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// OLGA KUZMINA AND VALENTINA MELENTYEVA

Gender Diversity in Corporate Boards: Evidence From Quota-Implied Discontinuities

Gender diversity in corporate boards: Evidence from quota-implied discontinuities *

Olga Kuzmina
New Economic School and CEPR

Valentina Melentyeva
ZEW and University of Mannheim

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Abstract

Using data across European corporate boards, we investigate the effects of quota-induced female representation on firm value and operations, under minimal identification assumptions. We consider sharp increases in the share of women on boards that arise due to rounding whenever percentage-based regulation applies to a small group of people. We find that having more women on corporate boards has large positive effects on Tobin's Q and buy-and-hold returns. This result is in stark contrast with previous empirical work that finds large negative effects. The reason for this discrepancy is that these papers considered firms with different pre-quota shares of women to be good counterfactuals to each other. In our data, we see that such firms had grown differently already before the regulation. Thus, assuming they are good comparables would result in a negatively biased estimate of the effect. Instead, we use quasi-random assignment induced by rounding and find that promoting gender equality is aligned with shareholder interests. This positive effect is not explained by increased risk-taking or changes in board composition, but rather by scaling down inefficient operations and empire-"demolishing".

Keywords: Gender diversity, women on boards, gender quota, performance

JEL codes: J16, G34, G38, D22

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1 Introduction

Gender equality, and its main business-world facet – increasing female representation in boards of directors, – has become the agenda of policy makers across the world. "The Big Three" institutional investors (BlackRock, State Street, and Vanguard) also recently launched campaigns to promote gender diversity on corporate boards (Gormley, Gupta, Matsa, Mortal, and Yang, 2020). However, the seminal papers by Ahern and Dittmar (2012) and Matsa and Miller (2013) demonstrate that the effects of gender quota on firm value and performance are large and negative. These results raise a major policy dilemma: should gender equality be imposed at the expense of shareholders? In this paper, we use a novel discontinuity-based identification strategy to show that in the European countries that introduced percentage-based regulation, promoting gender equality is aligned with shareholder interests. This means that this policy dilemma does not exist, and institutional investors' money is smart. We further demonstrate that these positive effects mainly come from less empire-building activity on the part of women directors.

Estimating the causal impact of gender diversity on firm value is challenging. Early studies (Carter, Simkins, and Simpson, 2003) show a positive correlation between female directors and firm value, but Adams and Ferreira (2009) and Adams, Hermalin, and Weisbach (2010) identify several sources of endogeneity in the context of corporate boards. In a search for causal estimates, previous empirical work has largely relied on either the assumption that private firms are similar on unobservables to public firms (such as in Matsa and Miller, 2013), or that firms with different pre-quota shares of women are otherwise comparable to each other (such as in Ahern and Dittmar, 2012, and including the most recent work by Bertrand, Black, Jensen, and Lleras-Muney, 2019, Hwang, Shivdasani, and Simintzi, 2019, and Eckbo, Nygaard, and Thorburn, forthcoming, among others).[1] But is the cure always better than the disease? Ferreira (2014) argues that such identification assumptions may be problematic. We show empirically that indeed firms that had more female directors at the time the regulation was announced had been growing faster prior to the regulation than firms with fewer women, in our sample of European public firms. We demonstrate that this implies that using past female share as part of the identifying variation would produce an overly negative estimate of the effect of female directors on firm value. To overcome this bias, we offer a new identification strategy that is inspired by Angrist and Lavy's (1999) Maimonides' rule and exploits discontinuities that naturally arise due to rounding whenever a percentage minimum is

[1] While the first two papers use pre-quota share directly as the instrument, the latter two calculate the difference between the fraction of female directors required by the quota and that of the current board, and that also mechanially depends on how many female directors the firm already has.

applied to a small-sized group of people.

Specifically, we note that any percentage quota applied to a relatively small-sized group of individuals produces sharp increases in the actual minimum share of women that is to be achieved. This happens because women come in whole numbers. For example, with a quota of 25%, a board of size 4 has to have at least one woman, making it exactly a 25% as the minimum to be achieved. However, a board with 5 members has to have at least two women, i.e. 40%. Such a sizeable difference from what the quota prescribes means that firms with 5 board members will respond disproportionately more than firms with 4 members to the same percentage regulation, purely due to rounding, as long as board size is not perfectly flexible.

We consider board sizes measured before the exact percentage to be achieved is announced, hence it is not known to the firm ex ante which board sizes will fall around this discontinuity. For example, for a 25% quota the close board sizes are 4 and 5, while for a 40% quota they are 5 and 6, and for a 33% quota – 6 and 7, etc. This ensures that the ex ante sorting of firms into the board sizes relevant for the particular percentage announced in a given country is likely to be random. Public firms within such narrowly-defined boards sizes form our close comparables. We further strengthen the argument by making the comparisons within a difference-in-difference framework, so as to account for any time-invariant differences between boards of slightly different sizes. Finally, we generalize this setting to multiple discontinuities within a country (i.e. also comparing 8- to 9-member and 12- to 13-member boards, in case of a 25% quota), to different percentages across countries (i.e. comparing 5- to 6-member boards, and 7- to 8-member boards, in countries where a 40% quota is introduced), and to different countries that introduced the same percentage in different years (i.e. comparing a 40% by 2015 mandate in Spain to a 40% by 2017 mandate in France). In our preferred and most saturated specifications, we can even identify the effects out of relative intensities, such as comparing the difference in performance between firms with 5 and 4 members within the same industry (which are predicted to have a 15% difference in minimum female share) to the difference in performance between firms with 9 and 8 members within the same industry (which are predicted to have only a 8.3% difference in minimum female share). The results are robust.

One limitation of our approach is that the ability to provide causal estimates under minimal possible identification assumptions comes at a cost of sample size requirements. Despite that, in our sample that covers more than 60% of all BoardEx-Eikon public firms in the countries considered, we are able to demonstrate that the main effects are similar for all countries together, as well as for many individual countries separately (such as the UK, France, the Netherlands, etc.).

We find a large and positive effect of the share of women in boards on Tobin's Q across European countries: a 10pp increase in the share of women increases Tobin's Q by 2.1 units, which is about 1.2 within-firm standard deviations of this variable. This positive effect is also present when we look at individual countries where our instrument provides enough variation to have a significant first stage. Additionally, the ultimate measure of investor performance – the buy-and-hold returns – turns out to be about 1.6-3.8% higher in the annually compounded equivalent for firms that end up with a higher female share due to rounding. We show that these higher returns are not explained by higher loadings on common risk factors. Interestingly, investors do not seem to anticipate these positive long-run effects: when we adapt our methodology to an event-study framework, the immediate abnormal returns for firms with different minimum shares of women are not statistically different from each other.

To explore the mechanisms behind these effects, we first look at board composition. We find that average board quality, as measured by average age, experience, network size, qualifications, and independence does not change. However, we do see a slight increase in average board meetings attendance, consistent with previous literature (Adams and Ferreira, 2009). While board meetings attendance contributes to the overall effect on value, its magnitude is too small to explain it fully. We proceed by decomposing the overall effect on Tobin's Q into the market-value and book-value components, and observe that the former primarily drives the effect. As our further analysis shows, this is not an artefact of a change in capital structure, or an increase in dividend payout, but rather the result of less empire-building activity. Specifically, we show that all size-related variables (assets, sales, employment) are disproportionately lower for firms with more female directors. Interestingly, we observe that firms with more women are less likely to incur merger-related expenses and are to invest in purchases of fixed assets. In the context of overcoming the agency costs that lead to empire-building, these findings are very much in the spirit of women being tougher monitors as in Adams and Ferreira (2009). The dramatic drop in sales, however, drives some short-run decreases in ROA. Nevertheless, they are not accompanied by any decrease in profit margins or labor productivity and wages, implying that firms still make the same profit out of each unit sold and worker quality does not deteriorate. Taken together and recalling that the long-run market reaction is positive, the evidence suggests that firms with more women are scaling down the inefficient operations in the process of empire-"demolishing".

Our main contribution is twofold. Methodologically, we offer an empirical approach that enables studying the effects of any universal percentage-based regulation, under the minimal possible identification assumptions. Our identification strategy has its highest power when applied to relatively small-team settings, and as such it can be used in many settings outside of corporate finance and the context of gender. In political economy settings, for example, one could consider regulation applying to members of the Cabinet or members of the European parliament (but not e.g. members of Congress, which are too numerous to provide any meaningful discontinuities). It is also adaptable to other empirical setups, whenever close counterfactuals are of interest (such as in event studies).

Substantively, we apply this strategy to revisit the main empirical results from the literature on gender diversity in corporate boards, and show that the "common wisdom" of large negative value effects of female directors reverses. Our paper contributes to the literature studying the quota-imposed effects of gender diversity on firm performance. Besides Ahern and Dittmar (2012) and Matsa and Miller (2013), the recent evidence on Norway is more mixed: Eckbo et al. (forthcoming) find zero effects, while Nygaard (2011) finds heterogeneous effects depending on information asymmetry. For Italy, Ferrari, Ferraro, Profeta, and Pronzato (2016) find no differences in performance, but some positive effects on stock prices. The first US-based studies by Hwang et al. (2019) and Greene, Intintoli, and Kahle (2020) find negative market reaction to the introduction of gender quota in a sample of Californian firms. Besides internal validity, our paper speaks towards external validity: rather than looking at one specific country or state, we consider virtually all European countries that introduced percentage quotas for public firms, both mandatory and voluntary. Finally, we propose a novel mechanism of female directors affecting firm value through empire-"demolishing".

More broadly, our paper is part of a larger literature on corporate board structure, such as board diversity (see a survey by Ferreira, 2010), board representation (Jäger, Schoefer, and Heining, forthcoming), gender differences across directors (Adams and Funk, 2012), gender spillovers (Matsa and Miller, 2011), and gender and team performance in general (see some experimental evidence in Hoogendoorn, Oosterbeek, and van Praag, 2013). Our paper's final contribution to this broader literature is to highlight another implication of our empirical strategy. As we show, the mere existence of a significant first stage implies a specific average way of adjusting to the regulation: we show that firms mostly comply by switching male directors for female directors, rather than altering board size to make it easier to comply. Such a "sticky" board size implies that the costs of switching directors are perceived to be lower than those of altering board size, for an average firm. While board size and its determinants have been extensively explored (see e.g. Coles, Naveen, and Naveen, 2008, and Lehn, Patro, and Zhao, 2009), to the best of our knowledge sticky board size is also a novel finding. It is important because it implies that such percentage-based regulations may have additional unintended effects by making firms shift away from their optimal board size.

The paper is organized as follows: Section 2 describes the empirical strategy; Section 3 discusses the data and provides summary statistics; Section 4 shows the first-stage results and validates the instrument; Section 5 presents the main results on the effects of female representation on value; Section 6 explores the mechanisms behind the effects; Section 7 concludes.

2 Empirical Strategy

To illustrate the idea behind our instrument, let's suppose that a firm faces a quota of 25% of women on the board (the specific number is used for illustration purposes). Does it mean that every firm that is *compliant* with this quota will have to have at least 25% women? Well, it turns out that most firms will actually have to have a percentage much higher than 25%, even if they want to only marginally comply with the 25%-quota. And the simple reason for that is that women (and men too) come in whole numbers. So a board of 2 directors will have to have at least 1 woman, making it a 50% share of women overall, while a board of 5 members will have to have at least 2 women, making it a 40% share of women. Only a board that is an exact multiple of 4 will have to have exactly 25% as the minimum to comply with the quota. Given how reluctant firms may have been in becoming compliant, even the differences in these minimum requirements induced by the same quota will likely produce enough powerful variation for us to identify the effects of interest. Overall, a firm with board size b, facing a quota q would need to have a minimum of

$$\frac{int((b-1)\cdot q)+1}{b},$$

where $int(a)$ is the integer part of a real number a, making this minimum a sawtooth-like function of the board size, such as the one in Figure 1 (drawn for the 25% case for concreteness).[2]

This pattern produces some natural discontinuities in the minimum required share of women, which is what we use in conjunction with our instrument. It is essential that we never use the contemporaneous board size when constructing the instrument (since it is likely to be endogenous), but rather the original board size that existed before the quota and its exact percentage were announced. Additionally, since firms of very different board sizes are likely to be fundamentally different, we want to isolate the closest possible comparisons. To do that we investigate only

[2] This can be equally spelled as $\frac{roundup(bq)}{b}$, where *roundup* is the upward-rounding function.

the upward parts of this sawtooth-like pattern (this is analogous to Angrist and Lavy's, 1999, "discontinuity sample", highlighted in red in Figure 1). While any of the neighboring board sizes would be close enough comparables in terms of minimizing omitted variable bias concerns, the treatment is highest precisely at these jumps. Thus, using the close board sizes at these jumps, essentially leads to the highest signal-to-noise ratio. It additionally allows us not to rely on any additional functional form assumptions and extrapolation on how female presence and our variables of interest depend on the board size itself.

Our simplest possible instrument in this framework, $Right_i$, is then the dummy that takes a value of 1 for firms that were located just to the right of the kink in the discontinuity sample (i.e. 5, 9, 13, etc. in the case of 25% quota), in the year before the quota was announced, and a value of 0 for firms located just to the left of this kink (i.e. 4, 8, 12, etc. in the case of a 25% quota), and missing for all other values.[3]

To give an example of the identifying variation, let's consider for simplicity just one country, e.g. the United Kingdom (which is where most of our observations will come from anyway), which in 2011 published a recommendation by Lord Davies (2011) to incentivize larger firms to have at least 25% of women on boards by 2015. Our discontinuity sample in the UK will thus consist of firms that in 2010 (a year before the announcement) had 4, 5, 8, 9, 12, 13, etc. board members (highlighted in red in Figure 1). We will be making all of our comparisons within each of these red pairs, and to do that, we use the kink-specific fixed effects, λ_{kc}, that capture separate intercepts for firms that have 4 and 5 board members, vs firms that have 8 or 9 board members, vs firms that have 12 or 13 board members, etc. Due to these fixed effects, we compare firms only within each kink, but not across. Hence, none of our results can be explained by potential differences across firms with larger vs smaller boards (see e.g. Yermack, 1996). It is also important to note that firms naturally sort into these original board sizes before the actual percentage of the quota gets revealed, which even further reduces any concerns for selection of firms into specific board sizes (e.g. multiples of 4 vs one more member). Our main argument will thus be that this pre-existing sorting of firms within a kink, e.g. into whether to have 8 or 9 board members (and conditional on other things that we control for later), is likely to be close to random. This will be further weakened in

[3] This most intuitive instrument has a much less intuitive mathematical formula that we only provide here for completeness:

$$Right_i = \begin{Bmatrix} 0 \text{ if } \frac{int((b_{i0}-1)\cdot q_c)+1}{b_{i0}} < \frac{int((b_{i0})\cdot q_c)+1}{b_{i0}+1} \\ 1 \text{ if } \frac{int((b_{i0}-2)\cdot q_c)+1}{b_{i0}-1} < \frac{int((b_{i0}-1)\cdot q_c)+1}{b_{i0}} \end{Bmatrix},$$

where b_{i0} is the board size of firm i in the year before the quota was announced (this year is country-specific), and q_c is the country-specific quota.

the more saturated specifications.

