

# Shirk or Work?

## On How Legislators React to Monitoring

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### Abstract

In 2014 the Swiss Upper House introduced an electronic voting system, which would make it easier to monitor the voting behavior of its legislators. In this system, individual decisions on specific exogenously defined vote types are published automatically, while all other votes are not publicly disclosed. The present paper uses this institutional change to determine, in a quasi-experimental setting, whether the monitoring of parliamentary voting influences legislators' incentives to participate in floor votes. In addition, video recordings of all sessions are used to determine pre- and post-reform attendance rates during secret votes.

Attendance rates increase once legislators are subject to monitoring. This result cannot be explained by anticipation effects of the reform, the introduction of an electronic voting system, or election cycles. Attendance rises more among legislators who depend more heavily on their political career (full-time politicians, those with few interest groups, and incumbents running for re-election) than among their peers with better outside career options. Moreover, when voting is monitored, legislators abstain less and vote more often in line with their party majority.

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# 1 Introduction

Floor attendance rates are frequently taken as a measure of the effort that individual legislators devote to their responsibilities as an elected representative (Dal Bó & Rossi, 2011). Although a great deal of political work happens in committees, voting requires the legislator to be physically present in the chamber to fulfill this aspect of his representative duty.<sup>1</sup>

Images of half-empty assemblies violate the “ideal” picture of the dutiful member of parliament. Attempts to count presence during legislative sessions have found that politicians miss a large number of votes: a third of all votes in Italy (Gagliarducci, Nannicini & Naticchioni 2010); 31% in the UK House of Commons (Besley & Larcinese 2011); and 8% in the German Bundestag (Arnold, Kauder & Potrafke 2014).

Attendance rates are calculated from official voting records. Whether presence in parliament can be detected thus hinges upon absences being recorded and being made publicly accessible. Regulation regarding the transparency of legislators’ behavior varies widely by assembly (Hug 2010; Hug, Wegmann & Wüest 2015): 25% of 92 parliaments covered in the study are completely non-transparent; 22% disclose all voting records; and the remaining share imposes certain restrictions on their publication.

I address the question of whether the possibility of tracking legislators’ presence during voting sessions impacts their attendance. As a quasi-experiment, I exploit an institutional change in the voting procedure of the Swiss Upper House, the *Ständerat*, which improved monitoring of legislators for several types of votes, but kept transparency constant for the remaining ones. Before 2014, members of the Swiss Upper House voted exclusively by a show of hands. Although all sessions were video recorded, neither the media nor researchers have ever undertaken the time-consuming and laborious work of documenting attendance rates in this chamber to date.<sup>2</sup> Attendance has not been publicly monitored.

All votes in the Upper House have been taken electronically since 2014. Individual decisions on several types of votes—emergency votes, debt brakes, ensemble votes, and final passage votes—are automatically published online in PDF format. As a result, it is now easier to monitor legislators’ attendance for these votes; the independent policy platform *Politnetz* has started publishing attendance rates of the Upper House on their website. In contrast, all other types of vote remain

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<sup>1</sup> The male form encompasses the female legislator as well.

<sup>2</sup> In contrast, the Swiss media regularly report on attendance rates in the Lower House, which publishes all voting results and is therefore easily traceable.

undisclosed. Comparing absences during the two types of votes both before and after the change in voting rules makes it possible to estimate a causal effect of monitoring on attendance. Since this change was introduced midway through the 2011–2015 legislative period, the composition of parliament has remained largely the same. It is therefore possible to compare the voting behavior of the same politicians under different monitoring technologies. As the reform does not coincide with elections, candidate-selection motives induced by monitoring changes can be excluded.

The reform was introduced as a consequence of increased media pressure after repeated counting mistakes were made public. It can thus be argued that the reform occurred for reasons exogenous to monitoring legislators’ attendance and voting decisions. Nevertheless, the Upper House legislators had to ratify the transparency bill in a vote. To address endogeneity issues, I show that legislators accepting the bill did not react differently to the reform than those who rejected it.

