of women, the share of working women, the share of working age individuals, the unemployment rate, the female vote share during the last election, a dummy indicating that the constituency was won by the left-wing during the last election, the share of female and left-wing candidates. We see that the smallest p-value is below 0.15 (and 0.1) for nearly all the windows larger than $[-3;3]$. Using a threshold of 0.05, the selected window is $[-6;6]$.

Table 2.E3 displays the results for the difference in means of the outcome between the two sides of the cutoff. We see that when the p-value threshold for the balance test is 0.15

or 0.1, the selected window is $[-3;3]$. Using this window, the difference in means is 0.256, which is significant at the 5% level (the related p-value is 0.008 as shown in column 4). The 95% level confidence interval is $[0.093;0.465]$. The inference is done on a sample including 43 legislators on each side of the cutoff.

Figure 2.E5: P-values from Balance Tests over a Set of Windows



Notes: The data comes from the French Lower House during the 2002-2017 period. Each dot corresponds to the smallest p-value of all the covariates included in a balance test over a given window. The y-axis represents the value of the p-values. The x-axis represents the window sizes. The covariates are the number of candidates, the number of registered voters, the abstention rate, the invalid vote rate, the total population in the constituency, the total male population, the total female population, the share of women, the share of working women, the share of working age individuals, the unemployment rate, the female vote share during the last election, a dummy indicating that the constituency was won by the left-wing during the last election, the share of female and left-wing candidates. The horizontal dashed-lines represent the 0.15, 0.1 and 0.05 significance levels.

Table 2.E3: Randomization Inference: Difference in Means Test

Balance Test P-value Threshold	Window	Diff. in Means Stat	P-value	95% Confidence Interval	N Left Cutoff	N Right Cutoff
(1)	(2)	(3)	(4)	(5)	(6)	(7)
0.15	[-3;3]	0.256	0.008	[0.093;0.465]	43	43
0.1	[-3;3]	0.256	0.008	[0.093;0.465]	43	43
0.05	[-6;6]	0.231	0.000	[0.08;0.373]	89	65

Notes: the data comes from the Lower House during the period 2002-2017. The dependent variable is a dummy that equals 1 if the parliamentarian initiated at least one women-related amendment. Column 1 displays the p-value threshold regarding the balance test for all the covariates to determine the window. Column 2 displays the selected window. Column 3 displays the value of the Difference in Means statistics between the two sides of the cutoff. Column 5 displays the p-value related to the diff. in means statistics. Column 6 displays the confidence interval at the 95% level. Column 7 and 8 respectively show the number of observations used on the left and the right side of the cutoff.

F Details on Topic Classification

F.1 Procedure

This section brings further details on the topic classification. The procedure used is the following:

1. Create a list of topics of interest. This was done by using the usual government ministries that existed during the 2002-2017 period. This leads to 27 non-mutually exclusive topics.
2. Remove stop words and stem all the words in the amendments and the bills' title. Then, return the 10,000 most recurring words in the amendments. Practically, I selected the closest threshold to the 10,000th word which is 49 and above which are 9,967 words. I thus obtain a sample of 9,967 words which occur at least 49 times in the amendments. The bills' title contain 1712 words. I pooled the two samples of words and obtained a final sample of 10,030 unique words.
3. Manually classify the words in the 27 categories.
4. Classify the amendments into each of the 27 non-mutually exclusive categories. The rule is that if an amendment contains one of the keywords included in a category, it is classified as belonging to the category.
5. Using the sample of amendments, return the 10,000 most recurring bigrams for each category and select only those that contain one of the classified keywords. Using this narrower sample of bigrams, I tagged obvious false matches. I then cleaned the classification of amendments by excluding these false matches from the classification.

Table 2.F1: Details on Topic Classification - Part 1

Topic	Top 10 Keywords	5 Most Frequent Bigrams	5 Most Frequent Trigrams	Excluded Matches	False	Notes
Agriculture	agricul, agricultur, alimentar, farmer, fishing, hunt, breeding, food, fruit, methanised (58)	agricul exploit, local autorit, sea fish, plant protection, alimentary product	rural sea fish, greenhouse gas, natural agricul forest, space natural agricul, emiss greenhouse gas	duplicate hunt, will be aliment, aliment font, aliment height		aliment is used as a verb in other meanings.
Business	business, sme, capit, bank, banking, shareholder, employer, entrepreneur, dividend, multinational (4)	works council, million euro, turnover, entrepreneur, social security	organis professional employer, level nation interprofessional, solidarity social economy, employer social contribution, professional employer applic	work undertaken		the french word for business is "entreprise" which may also mean undertaken
Child	child, kindergarten, infant, baby, child pornography, child psychiatrist, pedophilia	family allowanc, parental autorit, child right, franc televis, child protection	best interest child, television channel advertis, suppress advertis televis, social action family, international child right	None		
Civil	civil servant	right oblig civil servant, territorial public service, deontology right oblig, cumulative employment retirement, magistrate judiciary order	public service, delegation management, compulsory scheme, employee private, court audit	None		in french, civil servant is a one-word expression. There is always a ministry for this topic.
Culture	cultur, cultural, audiovisual, televis, art, radio, spectacl, artistic, artist, cinema (26)	franc televis, public audiovisual, local autorit, public servic, million euro	high council audiovisual, television channel advertis, suspend acc internet, public service audiovisual, commiss protect right	scientific cultur, cultur mathematics, farmer culture, gmo cultur, cultur diversit (17)		cultur is a word used in other contexts, especially agriculture.
Economics	economic, economy, growth, gdp, inflat, conjunctur, cyclical, dollar, inflationnist, economist (3)	million euro, public servic, economic develop, local autorit, economic social	solidarity social economy, greenhouse gas, emiss greenhouse gas, environment social economic, court auctionneer	growth aliment, demographic growth, justic economi, energy saving, substantial saving, budgetary saving		The French verb "economiser" means saving.
Education	teach, academic, educ, study, middle school, school, degree, universit, educativ, university (25)	public servic, educ national, health instit, higher educ, million euro	private health instit, higher educ instit, higher educ research, mission public servic, hospital public servic	study possibilit, study article, free license, equal license, open license (19)		the french word for bachelor also means license.
Elections	elected, elect, ballot, constituenc, elector, voter, eligibility, senate elections, ineligible, mandatur (10)	local autorit, local elected, council communautar, new municipalit, municipal council	tax overtime, EPCI own tax, direct universal suffrag, local autorit gener, intermunicipalit public cooper	None		EPCI is a conglomerate of municipalities.
Environment	energy, environment, energetic, water, electricity, biodiversit, gas, mountain, environmental, ecologic (138)	local autorit, renewable energy, transit energetic, sustainabl develop, public servic	greenhouse gas, emiss greenhouse gas, biodiversit french agenc, rural sea fish, economic social environment	work environment, institutional landscap, economic landscap, concurrential environment, administrativ environment (2)		
Europe	european, europ, ESF	european union, state member, direct european, commiss european, european parliament	council european parliament, member european union, state member union, european human right, other state member	None		ESF is the European Social Fund.

Notes: the data comes from all the amendments produced produced in the Lower House during the period 2002-2017.

Table 2.F2: Details on Topic Classification - Part 2

Topic	Top 10 Keywords	5 Most Frequent Bigrams	5 Most Frequent Trigrams	Excluded Matches	False	Notes
Family	family, parent, familial, spouse, couple, mother, parenthood, mariage, famili, divorc (11)	famil allowanc, social securit, handicap person, million euro, parent authorit	social action famil, best interest child, person situat handicap, solidarit civil pact, reduced rate vat	famil neonicotinoid, herbicid famil, parent company, parent rock, joint exclus (10)		the french word for spouse can also mean joint
Finance	financ, financial, financi, budget, budgetar, deficit, PLF, PLFSS, LFSS, refinanc	million euro, social security, health institut, local authorit, public servic	financ social securit, intermunicipalit public cooper, mission public servic, private health instit, public health cooper	deficit care, deficit competitiv, deficit attract, deficit housing		plf, plfss, lfss are acronym for finance bills.
Health	health, care, doctor, diseases, patient, sanitar, medical, medica, handicap, medico (130)	health instit, public health, social securit, professional health, insuranc diseases	financ social securit, health private instit, person situat handicap, public servic hospital, care follow readapt	care examin, care delimit, care rule, leav care, animal health (20)		as in english care may also be used as take care of.
Housing	housing, hous, rent, building, tenant, lessor, habitat, locativ, HLM, ALUR (13)	social housing, social housing (singular), public instit, local authorit, build hous	rent social housing, intermunicipalit public cooper, institut public cooper, EPCI own fiscalit, build social housing	wild habitat, habitat species, natural habitat, rent movie, rent car (18)		HLM are social housing and ALUR is a bill on housing.
International	international, international (singular), AFD, internationaliz, humanitar, unesco, diplomatic, diplomat, genocid	million euro, child right, illegal trade, economic develop, international convent	schem region develop, international right convent, region develop economic, economic develop innov, economic innov internationaliz	None		AFD is the French Development Agency.
Justice	justic, judiciar, lawyer, notary, juridict, inmat, tribunal, magistrat, jail, court (34)	penal procedur, constitutional council, state council, court appeal, european union	court auctioneer, account personal prevent, personal prevent penibilit, jail euro fine, court justic union	social justic, fiscal justic, economic justic, share held, fish held		the french word for inmate also means held
Labor	work, salaried, job, employer, syndicate, worker, dismiss, labor, wage, unemploy (14)	social securit, million euro, employment contract, overtime work, professional format	organis professional employer, solidarity social economy, financ social securit, account personal format, level national interprofessional	syndicate energy, mixt syndicate, transport syndicate, parliamentary work, governmental work (9)		
Local	authorit, region, metropolit, regional, EPCI, regional (singular), intermunicipalit, department, department (singular), metropolitan (77)	local authorit, public instit, intermunicipalit cooper, own tax, public servic	intermunicipalit public cooper, public institut cooper, gener local authorit, EPCI own tax, cooper intermunicipalit tax	float authorit, ultramarine authorit		Authorit is translated by "collectivités" in French which is specific to this theme and shows the number of false matches.
Migration	asylum, immigr, border, OFPRA, refugee, stateless, migrant, naturalize, migr, migrator	asylum seeker, right asylum, ask asylum, waiting area, residence permit	stay foreign right, stay residence foreign, foreign right asylum, temporary residence permit, country origin safe	fish migrator, bird migrator, river migrator, migr fish, migr wildlife (5)		OFPRA is a public organism protecting refugees.

Notes: the data comes from all the amendments produced produced in the Lower House during the period 2002-2017.

Table 2.F3: Details on Topic Classification - Part 3

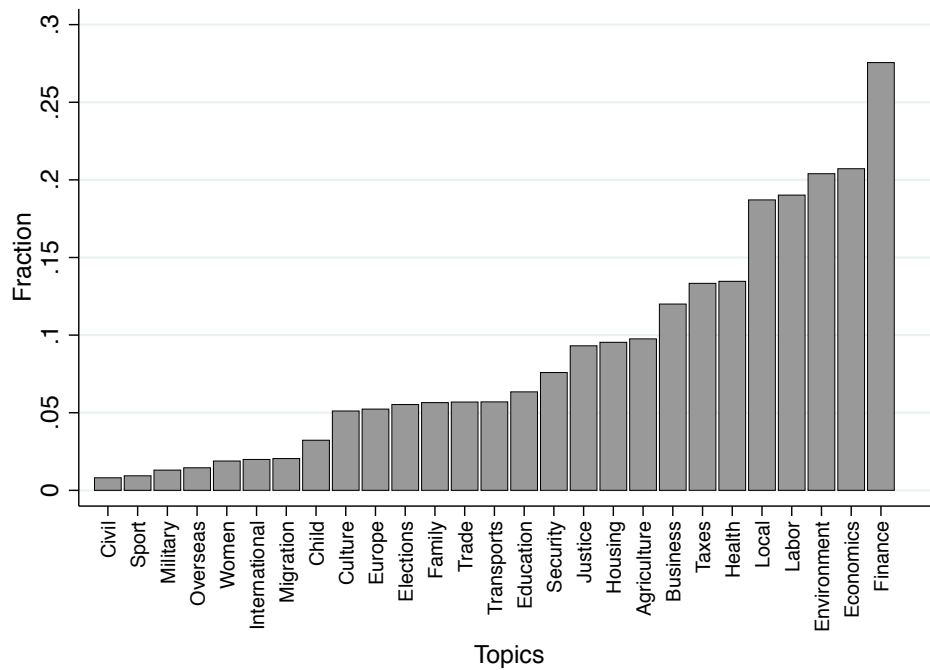
Topic	Top 10 Keywords	5 Most Frequent Bigrams	5 Most Frequent Trigrams	Excluded Matches	False	Notes
Military	militar, war, army, combat, weapon, soldier, armament, ONAC	veteran, armed force, penal constraint, civil right, civil statute	civil right statute, local civil right, day defense citizenship, armed force, action day defense	declaration war family, obstacle course, price war, collector weapon, truth armed (1)		In french obstacle course is translated as combat race. Onac is an organism helping veterans.
Overseas	corsica, guian, caledoni, polynesi, mayott, martiniqu, guadeloup, miquelon, futuna, antil, oversea (14)	local authorit, oversea, saint pierr, pierr miquelon, genetic resourc	saint pierr miquelon, local authorit corsica, department oversea, rural agricul develop, convent pass state	None		Keywords include names of oversea territories
Security	securit, polic, securis, violenc, delinqu, terrorism, crim, insecurit, terrorist, forgery (24)	penal procedur, judiciar polic, state member, constitutionnal council, million euro	suspend internet acc, violenc done wom, commiss protect right, statu civil right, jail euro fine	social securit, financ securit, alimentar securit, medical securit, fiscal insecurit		
Sport	sport, athletic, footbal, horse riding, doping, olympic, cyclist, hippodrom, hooliganism, uefa	million euro, bet onlin, gam onlin, local authorit, sport event	solidarity social economy, competit sport event, game bet online, gambling chance, nation develop sport	None		
Taxes	fiscal, tax, levy, tax system, VAT, fiscal (singular), taxat, CICE, ISF, tax exemption (20)	million euro, own tax, tax credit, gener tax, turnover	EPCI own tax, intermunicipalit public cooper, public instit cooper, tax intermunicipalit cooper, reduced rate vat			VAT, CICE, ISF are acronyms which stands for specific taxes.
Trade	trade, commerce, commercial, commerci, commercial (singular), export, customs, customs officer, import, exporter	million euro, trade industry, chamber commerc, public service, illegal trade	chamber commerc industr, clerk tribunal commerc, mission public service, decree state council, greenhouse gas	None		
Transports	transport, vehicle, train, airport, SNCF, automobil, auto, train station, car, carrier (35)	million euro, local authorit, public servic, organisat authorit, greenhouse	greenhouse gas, emiss greenhouse gas, regulat train activit, authorit organisat transport, EPCI own tax	legislative vehicle, pesticide vehicle, air pesticide, conductive thread, political driver (16)		
Women	wom, sex, gender	wom men, men wom, part time, equalit wom, professional equalit	equalit wom men, violenc done wom, work part time, high council equalit, equalit men wom	kind of offenses, unique kind, all kind, kind behavior, many kind (4)		the french word for gender also means genre or kind.

Notes: the data comes from all the amendments produced produced in the Lower House during the period 2002-2017.

F.2 Descriptive Statistics

Figure 2.F1 displays descriptive statistics on the prevalence of each topic. We see that the most prevalent topic is finance. About 28% of the amendments are classified as finance-related. At the other end of the spectrum, the least prevalent topic is related to civil-servant which include about 1% of all the amendments.

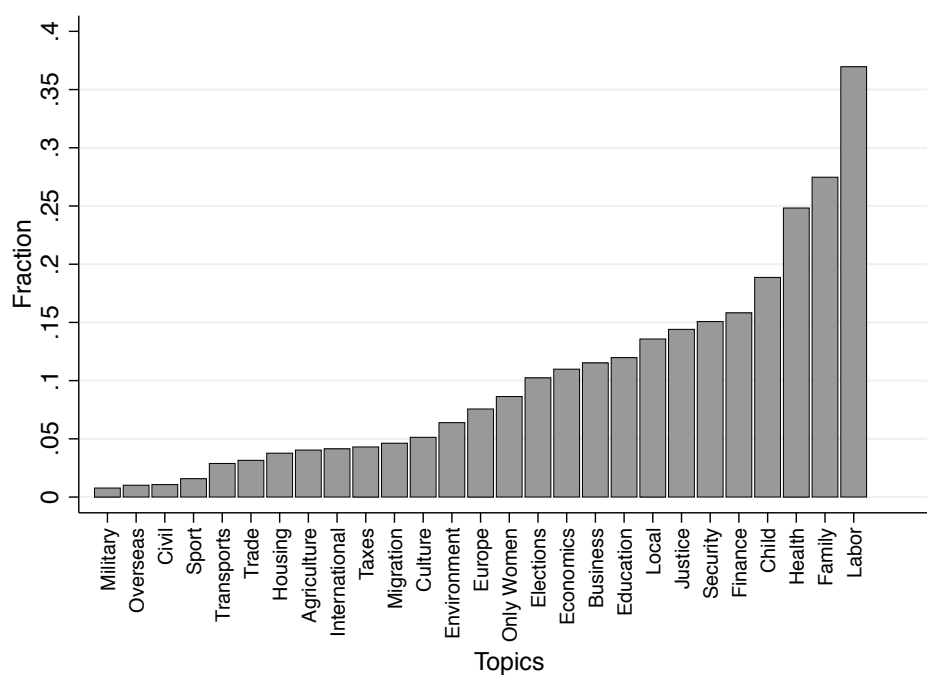
Figure 2.F1: Descriptive Statistics on Topics Prevalence



Notes: The data comes from the French Lower House during the 2002-2017 period. Each bar corresponds to a topic and represents the share of amendments associated to this topic. An amendment can be associated to several topics.

