Cracks in the boards: the opportunity cost of homogeneous boards of directors

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Abstract

Boards of directors' composition is closely related to firm inputs, output and financial returns. This paper uses the announcement and implementation of quotas in France as a quasi-experiment to test the effect of a shock on their composition. It moves beyond previous studies by establishing a clear causal link between governance change and firm total factor productivity (TFP) growth. It further contributes to the literature by disentangling between adjustment costs and benefits from new board members. The initial decline followed by a rise of the growth rate of TFP above pre-quota levels points to the positive impact of constrained change. It reflects the opportunity cost of homogeneous boards of directors for firms and the economy at large.

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1 Introduction

Amidst considerations about representation of women in the work-space, guidelines and quotas in board of directors have been put in place in many countries. As a strategic body responsible for decisions on long-term investments and supervision of senior management, its particularities should be reflected in the firm's characteristics. Until recently, boards of directors were mainly composed of a homogeneous group of white male seniors. To what extent could an exogenous shock on its composition alter the structure and performance of a firm?

The purpose of this paper is to quantify the impact of the 2011 announcement of a quota constraining certain firms to increase their share of women in boards of directors to 20% by 2014 and 40% by 2017 in France on their productivity growth.

Although many papers have identified the importance of this executive body for firm inputs/output (Matsa and Miller (2011), Matsa and Miller (2013) or Bertrand et al. (2019)) and financial returns (Ahern and Dittmar (2012), Adams and Ferreira (2009) or Erhardt et al. (2003)), none of them has established a clear causal link between an exogenous shock on its composition and variables of interest. Moreover, no consensus on the sign of the impact has emerged as previous works did not simultaneously identify and disentangle all the mechanisms at work.

This paper contributes to the literature in three ways. First of all, it uses a difference-in-discontinuities, a methodology widely used in political economy papers, to identify the causal impact of quotas in board of directors on firm productivity growth. Second of all, it takes advantage of the reform's time-frame to distinguish between an adjustment and a compliance period respectively from 2011 to 2013 and from 2014 to 2016. To the extent that the former relates to transition costs while the latter captures the static effect of a new type of governance, this paper hopes to separate two mechanisms with potentially opposite impacts on productivity growth. Lastly, this paper focuses on firms' economic efficiency rather than on individual proxies for firm performance and organizational change. This is in itself a contribution to the literature on corporate governance as productivity has large implications for firm dynamics and the economy as a whole.

Using information from the French National Statistical Institute (INSEE) on firms' balance sheet and their employees, I construct a panel dataset spanning the years 2001 to 2016. Consistent with the hypothesis on adjustment costs, I find that the growth rate of TFP (Total Factor Productivity) decreases by 10 to 16 percentage points during the 2011-2013 announcement period for firms targeted by the law. During the 2013-2016 compliance period it then increases by 23 to 30 percentage points while the growth rate of labour productivity increases by approximately 6 percentage points reflecting the positive impact of the new governance structure. The rise of the growth rate of TFP above pre-quota levels reflects the opportunity cost of homogeneous boards of directors for firms and the economy at large.

A wide literature in behavioural corporate governance has attempted to understand how the characteristics of individuals in a group matter for collective decision-making.
Several papers have shown that heterogeneity in skills within teams leads to higher productivity (Hamilton et al. (2003)) which on aggregate can positively affect a firm's performance (Iranzo et al. (2008)). Using a danish matched employer-employee data-set, Parrotta et al. (2012) show how diversity in education enhances a firm's value added. In the context of top executive bodies, Güner et al. (2008) find that financial experts in boards matter for investment and funding decisions while Kim and Starks (2016) show how higher firm value is achieved with a larger share of women as they bring unique skills to boards.
Other papers have focused on the relationship between preference heterogeneity, such as the degree of confidence or altruism, and risk-taking. Niederle and Vesterlund (2007) show that women exhibit less over-confidence and have a lower preference for competitive environments than men. This matters in a context of investment as individuals behave differently. Barber and Odean (2001) find that consistent with theoretical models on over-confidence and excessive trading men trade 45% more than women and see their return reduced by more than women. In another context, Andreoni and Vesterlund (2001) observe that women are kinder when altruism is expensive while men are kinder when it is cheap. Consistent with this finding, Apesteguia et al. (2012) observe less aggressive pricing strategies, less investment in R&D and more investment in social initiatives in only-women teams.
The extent of preference and skills heterogeneity in a group will matter for collective decision-making. Apesteguia et al. (2012), for instance, find that the best performing teams are those composed of two men and a woman. Consistent with this result, Hoogendoorn et al. (2013) show that teams with an equal gender mix perform better than men-dominated teams in terms of sales and profits. In a three person team, the most generous groups seem to be those composed of two women and a man (Dufwenberg and Muren (2006)). In a firm-setting, Erhardt et al. (2003) show that diversity in gender and minorities within boards of directors tend to be positively correlated with financial indicators that proxy for a firm's performance. In this context, however, it is difficult to go beyond marginal effects and identify the ideal level of diversity as most firms in the economy have a low share of women.

It is not clear through which mechanisms a change in the composition of boards should affect firm characteristics.
Beyond the potential direct effect on investment decisions of preference and skills heterogeneity, a channel through which team composition can affect performance comes from the change in culture and practices as discussed in Bertrand and Schoar (2003) and Bloom and Van Reenen (2007). Using a difference-in-differences method, Matsa and Miller (2013) note that the introduction of a 40% quota within boards of directors in Norway led firms to undertake fewer workforce reductions, thereby increasing relative labor costs and employment levels and reducing short-term profits. In another setting, Flabbi et al. (2016) show how firm performance measured by sales per worker increases under women leadership and even more

so under a higher share of women workers.
This last paper points to an interesting channel through which performance of a firm can be affected. The primary objective of quota laws in boards of directors have been to trigger an increase in the number of women within a firm's labour force. As discussed before, this can impact firm performance due to team diversity in the workplace. Using the introduction of the quota in Norway that was mentioned above, Bertrand et al. (2019) find no change in the composition of senior management and no trickle-down effect further down the hierarchy on wage and promotion gaps. Matsa and Miller (2011) perceive the opposite effect in corporations in the US. They show that firms with more women board members have a higher amount of women in top management positions.

Beyond the effect of a change in governance on firm characteristics, the issue of adjustment and initial firm structure are relevant.
Several papers have highlighted the fact that quotas induced transition costs and forced sub-optimal changes on the firms resulting in a negative performance in the short term. Using the introduction of the quota law in Norway, Ahern and Dittmar (2012) find that the increase in women in board of directors led to a lower average industry-adjusted stock return for firms that had no woman director. The authors take this finding as a proof of the sub-optimal maximization of shareholder value that occurred after the introduction of the quota.
Another set of recent papers have focused on the combination of firm characteristics with the type of boards. Although Adams and Ferreira (2009) note that gender diverse boards are more effective in firms with otherwise weak governance measured by their ability to resist takeovers, the reverse is true for firms with strong governance where shareholder value is reduced. This last paper points to an interesting aspect of firm heterogeneity in response to a shock.

This paper will be organized in the following way: Section 2 will present the institutional background and describe the data, section 3 will explain the empirical strategy and demonstrate its validity, section 4 will report the results, section 5 will discuss propositions to back the main findings and section 6 will conclude.

2 Institutional Background and Data

In this section, I will present the institutional background and describe the data as well as provide evidence on the first stage of the strategy.

2.1 The Quota

On the 27th January 2011, the French president promulgated a law imposing a quota on board membership (Conseil d'Administration) for firms meeting certain criteria. According to the law, those boards would need to have a share of 20 % of women by 2014 and 40% by 2017. Firms that fail to comply would have their corporate board nominations nullified as well as face severe financial penalties [1].

Firms falling under this law are those with a revenue of at least 50 million euros and with more than 500 employees in the past three years[2]. It is important to note that around this cutoff, firms are categorized as medium-sized. This classification corresponds to firms with employees ranging from 250 to 4,999 employees and revenues of up to 1, 500 M. euros. I will focus on this type of firms to avoid confounding factors.

The quota can be used as a quasi-experiment as the decision by the government to target those firms does not seem to correspond to any practical considerations and, as a result, can largely be considered as exogenous. As the parliamentary discussions started only in December 2010, it was not anticipated. Moreover, no other law targeting the same group of firms has been announced and/or implemented from 2011 onwards. This set of facts allows for a clean identification strategy of the treatment.