Absenteeism can be either *excused* for an entire day or *selective*, i.e. legislators are present at the beginning of a session, but leave their seats during debates and speeches, returning only for certain votes. They use this time away from the floor to meet with interest groups, carry out work unrelated to politics, or simply to take a break. The variation used for identification takes place at vote level. Therefore, selective absences during an ongoing parliamentary meeting are more relevant and will be the main focus of this paper. Moreover, I will show that such absences are empirically more relevant, as they account for the vast majority of floor absences.

Since the monitoring changes were introduced, attendance during transparent votes has increased by an average of 2.6 percentage points, from initial levels of 82%. This corresponds to a reduction in absences by an order of magnitude of 12%. In falsification exercises, I show that the result cannot be explained by the anticipation of the approaching transparency change, the introduction of an electronic voting machine, or election cycles.

The positive effect of monitoring on attendance could be rationalized in several ways. I explore two avenues in more detail: first, the value of a political career and legislators’ employment options outside politics; second, changed incentives due to transparency in the framework of committee voting.

Presence in parliament can be viewed as both a form of accountability towards voters, and *participatory shirking* a departure from it (Rothenberg & Sanders 2000).<sup>3</sup> Intuitively, if there are no means of identifying whether legislators are shirking their duties to the electorate or not, the

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<sup>3</sup> Accountability and shirking can come in many guises. Given that legislators are seen as representatives of electoral preferences, violating the electoral connection by voting against constituency interest is known as *ideological shirking*.

former have little incentive to attend all voting sessions. Instead, they will resort to participating in the most important votes while forgoing those which are less crucial. Yet, if voters have the means to find out whether politicians are fulfilling their representative duties, the former may decide to punish their representative for shirking. Say, for example, that there is negative press coverage of low attendance rates among members of parliaments, voters could respond by not reelecting their representative in retaliation for his lack of effort. This rationale is particularly appealing in settings that allow for the earning of non-political rents like the pursuit of a non-political occupation or fostering relationships with interest groups—which is the case for the Swiss Upper House. Compared to voting participation, these activities might offer more attractive rents either in the short run or in terms of the representative’s post-political career.

Results provide some evidence that such a mechanism might be at work. The increase in presence is particularly pronounced among full-time politicians (in contrast to those with outside occupations), legislators with few interest groups (who are less well connected with firms), and those running for reelection (in contrast to retiring ones). All of these groups share a common feature: they are more dependent on their political careers and theoretically more accountable to their electorate. In contrast, legislators with alternative occupations or many interest groups might have better outside career options in the event that they are not reelected.

For the second potential mechanism regarding the theory of voting in committees, the intuition is that absences and abstentions (i.e. being present but voting neither yes or no) are substitutes under secret voting; but they are not under transparency since abstentions count towards attendance rates. Non-voting may be optimal when a legislator is uncertain about the best vote choice and wants to delegate the decision to more knowledgeable colleagues, a situation known as “the swing voter’s curse” (Feddersen & Pesendorfer 1996). Under transparency, however, uninformed legislators might nevertheless have an incentive to vote yes or no in order to avoid being identified as such (Mattozzi & Nakaguma 2017). In contrast, if casting a valid vote is all that would suffice to improve one’s attendance record, then the legislator could simply abstain, insofar as such action would not affect the aggregate voting result.

I find evidence that strategic considerations regarding vote choices might be at play. On average, legislators are not only more likely to be present for votes but also less likely abstain if the process is monitored. This would indicate that there are other motivations involved than simply the desire of the legislator to improve his attendance record. In fact, legislators tend to reject legislation more often, under such circumstances. This reflects greater adherence to the party line taken by the

party majority. The effect is predominantly present among legislators who were not members of the committee responsible for the bill and might thus be less well-informed on the subject. However, the reduction in abstentions is observed among committee members, too.