One might argue that firms to the right of the kink mechanically have one more board member within each bin (as 5>4 and 9>8), and this might have its own effect on the dependent variables even in the absence of any quota (hence violating the exclusion restriction), we weaken the identification requirements further and move to a difference-in-differences setup, finally estimating the first stage specification as follows:

$$Share_{it} = \gamma Post_{ct} Right_i + \lambda_{kct} + \lambda_{sct} + \lambda_i + \omega_{it}, \quad (1)$$

where $Share_{it}$ is the proportion of women on the board of firm i in year t, $Post_{ct}$ is the country-specific dummy variable that takes the value of 1 for the years after compliance, and 0 for the years before announcement, $Right_i$ is the instrument as defined above, λ_{kct} are the kink-specific fixed effects (described above and kept country- and also year-specific, so as to absorb any country-year variation as well), λ_{sct} are the industry-year fixed effects (also specific to the country)[4], λ_i are firm fixed effects, and ω_{it} is the error term.

This move to difference-in-differences helps to address potential pre-existing differences in the value of the company or other dependent variables for boards of different sizes. Additionally, it allows to absorb any non-linear relationship between $Share$ and board size that may exist even in the absence of a quota, under the assumption that the form of this non-linear relationship is similar before and after the reform. It is worth noting that our setup is different from the usual use of DID in the quota setup (as in Ahern and Dittmar, 2012, and Matsa and Miller, 2013) in at least two important dimensions. First, the way how we construct counterfactual firms is different: we consider firms with very close ex ante board sizes, rather than firms with different ex ante shares of women or public and private companies, as these other papers. And second, because we have a meaningful and observable first stage, we don't have to guess when the shock happens.[5] In our setup we can first explore the dynamics of the first stage and observe when firms start responding to the instrument, and then consider the second stage only where the instrument provides a powerful enough variation. As we will see further, the cross-sectional differences in $Share$ start kicking in significantly after compliance years, so we set $Post$ to be 1 during the years post-compliance, and

[4] These are not necessary for identification and do not affect first-stage results. We add them in all first-stage specifications since they will be included as controls in the second stage for our dependent variables (Tobin's Q and others).

[5] This is perhaps one reason why different authors have disagreed on the timing of the Norwegian shock. While the share of women steadily rose from 2001 to 2009, papers have employed 2002 as the cutoff year (Eckbo et al, forthcoming), 2003 (Ahern and Dittmar, 2012), 2004 (Bertrand et al, 2019), 2005 (Nygaard, 2011), and 2006 (Matsa and Miller, 2013).

to 0 – during the years before announcement. The middle years are not used in the main part of the analysis, since empirically the firms do not respond to the instrument during these years.[6]

The implicit assumption in the first-stage equation (1) above is that the effect on $Share$ of being to the right of the kink is the same at different kinks. This is fine, as long as we believe that that the effect mostly comes from having one more woman (rather than the percentage share itself), and it is constant across kinks. But if not, then we want to identify from the relative sizes of these jumps as well. We therefore proceed to defining our second instrument, which was already graphed in Figure 1 for the UK, in the following way:[7]

$$MinShare_i = \frac{int((b_{i0} - 1) \cdot q_c) + 1}{b_{i0}},$$

This allows us to proceed to our fullest specifications, where we estimate the first stage of our main equations of interest as follows:

$$Share_{it} = \gamma Post_{ct} MinShare_i + \lambda_{kct} + \lambda_{sct} + \lambda_i + \nu_{it}. \tag{2}$$

In essence, we want to exploit the fact that the minimum share $MinShare_i$ is disproportionately larger between firms with 5 and 4 board members (40%-25%=15% difference) compared to that between firms with 9 and 8 board members (33.3%-25%=8.3% difference), and as such $Share_{it}$ is also expected to rise more on average in the former case than in the latter. This presents a very tight identification, under the minimal assumptions that are ever possible in the setting of a universal percentage quota.

As we will be measuring the effects over time, we cluster errors at the firm level to account for arbitrary autocorrelation within firms and heteroskedasticity. However, it is important to emphasize again that the identifying variation is mostly cross-sectional, which means that we do not have to explicitly rely on timings associated with the quota (speed of compliance, when to define pre vs post, etc.), which some authors (e.g. Ferreira, 2014, and Eckbo et al., forthcoming) have argued might present a problem in terms of coincidences with various macro events and the associated differential impact of these events across firms with different shares of women. This reinforces the importance of bringing the comparison firms as close to each other as possible and then let the data

[6] In reality this speed of compliance may also be specific to the country, but we choose to be as agnostic as possible. Our argument on "shopping" for the first stage mirrors the optimal selection of instruments, as long as identification assumptions are maintained (see e.g. Paravisini et al, 2014).

[7] Our instrument is different from the *Shortfall* instrument, introduced by Eckbo et al (forthcoming) in that *Shortfall* uses the ex ante share of women on the board as part of its construction. We discuss why instruments based on ex ante shares of women are likely to produce overly negative effects on value in Section 4.2.3.

tell us when the change happens. This is precisely what we do and estimate (1) and (2) on the data 3 years before the quota announcement ($Post_{ct} = 0$) and 3 years after the quota compliance year ($Post_{ct} = 1$), skipping the intermediary years altogether.

A natural question is why being on the right of the kink *before* the quota can at all predict the share of females after the quota when firms have so many different ways of adjusting board composition to satisfy the quota? For example, those 9s that really don't want to have 33.3%, can just reduce the board size to 8 to attain the required minimum of 25%. If all firms to the right adjust like that, then γ will be close to zero. Furthermore, if firms instead adjust only by adding new female members until quota is satisfied, then those on the right may even end up with a *lower* share than those on the left (e.g. among firms with zero females that adjust by adding new members only, 4s will need 2 extra women for an average of $2/6 = 33.3\%$ and 5's will also need 2 extra women for an average of $2/7 = 28.6\%$, so that the difference-in-difference coefficient is -4.76%). Only if firms exchange males for females at least to some extent would γ be positive (as it is in the extreme: 40% for 5-member boards and 25% for 4-board members, DID is +15%). However, how firms really adjust is ultimately an empirical question, and ex ante our identification strategy does not assume anything about their behavior.

This means three important things for us. First, from a purely econometric side, all other ways of adjusting, except exchanges, will bring γ closer to zero (or even negative), reducing the power of the first stage, and making it harder for us to track any changes in $Share_{it}$ at all (and later in the dependent variables of interest). Second, if we do find a positive and significant γ (which we do), this means that the predominant way of adjusting to the quota is actually exchanging males for females. This is an important empirical observation about board size being so sticky that all other ways of adjusting are more costly, at least on the margin. And third, different countries may have different ways of adjusting due to a variety of institutional and cultural reasons, suggesting that if we were to consider individual countries one-by-one, we may find a different γ across countries. To sum up, our identification strategy does not assume that all firms adjust by exchanging males for females, but empirically explores whether this is on average true or not, and then uses this empirical fact to track changes in the variables of interest.

3 Data and Summary Statistics

The results of our paper are based on two sets of data. We use an unbalanced panel of listed companies across European countries from BoardEx to obtain the director-level information on gender, age, number of qualifications, network size, role (independent or not), and other characteristics, and average them at the firm level. We then merge this dataset with financial data on public companies from Eikon. The exact set of countries is comprised of the United Kingdom, France, Italy, Belgium, Spain, the Netherlands and Norway. These are the countries that introduced formal (through quotas) or informal (through advisory recommendations) regulations on gender diversity that satisfy the following criteria: 1) this regulation contains a specific minimum percentage that has to be achieved (otherwise, we would not be able to exploit our discontinuity-based instrument); 2) it applies to a vast majority or even all public firms (rather than some narrowly-defined group, such as only state companies, – otherwise the power of the first stage will be low in case we don't measure firms subjected to regulation very precisely); 3) it has a compliance date of no later than 2017 (otherwise we will not have enough observations to measure the outcomes); and 4) there are at least 20 firms in the discontinuity sample (otherwise, our multiple-fixed-effects specifications would not be estimated; however, this is not a hard constraint, as it rules out only Iceland with its 3 firms in the discontinuity sample).[8] The period of study varies depending on the country and the respective year when the regulation was introduced and covers all years from 3 years before quota/regulation first announcement to 3 years after and including the compliance year (or to 2019, whichever is earlier). The complete coverage of countries with a short description of regulation and the relevant years is presented in Table 1.

As expected due to BoardEx coverage, most of our sample (slightly less than 60%), comes from the United Kingdom. We will therefore present all the analysis both for the UK alone, as well as for all countries together. The counts in Table 1 show the number of firms in the discontinuity sample as of the year before the regulation announcement. For example, there were 445 public firms in the UK in 2010 that had board sizes that are either exact multiples of 4, or had one more board member. The second largest country is France with 144 firms in 2009 in the relevant discontinuity sample (which for a quota of 40% covers many more board sizes). On the other hand, there were only 20 firms in the relevant discontinuity sample in Norway and 29 in the Netherlands. Since we

[8] We do, hovewer, have to exclude Germany, because listed companies above 2,000 employees (precisely the ones subject to the 30% gender quota after 2016) have to have either 12, or 16, or 20 supervisory board seats, depending on the number of employees (Co-determination Act, 1976). As such, there are no comparable firms within any discontinuity bin.

also perform our analysis for the UK alone, and our results are very similar, they are not driven by any of these countries having very small sets of firms.

In Figure 2 we further show the exact distribution of pre-announcement-year board sizes, by country, with red (dark) bars representing the discontinuity sample, and the grey (light) bars representing all other boards sizes not used in the analysis, across BoardEx-Eikon listed firms. We also do not consider (almost mechanically) very small boards of fewer than 4 members in the year before the quota announcement. Since French quota applies to non-executive members, the relevant discontinuity samples are based on the ex ante number of non-executive directors, rather than total board size. Depending on the exact quota percentage, which defines the board sizes to be included in the discontinuity sample, and the distribution of firms across board sizes, our sample covers from 54% of these firms in the UK, to 71% in France, and above 70% in most other countries, for a total of about 60% of all BoardEx-Eikon public firms in these countries. In the unreported results, we also show that variable distributions in the pre-announcement year are similar in the discontinuity sample and out of it, within each country, which is expected given the way it is constructed. This also speaks to the generalizability of our results to boards of different sizes. In what follows, we refer to the discontinuity sample as the sample.

Table 2 presents summary statistics for the main variables of interest, with all continuous variables winsorized at 1% tails. For comparability reasons, we present the statistics for the post-compliance period only. Companies in our sample have on average 23 bn Euro total assets (0.6 bn in the log form) and an average market capitalization of 4.5 bn Euro (0.4 bn in the log form). The average board size is equal to 8, both before the announcement of the regulation and also after, suggesting that on average firms do not reduce the number of board seats in order to avoid hiring an extra women and that board sizes are generally sticky. Firm boards have about 21% females post-compliance, compared to about 6% before the announcement. The former is somewhat smaller than any of the quotas considered, since not all regulation is mandatory, and not for all firms in the sample. The main instrument (predicted minimum share of women, $MinShare_i$) averages to 36% and summarizes the average quota-implied share of women in the discontinuity sample. As expected, about half of the firms are located to the right of the kink.

Following prior research on firm value and governance, we compute Tobin's Q as our main measure of firm value (Yermack, 1996; Adams and Ferreira, 2009; Ahern and Dittmar, 2012). It is defined as the sum of total assets and market equity less common book equity divided by total assets, and averages to 1.9 in the post-compliance period. About 19% of firms' capital comes from debt (as normalized to assets), and 74% of firms pay dividends. Average return on assets is slightly negative and amounts to -1%. On average companies' revenue is 0.3 bn per year, and they hire about 1700 workers, for an average labor productivity (revenue per worker) of 270 thousand Euro per worker, with an average wage of about 30 thousand Euro per worker (all values computed based on the log-form averages). There are slightly fewer observations available for other indicators. For the UK firms we also compute loadings on the 4 risk factors, as provided by Gregory, Tharyan, and Christidis (2013), as well as buy-and-hold returns.

Finally, the average age of a director in the sample is 58 years, s/he has on average 1.7 qualifications, a network size of about a thousand people, and has served in the company for around 8 years; about half of directors are independent.

4 First-Stage Results

4.1 The effect of the instrument on the actual share of women

The first empirical test of interest is the one that shows that the instruments (being to the right of the discontinuity, $Right_i$, or the predicted minimum share of women, $MinShare_i$) have a significant and direct impact on the actual share of women, $Share_{it}$. This is a necessary condition for further exploration of the effects of women on corporate outcomes in the IV framework. As discussed above, if firms on average adjust in a different way than substituting women for men, the first-stage coefficient would be close to zero (or even negative). This ultimately becomes an empirical question, which we now explore. To summarize, we find that the instrument does predict differences in female shares, and with a positive sign, implying that on the margin, firms adjust as prescribed by the instrument. In particular, they do not on average choose to change their board sizes to comply with the quota exactly, suggesting that the costs of adjusting board sizes are large enough.

We estimate (1) and (2) and report the results in Table 3. For illustrative purpose, in columns 1 to 3 we also consider post-announcement vs pre-announcement periods (when dummy $Post_{ct}$ takes the value of 1 for the years after the announcement, and 0 for the years before the announcement), while columns 4 to 6 are estimated on our main post-compliance vs pre-announcement period. Panel A shows the results for the United Kingdom, which constitutes the majority of our observations, and Panel B tracks all countries together.[9] Column 1 uses the simplest possible setup and estimates

[9]In all specifications throughout the paper, we drop a few firms that already had a higher share of women than the quota, before the quota was announced. Dropping these few unaffected firms naturally increases the power of the first stage. The results are, however, similar if these few firms are kept, and are available upon request.

(1) using the data from the first kink only (i.e. boards of 4 and 5 in the UK). The largest jump in the minimum share occurs at this kink, so the effect on $Share_{it}$ is expected to be the largest. The coefficient of 0.02 implies that there is a 2 pp difference in the share of women on average in the years post-announcement, compared to pre-announcement, between firms that used to have 4 and 5 board members before announcement. We see that while this effect is significant at the 5% level, the instrument is not exceptionally strong (with an F-statistic of 4.5), suggesting that compliance doesn't fully pick up in the first years after the announcement. We therefore move to the post-compliance period, where all firms have had enough time to follow the regulation. As we see in column 4, the similar difference is already 0.065 and significant at the 1% level. If all firms complied exactly with this voluntary regulation in all years and did not change their board sizes, then this magnitude would be 0.15 (the difference between 40% and 25%). However, as noticed before, none of this is assumed in the identification, and it is only important that this instrument does provide a significant explanatory power. The economic magnitude of this coefficient suggests that firms do comply to a large extent even with the voluntary quota in the UK (and among firms in the FTSE100 compliance rates are the highest at more than 60%). Column 5 repeats the same exercise for firms in all kinks and the magnitude of the coefficient expectedly drops, since the jumps become smaller and smaller, while the instrument is still significant at the 1% level.

In column 6, we turn to using intensities as in (2), where we can fully account for the relative size of the kinks, and this is where the most interesting observation on the economic magnitude comes from. One can think of it as a weighted average of how well firms comply with the instrument. If everybody would satisfy just the minimum required share, as prescribed by the instrument, then the coefficient would have been exactly 1. However, arguably, some firms would prefer to change the board size in the opposite direction (driving the magnitude closer to zero, as discussed above), some would not comply because they are not required to (again, making the coefficient closer to zero), and some may react more strongly and hire a higher percentage than the minimum predicted by the instrument (increasing the magnitude). As such, the obtained coefficients represent a weighted average of all these types of behavior.