2.2 Evidence on the First Stage

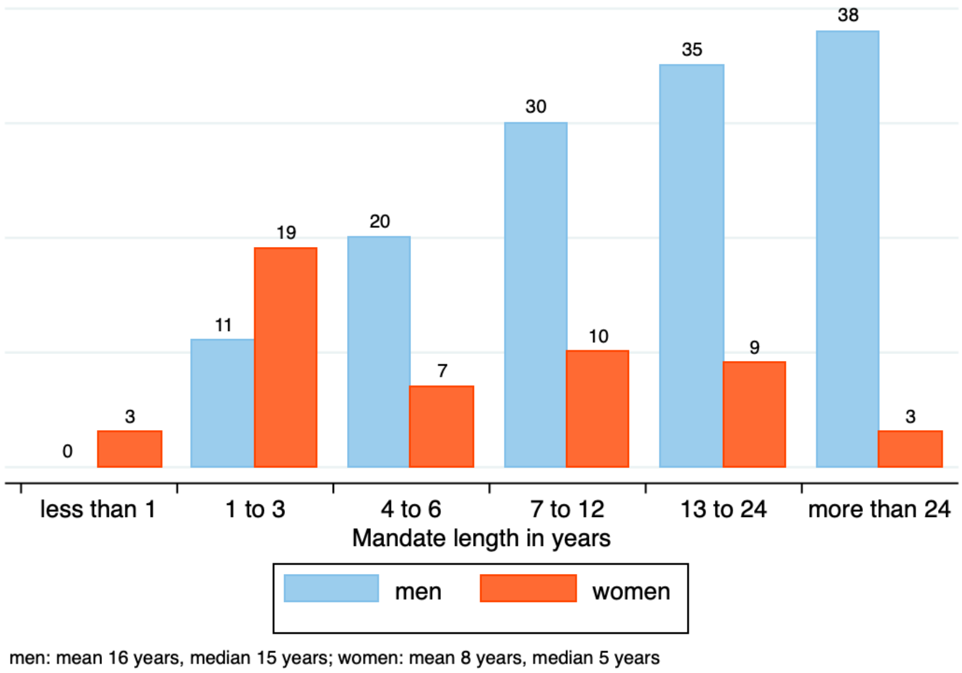
Before describing the data and empirical strategy used, it is important to establish the validity of the first stage as it is not uncommon for laws to induce no change in behaviour. Fortunately, there is evidence that the announcement of the law translated in an increase of the share of women in boards of directors. Figure 1 shows that women represented more than half of the nominations, 22 out of 33[3], between 2011-2015 in a sample of representative medium-sized firms.

[1] None of the directors in the board would get their salary until compliance is achieved.

[2] The past three years correspond to t, $t-1$ and $t-2$.

[3] To get this number, I add 3 nominations of women in 2014-2015 (moins de 1 an) and 19 nominations between 2011-2014 (de 1 à 3 ans).

Figure 1: Number of mandates per mandate length for medium-sized firms



Notes:
This figure shows the number of mandates per mandate length as of 2015; it is taken from a survey on medium-sized firms conducted by the French Institute of Administrators (IFA (2015)).

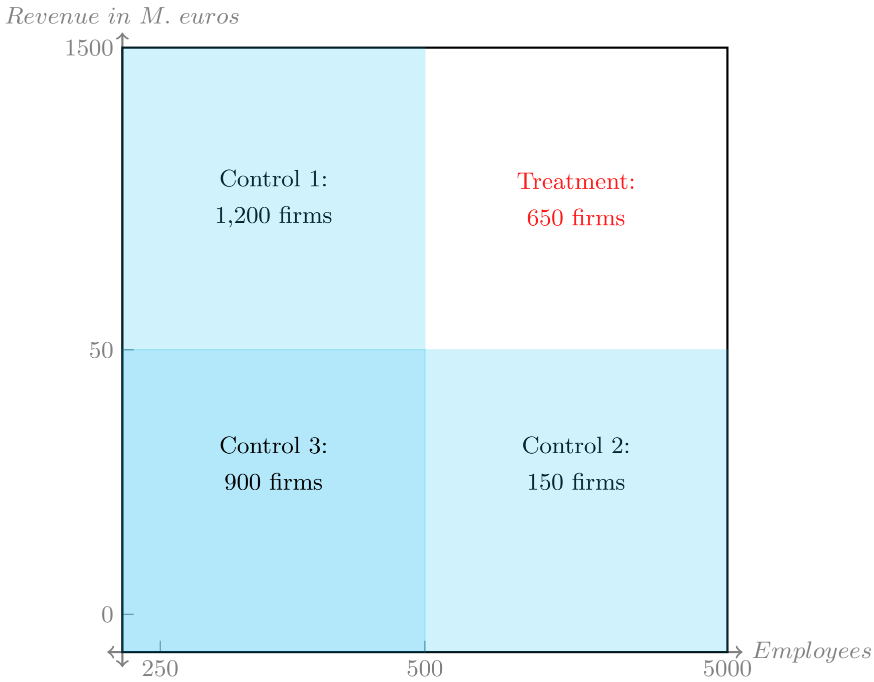
Although compliance will be taken for granted and treatment status will entirely be determined by whether firms should comply or not, the ultimate objective is to construct a comprehensive dataset on the composition of boards of directors for medium-sized firms. As such database is not readily available, I will combine information on the names, the gender and the age of members and the start and end dates of their mandates from Orbis (which has information on balance-sheet type variables and the corporate structure of private firms worldwide), Societe.com (a website which gives basic data such as the year of creation or revenue of all registered firms in France and, occasionally, their governance structure) and the BODACC (Bulletin Officiel des Annonces Civiles et Commerciales which compiles all the announcements of organizational changes within firms in France from 2008 onwards). Matching this dataset to the panel through the firms' identification number will enable me to confirm the first stage, track precisely the evolution of compliance rates and identify treatment status.

2.3 Data Description

I construct a panel dataset of approximately 2,900 private medium-sized firms in the manufacturing and services sectors from 2001 to 2016. Balance sheet information such as value added or cash flow is taken from FICUS and FARE while detailed information on employees such as their gender, professional category or gross/net salary is taken from the Déclaration Annuelle des Données Sociales (DADS) from the French National Statistical Institute (INSEE).

For the purpose of this paper, I create four groups of firms based on their revenue and employment in the past three years to reflect their treatment status. Figure 2 gives a visual of this classification across the two dimensions.

Figure 2: Classification of Firms across Treatment Status



Notes:
Treatment corresponds to firms with 50 to 1,500 M. euros in revenue at time t and 500 to 4,999 employees at t, $t-1$ and $t-2$; Control 1 corresponds to firms with 50 to 1,500 M. euros in revenue at time t and 250 to 499 employees at t, $t-1$ and $t-2$; Control 2 corresponds to firms with 0 to 49 M. euros in revenue at time t and 500 to 4,999 employees at t, $t-1$ and $t-2$; Control 3 corresponds to firms with 0 to 49 M. euros in revenue at time t and 250 to 499 employees at t, $t-1$ and $t-2$.

When the dataset on the composition of boards of directors will be constructed, it will be matched to the panel data. As Orbis has some indication on the type and level of education of board members, it will be possible to augment the data with an individual skills dimension.

3 Empirical Strategy

In this section, I will discuss the methodology used and provide evidence for its validity as well as explain the baseline specification.

3.1 A Difference-in-Discontinuities Design

In order to identify the treatment effect of the announcement and implementation of the quota, I implement an estimator that exploits both the discontinuity at the threshold of compliance and the time variation from the two events.

The first part of this strategy corresponds to a Regression Discontinuity Design. The RDD's main assumption is that units that have very similar values on the right and left of the threshold are comparable in everything apart from their treatment status. In this paper, firms that are in the treatment or control 2 groups (see Figure 2) but very close to the 50 M. euros in revenue cutoff, c, are regarded as similar (further discussion on the optimal choice of the window around the threshold will be given in 4.1). Formally, and since we assume that the probability of receiving the treatment changes suddenly at the threshold of compliance, we have the following treatment status, D_i:

$$D_i = \begin{cases} 1, & \text{if } \mathrm{X}_i \geq c \\ 0, & \text{if } \mathrm{X}_i < c \end{cases}$$

with the following average treatment effect:

$$\beta_{RDD} = E(Y_i(1) - Y_i(0)|X_i = c) = \lim_{x \downarrow c} E(Y_i|X_i = x) - \lim_{x \uparrow c} E(Y_i|X_i = x)$$

The second part of this strategy corresponds to a Difference-in-Differences design. The DiD's main assumption is that the treated and control groups were similar before the event studied. In this case, the time component would correspond to either of the two events (the announcement or the implementation) and the average treatment effect would simply compare outcomes in treatment and control groups as a whole:

$$\beta_{DID} = [E(Y_{idt}|d = 1, t = 1) - E(Y_{idt}|d = 1, t = 0)] - [E(Y_{idt}|d = 0, t = 1) - E(Y_{idt}|d = 0, t = 0)]$$

where d is a dummy variable equal to 1 for the treatment group and t is a dummy variable equal to 1 for years greater or equal to 2011 or 2014.

Taken together, the RDD and the DiD lead to a Difference-in-Discontinuities design which has both a time and a discontinuity component. In this case, using the treatment definition of the RDD, D_i, and the event definition of the DiD, t, we get the following average treatment effect:

$$\beta_{DDISC} = [\lim_{x \downarrow c} E(Y_{it}|X_{it} = x, t = 1) - \lim_{x \uparrow c} E(Y_{it}|X_{it} = x, t = 1)]$$
$$-[\lim_{x \downarrow c} E(Y_{it}|X_{it} = x, t = 0) - \lim_{x \uparrow c} E(Y_{it}|X_{it} = x, t = 0)]$$

Instead of average outcomes for all treated and control firms to be compared before and after the event as in a standard DiD, a set of firms to the right and to the left of the cutoff within the treatment and control groups are optimally selected. As such, firms that are close to the threshold are very similar and their treatment status is quasi-random.