Figure 2.F2 displays descriptive statistics on the prevalence of each topic within the women's issues category. We see that, among women-related amendments, about 35% also refer to labour issues and 25% to health or family issues. At the other end of the spectrum, the least represented topics are military and overseas issues. It should be noted that amendments can be associated to more than 2 topics and therefore, the fractions in the histogram do not sum to 1.

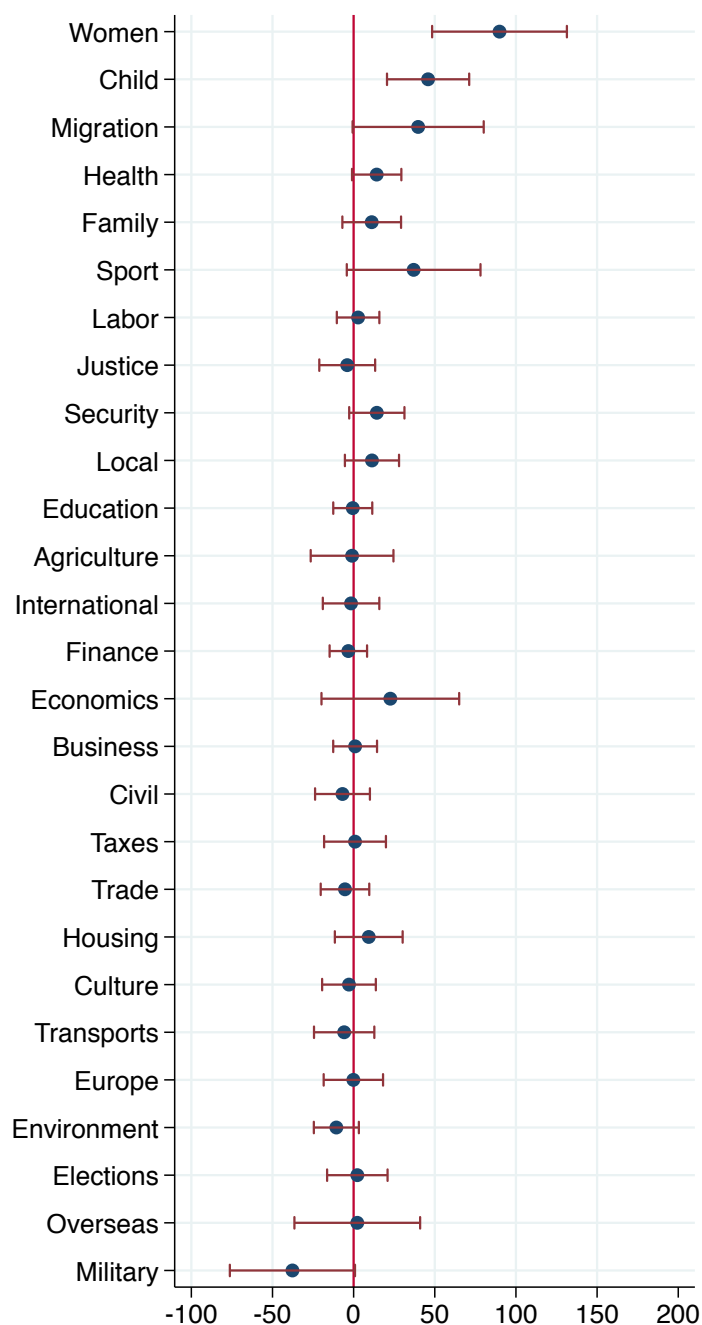
Figure 2.F2: Descriptive Statistics on Topics Prevalence Within Women's Issues



Notes: The data comes from the French Lower House during the 2002-2017 period. The sample is restricted to women-related amendments. Each bar corresponds to a topic and represents the share of amendments associated to this topic. An amendment can be associated to several topics.

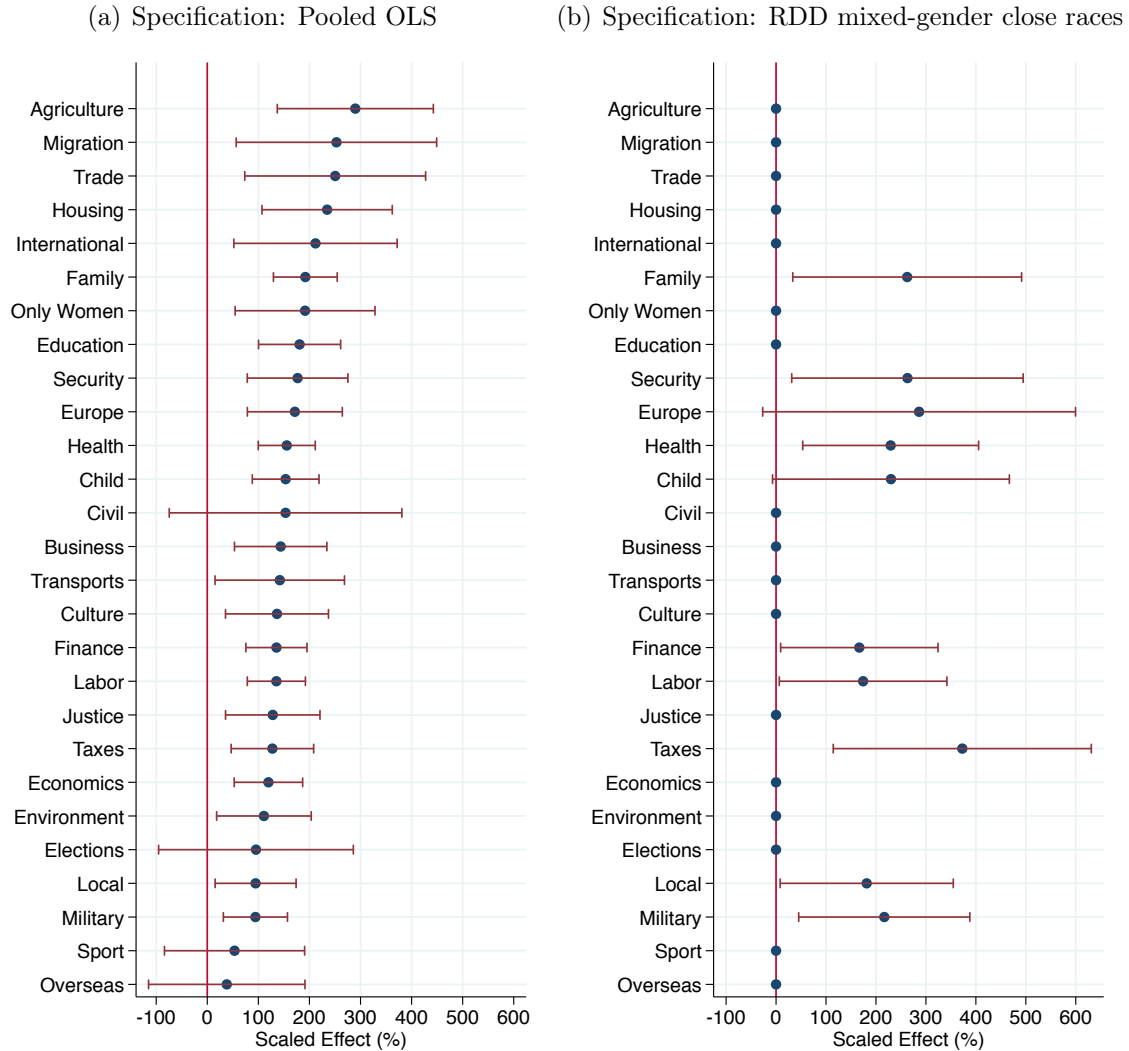
F.3 Additional Results

Figure 2.F3: Extension to Other Topics: Authorship Analysis - Fixed-effects specification



Notes: The data comes from the French Lower House during the 2002-2017 period. Each row corresponds to a topic. The outcome is a dummy that equals 1 if the legislator initiates at least one amendment on the topic considered. Each dot represents the coefficient associated to the variable *Woman* divided by the average of male legislators (scaled effect). Confidence intervals are represented at the 95% level. Estimates come from the fixed-effect specifications.

Figure 2.F4: Extension to Other Topics: Authorship Analysis Within Women-Related Amendments



Notes: The data comes from the French Lower House during the period 2002-2017. The sample is restricted to women-related amendments. About 91.34% of women-related amendments are also related to another topic. Each row corresponds to a topic within the sample of women-related amendments. The outcome is a dummy that equals 1 if the legislator initiates at least one amendment on the topic considered. Each dot represents the coefficient associated to the variable *Woman* divided by the average of male legislators (scaled effect). Confidence intervals are represented at the 95% level. Graph (a) and (b) respectively represent estimates from the pooled OLS and the RDD mixed-gender close race specifications with the CCT bandwidth. Because of the large standards errors for several topics in graph (b), only coefficients significant at the 10% level are displayed.

F.4 Classification Using Unsupervised Methods

This section uses unsupervised methods to uncover 30 topics in the full sample of amendments. The method used is a Latent Dirichlet Allocation and the inference is done with variational bayes . As compared to dictionary-based methods, the main advantage of this method is that the user does not need to specify words associated to topics. Therefore, it allows the research to abstract from its subjectivity in the first steps of the analysis.

This absence of subjectivity is true only in the pre-processing steps of the data. When analyzing the results, subjectivity is necessary. For instance, consider topic 8 obtained in Table 2.F4. the top words associated to this topic are clearly related to energy and it is tempting to label topic 8 as energy-related. Using this information, we could then construct a variable classifying an amendment as energy-related if topic 8 is the most prevalent in the amendment. Then, we could compare the relative involvement of male and female legislator on this topic and draw conclusions on their relative interest for this topic. While this is tempting, this methodology has two severe limitations. First, topic 8 is likely to be a subset of the full energy-related topic. For instance, 50% of the amendments related to energy could use associations of words found in topic 8 and the other half could be much more diverse and included in different topics. Therefore, classifying topic 8 as the "energy-related topic" would miss all the information not contained in topic 8. Second, while some parts of topic 8 are likely to be related to energy issues, some words are also likely to be classified as related to other topics such as transport (for instance the word vehicle). Therefore, topic 8 is likely to be a mixture of energy and transport issues.

Finally, one last and perhaps the most important limitation of unsupervised methods is that they do not deliver topics that we may be interested in. For instance, consider the main interest of this paper which is on women's issues. Reading the list of topics and their associated top words, topic 11 is perhaps the closest to these issues because it contains the word "wom" among its top words. But it also contains the words retired and family. Therefore, it is difficult to conclude on whether topic 11 represents women's issues, a subset of those, or a different topic. Moreover, some topics that we may be interested in such as "military issues" do not appear in the list.

Table 2.F4: Latent Dirichlet Allocation Classification Method

Topic	Top 5 words
Topic 1	fiscal, tax, rate, title, compens
Topic 2	agricul, product, exploit, price, metropol
Topic 3	national, governm, princip, assembl, text
Topic 4	municipal, territor, zon, habit, mountain, popul
Topic 5	be, same, done, effect, more
Topic 6	environm, protect, water, natural, biodiversit
Topic 7	french, transport, countr, european, road
Topic 8	energy, energetic, vehicle, transit, renewabl
Topic 9	financ, million, euro, fund, region
Topic 10	articl, present, redact, erase, new
Topic 11	individu, wom, situat, retired, famil
Topic 12	collectivit, territor, region, compet, local
Topic 13	politic, develop, economic, program, objectiv
Topic 14	housing, income, rate, social, tax
Topic 15	council, represent, mandate elected, member
Topic 16	amendment, redactional, precis, clarif, harmoniz
Topic 17	servic, public, activt, establish, contract
Topic 18	sentenc, diseas, regim, complementar, crim
Topic 19	notion, criter, definit, chamber, commerc
Topic 20	numeric, servic, public, operator, market
Topic 21	salaried, work, enterpris, job, social
Topic 22	text, justify, redistrict, high, board
Topic 23	procedur, control, demand, decis, precis
Topic 24	amendment, coordin, coherenc, consequ, withdraw
Topic 25	research, langu, scientific, cultur, teach
Topic 26	health, care, medecine, securit, establish
Topic 27	right, people, penal, judiciar, polic
Topic 28	format, child, professional, young, educ
Topic 29	year, delay, length, elect, period, ballot
Topic 30	bank, deliver, payment, banking, fee

Notes: the data comes from all the amendments produced produced in the Lower House during the period 2002-2017.

G Mechanisms

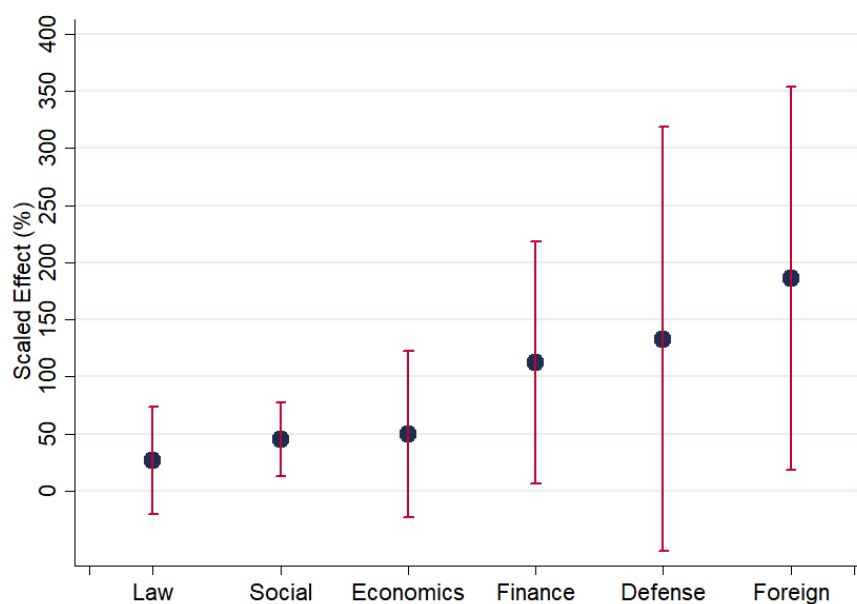
G.1 Results by Committee

Table 2.G1: Authorship of Women-Related Amendments Depending on Membership in Women's Rights Delegation- Lower House

<i>Dep. Var.: At Least one Amendment Initiated (1=Yes)</i>					
Specification	(1) Pooled OLS	(2) Fixed Ef- fects	(3) Polynomial	(4) LLR IK	(5) LLR CCT
<i>Panel A: All Parliamentarians</i>					
Woman (1=Yes)	0.17*** (0.03)	0.20*** (0.05)	0.24*** (0.06)	0.24*** (0.08)	0.23** (0.10)
Control Mean	0.22	0.22	0.19	0.22	0.20
Scaled Effect	75.2	90.5	123.8	111.2	114.8
Bandwidth Restriction			None	25.0	14.1
Observations	1663	1663	791	518	341
Constituencies	597	597	469	349	249
<i>Panel B: Outside Women's Rights Delegation</i>					
Woman (1=Yes)	0.11*** (0.04)	0.14*** (0.05)	0.18*** (0.07)	0.19** (0.08)	0.23** (0.09)
Control Mean	0.22	0.22	0.19	0.21	0.22
Scaled Effect	47.5	61.9	91.6	87.4	104.1
Bandwidth Restriction			None	24.3	18.6
Observations	1556	1556	733	467	391
Constituencies	590	590	443	323	277

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2002-2017. Standard errors clustered at the constituency level are given in parentheses. Panel A includes all parliamentarians. Panel B is restricted to parliamentarians outside the Women's Rights Delegation. Controls in specifications of column 1 and 2 include the age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects. Controls in column 2 also include constituency fixed-effects. Controls in column 3 include a second order polynomial in the running variable. Specifications of columns 4 and 5 fit a local linear regression around the cutoff that allows for a break in the slope at the cutoff. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean).

Figure 2.G1: Results by Committee



Notes: The data comes from the French Lower House during the period 2002-2017. Confidence intervals are represented at the 95% level. The x-axis represents the committees. The y-axis represents the coefficient associated to the variable *Woman* divided by the average of male legislators (scaled effect) in a regression where the outcome is a dummy that equals 1 if the legislator initiates at least one women-related amendment. The estimates are obtained with the Pooled OLS specification controlling for age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency and term fixed-effects.

G.2 Potential Influence of Parliamentary Assistants

Table 2.G2: Authorship of Women-Related Amendments Depending on the Composition of the Team of Parliamentary Assistants - Lower House

<i>Dep. Var.: At Least one Women-Related Amendment Initiated (1=Yes)</i>			
	(1)	(2)	(3)
Woman (1=Yes)	0.13* (0.07)	0.15* (0.08)	0.12 (0.07)
AboveMedianShareFemaleAssistant			-0.06 (0.05)
Woman*AboveMedianShareFemaleAssistant			0.06 (0.10)
Share Female Assistant Above Median	No	Yes	All
Control Mean	0.44	0.36	
Scaled Effect	29.1	40.0	
Observations	246	252	498

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Lower House during the period 2012-2017 for which information on the parliamentary assistants is available. The sample is restricted to parliamentarians who were in the House when the data was collected. Controls include the age at the beginning of the term, the political inclination (left or right-wing), the incumbency status, the margin of victory at the election, the female participation rate to the labor market in the constituency. The "Control Mean" line designates the outcome mean for the sample of male legislators. The "Scaled Effect" line designates the impact of female legislators scaled to the mean of male legislators (Treatment Effect/Control Mean). The median share of female assistant is 2/3.