In Panel B we consider all firms in our sample together (appropriately accounting for all fixed effects that are now country-specific, as explained in Section 2). As the dynamics of compliance (including the time between announcement and compliance) and the stickiness of boards are likely to be very different across countries, the economic magnitudes may naturally change, but they don't, and the coefficients remain very significant, all at least at the 1% level. The first-stage F-statistic becomes larger than in Panel A in all post-compliance specifications. It is notable that after accounting for all firm heterogeneity and industry-year fixed effects, $MinShare_i$ can still predict the actual share of women quite precisely even across all countries. This is notable because higher quota percentages in other countries also imply that the kinks are located much closer to each other (e.g. 5 vs 6, and 7 vs 8 in case of a 40% quota), and as such there is much less variation left when these firms are compared to each other within such narrowly-defined kinks. Still, we see that our instrument predicts the share of women very well.

4.2 Validating the instrument

4.2.1 Pre-existing differences and dynamics

We need to make sure that our instrument is not picking up some pre-existing trends across firms that may relate to future shares of women and future outcomes. We start by exploring visually the dynamics of the first stage in Figures 3 and 4. We plot coefficients from a regression similar to (1) and (2), where instead of $Post_{ct}$ we use the full set of dummy variables for years D_j(the year before announcement, D_0, is excluded to avoid perfect multicollinearity and all coefficient magnitudes are measured relative to this year). Specifically, we estimate:

$$Share_{it} = \gamma_{-4}D_{-4}Right_i + ... + \gamma_{-1}D_{-1}Right_i + \gamma_1 D_1 Right_i + ... + \gamma_8 D_8 Right_i + \lambda_{kt} + \lambda_{st} + \lambda_i + \omega_{it}, \quad (3)$$

$$\begin{aligned} Share_{it} \quad = \quad & \gamma_{-4}D_{-4}MinShare_i + ... + \gamma_{-1}D_{-1}MinShare_i + \\ & +\gamma_1 D_1 MinShare_i + ... + \gamma_8 D_8 MinShare_i + \\ & +\lambda_{kt} + \lambda_{st} + \lambda_i + \nu_{it} \end{aligned} \quad (4)$$

and plot the estimates of γ_j with their 95% confidence intervals over time. Since the period between announcement and compliance years varies significantly by country, for illustrative purposes we plot the dynamics for the UK only and highlight 2011 and 2015 on the graph as the announcement and compliance years, respectively. As we observe in Figure 3, the relative difference between firms with closely-held board sizes, γ_j, is statistically zero before announcement, not just in levels (which is interesting), but also in trends (which is important in the DID setting). This difference also rises steadily starting with the announcement and gets significantly pronounced after the compliance year, validating our primary focus on the post-compliance period. The dynamics are also similar for the $MinShare_i$ instrument depicted in Figure 4.

4.2.2 Pre-existing trends

We now formalize the placebo pre-trend regressions. Specifically, we consider the differenced form of (1) and (2), which allows us to use as many years prior to quota announcement for each country as are available in the data (capped at up to 10 years before announcement), and compare the average long-run trends between firms to the left and right of the kink, and firms with different values of the $MinShare_i$ instrument. We estimate:

$$\Delta Share_{it} = \gamma Right_i + \lambda_{kc} + \omega_{it}, \tag{5}$$

$$\Delta Share_{it} = \gamma MinShare_i + \lambda_{kc} + \nu_{it}. \tag{6}$$

The results are reported in Appendix Table 1 for all countries together in columns 1 to 3 and for the UK in columns 4 to 6. As expected, we see no significant effects in any of the specifications, suggesting that there are no pre-existing differences in trends between firms to the left and right of the kink.

We also replicate these regressions for our main second-stage dependent variable – Tobin's Q in Appendix Table 2, columns 1 to 3 for all countries together and 4 to 6 for the UK. Again, we see no significant differences in past trends between firms to the left and to the right of the kink. While this is reassuring and suggests that there is no apparent pre-selection of firms into boards of different sizes, this is also somewhat expected from the way the instrument and the discontinuity sample are constructed to start with.

4.2.3 Why instruments based on past female share cannot be applied

To finalize this section, we want to explore why instruments based on pre-announcement shares of women should not be applied in a difference-in-differences setting, at least in our sample. To do that we also consider past trends in Tobin's Q for firms with different pre-announcement shares of women. For illustrative purposes, we first divide firms into those which have at least one female in the year before announcement ($Woman_i = 1$, these are approximately 43% of all firms) and those which have no women before the quota is announced ($Woman_i = 0$, the remaining 57% of all firms).

As we see in Appendix Table 2, column 7 for all countries and column 9 for the UK, firms with at least one woman in the year before the announcement had statistically significantly grown *faster* in terms of Tobin's Q already before the quota was announced, relative to those that had no women. This is also true for the share of women as of pre-announcement year, $Share_i$ (columns 8 and 10). This means that a difference-in-differences setting that is based on assuming such firms would have had the same trends had the reform not happened, will likely find its identification assumptions not satisfied. Specifically, if such an instrument were used to evaluate the effects of the quota, and these differential trends continued to follow, then the reduced form of Tobin's Q on $Post_{ct} * Woman_i$ or $Post_{ct} * Share_i$ will produce an *upward-biased* coefficient.

The difference-in-differences coefficient of the first stage ($Share_{it}$ on $Post_{ct} * Woman_i$ or $Share_{it}$ on $Post_{ct} * Share_i$), on the other hand, is by construction negative with these instruments. This happens almost mechanically since to get to the same quota firms with more women need to increase their share by less than firms with fewer women. These two observations mean that the IV coefficient (the ratio of an upward-biased reduced form to a negative first stage) will be *downward biased*: more negative if the reduced form is positive, or less positive if the reduced form is negative. Therefore, using an instrument that is based on past shares of women will produce a biased and overly negative view of the effect of women directors on Tobin's Q.

5 The Effect of Gender Diversity on Firm Value

5.1 The effect of Gender Diversity on Tobin's Q

5.1.1 Average effect

We now employ our strategy to estimate the effects of gender diversity on firm performance and other variables. We start by considering Tobin's Q – the most common long-run measure of firm value – as the dependent variable and report reduced-form results (and IV-2SLS) in Table 4.

The reduced form corresponds to the following equations:

$$Y_{it} = \gamma Post_{ct} Right_i + \lambda_{kct} + \lambda_{sct} + \lambda_i + \nu_{it} \tag{7}$$

$$Y_{it} = \gamma Post_{ct} MinShare_i + \lambda_{kct} + \lambda_{sct} + \lambda_i + \nu_{it}, \tag{8}$$

and the IV-2SLS is given by:

$$Y_{it} = \beta Share_{it} + \lambda_{kct} + \lambda_{sct} + \lambda_i + \kappa_{it}, \tag{9}$$

where the instrument is either $Post_{ct}Right_i$ (columns 1 and 2 – for all countries, and 4 and 5 – for the UK) or $Post_{ct}MinShare_i$ (column 3 – for all countries, and 6 – for the UK), and all the variables and fixed effects are defined as above.

We include industry-country-year fixed effects in all specifications (based on Eikon 52 industry groups), to make sure the differences in Q are not accidentally driven by non-random composition of board sizes across different industries and shocks to them, as well as to explain more variation in Tobin's Q. The coefficient 1.011 in column 1 suggests that firms to the right of the discontinuity (at the first kink) have on average 1.011 units higher Tobin's Q than those to the left of the discontinuity, after quotas were introduced vs any potential difference before. In column 2 we replicate this analysis across all kinks and see similar results. In column 3, we move to the second instrument, $MinShare_i$, which is based on intensities, and again we see very significant reduced-form results.

Just below, we also report the corresponding IV-2SLS coefficients with their standard errors that give the magnitude of the effect, rather than just the sign, as well as robust weak-IV Anderson-Rubin confidence sets, which provide a more accurate p-values when F-statistics are not large (Andrews et al., 2019). While the reduced-form coefficients are obviously different in magnitude, once we rescale them into the actual magnitudes of interest – the IV-2SLS effects of women on firm value, – we see similar magnitude across all three instruments. This is remarkable, given that they are based on slightly different samples (largest kink vs all kinks) and slightly different identification assumptions, suggesting that this average positive effect of women on Tobin's Q is very robust. The preferred IV-2SLS estimate of 20.76 in column 3 means that for a one within-firm standard deviation in the share of women (which is about 10 pp in our data), Tobin's Q on average increases by 2 units (which is about 1.2 within-firm standard deviations of this variable). This suggests that women do have an economically large effect on Tobin's Q, across European listed firms. The Anderson-Rubin p-values further confirm that this effect is significant at the 5% level in all three specifications. In columns 4 to 6 we redo the analysis for the UK only, and the results are similar.

These large magnitudes are also sensible when discussed in the framework of heterogeneous treatment effects. If the effects are heterogeneous, then our IV estimates correspond to firms that "comply" with our instrument, i.e. those that have more women only because they end up to the right of the threshold and rounding is not in their favor. These are likely to be firms with the stickiest board sizes, i.e. firms for which the costs of having more women are smaller (or nonexistent) relative to the costs of changing the board size. It is thus expected that this type of firms may have quite high positive effects of women directors.[10] While it is inherently impossible to observe which firms are compliers to the instrument, we can calculate how many they are in our sample. To do that, in unreported results, we estimate (7) for a complier-with-the-quota dummy (i.e. having the share of women at or above the quota) directly.[11] We observe that the percent of instrument compliers varies from 13% across all countries and all kinks to 18% in the UK for firms at the first kink. This is similar to the percent of compliers that would be achieved if one used the $Post_{ct} * Woman_i$ instrument, discussed above, even in the full sample of all board sizes. This means that instruments based on pre-existing heterogeneity of female shares are likely to neither produce consistent estimates, as discussed in Section 4.2.3, nor apply to larger subsamples of firms.

5.1.2 Individual country-level analysis

We now further decompose the overall average effect from Table 4 to see if it is driven entirely by the UK or if there is evidence from other countries as well. The very basic obstacle to analyzing country-by-country is the individual-country-level first stage, as there are multiple reasons why it may have different strengths across countries, especially given a small number of listed companies in other countries. First, the regulation is different across countries in terms of how obligatory compliance is de jure and de facto, i.e. whether the sanctions for non-compliance are significant enough to alter firms' behavior in the institutional environment of a specific country. Second, the speed of compliance with the quota may be different, depending on how easy it is to change board members, and how big the lag is between announcement and actual compliance date, both affecting the timing of the first-stage effects. Finally, as discussed in Section 2, the exact way how firms adjust to the quota (by substituting women for men or hiring additional women until the quota is satisfied or a combination of the two) directly affects the value of the first-stage coefficient. Given these many obstacles, we can only explore the effect of interest in countries or subsamples of countries where the first stage proves significant enough.[12]

To increase the power of the first stage at the individual-country level we estimate our main equations of interest using post-compliance vs pre-announcement periods, for the first two kinks. This focuses on the jumps with the highest magnitude and hence has the most power to detect

[10] All previous IV-based work is of course also subject to such a LATE interpretation.

[11] This is a virtue of having binary variables: in the Angrist and Pischke (2009) notation, this is a regression of the endogeneous treatment indicator on the instrument, and it calculates the proportion of compliers to the instrument.

[12] One may be tempted to simply explore the effect of the instrument on Tobin's Q (the reduced form), even if that first stage is not powerful enough. However, this is not reasonable in our case, because even the sign of the first stage is unknown (as it depends on the way firms adjust to the quota). Hence there is no reason to expect any specific sign from the reduced form either.

enough variation in the female ratio. We report the results in Table 5, where Panels A and B correspond to the $Right_i$ and $MinShare_i$ instruments, respectively. The coefficients within each column refer to the results of separate regressions: the first stage in the first row, the reduced form in the second row, and below we also report the the implied 2SLS estimate as well as the Anderson-Rubin robust confidence sets with p-values. In column 1 we consider all countries together, and in column 2 – just the UK. We provide these for reference only to check that our main results from Tables 3 and 4 are not affected by this change towards a more powerful sample.

Now in column 3 we report the effect on all countries except the UK. As we see, both the first stage (0.683) and the reduced form (2.459) are significant at the 5% level and have the expected signs. However, the first-stage F-statistic of 6.86 is not high enough for 2SLS to provide any credible inference, and the magnitudes of IV-2SLS cannot be directly compared (with weak instruments 2SLS is biased towards OLS which is close to zero in our case). Therefore, we have to refer to Anderson-Rubin confidence sets and p-values that are robust to the presence of arbitrarily weak instruments (Andrews et al., 2019). As we see, the 95% confidence set does not include zero, meaning that the effect of female share on Tobin's Q is also positive and significant at the 5% level for all countries other than the UK. This effect is also robust to using the $Right_i$ instrument instead (Panel B). Overall, we see that the effect of female share on Tobin's Q is pronounced both in the UK and in all non-UK countries taken together.

Furthermore, there are three more individual countries for which the first stage coefficient turns out to be significant at least at the 5% level, and hence we can explore the effects for them: these are France, the Netherlands, and Norway (columns 4 to 6, respectively).[13] As we see, all three show positive effects in the reduced form, with France and Norway being significant at the 5% level, while for the Netherlands the coefficient has a 10.6% p-value. A similar pattern is observed once we consider AR confidence sets. Both France and Norway show significant positive effects of female directors on Tobin's Q, while the effect is positive but only marginally significant for the Netherlands (p-value of 10.6%). For the Netherlands and Norway, which have a relatively high F-statistic, we can look directly at the implied IV-2SLS estimates, and it is interesting to see Norway having a very similar magnitude to the one in the UK. For the Netherlands the magnitude is much lower, but it is clearly not negative (rejected at the 10.6/2=5.3% significance level). Finally, in column 7 we report the results for all other countries together, for completeness only: the first-stage F-statistic of 0.01 prevents any separate inference for these countries, even though it is reassuring

[13] For each other country considered individually, the first stage is not significant even at 5%. Hence the second stage cannot be identified, as the AR confidence sets would automatically include plus and minus infinity.

that the reduced-form coefficient of 0.0464 is also positive. Panel B replicates the results using the $MinShare_i$ instrument, which may captures relative intensities better, and the results are robust.[14]

To sum up, we find no evidence of women affecting firms' value negatively. If any, the evidence for both the UK and all other non-UK countries demonstrates a significant positive effect on Tobin's Q, and the results for individual countries where the effect can be explored also show a positive effect.

