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Keywords:
political economy, political accountability, sanctions, reputation

JEL codes:
D72, D78, K42

Working Paper

Can public and private sanctions discipline politicians? Evidence from the French Parliament

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April 2018

Abstract

This paper investigates the effects of sanctions on the behavior of deputies in the French National Assembly. In 2009, the Assembly introduced small monetary sanctions to prevent absenteeism in weekly standing committee meetings (held on wednesday mornings). Using a rich monthly panel dataset of parliamentary activity for the full 2007-2012 legislature, we study the reactions of deputies to (i) the mere eligibility to new sanctions, (ii) the actual experience of a salary cut, and (iii) the public exposure of sanctioned deputies in the media. First, our diff-in-diff estimates show very large disciplining effects of the policy in terms of committee attendance, and positive or null effects on other dimensions of parliamentary work. Second, exploiting the timing of exposure to actual sanctions (monthly salary cuts versus staggered media exposure), we find that deputies strongly increase their committee attendance both after the private experience of sanctions and after their public exposure. These results suggest that monetary and reputational incentives can effectively discipline politicians without crowding out intrinsic motivation.

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The raw data on parliamentary activity is publicly available at *www.nosdeputes.fr* under ODbL license. We thank members of the non-profit organization *Regards Citoyens* for useful discussions regarding this project and Quentin Thévenet for his help in handling the data. We are also indebted to Renaud Huerta and Claude Mussou at *Institut National de l'Audiovisuel* for providing media presence data, *ACPM* for data on county-level newspaper circulation, and Alexandre Foatelli for more aggregate newspaper statitistics. We also thank Frédéric Jouneau-Sion, Marie-Claire Villeval, two anonymous internal reviewers at EconomiX, and seminar participants at GATE (Lyon), JMA (Le Mans), EconomiX (Paris Nanterre), AFED (Aix-en-Provence), CREM (Rennes) and CRED (Paris) for helpful comments.

Introduction

Politicians often face public suspicion and even hostility for their (actual or perceived) low public spirit and opportunistic behavior, in the form of absenteeism, moonlighting, and even corruption. In France for example, 68% of people feel disgust or suspicion from politics, 89% think that politicians pay little to no attention to their opinion, and only 45% of people trust their deputy (Cevipof, 2017). Similarly, in the United States, the job approval of Congressmen has dramatically eroded for the last fifteen years, from 50% in 2003 to below 20% today (Gallup, 2018). Ample research in political economy shows that periodic elections are imperfect institutions to make politicians accountable and trustworthy. Other mechanisms, such as increased public scrutiny, media exposure, or even high-powered incentives like monetary sanctions, have the potential to reduce problems of moral hazard (low effort) and adverse selection (low honesty or competency) into politics.

Following other Parliaments worldwide, the French National Assembly adopted an interesting innovation on this issue: in May 2009, as part of a larger reform of the functioning of the Assembly, deputies introduced monetary penalties to sanction low attendance in standing committee meetings (traditionally held on wednesday mornings), the main working body of the Parliament. Historically, although attendance has long been explicitely mandatory[1], many deputies skipped the wednesday-morning meetings of their standing committee: the attendance rate averaged one third in late 2008 (Regards Citoyens, 2010). In order to discipline their colleagues, and despite resistance[2], French deputies decided to enforce monetary penalties. As of October 2009, most of the 577 deputies incurred a small salary-cut in case of repeated absenteeism in wednesday-morning committee meetings: if they missed more than two meetings per month, deputies would have their monthly salary cut by 355 euros per additional absence (about 5% of the gross monthly salary of 7100 euros). However, a small fraction of deputies were ineligible to these sanctions, because of the remoteness of their constituencies (overseas) or because of their responsibilities in the Assembly (members of the board, party representatives).

Interestingly, while monetary sanctions were enforced each month through a deduction on deputies' pay slip, the information remained private (the Assembly consistently refused to publish any list of sanctioned deputies). However, a French non-profit organization promoting

[1]Prior to 2009, the internal rules of the Assembly already mentioned formal and monetary sanctions in case of repeat absenteeism on wednesday-morning meetings, but it was common knowledge that these rules were not enforced in practice (not once). See details in Section 2.

[2]Many deputies viewed the proposal as populist and trusted electoral accountability rather than fines to discipline deputies (e.g. "if deputies didn't work, they would get sanctioned by voters", Bernard Accoyer, President of the Assembly). However, the public opinion was highly supportive with a 85% rate of approval. Plus, the public scandal created in April 2009 by the release of a book by two journalists on the functioning of the Assembly (documenting absenteeism and lobbying) was probably instrumental in the introduction of sanctions.

government transparency, *Regards Citoyens*, published online the names of all the deputies who were sanctionable each month according to the new rules[3]. The publication of these lists occured on three particular dates: in mid-January 2010 (short list of the first 11 sanctionable deputies), in the end of July 2010 (91 names), and then again in early August 2011 (97 names). The lists of sanctionable deputies received large media coverage in France, particularly in July 2010 and August 2011, and was viewed as a scandal by many deputies (including the President of the Assembly, Bernard Accoyer). The Assembly kept refusing to confirm those lists, but admitted that about 30 deputies were sanctioned each month.

The introduction of new sanctions in October 2009 and the staggered exposure of sanctioned deputies in the media, consitute two natural experiments. Combined with rich daily and monthly data on parliamentary activity collected from official sources, spanning from June 2007 to May 2012 (full legislature) for all French deputies, they allow us to make several contributions in the fields of political economy and theory of incentives. First, we estimate the effects of this new policy on the behavior of current deputies in terms of committee attendance, but also regarding other non-targeted meetings and activities. Our diff-in-diff estimates, using ineligible deputies as controls (as well as French Senators), document the merits and limits of incentives to reduce moral hazard among politicians. Second, thanks to the discrepency between the timing of monetary fines (monthly) and the staggered coverage of sanctions in the media (on 3 different dates), we are able to better understand the motivations of politicians and disentangle the role of image concerns from private motives (e.g. money) in disciplining politicians[4].

Our results show that the introduction of sanctions had a large, significant, and long-lasting effect on attendance to wednesday-morning committee meetings (approximately +40%). We also provide strong evidence that the new policy also increased attendance to other meetings and many parliamentary activities not targeted by the sanctions. Conversely, we find little evidence of shirking (e.g. attending without active participation) or "gaming" of the rules (e.g. moving to low-activity committees, rescheduling meetings away from wednesday mornings).

Second, by tracking the reaction of sanctioned deputies over time, we disentangle the effects of getting sanctioned privately *versus* publicly: our estimates show very large increases in committee attendance after experiencing salary cuts (+50%) but also sizeable positive changes after public exposure in the media (+20%). However, other parliamentary activities and attendance on other days are less responsive to the experience of public or private sanctions. These results suggest that French deputies are sensitive to private and reputational costs, but react somewhat

[3]The organization exploited all publicly available data on standing committee attendance to precisely track the record of each deputy, and contacted all sanctionable deputies to make sure that no mistakes were made.

[4]In a similar attempt to distinguish between the effects of monitoring and money, Duflo et al. (2012) exploit non-linearities in the monetary incentives of teachers in India deciding to attend school (or not) every day.

stategically to punishment.

1 Theory and current evidence

As a general framework, we consider a Principal-Agent relationship where citizens delegate power to political representatives to serve their best interest. In theory, periodic elections allow the principal (voters) to exert control over agents (elected officials), first by choosing the best-suited politicians among the pool of candidates, and second by disciplining them while in office thanks to the threat of (non-) reelection.

However, in addition to the imperfections of voters' decisions (e.g. inattention, present-orientation), a large body of models document the limits of elections to make politicians accountable. The seminal contribution of Barro (1973) shows that while elections are useful incentives, core ingredients of the political structure (fixed wage, term length, limits on tenure in office) can result in undesired rent extraction among democratically elected officials. One famous example is the case of incumbents not seeking reelection, who have little to no incentives to align their behavior with voters' desires (the so-called *lame-duck* effect). Overall, this literature shows that even well-functioning democracies with a perfectly rational principal (voters) leave room for opportunistic behavior by agents (politicians).

Another key difficulty stems from information asymmetries between citizens and their political representatives. Whenever politicians' type and behavior are not perfectly observable by voters, situations of adverse selection and moral hazard arise. Regarding adverse selection, the citizen-candidate model of Caselli and Morelli (2004) show how particularly unfit citizens may well self-select into politics, either because they have low general competence (hence low opportunity cost of running for office) or low honesty (hence low cost of lying or extracting personal rents from public office). Fehrler et al. (2016) provide some experimental evidence of such adverse selection, finding that dishonest people (i.e. those eager not to respect their unbinding campaign promises) tend to get overly represented into politics. Another problem examined by Gagliarducci et al. (2010) arises when elected officials are allowed to earn outside earnings in addition to their public-office salary: while this possibility can attract a pool of better candidates (more competent), they will prefer to moonlight once elected (instead of working in the Assembly), yielding ambiguous net effects.

These two problems of moral hazard and adverse selection can lead to a political class composed of low quality citizens (low public spirit and/or low competence) whose private incentives are misaligned with the social good. Fortunately, institutional devices can help solve the problem, either by an improvement of the political and electoral system (increased monitoring,

transparency, media coverage) or through the use of high-powered incentives (e.g. monetary rewards and sanctions).