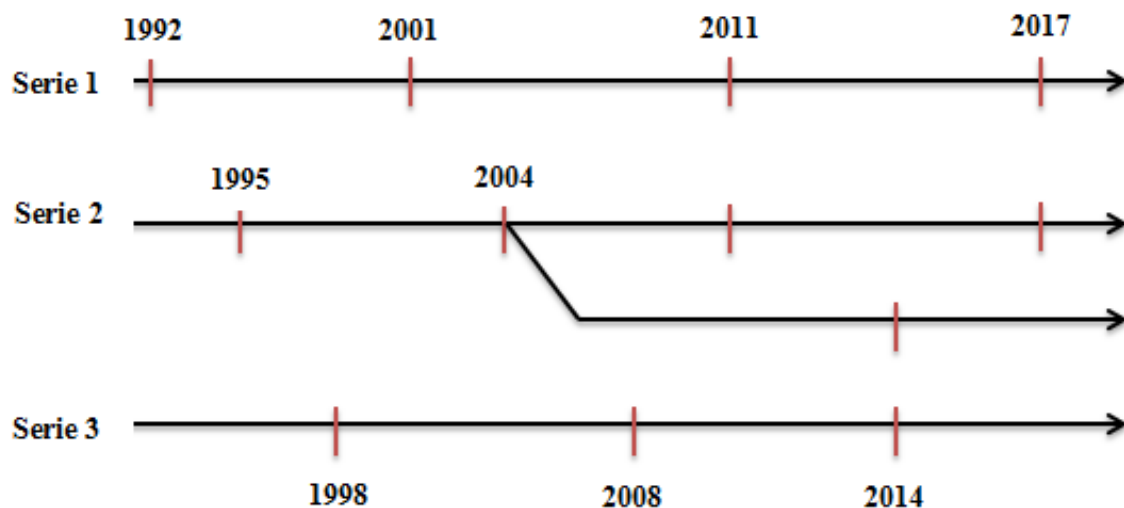
H Evidence from the Upper House

H.1 Institutional Setting

Figure 2.H1 schematizes the schedule of the Upper House election system. Initially, there were 3 series. The first had elections in 1995, 2001, 2011 and 2017. The second had election in 1995 and 2004. After 2004, this serie was splitted in two (randomly) and half of the senators were reelected in 2014 (and will be in 2020) and the other half in 2011 and then 2017. Finally, the last serie had elections in 1998, 2008 and 2014 (and will have in 2020).

Gender quotas were voted in 2000. As such, they were applied for the first time in 2001 (serie 1), 2004 (serie 2) and 2008 (serie 3).

Figure 2.H1: Upper House Election Schedule



Notes: This figure schematizes the schedule of the French Upper House election system. Each short vertical red line represents an election.

H.2 Descriptive Statistics

Table 2.H1: Descriptive Statistics on Constituencies' Characteristics and their Activity Related to Amendments

	(1) Mean	(2) S.D.	(3) Min	(4) Max
N Elected	3.04	1.81	1.0	12.0
N Elected Women	0.53	0.89	0.0	5.0
Share Women Elected	0.12	0.18	0.0	1.0
N Co-Sponsored Per Year	135.65	93.98	2.0	451.3
N Co-Sponsored Per Year Gender	2.57	2.63	0.0	14.3
Share Co-Sponsored Gender	0.02	0.02	0.0	0.1
N Authored Per Year	21.45	22.59	0.0	169.3
N Authored Per Year Gender	0.30	0.61	0.0	4.9
Share Authored Gender	0.01	0.02	0.0	0.2
At Least One Authored Gender (1=Yes)	0.56	0.50	0.0	1.0
At Least One Authored (1=Yes)	1.00	0.07	0.0	1.0
Observations	216			

Notes: the data comes from the French Upper House during the period 2001-2017. *Gender* indicates that the amendment is identified as related to women's issues with a dictionary-based method.

Table 2.H2: Most Frequent Trigrams and Bigrams in the Sample of Amendments Related to Women's Issues - Upper House

(1)	(2)	(3)	(4)	(5)	(6)
		<i>Trigrams</i>		<i>Bigrams</i>	
Rank	N	Keywords	N	Keywords	
1	111	delegation rights women	480	women men	
2	110	equality women men	265	national assembly	
3	54	professional women men	264	men women	
4	51	equality professional women	236	part time	
5	50	equal access women	185	equality professional	

Notes: the data comes from all the amendments produced in the French Upper House during the period 2001-2017. It is restricted to amendments identified as related to women's issues with a dictionary-based method.

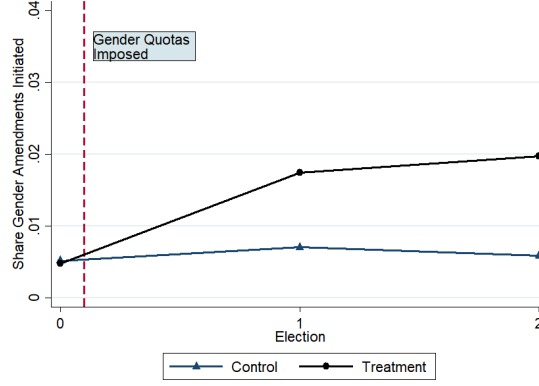
H.3 Results

Table 2.H3: Overall Gender Differences in Parliamentary Activities - OLS and Wald Estimates in the Upper House

	(1)	(2)
Dependent Variable	N Authored	N Co-sponsored
<i>Panel A: OLS Estimates</i>		
N Women	5.07 (6.92)	0.65 (2.42)
Observations	216	216
Constituencies	72	72
<i>Panel B: Wald Estimates</i>		
N Women	-4.54 (16.99)	2.32 (4.80)
Observations	216	216
Constituencies	72	72

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Upper House during the period 2001-2017. The dependent variable is respectively the number of amendments initiated and the number of amendments co-sponsored by the constituency in column 1 and 2. Standard errors clustered at the constituency level are given in parentheses. Panel A displays OLS estimates and Panel B Wald estimates.

Figure 2.H2: Impact of the Gender Quota on Authorship of Women-Related Amendments in the Upper House



Notes: the data comes from the French Upper House during the period 2001-2017. Circles and triangles respectively designate the average number of women per constituency for those that have to comply with the quota and those that do not need to. The y-axis represents the share of authored women-related amendments. The vertical red dashed line corresponds to the time where gender quotas were introduced.

Below, I estimate the reduced-form impact of the gender quotas in the Upper House. Formally, I estimate:

$$Y_{ct} = \alpha + \delta Treatment_c + \gamma Post_t + \beta Treatment_c * Post_t + \epsilon_{ct} \quad (8)$$

Where c is the subscript for the constituency level and t for time. $Treatment_c$ equals 1 if the constituency has to comply with the quota. $Post_t$ equals 1 if the election happened after the introduction of the gender quota.

Table 2.H4: Reduced-Form Impact of Gender Quotas on Initiation of Women-Related Amendments - Upper House

Dependent Variable	(1) N	(2) Share	(3) At least One (1=Yes)
Treatment	0.06 (0.08)	-0.00 (0.00)	0.08 (0.13)
Post	0.07 (0.08)	-0.00 (0.00)	0.15* (0.09)
Treatment*Post	0.48*** (0.16)	0.02*** (0.01)	0.35** (0.14)
Observations	216	216	216
Constituencies	72	72	72

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Upper House during the period 2001-2017. Standard errors clustered at the constituency level are given in parentheses. Treatment corresponds to constituency that have to comply with the quota, i.e. those with more than 4 representatives. Post corresponds to the period after the first election with quotas.

Table 2.H5: Reduced-Form Impact of Gender Quotas on Co-Sponsorship of Women-Related Amendments - Upper House

Dependent Variable	(1) N	(2) Share
Treatment	0.24 (0.37)	0.00 (0.00)
Post	2.04*** (0.39)	0.01* (0.00)
Treatment*Post	1.07* (0.57)	0.00 (0.00)
Observations	216	216
Constituencies	72	72

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Upper House during the period 2001-2017. Standard errors clustered at the constituency level are given in parentheses. *Quota* corresponds to constituency that have to comply with the quota, i.e. those with more than 4 representatives. *PostPool* corresponds to the period after the first election with quotas.

Table 2.H6: First Stage Results: Impact of Gender Quotas - Upper House

Dependent Variable	(1) N Women	(2) N Women	(3) At Least One Woman (1=Yes)	(4) At Least One Woman (1=Yes)	(5) Share Women	(6) Share Women
Quota*Post Pool	1.23*** (0.24)		0.35*** (0.12)		0.13*** (0.05)	
Post Pool	0.22*** (0.05)		0.22*** (0.05)		0.09*** (0.02)	
Quota*Post 1		1.20*** (0.25)		0.45*** (0.12)		0.14*** (0.05)
Quota*Post 2		1.26*** (0.26)		0.26* (0.13)		0.13** (0.06)
Post 1		0.15*** (0.05)		0.15*** (0.05)		0.06*** (0.02)
Post 2		0.29*** (0.07)		0.29*** (0.07)		0.12*** (0.03)
Quota	0.48*** (0.15)	0.48*** (0.15)	0.38*** (0.11)	0.38*** (0.11)	0.09*** (0.03)	0.09*** (0.03)
F-Statistic	35.7	22.7	316.4	565.6	72.2	53.9
Observations	216	216	216	216	216	216
Constituencies	72	72	72	72	72	72

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Upper House during the period 2001-2017. The dependent variables are the number of women (column 1 and 2), a dummy that equals 1 if at least one woman is elected (column 3 and 4), the share of women elected (column 5 and 6). The regressions are run at the constituency level.

Table 2.H7: Instrumental Variable Strategy

<i>Dep Var: Share Women-Related Amendments Initiated</i>			
	(1)	(2)	(3)
N Women	0.02** (0.01)		
Share Women		0.16** (0.07)	
At Least One Woman			0.06** (0.03)
Treated	-0.01* (0.01)	-0.02 (0.01)	-0.03 (0.02)
Post	-0.01 (0.01)	-0.02 (0.01)	-0.01 (0.01)
Observations	215	215	215
Constituencies	72	72	72

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Upper House during the period 2001-2017. Standard errors clustered at the constituency level are given in parentheses. The dependent variable is the share of women-related amendment initiated in a constituency. Each column displays the results using a different measure of the presence of elected women per constituency (number of women for column 1, the share of women for column 2 and a dummy that equals 1 if at least one woman is elected in column 3).

Analyzing co-sponsorship, Table 2.H8 provides the OLS and Wald estimates. Looking at Panel A, using OLS estimations, we see that the number of women in a constituency is strongly correlated with the number and the share of women-related amendments co-sponsored. In column 1, we see that one additional woman increases the number of amendments co-sponsored by 0.77 per year. Similarly, in column 2, the share of women-related amendments increases by 0.004 percentage points when one additional woman is elected. Turning to Wald estimates, estimates lose in precision. We see that an additional woman introduced through gender quotas increases the number of co-sponsors by 0.87, this coefficient being significant at the 10% level. Turning to column 2, the increase in the number of amendments co-sponsored does not lead to an increase in the share of amendments co-sponsored. One potential explanation for this finding is that due to the lower co-sponsorship activity in the Upper House, women may prefer to engage in authorship

rather in co-sponsorship. This would be increasingly valued in the Upper House context.

Table 2.H8: Impact of Gender Quotas on Co-Sponsorship of Women-Related Amendments - OLS and Wald Estimates in the Upper House

Dependent Variable	(1) N	(2) Share
<i>Panel A: OLS Estimates</i>		
N Women	0.775*** (0.243)	0.004*** (0.001)
Observations	216	216
Constituencies	72	72
<i>Panel B: Wald Estimates</i>		
N Women	0.872* (0.500)	0.003 (0.003)
Observations	216	216
Constituencies	72	72

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Upper House during the period 2001-2017. Standard errors clustered at the constituency level are given in parentheses. Panel A displays OLS estimates and Panel B Wald estimates.

Table 2.H9: Extrapolating to the Macro Effect - Wald Estimates

Dependent Variable	(1) Share Amendments Women's Issues
Share Women	0.159** (0.071)
Observations	216
Constituencies	72

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the French Upper House during the period 2001-2017. Standard errors clustered at the constituency level are given in parentheses.

H.4 Robustness

In Table 2.H10, I replicate the main analysis using a restrictive definition of women-related amendments. Only amendments containing the word "wom" are classified as women-related. The results are essentially similar although slightly lower in magnitude.

Table 2.H10: Impact of Gender Quotas on Initiation of Women-Related Amendments Restricted Definition - OLS and Wald Estimates in the Upper House

Dependent Variable	(1) N	(2) Share	(3) At least One (1=Yes)
<i>Panel A: OLS Estimates</i>			
N Women	0.166* (0.084)	0.005*** (0.002)	0.122*** (0.039)
Observations	216	215	215
Constituencies	72	72	72
<i>Panel B: Wald Estimates</i>			
N Women	0.391** (0.155)	0.017** (0.007)	0.278** (0.132)
Observations	216	215	215
Constituencies	72	72	72

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes French Upper House during the period 2001-2017. Standard errors clustered at the constituency level are given in parentheses. Panel A displays OLS estimates and Panel B Wald estimates.

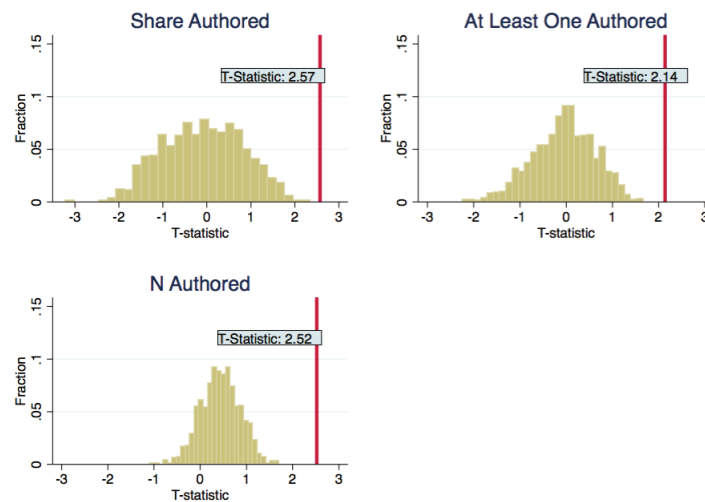
While the introduction of a gender quotas has increased the share of amendments initiated on women-related topics in the Upper House, the question remains as to what extent this finding depends on the fact that these amendments are related to women's issues?. To investigate, I build a placebo test that consists in drawing a set of random sample of amendments (500) of equal size to the sample used in the main regression (2,064) and comparing the estimates related to the difference-in-differences coefficient (*Quota * Post*) to the one with the main sample.

Figures 2.H3 displays the t-statistics for the three outcomes which are respectively: the share of amendments initiated, the probability to initiate at least one amendment and the

raw count of amendments initiated. The placebo test shows that it is very unlikely to obtain findings consistent with the body of the article using a random sample of amendments.

For the main outcome which is the share of amendments initiated, there does not exist any random sample that delivers a larger t-statistic than the one with the main sample. Only 7 (1.4%) samples deliver estimates significant at the 5% level. These figures are equal to zero for the other two outcomes. Consequently this test provides supporting evidence of the existence of homogeneity and relevance in the sample of amendments selected using the dictionary-based approach.

Figure 2.H3: Placebo Test Random Sample Amendments - Upper House



Notes: The data comes from the French Upper House during the period 2001-2017. The histogram represents the distribution of T-statistics related to the difference-in-differences coefficient $Quota * PostPool$ using 1000 placebo samples constituted of random draws of amendments. Each graph represents a different outcome.

Chapter 3: Persistence of Incumbents and Female Access to Political Positions

This chapter is sole-authored.

Abstract

This article investigates whether the persistence of incumbents hinders female access to political positions when incumbents are predominantly men. I exploit regression discontinuity from close electoral races in French municipalities to randomize the eligibility of incumbent mayors for reelection. Despite a context increasingly favorable to the election of women, I find that the persistence of incumbents does not block female access to the position of mayor. I investigate the mechanisms and show that it is more difficult for a woman to replace a female incumbent than a male one. This is consistent with a backlash or stereotype threat effect penalizing women after a female incumbent.

1 Introduction

Women are underrepresented in politics. In 2017, only 24% of parliament seats around the world were held by women⁴⁴. Several reasons have been put forward to explain this phenomenon. On the demand side, women may be discriminated by electors or insiders. On the supply side, women may have specific preferences or constraints that makes them less willing than men to run for political positions.

On top of these gender-specific factors, women may face institutional constraints that are independent of their gender. Incumbency is cited as one of the main barriers. The argument is that the access of women to political positions may be limited when incumbents are predominantly men who run for reelection and enjoy an electoral advantage. Existing studies have provided support for this argument by establishing positive correlations between incumbents' turnover rate and the share of women in politics.

Yet, despite important implications, we know very little on the causal impact of incumbency. Correlations may be caused by omitted variables undermining the causal interpretation. The mechanisms behind the impact of incumbency are also unclear. For incumbency to reduce women's access to political positions, it is implicitly assumed that absent the incumbent, women and men are equally likely to be elected regardless of the gender of the incumbent. Yet, little is known on the validity of this assumption.

The main contribution of this article is to investigate the causal impact of incumbency on the election of women. The key empirical challenge is to use exogenous variations randomizing the reelection of incumbents. To overcome this challenge, I exploit regression discontinuity from close electoral races in a preferential voting system in small municipalities of France. In towns below 1,000 inhabitants, the election of the mayor follows a two-step process. First, voters have to elect the members of the municipal council following a full preferential voting system where votes are counted at the candidate level. Then, the municipal council elects one of its members to the position of mayor. Using data from the 2014 municipal elections, I exploit close races in access to the municipal council to compare the sex of the newly elected mayor when the incumbent mayor is closely reelected in the municipal council and where he/she is closely eliminated. As only municipal councillors are eligible for the position of mayor, this empirical strategy randomizes the eligibility of the incumbent mayor.