5.2 Is this really about firm value?

While there is a strong and robust positive effect of female directors on Tobin's Q, this measure has been somewhat criticized for not being the best one (see e.g. Dybvig and Warachka, 2015). The benefit of our approach is that we can use the same shock to female directors to study any measure of performance, both using the annual data and adapting the methodology for an event-study. To consider the long-run value effects, we therefore look at the ultimate measure that investors would earn – buy-and-hold returns (Erkens et al., 2012) – in Section 5.2.1. Then we explore whether these effects were anticipated in advance – by using a short-run event study around the regulation announcement – in Section 5.2.2. Finally, we investigate if these effects are explained by a different risk-taking profile of firms with more and less women – in Section 5.2.3.[15]

5.2.1 Long-run buy-and-hold returns

Figures 5 and 6 plot the buy-and-hold returns that investors would earn by each date if they bought a portfolio of firms to the right (solid line) or to the left (dashed line) of the kink a year before the announcement of the regulation, for the first and all kinks, respectively. As we see, if investors bought and held firms to the right, they would have earned a much higher return over these years than had they bought and held firms to the left. Specifically, the difference amounts to about 3.8% in the average annual compounded return for firms at the first kink, and for 1.6% across all right and left firms.[16] This means that investors do indeed win in the long-run from having more women in the board.

[14] These cross-country results also demonstrate that our main results cannot be explained by different trends in odd-number-sized boards which may have a higher effectiveness of per se (see e.g. Deng et al, 2012), since whether an odd-numbered board ends on the right or left depends on the quota percentage (e.g. right for the UK and Netherlands, left for France and mixed for Norway).

[15] In this section we consider the UK only, which is where we were able to locate country-specific risk factor returns, that are needed for large parts of our analysis.

[16] The average annual compounded return for firms with 5 board members is 10.12% = 2.38^(1/9)-1 and for firms with 4 board members it is 6.33% = 1.74^(1/9). All firms on the right earned 10.31% = 2.42^(1/9), and all firms on the left earned 8.69% = 2.12^(1/9).

We further support this conclusion in Table 6 by regressing these buy-and-hold returns on our instrument, $Right_i$, for each year. To do that, we estimate the following equation:

$$BH_{it} = \gamma_{2011} D_{2011} Right_i + ... + \gamma_{2019} D_{2019} Right_i + \lambda_{kt} + \lambda_{st} + \lambda_i + \nu_{it}, \tag{10}$$

where BH_{it} is the buy-and-hold return of firm i in year t (i.e. the total return that an investor would earn if she held this stock till year t), $Right_i$ is defined as before, and D_j are the indicator variables for each particular year j after announcement. All returns are measured relative to the year before the announcement (February 24th, 2010) when all D_j are zero and buy-and-hold returns of all firms are mechanically set to 1. To make our results more comparable to the previous section, we also add kink-year fixed effects λ_{kt} (to make sure we compare only within closely-held board sizes and not across), firm fixed effects, λ_i, and industry-year fixed effects, λ_{st}, which make sure we compare firms within the same industry, and that our results are not explained by e.g. firms to the right accidentally being located in industries that experienced a boom during this period.

Panel A reports the results for firms with 4 and 5 board members, while Panel B considers all firms together. As we observe, the differences in buy-and-hold returns are not only economically large, but also statistically significant for most years for 4 vs 5 firms, and in later years for all firms, and the magnitudes are almost identical to what we have just seen in the graphs (without controls). Similar to Figure 6, where we did not use any controls, it is also interesting to see that these returns start kicking in relatively late, suggesting that the positive value effects of having more women on the board were not realized in the beginning. We now explore this conjecture a bit further by adapting our methodology to look directly at abnormal returns around the announcement using the event-study approach.

5.2.2 Did investors anticipate the positive effects of the quota?

So did investors anticipate the future effects of the gender quota? To answer this question, we estimate the following equation:

$$AR_{it} = \gamma_1 Day_1 Right_i + ... + \gamma_{10} Day_{10} Right_i + \lambda_{kt} + \lambda_{st} + \lambda_i + \nu_{it}, \tag{11}$$

where AR_{it} is the Carhart (1997) 4-factor abnormal return of firm i, now in *day* t (betas estimated using observations from the previous 252 trading days with a minimum of 100 days non-missing observations, as in Hwang et al., 2019, but using UK factor returns constructed by Gregory et al., 2013), $Right_i$ is defined as before, Day_j are the indicator variables for each particular day

j after announcement (where day 1 corresponds to the day of the announcement, February 24th, 2011), λ_{kt} are kink-day fixed effects (as before: to make sure we compare only within closely-held board sizes and not across), λ_{st} and λ_i are industry-day and firm fixed effects, respectively.[17] We cluster standard errors at the industry level to account for potential within-industry commonality in returns within each day, as well as to account for any time-series correlation within firms.

As before, this is a difference-in-difference specification (where days are subsumed by λ_{st} and $Right_i$ is subsumed by λ_i). In this specification γ_j measures how much the abnormal returns of firms to the right of the kink are higher than the abnormal returns of firms to the left of the kink (relative to any possible pre-existing difference before announcement) on each day j after the announcement.[18] The cumulative abnormal returns in this specification are then given by γ_1, $\gamma_1 + \gamma_2$, and so on till $\sum_{j=1}^{10}\gamma_j$ for days 1, 2, ... 10, respectively, and we plot these cumulative abnormal returns with their 95% confidence intervals in Figures 7 and 8 for the first kink and all kinks, respectively. The correspondent CAR coefficients for each day with their standard errors are reported in Appendix Table 3.

These cumulative abnormal returns indicate that there is no differential reaction to the regulation for firms to the right vs those to the left of the kink immediately upon announcement. Given that we test about 10 coefficients here, we expect to find one significant at the 10% level just by chance. This is what we see with the relatively smaller firms at the first kink, which have one significantly positive CAR of about 1.5% about a week afterwards. There are no significant CARs when we look at all kinks (where larger firms are also present), where positive and negative abnormal returns keep alternating. Taken together, our evidence suggests that upon announcement investors on average do not view having more women on board as value-destructing or value-improving.

5.2.3 Are the positive value effects explained by higher risk taking?

Finally, we want to know if the positive long-run value effects are explained by the different risk-taking profiles of firms with more and fewer women, as can be measured by their differential loadings on common risk factors. To do that we calculate betas with respect to the 4 factors for each firm for each calendar year (with a minimum of 100 days of non-missing observations within that year), and estimate the same equation as in (7), but now using these betas as dependent variables.[19]

[17] As Gorovyy et al (2020) argue, fixed effects can flexibly take care of loadings on other unaccounted risk factors. In our case, these would be all observed and unobserved risk factors common to the industry.

[18] We estimate this regression on data from 6 weeks before the announcement to 2 weeks after the announcement, so that the abnormal returns for each day D_j are measured relative to the average abnormal return during the previous 6 weeks.

[19] We use years up to 2017, as this is the last year when UK factor returns are available. We also do not include

The coefficients thus measure whether firms to the right of the kink changed their betas more than firms to the left of the kink, post-compliance relative to pre-announcement periods.

The results are reported in Table 7 for firms at the first kink (Panel A) and all firms together (Panel B). As we see, for neither of the 4 risk factors did the loading of firms to the right change differentially over time compared to that of firms to the left. For robustness we also check the post-announcement period relative to pre-announcement (Appendix Table 4). As we see, there is only one significant coefficient at the 10% level (higher momentum loading of firms to the right in the specification of all firms). Given our 16 specifications in these results, this is expected and likely to be an artefact of type I error, especially since the timing of the buy-and-hold return is different (later rather than earlier) and also because this difference is not present for the firms to the right of the first kink, which have the highest return premium. Nevertheless, we do an additional back-of-the-envelope calculation and find that the magnitude of this difference is far enough from explaining the difference in buy-and-hold returns between firms to the right and left.[20] All in all, this means that the higher buy-and-hold returns of firms to the right cannot be attributed to their higher risk profiles post reform.

6 The Effect of Gender Diversity on Other Firm and Board Characteristics

So why does the gender reform bring positive value effects? In this section we explore various firm and board characteristics to shed light on this important question.

6.1 Does board composition change?

6.1.1 Average director attributes and attendance

We start by exploring the average characteristics of the board members, such as age, number of qualifications, share of independent non-executive directors, network size, and time in the company, as some of these have been proposed as potential mechanisms before (see e.g. Bertrand et al., 2019, Ferreira et al., 2020). The results of estimating (8) with these dependent variables are reported in

industry-year fixed effects in these specifications because of the nature of beta coefficients, as well as to increase power when observations are relatively few.

[20] According to Gregory et al (2013) Table 9, the price of risk for a unit of momentum factor loading was about 0.58 in the UK (significant at 10%). This means that a 0.06 higher beta implies a 0.035%(= 0.06*0.58) higher monthly return, or about a 0.4% higher annual return. This is much smaller than our effects in Section 5.2.1.

Table 8, with Panel A for all countries and Panel B for the UK.[21] As we see all results consistently indicate that the average characteristics of the boards with relatively more women (those on the right) are the same as those with fewer women (those on the left). If anything most results are negative (insignificant), indicating that there are no differences in the attributes of the incoming women directors, and hence they are unlikely to be responsible for the positive value effects.

We can also measure one behavioral response – average board meetings attendance; we report these results in column 7. First, we see that average board meeting attendance significantly increases with more women, consistent with the evidence in Adams and Ferreira (2009). Although the average percentage attendance cannot proxy how efficient these meetings are, we can back up the economic magnitude of this coefficient. Dividing the 18.12 in column 7 by the corresponding first-stage estimate of 0.319, we get a magnitude of 56.8. This means that a 10pp increase in the share of women, induced by the instrument, increases attendance by 5.68 pp. Given the overall average of about 95%, this may look small at first sight, but it also means that in many firms directors will shift from missing one time out of 20 towards full attendance. This does not look large in real-life terms, yet it still may be if the full discipline comes only when nothing is ever missed. As such, board meeting attendance appears to be at least part of the mechanism of how having more women on the board increases value.

6.1.2 Board size and the exclusion restriction

As we have seen from the first-stage results, board size appears to be sticky enough to make firms marginally prefer switching men for women. However, some may also decide to adjust the board size as a response to the regulation: firms to the right of the kink may choose to remove one board member in order to not have to hire too many women directors, making compliance easier. While this additional indirect response to the quota is an interesting and important question itself, one may also have a concern that a smaller board size may increase performance (see e.g. Jenter, Schmid, and Urban, 2019, for recent quasi-experimental evidence), and as such violate the exclusion restriction of the instrument. We report the results of estimating specifications (7) and (8) for the board size as a dependent variable in Appendix Table 5. We see that there is also some downward adjustment of the board size on average, as indicated by columns 5 and 6, suggesting that regulation also induces a change in the optimal board size.

As for the exclusion restriction, two things are important. First, it is not present for firms with

[21] Because the number of observations varies dramatically across various dependent variables, in this and the following sections we primarily explore the reduced form, as it is not dependent on the strength of the 1st stage in smaller samples.

4 vs 5 directors (neither for all countries, in column 1, nor for the UK, in column 4), and thus the sharp increases in Tobin's Q in this group of firms cannot be attributed to board size adjustments. Furthermore, we can make a back-of-the-envelope calculation based on the estimates of board size effects presented by Jenter et al.(2019). Their magnitudes suggest that one additional member of the board increases Tobin's Q by about 0.05-0.06. Multiplying this by our estimate of -2.677 in column 3, we get an effect of about 0.13-0.16 on Tobin's Q, had it been explained by the board size channel. Our estimate of 6.615 in column 3 Table 4 is clearly an order of magnitude larger, suggesting that only about one fiftieth to one fortieth of the total positive effect of the reform on firm value can be attributed to board size adjustments.

6.2 Performance decomposition and empire building

In this most common definition, Tobin's can be decomposed into $1 + MV/TA - BV/TA$, i.e. the difference between the ratios of market value of equity to total assets and book value of equity to total assets (plus 1). To see which of the two parts drives the main result, we estimate our main specification 8 with these two ratios as dependent variables in Table 9 for all countries (Panel A) and the UK separately (Panel B). We observe that it is the first part that mostly contributes to an increase in Tobin's Q: market value of equity to assets rises by about 5.3, while book value of equity to assets drops by about 0.3 (insignificantly), for every 10pp of the expected minimum share of women on boards. These coefficients are similar across panels.

So why do firms have higher market, but similar or lower book values at the same time? This can be consistent with at least four (non-mutually exclusive) explanations: higher leverage, higher dividends paid, decrease of scale (e.g. writing off some unproductive assets), and a temporary negative performance shock (that drills down retained earnings). In Table 9 we explore each of them. We observe that firms do not increase debt-to-assets ratio (column 3). Nor do they increase dividends: neither in the dividend yield (column 4), nor in dividend payout or propensity of paying any dividend (unreported for brevity). If any, the evidence suggests the opposite: firms with more women pay fewer dividends. This suggests that the first two explanations are not responsible for the observed effects on Tobin's Q.

At the same time, there is strong evidence that both assets and operating return on assets fall (columns 5 and 6). This means that there is a negative effect on the size of the firm and its operating performance, and it is worth exploring both the asset and operating sides in more detail.

6.2.1 Are women less prone to empire building?

The literature has shown that men are more overconfident (see Croson and Gneezy, 2009, for a survey) and that overconfidence leads to more investment distortions and empire building (Malmendier and Tate, 2005). This means that the potential positive effect of more women on firm value may in fact come from less empire building activity (see e.g. Levi et al., 2014, for documenting a correlation between gender and M&A activity). We now explore this channel using our causal framework.

In Table 9 we observe that total assets are about 20% lower for every 10pp of the expected minimum share of women in boards. To see if this is explained by less empire building, we explore several measures of investment activities. Specifically, in Table 10 we examine indicator variables for whether the firm spent money on acquiring a business (merger-related expenses), whether it received money from selling a business (including discontinued business units, branches, and divestitures), whether it purchased fixed assets, and whether it received a cash inflow from selling fixed assets.

As we see in column 1, firms with more women on their board are relatively less likely to acquire a business: for every 10 pp expected minimum share of women, firms are 10pp less likely to have incurred any merger-related expenses, which is clearly a large economic magnitude. Interestingly, this effect is not about switching the types of business, since there is no simultaneous change in sales of business (column 2). We observe very similar dynamics in the fixed assets: firms with more women are less likely to purchase new fixed assets (column 3), but no less likely to generate cash from selling them (column 4). This asymmetry is interesting in that it suggests that firms with more women are not just writing off some unproductive assets, but instead they are less likely to buy these assets to start with. Given that these changes are regarded positively by the market (as indicated by positive buy-and-hold returns), this evidence shows that women are indeed less prone to empire building.

6.2.2 Operating performance

Since the return on assets falls a lot (column 6 of Table 9), we further investigate the operating performance and present the results in Table 11. First, we observe that sales (both as a logarithm, in column 1, and as a share of assets, in column 2) fall significantly, with a magnitude of about 30% for each 10 pp increase in the predicted share of women. At the same time, there is no significant increase in the operating expenses to assets ratio (in column 3), nor is there any increase in R&D or labor expenses, as a share of assets (unreported for brevity, though the number of observations is much smaller for these variables). This means that the change in OROA is mostly driven by a huge decrease in sales, rather than worsening of the costs side. This is further supported by the observation, that none of the profit margins are smaller: both gross and operating profit margins (as a share of sales) are stable (columns 4 and 5).

As such, while the general short-run fall in OROA is consistent with the evidence in Matsa and Miller (2013), the underlying reason is very different (lower sales rather than higher labor expenses). To further observe that, in column 6 we see that employment also falls a lot, by about 20% for each 10 pp increase in the predicted share of women, but not the labor productivity (column 7) or average wage (column 8). This means that the quality of workers does not deteriorate. All these results are important together, since they indicate that the primary driver of a fall in OROA is the dramatic drop in sales (which is accompanied by all other proportional changes). At the same time, none of the sales-based margins fall, suggesting that the company makes the same profit out of each unit sold and that workers are similarly productive and paid similarly.