Regarding the functioning of the political system, a series of papers studying the effects of random government audits of municipalities in Brazil show that the experience of audits reduces corrupt incumbents' reelection prospects as well as future levels of corruption, thanks to the disciplining effects of public exposure and risk of conviction (Avis et al., 2017; Ferraz and Finan, 2008). Bobonis et al. (2016) also find consistent evidence of a disciplining effect of audits in Puerto Rico. In India, Besley and Burgess (2002) show that government responsiveness after natural disasters is lower in regions with exogenously low newspaper circulation. Similarly, in the United States, Snyder and Strömberg (2010) find that lower press coverage at the local level reduces voters' ability to recall the name of their representative and reduces U.S. congressmen's effort on several dimensions (e.g. attracting federal funding for the constituency). Finally, in Switzerland, Hofer (2016) finds that greater vote transparency in the Upper House of the Swiss Parliament (thanks to the introduction of electronic voting in 2014) led to an increase in voting by members of Parliament. Hence, there is solid quasi-experimental evidence from diverse political systems that increased monitoring, news coverage and public exposure of elected officials have the potential to discipline opportunistic politicians.

Another option to reduce moral hazard is the use of monetary sanctions (or rewards). Such incentives can be effective whenever agents are driven by money (Bénabou and Tirole, 2006; Gagliarducci et al., 2010; Ferraz and Finan, 2011), care about the expressive function of such sanctions (Funk, 2007), or consider other non-material costs of punishment such as self-image and public reputation (Ariely et al., 2009; Besley et al., 2009). These image concerns may be instrumental (e.g. securing reelection to keep extractring political rents) or purely affective.

However, several experiments shed light on the dark side of incentives, and in particular on the fact that monetary sanctions have hidden consequences (Frey and Oberholzer-Gee, 1997; Gneezy and Rustichini, 2000a,b; Ariely et al., 2009). This literature suggests that the introduction of sanctions can substitute an internal motivation (e.g a norm) by an external one (e.g monetary sanction, social concerns) leading to a motivational crowding-out. In their field experiment with parents picking up their children at day-care centers in Israel, for example, Gneezy and Rustichini (2000a) find that small monetary sanctions can crowd-out agents' intrinsic motivation, yielding a net increase in undesired behaviors (e.g. late pickup by parents). Similar evidence of crowding-out were found by Holmås et al. (2010) in Norwegian hospitals where fines imposed on owners of long-term care institutions to decrease patients' length of stay in hospital had the opposite effect.

In addition, Bénabou and Tirole (2006) show that the motivation-enhancing effect of monetary incentives can be partially or fully offset by image-related concerns stemming from a reduction in the signaling value of doing the "right thing", especially when actions can't be properly observed. Exploiting the introduction of postal voting in Switzerland, Funk (2010) finds that the reduced visibility of voting behavior decreased turnout in small municipalities where social pressure is likely stronger. Similarly, Carpenter and Myers (2010) show that image concerns play a positive role on volunteer firefighters's visible activities (volunteering and responding to calls) and that monetary rewards reduce call response. In their laboratory experiment, Ariely et al. (2009) find that monetary incentives aimed at pro-social behavior are less effective when actions are publicy observable (instead of private), supporting the crowding-out effect of monetary sanctions and rewards.

Applied to our context, the crowding-out literature suggests that divergent reactions can be expected from deputies in addition to simple deterrence. Incentives can have limited effects if incumbents facing sanctions keep their attendance behavior unchanged not to appear greedy, or can even backfire (i.e. increase absenteeism) if monetary sanctions are interpreted as a simple price (morally neutral). Conversely, deputies may increase attendance to avoid the social shame and the self-image stigma of getting fined and publicly exposed.

Another limitation of incentives such as salary cuts is their reliance on measurable, proxy variables instead of the final objective(s) of the policy. The multi-task principal-agent model of Holmstrom and Milgrom (1991) shows that, in situations involving team work and multiple qualitative tasks (e.g. parliamentary work), setting incentives on proxy variables (like attendance on wednesday-morning committee meetings) can backfire and result in undesirable behaviors: focusing on quantity instead of quality (attend all meetings without any active participation), shifting efforts from non-monitored to monitored activities (from hemicycle to standing committees), or even gaming the rules (sign the presence sheet and go[5]).

2 Institutional Context and Data

2.1 The National Assembly and standing committees

The French Parliament is composed of two legislative chambers, the Senate (upper) and the National Assembly (lower). The Assembly consists of 577 deputy seats, renewed every 5 years through direct, two-round runoff elections held on the same day in each constituency. In addition to plenary sessions in the hemicycle, deputies participate to the works of one of the 8

[5]Several deputies were publicly accused of such shirking behavior by colleagues.

standing committees of the Assembly. Each committee is composed of 1/8 of the Assembly (73 deputies) and devoted to one specific domain of legislation (e.g. Committee of Economic Affairs, Committee of Defense, Committee of Social Affairs). Each party is entitled a number of seats in each standing committee (depending on the party's share of seats in the Assembly) to ensure representativity. Following these rules, the President of the Assembly nominates all deputies in one of the 8 different committees at the beginning of the mandate (and then again at the beginning of each yearly session), usually in an attempt to match the wishes of deputies.

Standing committees are the essential working bodies of the National Assembly, where each new piece of legislation is discussed and amended by committee members, before the final vote of the law by all deputies in the hemicycle. They traditionally meet on wednesday mornings, but committee chairs can schedule other meetings during the week (usually on tuesday and wednesday afternoon). Therefore, attendance in weekly standing committee meetings is crucial and has long been mandatory according to National Assembly's rules. Prior to 2009, mild sanctions against absenteeism existed on paper (formal and monetary[6]) but those sanctions were not enforced any single time in practice. Thus deputies could routinely miss wednesday-morning meetings of their standing committee without consequences, and attendance rates were very low (1 in 3 meetings in 2008 for the average deputy, Regards Citoyens (2010)). In contrast, the new monetary penalties were actually enforced, at the end of every month (see below).

2.2 The new rules

In May 2009, the Assembly adopted a new set of internal rules, which were intended to adapt the institution to the revised Constitution voted in July 2008. Almost all 150 articles of the internal rules were amended. Except for a handful of deputies, all parliamentarians voted in accordance with the party line, either for or against this new piece of legislation (opposition groups voted against it)[7].

Overall, the new rules intended to give more power to standing committees, to opposition parties, and allowed better planning of parliamentary work. In order to have more efficient and more specialized standing committees, their number was increased from 6 to 8 committees (changes are listed in Table 12 in Appendix). Importantly, all those institutional changes affected the functioning of the whole Assembly, i.e. all deputies.

[6]Two types of sanctions existed on paper for deputies absent to more than 30% of standing committee meetings held on wednesday mornings throughout the parliamentary year (October-June) : first, absent deputies could get expelled from their committee (deputy no longer authorized to seat) ; second, they could suffer a salary cut of 473 euros per month during the next three months until the beginning of the new parliamentary year (July, August, September). However, those sanctions were never enforced, partly because they were difficult to implement in practice.

[7]Unfortunately, we can't track individual votes on the precise amendment introducing monetary sanctions since it was voted as a show of hands.

Monthly monetary sanctions against absenteeism were introduced as part of this large reform, through a modification of Article 42 of the internal rules (see the new article in Figure 3 in Appendix). As explained earlier, these sanctions only targeted wednesday-morning standing committee meetings: above two unjustified absences in a given month, deputies would have their monthly salary cut by 355 euros for each extra absence (5% of their monthly salary). There are typically 4 meetings in a given month (the maximum is 5) but holidays can reduce this number (down to zero in August). The list of acceptable excuses for not attending a meeting was published: sickness (doctor's note), major family incident, military service, special mission from the government, concomitant "special committee" meeting, concomitant meeting of an international assembly (APF, UEO, etc.). While we can't recover potential excuses for each absence[8], our data on sanctioned deputies from *Regards Citoyens* accounts for legitimate excuses.

Interestingly, about 6% of deputies (N=36) were not eligible to the monetary sanctions: the 10 members of the Board of the Assembly[9], the 4 presidents of the parties represented in the Assembly, and the 22 deputies from overseas constituencies. Of course, ineligible deputies could lose their privilege during their mandate (for example if they stop being members of the Board) but few changes occured: only 52 out of the 639 deputies were ineligible at some point from 2007 to 2012, for a stock of 36 ineligible positions.

According to the new rules, monetary sanctions were supposed to start with the application of the reform in October 2009 (beginning of the next legislative year). However, the President of the Assembly declared in a press release in January 2010 that the enforcement of the new penalties had been unofficially postponed to December 2009 (a 2-month delay), allegedly to give deputies more time to accomodate with the change[10]. Since the penalties were expected to start in October, we take it as the cutoff date in our empirical analysis.

2.3 Media coverage of sanctions

As explained earlier, the French non-profit *Regards Citoyens* published online the names of all the deputies who were sanctionable each month according to the new rules. The publication of these lists occured on three particular dates: in mid-January 2010 (short list of the first 11 sanctionable deputies), in the end of July 2010 (91 names), and then again in early August 2011 (97 names).

[8] Excuses provided by deputies are not systematically published, and among those published, some are eventually deemed illegitimate by the board of the Assembly.

[9] The 10 ineligible board members include the President of the Assembly, the 6 Vice-Presidents and the 3 *"questeurs"*. The 12 secretaries of the board were eligible.