⁴⁴<http://data.worldbank.org/indicator/SG.GEN.PARL.ZS>

The first set of results establishes that, on average, the incumbent advantage of mayors for reelection does not block female access to the position of mayor. While incumbents are strongly persistent with a reelection rate above 60%, the share of female mayors does not increase when they are not eligible to their succession. It remains at about 20% independently of the eligibility of the incumbent. This finding contrasts with common expectations assuming that, in a context increasingly favorable to the election of women, a large incumbent advantage and a predominance of male politicians are sufficient for incumbency to hinder female access to political positions.

Second, I investigate the mechanisms behind this finding. I find that the share of newly elected female mayors is significantly lower after a female incumbent than a male one. When incumbent mayors are not eligible for reelection, about 23% of male incumbents are replaced by women while only 13% of municipalities with female incumbents elect a new female mayor. These different replacement rates are accompanied by a large gender gap in the probability to be elected mayor among experienced councillors only in municipalities with a female incumbent. After a male incumbent, female councillors with experience have the same probability to become mayor than their male counterparts but the picture is completely different after a female incumbent. In this case, there exists a large gender gap disadvantaging women suggesting that potential female candidates suffer from a penalty undermining their chances to replace the incumbent.

These findings are consistent with the fact that, in a context with a low share of female mayors, women's failure to be reelected could be costly for new female candidates. The actions of past female politicians would act as a signal for the success of future potential female leaders. This could be because of a backlash effect where voters' gender biases against women would be reinforced and lead municipal councillors to vote less for new female candidates. Alternatively, this could also be explained by a stereotype threat effect where the female incumbent's loss could remind potential female candidates that politics is a male-dominated area and decrease their willingness to run for political positions.

While it is difficult to precisely pinpoint what lies behind this signal effect with the data at hands, it is possible to rule out several alternative mechanisms. This is important to understand whether the penalty truly reflects a signal effect. I consider three alternative mechanisms and show that none is supported in the data. If anything, it seems that women should have better chances to replace a female incumbent than a male one. First, I show that the results are not driven by a shortage of qualified women in places with a

female incumbent mayor. On the opposite, it appears that women are more qualified in municipalities with a female incumbent mayor. Second, female mayors also do not seem to perform worse than their male counterparts when they manage the municipality. This rules out the possibility that female incumbents are objectively less competent than their male counterparts. Finally, to alleviate concerns about the role of the gendered composition of the municipal council, I implement a systematic placebo exercise replicating the results with the sample of incumbent councillors. Their non-reelection do not trigger similar results conforing the idea that the results stem from incumbency at the position of mayor and cannot be explained by a variation of the share of women in the council.

These results make two contributions to the literature related to the determinants of the underrepresentation of women in politics and high-status occupation. First, this paper adds to this literature by studying the causal effect of incumbency on the election of women. While existing studies have shown positive correlations between incumbents' turnover rate and the underrepresentaion of women (see for instance Andersen and Thorson, 1984, Studlar and McAllister, 1991, Darcy *et al.*, 1994, Gaddie and Bullock, 2000, Matland and Studlar, 2004 and Schwindt-Bayer, 2005), the causality of this channel has, to my knowledge, not been investigated.

Second, in contrast with most studies who examine what happens when a woman wins an election, this paper studies what happens when they fail to be reelected. It appears that it is more difficult for women to replace a female incumbent than a male one. This is consistent with a signal effect where, in a context of low female political representation, women's errors would be costly for future female politicians but not men's errors. This mechanism could constitute an important obstacle to women's ascent to positions of power. This could be explained by a backlash effect against women in the sense that voters could attribute less votes to new female candidates once the female incumbent has failed to be reelected. This effect is close to gender discrimination in politics highlighted by several studies (Sanbonmatsu, 2002, ?, Fréchette *et al.*, 2008, Beaman *et al.*, 2009, Gagliarducci and Paserman, 2012, Esteve-Volart and Bagues, 2012, Casas-Arce and Saiz, 2015, Bhalotra *et al.*, 2017, ?). This could also be because women would be less willing to replace a female incumbent than a male one. This supply-side argument is closer to studies establishing the existence of gender differences towards ambition (Fox and Lawless, 2004 and Fox and Lawless, 2014), competition (Niederle and Vesterlund, 2007), self-confidence (Croson and Gneezy, 2009), election (Kanthak and Woon, 2015), persistence in politics (?) or a lack of

role models (Gilardi, 2015 or Baskaran and Hessami, 2017).

This paper is organized as follows. Section 2 develops a simple theoretical framework to understand how the persistence of incumbents reduces the share of female politicians. Section 3 describes the institutional setting. Section 4 describes the data and the empirical strategy. Then, section 5 presents the results. Section 6 investigates the underlying mechanisms. Finally, Section 8 concludes.

2 Conceptual Framework

This section develops a simple framework establishing the key conditions under which the persistence of incumbents reduces the share of female politicians. Consider two periods denoted by t and $t+1$. There are no incumbents in t .

Note respectively F_t and F_{t+1} the likelihood that a woman is elected in period t and $t+1$ when the incumbent is not reelected. F_t is determined by the demand and the supply of female politicians. For instance, F_t increases when discrimination towards women is weakened or when women are increasingly willing to run for political positions. By default in period t , F_t also corresponds to the share of women elected since there are no incumbents. In period $t+1$, F_{t+1}^F and F_{t+1}^M designate the likelihood to elect a woman after a female and a male incumbent.

Note the likelihood that an incumbent is reelected β where β is greater than zero, high and stable across periods. Finally, if an incumbent is not reelected, assume that a female candidate is drawn from the pool of other good candidates with probability F_{t+1} . Using these notations, the share of female politicians in second period SF_{t+1} is equal to:

$$SF_{t+1} = F_t * \beta + F_t * (1 - \beta) * F_{t+1}^F + (1 - F_t) * (1 - \beta) * F_{t+1}^M \quad (9)$$

Where the first term corresponds to the share of female incumbents reelected, the second to the share of female incumbents who are replaced by newly elected women and the third term to the share of male incumbents replaced by a woman.

Assuming that the likelihood to elect a candidate of either sex does not depend on the sex of the incumbent, i.e. $F_{t+1}^M = F_{t+1}^F = F_{t+1}$ leads to:

$$SF_{t+1} = F_{t+1} + \beta(F_t - F_{t+1}) \quad (10)$$

This equation determines the share of female politicians in the second period. It provides one simple prediction for the impact of incumbency: the persistence of incumbents impedes the election of women only when the context has become more favorable to the election of women in $t+1$, i.e. when $F_{t+1} > F_t$. This could be due to an increased demand for female politicians or alternatively to an increased supply.

Relaxing the assumption $F_{t+1}^M = F_{t+1}^F = F_{t+1}$ leads to a different conclusion. If the success of a female candidate depends on the actions of the female incumbent, it could be more difficult (or easier) for a woman to replace a female incumbent than a male one. In this case, incumbency could fail to impede women from getting elected. Assume that $F_{t+1}^F = (1 + \delta)F_{t+1}^M$, where $\delta < 0$ indicates that fewer women are elected after a female incumbent than a male one. Rearranging Equation 9 leads to:

$$SF_{t+1} = F_{t+1} + \beta(F_t - F_{t+1}) + \delta F_t F_{t+1}(1 - \beta) \quad (11)$$

Where the last term designates the difference between the share of women elected and the share that would have prevailed absent any signal effect. If less women are elected after a female incumbent than a male one, i.e. if $\delta < 0$, incumbency will not necessarily reduce the share of women elected even if the context becomes more favorable towards women. It will depend on the relative size of the terms related to the incumbent advantage (second term) and to the signal effect (third term).

In the rest of the paper, I test the empirical validity of the model and of its assumptions.

3 Institutional Context

The analysis focuses on the French municipal elections in towns below 1,000 inhabitants. There are about 26,000 municipalities below 1,000 inhabitants in France which follow a preferential voting election system. In the past two decades, women have been increasingly involved at this political level.

3.1 The French Municipalities

3.1.1 Demographics

France is unique with regards to the distribution of its municipalities. On January 1st 2016, with a population of 67.357 million people living in 35,911 municipalities, France accounted for about 13% of the population of the European Union but 30% of the municipalities⁴⁵. The distribution is very skewed as more than 70% of the municipalities have less than 1,000 inhabitants and about 10 million people were living in such cities. There are thus 26,879 municipalities below 1,000 inhabitants.

The competencies of municipalities include the creation and financing of pre-elementary and elementary schooling, the creation and management of cultural organisms and sports equipments, the delivery of construction permits, the maintenance of public roads and water system, etc. Their resources come mainly from local taxes that are directly determined by the municipal council and direct transfers from the State.

3.1.2 Election System of Municipalities Below 1,000 Inhabitants

Municipal elections occur every 6 years in France. Electors choose the municipal council which designates a mayor among its members. The size of the municipal council increases with the population of the town at two different thresholds: 100 (from 7 to 11 councillors) and 500 (from 11 to 15 councillors). In municipalities below 1,000 inhabitants⁴⁶, the election system of the municipal council follows a two round plurality rule with a split ticket option, where electors vote for individuals rather than for lists.

First Step - Election of the Municipal Council. The election is individual and each candidate must obtain a sufficient number of votes to be on the council. Candidates can either run alone or put their name on the same ballot but electors can modify the ballot by erasing names and voting for alternative candidates. This procedure is called *Panachage*. To be elected at the first round, a candidate has to obtain the absolute

⁴⁵Source: Eurostat data. Data for the number of municipalities comes from the correspondance table at the Local Administrative Unit 2. Data for the population comes from the population accounts on 1st January of 2016.

⁴⁶In municipalities above 1,000 inhabitants, the election system is a closed-list system where electors vote for only one list. As such, in this system, it is not possible to observe the individual popularity of candidates which is needed for the current analysis and the newly elected mayor is almost always the individual on the first position of the winning list.

majority (and several can). If some seats remain vacant, a second round is organized and relative majority is sufficient to win. An elector can vote for as many people as the size of the municipal council. For instance, if the municipal council contains 15 members, the elector can vote only once for 15 individuals or less.

Second Step - Election of the Mayor. The newly elected municipal council has to elect one of its member to the position of mayor. To be elected, the mayor has to obtain the absolute majority among the municipal council and if none is reached after two rounds, relative majority is sufficient. Councillors are free to vote for their favorite candidate and the vote is done with secret ballot.

3.1.3 The Position of Mayor

The mayor is the key character in the management of a municipality. He/she is the elected authority and the representative of the State in the town. He/she is responsible for the preparation and implementation of the decisions of the council which mainly votes the budget and can pass a motion of confidence in the mayor. Mayors also manage all employees who are appointed by them. The municipal council also has to elect at least one deputy mayor who will constitute, together with the mayor, the executive branch of the city hall. The mayor's wage increases at the 500 inhabitants threshold from 646.25 euros to 1,178.46 euros per month in 2017⁴⁷. There is thus a clear hierarchy among the municipal council where at the top stands the mayor, then comes the deputy mayor(s), then the municipal councillors.

3.2 A Rising Share of Female Politicians

Given that the persistence of incumbents should reduce the share of female politicians in a situation increasingly favorable to the election of women (see Section 2), it is important to understand to which situation the present context corresponds. In the French municipalities, although men remain largely overrepresented at the position of mayor, the share of female mayors has been steadily increasing during the past 25 years. Figure 3.1 describes this evolution from 1989 until 2014. In 1989, only 5% of mayors were women. Since that date, the share of female mayors has increased by about 3 percentage points at

⁴⁷Municipal councillors are generally unpaid but, if they are, their total salary cannot exceed 6% of the total budget of the town.

each election to reach 18% in 2014. Consistently, the share of female councillors has also been rising. Between 2008 and 2014, it increased by more than 10%, from 30% to 34% of all the councillors.

Therefore, it is very likely that the present context is increasingly favorable to the election of women. Related this assertion to the theoretical framework of Section 2, we should expect the potential pool of female mayors to be higher in 2014 than it was in 2008, i.e. that $F_{2014} > F_{2008}$). We may therefore expect the persistence of incumbents to impede the election of women.

4 Empirical Setting

4.1 Data and Descriptive Statistics

The main data source comes from the 2014 municipal elections results. This dataset provides the name and vote share of each candidate for the municipal council. I focus on the 2014 elections because starting from this election, it became mandatory to be registered to run for a position of councillor. Therefore, I can observe the score of each candidate in 2014, whether they were elected or not. For the 2008 election results, only the vote share of elected individuals are observable.

To identify the identity of the incumbent and of the newly elected mayor, I use an administrative dataset produced by the French Ministry of Interior: the *Répertoire National des Elus*. It provides information on mayors from 2001 onwards and information on other members of the municipal council only since the 2008 elections. These information include the name, sex, age, profession, position held in the municipal council and the date at which the individual was elected and at which the term stopped.

Table 3.1 provides descriptive statistics on the sample used for the analysis. This sample consists of incumbents running for reelection in 2014 in a competitive election⁴⁸. Panel A refers to individual level characteristics related to the incumbent mayor and panel B to municipality characteristics. There are 5,455 incumbent mayors in the sample, 16% of them are women and they are about 55 years old.

⁴⁸A competitive election is defined as an election where the number of candidates is higher than the number of seats in the council. Given that the empirical strategy exploits close-races in access to the municipal council, the running variable is defined only for municipalities with competitive elections.

In this sample, the context also seems to be increasingly favorable to the election of women. The share of female mayors has been increasing by about 3 percentage points at each election since 1989 from 5% to 16% in 2008. Between 2008 and 2014, the female vote share increased by 3 p.p. and consistently, the share of female councillors increased by 4 percentage points. Scaling these variations to the 2008 baseline, they represent a 10% increase.

4.2 Methods: Regression Discontinuity Design

The objective of this work is to measure whether the reelection of incumbent mayors reduces female access to the position of mayor. To causally measure this phenomenon, I exploit a discontinuity caused by the election system of French municipalities below 1,000 inhabitants which randomizes the eligibility of an incumbent for the position of mayor. This strategy allows me to compare exogenously two situations with and without the incumbent.

As explained in section 3, the election of the mayor follows a two-step process. First, an election takes place to determine who will hold a seat in the municipal council. Then, another election takes place in the municipal council to determine which councillor will become the mayor. The election of the municipal council follows a preferential voting system where every elector votes for individuals that he/she deems fit for the municipal council. As such, there are cases where the incumbent mayor is reelected in the municipal council by a narrow margin and cases where the incumbent mayor is not reelected by a narrow margin. In the first cases, the incumbent has the possibility to run for the position of mayor but in the other cases, he/she is ineligible as one has to be a member of the municipal council to run for the position of mayor.

Comparing the identity of the newly elected mayor in these two cases over a narrow margin boils down to a regression discontinuity design. Providing that incumbents cannot manipulate their reelection status in this narrow margin and that the distribution of confounders changes continuously at the threshold, this framework randomizes the eligibility of the incumbent to run for the position of mayor. The running variable is the vote margin relative to the threshold to be a member of the municipal council on the last round of the election. For a reelected individual, this running variable is positive and equal to his/her vote share minus the vote share of the first non-elected candidate. For a non-reelected individual, this running variable is negative and equal to his/her vote share minus the vote

share of the last elected individual⁴⁹. Formally, I estimate the following equation:

$$FemaleMayor_i = \alpha + \beta IncumbentNotEligible_i + f(X_i) + \epsilon_i \quad (12)$$

Where i is the subscript for the municipality (since there is only one incumbent mayor per municipality). X_i is the running variable, i.e. the vote margin of the incumbent mayor of municipality i . *IncumbentNotEligible* is a dummy that equals 1 if $X_i < 0$, i.e. if the incumbent is not reelected in the municipal council and therefore not eligible to the position of mayor. $f(X_i)$ is a polynomial interacted with *IncumbentNotEligible*. The key variable is *IncumbentNotEligible* and β identifies the impact of incumbency at the position of mayor.

This equation is estimated on a narrow margin around the elimination threshold. The reference bandwidth is selected following the approach of Calonico *et al.* (2014) depending on the vote margin. To probe the robustness of the results, I also estimate the equation using the IK bandwidth (Imbens and Kalyanaraman, 2012) which is often larger than the reference bandwidth⁵⁰. In both specifications, observations are weighted with a rectangular kernel (following Imbens and Lemieux, 2008). I also run an additional specification on the entire sample which fits a second order polynomial in the running variable X_i .

4.3 Internal Validity Tests

The validity of the empirical strategy relies on the randomization of the treatment over a narrow margin. In this section, I provide evidence on this matter by establishing that the vote margin is not manipulated and that potential confounders behave continuously at the threshold.

First, in Figure 3.2, I provide evidence on the absence of manipulation of the running variable around the elimination threshold. Visually, there is no evidence of a discontinuity in the density of the incumbent vote margin relative to the elimination threshold. The McCrary test (McCrary, 2008) does not reject the null hypothesis of no manipulation⁵¹.

⁴⁹For example, if, the last elected individual obtains 55% of the votes, the first not elected 48% and the incumbent 60%. Then, the running variable would be equal to 12 percentage points.

⁵⁰The bandwidths were selected with the Stata packages *rdrobust* and *rdob*

⁵¹I also ran an additional manipulation test based on Cattaneo *et al.* (2018) which confirmed that the drop is not significant (p-value = 0.79)

This was expectable as manipulation would require to either commit electoral frauds which are extremely rare in France or predict the election results with extreme accuracy which is unlikely because in these municipalities, there are no polls and because unpredictable events (such as weather condition) on the election day could still happen and change the results.