As Matsa and Miller (2013) discuss, such differences in the return on assets can diminish over time. And in fact by examining a longer sample, Eckbo et al (forthcoming) show that the negative ROA effects disappear over time. Additionally, any perturbation of the board per se, even the one unrelated to gender, is likely to affect performance in the short run (see e.g. Nguyen and Nielsen, 2010, on the negative reaction to sudden director deaths). Thus, the remaining question is whether these changes are in fact short-run or not. Most countries have only recently implemented the regulation, so we haven't yet lived to observe and analyze the more long-run data. What we can do now is to explore the yearly dynamics, and specifically for the most interesting variable, which is the main driver of the operating performance decreases, – sales. We therefore plot the dynamics for the logarithm of sales, based on a yearly regression similar to (3), in Figure 9 (we do so for the UK, again because different countries have different durations between announcement and compliance). As we notice, there are some slight upward dynamics in the later years, and sales in the later two years are not significantly different from the whole pre-announcement period. This is suggestive of the shock to sales being more transitory, rather than permanent.

To sum up, we see that firms with more women on the board are less prone to empire building. This drop in assets is accompanied by a more than proportional drop in sales that drives some of the operating performance indicators, such as OROA, down. However, all profit margins are stable, and workers do not become any less productive, suggesting that it is indeed a huge decrease in sales that drives everything down. This shock is likely to be temporary, and later research will be able to explore that. Importantly, none of these changes are accompanied by negative market reactions. If anything it is positive, suggesting that these operating changes are viewed positively by the market, consistent with firms with more women scaling down the inefficient operations.

7 Conclusion

In this paper we explore the effects of increased female presence on corporate boards on value, operating performance, and other firm and board characteristics, for a set of European countries that introduced soft or hard regulation with respect to the share of women. While previous research has extensively looked at these questions using instruments based on past female shares, we show that they cannot be applied, at least in our sample of all countries, because firms with women and those without already grow at different rates before any regulation is introduced. This difference in trends would thus produce an overly pessimistic view of the effects of women on corporate performance.

Instead, we use a novel identification strategy that allows us to estimate causal effects under the minimum possible assumptions in a setting with a universal quota. We find evidence of positive effects on the value of the company (as measured by Tobin's Q and buy-and-hold returns), which are not anticipated by the market when regulation is announced. We further dig into possible mechanisms and observe that these positive effects are not explained by higher riskiness of firms or higher-quality boards (except for a slight positive effect on board meetings attendance). The main driver of our effects is the reduction in empire building, as proxied by lower assets, lower merger-related expenses and less investment in purchases of fixed assets. These effects are accompanied by a disruption in sales, which drives some of the operating performance indicators down. However, since margins and labor productivity are stable, and these operating performance decreases are not accompanied by lower market values, this is further evidence that firms with more women scale down the inefficient operations.

Our results have important policy implications. With a general socially-based move towards gender equality, many countries have pushed quotas in corporate boards, yet the effects on shareholder value and firm policies have been debatable. We show that there is no negative effect on value, and boards do not become any less competent with more women on boards. Having more women on boards, besides being socially important, therefore, is also in favor of corporate interests.

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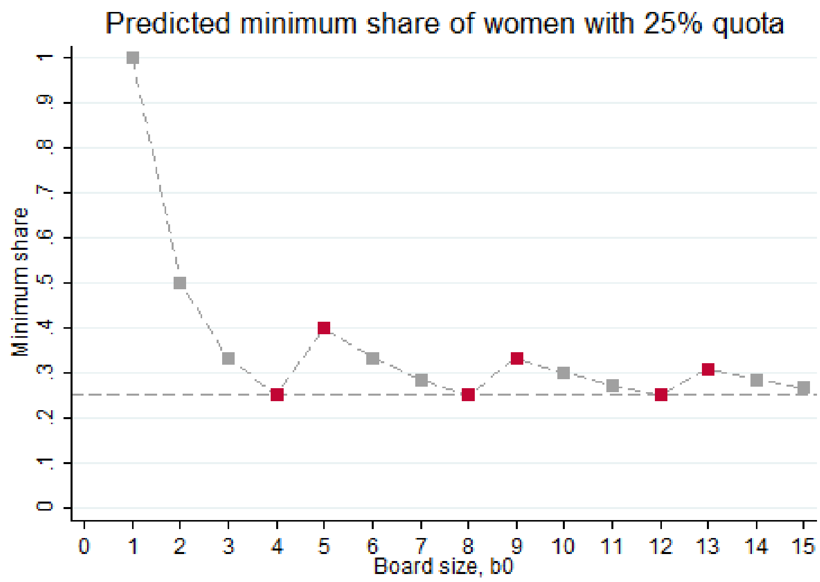
Figures and Tables

Figure 1



Note: This figure plots the minimum share of women that firms must have to comply with the 25% quota, as a function of board size. The discontinuity samples are highlighted in red.