This paper links in to several strands of the literature. It extends research dealing with the effects of transparency in general (e.g., Fox 2007; Prat 2005), and in the context of legislative voting in particular. Benesch, Bütler and Hofer (2015) focus on a different aspect of the transparency reform in the Swiss Upper House. They find more voting in accordance with party lines during final passage votes, once individual voting decisions become observable. Lindstädt and Vander Wielen (2011) provide empirical evidence for electoral cycles in monitoring through increased information provision by the media and political elites during elections, which in turn impacts legislator voting. In a setting in which leaders of different abilities provide a public good, Grossman and Hanlon (2014) find a u-shaped relation between monitoring intensity and the amount of the public good produced.

The main contribution of this paper is to provide empirical evidence for the causal effect of monitoring on legislators' effort. With the exception of the experimental evidence provided by Mattozzi and Nakaguma (2017), empirical investigations in the above literature rely on panel or cross-sectional data analysis. In contrast, the empirical strategy in this paper is based on a quasi-experimental setting. It exploits exogenous variation in monitoring and makes it possible to capture a causal relationship. The authors analyze committee voting with and without transparency depending on expertise, pre-existing biases and career concerns.<sup>4</sup>

A further strength of the paper is the use of individual vote-level data. This type of detailed data, which can be matched with legislator and vote characteristics, is unavailable in many settings, and research often needs to resort to using attendance data aggregated over a certain time period (e.g. month or year). I show that results change when legislator and vote characteristics are controlled for. Moreover, I show that taking into account the timing of votes is crucial. Attendance is different for votes that are taken "independently" or in "blocks" of consecutive votes, irrespective of whether the vote is being monitored or not. This paper contributes to the understanding that institutional features defining how votes are being taken might have (potentially undesired) effects on attendance and thus vote results.

This paper also complements the literature exploring the determinants of effort and presence

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<sup>4</sup> A closely related literature deals with transparency in monetary policy. A number of contributions uses variation in the publicity of debates in the US Federal Open Market Committee (Hansen, McMahon & Prat 2017; Meade & Stasavage 2008; Swank, Swank & Visser 2008).

in parliament. Research has focused heavily on the impact of salaries (Fisman et al. 2015; Mocan & Altindag 2013), outside earnings<sup>5</sup> (Arnold, Kauder & Potrafke 2014; Gagliarducci, Nannicini & Naticchioni 2010), electoral competition (Bernecker 2014; Galasso & Nannicini 2011), and institutions (Gagliarducci et al. 2011) on floor voting participation.

The remainder of the paper is structured as follows. The institutional design and transparency reform are explained in Section 2. The research design is in Section 3, data and descriptives follow in Section 4. Results are reported in Section 5. Section 6 offers concluding remarks.

## 2 Institutional Background and Descriptives

### 2.1 The Swiss Upper House

Switzerland has a bicameral parliamentary system. The Lower House (Nationalrat) is elected through a proportional system. The Upper House (Ständerat)—the focus of this paper—represents Switzerland’s 26 states, or cantons. All but two cantons hold majoritarian elections. Each of the 20 full cantons is represented by two legislators, and the six half-cantons hold one seat each, bringing the total to 46 members of the Upper House.

The 2011–2015 legislative period is used for the empirical examination. The members of the 49th Upper House<sup>6</sup> were on average born in 1956, the oldest in 1945 and the youngest in 1979. Almost 20% are female and the majority of 74% has German as their official language.<sup>7</sup> 27% hold a doctoral degree which is significantly above the Swiss average of 2.3%.<sup>8</sup> About one third holds an officer rank in the Swiss army. 74% are married and have an average of 2 children.

The Swiss Parliament is traditionally viewed as a militia assembly. The original idea rests on the notion that citizens should engage themselves in politics or voluntary activities on top of their paid employment. Although both chambers have undergone significant professionalization in the past decades (Müller 2015), the option of pursuing a paid outside career alongside holding a seat in parliament still exists and more than two thirds of Upper House members do so. The most commonly encountered professions are lawyers (32.4%) and entrepreneurs (17.6%); roughly one

<sup>5</sup> Geys and Mause (2013) provide a survey of moonlighting in parliaments and also discuss the connection with legislative effort.