3.2 Validity of the Design

Before moving on to the baseline specification and main results, it is important to test the validity of the difference-in-discontinuities (diff-in-disc).

As in a standard RDD framework, there can be no manipulation of the running variable which, in this case, corresponds to either the amount of revenue in any given year and/or the amount of employees in the past three years. The MCcrary does that by testing the null hypothesis of a discontinuity in the density of firms around the threshold for both variables (McCrary (2008)). This condition is necessary as a manipulation would indicate that some firms managed to self-select themselves out of treatment. Table 1 shows that we fail to reject the null hypothesis of no difference in the density of treated and control observations at the threshold.

Table 1: McCrary test for each threshold

	(1)			(2)	
	Employment (L)			Revenue (R)	
	T	P $\geq$| T |		T	P $\geq$| T |
Conditions			**Conditions**		
	0.77	0.44		0.16	0.87
$R \geq 50$	0.69	0.49	$L_{t,t-1,t-2} \geq 500$	0.91	0.36
$R \geq 50$ & $T \geq 2011$	0.83	0.41	$L_{t,t-1,t-2} \geq 500$ & $T \geq 2011$	1.55	0.12

Notes:
All the coefficients displayed use clustered-robust standard errors. The first row of column (1) tests the null hypothesis of a discontinuity in the density of firms around 500 employees while that of column (2) tests it around 50 M. euros in revenues. The second and third rows add conditions on the amount of revenue or employees and/or the year to reflect the absence of manipulation across all specifications. For the tests of density around 500 employees (all rows in column (1)), a condition on at least 500 employees in the past two years is set to reflect the law accurately. For the test of density around 50 M. euros in revenue (second and third row in column(2)), the condition on employment being at least 500 is done on the past three years.

Another test to conduct in order for the strategy to be used neatly relates to the condition on parallel trends and similarity in covariates. Table 2 shows the mean of key variables of interest prior to the announcement of the quota. Firms in control groups 2 and 3 seem to be slightly younger. However, there are no striking differences in total factor productivity (TFP, which I will define in 4.1), labour productivity or value added.

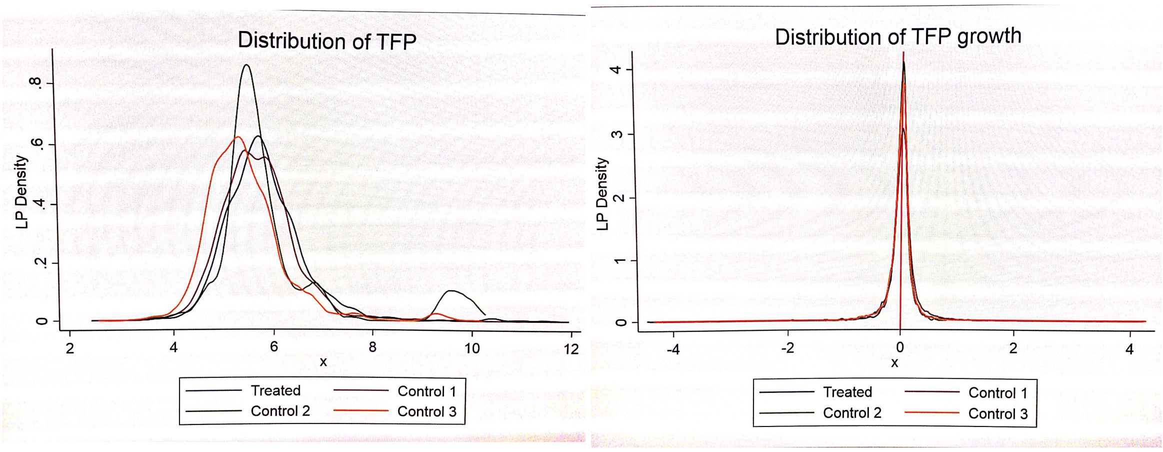
Table 2: Descriptive Statistics for each group

Group	**TFP**	**Labour Prod.**	**Value added**	**Age**	**Observations**
Treatment	7.23	3.98	10.60	28	5,918
Control 1	7.08	4.17	10.01	28	11,040
Control 2	7.10	3.55	10.11	24	1,429
Control 3	6.61	3.87	9.64	25	8,207
Total	6.97	4.00	10.03	27	26,594

Notes:
This table gives the mean value of key firm variables per group status before 2011. Treatment corresponds to firms with 50 to 1,500 M. euros in revenue at time t and 500 to 4,999 employees at t, $t-1$ and $t-2$; Control 1 corresponds to firms with 50 to 1,500 M. euros in revenue at time t and 250 to 499 employees at t, $t-1$ and $t-2$; Control 2 corresponds to firms with 0 to 49 M. euros in revenue at time t and 500 to 4,999 employees at t, $t-1$ and $t-2$; Control 3 corresponds to firms with 0 to 49 M. euros in revenue at time t and 250 to 499 employees at t, $t-1$ and $t-2$. (see Figure 2). Labour productivity is defined in a standard way as $\frac{value\ added}{\#Employees}$. TFP is calculated à la Foster et al. (2001) and Baily et al. (1992) as explained in 4.1.

Another proof of the lack of difference in outcomes before 2011 is shown in figures 3a and 3b which respectively plot the distribution of TFP and its growth rate. Although there are some variations in the distributions, there is clearly a common support for TFP.

Figure 3: TFP and its growth rate before 2011



(a) TFP

(b) TFP growth

Notes:
The two figures plot the distribution of TFP and its growth rate per treatment status before 2011. TFP is calculated à la Levinsohn and Petrin (2003) as explained in 4.1. The same figures for TFP à la Foster et al. (2001) and Baily et al. (1992) is available in the appendix A.1

3.3 Baseline Specification

The Diff-in-Disc relies on a set of parameters that can affect the accuracy of the estimator. As a result, the baseline specification will always be accompanied by variants of it in the appendix.

As the regression tests for a discontinuity by fitting a polynomial to the right and to the left of the cutoff, it is important to make sure that the results are robust to polynomials of different orders. The baseline specification will be a local linear regression[4] as it is the most optimal one (Porter (2003)). However, results with a polynomial of order 0 will be reported in the appendix.

The choice on the window of values to the right and to the left of the cutoff matters as it can lead to biased confidence intervals. Imbens and Kalyanaraman (2012) and Calonico et al. (2014) have proposed a data-dependent way of choosing the optimal bandwidth whereby the mean squared error is minimized and robust standard errors are reported. As they differ in their estimations of the variance and bias, I will use the former for the baseline specification and will provide results with the latter in the appendix.

The last parameter that can affect the results of the regression relates to the choice of the kernel as each type weighs observations within the bandwidth differently. A triangular one will be used for the baseline specification while an epanechnikov one will be used in the appendix.

Although the law has two thresholds, the discontinuity will be tested only around the revenue

[4] A polynomial of order 1

threshold with a condition on employment of at least 500 due to data limitations.
Indeed, in a given year, only 1 to 5 % of firms with more than 500 employees transition to less than 500 employees the following year. As the law stipulates that the condition occurs on the past three years of employment, the accurate way of testing for the discontinuity would be to condition on the level of employment in the previous two years. This would require a comparison between firms that had at least 500 employees for the past three years with those that had at least 500 employees for the past two years but switched to less than 500 during the last year. The amount of firms that transition between the two levels of employment is, however, too small to yield sufficient statistical power.
In order to deal with this limitation and confirm the impact of the law, the discontinuity will be tested around the revenue threshold conditioning on less than 500 employees in addition to being tested for those firms that have more than 500 employees. In practice, this amounts to using firms in the control groups 1 and 3 (see Figure 2) as placebos. If the diff-in-disc estimator is statistically significant for the real specification while it is not for the one using group 1 as a fake treatment group, then we can conclude that the effect comes solely from the law.

4 Empirical Results

In this section, I will report the results along with their robustness checks.

4.1 Main Results

To estimate the causal impact of the announcement and implementation of the law on treated firms, I run the following regression:

$$\Delta Y_{i,t} = \delta_0 + \delta_1(X^*_{i,t}) + D_i[\gamma_0 + \gamma_1(X^*_{i,t})] + T_t[\alpha_0 + \alpha_1(X^*_{i,t})$$
$$+D_i(\beta_{DDISC} + \beta_1(X^*_{i,t}))] + \psi_0 ln(Y_{i,t-1}) + \psi_1 ln(L_{i,t}) + v_{s,t} + \epsilon_{i,t}$$

with

$$X^*_{i,t} = (X_{i,t} - c)$$

where $\Delta Y_{i,t}$ is the firm i's TFP growth at t, $X_{i,t}$ is the level of revenue in euros of firm i, c is the 50 M. euros threshold, D_i is a dummy equal to 1 if $X_{i,t} \geq c$, T_t is a dummy equal to 1 if the year is at least 2011 or 2014 (depending on the estimation), $ln(Y_{i,t-1})$ is the log level of TFP at $t-1$, $ln(L_{i,t})$ is the log level of employment at t and $v_{s,t}$ is a sector-year fixed effect. β_{DDISC} is the Diff-in-Disc estimator that corresponds to time, T_t, and group, D_i, treatment.
The log level of TFP at $t-1$ is included to capture natural catching-up dynamics whereby firms that have a lower productivity tend to grow faster. It is particularly relevant to include it in this regression as the size of a firm, which constitutes the threshold, is related to its

level of TFP.