Figure 2. Distribution of Board Size as of pre-announcement year

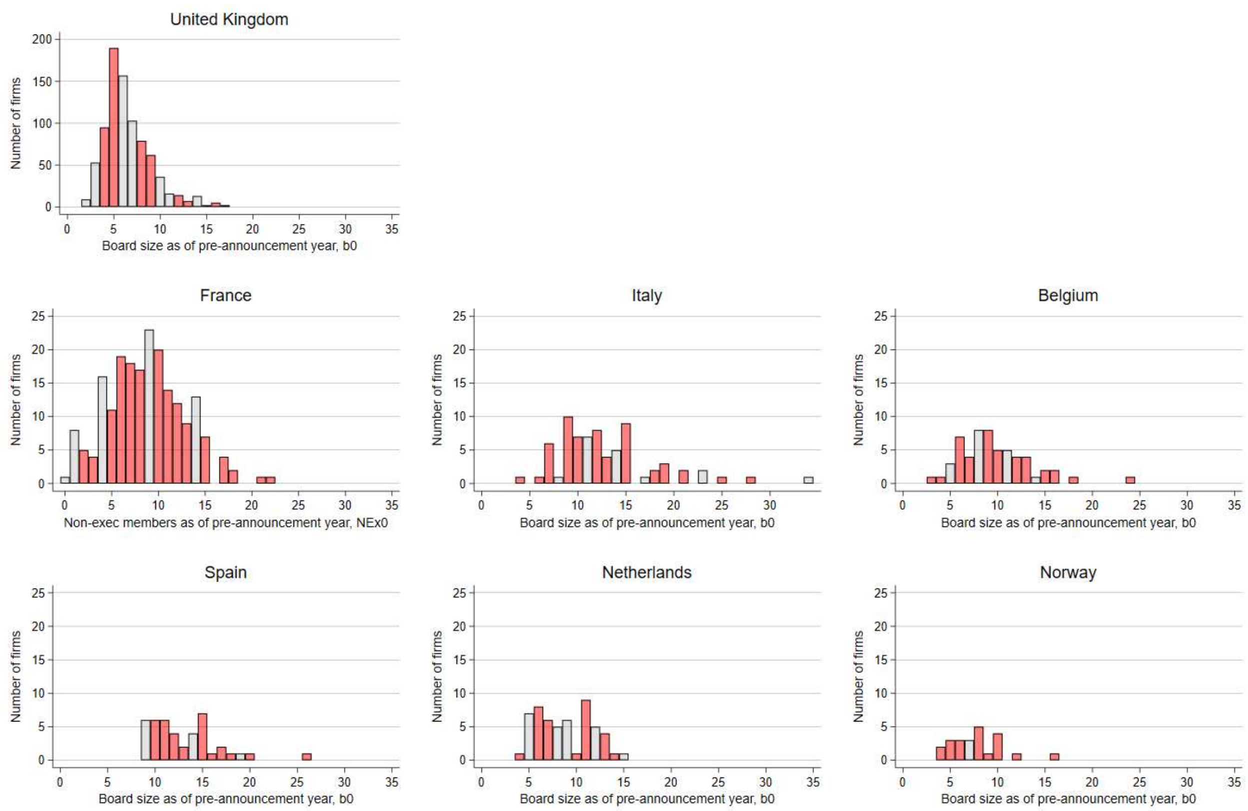


Figure 3. First-stage dynamics

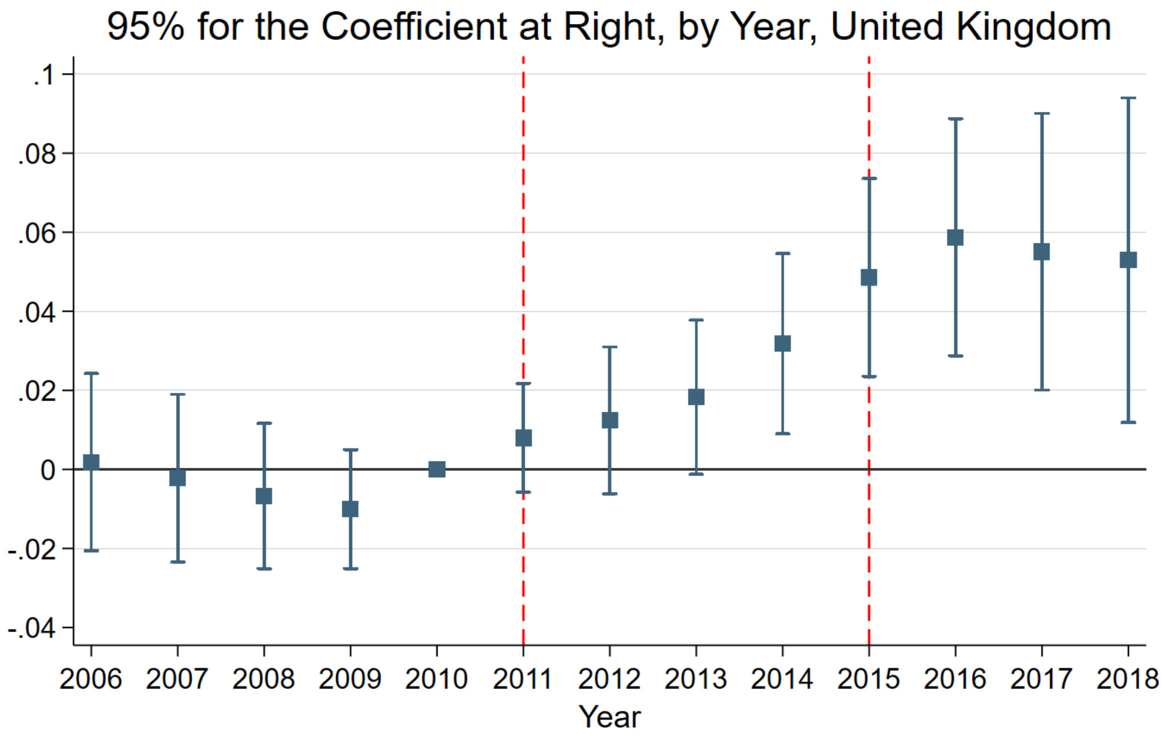


Figure 4. First-stage dynamics

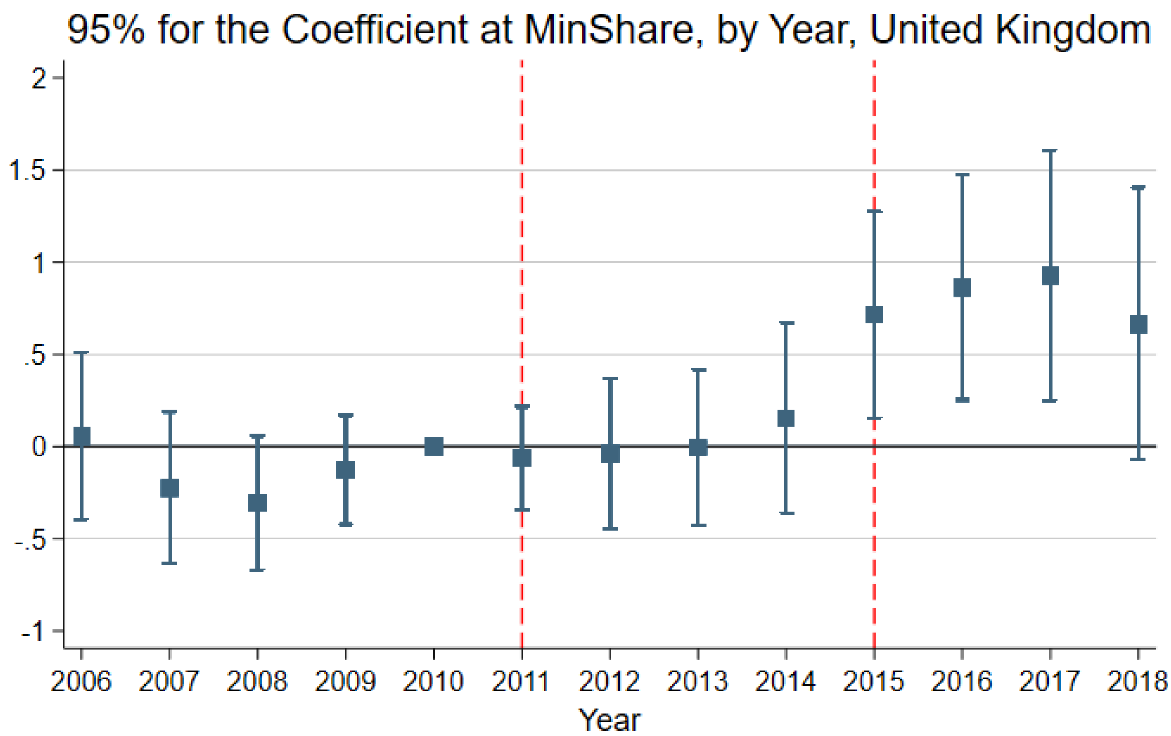


Figure 5. Buy-and-hold returns in the UK, firms at the largest kink

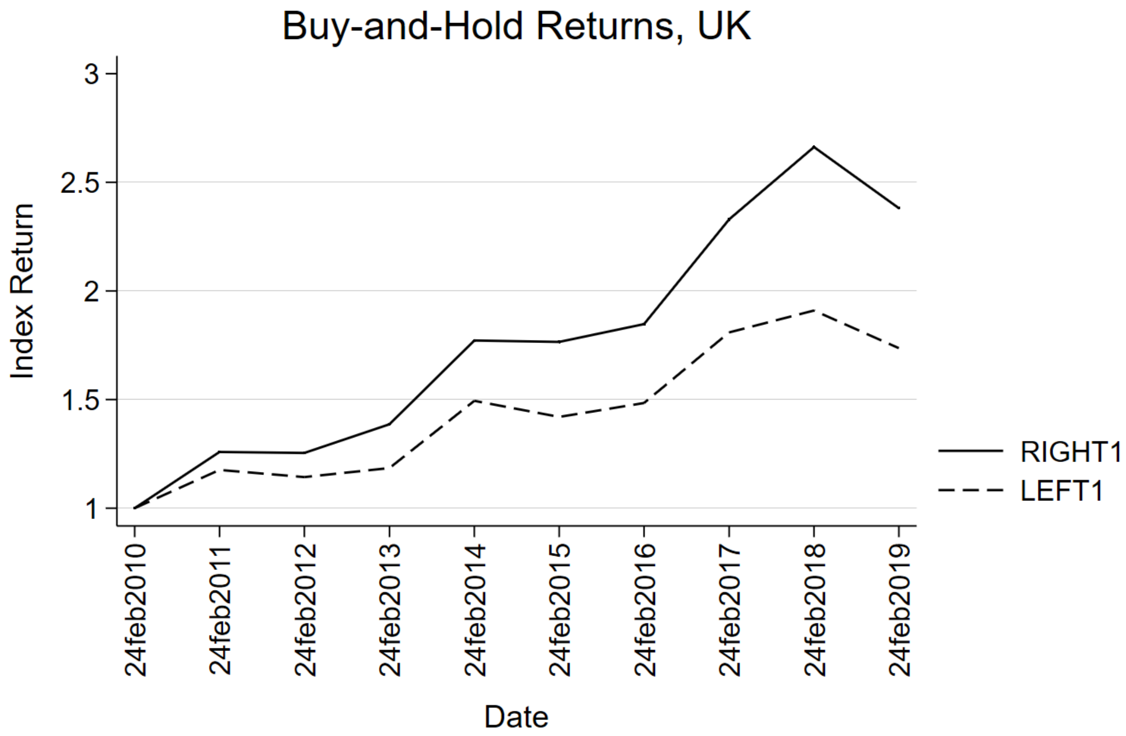


Figure 6. Buy-and-hold returns in the UK, firms at all kinks

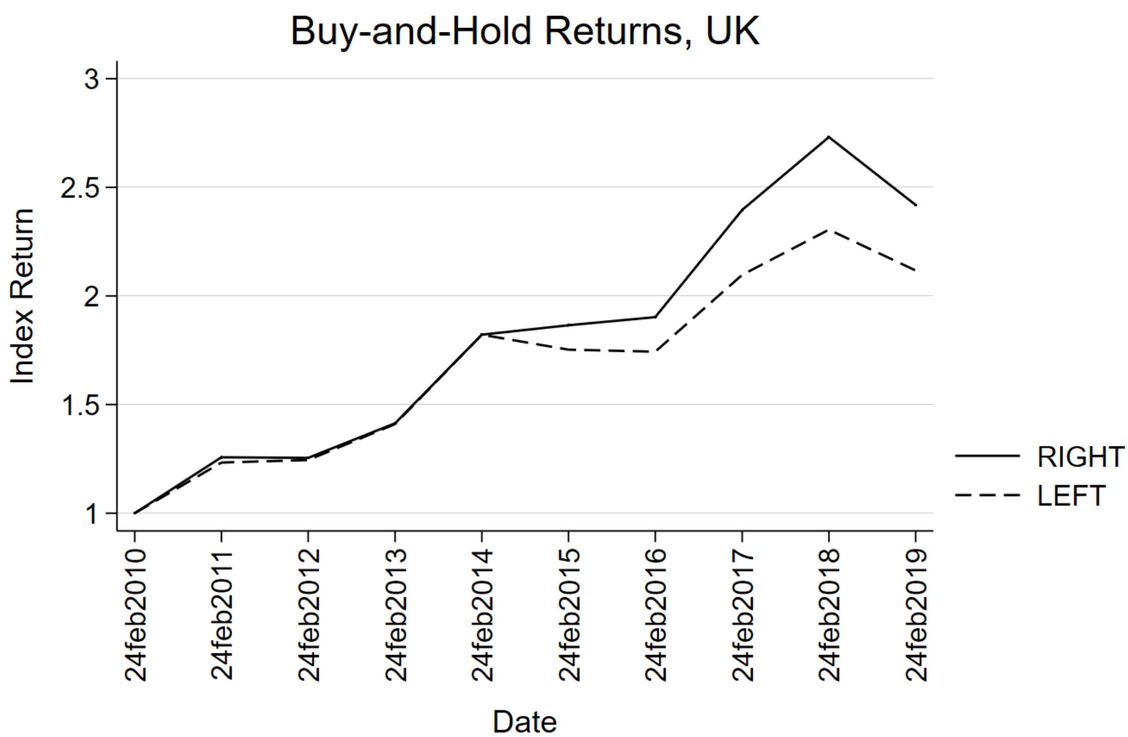


Figure 7. The difference in CARs in the UK, across firms at the largest kink

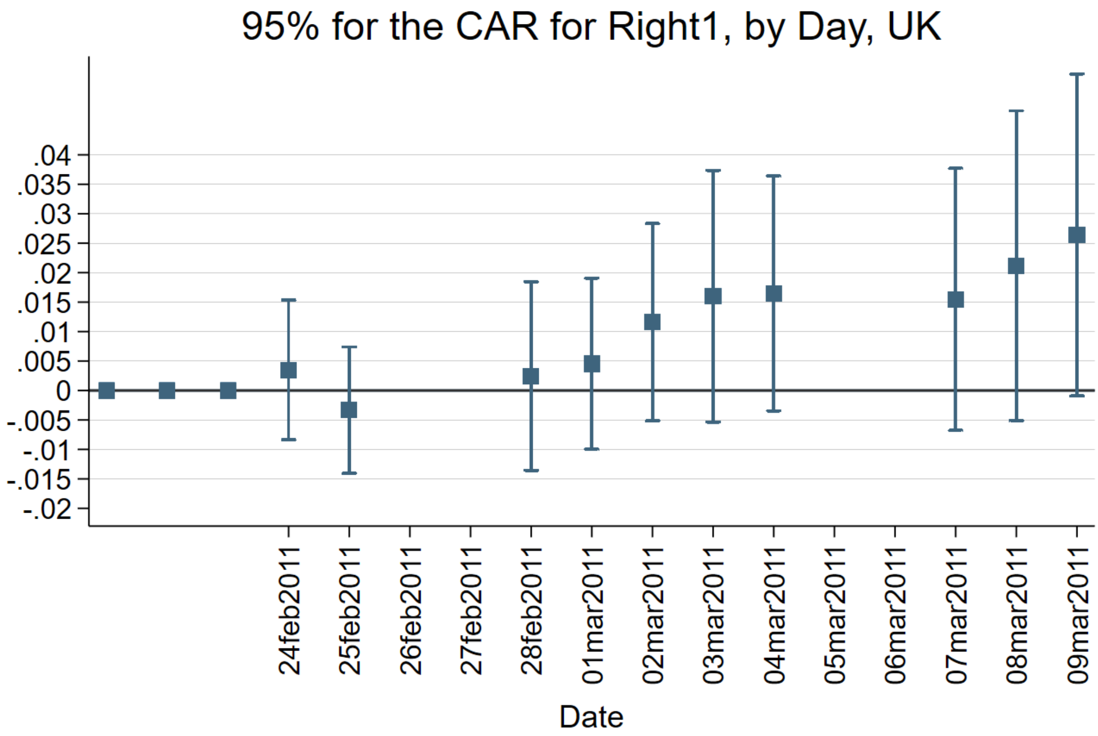


Figure 8. The difference in CARs in the UK, across firms at all kinks

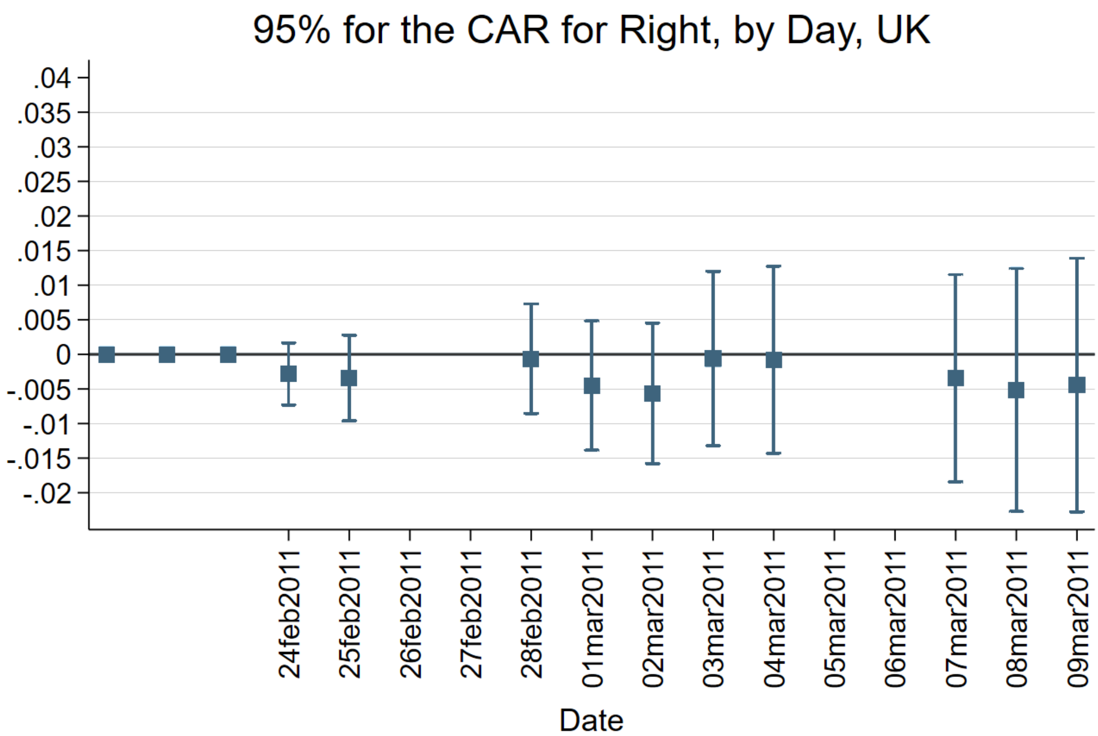


Figure 9

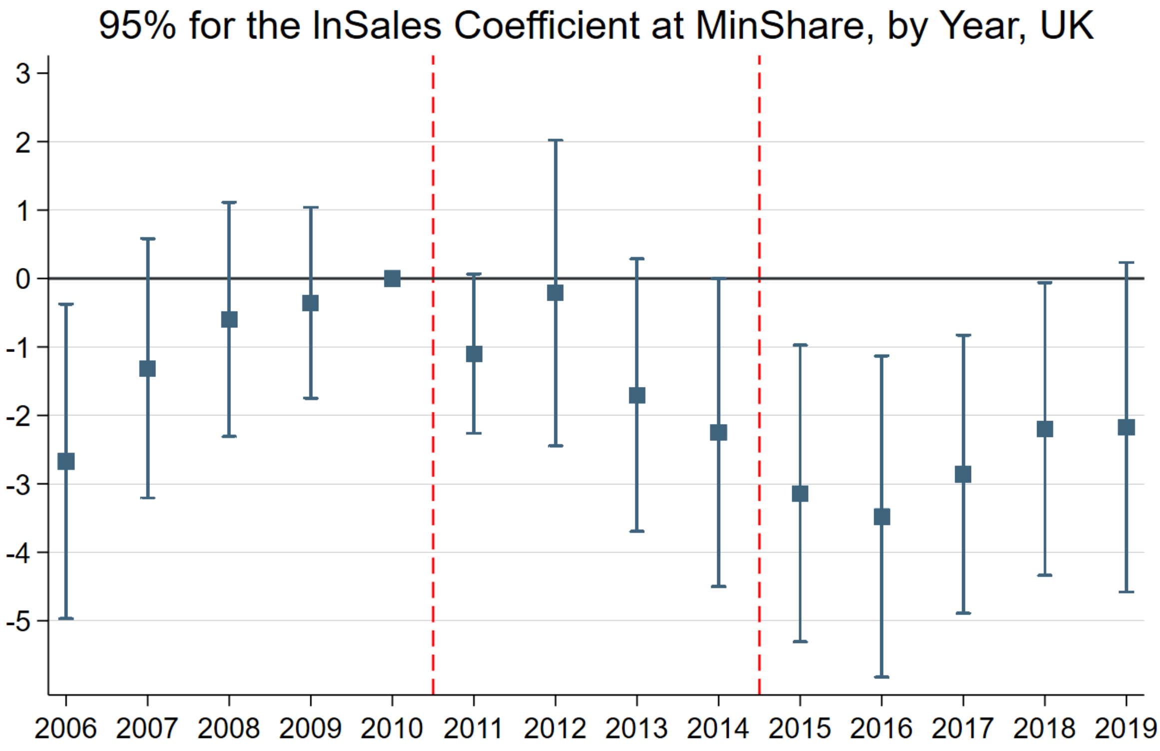

95% for the lnSales Coefficient at MinShare, by Year, UK
3
2
1
0
-1
-2
-3
-4
-5
2006
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
2018
2019

Table 1. Summary of Quotas and Soft Regulation in European Countries in Sample

Country	Quota or soft regulation in place	Minimum % required	Pre-announcement placebo years in sample	Regulation announcement year	Compliance year	Post-compliance years in sample	N
UK	Self-regulation – from 2012 on the basis of principles of UK CG Code (following the Lord Davies' recommendation). The recommended target for listed companies in FTSE 100: 25%, by 2015 is applicable to all board members. FTSE 350 companies were recommended setting their own aspirational targets to be achieved.	25%	2008-2010	2011	2015	2015-2018	445
France	Quota of 40% applicable to non-executive directors in large listed and nonlisted companies.	40%*	2007-2009	2010	2017	2017-2019	144
Italy	Quota of one-third of each gender for listed companies and state-owned companies to be achieved by 2015.	33%	2008-2010	2011	2015	2015-2018	56
Belgium	Quota for executives and non-executives in state-owned and listed companies - by 2017, in listed SMEs - by 2019.	33%	2008-2010	2011	2017	2017-2019	41
Spain	A gender equality law obliging public companies and IBEX 35-quoted firms with more than 250 employees to attain a minimum 40% share of each gender by 2015.	40%	2004-2006	2007	2015	2015-2018	31
Netherlands	Target of 30% in the boards of large companies by 2016 - "comply or explain" mechanism.	30%	2010-2012	2013	2016	2016-2019	29
Norway	Quota: in February 2002, the government gave a deadline of July 2005 for private listed companies to raise the proportion of women on their boards to 40%. In January 2006 legislation was introduced giving companies a final deadline of January 2008.	40%**	1999-2001	2002	2005	2005-2008	20
						Total:	766

Sources: Davies (2012), European Commission (2016), Seierstad et al (2017)

N is the number of companies in the discontinuity sample as of pre-announcement year that have at least one observation post-compliance.

* As this quota is applicable to non-executive directors only, we consider discontinuity samples that are based on the ex ante number of non-executive directors, rather than the total board size.

** For smaller boards the quota is stated in terms of the number of women, which we account in the analysis. Specifically, boards of less than 3 people should have at least 1 woman, boards of 4-5 people - at least 2 women, boards of 6-8 people - at least 3 women, and larger boards - at least 40% of women.

Table 2. Descriptive Statistics

Note: The table reports the number of observations as of post-compliance years only.