<sup>6</sup> During the legislative period, three of the 46 members left the chamber prematurely, either on health grounds or due to their appointment to the Federal Council (Swiss cabinet); they were replaced by three new members. Table 14 in the Appendix lists the names of the successors and the dates on which they entered the Upper House.

<sup>7</sup> Note that legislators in the Upper House speak German, French and Italian. While the first two dominate, legislators typically speak in their preferred language during debates.

<sup>8</sup> The number is calculated as a percentage of the population older than 15 years. It was issued, on request, by the Swiss Statistical Office and reflects the status quo in 2014.

Table 1: DESCRIPTIVE STATISTICS OF INDIVIDUAL LEGISLATOR CHARACTERISTICS

| Variable             | Mean  | # Legislators | Std. Dev | Min. | Max.  |
|----------------------|-------|---------------|----------|------|-------|
| Year of birth        | 1956  |               | 7.572    | 1945 | 1979  |
| Female               | 0.184 | 9             | 0.391    | 0    | 1     |
| German               | 0.735 | 36            | 0.446    | 0    | 1     |
| Doctor               | 0.265 | 13            | 0.446    | 0    | 1     |
| Officer              | 0.347 | 17            | 0.481    | 0    | 1     |
| Married              | 0.735 | 36            | 0.446    | 0    | 1     |
| Children             | 1.98  |               | 1.49     | 0    | 6     |
| Full-time politician | 0.286 | 14            | 0.456    | 0    | 1     |
| Interest groups      | 8.37  |               | 6.20     | 1    | 30    |
| Years in parliament  | 7.57  |               | 6.08     | 0    | 25.53 |
| First term           | 0.184 | 9             | 0.391    | 0    | 1     |
| Retiring             | 0.239 | 11            | 0.431    | 0    | 1     |

NOTE: Descriptive statistics based on 49 legislators who were part of the chamber at some point during the 2011–2015 legislative period. *Retiring* is based on the 46 legislators who have been in the Upper House since 2014. Mean is the average values and # Legislators shows the number of legislators for whom the variable takes on a value of 1. Source: Swiss Parliament website [www.parlament.ch](http://www.parlament.ch).

third of members are full-time politicians. Moreover, legislators reported ties to an average of eight interest groups. At the beginning of the 49th legislative period, they had served an equivalent of two terms in parliament. 19% were in their first term; three quarters were running for reelection in 2015; 11 retired afterwards.

Table 1 summarizes the descriptives of individual legislator characteristics. All personal information comes from the Swiss Parliament website.

## 2.2 Institutional Change: Increase of Transparency

Traditionally, the Upper House has voted by a show of hands. The chamber’s president announced the voting alternatives sequentially and legislators raised their hands for their preferred alternative. Two designated members of the Upper House served as vote counters, and the aggregate voting outcome was documented in the chamber’s minutes, which are publicly accessible. Additionally, since 2006 all sessions have been videotaped and can be watched online. Other than through the laborious screening of video records it was virtually impossible to monitor individual legislators’ attendance and voting decisions. Only a restricted number of seats is open to the public when the House is in session. Thus, pre-reform monitoring would have been possible, but at a high cost. Indeed, while attendance rates in the Lower House—which has recorded and published all votes

since 2007—have been covered extensively by the media (e.g., 20 Minuten 2012a, 2012b; Neue Zürcher Zeitung 2014; SRF 2012),<sup>9</sup> there was nothing comparable on the Upper House.

The introduction of a more transparent voting system was preceded by a long and heated debate. Although initially rejected by the majority of members, the transparency bill<sup>10</sup> was resumed after counting mistakes were repeatedly uncovered and prominently publicized by the media in winter 2012. It can thus be argued that the transparency bill was revived by an exogenous shock, i.e. media pressure. While the councilors generally agreed that a reduction in counting errors was desirable, concerns were voiced over the potential weakening effect it would have on the debating culture of the chamber. Increased party pressure and the possibility of the media producing rankings and statistics were cited as the main arguments against publishing voting records (Staatspolitische Kommission des Ständerates 2012). The new voting system was approved with a majority of 28 to 14 in spring 2013, and launched one year later after installation of the electronic voting machine. Later on, I will return to the fact that the new voting system required approval by a majority of Upper House members, and show that this is not an issue for identification.