[10] The press release is available on the Assembly's website : *http://www.assembleenationale.fr/presse/communiques/20100113-01.asp*

To illustrate the large media coverage of those lists in France, we provide two pieces of evidence in Figure 1. First, we plot the volume of weekly Google searches for the keyword "Regards Citoyens", from 2007 to 2012. Google searches reached an historical peak in the week of July 31 2011, i.e. exactly when *Regards Citoyens* released the third list of 97 names. The previous peak of July 18, 2010, corresponds to the public release of the second list, with about half as many weekly searches[11]. Finally, the first list of January 2010, which contained only 11 names, seems to have received limited public exposure as there is no particular peak in searches around this date. Daily traffic data from *Regards Citoyens*'s website yields a similar pattern, with a record level of about 30,000 page views in the week of July 31, 2011.

Another piece of evidence is the weekly number of media reports on the lists (bottom part of the graph). This data comes directly from the website of *Regards Citoyens*, which provides a list of all press, TV and radio coverage of their lists by French national (and sometimes local) media. Again, the lists of July 2010 and early August 2011 received a lot of media attention, with about 50 and 80 reports respectively in the following week. The first list of January 2010 received far less exposure.

2.4 Dataset and descriptive statistics

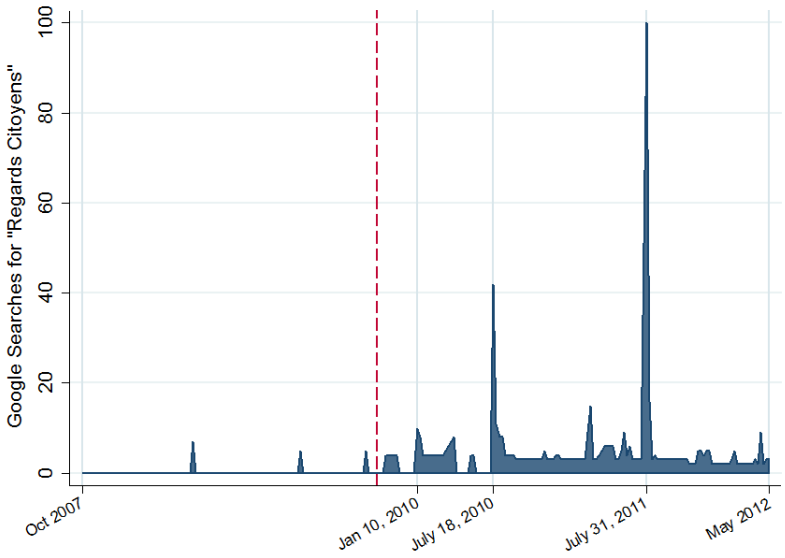
Our sample is composed of all 639 individuals who served at some point as deputies during the 13th National Assembly, from June 2007 to May 2012. The sample size is larger than the number of seats (577) because some deputies did not go through their full mandate (e.g. resignation, nomination in the government) and were replaced by new parliamentarians. Table 13 in Appendix reports summary statistics for basic sociodemographics (age, gender) and political characteristics (number of current mandates, type of other current mandates, party affiliation) for the full sample of deputies.

As our main dependent variables, we recover very rich data on individual attendance to all parliamentary meetings during the full legislature (June 2007-May 2012), compiled by the French non-profit organization *Regards Citoyens* on the website *nosdeputes.fr*. This data is extracted exclusively from official sources: the Assembly's online archives and the *Journal Officiel* (JORF). The original data from *Regards Citoyens* allows us to build a monthly panel dataset measuring the number of meetings attended in a given month by each deputy, by type of meeting (standing committees versus "extra" committees such as information committees, investigation committee, etc.), day of the week, and even time of the day.

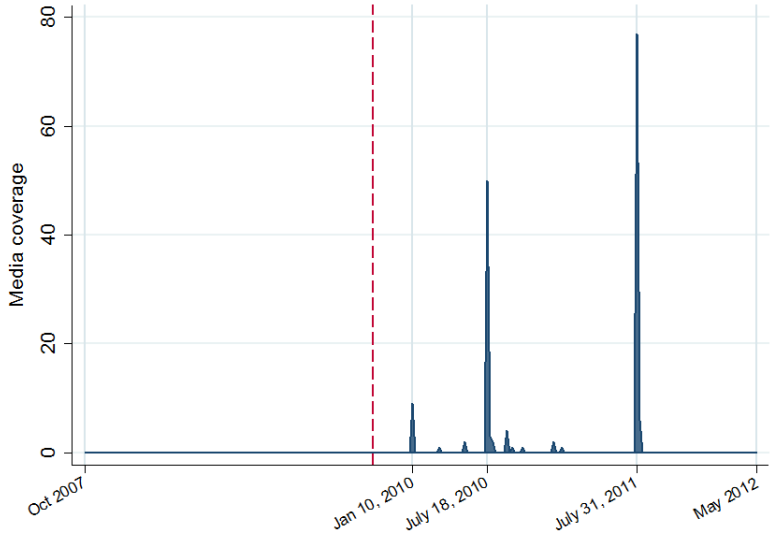
Table 1 provides descriptive statistics for these attendance variables for the full panel. On

[11] Google Trend only provides search volumes relative to the maximum value, denoted 100.

Figure 1: Public Interest and Media Coverage



Note: Google Trend for worldwide Google searches on keyword *"Regards Citoyens"* during the 2007-2012 period.



Note: Weekly media coverage (nb of reports) of *Regards Citoyens*'s lists of sanctioned deputies during the 2007-2012 period.

average, deputies attend 2.7 parliamentary meetings per month: a small fraction concerns "extra" commissions (0.3 per month), and the vast majority are standing committee meetings (2.3 per month). About half of attendance in committee meeting throughout the week is observed on wednesday morning: on average, each deputy attends 1.2 standing committee meetings held on wednesday mornings (from 9AM to 12PM) each month. Sometimes, a given committee meets twice during the same wednesday morning (for example at 9PM and then again at 11PM) but sanctions are unlikely to apply in those rare cases where a deputy attends the first meeting of the morning but not the second one (or vice versa). Therefore, we finally calculate the number of wednesday mornings where a deputy is detected present (once or more) at a committee meeting in a given month. This adjusted variable averages at 1 wednesday morning of presence per month, and naturally ranges from 0 to 5.

Using the data on attendance, we can infer which committee deputies belong to every month[12] and how many meetings were scheduled this month for that committee. As shown in Table 14 in Appendix, committees meet on 2.1 wednesday mornings every month on average, but large differences exist from one month to another (because of holidays mostly) and between committees (1.7 per month for the comittee of defense compared to 2.6 for the committee of cultural and educational affairs).

We also recover from *nosdeputes.fr* other monthly indicators of parliamentary activity, such as the number of amendments signed or adopted, the number of bills written or signed, the number of oral interventions in committees and plenary sessions, the number of questions to the government, and the number of parliamentary reports. These data are again extracted from official, publicly available sources (Assembly's archives and *JORF*). In addition to parliamentary activity, we also collect monthly data on media appearances on national TV and radio for a large subset of deputies (N=546), thanks to the publicly-funded French institute for media archives, INA (*Institut National de l'Audiovisuel*). We restrict our attention to deputies' voluntary participations in the media, e.g. giving a radio interview or participating in a TV documentary, not media reports or stories about deputies. All these data allow us to construct a monthly panel dataset of deputies' activity between June 2007 and May 2012, both in the Assembly and in the public arena. Table 2 reports summary statistics for all those other outcome variables. On average, each deputy speaks 9 times every month (3 in committee meetings and 6 in plenary sessions), signs 14 amendments per month (but only one is adopted), and appears twice on TV or radio.

[12] Deputies can ask to seat in another standing committee anytime, though most changes occur at the beginning of the parliamentary year. In theory, we could recover the composition of each standing committee at the beginning of each month from the Assembly's website, but those lists do not seem to be fully reliable.

Table 1: Descriptive Statistics for Attendance Variables

	mean	sd	min	p50	max
Total Attendance	2.659	3.095	0	2	43
– Extra Meetings	0.339	1.039	0	0	21
– Committee	2.320	2.664	0	2	32
—– Monday	0.036	0.238	0	0	9
—– Tuesday	0.657	1.066	0	0	11
—– Wednesday	1.537	1.802	0	1	13
——– Wednesday Morning	1.159	1.405	0	1	10
—– Thursday	0.083	0.375	0	0	8
—– Friday	0.005	0.099	0	0	4
Nb of Wednesday Morning Present	1.009	1.177	0	1	5
Observations	34881				

Table 2: Descriptive Statistics for Other Outcome Variables

	mean	sd	min	p50	max
Committee Speeches	3.158	11.436	0	0	407
Plenary Speeches	5.570	20.974	0	0	487
Amendments Adopted	1.086	4.894	0	0	264
Amendments Signed	13.607	40.781	0	0	527
Bills Written	0.062	0.355	0	0	19
Bills Signed	1.816	2.709	0	1	29
Written Questions	3.778	11.946	0	1	685
Oral Questions	0.162	0.408	0	0	3
Reports	0.073	0.319	0	0	9
Media Appearances	1.638	7.280	0	0	456
Media Interviews	0.838	2.505	0	0	76
Observations	34881				

As can be seen in Tables 1 and 2, most outcome variables are right-skewed: some deputies display very large activity in a given month compared to the sample mean and median. The shape of these distributions leads us to use in our regressions either a logarithmic transformation of continuous outcome variables (with $Y = log(y+1)$) or dummy variables (e.g. deputy i wrote at least one bill proposal in month t).