Variable	Mean	STD	N
Financials:			
Total Assets	23 bln	117 bln	2,857
ln (Total Assets)	20.155	2.907	2,857
Market Capitalization	4.5 bln	1.3 bln	2,857
ln (Market Capitalization)	19.812	2.577	2,857
Board Structure and Instrumental Variables:			
Board Size	8.109	3.722	2,857
Board Size as of pre-announcement year (b_{i0})	8.193	4.024	2,857
Share of female directors	0.211	0.164	2,857
Share of female directors as of pre-announcement year	0.062	0.087	2,857
Complier	0.377	0.485	2,857
Predicted minimum required share of women ($MinShare_i$)	0.357	0.077	2,857
Dummy for being to the right of the kink ($Right_i$)	0.526	0.499	2,857
Dependent Variables: Value, Performance, and Other Firm Characteristics			
Tobin's Q	1.888	2.706	2,857
Market Value of Equity to Total Assets	1.361	2.522	2,857
Book Value of Equity to Total Assets	0.486	0.399	2,857
Total Debt to Total Assets	0.185	0.197	2,854
Dividend Payer	0.742	0.437	2,225
Dividend Yield	0.024	0.024	2,225
ln (Sales)	19.353	2.860	2,582
Gross Profit Margin	0.521	0.284	2,286
Operating Profit Margin	-0.758	9.022	2,529
OROA	-0.010	0.320	2,769
ln (Employment)	7.432	2.586	2,257
ln (Labor Productivity)	12.501	1.128	2,085
ln (Average Wage)	10.296	1.671	1,116
Acquisition of a Business	0.856	0.351	1,187
Sale of a Business	0.707	0.455	540
Purchase of Fixed Assets	0.985	0.120	2,249
Sale of Fixed Assets	0.880	0.325	1,116
Beta (Rmrf) - UK only	0.812	0.524	777
Beta (SMB) - UK only	0.471	0.571	777
Beta (HML) - UK only	0.095	0.641	777
Beta (Momentum) - UK only	-0.019	0.359	777
Buy-and-Hold Returns - UK only	2.237	2.556	1,760
Dependent Variables: Board Characteristics			
Average age	58.062	4.475	2,855
Average number of qualifications	1.702	0.588	2,857
Average director network size	974.886	730.746	2,857
Average time in company	7.889	4.074	2,857
Share of independent directors	0.524	0.250	2,857
Board meeting attendance	95.093	5.755	1,038
Board specific skills	46.103	22.532	1,205

Table 3. Share of Women, Right and Predicted Minimum Share: First-Stage Results

This table reports the results of estimating the following specification using the OLS framework:

$$Share_{it}=\gamma Post_{ct}Right_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+v_{it} \text{ (columns 1, 2, 4, 5) or}$$

$$Share_{it}=\gamma Post_{ct}MinShare_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+v_{it} \text{ (columns 3 and 6),}$$

where $Share_{it}$ is the fraction of women directors of firm *i* in year *t*, $Post_{ct}$ is the (country-specific) dummy variable that takes value of 1 from announcement year to up to three years afterwards (columns 1 to 3), or from compliance year to three years afterwards (columns 4 to 6), and zero - for the year before announcement and up to three years before that, $Right_i$ is the dummy for being to the right of the kink and $MinShare_i$ is the predicted minimum share of firm i (the instruments, defined in Section 2), measured in the base year, λ_{kct} are kink-year fixed effects (specific to the country), λ_{sct} are industry-year fixed effects (specific to the country), λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons.
* indicates 10% significance; ** 5% significance; *** 1% significance.

Panel A: UK	**Post-announcement vs pre-announcement years**			**Post-compliance vs pre-announcement years**		
	1	2	3	4	5	6
$Post_{ct}$ * $Right_i$	0.0216**	0.0161**		0.0647***	0.0303***	
	(0.0102)	(0.00734)		(0.0153)	(0.0112)	
$Post_{ct}$ * $MinShare_i$			0.133**			0.305***
			(0.0600)			(0.0901)
Number of firms	280	444	444	281	445	445
Observations	2,182	3,466	3,466	1,931	3,064	3,064
1st stage F-statistic	4.53	4.80	4.95	17.87	7.38	11.42
Panel B: All countries	**Post-announcement vs pre-announcement years**			**Post-compliance vs pre-announcement years**		
	1	2	3	4	5	6
$Post_{ct}$ * $Right_i$	0.0201**	0.0100*		0.0658***	0.0308***	
	(0.00961)	(0.00599)		(0.0144)	(0.00896)	
$Post_{ct}$ * $MinShare_i$			0.107*			0.319***
			(0.0561)			(0.0840)
Number of firms	360	756	756	361	757	757
Observations	2,769	5,838	5,838	2,418	5,004	5,004
1st stage F-statistic	4.39	2.82	3.62	20.78	11.79	14.38
Kink * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample: Largest kink only	Yes			Yes		
Sample: All kinks		Yes	Yes		Yes	Yes

Table 4. Tobin's Q and the Share of Women: Reduced-form and Second-stage Results

This table reports the results of estimating the following specification using the OLS framework:

$$Y_{it}=\gamma Post_{ct}Right_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+v_{it} \text{ (columns 1, 2, 4, 5) or}$$

$$Y_{it}=\gamma Post_{ct}MinShare_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+v_{it} \text{ (columns 3 and 6),}$$

where Y_{it} is Tobin's Q of firm *i* in year *t*, $Post_{ct}$ is the (country-specific) dummy variable that takes value of 1 from compliance year to three years afterwards, and zero - for the year before announcement and up to three years before that, $Right_i$ is the dummy for being to the right of the kink and $MinShare_i$ is the predicted minimum share of firm *i* (the instruments, defined in Section 2), measured in the base year, λ_{kct} are kink-year fixed effects (specific to the country), λ_{sct} are industry-year fixed effects (specific to the country), λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons.
* indicates 10% significance; ** 5% significance; *** 1% significance.

Tobin's Q	**All countries: Post-compliance vs pre-announcement years**			**UK: Post-compliance vs pre-announcement years**		
	1	2	3	4	5	6
$Post_{ct}$ * $Right_i$	1.011**	0.643**		1.027*	0.799**	
	(0.501)	(0.256)		(0.537)	(0.342)	
$Post_{ct}$ * $MinShare_i$			6.615**			6.947**
			(2.792)			(3.067)
IV-2SLS coefficient	15.36*	20.92**	20.76**	15.88*	26.38*	22.81**
IV-2SLS standard error	(7.982)	(9.710)	(9.817)	(8.726)	(13.92)	(11.49)
Robust Weak-IV AR 95% CI	[0.45, 35.5]	[4.71, 54.04]	[3.69, 50.35]	[-0.41, 38.75]	[4.46, 97.22]	[3.19, 61.74]
Robust Weak-IV AR P-value	0.0438	0.0120	0.0178	0.0558	0.0193	0.0235
1st stage F-statistic	20.78	11.79	14.38	17.87	7.38	11.42
Number of firms	361	757	757	281	445	445
Observations	2,418	5,004	5,004	1,931	3,064	3,064
Kink * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample: Largest kink only	Yes			Yes		
Sample: All kinks		Yes	Yes		Yes	Yes

Table 5. Tobin's Q and the Share of Women: Country-Level Analysis

This table reports the results of estimating the following specification using the OLS framework:

$$Y_{it}=\gamma Post_{ct}Right_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+v_{it} \text{ (Panel A) or}$$

$$Y_{it}=\gamma Post_{ct}MinShare_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+v_{it} \text{ (Panel B),}$$

where Y_{it} is Tobin's Q (logarithm of Tobin's Q) of firm *i* in year *t* (Panel A and B, respectively), $Post_{ct}$ is the (country-specific) dummy variable that takes value of 1 from compliance year to three years afterwards, and zero - for the year before announcement and up to three years before that , $Right_i$ is the dummy for being to the right of the kink and $MinShare_i$ is the predicted minimum share of firm *i* (the instruments, defined in Section 2), measured in the base year, λ_{kct} are kink-year fixed effects (specific to the country), λ_{sct} are industry-year fixed effects (specific to the country), λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons.
* indicates 10% significance; ** 5% significance; *** 1% significance.

Panel A: $Post_{ct}$ * $Right_i$	**All**	**UK**	**Non-UK**	**FR**	**NL**	**NO**	**BE+IT+SP**
	1	2	3	4	5	6	7
First Stage	0.0337***	0.0296**	0.0640**	0.0910**	0.0649***	0.0423***	0.00217
	(0.0108)	(0.0118)	(0.0249)	(0.0379)	(0.00650)	(0.0123)	(0.0224)
Reduced Form	0.765**	0.816**	0.391**	0.531**	0.169	0.984**	0.0464
	(0.324)	(0.367)	(0.168)	(0.259)	(0.105)	(0.388)	(0.258)
IV-2SLS coefficient	22.68**	27.57*	6.105*	5.836	2.606	23.27***	21.43
IV-2SLS standard error	(11.31)	(15.49)	(3.520)	(3.583)	(1.785)	(2.391)	(322.9)
Robust Weak-IV AR 95% CI	[4.01, 66.21]	[3.45, 125.81]	[0.93, 28.07]	[0.29, 32.45]	[-0.49, 6.71]	[12.34, 26.26]	(-inf, +inf)
Robust Weak-IV AR P-value	0.018	0.026	0.020	0.040	0.106	0.011	0.9370
1st stage F-statistic	9.72	6.29	6.60	5.76	99.74	11.77	0.01
Number of firms	587	419	168	65	23	12	68
Observations	3,923	2,882	1,041	377	157	71	436
Panel B: $Post_{ct}$ * $MinShare_i$	**All**	**UK**	**Non-UK**	**FR**	**NL**	**NO**	**BE+IT+SP**
	1	2	3	4	5	6	7
First Stage	0.328***	0.301***	0.793***	1.133***	0.800***	0.423***	-0.0466
	(0.0881)	(0.0915)	(0.303)	(0.437)	(0.117)	(0.123)	(0.286)
Reduced Form	6.780**	6.866**	5.261**	6.465**	1.770	9.836**	2.168
	(2.956)	(3.120)	(2.084)	(3.151)	(1.306)	(3.878)	(2.992)
IV-2SLS coefficient	20.70**	22.79*	6.630*	5.706*	2.213	23.27***	-46.52
IV-2SLS standard error	(10.07)	(11.80)	(3.661)	(3.458)	(1.572)	(2.391)	(235.0)
Robust Weak-IV AR 95% CI	[3.12, 51.37]	[2.60, 63.79]	[1.38, 28.87]	[0.27, 25.43]	[-1.09, 5.35]	[12.34, 26.26]	(-inf, +inf)
Robust Weak-IV AR P-value	0.021	0.028	0.012	0.040	0.175	0.011	0.8270
1st stage F-statistic	13.83	10.85	6.86	6.72	47.16	11.77	0.03
Number of firms	587	419	168	65	23	12	68
Observations	3,923	2,882	1,041	377	157	71	436
Kink * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample: Two largest kinks	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6. Long-run Buy-and-Hold Returns and the Share of Women: Reduced-form Results for the UK firms

This table reports the results of estimating the following specification using the OLS framework on the sample of UK firms:

$$BH_{it}=\gamma_{2011}D_{2011}Right_i+\gamma_2D_2Right_i+\ldots+\gamma_{2019}D_{2019}Right_i+\lambda_{kt}+\lambda_{st}+\lambda_i+v_{it} ,$$

where BH_{it} is the is the buy-and-hold return of firm *i* in year *t* (i.e. the total return that an investor would earn if she held this stock till year *t*), $Right_i$ is the dummy for being to the right of the kink (as defined before), D_j are the indicator variables for each particular year *j* after announcement, λ_{kt} are kink-year fixed effects, λ_i and λ_{st} are firm and industry-year fixed effects. All returns are measured relative to the year before the announcement (February 24th, 2010), when all D_j are zero and buy-and-hold returns of all firms are mechanically set to 1. The coefficients below report the difference in buy-and-hold returns in each year j, γ_j. Panel A reports the results for the largest kink only, while Panel B considers all kinks. Standard errors are clustered at the industry level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	Long-run Buy-and-Hold Returns, up to year j after announcement, UK firms								
j =	2011	2012	2013	2014	2015	2016	2017	2018	2019
	Panel A: Largest kink only								
$Right_i$	0.124	0.141	0.190*	0.289*	0.352*	0.403*	0.549*	0.703**	0.630**
	(0.0790)	(0.0976)	(0.112)	(0.156)	(0.185)	(0.222)	(0.311)	(0.350)	(0.316)
Number of firms	280	280	280	280	280	280	280	280	280
	Panel B: All kinks								
$Right_i$	0.0782	0.0683	0.0743	0.117	0.230	0.245	0.371*	0.462*	0.346
	(0.0546)	(0.0669)	(0.0829)	(0.122)	(0.143)	(0.159)	(0.225)	(0.268)	(0.238)
Number of firms	443	443	443	443	443	443	443	443	443
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 7. Risk Taking and the Share of Women: Reduced-form Results for the UK firms

This table reports the results of estimating the following specification using the OLS framework:

$$Beta_{it}=\gamma Post_{ct}Right_i+\lambda_{kt}+\lambda_{st}+\lambda_i+v_{it},$$

where $Beta_{it}$ is a beta with respect to the 4 factors (with a minimum of 100 days of non-missing observations within that year) of firm *i* in year *t*, $Post_t$ is the dummy variable that takes value of 1 from compliance year to three years afterwards, and zero - for the year before announcement and up to three years before that, $Right_i$ is the dummy for being to the right of the kink (the instrument, defined in Section 2), measured in the base year, λ_{kt} are kink-year fixed effects, λ_{st} are industry-year fixed effects, λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	Betas, with respect to the 4 factors, UK firms			
	Beta (Rmrf)	Beta (SMB)	Beta (HML)	Beta (Momentum)
	Panel A: Largest kink only			
$Post_{ct}$ * $Right_i$	0.0984	0.100	0.202	0.0771
	(0.115)	(0.101)	(0.135)	(0.105)
Number of firms	157	157	157	157
Observations	618	618	618	618
	Panel B: All kinks			
$Post_{ct}$ * $Right_i$	-0.00719	-0.00945	-0.0715	0.0555
	(0.0547)	(0.0580)	(0.0899)	(0.0405)
Number of firms	302	302	302	302
Observations	1,382	1,382	1,382	1,382
Kink * Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry * Year FE	Yes	Yes	Yes	Yes

Table 8. Board Characteristics and the Share of Women: Reduced-form Results

This table reports the results of estimating the following specification using the OLS framework:

$$Y_{it}=\gamma Post_{ct}MinShare_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+\nu_{it} \ ,$$

where Y_{it} is the dependent variable of firm *i* in year *t*, $Post_{ct}$ is the (country-specific) dummy variable that takes value of 1 from compliance year to up to three years afterwards, and zero - for the year before announcement and up to three years before that, $MinShare_i$ is the predicted minimum share of firm i (the instruments, defined in Section 2), λ_{kct} are kink-year fixed effects (specific to the country), λ_{sct} are industry-year fixed effects (specific to the country), λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	Average age	Average number of qualifications	Average network size	Average time in company	Share of independent directors	Average board-specific skills	Average board meetings attendance
	Panel A: All countries						
	1	2	3	4	5	6	7
$Post_{ct}$ * $MinShare_i$	-2.601	-0.116	-410.4	-2.344	-0.0793	35.13	18.12**
	(3.056)	(0.375)	(465.0)	(2.548)	(0.127)	(39.70)	(8.389)
Number of firms	766	766	766	766	766	338	308
Observations	5,055	5,055	5,055	5,055	5,055	1,815	1,605
	Panel B: UK						
	1	2	3	4	5	6	7
$Post_{ct}$ * $MinShare_i$	-1.236	-0.0738	-453.6	-1.423	-0.0389	31.01	15.54*
	(3.252)	(0.410)	(513.7)	(2.735)	(0.137)	(45.00)	(8.749)
Number of firms	445	445	445	445	445	142	143
Observations	3,064	3,064	3,064	3,064	3,064	851	854
Kink * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 9. Q Decomposition, Leverage, and Operating Performance: Reduced-form Results