TFP is defined in two alternative ways.
The first one is a simple decomposition of the value added between the contribution of labour and capital to value added and its residual à la Foster et al. (2001) and Baily et al. (1992):

$$ln(Y_{i,t}) = ln(Q_{i,t}) - \alpha_K ln(K_{i,t}) - \alpha_L ln(L_{i,t}) - \alpha_M ln(M_{i,t})$$

where $ln(Y_{i,t})$, $ln(Q_{i,t})$, $ln(K_{i,t})$, $ln(L_{i,t})$ and $ln(M_{i,t})$ are, respectively, the log levels of TFP, value added, capital, labour and materials and the α's their elasticities to value added. It will be referred to as TFP (1) throughout.

The second definition of TFP follows Levinsohn and Petrin (2003) and controls for the correlation between input levels and unobserved firm-specific productivity processes by using intermediate inputs as a proxy. The first stage regression writes the production function as the log of inputs and shocks in the following way:

$$y_t = \beta_l l_t + \phi_t(i_t, k_t) + \eta_t$$

where

$$\phi_t(i_t, k_t) = \beta_0 + \beta_k k_t + \beta_i i_t + \omega_t(i_t, k_t)$$

with $\omega_t(i_t, k_t)$ the inverse of the input demand function.
After obtaining a consistent estimator for labour β_l, the second stage regression is run:

$$y_t^* = y_t - \beta_l l_t = \beta_0 + \beta_k k_t + \beta_i i_t + E(\omega_t|\omega_{t-1})$$

β_k and β_i, respectively the capital and intermediate input coefficients, can be consistently estimated as the shocks $\eta_t^* = \eta_t + \epsilon_t$ are uncorrelated with k_t and i_{t-1}. It will be referred to as TFP (2) throughout. In both cases, TFP growth is trimmed of its top and bottom 1% values in order to reduce biasedness from extreme results.

The regression will be tested on 2011 and 2014 separately to identify the announcement and compliance effects of the law.[5]
The regression is first ran with 2011 as the timing of the event. Table 3 reports Diff-in-Disc estimators for firms around the revenue threshold with at least 500 employees in the past three years (Treatment and control groups 2 in Figure 2) using TFP(1) (column (1)) and TFP(2) (column (2)) as the dependent variable.

[5]The regressions are ran using a code developed in Matta et al. (2016).

Table 3: Impact of the announcement of the law on TFP growth

	Δ TFP(1)	Δ TFP(2)
Procedure		
Conventional	-0.10**	-0.13**
	(0.05)	(0.06)
Bias-corrected	-0.12***	-0.16***
	(0.05)	(0.06)
Robust	-0.12*	-0.16**
	(0.07)	(0.07)
Observations	3,276	3,276

Notes :
The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having at least 500 employees in the past three years and use 2011 as the event. They are ran on the 2001-2013 time-frame in order to estimate the impact of the announcement period separately from the compliance one. The dependent variable in column (1) is the growth rate of TFP à la Foster et al. (2001) and Baily et al. (1992). The dependent variable in column (2) is the growth rate of TFP à la Levinsohn and Petrin (2003). The estimators for all the procedures and its standard errors are reported. The conventional procedure reports conventional estimates and their conventional standard errors. The bias-corrected procedure reports bias-corrected estimates and their conventional standard errors. The robust procedure reports bias-corrected estimates and their robust standard errors. The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

The announcement of the law induces a significant drop of 10 to 16 percentage points, depending on the specification, in the growth rate of TFP for treated firms relative to those that are not affected by it. Recall that this result controls for $lnY_{i,t-1}$ and, as such, reflects a decline in the growth dynamic rather than a drop in the productivity level. The fact that the coefficients in the two columns are similar proves the insignificance of mismeasurement errors that could arise with the use of a particular definition of TFP. It confirms the sign and size of the outcome. The results are robust to running the same regression with a polynomial of order 0, the Imbens and Kalyanaraman (2012) optimal bandwidth or the epanechnikov kernel (see B.1).

As announced in 3.3, a placebo discontinuity is tested on control groups 2 and 3 (see Figure 2) in table 4. The fact that the significance of the coefficients disappears attests of the law having only affected treated firms.

Table 4: Impact of the announcement of the law on TFP growth

	Δ TFP(1)	**Δ TFP(2)**
Procedure		
Conventional	-0.04	-0.02
	(0.04)	(0.02)
Bias-corrected	-0.05	-0.03
	(0.04)	(0.02)
Robust	-0.05	-0.03
	(0.05)	(0.03)
Observations	8,698	8,698

Notes :
The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having less than 500 employees in the past three years and use 2011 as the event. They are ran on the 2001-2013 time-frame in order to estimate the impact of the announcement period separately from the compliance one. The dependent variable in column (1) is the growth rate of TFP à la Foster et al. (2001) and Baily et al. (1992). The dependent variable in column (2) is the growth rate of TFP à la Levinsohn and Petrin (2003). The estimators for all the procedures and its standard errors are reported. The conventional procedure reports conventional estimates and their conventional standard errors. The bias-corrected procedure reports bias-corrected estimates and their conventional standard errors. The robust procedure reports bias-corrected estimates and their robust standard errors. The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

The observed drop in the growth rate of TFP is largely consistent with Ahern and Dittmar (2012) who noted a decline in performance after the quota law in Norway. It could be related to adjustment issues which encompass uncertainty, search costs or sub-optimal board composition resulting from new inexperienced board members. As the regressions are ran until 2013, the adjustment period is separated from the year when treated firms should be fully compliant with the law. Testing the same regression with 2014 as the timing of the event should shed light on the extent to which the drop in the growth rate of TFP reflects a transition period or a long-term decline in firm performance.

Using 2014 as the timing of the event, table 5 reports Diff-in-Disc estimators for firms around the revenue threshold with at least 500 employees in the past three years (Treatment and control groups 2 in Figure 2) and TFP(1) (column (1)) or TFP(2) (column (2)) as the dependent variable.

Table 5: IMPACT OF COMPLIANCE WITH THE LAW ON TFP GROWTH

	Δ TFP(1)	Δ TFP(2)
Procedure		
Conventional	0.23**	0.24**
	(0.10)	(0.11)
Bias-corrected	0.28***	0.30***
	(0.10)	(0.11)
Robust	0.28**	0.30**
	(0.13)	(0.14)
Observations	1,483	1,483

Notes :
The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having at least 500 employees in the past three years and use 2014 as the event. They are ran on the 2011-2016 time-frame in order to estimate the impact of the compliance period separately from the announcement one. The dependent variable in column (1) is the growth rate of TFP à la Foster et al. (2001) and Baily et al. (1992). The dependent variable in column (2) is the growth rate of TFP à la Levinsohn and Petrin (2003). The estimators for all the procedures and its standard errors are reported. The conventional procedure reports conventional estimates and their conventional standard errors. The bias-corrected reports bias-corrected estimates and their conventional standard errors. The robust procedure reports bias-corrected procedure estimates and their robust standard errors. The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

Compliance with the law in 2014 induces a significant increase of 23 to 30 percentage points in the growth rate of TFP for treated firms relative to those that are not affected by it. The growth dynamic seems to be indicating more than a recovery from the loss of TFP growth that occurred in the announcement period. Again, this is confirmed by the fact that the coefficients in the two columns are similar and that the results are robust to running the regression with the polynomial, bandwidth or kernel variants mentioned beforehand (see B.2).[6]

Testing for a placebo discontinuity on control groups 2 and 3 (see Figure 2) in table 6 makes the coefficients insignificant and confirms the pure effect of the law on treated firms.

[6] As a reminder, the regression is ran with a polynomial of order 0, the Imbens and Kalyanaraman (2012) optimal bandwidth or the epanechnikov kernel as a robustness check.

Table 6: Impact of compliance with the law on TFP growth

	Δ **TFP(1)**	Δ **TFP(2)**
Procedure		
Conventional	-0.03	0.01
	(0.02)	(0.02)
Bias-corrected	-0.02	0.02
	(0.0.02)	(0.02)
Robust	-0.02	0.02
	(0.04)	(0.03)
Observations	4,047	4,047

Notes :
The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having less than 500 employees in the past three years and use 2014 as the event. They are ran on the 2011-2016 time-frame in order to estimate the impact of the compliance period separately from the announcement one. The dependent variable in column (1) is the growth rate of TFP à la Foster et al. (2001) and Baily et al. (1992). The dependent variable in column (2) is the growth rate of TFP à la Levinsohn and Petrin (2003). The estimators for all the procedures and its standard errors are reported. The conventional procedure reports conventional estimates and their conventional standard errors. The bias-corrected procedure reports bias-corrected estimates and their conventional standard errors. The robust procedure reports bias-corrected estimates and their robust standard errors. The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

The fact that the increase in the growth rate of TFP due to the law, 23 to 30 percentage points, is larger than its preceding decline, 10 to 16 percentage points, is informative of the type of costs and potential channels through which the law affects firms. The change in the sign of the effect arising with the new governance structure could indicate a learning process from new board members who catch up with the level of experience of previous directors. This would confirm Ahern and Dittmar (2012)'s claim that quotas induce sub-optimal changes only in the short-run. Under this hypothesis and the assumption that the new board members cannot surpass the experience of previous ones, however, we would expect the growth rate of TFP to at most converge to pre-quota levels.