3 Identification strategies

3.1 Overall causal effects of the policy

Our first objective is to estimate the causal effects of this new policy on deputies' behavior in office. In particular, we want to measure the effectiveness of sanction eligibility in increasing attendance to weekly committee meetings. We also investigate whether those sanctions had other positive effects on other non-targeted activities or backfired in some way (shirking, rescheduling of meetings, etc.)

To measure the disciplining effect of sanctions, one might simply compare deputies' behavior before and after the introduction of sanctions in October 2009. However, some other aspects of the new internal rules (such as the increased role of standing committees) or contemporaneous changes in the broader political context could easily contaminate this before/after estimator. Thus, we rely on a Difference-In-Difference strategy where we compare changes in behavior before and after the reform between eligible and ineligible deputies. As explained earlier, all deputies were similarly affected by the new internal rules, except for the monetary sanctions which excluded a group of 36 ineligible deputies. The DID estimator is robust to contemporanous changes affecting all deputies similarly.

We estimate a DID regression with individual and time fixed-effects of the following form:

$$Y_{it} = \alpha_i + \theta_t + \alpha_1 DD_{it} + \alpha_2 Elig_{it} + \alpha' X_{it} + \epsilon_{it} \quad (1)$$

α_i is a set of individual fixed effects capturing time-invariant differences in activity between parliamentarians. The set of time dummies θ_t ($year * month$) controls for month-to-month changes in activity in the Assembly in order to net out all common temporal variations (e.g. sudden shocks, seasonal effects). The dummy variable $Elig$ captures the potential differences in activity between eligible ($Elig = 1$) and ineligible deputies (0), whose status can possibly evolve over time due to turnover in positions of ineligibility (although this is rare in practice). The variable DD takes 1 for eligible deputies after the reform (starting in October 2009) and 0 for all other observations. The coefficient α_1 thus represents our diff-in-diff estimator. Importantly,

standard errors are robust to heteroscedasticity and clustered at the individual level to allow autocorrelation. Also recall that attendance is log-transformed ($Y = log(y + 1)$) while other outcomes are treated as dummies to deal with their right-skewed distribution.

While we use Equation 1 as our main model of the effects of eligibility to sanctions, we also run simpler DID regressions where eligibility is assumed constant over time : it takes 1 for deputies who were eligible when sanctions started (in October 2009) and 0 for deputies not eligible at that time. Since eligibility is no longer allowed to change over time, the individual fixed effects now get rid of this variable in the regression, which becomes :

$$Y_{it} = \alpha_i + \theta_t + \alpha_1 DD2_{it} + \alpha' X_{it} + u_{it}$$

where $DD2$ takes 1 for observations refering to deputies eligible on October 2009 in post-reform periods (and 0 for all other observations). This formulation yields a simpler interpretation for α_1 in the context of log-linear models.

The DID approach makes two key identifying assumptions : common trend between the two groups (eligibles and ineligibles would have followed parallel trends in attendance had there been no sanctions in October 2009), and stable unit treatment value or SUTVA (the new sanctions did not affect the behavior of ineligibles). First, in order to check the plausibility of the common trend assumption, Figure 2 displays the average monthly number of committee meetings attended during the five years of mandate. Eligible and ineligible deputies exhibit quite different base levels in terms of attendance (probably because ineligibles face particularly high traveling costs and/or burdensome administrative duties), but their trends in attendance are highly similar before the reform. We are therefore confident that the common trend assumption is likely to hold in our context. Figure 2 also offers preliminary evidence that the effects on attendance are positive and sizable: changes in attendance around October 2009 seem much larger among eligibles than ineligibles.

Second, regarding SUTVA, we can't reject the possibility that the introduction of sanctions in the Assembly influenced the behavior of ineligible deputies, for example through peer effects in attendance between colleagues of the same institution. However, note that such violation is likely to bias α_1 downward, yielding a conservative estimate of the true effect of sanctions. Still, in order to assess the robustness of our results to a violation of this assumption, we also employ the 348 French senators as an alternative control group, since they are less likely to be "contaminated" by the introduction of sanctions in the Assembly (senators seat in a different building and follow their own internal rules). However, we view senators as an inferior control group (and only use it as a robustness check) for three reasons : first, we only have access

to aggregate measures of attendance for senators (all week, all types of meetings) ; second, the Senate like the Assembly adopted a new set of internal rules in 2009 to adapt to the revised Constitution, although it crucially did not include the introduction of sanctions against absenteeism ; third, elections for the Senate occur every three years with half-replacement of the chamber (two senatorial elections occured during the 2007-2012 period, in September 2008 and in September 2011) and election periods are matched with particular patterns in attendance in the Senate (lower attendance during the preceding months, and higher attendance during the next months). This electoral-cycle effect in the Senate weakens the plausibility of common trends between deputies and senators, as depicted in Figure 4 (left plot) in Appendix. To avoid this problem, we use attendance levels corrected for this electoral-cycle effect (the procedure is detailed below Figure 4 in Appendix) and recover a DID graph offering stronger support for the common trend assumption between French deputies and senators (right plot of Figure 4). The increase in cycle-corrected attendance around October 2009 is large in both chambers of the Parliament, as they both adopted new rules, but much larger in the Assembly. This pattern is consistent with a large, positive effect of sanctions on attendance in the Assembly, confirming the preliminary evidence using ineligible deputies as controls (Figure 2).

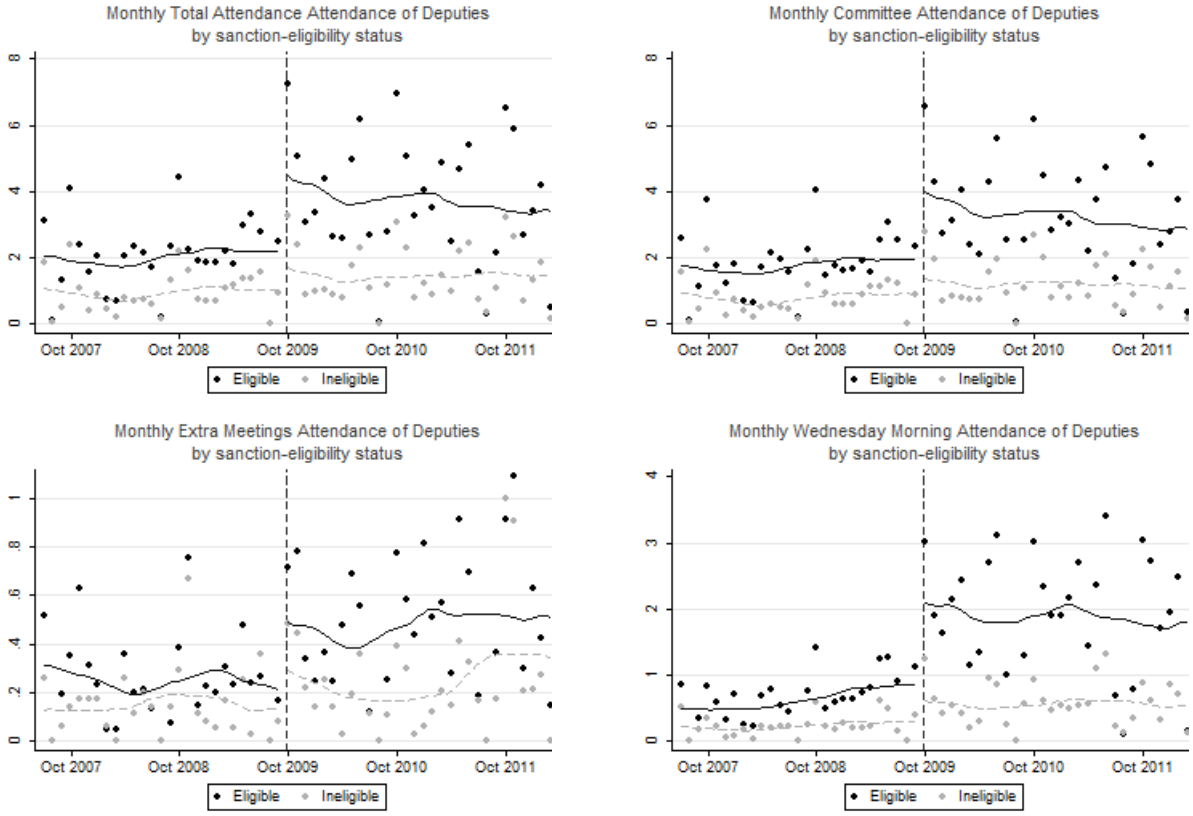
3.2 Disentangling public and private motives

Our second objective is to understand which mechanisms explain the overall reaction of deputies. In particular, we want to know (1) whether deputies react to the mere threat of sanctions, to actual punishment, or both, and (2) whether deputies' reactions are driven by private motives (e.g. money, self-image), reputational concerns, or both.

To disentangle these mechanisms, we exploit the fact that sanctions were enforced privately every month (deduction on the pay slip) while public exposure of sanctioned deputies occured on three particular occasions: first, in mid-January 2010, just after the introduction of sanctions (short list of the first 11 sanctionable deputies), then in the end of July 2010 (91 names), and then again in early August 2011 (97 names). This staggered public exposure of deputies over time, compared to the regular enforcement of private sanctions every month, allows us to disentangle the effect of private and public sanctions. Overall, we can track the reactions of sanctioned deputies to three successive events : becoming eligible to new sanctions, experiencing a private sanction, and being exposed publicly in the media.