All votes in the Upper House have been taken electronically since spring 2014. The system is operationalized by a set of three buttons at every legislator’s desk. Each vote lasts for 30 seconds during which vote choices can be adjusted flexibly. A flashing mark on three clearly visible electronic boards signals the last eight seconds of the vote. The boards display a seating chart of the chamber, and vote choices (green for yes, red for no, white for abstain) appear in real time and are visible to everyone present in the chamber.

The transparency of the voting process was affected as follows. While all votes are conducted electronically, individual decisions on four types of votes are published automatically on the Swiss Parliament website in PDF format: (a) *final passage votes* (ultimate decision on acceptance/rejection of bill); (b) *ensemble votes* (votes taking place after several paragraph-by-paragraph votes); (c) *debt brakes* (required for one-time expenses above CHF 20 million or recurring expenses above CHF 2 million); (d) *urgent votes* (bills requiring immediate implementation). The PDF displays information on how each individual legislator has voted (yes, no, abstain), whether he was excused or did not participate in the vote, and who was the acting chamber president. I will refer to vote types (a)–(d) as *Public* votes. All others, including detail and procedural votes, which remain

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<sup>9</sup> While most are high-circulation tabloids, they also include respected broadsheets. They published rankings naming the least and most dutiful legislators in the Lower House.

<sup>10</sup> The bill was a parliamentary initiative (bill number 11.490) launched by This Jenny, a member of the Upper House. All debates can be found in the Parliament’s minutes, the “Amtliches Bulletin” (Parlamentsdienste), which are available online ([www.parlament.ch](http://www.parlament.ch)).

undisclosed even after the reform, will be referred to as *Secret* votes.<sup>11</sup> The distinction of *Secret* vs. *Public* votes thus relates to whether a vote type is automatically published or not at *some* point in time.

Votes are decided with an absolute majority rule of legislators present during the vote (excluding abstentions). Exceptions are debt brakes and urgent votes which require the approval of the majority of members of the Upper House. For such a vote to take place, the presence of at least 24 legislators is required. In theory, the decision to be present during such a vote could thus be affected. In practice, however, turnout is typically above the required threshold (cf. density plot in Figure 4 in the Appendix). For robustness, I show that results do not change qualitatively when votes requiring a quorum are dropped (Table 15 in the Appendix).

### 3 Research Design

The goal is to measure the impact of vote monitoring on floor attendance. Identification is based on the variation of monitoring technology for some votes (public votes, the treatment group) while the remaining ones are never disclosed (secret votes, the control group). Control and treatment are thus defined according to vote type, and legislators are observed during both types before and after the transparency reform. Comparing attendance during pre- and post-reform *public* votes is likely to be inadequate. Time-variant factors unrelated to monitoring—like time trends, common shocks due to news or events, or varying bill characteristics—might be at play. Optimally, the control and treatment group would comprise the same set of votes which remained undisclosed during the observation period. While *secret* votes almost meet this requirement in terms of being constantly unrecorded, some (potentially unobserved) differences between vote types exist. Identification is unproblematic as long as these differences do not change over time.

Allocation to the control or treatment group must be exogenous for valid inference, which is the case as I will argue below. Whether a vote is (or would have been) published is defined by its legal form. The type of vote cannot be manipulated and is prescribed by the law. This was confirmed in personal communication with the Secretary of the Upper House.<sup>12</sup> Alternatively, members of the Upper House could try to manipulate the timing of vote types: e.g. if legislators were concerned

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<sup>11</sup> Before and since 2014, ten members of the Upper House are needed to request a recorded vote on any type of vote. However, legislators hardly ever make use of this option.