Second, I test the continuity assumption for each potential confounder. To do so, I run Equation 12 using as an outcome variable a set of covariates. If the setting is valid, there should not be any discontinuity in these covariates and the coefficient β related to the treatment effect, i.e. *IncumbentNotEligible_i* should not be significant. Three sets of covariates are considered: one representing preferences for women in politics (a dummy representing whether the incumbent mayor is a woman, the share of women in the council as well as the total vote share for women in 2014 and their variation between 2008 and 2014), one representing election characteristics (the number of registered voters, the abstention rate, total number of candidates, the share of female candidates) and a last one representing municipal council characteristics (size of the council, incumbent's mayor age and the number of term served).

As shown in Table 3.2, the continuity assumption seems verified as there is no significant jump at the cutoff for each of these covariates (the relevant graph for each of the covariates can be found in Figure 3.A2). Consequently, there is no evidence that the results are driven by any other characteristics than the treatment considered.

5 Results - Impact of Incumbency

This section first establishes the contribution of incumbency to the inertia at the position of mayor. Then, I measure whether the persistence of incumbents reduces female access to the position of mayor.

5.1 Electoral Advantage of Incumbent Mayors

For incumbents to impede the election of women, they must enjoy an electoral advantage. This section establishes the existence of this advantage which designates the probability that an incumbent is reelected mayor when eligible for this position. It differs from the usual incumbent advantage corresponding to the increased probability that an

incumbent is reelected due to his/her incumbency. To measure the electoral advantage of incumbents, I estimate Equation 12 using as an outcome variable the probability that an incumbent mayor is reelected mayor depending on his/her eligibility for the position.

Figure 3.3 plots the share of incumbents who are reelected mayor depending on their vote margin relative to the elimination threshold. On the left side of the vertical dashed line, the incumbent is not reelected in the municipal council and consequently cannot be reelected as mayor. On the contrary, on the right side, the incumbent is reelected in the council and, as we can see, the incumbent's probability to be reelected mayor jumps starkly by about 50-60 percentage points. The size of this jump is all the more impressive that when incumbent mayors are closely reelected in the municipal council, they are ranked among the council's least popular members and yet, by far, the ones with the highest chances to be elected mayor.

Table 3.3 confirms the visual impression of an impressive 50 percentage points jump in the probability to become mayor. The coefficient related to *IncumbentNotEligible*, which measures the discontinuity, is significant at the 1% level and ranges from 42 (column 1 with the local linear specification and half the CCT bandwidth) to 76 percentage points (column 4 with the polynomial specification) depending on the specification.

Importantly, the incumbent advantage appears to be similar for male and female incumbents. In Figure 3.A5 and Table 3.A1, I test for the equality of the discontinuity between male and female incumbent mayors using the usual specifications. They show that there is no statistical difference between the incumbent effect of male and female incumbent mayors.

5.2 Impact on Female Access to the Position of Mayor

We first consider the average impact of incumbency on female access to the position of mayor. Figure 3.4 displays the main graph. The horizontal axis represents the incumbent vote margin relative to the elimination threshold and the vertical axis corresponds to the share of women elected. Visually, it is difficult to discern a discontinuity in the share of female mayors when incumbents are not eligible for reelection. It seems to remain at about 20% on both sides of the cutoff. Table 3.4 confirms this visual impression. From column 1 to 4, the coefficient related to *IncumbentNotEligible* is not significant and, as we restrict the sample to narrower bandwidths closer to the threshold, point estimates even

turn negative and reach -0.061 in the specification of column 1 using half the reference CCT bandwidth, suggesting that the impact of incumbency is very limited if not nil.

This finding contrasts with existing research on the topic which has established positive correlations between incumbents' turnover rate and the share of women in politics at the regional or cross-country level. This stagnation could nevertheless hide displacement effects where women would manage to replace male incumbents at the cost of having female incumbents overwhelmingly replaced by male. To enquire, I distinguish the impact of incumbency depending on the sex of the incumbent mayor.

Figure 3.5 displays the two discontinuity graphs. Looking at municipalities with a male incumbent mayor (graph a), we observe that when the mayor is not eligible for reelection, the probability that a female becomes mayor jumps by more than 10 percentage points and is about 23%. Given that the vast majority of incumbents are men, this effect surely corresponds to the common expectation on the incumbency channel which is assumed to block women from accessing political positions in all cases. But since 18% of incumbents are women, their rate of replacement by men should be taken into account in order to obtain the net effect on the share of female politicians. In municipalities with a female incumbent mayor (graph b), when the mayor is not eligible for reelection, the probability that a woman becomes mayor drops sharply and falls below 20%.

Table 3.5 confirms the existence of these displacement effects. Columns 1 and 2 are respectively restricted to municipalities with male and female incumbents. Columns 3 to 5 interact the coefficient of interest with the sex of the incumbent. Looking at column 1, we see that when male incumbents are not eligible to their succession, the probability that a woman becomes mayor increases by about 16 percentage points. In column 2, we see that when female incumbents are not eligible to their succession, they are overwhelmingly replaced by male politicians. The probability that a woman becomes mayor sharply drops by about 62 percentage points. Columns 3 to 5 confirm the existence of these displacement effects using three different specifications including interaction terms. Consistent with the estimates obtained on subsamples, we see in column 3 that the baseline coefficient *IncumbentNotEligible* is about 0.18 while the interaction *IncumbentNotEligible * FemaleIncumbent* is of -0.9 (the slight differences with estimates from columns 1 and 2 are due to the different bandwidths).

6 Mechanisms - Are Women Penalized after a Female Incumbent?

The previous findings established that, when ineligible, a slight share of male incumbent mayors are replaced by women while an overwhelming share of female incumbent mayors are replaced by men. If female incumbent mayors were to be excessively replaced by men, this could explain why incumbency does not impede women from getting elected. This section investigates this channel.

6.1 Municipality Level Analysis

Visually, the share of newly elected female mayors seems to be lower after a female incumbent than a male one (see Figure 3.5). To statistically test this hypothesis, I restrict the sample to municipalities where the mayor is ineligible and estimate the probability that a woman is elected mayor depending on the sex of the incumbent .

The results are displayed in Table 3.6. Column 1 displays the results using the entire sample of ineligible incumbent mayors. We see that after a female incumbent, the probability to elect a female mayor is reduced by about 10 percentage points, out of a baseline of 23 %. This effect persists when we restrict the sample to municipalities where the incumbent fail by a narrow margin (column 2) and when we include control variables likely to represent the demand and supply for female politicians such as the share of women in the council, the total female vote share.

Linking this result to the conceptual framework of Section 2, it appears that the likelihood to elect a woman depends on the sex of the incumbent. Therefore, the model of Equation 11 which includes signal effects seems closer to what is observed in the data. Two elements provide support for this claim. First, there is a discrepancy between the increasingly favorable context towards the election of women suggesting that incumbency should block women's election and the absence of effect. Second, less women are elected after a female incumbent than a male one suggesting that the likelihood to elect a woman depends on the sex of the incumbent⁵².

⁵²An alternative hypothesis would be that male and female incumbents enjoy different incumbent advantages. While this hypothesis does not find support in the data (see Section C.1, its theoretical consequences are derived in Section A

6.2 Within Council Analysis

To further investigate the possibility that women suffer from a penalty after a female incumbent, I switch the level of analysis to the individual level within council. It allows to identify the size of the penalty and the categories of women who experience a penalty after a female incumbent. To do so, I estimate the following Equation:

$$Mayor_i = \alpha + \delta Woman_i + \delta FemaleIncumbent_c + \gamma FemaleIncumbent_c * Woman_i + f(X_c) + \epsilon_{ic} \quad (13)$$

Where i is the indicator for the individual and c for the city. $f(X_c)$ is a first degree polynomial controlling for the distance to the elimination threshold. $FemaleIncumbent_c$ is a dummy that equals 1 when the incumbent mayor is a woman. $Woman_i$ is a dummy that equals 1 if the councillor is a woman. $Mayor_i$ is the outcome variable. It is a dummy that equals 1 if the councillor is elected mayor.

This equation is estimated on the sample of incumbents who fail to be reelected by a narrow margin⁵³. If women suffer from a penalty after a female incumbent, we expect γ to be negative and δ to be positive indicating that the gender gap is larger in municipalities with a female incumbent. Moreover, because the newly elected mayor is often chosen among experienced councillors, we distinguish between councillors who served in the council during the previous term and those that are newly elected.

Table 3.7 displays the results. Estimates in column 1 are based on the entire sample of councillors while those of columns 2 and 3 are respectively restricted to inexperienced and experienced councillors. Looking at column 1, we see that the coefficient related to *Woman* is negative and significant at the 1% level indicating that after a male incumbent, the probability that a woman is elected mayor is 5 p.p. lower than the one of male councillors. The next two rows inform us on the differences between municipalities with a male and female incumbent. We see that the coefficient related to *FemaleIncumbent* is positive and the one related to *FemaleIncumbent * Woman* is negative and significant at the 1% level. This shows that the gender gap in the probability to be elected mayor is

⁵³The results are displayed using the bandwidth selected to determine the average impact of incumbency on the share of female mayors. I use only the bandwidth on the left-side of the elimination threshold. In the Appendix, Section C.2, I show that the findings are robust to a wide range of bandwidths closer to the threshold.

larger in cities with a female incumbent and at the disadvantage of women.

In columns 2 and 3, we can observe the heterogeneity depending on the experience of councillors. In column 2, we see that, for inexperienced councillors, the gender gap is not different in municipalities with a male and female incumbent. Women are less likely to be elected mayor but the effect is similar in both places. While in column 3, we observe a large gender gap among experienced councillors only in cities with a female incumbent mayor. This implies that the results observed on the entire sample of councillors are driven by the inability of experienced female councillors to be elected mayor.

These results suggest two different patterns depending on the sex of the incumbent. First, in cities headed by a man, the impact of incumbency on the access of women to the position of mayor seems to depend almost entirely on the pool of experienced female councillors. Since their probability to become mayor is the same as the one of male councillors, we can expect that if they were as numerous as their male colleagues, there would be as many female mayor than male mayors after a male incumbent mayor.

Second, in cities headed by a woman, there exists a larger gender gap in the propensity to replace the female incumbent and it is more difficult for women to replace a female incumbent than it is for a man. This effect is concentrated on the pool of experienced councillors. It is consistent with a signal effect where, in a context of low female political representation, the errors of past female politicians would be costly for future female leaders. This effect could come from insiders who discriminate against new women and vote less for them (backlash effect) or alternatively from a stereotype threat leading experienced female councillors to be reluctant at replacing the female incumbent due to her failure to be reelected.

7 Robustness and Alternative Mechanisms

We are inclined to attribute the differences observed in the previous section to signal effects which penalize potential female candidates after the loss of a female incumbent. Yet, other mechanisms could be at play. This section tests their relevance.

7.1 Gender Gap in Candidates' Quality?

It is possible that the larger gender gap to access the position of mayor in places where the incumbent is a woman comes from a larger gender gap in terms of quality of candidates. In particular, female councillors could be of lower quality in municipalities with a female incumbent. This could explain the different gender gaps.

To investigate this channel, I take advantage of all the data available on councillors, namely their age, profession (9 categories) and electoral outcome (measured by the rank at election in 2014 and the election round in 2014). Based on these information, I predict the probability to be elected mayor. I then estimate Equation 13 using this predicted probability as dependent variable. If differences in terms of observable characteristics mattered, we should observe a similar gender gap in the probability to become mayor in female-headed cities and nothing in male-headed cities.

The results are displayed in Table 3.8. Looking at the first row of column 1, we see that in municipalities with a male incumbent, female councillors are of lower quality than men. However, this gender gap in quality is *lower* in municipalities with a female incumbent, suggesting that women should access more often to the position of mayor. Indeed, the coefficient related to *FemaleIncumbent * Woman* is positive and significant at the 5% level. In column 2, we see that the greater quality of female councillors is concentrated on the sample of inexperienced councillors. Finally, in column 3, looking at the sample of experienced councillors, we do not observe a gender gap in quality, neither in municipalities with a male incumbent, nor in those with a female incumbent mayor.

These findings suggest that (i) inexperienced women should access more often to the position of mayor in municipalities with a female incumbent mayor and (ii) experienced female councillors should access to the position of mayor as often as their male counterparts in both types of cities. Therefore, differences in observable characteristics cannot explain the gender gap in the probability to become mayor after a female incumbent. On the contrary, they suggest that the gender gap should be lower.

7.2 Gender Gap in Incumbents' Performance?

One alternative explanation would be that incumbent female mayors are less successful than their male counterparts in running the municipality. If this was true, it could cast doubts on the competencies of female candidates to the position of mayor and impede them

from replacing the incumbent mayor. Measuring the performance of an individual running a municipality is an empirical challenge as it is obviously difficult to develop indicators encompassing all the dimensions of performance and because these dimensions may vary for each individual. For this reason, I follow the existing literature and focus on budget variables (Gagliarducci and Nannicini, 2013) and the population growth rate that has been identified as a predictor of economic growth rate (Glaeser *et al.*, 1995).

Budget variables come from an administrative dataset that includes fiscal and accounting characteristics of French municipalities from 2000 until 2015. I select three budget indicators per capita: total expenditure, total revenues and deficit. These variables are averaged over the term from 2009 until the end of 2013 (hence excluding election years). The population growth rate is measured with the official population figures of municipalities from 2009 until the end of 2013. Using these variables as outcomes, I estimate whether they differ significantly depending on the sex of the incumbent.

Table 3.A2 displays the results. The estimates suggest that none of these indicators differ depending on the sex of the incumbent. With the data at hand, there is thus little evidence that female incumbent are less successful in running the municipality than their male counterparts and therefore .

7.3 Gendered Composition of the Council

Finally, I implement a placebo test to tackle the potential role of the gendered composition of the municipal council. When an incumbent mayor becomes ineligible for reelection, the gendered composition of the council may change and alter the probability to elect a mayor of either sex. This could be true if individuals of same sex tended to vote for each other.

To alleviate this concern, I replicate the main results using the sample of incumbent councillors. For instance, if we observed that female (male) mayors were less likely to be elected when a female (male) incumbent councillor was not eligible, this would suggest that the gendered composition of the council matters. This exercise also serves as a placebo test to confirm that the results are driven by incumbency at the position of mayor and not simply at the position of councillor.

The results are displayed in Section C.4. There is no evidence of a jump in the probability to elect a woman when an incumbent councillor, man or woman, is not eligible for

the position of mayor (Table 3.A4 and 3.A5). This suggests that the results are driven by the ineligibility of the incumbent mayor and not by the mere loss of an experienced man/woman from the municipal council or by incumbency at any position within the council.

8 Conclusion

This article has investigated the impact of incumbency on the election of women. I exploited regression discontinuity from close electoral races in France to randomize the eligibility of incumbents for reelection. While it is often assumed that the persistence of incumbents hinders the election of women, providing that the majority of incumbents are men and that they enjoy an incumbent advantage, this article has revealed a more complex reality. In contrast with previous studies on the topic, I find that, on average, the persistence of incumbents at the position of mayor does not reduce the share of female mayors.

Investigating the underlying mechanisms, I showed that this is because women are penalized after a female incumbent. When incumbent mayors are not eligible for reelection, about 23% of male incumbents are replaced by women while only 13% of municipalities with female incumbents elect a new female mayor. These different replacement rates are explained by the existence of a large gender gap in the probability to be elected mayor among experienced councillors only in municipalities with a female incumbent. This is consistent with the existence of a signal effects where, in a context of low female political representation, the actions of past female politicians would act as a proxy for the success of future female leaders.

This mechanism has important implications. If women are penalized after a female incumbent, this could be a channel favoring men and restraining women's ascent to positions of power. It could also have implications outside the political sphere. For instance, in companies, women could be less likely to replace female executives than male ones when they have been laid off. Future research could therefore investigate the validity of this mechanism in other contexts and outside the political sphere.

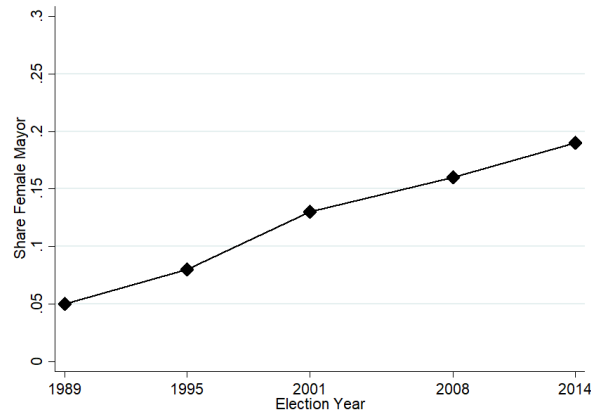
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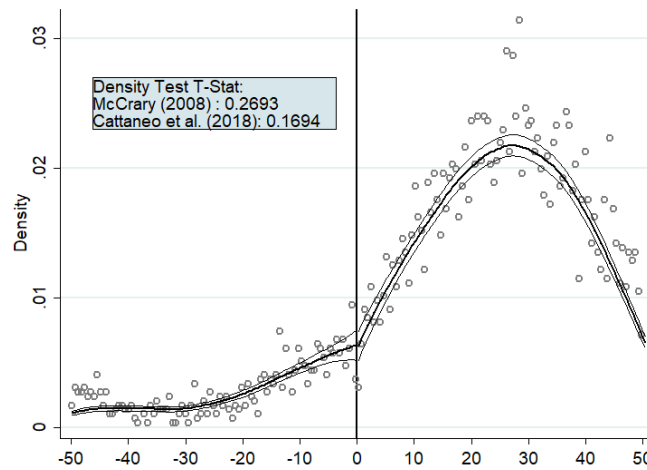
9 Figures

Figure 3.1: Evolution of the Share of Female Mayors from 1989 to 2014



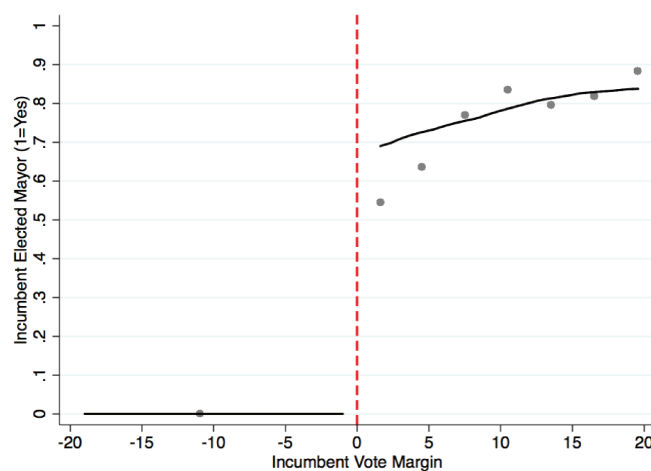
Notes: The data comes from the 1989, 1995, 2001, 2008 and 2014 French municipal elections results in municipalities below 1,000 inhabitants.