To distinguish these three effects empirically, we augment our previous model with two additional dummy variables, *Private* and *Public*, and allow deputy-specific trends in unobservables,

Figure 2: Attendance to Committee and Extra Meetings over Time and Eligibility



Note: Each dot measures the average number of meetings attended by deputies in a given month of the legislature. The fitted lines represent kernel functions (using Epanechnikov smoothing with ± 3-month bandwidth). The vertical dashed line represents the original starting date of monetary sanctions (October 2009).

ω_i. This yields the following random trend model (Wooldridge, 2010) :

$$Y_{it} = \alpha_i + \theta_t + \omega_i t + \beta_1 DD_{it} + \beta_2 Elig_{it} + \beta_3 Private_{it} + \beta_4 Public_{it} + \beta' X_{it} + e_{it} \qquad (2)$$

where *Private* takes 1 if the deputy has already been sanctioned privately in a previous month (at least once), and 0 otherwise. *Public* takes 1 if the deputy's name has already been leaked in the media, and 0 otherwise, thus capturing the role of public exposure (reputational costs). Again, α_i captures individual fixed effects (time-invariant) and θ_i all common month-to-month changes. Since the effect of experiencing sanctions personnaly (either publicly or privately) are now captured in the model by β_3 and β_4, the coefficient associated with DD, β_1, can be interpreted as the general deterrent effect of being eligible to sanctions (threat effect). Conversely, β_3 and β_4 correspond to deputies' reaction after getting sanctioned (punishment effect). The deputy-specific time trends, $\omega_i t$, allow the unobservable determinants of attendance to evolve over time, i.e. they relax the assumption of time-invariant fixed effects. This is particularly important in our application, since both activity (Y) and getting sanctioned (*Private*) are probably explained by unobservable, time-varying variables like motivation for parliamentary work: some deputies may lose motivation over time, yielding simultaneously lower attendance and the triggering of monetary sanctions, with no causal link between the two. Including $\omega_i t$ in the model thus helps to solve the endogeneity problem contaminating β_3. Finally, the specification still allows autocorrelation and heteroscedasticity in ϵ_{it}.

To estimate our random trend model (2), we first differenciate all variables (e.g. $\Delta Y_{it} = Y_{it} - Y_{it-1}$) and then apply the *within* transformation ($\Delta \tilde{Y}_i = \Delta Y_{it} - \overline{\Delta Y_i}$) to get rid of both deputy-specific intercepts and trends in unobservables (Wooldridge, 2010). Double-differencing yields similar results.

4 Results: overall effects of the policy

4.1 Attendance in standing committee meetings

In Table 3, we regress Equation 1 using the within estimator to measure the effect of the new policy in terms of attendance to standing committee meetings during the whole week (column 1) or particular days (columns 2 to 6). Table 4 reports the estimates when eligibility is considered constant over time to ease the interpretation of the DID coefficients.

As shown in column 1 (Table 3), monthly committee attendance increased significantly more among eligible deputies after October 2009 than among ineligibles. As expected from the design of the sanctions, most of the total effect is explained by a very large increase in wednesday-

morning attendance. The introduction of sanctions also increases attendance on tuesdays among eligibles. Conversely, attendance on wednesday afternoon, evening and on thursday is not affected. Regarding the coefficient of *EligibleDeputy*, remember that we estimate a fixed-effects regression so this variable only captures the (log) change in attendance for the few deputies who change in eligibility status before the reform.

Table 4 using eligibility status as of October 2009 provides similar and easy-to-interpret results. Our DID estimates suggest that the total number of committee meetings attended rose by 34% on average thanks to the new sanctions[13]. Relative to the mean attendance before the reform (1.7 meetings per month), our estimate corresponds to an increase of about 0.6 meeting per month. Regarding wednesday-morning meetings, our estimate suggests an increase in attendance by 39% on average. Attendance rose significantly, by 8%, on tuesdays, while the estimates remain close to zero for other periods of the week. Overall, these results by weekdays are consistent with eligible deputies showing up more often at the Assembly on wednesday morning and the day before, but not changing their habits later in the week.

Table 3: Diff-in-Diff Estimates on Committee Attendance

	(1)	(2)	(3)	(4)	(5)	(6)
	AllWeek	Tuesday	WedMorning	WedAftern	WedEvening	Thursday
DD (After*Elig)	0.320***	0.0887***	0.350***	0.00870	0.00865	-0.00289
	(0.0343)	(0.0175)	(0.0285)	(0.0129)	(0.00602)	(0.00764)
Eligible Deputy	0.00586	0.0661^{+}	-0.0859	0.0406	0.0210^{+}	0.0136
	(0.0754)	(0.0338)	(0.0601)	(0.0297)	(0.0108)	(0.0157)
Time_FE	X	X	X	X	X	X
obs	34881	34881	34881	34881	34881	34881
obs_i	639	639	639	639	639	639
r2_o	0.420	0.192	0.476	0.126	0.0740	0.124
mean_y	1.673	0.545	0.589	0.341	0.0501	0.107

Robust standard errors, clustered at the individual level.

4.2 Genuine change or shirking ?

Deputies could develop several strategies to avoid sanctions and shirk. First, deputies may attempt to become ineligible. However, of the 36 positions of ineligibility, 22 can not be reached while in office as they are dedicated to overseas constituencies. The only way to become ineligible to sanctions is to become one of the 10 members of the board of the Assembly, or one of the 4 party representatives. These positions are highly political and are usually not accessible to lay deputies. Overall, there is very little turnover over time: only 52 different deputies were

[13]The effect of sanctions is computed as $exp(0.293) - 1 = 0.340$

Table 4: Diff-in-Diff Estimates on Committee Attendance (with Eligibility on Oct. 2009)

	(1) AllWeek	(2) Tuesday	(3) WedMorning	(4) WedAftern	(5) WedEvening	(6) Thursday
DD2 (After*Elig10/09)	0.293***	0.0766***	0.327***	0.00768	0.00483	-0.00904
	(0.0397)	(0.0176)	(0.0347)	(0.0118)	(0.00553)	(0.00670)
Time_FE	X	X	X	X	X	X
obs	33921	33921	33921	33921	33921	33921
obs_i	576	576	576	576	576	576
r2_o	0.417	0.187	0.481	0.124	0.0733	0.124
mean_y	1.677	0.547	0.591	0.341	0.0502	0.108

Robust standard errors, clustered at the individual level. N = 576 (deputies in office in October 2009).

ineligible at some point between 2007 and 2012, for a stock of 36 positions.

Second, eligible deputies could try to move between standing committees in order to seat in a low-frequency committee, e.g. committee of defense (hence reducing the risk of getting sanctioned). However, those movements are limited by the fact that each committee has to be composed of 1/8 of the Assembly, with political representativity in each committee. To investigate the importance of such moves, we report in Figure 6 (appendix) the evolution of the average number of committee meetings held on wednesday mornings from 2007 to 2012, for eligibles and ineligibles. The two curves are perfectly aligned before and after the reform, suggesting that eligible deputies did not move strategically to low-frequency committees. A more direct way of testing whether strategic between-committee moves are present is to include as regressors in our DID equation the standing committee each deputy belongs to. Controlling for committee fixed effects should take the effect of those movements out of the DID estimate. However, as shown in Table 15 (appendix), the DID estimate remains remarkably similar with and without committee fixed effects (0.338 versus 0.350), suggesting that little opportunistic moves occured after the reform to avoid sanctions.

Third, another possibility to avoid sanctions would be to reschedule meetings away from wednesday mornings. Committee meetings are usually planned by the President of each committee, in accordance with the President of the Assembly. As can be seen in Figure 6 again, the number of meetings scheduled on wednesday mornings did not decrease over time: it actually increased in years 2007-2008, and then again in October 2009 when two additionnal standing committes were created.

Finally, deputies may substitute their efforts away from non-targeted meetings, or attend wednesday-morning meetings without producing any genuine output (i.e. discussing and amending bills)[14]. To investigate such shirking, we estimate the effect of the new policy on the number

[14]According to media reports and testimonies, a few shirking deputies just sign the presence sheet and go, thus producing no additional output.

of oral interventions in committees and plenary sessions, on attendance to extra meetings, media appearances, and on a wide range of other parliamentary activities. The results appear in Tables 5 and 6. As shown in columns 1 and 2 (Table 5), speeches in committees as well as plenary sessions increased significantly thanks to the sanctions, notably with a 13 percentage point increase in the probability that a deputy speaks in a committee in a given month (from a 33% pre-reform mean). These estimates suggest that the new policy had similarly large positive effects on committee attendance (Tables 3-4) and oral participation. Moreover, the results in columns 3 to 7 show positive and significant effects of sanctions on attendance to extra meetings (non-standing committees) on tuesdays and wednesdays, and more media presence through interviews. Table 6 shows similar positive effects of sanctions on amendments adopted and signed, and parliamentary reports. The slight reduction in oral questions to the government may come from the fact that question sessions are held on wednesday afternoon (deputies may have less time to prepare them in the morning).

Overall, these results strongly suggest that deputies did not shirk to adapt to, or even avoid, the new sanctions. We find that the policy had very large positive effects on attendance to different types of meetings, not just wednesday-morning standing committees, and also increased several measures of parliamentary output. We do not detect any adverse consequences of sanctions, except for the slight reduction in oral questions to the government.