An alternative or accompanying explanation for the observed increase in the growth dynamic of firms could arise from the newcomers' contribution to the skills pool within the board. If the new directors bring unique skills to the board, as in Kim and Starks (2016), their potential benefit to the firm should not be immediate. We can expect that during the announcement period, adjustment costs are larger than the positive impact from, for instance, one more women in the board. However, by the time the new skills represent a sufficiently large share of the executive body, they could lead to a significant change in the strategic and hiring decisions of the firm. Research on the characteristics of board members in France tends to support this view (Bender et al. (2016)). Women seem to have significantly more expertise in human resources and communication as well as have more experience in firm settings (they are themselves managers, directors or CEOs).

The fact that compliance actually corresponds to a second adjustment period to meet the 40% share requirement by 2017 would tend to confirm the hypothesis of a break-even point. Beyond a 20% share of women in boards of directors, benefits could be outweighing costs systematically. The construction of the dataset on the composition and characteristics of boards of directors will allow to back or refute those propositions empirically. The strategy to test them will be further discussed (see 5).

4.2 Robustness checks

A set of robustness checks are conducted to address potential concerns and support the validity of the results.

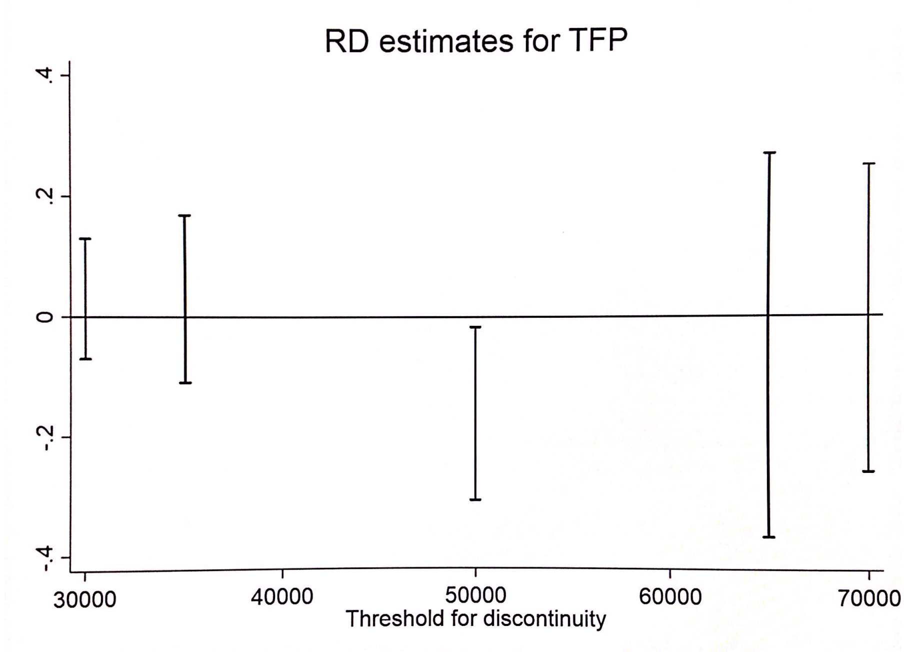
In order to make sure that the discontinuity only appears at 50 M. euros in revenue, it is tested with placebo thresholds close to the true cut-off. For the sake of clarity, the coefficients' confidence intervals are plotted in figures 4 and 5.

Figure 4 runs the main specification with 2011 as the timing of the event and the 500 employment condition for the past three years with placebo thresholds at 30, 35, 65 and 70 M. euros in revenue. The true diff-ind-disc using 50 M. euros as the threshold is plotted as well to provide a compelling visual comparison.

In order to avoid capturing the effect of the real discontinuity around the 50 M. euros threshold, any values above ore below that is removed. The discontinuity at the 35 M. euros threshold, for instance, is tested on values ranging from 0 to just below 50 M. euros while that at 65 M. euros is tested on values ranging from just above 50 to 1,500 M. euros.

Although this enables to have clear effects at each threshold, it makes it impossible to test placebo discontinuities even closer to the real one such as at 40, 45, 55 or 60 M. euros. There are not enough observations to the right or to the left to construct an optimal data-driven bandwidth.

Figure 4: Impact of the announcement of the law per threshold of discontinuity

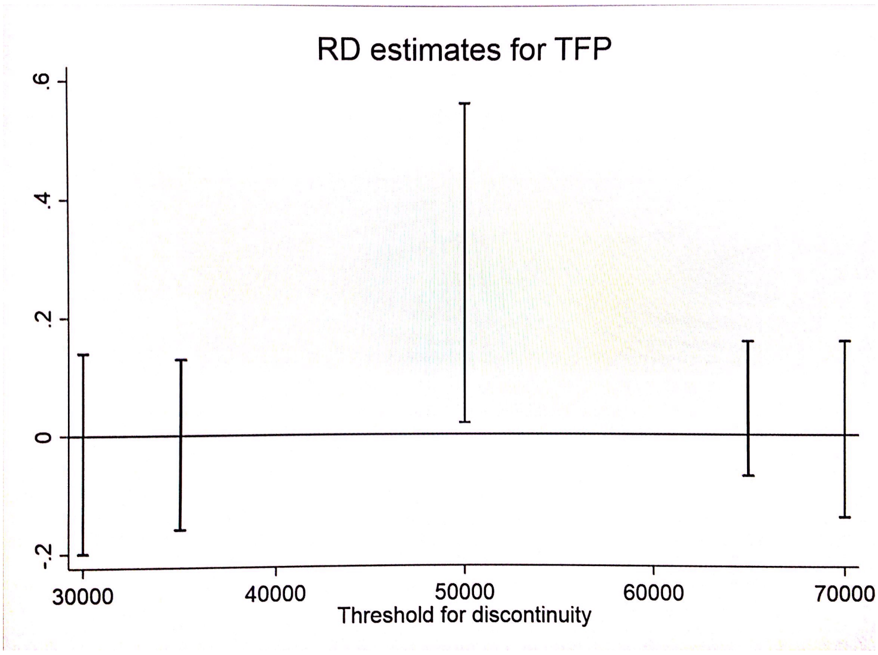


Notes:
The figure plots the confidence intervals (CI) of coefficients obtained from the robust procedure where standard errors are robust. Each CI corresponds to a regression using either 30, 35, 50, 65 or 70 M. euros as the cut-off. For the 30 and 35 M. euros placebo cut-offs, any observation with at least 50 M. in revenue is removed while for the 65 and 70 M. euros cut-offs, any observation with a 50 M. or lower level of revenue is removed. The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having more than 500 employees in the past three years and use 2011 as the event. They are ran on the 2001-2013 time-frame in order to estimate the impact of the announcement period separately from the compliance one. The dependent variable is the growth rate of TFP à la Levinsohn and Petrin (2003). The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

Nevertheless, the figure proves the validity of the results discussed in 4.1. The Diff-in-Disc estimator is only significantly different from zero at the real threshold.

Figure 5 reiterates the robustness check with 2014 as the timing of the event. Again, the only Diff-in-Disc estimator that is significantly different from zero uses 50 M. euros in revenue as its threshold.