This table reports the results of estimating the following specification using the OLS framework:

$$Y_{it}=\gamma Post_{ct}MinShare_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+\nu_{it} ,$$

where Y_{it} is the dependent variable of firm *i* in year *t*, $Post_{ct}$ is the (country-specific) dummy variable that takes value of 1 from compliance year to three years afterwards, and zero - for the year before announcement and up to three years before that, $MinShare_i$ is the predicted minimum share of firm *i* (the instrument, defined in Section 2), measured in the base year, λ_{kct} are kink-year fixed effects (specific to the country), λ_{sct} are industry-year fixed effects (specific to the country), λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	Market Value of Equity / Total Assets	Book Value of Equity / Total Assets	Total Debt / Total Assets	Dividend Yield	ln Total Assets	OROA
	Panel A: All countries					
	1	2	3	4	5	6
$Post_{ct}$ * $MinShare_i$	5.349**	-0.265	-0.0464	-0.0334**	-2.275***	-0.926***
	(2.700)	(0.278)	(0.135)	(0.0161)	(0.722)	(0.314)
Number of firms	766	766	766	754	766	742
Observations	5,055	5,055	5,048	4,292	5,055	4,895
	Panel B: UK					
	1	2	3	4	5	6
$Post_{ct}$ * $MinShare_i$	5.362*	-0.283	-0.0291	-0.0295*	-2.395***	-1.055***
	(2.808)	(0.309)	(0.148)	(0.0173)	(0.793)	(0.348)
Number of firms	445	445	445	442	445	440
Observations	3,064	3,064	3,059	2,690	3,064	3,029
Kink * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 10. Investments and the Share of Women: Reduced-form Results

This table reports the results of estimating the following specification using the OLS framework:

$$Y_{it}=\gamma Post_{ct}MinShare_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+\nu_{it} ,$$

where Y_{it} is the dependent variable of firm *i* in year *t*, $Post_{ct}$ is the (country-specific) dummy variable that takes value of 1 from compliance year to three years afterwards, and zero - for the year before announcement and up to three years before that, $MinShare_i$ is the predicted minimum share of firm *i* (the instrument, defined in Section 2), measured in the base year, λ_{kct} are kink-year fixed effects (specific to the country), λ_{sct} are industry-year fixed effects (specific to the country), λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	Acquisition of Business	**Sale of Business**	**Purchase of Fixed Assets**	**Sale of Fixed Assets**
	Panel A: All countries			
	1	2	3	4
$Post_{ct}$ * $MinShare_i$	-1.039**	0.364	-0.182*	-0.0950
	(0.476)	(0.799)	(0.107)	(0.318)
Number of firms	463	207	661	372
Observations	2,137	805	4,089	1,880
	Panel B: UK			
	1	2	3	4
$Post_{ct}$ * $MinShare_i$	-1.303**	0.419	-0.178	-0.0803
	(0.521)	(0.805)	(0.122)	(0.323)
Number of firms	251	152	353	267
Observations	1,167	593	2,325	1,428
Kink * Country * Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry * Country * Year FE	Yes	Yes	Yes	Yes

Table 11. Operating Performance and the Share of Women: Reduced-form Results

This table reports the results of estimating the following specification using the OLS framework:

$$Y_{it}=\gamma Post_{ct}MinShare_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+\nu_{it} ,$$

where Y_{it} is the dependent variable of firm *i* in year *t*, $Post_{ct}$ is the (country-specific) dummy variable that takes value of 1 from compliance year to three years afterwards, and zero - for the year before announcement and up to three years before that, $MinShare_i$ is the predicted minimum share of firm *i* (the instrument, defined in Section 2), measured in the base year, λ_{kct} are kink-year fixed effects (specific to the country), λ_{sct} are industry-year fixed effects (specific to the country), λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	ln Sales	**Sales / Total Assets**	**Operating Expenses / Total Assets**	**Gross Profit Margin**	**Operating Profit Margin**	**ln Employment**	**ln Labor Productivity**	**ln Average Wage**
	Panel A: All countries							
	1	2	3	4	5	6	7	8
$Post_{ct}$ * $MinShare_i$	-3.043***	-0.818**	0.432	-0.256	0.395	-2.039***	-0.420	-0.868
	(0.766)	(0.324)	(0.493)	(0.165)	(7.606)	(0.755)	(0.576)	(1.473)
Number of firms	725	742	742	667	711	691	643	359
Observations	4,511	4,894	4,894	4,011	4,418	4,210	3,868	1,889
	Panel B: UK							
	1	2	3	4	5	6	7	8
$Post_{ct}$ * $MinShare_i$	-3.318***	-0.919***	0.498	-0.219	-0.001	-2.448***	-0.364	-2.317
	(0.856)	(0.356)	(0.545)	(0.187)	(8.724)	(0.885)	(0.690)	(2.307)
Number of firms	423	440	440	389	419	372	343	134
Observations	2,649	3,028	3,028	2,336	2,622	2,287	2,073	681
Kink * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Country * Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix Tables

Appendix Table 1. Share of Women and the Instruments: Placebo Past Trends

This table reports the results of estimating the following specification using the OLS framework:

$$\Delta Share_{it}=\gamma Right_i+\lambda_{kc}+\lambda_{sc}+v_{it} \text{ (columns 1, 2, 4, 5) or}$$

$$\Delta Share_{it}=\gamma MinShare_i+\lambda_{kc}+\lambda_{sc}+v_{it} \text{ (columns 3 and 6),}$$

where $\Delta Share_{it}$ is the yearly change in the fraction of women directors of firm *i* in year *t*, $Right_i$ is the dummy for being to the right of the kink, $MinShare_i$ is the predicted minimum share of firm *i* (the instruments, defined in Section 2), all as of the year before announcement, λ_{kc} are kink fixed effects (specific to the country), λ_{sc} are industry fixed effects (specific to the country). Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). All regressions are estimated using all available data before announcement (up to 10 years before). The number of firms and observations excludes singletons.
* indicates 10% significance; ** 5% significance; *** 1% significance.

	Instruments used in the paper					
Share of female directors	**All countries**			**UK**		
	1	2	3	4	5	6
$Right_i$	-0.00225	-0.00109		-0.00133	-0.000433	
	(0.00252)	(0.00127)		(0.00269)	(0.00154)	
$MinShare_i$			-0.00707			-0.00333
			(0.0131)			(0.0140)
Number of firms	364	771	771	289	455	455
Observations	2,088	5,168	5,168	1,659	3,048	3,048
Kink * Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample: Largest kink only	Yes			Yes		
Sample: All kinks		Yes	Yes		Yes	Yes

Appendix Table 2. Tobin's Q and the Instruments: Placebo Past Trends

This table reports the results of estimating the following specification using the OLS framework:

$\Delta Y_{it}=\gamma Right_i+\lambda_{kc}+\lambda_{sc}+\nu_{it}$ (columns 1, 2, 4, 5) or

$\Delta Y_{it}=\gamma MinShare_i+\lambda_{kc}+\lambda_{sc}+\nu_{it}$ (columns 3 and 6) or

$\Delta Y_{it}=\gamma Woman_i+\lambda_{sc}+\nu_{it}$ (columns 7 and 8),

$\Delta Y_{it}=\gamma Share_i+\lambda_{sc}+\nu_{it}$ (columns 9 and 10),

where ΔY_{it} is the yearly change in Tobin's Q of firm *i* in year *t*, $Right_i$ is the dummy for being to the right of the kink, $MinShare_i$ is the predicted minimum share of firm *i* (the instruments, defined in Section 2), $Woman_i$ is the dummy for having at least one woman, $Share_i$ is the share of women, all as of the year before announcement, λ_{kc} are kink fixed effects (specific to the country), λ_{sc} are industry fixed effects (specific to the country). Standard errors are clustered at the firm level and are reported below the coefficients. Columns 1 to 6 restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). Columns 7 to 10 consider all firms. All regressions are estimated using all available data before announcement (up to 10 years before). The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

Tobin's Q	Instruments used in the paper						Not used			
	All countries			UK			All countries		UK	
	1	2	3	4	5	6	7	8	9	10
$Right_i$	-0.0398	0.0717		-0.0267	0.0859					
	(0.276)	(0.135)		(0.298)	(0.177)					
$MinShare_i$			0.473			0.496				
			(1.491)			(1.655)				
$Woman_i$							0.133**		0.153*	
							(0.0670)		(0.0881)	
$Share_i$								0.625**		0.688**
								(0.263)		(0.338)
Number of firms	371	784	784	291	457	457	1311	1311	850	850
Observations	2,314	5,533	5,533	1,832	3,290	3,290	9,441	9,441	6,241	6,241
Kink * Country FE	Yes	Yes	Yes	Yes	Yes	Yes				
Industry * Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample: Largest kink only	Yes			Yes						
Sample: All kinks		Yes	Yes		Yes	Yes				
Sample: All board sizes							Yes	Yes	Yes	Yes

Appendix Table 3. Cumulative Abnormal Returns and the Share of Women: Reduced-form Results

This table reports the results of estimating the following specification using the OLS framework:

$$AR_{it}=a+\gamma_1 D_1 Right_i+\gamma_2 D_2 Right_i+\ldots+\gamma_{10} D_{10} Right_i+\lambda_{kt}+\lambda_{st}+\lambda_i+\nu_{it}\ ,$$

where AR_{it} is the abnormal return (relative to 4-factor model of Carhart, 1997) of firm *i* in day *t*, $Right_i$ is the dummy for being to the right of the kink (as defined before), D_j are the indicator variables for each trading day j after announcement (where day 1 corresponds to the day of the announcement, February 24th, 2011), λ_{kt} are kink-day fixed effects, λ_i and λ_{st} are firm and industry-day fixed effects. The regression is estimated on the data from 6 weeks (30 trading days) before the announcement to 2 weeks (10 trading days) after the announcement. The coefficients below report CARs, up to each day j, i.e. $\gamma_1+\ldots+\gamma_j$ for each j. Panel A reports the results for the largest kink only, while Panel B considers all kinks. Standard errors are clustered at the industry level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons.
* indicates 10% significance; ** 5% significance; *** 1% significance.

	Cumulative Abnormal Returns, up to day j after announcement									
j =	1	2	3	4	5	6	7	8	9	10
	Panel A: Largest kink only									
$Right_i$	0.00350	-0.00332	0.00243	0.00454	0.0116	0.0160	0.0165	0.0155	0.0212	0.0264*
	(0.00605)	(0.00547)	(0.00817)	(0.00741)	(0.00854)	(0.0109)	(0.0102)	(0.0113)	(0.0134)	(0.0139)
Number of firms	112	112	112	112	112	112	112	112	112	112
	Panel B: All kinks									
$Right_i$	-0.00283	-0.00342	-0.000627	-0.00452	-0.00563	-0.000612	-0.000786	-0.00345	-0.00515	-0.00444
	(0.00229)	(0.00315)	(0.00404)	(0.00476)	(0.00519)	(0.00643)	(0.00690)	(0.00765)	(0.00896)	(0.00935)
Number of firms	249	249	249	249	249	249	249	249	249	249
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry * Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix Table 4. Risk Taking and the Share of Women: Reduced-form Results for the UK firms

This table reports the results of estimating the following specification using the OLS framework:

$$Beta_{it}=\gamma Post_{ct}Right_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+\nu_{it} ,$$

where $Beta_{it}$ is a beta with respect to the 4 factors (with a minimum of 100 days of non-missing observations within that year) of firm *i* in year *t*, $Post_t$ is the dummy variable that takes value of 1 from announcement year to three years afterwards, and zero - for the year before announcement and up to three years before that, $Right_i$ is the dummy for being to the right of the kink (the instrument, defined in Section 2), measured in the base year, λ_{kt} are kink-year fixed effects, λ_{st} are industry-year fixed effects, λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	Betas, with respect to the 4 factors, UK firms			
	Beta (Rmrf)	Beta (SMB)	Beta (HML)	Beta (Momentum)
	Panel A: Largest kink only			
$Post_{ct}$ * $Right_i$	0.106	0.108	-0.0844	0.113
	(0.114)	(0.126)	(0.217)	(0.0917)
Number of firms	149	149	149	149
Observations	697	697	697	697
	Panel B: All kinks			
$Post_{ct}$ * $Right_i$	0.0161	0.0306	-0.0713	0.0632*
	(0.0489)	(0.0587)	(0.0848)	(0.0359)
Number of firms	293	293	293	293
Observations	1,581	1,581	1,581	1,581
Kink * Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry * Year FE	Yes	Yes	Yes	Yes

Appendix Table 5. Board Size: Reduced-form Results

This table reports the results of estimating the following specification using the OLS framework:

$$Y_{it}=\gamma Post_{ct}Right_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+\nu_{it} \text{ (columns 1, 2) or}$$

$$Y_{it}=\gamma Post_{ct}MinShare_i+\lambda_{kct}+\lambda_{sct}+\lambda_i+\nu_{it} \text{ (column 3),}$$

where Y_{it} is Board size of firm *i* in year *t*, $Post_{ct}$ is the (country-specific) dummy variable that takes value of 1 from compliance year to three years afterwards, and zero - for the year before announcement and up to three years before that, $Right_i$ is the dummy for being to the right of the kink and $MinShare_i$ is the predicted minimum share of firm *i* (the instruments, defined in Section 2), measured in the base year, λ_{kct} are kink-year fixed effects (specific to the country), λ_{sct} are industry-year fixed effects (specific to the country), λ_i are firm fixed effects. The base year is the year before announcement. Standard errors are clustered at the firm level and are reported below the coefficients. All columns restrict sample to firms in the discontinuity sample, excluding the potentially non-affected firms (firms with $Share_{i0}$ above the quota). The number of firms and observations excludes singletons. * indicates 10% significance; ** 5% significance; *** 1% significance.

	Board Size		
	1	2	3
	Panel A: All countries		
$Post_{ct}$ * $Right_i$	-0.218	-0.383***	
	(0.153)	(0.138)	
$Post_{ct}$ * $MinShare_i$			-2.677**
			(1.041)
Number of firms	361	766	766
Observations	2,418	5,055	5,055
	Panel B: UK		
$Post_{ct}$ * $Right_i$	-0.214	-0.371***	
	(0.155)	(0.135)	
$Post_{ct}$ * $MinShare_i$			-2.480**
			(1.012)
Number of firms	281	445	445
Observations	1,931	3,064	3,064
Kink * Country * Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Industry * Country * Year FE	Yes	Yes	Yes
Sample: Largest kink only	Yes		
Sample: All kinks		Yes	Yes

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ZEW – Leibniz-Zentrum für Europäische Wirtschaftsforschung GmbH Mannheim

ZEW – Leibniz Centre for European Economic Research

L 7,1 · 68161 Mannheim · Germany
Phone +49 621 1235-01
info@zew.de · zew.de