Table 5: Diff-in-Diff Estimates on Other Activities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ComSpeechD	PlenSpeechD	ExtraTue	ExtraWed	ExtraThu	Media	Interviews
DD (After*Elig)	0.130***	0.0686***	0.0389***	0.0447***	-0.00980	0.0291	0.0466**
	(0.0218)	(0.0186)	(0.0108)	(0.0110)	(0.0112)	(0.0257)	(0.0170)
Eligible Deputy	0.0425	-0.300***	0.0358^{+}	0.0134	-0.000552	-0.107***	-0.0806***
	(0.0473)	(0.0443)	(0.0198)	(0.0218)	(0.0137)	(0.0312)	(0.0229)
Time_FE	X	X	X	X	X	X	X
obs	34881	34881	34881	34881	34881	31607	31607
obs_i	639	639	639	639	639	546	546
r2_o	0.185	0.162	0.0484	0.0702	0.0301	0.0201	0.0443
mean_y	0.331	0.331	0.0973	0.0904	0.0397	0.275	0.252

Robust standard errors, clustered at the individual level.

Table 6: Diff-in-Diff Estimates on Parliamentary Output

	(1) AmendAdoptD	(2) AmendSignD	(3) BillWrittenD	(4) BillSignD	(5) WrittenQD	(6) OralQD	(7) ReportD
DD (After*Eligible)	0.0446*	0.0563*	0.00194	0.0153	0.00690	-0.0216^{+}	0.0213***
	(0.0180)	(0.0238)	(0.0114)	(0.0265)	(0.0340)	(0.0127)	(0.00508)
Eligible Deputy	-0.0178	-0.0303	-0.0466*	-0.0189	-0.0332	-0.0195	0.00901
	(0.0286)	(0.0370)	(0.0199)	(0.0361)	(0.0405)	(0.0249)	(0.00855)
Time_FE	X	X	X	X	X	X	X
obs	34881	34881	34881	34881	34881	34881	34881
obs_i	639	639	639	639	639	639	639
r2_o	0.165	0.275	0.0121	0.280	0.0884	0.0925	0.0560
mean_y	0.224	0.390	0.0413	0.586	0.569	0.132	0.0538

Robust standard errors, clustered at the individual level.

5 Results: responses to public and private sanctions

Our second main objective is to understand which mechanisms drive deputies' reaction to the new policy. We investigate two issues: first, is it the threat of sanctions or the experience of sanctions that make deputies react positively? These two effects can both play a role. Second, are the disciplining effects of sanctions explained by the private experience of punishment, or by subsequent public exposure in the media? To decompose all these channels, we estimate Equation 2 with the within estimator on first-differenced data. The three coefficients of main interest will provide estimates for the behavioral change induced by the threat of sanctions after October 2009 (β_1), by the private experience of sanctions (β_3), and by the public exposure of sanctioned deputies in the media (β_4). The results of our random trend regression for committee attendance appear in Table 7, while results on other activities appear in Tables 8 and 9.

First, it is noteworthy that the diff-in-diff estimate for monthly committee attendance in Table 7 is similar in magnitude to that obtained in Table 3 (0.39 instead of 0.32). This similar estimate suggests that the large increase in attendance observed after October 2009 among eligible deputies is not explained by the (positive) reaction of deputies who get sanctioned over time. The reform had a large threatening effect for many deputies, and not only a punishment effect on those who were effectively sanctioned at some point.

Second, private experience of sanctions significantly increases committee attendance: getting sanctioned (for the first time) leads to a 58% increase in the number of committees attended on average each following month[15]. This very large effect suggests that experiencing private sanctions (in terms of salary cut, self-image, etc.) strongly deters future absenteeism, though the effect is mostly driven by wednesday mornings.

Third, subsequent public exposure of sanctioned deputies also significantly increases future committee attendance. On average, the leak of one's name in the media leads to a 17% rise in attendance in the following months. The large effect of media exposure suggests that French deputies are sensitive to reputational costs.

Table 8 provides estimates for committee speeches and attendance to extra meetings. Again, experiencing public and private sanctions significantly increase oral participation and, to a lower extent, attendance to other meetings in the Assembly. However, parliamentary output measured as amendments, bill proposals, questions to the government or reports are not significantly affected by punishment, either private or public (Table 9). All of the estimated coefficients (except one) are positive but only one reaches statistical significance (public exposure increases written questions to the government). Therefore, while we do not detect adverse effects of sanc-

[15] As attendance is still measured in log, the 58% effect is obtained as $(\exp(\beta_3) - 1)$.

tioning or exposing deputies in the data, we only find limited positive spillovers of punishment on other parliamentary activities. It seems that sanctioned, exposed deputies mostly adjusted their committee attendance level to avoid future sanctions.

Table 7: Public and Private Sanctions

	(1) AllWeek	(2) WedMorning	(3) WedAfternoon	(4) WedEvening	(5) Tuesday	(6) Thursday
DD	0.393**	0.329**	0.197*	0.104*	0.325***	0.0172
	(0.150)	(0.104)	(0.0875)	(0.0426)	(0.0863)	(0.0755)
Elig	-0.207	-0.226^{+}	-0.102	-0.0393	-0.0849	0.00798
	(0.180)	(0.131)	(0.116)	(0.0666)	(0.110)	(0.0957)
Private	0.456***	0.502***	0.129***	-0.0182	0.0338	0.00650
	(0.0598)	(0.0497)	(0.0328)	(0.0205)	(0.0479)	(0.0146)
Public	0.160*	0.153***	0.00394	-0.00834	0.0566	-0.0100
	(0.0636)	(0.0414)	(0.0268)	(0.0142)	(0.0410)	(0.0224)
Time_FE	X	X	X	X	X	X
obs	34242	34242	34242	34242	34242	34242
obs_i	638	638	638	638	638	638
r2_o	0.391	0.322	0.153	0.0623	0.184	0.0803
mean_y	1.673	0.589	0.341	0.0501	0.545	0.107

Robust standard errors, clustered at the individual level. Random Trend Models.

Table 8: Effect of Public and Private Sanctions on Other Outcomes

	(1) comSpeechD	(2) PlenSpeechD	(3) Extra	(4) Media	(5) Interviews
DD	-0.119	0.0517	0.0937	-0.0569	-0.0791
	(0.0927)	(0.0886)	(0.0974)	(0.0731)	(0.0924)
Elig	0.0188	-0.200^{+}	0.0870	0.00329	0.121
	(0.151)	(0.109)	(0.148)	(0.122)	(0.139)
Private	0.204***	0.0421	0.0753**	0.0865^{+}	0.119*
	(0.0504)	(0.0446)	(0.0290)	(0.0465)	(0.0517)
Public	0.0940*	0.109**	0.0360*	-0.0150	0.0205
	(0.0437)	(0.0364)	(0.0179)	(0.0420)	(0.0391)
Time_FE	X	X	X	X	X
obs	34242	34242	34242	31061	31061
obs_i	638	638	638	546	546
r2_o	0.144	0.125	0.0943	0.0170	0.0336
mean_y	0.331	0.339	0.122	0.275	0.252

Robust standard errors, clustered at the individual level. Random Trend Models.

Table 9: Effect of Public and Private Sanctions on Parliamentary Output

	(1) AmenAdoD	(2) AmenSignD	(3) BillWritD	(4) BillSignD	(5) WritQD	(6) OralQD	(7) ReportD
DD	0.174^{+}	0.128	-0.00795	0.185^{+}	-0.0238	0.0656	0.0414
	(0.0984)	(0.0804)	(0.00958)	(0.112)	(0.0907)	(0.0762)	(0.0420)
Elig	-0.135	-0.117	0.00703	-0.190	-0.111	-0.00155	0.0473
	(0.135)	(0.134)	(0.00829)	(0.123)	(0.117)	(0.116)	(0.0749)
Private	0.0153	0.0296	0.0174	-0.0426	0.0409	0.0629	0.0190
	(0.0389)	(0.0451)	(0.0258)	(0.0424)	(0.0446)	(0.0407)	(0.0250)
Public	0.0209	0.0297	0.0259	0.0220	0.104*	0.0279	0.0235
	(0.0257)	(0.0374)	(0.0189)	(0.0388)	(0.0524)	(0.0189)	(0.0206)
Time_FE	X	X	X	X	X	X	X
obs	34242	34242	34242	34242	34242	34242	34242
obs_i	638	638	638	638	638	638	638
r2_o	0.147	0.213	0.00881	0.218	0.0606	0.0562	0.0592
mean_y	0.224	0.390	0.0413	0.586	0.569	0.132	0.0538

Robust standard errors, clustered at the individual level. Random Trend Models.

Heterogeneity

In order to investigate the mechanisms driving politicians' reactions after salary cuts and public exposure, we successively estimate the same model of wednesday-morning attendance for different subgroups. We look at two main sources of heterogeneity.

First, we investigate whether multiple-office holders react differently from politicians who hold only one political mandate (as deputy). Among all deputies, only 98 do not serve any other official elected position. Most deputies combine their seat in the Parliament with local mandates in city councils, county councils or regional assemblies. In more than half of cases, multiple-office holders even serve a local executive position, as city mayor or president of a county council or a regional assembly. In columns 1 to 3 of Table 10, we split our sample between these three groups (full-time deputies with no other mandate, multiple-office holders with no local executive power, and deputies who are also local executives).