Figure 5: Impact of compliance with the law per threshold of discontinuity



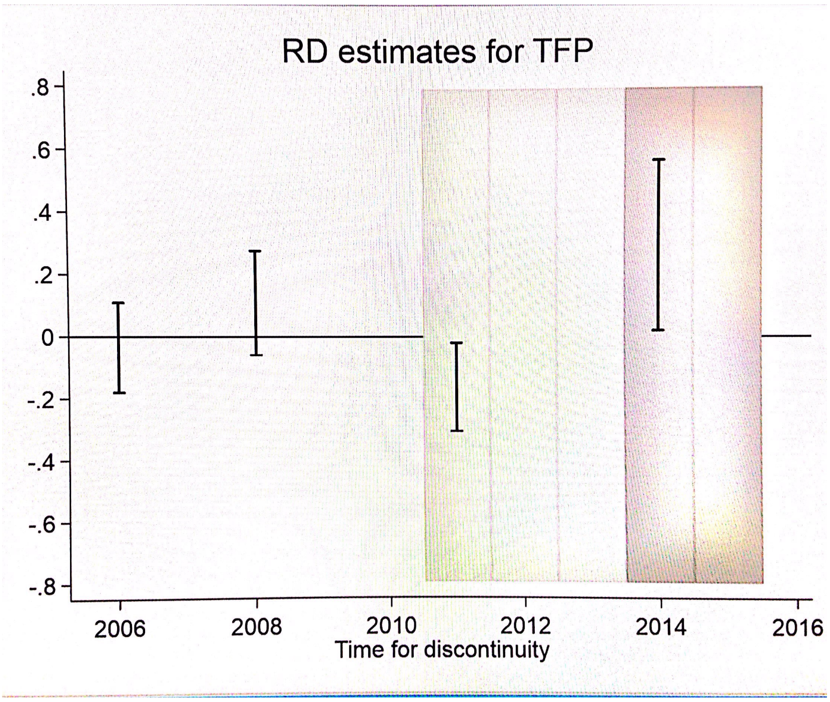
Notes:
The figure plots the confidence intervals (CI) of coefficients obtained from the robust procedure where standard errors are robust. Each CI corresponds to a regression using either 30, 35, 50, 65 or 70 M. euros as the cut-off. For the 30 and 35 M. euros placebo cut-offs, any observation with at least 50 M. in revenue is removed while for the 65 and 70 M. euros cut-offs, any observation with a 50 M. or lower level of revenue is removed. The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having more than 500 employees in the past three years and use 2014 as the event. They are ran on the 2011-2016 time-frame in order to estimate the impact of the compliance period separately from the announcement one. The dependent variable is the growth rate of TFP à la Levinsohn and Petrin (2003). The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

The same figures are reproduced using the alternative definition of TFP as its outcome in the appendix (see C.1 and C.2).

In the same spirit, the discontinuity is tested using placebo years for the timing of the event. The coefficients' confidence intervals are plotted in figure 6 after running the main specification with 50 M. euros in revenue as the threshold and the 500 employment condition for the past three years with placebo years in 2006 and 2008. The true diff-in-discs with 2011 and 2014 as the event years are plotted as well.
In order to avoid capturing the effect of the announcement and compliance with the law, observations after 2010 are removed when running the placebo regressions. Although observations are available from 2001 onwards, the employment condition automatically restricts the regression to years above 2003. The first placebo for which there are enough pre- and post-treatment years is, therefore, 2006. For the same practical reasons, as observations after 2010 are removed, the closest placebo to the 2011 event that we can test is 2008.
The figure confirms the absence of pre-trends in the growth rate of TFP between treated

and untreated firms as well as providing causal evidence of the effect of the quota.

Figure 6: Impact of the event



Notes:
The figure plots the confidence intervals (CI) of coefficients obtained from the robust procedure where standard errors are robust. Each CI corresponds to a regression using either 2006, 2008, 2011 or 2014 as the event. For the two placebo years, the regressions are ran on the 2001-2010 time-frame. The regression using 2011 as the event is ran on the 2001-2013 time-frame while that using 2014 is ran on the 2011-2016 period. The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having more than 500 employees in the past three years and use 50 M. euros in revenue as the cut-off. The dependent variable is the growth rate of TFP à la Levinsohn and Petrin (2003). The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

The same figure is reproduced using the alternative definition of TFP as its outcome in the appendix (see C.3).

4.3 Additional Results

As a measure of economic efficiency, TFP has large implications for firm dynamics and the economy. Variations in its level and/or growth rate can induce substantial changes in the market. On its own, however, it gives little information on behavioural and structural changes that occur within firms. Other variables might provide an insight on the ways the quota law affected their organization. They might also contribute to understanding the reasons for the observed responses of TFP. For this purpose, the impact of the law on inputs, output, alternative measures of productivity and investment are presented in tables 7 and 8.

Table 7 reports diff-in-disc estimators for a set of firm variables using 2011 as the timing of the event and 50 M. euros in revenue as the cut-off. The regressions are ran on firms with less (control groups 2 and 3 in column (1)) and more (treatment and control groups 1 in column (2)) than 500 employees for the past three years.

Table 7: Impact of the announcement of the law on firm variables

	(1)	(2)
Dependent variable		
Δ Value added	-0.01	-0.08
	(0.03)	(0.07)
Δ Employment	0.01	0.01
	(0.02)	(0.04)
Δ Labour Productivity	-0.02	-0.06
	(0.04)	(0.04)
Δ Capital	0.05	0.11
	(0.26)	(0.13)
Δ Capital Productivity	0.14	-0.21
	(0.06)	(0.14)
Δ Working Capital	-0.07	0.49
	(0.22)	(0.86)
Δ Investment Rate	-0.01	0.21
	(0.06)	(0.14)
Observations	8,698	3,276

Notes :
The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) and use 2011 as the event. They are ran on the 2001-2013 time-frame in order to estimate the impact of the announcement period separately from the compliance one. The regression in column (1) is ran conditional on having less than 500 employees in the past three years while that in column (2) is ran conditional on having more than 500 employees in the past three years. All the dependent variables are expressed as growth rates. Labour productivity is defined in a standard way as $\frac{value\ added}{\#Employees}$. Capital productivity is defined as $\frac{value\ added}{\#capital}$ where capital is the amount of tangible assets. Working capital is defined as $\frac{assets}{liabilities}$. Investment rate is defined as $\frac{tangible\ assets}{value\ added}$. Bias-corrected estimates and their robust standard errors are reported. The regressions for employment and labour productivity are ran with sector-year fixed effects. The others are ran with both $ln(L_{i,t})$ controls and sector-year fixed effects.

Consistent with the hypothesis of uncertainty and adjustment costs where the new mem-

bers do not alter the board composition and its decision-making immediately, no significant change occurs in the structure and investments of treated firms.
Value added, labour and capital productivity decline insignificantly while employment and capital increase insignificantly. The announcement of the law rather acts as a small-scale shock on the firm. Without digging deeper into the reasons for changes in firm variables as they are not clear-cut, the fact that value added declines and inputs increase is relevant for the previous discussion on TFP in 4.1. Indeed, as it is roughly speaking a residual of output minus inputs, a insignificant decline in the former and increase in the latter mechanically explains the significant drop in its growth rate of 10 to 16 percentage points.
The absence of a significant change in working capital which is a measure of liquidity, operational efficiency and financial health, or the investment rate tends to confirm the hypothesis of the absence of an immediate impact of the law's announcement on firms' strategy.

Table 8 reiterates the results using 2014 as the timing of the event. While no changes were apparent during the announcement period, labour productivity growth increases significantly by approximately 6 percentage points for treated firms in the compliance one [7].
As before, the significant increase in the growth rate of TFP of 23 to 30 percentage points can be explained mechanically by an insignificant increase in value added and decrease in inputs. The puzzle of its rise above pre-announcement rates could, however, partially be explained by the response of labour productivity to the new board composition.
As no significant change in the firms' investment strategy is observed, the impact of the law could be embedded in a shift in management practices. The increase in labour productivity could, for instance, be due to the new board better matching managers with tasks and/or replacing them. This would be both consistent with women board members having more management-related skills and the importance of management practices for productivity. Although Bloom et al. (2019) do not delve into issues of corporate governance, they find that better management practices induce higher TFP and account for around 18% of its variation across firms.

[7] Although the table reports only bias-corrected estimates and their robust standard errors, similar results are found when using the conventional and bias-corrected procedures.

Table 8: Impact of compliance with the law on firm variables

	(1)	(2)
Dependent variable		
Δ Value added	0.01	0.01
	(0.06)	(0.09)
Δ Employment	-0.02	-0.04
	(0.02)	(0.06)
Δ Labour Productivity	-0.01	0.06*
	(0.02)	(0.04)
Δ Capital	-0.08	-0.15
	(0.06)	(0.16)
Δ Capital Productivity	0.10	0.26
	(0.08)	(0.18)
Δ Working Capital	-0.24	-0.42
	(0.15)	(0.37)
Δ Investment Rate	-0.10	-0.26
	(0.08)	(0.18)
Observations	4,047	1,483

Notes :
The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) and use 2011 as the event. They are ran on the 2011-2014 time-frame in order to estimate the impact of the compliance period separately from the announcement one. The regression in column (1) is ran conditional on having less than 500 employees in the past three years while that in column (2) is ran conditional on having more than 500 employees in the past three years. All the dependent variables are expressed as growth rates. Labour productivity is defined in a standard way as $\frac{value\ added}{\#Employees}$. Capital productivity is defined as $\frac{value\ added}{\#capital}$ where capital is the amount of tangible assets. Working capital is defined as $\frac{assets}{liabilities}$. Investment rate is defined as $\frac{tangible\ assets}{value\ added}$. Bias-corrected estimates and their robust standard errors are reported. The regressions for employment and labour productivity are ran with sector-year fixed effects. The others are ran with both $ln(L_{i,t})$ controls and sector-year fixed effects.

5 Discussion

A set of findings on the impact of the quota announcement and change in the composition of boards of directors on the growth rate of TFP have been established in 4.1 and 4.3. The hypotheses backing its drop and increase during respectively the transition and compliance periods would, however, need to be confirmed by the data. As announced in 2.3, information on the composition of boards of directors and individual characteristics of its members will be necessary in this endeavour. An exhaustive dataset will be constructed by using three sources. Basic information on the names, the gender and the age of members and the start and end dates of their mandates will be collected from Orbis, Societe.com and BODACC[8]. Although they will only be available for boards of directors present in the Orbis database, details on members' education and role in firms will be gathered to be used as proxies for the directors' skills.