Figure 3.2: Manipulation Test



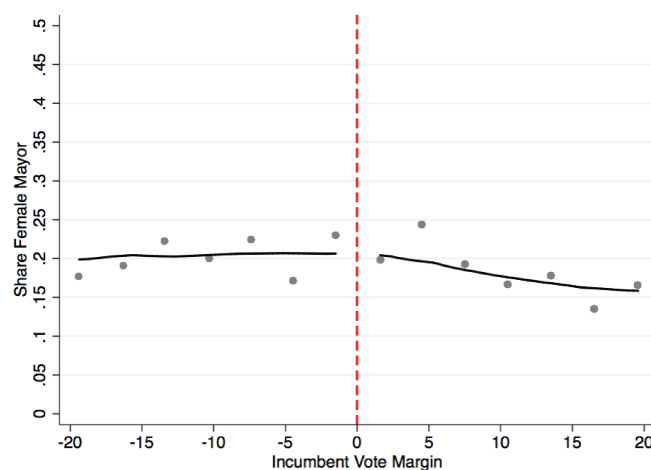
Notes: the data comes from the 2014 French municipal elections. The bin-size and bandwidth are of 0.68 and 16.203.

Figure 3.3: The Electoral Advantage of Incumbent Mayors



Notes: the data comes from the 2014 French municipal elections. The y-axis represents the share of incumbents reelected mayor. The x-axis represents the incumbent vote margin relative to the elimination threshold. The solid line represents a lowess fit of the bin-averages.

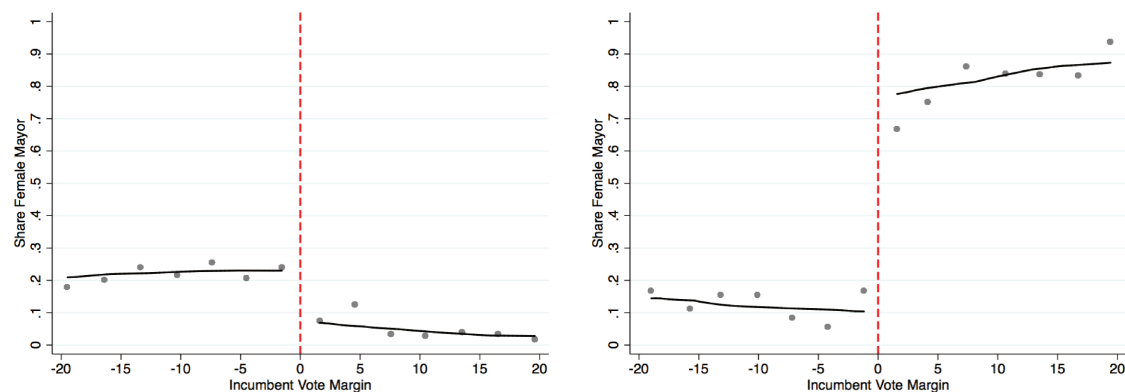
Figure 3.4: Consequences of Incumbency on the Share of Female Mayors



Notes: the data comes from the 2014 French municipal elections. The y-axis represents the share of female mayors. The x-axis represents the incumbent vote margin relative to the elimination threshold. The solid line represents a lowess fit of the bin-averages.

Figure 3.5: Displacement Effects

(a) Male Incumbents, outcome: Female Mayor (b) Female Incumbents, outcome: Female Mayor



Notes: the data comes from the 2014 French municipal elections. The y-axis represents the share of female mayors. The x-axis represents the incumbent vote margin relative to the elimination threshold. The left-graph (a) is restricted to municipalities with male incumbent mayors and the graph on the right (b) to those with female incumbent mayors.

10 Tables

Table 3.1: Descriptive Statistics

	Mean (1)	S.D (2)
<i>Panel A: Incumbent Mayor Level</i>		
Female (1=Yes)	0.16	0.36
Age	55.46	8.62
Rank Election 14	6.63	4.81
Vote Margin	24.81	26.71
Observations	5455	
<i>Panel B: Municipality Level</i>		
Female Mayor 89	0.05	0.22
Female Mayor 95	0.08	0.27
Female Mayor 01	0.13	0.33
Female Mayor 08	0.16	0.36
Share Female Councillors	0.34	0.12
Female Vote Share	0.31	0.13
Δ Female Vote Share (2014 - 2008)	0.03	0.09
Δ Share Female Council (2014 - 2008)	0.04	0.12
N Registered Voters	298.57	189.40
Abstention Rate	0.20	0.08
N Candidates Municipality	17.73	6.68
N Elected Municipality	11.70	2.42
Observations	5455	

Notes: the data comes from the 2014 French municipal elections. Panel A refers to descriptive statistics on incumbent mayors and Panel B to municipalities. Columns 1 and 2 respectively display the mean and the standard deviation of each variable.

Table 3.2: Balance Checks - Continuity Assumption

	<i>Local Linear Regression</i>		
	Discontinuity Estimate	Bandwidth Restriction	N Obs
	(1)	(2)	(3)
FemaleIncumbentMayor	-.058 (.045)	16.96	1477
ShareFemaleCouncil14	.007 (.015)	17.46	1526
FemaleVoteShare14	.016 (.015)	18.73	1652
VariationFemaleVoteShare	.007 (.013)	15.85	1350
VariationShareFemaleCouncil	.011 (.016)	16.1	1384
NTotalCandidates	.236 (.796)	18.42	1629
ShareFemaleCandidates	-.01 (.011)	18.93	1672
NRegisteredVoters	6.706 (20.503)	20.88	1898
AbstentionRate	.006 (.01)	14.29	1204
SizeCouncil	.337 (.294)	15.65	1339
IncumbentAge	-.257 (1.092)	14.67	1234
NTerm	.02 (.09)	18.52	1639

Notes: the data comes from the 2014 French municipal elections. Standard errors clustered at the municipality level are given in parentheses. Each line corresponds to one dependent variable. Column 1 displays the discontinuity estimate, column 2 the bandwidth restriction and column 3 the number of observations. Column 1 to 3 correspond to a local linear regression around the cutoff that allows for a break in the slope at the cutoff. The bandwidth is computed following the CCT approach.

Table 3.3: Incumbency and the Inertia at the Position of Mayor

<i>Dep. Variable: Incumbent is Reelected Mayor (1=Yes)</i>				
	(1)	(2)	(3)	(4)
	LLR	LLR	LLR	Poly
	CCT/2	CCT	IK	
IncumbentNotEligible	-0.42*** (0.08)	-0.52*** (0.05)	-0.56*** (0.05)	-0.76*** (0.02)
Bandwidth Restriction	6.33	12.67	14.29	None
Observations	444	1028	1204	5774

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. Standard errors are given in parentheses. The dependent variable is a dummy that equals 1 if the municipality elects a female mayor. Columns 1, 2 and 3 correspond to a local linear regression around the cutoff using respectively half the CCT, the CCT and the IK bandwidth. Column 4 corresponds to the specification that fits a second order polynomial. Specifications in columns 1, 2 and 3 further allow for a break in the slope of the running variable at the cutoff.

Table 3.4: Consequences of Incumbency on the Share of Female Mayors

<i>Dep. Variable: Female Mayor (1=Yes)</i>				
	(1)	(2)	(3)	(4)
	LLR	LLR	LLR	Poly
	CCT/2	CCT	IK	
IncumbentNotEligible	-0.061 (0.059)	-0.008 (0.040)	0.019 (0.029)	0.016 (0.023)
Bandwidth Restriction	11.06	22.13	32.90	None
Observations	877	2040	5498	5774

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. Standard errors are given in parentheses. The dependent variable is a dummy that equals 1 if the municipality elects a female mayor. Columns 1, 2 and 3 correspond to a local linear regression around the cutoff using respectively half the CCT, the CCT and the IK bandwidth. Column 4 corresponds to the specification that fits a second order polynomial. Specifications in columns 1 and 2 further allow for a break in the slope of the running variable at the cutoff.

Table 3.5: Behind the Stagnant Flow: Hidden Displacement Effects

<i>Dep. Variable: Female Mayor (1=Yes)</i>					
	(1)	(2)	(3)	(4)	(5)
	LLR	LLR	LLR	LLR	Poly
	CCT	CCT	CCT	IK	
IncumbentNotEligible	0.16*** (0.03)	-0.62*** (0.10)	0.18*** (0.03)	0.18*** (0.02)	0.19*** (0.02)
IncumbentNotEligible*FemaleIncumbent			-0.90*** (0.04)	-0.96*** (0.03)	-0.98*** (0.02)
Incumbent Sex	Male	Female	All	All	All
Bandwidth Restriction	22.53	18.18	22.13	32.90	None
Observations	1733	283	2040	5498	5774

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. Standard errors are given in parentheses. The dependent variable is a dummy that equals 1 if the municipality elects a female mayor. Columns 1, 2, 3 and 4 correspond to a local linear regression around the cutoff using respectively the CCT and the IK bandwidth. Column 3 corresponds to the specification that fits a second order polynomial. Specifications in columns 1, 2, 3 and 4 further allow for a break in the slope of the running variable at the cutoff.

Table 3.6: Lower Share of Female Mayors After a Female Incumbent?

<i>Dep. Variable: Female Mayor (1=Yes)</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Female Incumbent Mayor	-0.107*** (0.037)	-0.106** (0.048)	-0.122* (0.062)	-0.112*** (0.036)	-0.110** (0.047)	-0.121** (0.060)
Controls	No	No	No	Yes	Yes	Yes
Bandwidth Restriction	None	[-22.13;0]	[-11.06;0]	None	[-22.13;0]	[-11.06;0]
Observations	798	446	287	798	446	287

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. The sample is restricted to municipalities with incumbents who were not reelected in the council. Standard errors are given in parentheses. The dependent variable is a dummy that equals 1 if the municipality elects a female mayor. All specifications control for the distance to the elimination threshold. Specifications in columns 4, 5 and 6 additionally control for the share of female councillors in the council and the total female vote share in 2014.

Table 3.7: Gender Gap to be Elected Mayor

<i>Dep. Variable: Elected Mayor (1=Yes)</i>			
	(1)	(2)	(3)
Woman	-0.05*** (0.01)	-0.05*** (0.01)	-0.02 (0.03)
FemaleIncumbent	0.02** (0.01)	0.00 (0.01)	0.10*** (0.03)
FemaleIncumbent*Woman	-0.04*** (0.01)	-0.01 (0.01)	-0.18*** (0.05)
Sample Councillors	All	Inexperienced	Experienced
Bandwidth Restriction	[-22.13;0]	[-22.13;0]	[-22.13;0]
Observations	5379	4138	1241
Municipalities	446	445	379

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. The sample is restricted to municipalities with incumbents who were not reelected in the council. Standard errors clustered at the municipality level are given in parentheses. The dependent variable is a dummy that equals 1 if the municipality elects a female mayor.

Table 3.8: Is the Gender Gap Driven by Differences in Observable Characteristics?

<i>Dep. Variable: Predicted Probability Elected Mayor (1=Yes)</i>			
	(1)	(2)	(3)
Woman	-0.024*** (0.002)	-0.012*** (0.002)	-0.003 (0.004)
FemaleIncumbent	-0.009** (0.004)	-0.004 (0.003)	0.006 (0.006)
FemaleIncumbent*Woman	0.010** (0.005)	0.011** (0.005)	-0.012 (0.010)
Sample Councillors	All	Inexperienced	Experienced
Bandwidth Restriction	[-22.13;0]	[-22.13;0]	[-22.13;0]
Observations	5338	4110	1228
Municipalities	445	444	379

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. The sample is restricted to municipalities with incumbents who were not reelected in the council. Standard errors clustered at the municipality level are given in parentheses. The dependent variable is a dummy that equals 1 if the municipality elects a female mayor.

11 Appendix

A Incorporating an incumbent advantage that depends on the sex of the incumbent

Assume that the incumbent advantage depends on the sex of the incumbent. Male incumbents enjoy an advantage equals to β_M and female incumbents to β_F . Rewriting Equation 9 leads to :

$$S_{t+1}^F = F_t * \beta_F + F_t * (1 - \beta_F) * F_{t+1} + (1 - F_t) * (1 - \beta_M) * F_{t+1} \quad (14)$$

Where the first term corresponds to the share of female incumbents reelected, the second to the share of female incumbents who are replaced by newly elected women and the third term to the share of male incumbents replaced by a woman. Rearranging these terms leads to:

$$S_{t+1}^F = F_{t+1} + \beta_F F_t [1 - F_{t+1}] - \beta_M F_{t+1} [1 - F_t] \quad (15)$$

Where we see that the share of women elected in second period increases with the incumbent advantage of women and decreases with the one of males.

Prediction: if $\frac{\beta_F}{\beta_M} > \frac{F_{t+1}[1-F_t]}{F_t[1-F_{t+1}]}$, then the share of women elected in second period will be higher than the one in the first period.

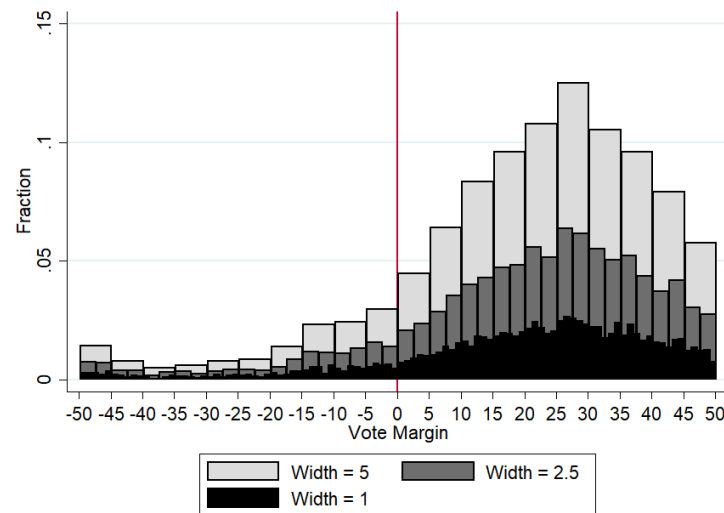
This prediction has two important consequences. First, it is possible that the share of female elected varies across the two periods even when the demand or supply of female politicians remains the same, i.e. when $F_t = F_{t+1}$. Second, if male incumbents enjoy a greater incumbent advantage, the variation of the context towards the election of women must be substantial to reach a higher share of female elected.

B Validity Tests

B.1 Internal Validity

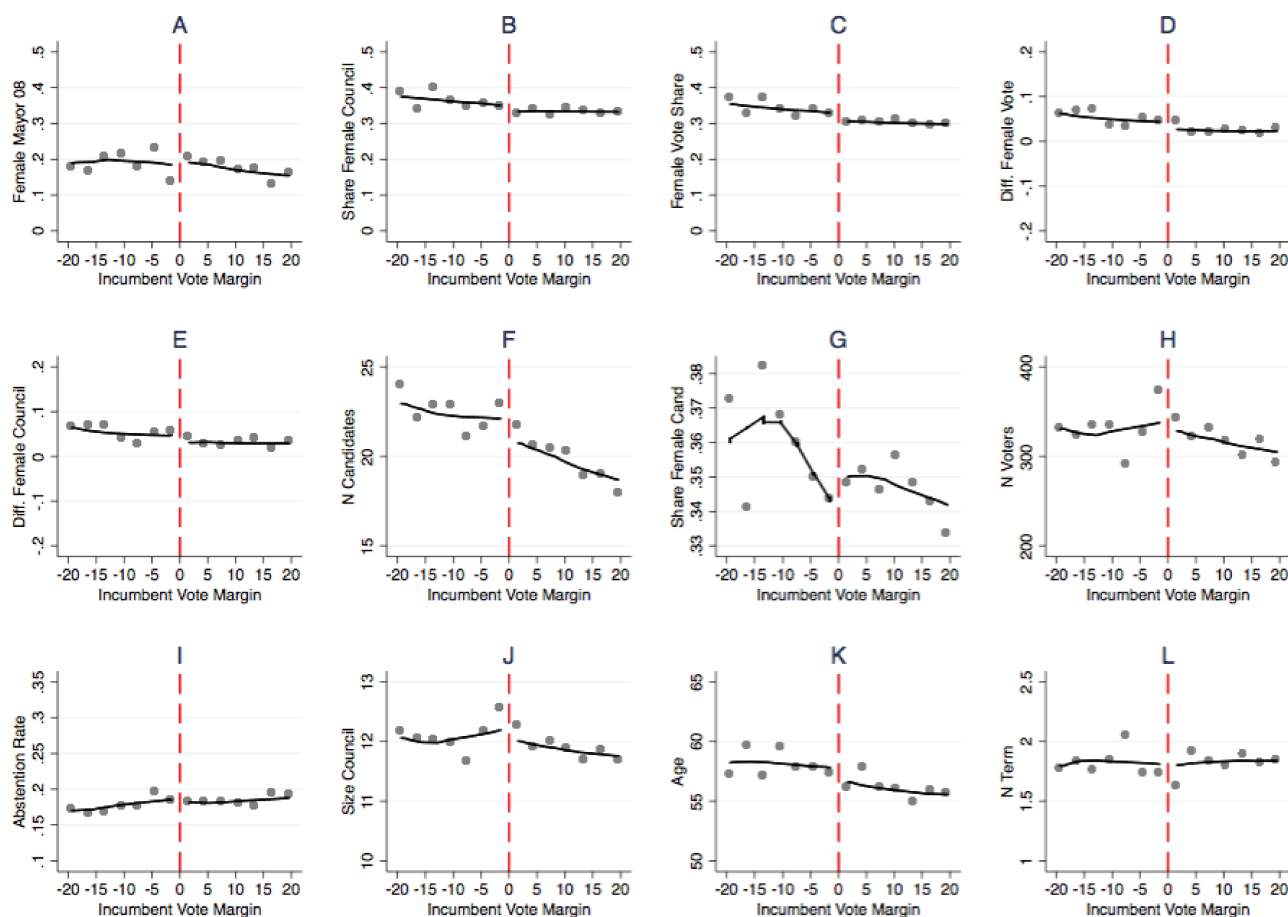
This section provides additional validity tests supporting the empirical strategy. Figure 3.A1 first plots histograms representing the distribution of the running variable with different widths. They show no evidence of manipulation. Figure 3.A2 depicts the behavior of each of the covariates tested in Table 3.2 for the continuity assumption around the threshold.