Full-time deputies are equally responsive to private sanctions and public exposure (about + 35% in wednesday-morning attendance in both cases). Conversely, the responses of multiple-office holders differ and depend on their type of local position : all multiple-office holders (either as local executives or not) respond very strongly to private monetary punishment (with about 60% increases in wednesday-morning attendance) but interestingly, local executives also display a significant positive increase in attendance after public exposure whereas non-executives show no significant reaction to exposure. This difference in reactions to public sanctions may be explained by the larger reputational costs of such exposure among local executives who hold a high-profile position. Taken together, these results suggest that while private sanctions are effective for all deputies, public santions are particularly deterrent for the most electorally-exposed deputies (those with no other mandate or holding a high-profile local position).

The next colums (4 to 7) also split the sample by gender and age. No clear differences emerge, except for the absence of reaction by female deputies after public exposure.

Second, in order to further explore the role of media exposure, we run the same heterogeneity analysis based on previous TV and radio appearances (to proxy national fame and exposure) and on local daily newspaper circulation[16] (to proxy exposure to local constituents). Local newspapers are pretty popular in France, with an average of 15 million readers for daily journals[17] (for a total population of about 67 millions). Local newspapers usually cover one or a handful of counties, and report extensively on local politics and deputies. In the average

[16]We use statistics from $ACPM$, the professional organization in charge of collecting reliable sales and ratings of media in France, which generously provided the data.

[17]Statistics are obtained from Alexandre Foatelli (Ina Global) and are available at *http://www.inaglobal.fr/presse/article/l-offre-de-presse-locale-est-tres-inegalement-repartie-en-france-10035*

metropolitan county, 9.7 daily newspapers were sold per 100 inhabitants in year 2009, but there exist large disparities in local newspaper circulation (mean = 9.7 ; SD = 4.1): across the 96 metropolitan counties, circulation ranges from 1.75 (in *Hauts-de-Seine*) to 23 daily sales per 100 inhabitants (in *Finistère*). Figure 5 in appendix shows a map of daily newspaper circulation rates in metropolitan France in year 2009. In the same spirit as Ferraz and Finan (2008) and Besley and Burgess (2002), we exploit this geographical variation in media coverage to investigate whether the publicity of sanctions has more disciplining effects where voters are likely better informed (although we can't rule out other channels).

The results appear in Table 11. The most mediatic deputies at the national level (above-median monthly appearances on national TV and radio from June 2007 to September 2009) and less mediatic ones show pretty similar reactions (columns 1 and 2), both after private and public punishment. However, when we split deputies based on daily newspaper circulation in their county, we find large increases (+33%) in wednesday-morning attendance for deputies elected in a high-coverage county (counties in the top third in terms of circulation) and smaller but still significant (+14%) increases in other areas. The difference in coefficients is however not significant (*p-value* of 14%). Similar results are obtained when the county of Paris (where there are very few local newspapers) is assimilated to high-coverage counties (columns 5 and 6). These results offer suggestive evidence that the disciplining effect of publicly releasing the names of sanctioned deputies is partially mediated by local media circulation, but not by national media exposure.

Table 10: Heterogeneity in Estimates of Public vs Private Sanctions on Wednesday-Morning Attendance

	(1) Full-Time	(2) OtherMandates	(3) LocalExecutives	(4) Male	(5) Female	(6) Over58	(7) Under58
DD	0.605	0.167	0.459**	0.377**	0.198	0.394*	0.250+
	(0.474)	(0.135)	(0.153)	(0.120)	(0.205)	(0.153)	(0.140)
Elig	-0.272	-0.127	-0.552**	-0.300+	-0.113	-0.334+	-0.0933
	(0.477)	(0.184)	(0.201)	(0.159)	(0.240)	(0.195)	(0.166)
Private	0.290*	0.521***	0.527***	0.514***	0.422***	0.504***	0.508***
	(0.141)	(0.0812)	(0.0703)	(0.0571)	(0.0905)	(0.0701)	(0.0698)
Public	0.314***	0.0532	0.193**	0.179***	0.0202	0.136*	0.173**
	(0.0762)	(0.0636)	(0.0612)	(0.0449)	(0.102)	(0.0540)	(0.0644)
Time_FE	X	X	X	X	X	X	X
obs	4174	13677	16391	27764	6478	18332	15910
obs_g	98	251	289	513	125	335	303
r2_o	0.335	0.331	0.319	0.324	0.325	0.322	0.327
mean_y	0.693	0.585	0.566	0.593	0.571	0.579	0.601

Robust standard errors, clustered at the individual level. Random Trend Models using the within estimator on first-differenced data.

Table 11: Heterogeneity By National and Local Media Coverage

	(1) MostMediatic	(2) LessMediatic	(3) High Coverage	(4) Low Coverage	(5) High Coverage + Paris	(6) Low Coverage - Paris
DD	0.366**	0.294	0.616***	0.279	0.286+	0.539*
	(0.120)	(0.198)	(0.145)	(0.178)	(0.164)	(0.262)
Elig	-0.362*	0.0560	-0.672+	-0.145	-0.0721	-0.474*
	(0.148)	(0.236)	(0.380)	(0.175)	(0.259)	(0.239)
Private	0.485***	0.550***	0.598***	0.471***	0.519***	0.648***
	(0.0677)	(0.0801)	(0.0967)	(0.0590)	(0.0888)	(0.0812)
Public	0.127*	0.178**	0.284**	0.130**	0.286***	0.154*
	(0.0619)	(0.0589)	(0.0927)	(0.0472)	(0.0812)	(0.0640)
Time_FE	X	X	X	X	X	X
obs	15497	15564	7955	24604	9205	23354
obs_i	279	267	146	469	168	447
r2_o	0.303	0.346	0.341	0.330	0.340	0.329
mean_y	0.585	0.593	0.651	0.590	0.641	0.591

Robust standard errors, clustered at the individual level. Metropolitan France only for columns 3-6.

6 Conclusion

This paper investigates the effects of a new policy introduced in October 2009 in the French National Assembly. In order to reduce absenteeism in standing committee meetings, small monetary penalties were incurred by most deputies. Comparing the behavior of eligible and ineligible deputies before and after the reform, we find large positive changes in attendance on committee meetings as well as many other measures of parliamentary work. Our DID results suggest that introducing monetary sanctions achieved large reductions in absenteeism with no adverse effects on other activities.

Exploiting the timing of actual punishment of sanctioned deputies (salary cut later followed by media exposure), we are able to disentengle the different effects driving this large net positive result. First, we find that the mere eligibility to sanctions had large disciplining effects. Second, the actual experience of private sanctions also impacted deputies' behaviors : suffering a salary cut had a very large positive effect on future attendance on wednesday mornings. Third, subsequent media exposure further increased attendance.

Looking at the heterogeneity of those effects, we shed light on some of the underlying mechanisms. First, deputies with no other political mandates and multiple-office holders with local executives positions (e.g. city mayors) strongly react to public exposure (and private punishment). Conversely, deputies with politically less-exposed local mandates (e.g. city councilmen) react to monetary punishment but not to public exposure. These results suggest that the disciplining effects of public sanctions concentrate on politicians who are electorally exposed (because they have no other mandates, or because they hold high-profile local positions). Second, we find suggestive evidence that public exposure is mostly mediated by local media coverage instead of national media exposure. One interpretation may be that politicians mostly fear the reactions of their actual constituents, and not public opinion at large.

Overall, our paper shows that labor supply among politicians is malleable and can dramatically respond to monetary as well as reputational incentives, with no apparent adverse effects on intrinsic motivation. Although deputies' image concerns appear instrumental (to secure electorally-exposed positions) and point towards a fear of electoral accountability, it remains unknown whether such concerns are well-founded in practice. Future work should attempt to directly measure the actual electoral consequences of sanctions and investigate whether French voters held deputies accountable in the polls.

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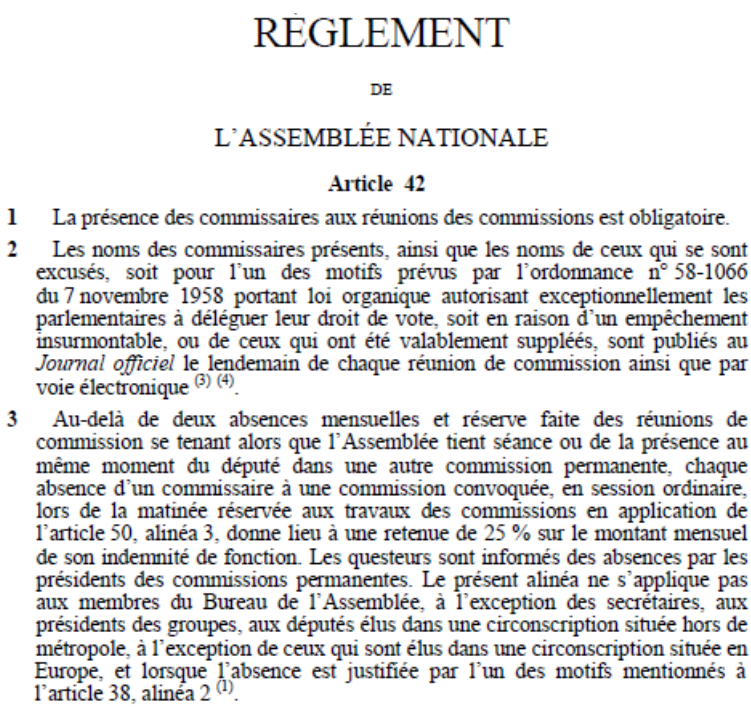
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Appendix

Figure 3: Revised article on monthly monetary sanctions



RÈGLEMENT

DE

L'ASSEMBLÉE NATIONALE

Article 42

1 La présence des commissaires aux réunions des commissions est obligatoire.

2 Les noms des commissaires présents, ainsi que les noms de ceux qui se sont excusés, soit pour l'un des motifs prévus par l'ordonnance n° 58-1066 du 7 novembre 1958 portant loi organique autorisant exceptionnellement les parlementaires à déléguer leur droit de vote, soit en raison d'un empêchement insurmontable, ou de ceux qui ont été valablement suppléés, sont publiés au *Journal officiel* le lendemain de chaque réunion de commission ainsi que par voie électronique [3] [4].