First of all, treatment should be confirmed by an increase in the share of women in boards of directors for firms that are targeted by the quota. It should at least be significantly larger than that of firms below the revenue and employment threshold. If some treated firms actually do not comply with the law and prefer paying financial penalties, it would be interesting to establish how different they are from those that decide to comply.
In order to test for the hypothesis of adjustment costs, a gap to compliance as in Bertrand et al. (2019) should be calculated. It would indicate the number of women needed for firms to achieve compliance with the law and thereby be a measure of the level of costly efforts required from them. Interacting it with the Diff-in-Disc estimators would provide evidence backing or refuting the hypothesis of the announcement as a transition period. If the estimators are significantly more negative for firms with a larger gap to compliance, it would allow to validate it. Moreover, this measure of cost would enable to identify a potential break-even point where the benefits of new members would outweigh the costs related to their hiring.
In order to test for the hypothesis of skills heterogeneity, a measure of diversity will be calculated based on differences in individual characteristics within boards of directors. In particular, education and the directors' role in firms could be used as proxies for their skills. In an interaction with the Diff-in-Disc estimators, other characteristics such as age would be controlled for. A significantly more positive estimator for firms with a higher level of skills heterogeneity would provide evidence for its role in the positive increase in the growth rate of TFP. The marginal contribution of particular skills could be identified to establish to what extent they are unique within the board (as in Kim and Starks (2016)). If diversity of skills does not increase the positive impact on TFP, it could indicate that preferences differ across individuals based on their gender. In the spirit of Apesteguia et al. (2012), there could be a mix of women and men in boards of directors that maximizes productivity. Information on the content of board meetings such as voting behaviour would help to disentangle this from the skills aspect. Unfortunately, to my knowledge such data does not exist.
For the purpose of this paper, it would be informative to test the shift in management practices as the main channel of impact. As employees in the DADS database have a unique identifier, it is possible to track changes within the labour force of a firm. If treated firms re-

[8] As a reminder, Orbis is a database on private firms, Societe.com is a website on registered firms in France and BODACC is a database on announcements of organizational changes within firms in France from 2008 onwards. Societe.com will be web-scrapped and announcements on changes specifically related to boards of directors will be identified in BODACC.

place significantly more their employees, especially those in senior management (as in Matsa and Miller (2011)) who are closely supervised by the boards of directors, around the observed increase in labour productivity it would provide evidence for this mechanism. The absence of replacement could, however, also be an informative result. It could indicate that labour productivity growth is achieved through better worker-task matches. The current dataset unfortunately does not allow to test the validity of this hypothesis.

6 Conclusion

This paper uses the 2011 announcement and 2014 implementation of quotas in boards of directors in France as a quasi-experiment to test the effect of a shock on their composition on firms. It presents causal evidence on TFP growth's variations by using a difference-in-discontinuities, a methodology so far widely used in political economy papers.

Consistent with the hypothesis on adjustment costs, I find that the growth rate of TFP decreases by 10 to 16 percentage points during the 2011-2013 announcement period of the law for firms targeted by it. During the 2013-2016 compliance period it increases by 23 to 30 percentage points while the growth rate of labour productivity increases by approximately 6 percentage points reflecting the positive impact of the new governance structure. The initial decline followed by a rise of the growth rate of TFP above pre-quota levels points to the positive impact of constrained change. It reflects the opportunity cost of homogeneous boards of directors for firms and the economy at large.

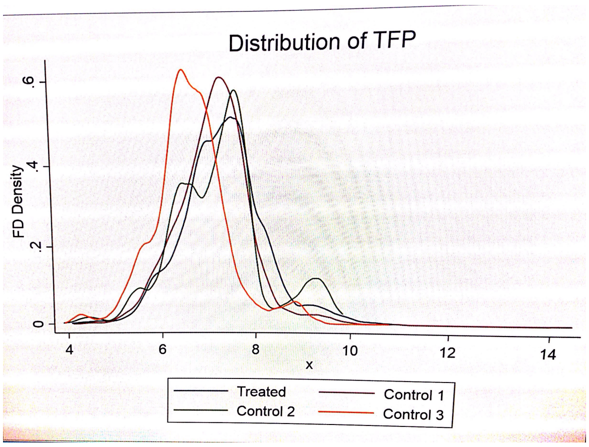
As a measure of economic efficiency, TFP has large implications for firm dynamics and the economy. Although this paper does not tackle the impact of the variations in TFP on entry and exit of firms, it would be interesting to compare it to those found in papers studying other types of constraints such as financial ones. It could give an economy-wide quantification of the real cost of homogeneity in governance.

Appendix

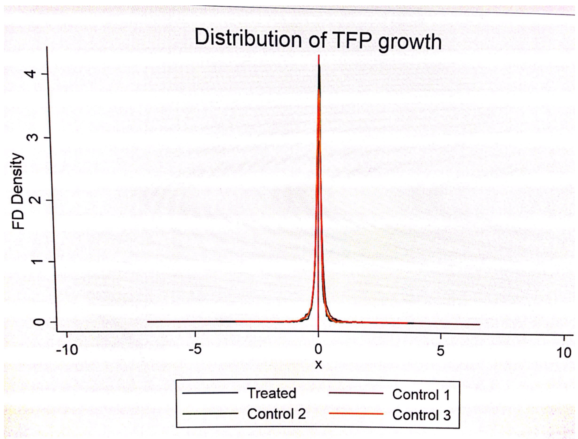
A Validity of the Design

Figures 3a and3b are reproduced with TFP à la Foster et al. (2001) and Baily et al. (1992).

Figure A.1: TFP and its growth rate before 2011



(a) TFP



(b) TFP growth

Notes:
The two figures plot the distribution of TFP and its growth rate per treatment status before 2011. TFP is calculated à la Foster et al. (2001) and Baily et al. (1992) as explained in 4.1.

B Main Results

Table 3 is run with a polynomial of order 0 (column (1)), with the alternative bandwidth (column (2)) and the epanechnikov kernel (column (3)).

Table B.1: IMPACT OF THE ANNOUNCEMENT OF THE LAW ON TFP GROWTH

	(1)		(2)		(3)	
	Δ TFP(1)	**Δ TFP(2)**	**Δ TFP(1)**	**Δ TFP(2)**	**Δ TFP(1)**	**Δ TFP(2)**
Proced.						
Convent.	-0.08**	-0.10**	-0.07*	-0.08*	-0.10**	-0.15**
	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.07)
Bias-corr.	-0.12***	-0.14***	-0.12***	-0.11**	-0.12***	-0.18***
	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.07)
Robust	-0.12**	-0.14**	-0.12**	-0.11*	-0.12*	-0.18**
	(0.06)	(0.06)	(0.05)	(0.06)	(0.07)	(0.08)
Obs.	3,276	3,276	3,276	3,276	3,276	3,276

Notes :
The regressions are a variant of the main specification. They are ran conditional on having at least 500 employees in the past three years and use 2011 as the event time. The regressions in column (1) use a polynomial of order 0, in column (2) use an alternative bandwidth while in column (3) use an epanechnikov kernel. They are ran on the 2001-2013 time-frame in order to estimate the impact of the announcement period separately from the compliance one. The dependent variable in column (1) is the growth rate of TFP à la Foster et al. (2001) and Baily et al. (1992). The dependent variable in column (2) is the growth rate of TFP à la Levinsohn and Petrin (2003). The estimators for all the procedures and its standard errors are reported. The conventional procedure reports conventional estimates and their conventional standard errors. The bias-corrected procedure reports bias-corrected estimates and their conventional standard errors. The robust procedure reports bias-corrected estimates and their robust standard errors. The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

Table 5 is run with a polynomial of order 0 (column (1)), with the alternative bandwidth (column (2)) and the epanechnikov kernel (column (3)).