Figure 3.A1: Histogram Distribution Running Variablen



Notes: The data comes from the 2014 French municipal elections. The horizontal axis represents the incumbent vote margin relative to the elimination threshold.

Figure 3.A2: Testing the Continuity Assumption

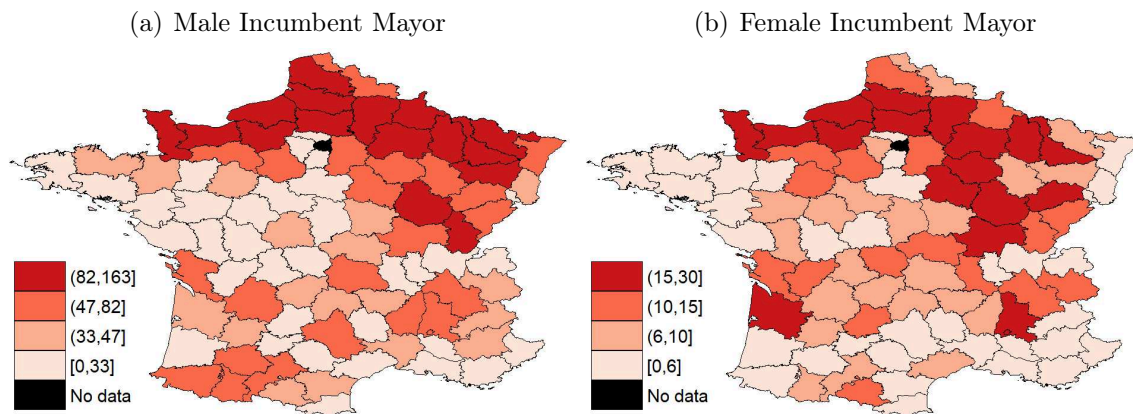


Notes: The data comes from the 2014 French municipal elections. The black solid line corresponds to a lowess fit of the bin-averages. Dependent variables are for graph A: a dummy that equals 1 if the incumbent mayor is a woman; for graph B: the share of women in the municipal council in 2014; graph C: the total female vote share in 2014; graph D: the variation of the female vote share between 2014 and 2008; graph E: the variation of the share of women in the council between 2014 and 2008; graph F: the total number of candidates in 2014; graph G: the share of female candidates in 2014; graph H: the number of registered voters; graph I: the abstention rate at the 2014 election; graph J: the size of the municipal council; graph K: the age of the incumbent; graph L: the number of term done by the incumbent mayor.

B.2 External Validity

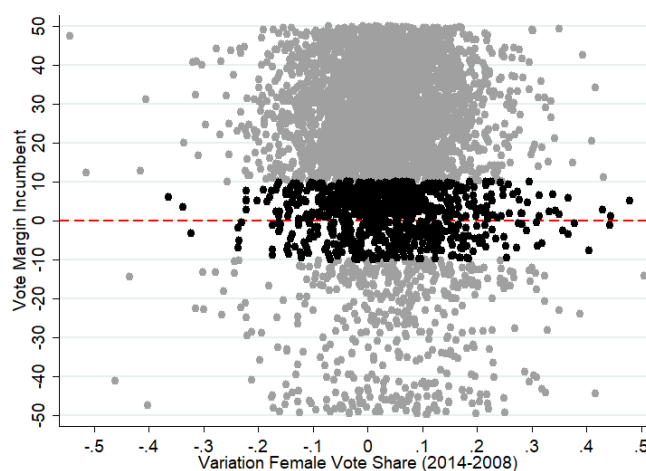
Figure 3.A3 depicts the distribution of the observations on the French territory. Importantly, we see that they are scattered throughout the territory with a larger concentration in the north and east of France. Figure 3.A4 shows the degree of heterogeneity for the variation of the total female vote share. We see that for municipalities in the close race sample, this variation is concentrated in the $[-0.2; 0.2]$ interval.

Figure 3.A3: Where Are the Close-Races?



Notes: The data comes from the 2014 French municipal elections. Each class represents a quantile of the distribution of observations.

Figure 3.A4: Variation Preferences for Women in Close Race Elections



Notes: the data comes from the 2014 French municipal elections. The graph represents the variation of the total vote share of women on the horizontal axis between 2014 and 2008 and the incumbent vote margin on the vertical axis. The black dots are located in a 10 points interval around the elimination threshold.

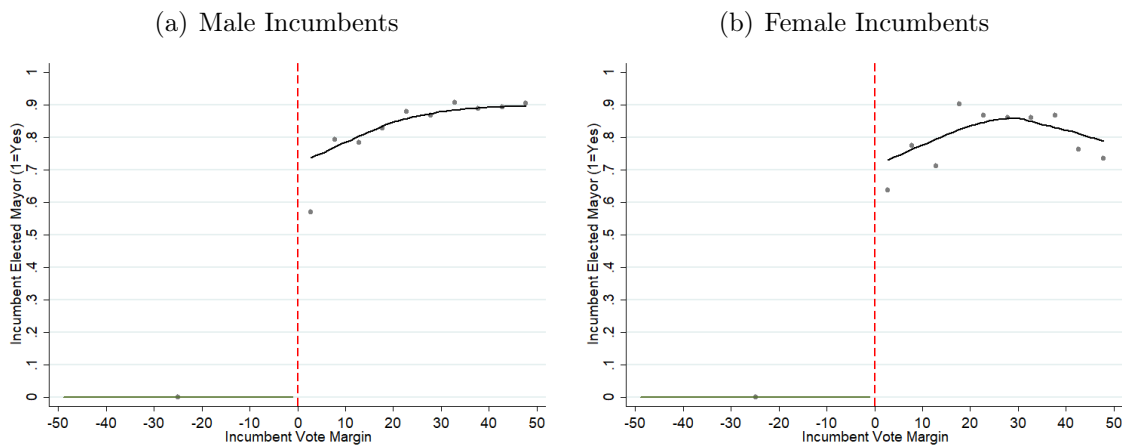
C Robustness Results

This section describes further robustness tests. First, I examine how the electoral advantage of incumbents depends on the incumbent's sex. Second, I test the sensitivity of the results to the size of the bandwidth. Third, I check whether female incumbents perform worse than male incumbents. Finally, I run a placebo test replicating the main results on the sample of incumbent councillors.

C.1 Impact of Incumbency by sex

Figure 3.A5 depicts the discontinuity graph of the impact of incumbency on the probability to be elected mayor depending on the sex of the incumbent. Visually, the two graphs look very similar suggesting that male and female incumbents enjoy a similar advantage. Table 3.A1 verifies that the two jumps are not statistically different. The coefficient related to *IncumbentNotEligible * Woman*, which measures the difference in the discontinuity, is not significant in the three specifications.

Figure 3.A5: Does the Impact of Incumbency Depend on the Sex of the Incumbent?



Notes: the data comes from the 2014 French municipal elections. The sample is respectively restricted to male incumbents and female incumbents in graph (a) and (b). The y-axis represents the share of incumbents reelected mayor. The x-axis represents the incumbent vote margin relative to the elimination threshold. The solid line represents a lowess fit of the bin-averages. There are 10 bins on the right-side of the cutoff.

Table 3.A1: Does the Impact of Incumbency Depend on the Sex of the Incumbent?

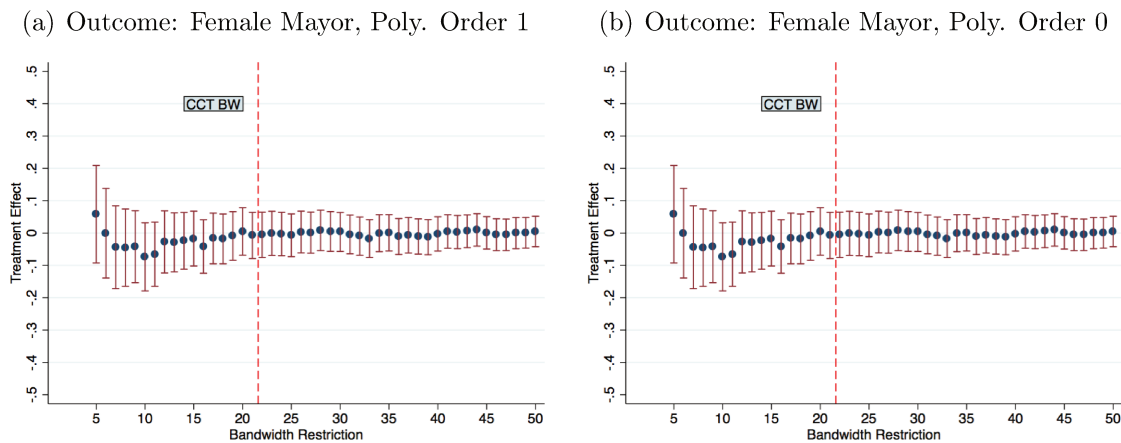
<i>Dep. Variable: Incumbent is Reelected Mayor (1=Yes)</i>			
	(1) LLR CCT	(2) LLR IK	(3) Poly
IncumbentNotEligible	-0.45*** (0.06)	-0.55*** (0.05)	-0.75*** (0.02)
IncumbentNotEligible*FemaleIncumbent	-0.02 (0.07)	0.03 (0.06)	0.03 (0.03)
Bandwidth Restriction	10.89	14.29	None
Observations	796	1115	5455

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. Standard errors clustered at the municipality level are given in parentheses. Columns 1 and 2 correspond to a local linear regression around the cutoff using respectively the CCT and the IK bandwidth. Column 3 corresponds to the specification that fits a second order polynomial. Specifications in columns 1 and 2 further allow for a break in the slope of the running variable at the cutoff.

C.2 Alternative Bandwidths

This section tests the sensitivity of the results to different bandwidths. Figure 3.A6 replicates the estimates of Table 3.4. Figures 3.A7 and 3.A8 replicate the estimates of Table 3.5. Figure 3.A9 replicates the estimates of Table 3.6. Figure 3.A10 replicates the estimates of Table 3.7. All bandwidths with positive integers from 5 to 50 are used on different specifications (polynomial of first and zero order). The results appear to be robust to smaller and larger bandwidth than the CCT bandwidth.

Figure 3.A6: Alternative Bandwidths - All Incumbents



Notes: These figures test the robustness of the results in Table 3.4 to alternative bandwidths. The data comes from the 2014 French municipal elections. The x-axis represents the size of the bandwidth. The y-axis represents the probability that a woman is elected mayor (1=Yes). The dots represent the coefficient associated to the variable *IncumbentNotEligible*. Confidence intervals are represented at the 95% level. The vertical red dashed line represents the CCT bandwidth.

Figure 3.A7: Alternative Bandwidths - Male incumbents

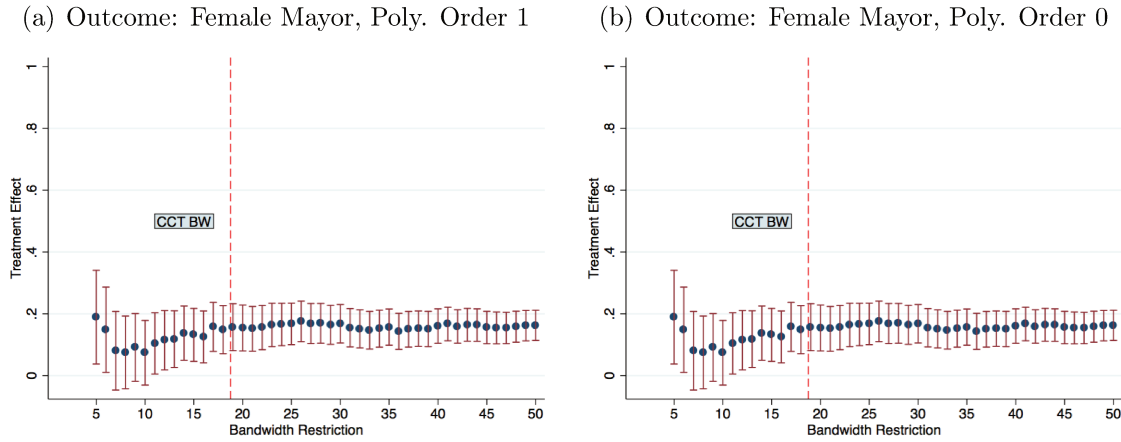


Figure 3.A8: Alternative Bandwidths - Female Incumbents

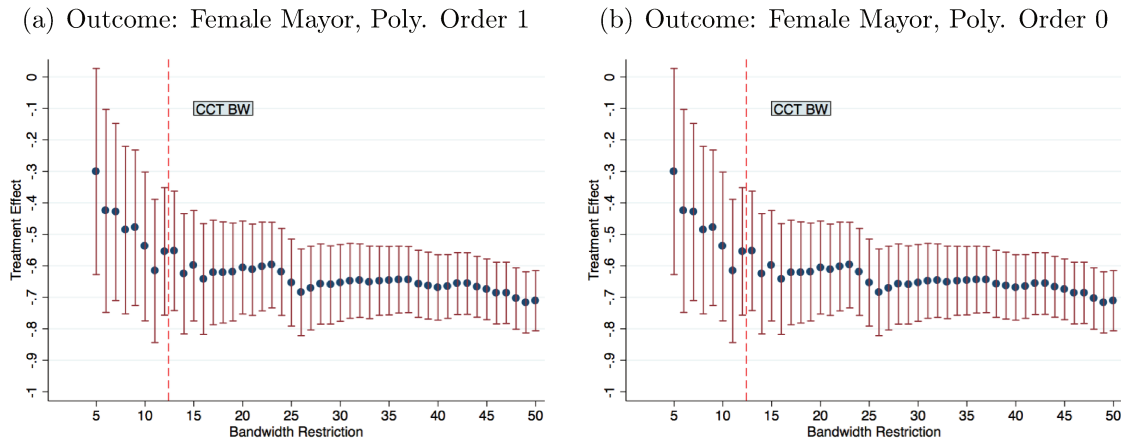
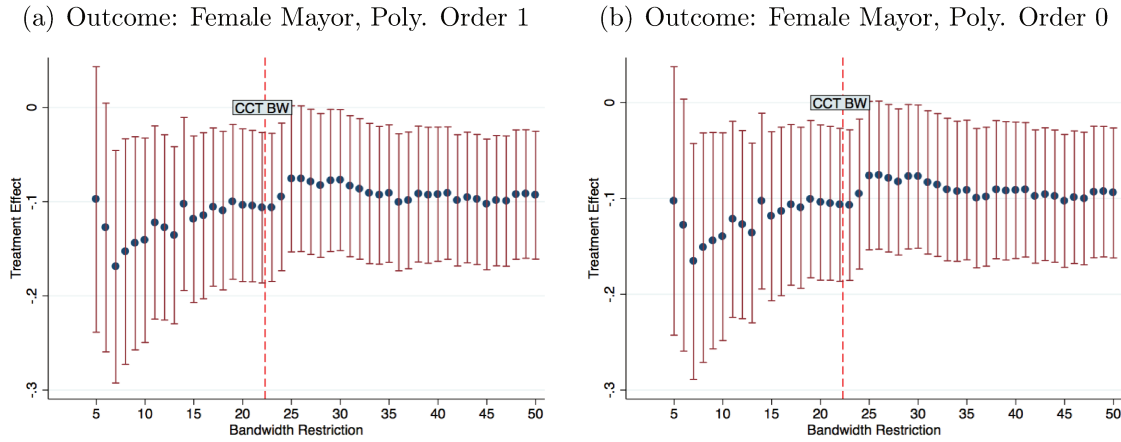
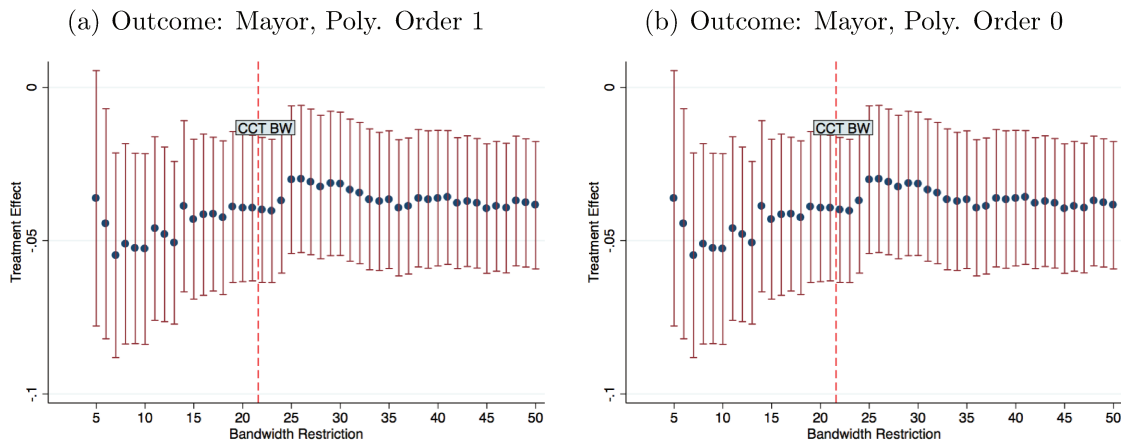


Figure 3.A9: Alternative Bandwidths - Fewer Female Mayors After a Female Incumbent



Notes: These figures test the robustness of the results in Table 3.6 to alternative bandwidths. The data comes from the 2014 French municipal elections. The sample is restricted to municipalities with incumbents who were not reelected in the council. The x-axis represents the size of the bandwidth. The y-axis represents the probability that a woman is elected mayor (1=Yes). The dots represent the coefficient associated to the variable *FemaleIncumbent*. Confidence intervals are represented at the 95% level. The vertical red dashed line represents the CCT bandwidth.