<sup>12</sup> Contact in September 2017. The Secretary further mentioned that the allocation of votes is common practice and not questioned by the members of the chamber.

about their absences being recorded during public votes, they might have tried to push through as many public votes as possible before the reform. The reverse would be the case if legislators wanted to show off with high attendance or force other legislators to reduce their absences by postponing public votes. To address this concern, I compare the annual shares of public votes around the reform (cf. Figure 1). There is a general downward trend the share of public votes but without discontinuities. Between 2013 and 2014, the share drops only slightly from 30.5 to 28.9%, which is an insignificant change (p-value of t-test: 0.658). This would suggest that the timing of the public votes was not manipulated in anticipation of the reform. In the Results Section I also present placebo regressions around the time when the reform was accepted (which turn out to be insignificant). Moreover, the responsibility for agenda-setting lies with the Upper House's office and not individual legislators. The Secretary cited purely practical reasons like availability of government members for debates and time constraints when deciding on the voting schedule.

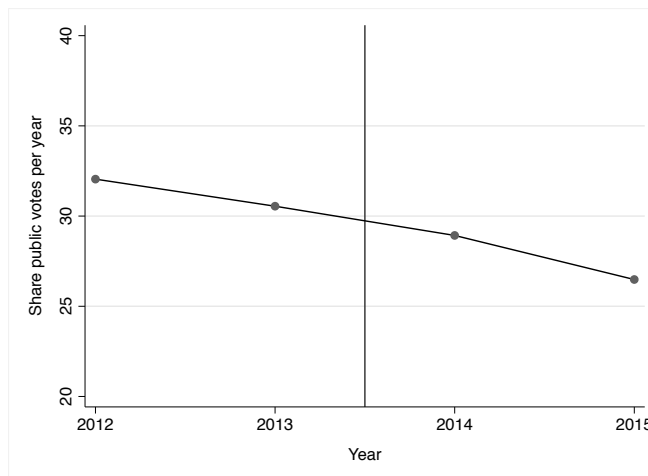
Lastly, as argued before, it was media pressure due to counting mistakes that has ultimately led to the acceptance of the bill in the Upper House. Nevertheless, it remains a concern that the reform was introduced by the ones who were affected by it. Suppose only those politicians accepted the reform who planned to increase their presence even in the absence of the reform. Then the estimated effect would not be causal but capture the legislators' intention to reduce their absences. To address this issue, I estimate the main equation (1) interacted with an indicator taking on value 1 for legislators accepting the transparency bill in the final passage vote in spring 2013. There is no statistically significant difference in treatment effects between these subgroups speaking against reform endogeneity (cf. Table 15 in the Appendix).

Absence during votes can take one of two forms: excused absences during an entire voting day or selective absences during some votes. *Excused absences* are controlled daily by a roll call at the beginning of each day (Standing Orders of the Council of States 2016). Preferably, legislators should inform the House's secretary about their absence in advance. Excused members thus miss an entire day and all of the votes taken on that particular day. The two reasons for excused absences recorded in the data are illness (37% of excused absences) and maternity leave (13% of excused absences).<sup>13</sup> There is no explicit cause of absence documented for the remaining 50%. This residual category encompasses absences on other grounds such as committee work and traveling abroad. However, these cannot be directly attributed to more specific causes. For 59% of excused missing days, spells of absence last longer than one day (it's 91% and 100% when the recorded

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<sup>13</sup> It is due to one legislator taking her two statutory maternity leaves.

FIG. 1: SHARES OF PUBLIC VOTES PER YEAR



NOTE: The line shows the share of public votes out of total votes. The vertical line marks the reform. Based on the sample of votes used for estimations. All votes taken in the winter session 2011 are added to observations from 2012.

reasons are illness or maternity respectively), speaking against strategic motifs. Missing only single days is most likely among the category of unrecorded causes (72%). While this may fit with official meetings or travel, it would also be the category most prone to strategic considerations.

*Selective absences* describe legislators' failed appearance for a vote even though they were present during the roll call.<sup>14</sup> During the meetings, legislators are free to leave the chamber at any time. Most make use of this time to work or hold meetings with interest groups and lobbies. Other activities include private meetings, appointments with voters, school classes or simply taking a break.<sup>15</sup> Given the variation in monitoring by vote type, *selective absences* are the natural choice to look at, because attendance can vary across type. In contrast, excused absences cannot discriminate between vote types as all votes on a given day are missed. Selective absences are also the empirically more relevant phenomenon as they cover most of the recorded absences. In the Results Section, I show that the inclusion of excused absences has little influence on the results.