Table B.2: Impact of compliance with the law on TFP growth

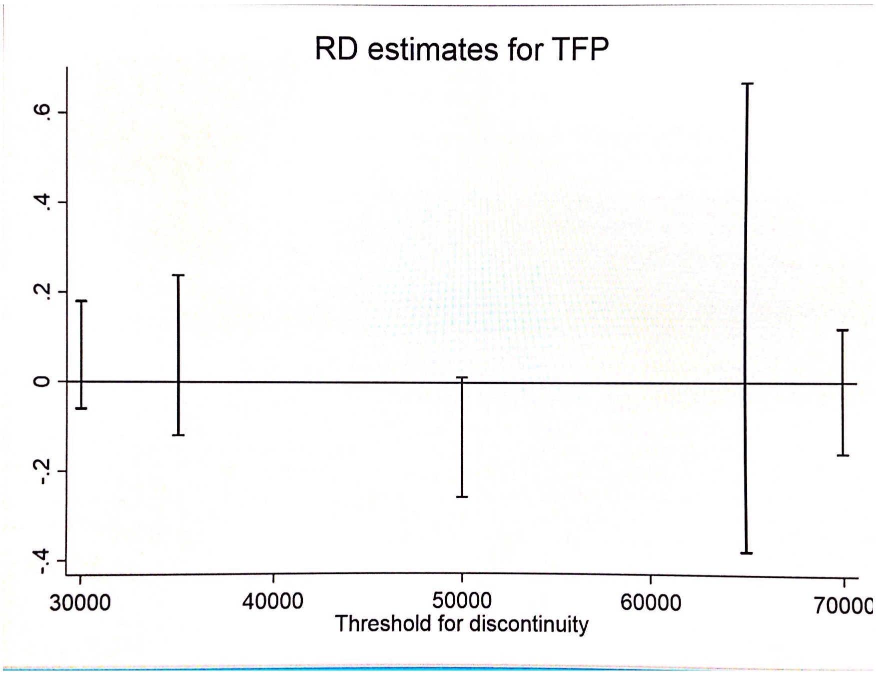
	(1)		(2)		(3)	
	Δ TFP(1)	**Δ TFP(2)**	**Δ TFP(1)**	**Δ TFP(2)**	**Δ TFP(1)**	**Δ TFP(2)**
Proced.						
Convent.	0.17**	0.18*	0.23**	0.24**	0.15**	0.14**
	(0.08)	(0.09)	(0.10)	(0.11)	(0.07)	(0.06)
Bias-corr.	0.24***	0.24***	0.29***	0.29***	0.25***	0.27***
	(0.08)	(0.09)	(0.10)	(0.11)	(0.07)	(0.06)
Robust	0.24**	0.24**	0.29**	0.29**	0.25**	0.27**
	(0.11)	(0.12)	(0.10)	(0.14)	(0.11)	(0.13)
Observations	1,483	1,483	1,483	1,483	1,483	1,483

Notes :
The regressions are a variant of the main specification. They are ran conditional on having at least 500 employees in the past three years and use 2014 as the event time. The regressions in column (1) use a polynomial of order 0, in column (2) use an alternative bandwidth while in column (3) use an epanechnikov kernel. They are ran on the 2011-2016 time-frame in order to estimate the impact of the compliance period separately from the announcement one. The dependent variable in column (1) is the growth rate of TFP à la Foster et al. (2001) and Baily et al. (1992). The dependent variable in column (2) is the growth rate of TFP à la Levinsohn and Petrin (2003). The estimators for all the procedures and its standard errors are reported. The conventional procedure reports conventional estimates and their conventional standard errors. The bias-corrected procedure reports bias-corrected estimates and their conventional standard errors. The robust procedure reports bias-corrected estimates and their robust standard errors. The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

C Robustness Checks

Figure 4 is reproduced using TFP à la Foster et al. (2001) and Baily et al. (1992) as the dependent variable with 2011 as the timing of the event.

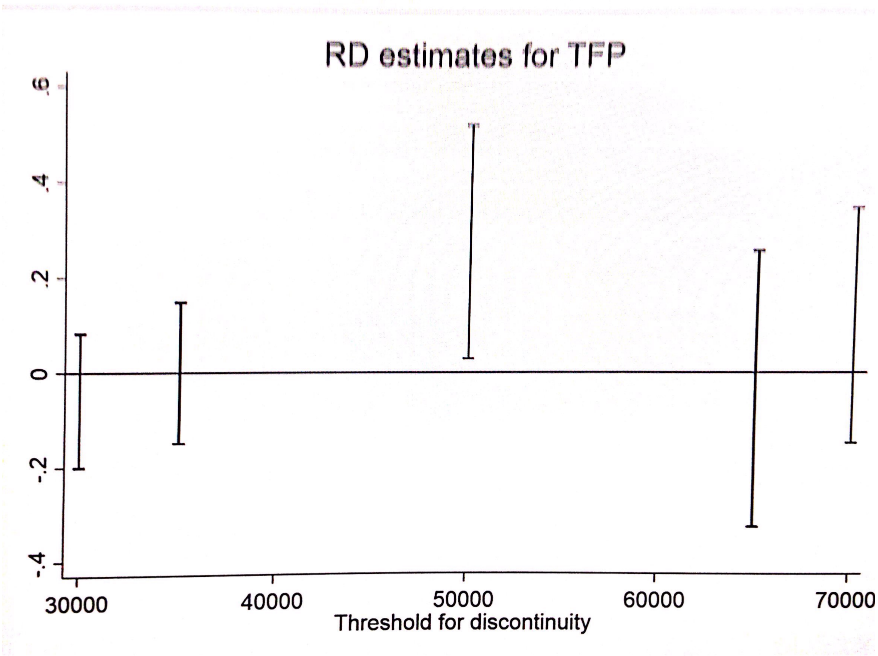
Figure C.1: Impact of the announcement of the law per threshold of discontinuity



Notes:
The figure plots the confidence intervals (CI) of coefficients obtained from the robust procedure where standard errors are robust. Each CI corresponds to a regression using either 30, 35, 50, 65 or 70 M. euros as the cut-off. For the 30 and 35 M. euros placebo cut-offs, any observation with at least 50 M. in revenue is removed while for the 65 and 70 M. euros cut-offs, any observation with a 50 M. or lower level of revenue is removed. The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having more than 500 employees in the past three years and use 2011 as the event. They are ran on the 2001-2013 time-frame in order to estimate the impact of the announcement period separately from the compliance one. The dependent variable is the growth rate of TFP à la Foster et al. (2001) and Baily et al. (1992). The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

Figure 5 is reproduced using TFP à la Foster et al. (2001) and Baily et al. (1992) as the dependent variable with 2014 as the timing of the event.

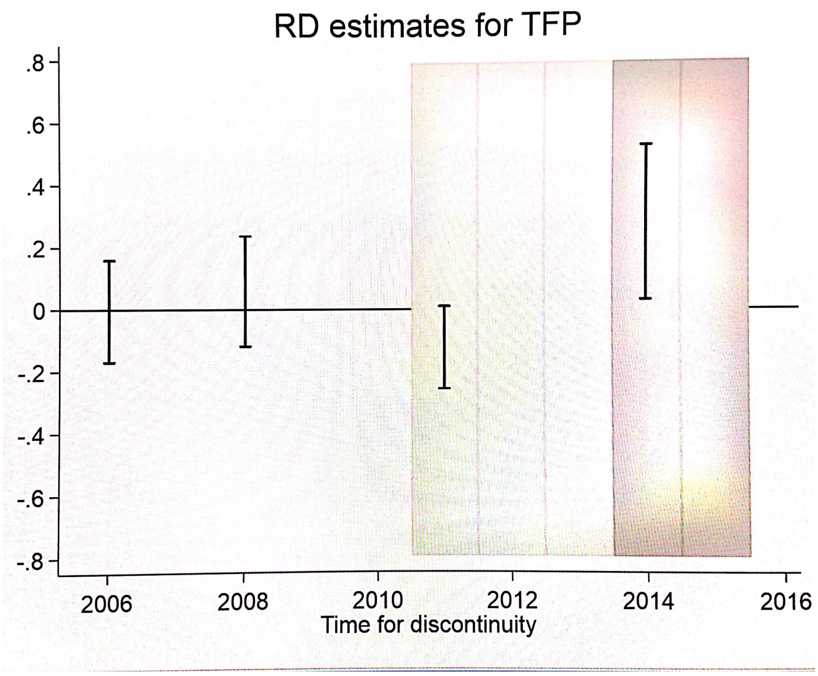
Figure C.2: Impact of compliance with the law per threshold of discontinuity



Notes:
The figure plots the confidence intervals (CI) of coefficients obtained from the robust procedure where standard errors are robust. Each CI corresponds to a regression using either 30, 35, 50, 65 or 70 M. euros as the cut-off. For the 30 and 35 M. euros placebo cut-offs, any observation with at least 50 M. in revenue is removed while for the 65 and 70 M. euros cut-offs, any observation with a 50 M. or lower level of revenue is removed. The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having more than 500 employees in the past three years and use 2014 as the event. They are ran on the 2011-2016 time-frame in order to estimate the impact of the compliance period separately from the announcement one. The dependent variable is the growth rate of TFP à la Foster et al. (2001) and Baily et al. (1992). The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

Figure 6 is reproduced using TFP à la Foster et al. (2001) and Baily et al. (1992) as the dependent variable.

Figure C.3: Impact of the event



Notes:
The figure plots the confidence intervals (CI) of coefficients obtained from the robust procedure where standard errors are robust. Each CI corresponds to a regression using either 2006, 2008, 2011 or 2014 as the event. For the two placebo years, the regressions are ran on the 2001-2010 time-frame. The regression using 2011 as the event is ran on the 2001-2013 time-frame while that using 2014 is ran on the 2011-2016 period. The regressions follow the main baseline described in 3.3 (polynomial of order 1, Calonico et al. (2014) bandwidth and triangular kernel) conditional on having more than 500 employees in the past three years and use 50 M. euros in revenue as the cut-off. The dependent variable is the growth rate of TFP à la Foster et al. (2001) and Baily et al. (1992). The regressions are ran with $ln(Y_{i,t-1})$ and $ln(L_{i,t})$ controls and sector-year fixed effects.

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