Figure 3.A10: Alternative Bandwidths - Gender Gap Elected Mayor



Notes: These figures test the robustness of the results in Table 3.7 to alternative bandwidths. The data comes from the 2014 French municipal elections. The sample is restricted to municipalities with female incumbents. The x-axis represents the size of the bandwidth. The y-axis represents the probability that a woman is elected mayor (1=Yes). The dots represent the coefficient associated to the variable *FemaleIncumbent * Woman*. Confidence intervals are represented at the 95% level. The vertical red dashed line represents the CCT bandwidth.

C.3 Gender Gap in Incumbent's Performance

Table 3.A2: Are Female Incumbent Mayors Less Performant?

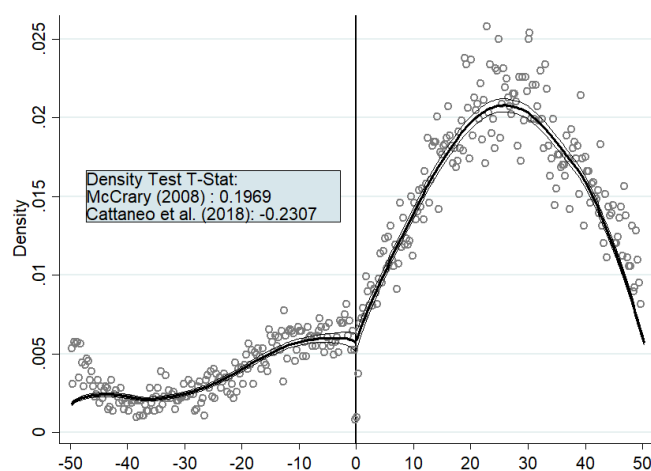
Dependent Variable	(1) Population Growth	(2) Expenditures	(3) Revenues	(4) Deficit
Female Incumbent Mayor	-0.001 (0.011)	-89.293 (105.224)	-91.772 (123.556)	-2.043 (27.061)
Bandwidth Restriction	None	None	None	None
Observations	448	448	448	448

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. The sample is restricted to municipalities with incumbent mayors who failed to be reelected in the council. The outcome in column 1 corresponds to the population growth rate from 2009 until the end of 2013. Outcomes in columns 2 to 4 are measured in euros, per capita and averaged over the entire term excluding election years. All columns fit an OLS regression with Female Incumbent Mayor and the distance to the eligibility threshold as controls variables.

C.4 Analysis on the Sample of Incumbent Councillors

This section replicates the main analysis on the sample of incumbent councillors. Their non reelection should not trigger similar results. I first provide evidence supporting the internal validity of the analysis on this sample. Second, I show that when they are not eligible, we do not find evidence of displacement effects.

Figure 3.A11: Manipulation Test



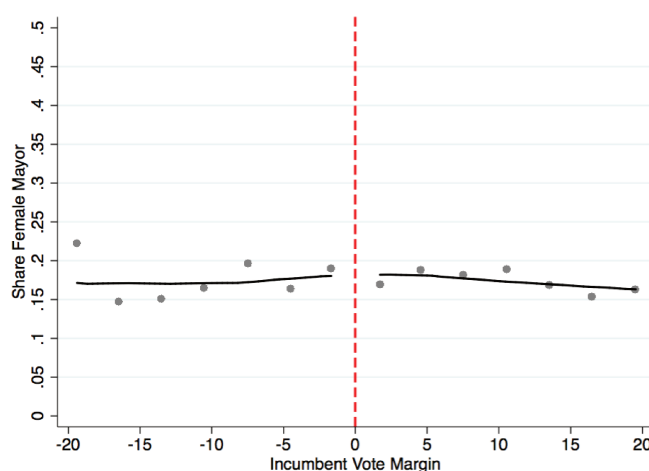
Notes: the data comes from the 2014 French municipal elections. The bin-size and bandwidth are of 0.27 and 11.32.

Table 3.A3: Balance Checks - Continuity Assumption

	<i>Local Linear Regression</i>		
	Discontinuity	Bandwidth	N Obs
	Estimate	Restriction	
	(1)	(2)	(3)
FemaleIncumbentMayor	-.011 (.021)	13.69	6643
ShareFemaleCouncil14	.007 (.007)	16.81	8672
FemaleVoteShare14	.007 (.007)	17.59	9146
VariationFemaleVoteShare	.006 (.005)	21.69	11880
VariationShareFemaleCouncil	.001 (.006)	20.15	10901
NTotalCandidates	.485 (.489)	11.49	5291
ShareFemaleCandidates	.001 (.005)	17.96	9340
NRegisteredVoters	14.581 (12.527)	12.26	5763
AbstentionRate	.019** (.006)	11.43	5267
SizeCouncil	.154 (.147)	11.81	5486
IncumbentAge	.448 (.51)	18.96	10028
NTerm	-.039 (.043)	16.02	8159

Notes: the data comes from the 2014 French municipal elections. Standard errors clustered at the municipality level are given in parentheses. Each line corresponds to one dependent variable. Column 1 displays the discontinuity estimate, column 2 the bandwidth restriction and column 3 the number of observations. Column 1 to 3 correspond to a local linear regression around the cutoff that allows for a break in the slope at the cutoff. The bandwidth is computed following the CCT approach.

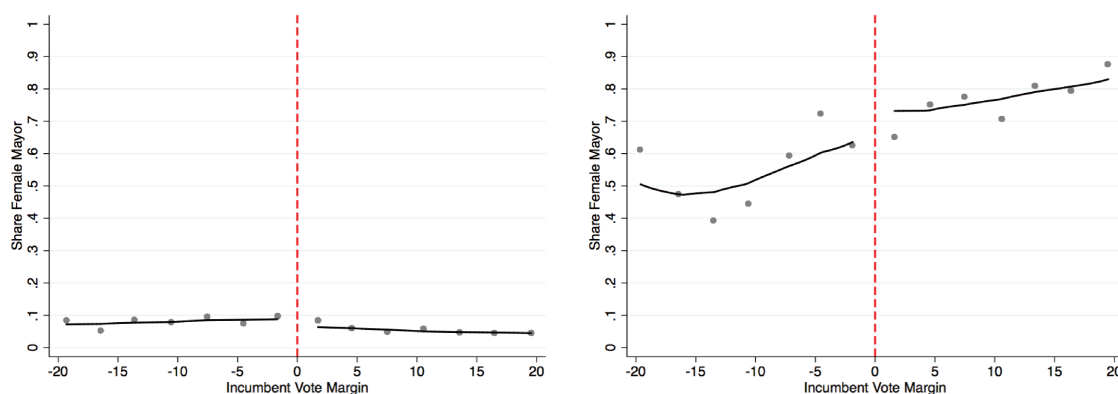
Figure 3.A12: Average Impact of Incumbency - Placebo Test Incumbent Councillor Sample



Notes: the data comes from the 2014 French municipal elections. The y-axis represents the share of female mayors. The x-axis represents the incumbent councillor vote margin relative to the elimination threshold. The solid line represents a lowess fit of the bin-averages. There are 10 bins on each side of the cutoff.

Figure 3.A13: Displacement Effects - Placebo Test Incumbent Councillor Sample

(a) Male Incumbents, outcome: Female Mayor (b) Female Incumbents, outcome: Female Mayor



Notes: the data comes from the 2014 French municipal elections. The y-axis represents the share of female mayors. The x-axis represents the incumbent vote margin relative to the elimination threshold. The left-graph (a) is restricted to municipalities with male incumbent councillors and the graph on the right (b) to those with female incumbent councillors

Table 3.A4: Placebo Test - Consequences of Incumbency at the Councillor Position on the Share of Female Mayors

<i>Dep. Variable: Female Mayor (1=Yes)</i>				
	(1)	(2)	(3)	(4)
	LLR	LLR	LLR	Poly
	CCT/2	CCT	IK	
IncumbentNotEligible	0.02 (0.03)	0.02 (0.02)	0.01 (0.01)	0.01 (0.01)
Bandwidth Restriction	5.96	11.91	44.05	None
Observations	1615	3650	16680	22055

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. The running variable is defined with respect to incumbent councillors' vote share. The variable *IncumbentNotEligible* is equal to one when the incumbent councillor is not reelected in the council. Standard errors clustered at the municipality level are given in parentheses. The dependent variable is a dummy that equals 1 if the municipality elects a female mayor. Columns 1, 2 and 3 correspond to a local linear regression around the cutoff using respectively half the CCT, the CCT and the IK bandwidth. Column 4 corresponds to the specification that fits a second order polynomial. Specifications in columns 1, 2 and 3 further allow for a break in the slope of the running variable at the cutoff.

Table 3.A5: Placebo Test - Displacement Effects

<i>Dep. Variable: Female Mayor (1=Yes)</i>				
	(1)	(2)	(3)	(4)
	LLR	LLR	LLR	Poly
	CCT/2	CCT	IK	
Panel A: Male Incumbents				
IncumbentNotEligible	0.02 (0.02)	0.02 (0.02)	0.02* (0.01)	0.02* (0.01)
Bandwidth Restriction	5.96	11.91	44.05	None
Observations	2666	6486	15641	20702
Panel A: Female Incumbents				
IncumbentNotEligible	-0.02 (0.17)	0.06 (0.12)	-0.16** (0.07)	-0.17*** (0.06)
Bandwidth Restriction	7.62	15.23	44.05	None
Observations	145	353	1039	1353

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The data comes from the 2014 French municipal elections. The sample is restricted to cities with male incumbent mayors in Panel A and female incumbent mayors in Panel B. The running variable is defined with respect to incumbent councillors' vote share. The variable *IncumbentNotEligible* is equal to one when the incumbent councillor is not reelected in the council. Standard errors clustered at the municipality level are given in parentheses. The dependent variable is a dummy that equals 1 if the municipality elects a female mayor. Columns 1, 2 and 3 correspond to a local linear regression around the cutoff using respectively half the CCT, the CCT and the IK bandwidth. Column 4 corresponds to the specification that fits a second order polynomial. Specifications in columns 1, 2 and 3 further allow for a break in the slope of the running variable at the cutoff.

Main Conclusion

This dissertation presented three essays on the topic of gender, institutions and politics. Three questions were studied: can institutions undo gender? Would institutions be more gender-egalitarian if they had more women heading them? Why are women absent from such positions?

In the first chapter, we studied whether institutions have the potential to undo gender. Since gender-equalizing institutions are much more likely to emerge in an environment where mentalities have already become more gender friendly, the main empirical challenge was to disentangle the influence of institutions from the pre-existing culture. To do so, we used the German division as a natural experiment. Before the division, gender norms were essentially similar in Eastern and Western regions. During the division, two completely different sets of institutions were applied, the eastern ones being much more gender equalizing than their western counterparts. After reunification, the government of the former Federal Republic of Germany (FRG) took over East Germany and rapidly dissolved its institutions and structures and absorbed them into those of West Germany, which remained unchanged.

We showed that since reunification, the male breadwinner norm has been prevalent in West Germany but not in the East. East German women can earn more than their husbands without having to increase their number of housework hours, put their marriage at risk or withdraw from the labor market. By contrast, the norm of higher male income, and its consequences, are still prevalent in West Germany.

The main limits of this chapter are on the exact mechanism driving the lower gender gaps in the East. Is it the communist ideology or the public policies that were set up during the communist period? To disentangle these two mechanisms, an interesting empirical strategy could be to exploit the reception of West German television. In the Western part of East Germany, it was possible to watch West German television and therefore, these

people should have been less exposed to the communist institutions.

The German setting is an exceptional laboratory to study the fabric of gender roles. Yet, few studies have used this setting. It could be exploited to study the resilience of many gender gaps. For instance, with Claudia Senik, we used this setting to study the resilience of the gender gap in mathematics. We showed that, as compared to West Germany, the gender gap in mathematics is lower in East Germany among teenagers. We also plan to run experiments to study psychological differences. On a different note, the persistence of gender gaps on the labor market could be studied. For instance, an ongoing study analyzes the child penalties in East and West Germany on the labor market and show that it is much lower in the East.

In the second chapter, I studied whether female politicians have the same priorities as their male counterparts. Many studies have tried to answer this question. Yet, there is no consensus on the answer. First, there is a discrepancy between the results obtained in developed and developing countries. In developing countries, there is evidence showing that women deliver different types of policies but this has been harder to replicate in the setting of developed countries. Second, there are important data limitations that limit the comparability of existing studies. Researchers are often constrained by existing datasets which vary from one country to another. Therefore, the main contribution of the second chapter is to derive a new and replicable method that could lead to the production of comparable studies across contexts. This method combines text analysis to select the topics of policies with quasi-experimental variations to randomize legislators' gender.

Using this method, I showed that female legislators are about twice more active than their male counterparts on women's issues. As compared to other topics, women's issues is the key topic on which women are more active relatively to men, followed by health and childhood issues whereas men are more active on military issues. I also provide evidence that these results are driven by the individual interest of legislators. Finally, I use a difference-in-differences strategy exploiting the introduction of a gender quota to show that these results are externally valid and that gender quotas could increase the prevalence of women's issues.

The main limits of this chapter are on the general equilibrium effects of the entry of female politicians. While the empirical evidence unambiguously points towards the fact that women focus on different topics than men, it is difficult to provide a precise answer to the question of how much the prevalence of women's issues has increased due to the

entry of women in the French Parliament. This is because there could exist a substitution effect where amendements defended by women would have been defended by men in their absence. Unfortunately, with the data at hands, it is difficult to measure this substitution effect.

The use of text analysis along with quasi-experimental variations is a fruitful avenue for future research. First, this method could be used in alternative settings to understand when female politicians promote more women friendly policies than their male counterparts. Second, it could be used to study alternative dimensions of identity. Examples of such dimensions include past occupations and income, ethnicity, caste, religion, etc. It is still questionable whether these dimensions matter for policymaking and this methodology could shed light on these questions.

Finally, the last chapter of this dissertation has investigated why women are underrepresented from political positions. I tested whether the persistence of incumbents hinders the election of female politicians. This mechanism is often assumed to slow down the ascent of women in politics. The argument is that, as most politicians are men and as most politicians enjoy an incumbent advantage for reelection, the entry of new candidates, including women, is limited.

Exploiting regression discontinuity to randomize the eligibility of incumbents for reelection in French municipalities, I found that the persistence of incumbents does not block female access to the position of mayor. I investigated the mechanisms and showed that it is more difficult for a woman to replace a female incumbent than a male one.

The main limits of this chapter are on the exact mechanisms driving the penalty affecting women. First, since the sex of the incumbent is not randomized, it is difficult to rule out that differences in unobservables characteristics between male and female headed municipalities do not drive the results. Nevertheless, it could be argued that these unobservables should favor the election of women and not penalize it. Second and more importantly, it is difficult to determine what drives the penalty that women suffer from. Is it that women do not want to replace a female incumbent or that individuals do not want to elect a woman after a failing female incumbent? In other words, is the penalty driven by a demand or a supply effect? With the data at hands, it is unfortunately difficult to answer this question and future research could thus attempt to answer it.

The main result of the third chapter suggests that women suffer from the negative actions of past women while men do not. Studying the validity of this mechanism outside

the political sphere could be an interesting avenue for future research. For instance, on the labor market, as compared to men, it could be more difficult for women to replace an underperforming woman. If this is true, this mechanism could constitute a channel favoring men and restraining women's ascent to positions of power.

From a public policy perspective, the three chapters of this thesis have direct implications. The main findings are that institutions can transform gender norms and that making them more gender inclusive could lead to a society that is more gender-egalitarian. Therefore, to reduce the pervasive gender inequalities that persist in all societies, these findings suggest that more can be done. As gender quotas have been introduced in politics in more than 100 countries, these findings also suggest that more will certainly be done. Will it be enough to eradicate all types of gender inequalities remains an open question.