3 Au-delà de deux absences mensuelles et réserve faite des réunions de commission se tenant alors que l'Assemblée tient séance ou de la présence au même moment du député dans une autre commission permanente, chaque absence d'un commissaire à une commission convoquée, en session ordinaire, lors de la matinée réservée aux travaux des commissions en application de l'article 50, alinéa 3, donne lieu à une retenue de 25 % sur le montant mensuel de son indemnité de fonction. Les questeurs sont informés des absences par les présidents des commissions permanentes. Le présent alinéa ne s'applique pas aux membres du Bureau de l'Assemblée, à l'exception des secrétaires, aux présidents des groupes, aux députés élus dans une circonscription située hors de métropole, à l'exception de ceux qui sont élus dans une circonscription située en Europe, et lorsque l'absence est justifiée par l'un des motifs mentionnés à l'article 38, alinéa 2 [1].

Table 12: List of Standing Committees Before and After Reform

Before Oct. 2009	After Oct. 2009
Defense	Defense
Foreign Affairs	Foreign Affairs
Public Finances	Public Finances
Laws	Laws
Economic Affairs, Territories	Economic Affairs
	Sustainable Development, Territories
Cultural, Social and Family Affairs	Cultural and Educational Affairs
	Social Affairs
Number : 6	Number : 8

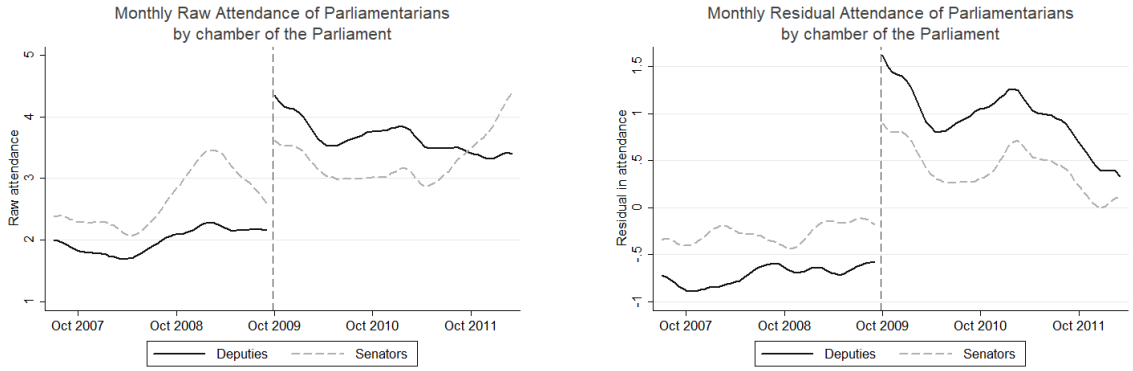
Table 13: Socio-demographic and Political Variables

	Mean (SD)
Age (on 12/1/2009)	57.373 (8.556)
Nb of mandates	2.230 (0.799)
Male (dummy)	0.803
Local Executive Position (dummy)	0.452
as Mayor	0.408
as President of Local Administration	0.135
Party (dummies)	
UMP	57.28
SRC	32.86
NC	4.38
GDR	3.29
NI	2.19
Observations	639

Table 14: Number of Wednesday Mornings with Meeting Scheduled

	mean	sd	min	median	max
Cultural and Educational Affairs	2.556	(1.539)	0	3	5
Sustainable Development and Territories	2.361	(1.379)	0	2	5
Social Affairs	2.333	(1.394)	0	2	5
Economic Affairs	2.262	(1.353)	0	3	5
Laws	2.197	(1.412)	0	2	5
Foreign Affairs	2.131	(1.323)	0	2	5
Public Finances	1.951	(1.284)	0	2	4
Cultural, Social and Family Affairs	1.923	(1.324)	0	2	5
Defense	1.689	(1.272)	0	1	4
Total	2.142	(1.368)	0	2	5

Figure 4: Attendance over Time and Chamber



Note: The fitted lines represent kernel functions of the average number of meetings attended by deputies and senators in the 2007-2012 period (using Epanechnikov smoothing with ± 3-month bandwidth). The left plot uses raw data on attendance, which exhibits an electoral cycle around senatorial elections (Sept. 2008 and Sept. 2011). The right plot uses cycle-corrected attendance, i.e. residuals of chamber-specific regressions of raw attendance on a constant and two dummy variables for the 6-month period before and after senatorial elections. The vertical dashed line represents the original starting date of monetary sanctions in the Assembly (October 2009).

Figure 5: Local Newspapers Circulation

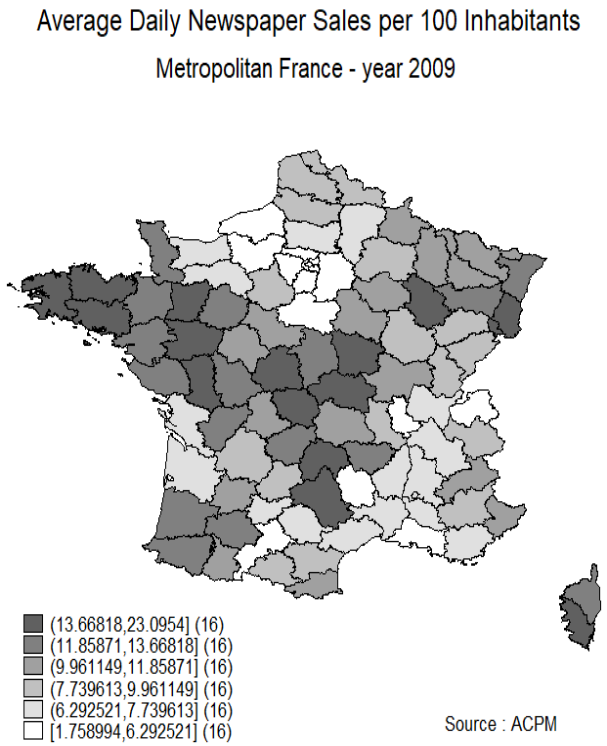
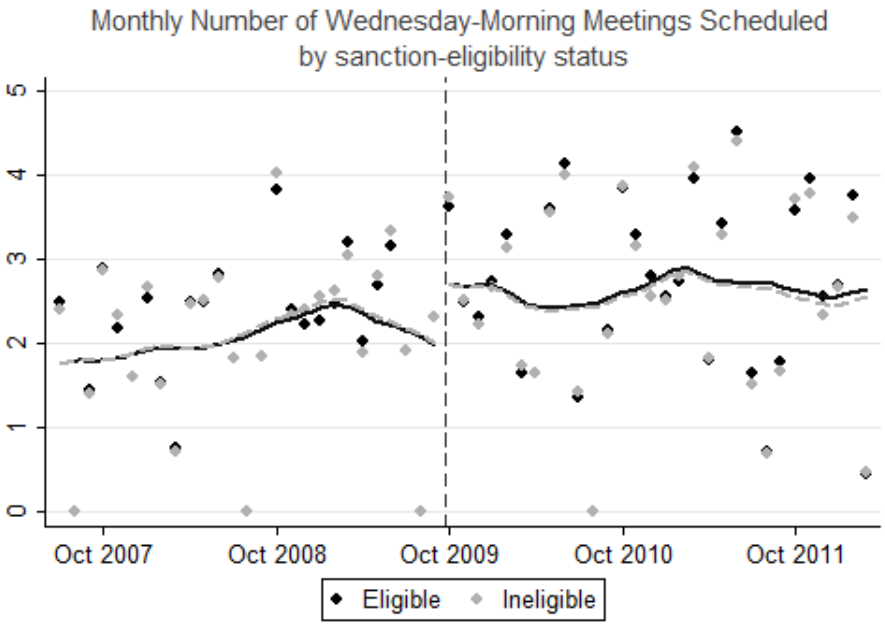


Figure 6: Scheduled Meetings



Note: Each dot measures the number of committee meetings scheduled on wednesday morning for the average deputy in a given month of the legislature. The fitted lines represent kernel functions (Epanechnikov with ± 3-month bandwidth). The vertical dashed line represents the original starting date of monetary sanctions (October 2009).

Table 15: Diff-in-Diff Estimates With/out Committee Fixed Effects)

	(1)	(2)
	WedMorning	WedMorning
DD (After*Eligible)	0.350***	0.338***
	(0.0285)	(0.0287)
Eligible Deputy	-0.0859	-0.0832
	(0.0601)	(0.0618)
Time_FE	X	X
Committee_FE	-	X
obs	34881	34881
obs_i	639	639
r2_o	0.476	0.485
mean_y	0.589	0.589

Robust standard errors, clustered at the individual level.