For the transparency reform to be effective, two conditions are required. First, legislators need to know that a vote is going to take place so that they can make a conscious decision to attend or not. Curiously, shortly before a vote a bell activated by the chamber president or the secretary signals

<sup>14</sup>Fisman et al. (2015) report that members of the European Parliament only signed the register in the morning to secure their daily allowance and subsequently left the parliament for the entire day. Such behavior on a regular basis is uncommon in Switzerland. Legislators would typically remain within reach of the chamber.

<sup>15</sup>This information is based on two interviews with members of the Upper House conducted by the author of this paper. Both legislators independently mentioned most of the points listed above.

to the legislators that they should return to the chamber. It is only audible in the antechambers adjacent to the main chamber. If councilors spend their time in the parliament’s cafe, they can follow the meetings on TV screens. However, these are not equipped with a sound system.<sup>16</sup> In theory, councilors have the opportunity to return to the chamber in time for the vote.

Second, since the reform legislators need to be aware of whether a vote will be secret or public in order to be able to discriminate between the two. Legislators confirmed in interviews that this is possible thanks to a daily schedule that all legislators receive before the meetings. Moreover, in 2014 a SMS service was introduced that sends voting announcements to the legislators’ phones.

Additionally, several institutional factors facilitate identification. First, complete video recordings of all legislative sessions exist, which means that the sample is not selective.<sup>17</sup> Second, the focus on a single legislative period ensures an almost identical composition of the chamber. By controlling for individual legislator fixed effects, I estimate the within variation of attendance while accounting for all time-invariant legislator characteristics. Among them are demographic and socio-economic factors like gender, age, marital status, or number of children, as well as political variables like party, canton, or tenure. Third, since the institutional change takes place almost in the middle of the legislative period, the common support assumption, which requires a sufficient number of observations in all possible subgroups defined by treatment and control, is thus fulfilled.

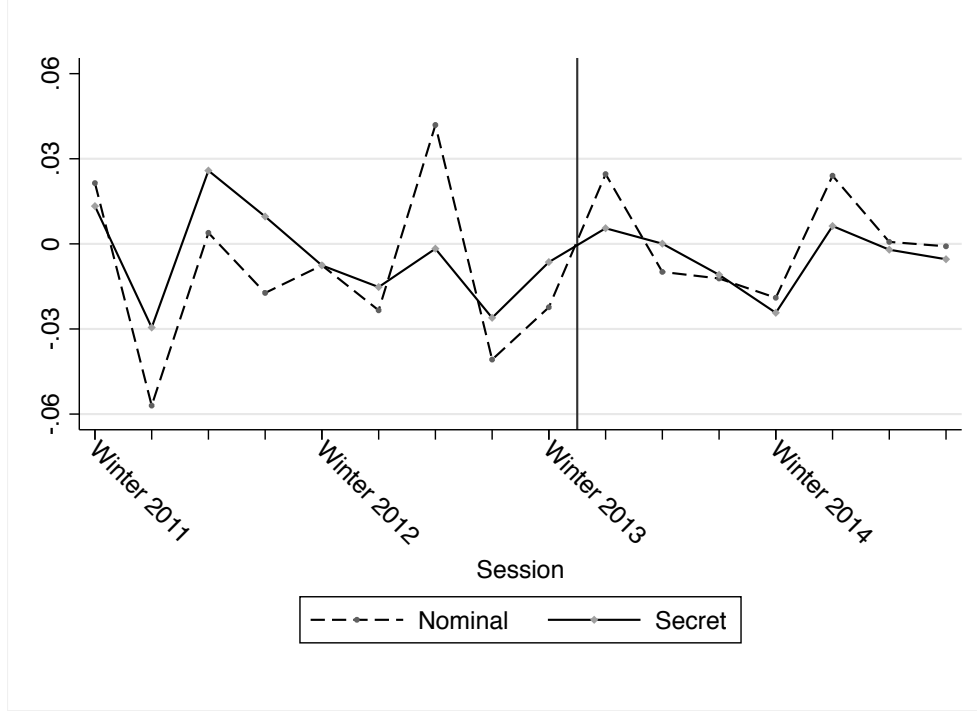
The institutional change affecting vote transparency makes it possible to use a difference-in-difference regression to estimate the causal effect of monitoring on floor attendance. The dependent variable  $Present_{ij}$  takes on value 1 if legislator  $i$  was present during vote  $j$ , and 0 if he was absent.  $Public_{ij}$  takes on value 1 if vote  $j$  has a vote type that is published after the reform. It is 0 for all secret votes.  $Reform_{ij}$  is defined as 1 for all votes taken after the change in voting procedures in 2014, and 0 before the reform. The effect of publishing vote records on floor attendance is estimated with the following difference-in-difference regression:

$$Present_{ij} = \alpha + \beta_1 Public_{ij} \times Reform_{ij} + \beta_2 Public_{ij} + \beta_3 Reform_{ij} + \xi X_{ij} + u_i + \tau_s + \epsilon_{ij}$$

<sup>16</sup>Traditionally, a member of the cabinet is the last to speak before a vote. Seeing such a speech on the screen is therefore an indicator of an approaching vote.

<sup>17</sup>Recorded votes might constitute a selective sample of the universe of votes (Carrubba, Gabel & Hug 2008; Carruba et al. 2006; Hug 2010). The bias is particularly strong if recorded votes are conducted on request: members of parliament are called to order by the mere appearance of a roll call vote and consequently are present more often during recorded votes. The bias is less pronounced in countries like Italy where most votes are taken electronically (cf. description by Gagliarducci et al. 2010).

FIG. 2: INSPECTION OF COMMON TREND ASSUMPTION



NOTE: The x-axis is a continuous indicator of voting sessions. The y-axis shows the aggregate residuals per session and type of vote of a regression of *Present* on a set of covariates.

Let  $\alpha$  be the intercept,  $\beta_1$  to  $\beta_3$  the main coefficients,  $X_{ij}$  a vector of control variables and  $\xi$  its vector of coefficients,  $u_i$  legislator fixed effects,  $\tau_s$  season fixed effects (each vote  $j$  can be uniquely attributed to a season  $s$ ) and  $\epsilon_{ij}$  the error term. The coefficient  $\beta_1$  identifies the average treatment effect on the treated under several assumptions, which I will detail below.  $\beta_2$  captures the pre-reform difference in attendance between public and secret votes that remains unaccounted for by control variables.  $\beta_3$  is the average change in presence during secret votes around the reform.

The most important identifying assumption is a common pre-trend in absences between public and secret votes:

**Assumption 1 (Common pre-trend conditional on covariates)** *If the voting system in the Upper House had not changed, the difference in attendance between public and secret votes, conditional on covariates, would have evolved in the same way as during the pre-reform period.*

Although the validity of this assumption cannot be investigated in full since it relies on unobserved potential outcomes, it is possible to examine the development of pre-reform trends in the control and treatment groups. I regress the dependent variable  $Present_{ij}$  on a set of control variables

(which I detail in Section 4) for each of the four subgroups defined by public/secret votes and pre/post reform. I then calculate the residuals, which are attendance unexplained by covariates. Figure 2 shows the development of the mean residuals aggregated by voting session over time. The vertical line marks the reform. Control and treatment groups evolve in parallel during most of the pre-reform periods, providing suggestive evidence for the validity of the common trend assumption.

The next assumption relates to the fact that not only have the rules regarding the publication of individual voting decisions changed, but also the procedure has switched from voting by a show of hands to electronic voting for all types of votes.

**Assumption 2 (Electronic Voting)** *If electronic voting had an effect on the probability of being present, the pure “electronic voting effect” should be the same for public and secret votes.*

The assumption states that electronic voting should have affected the treatment and control groups in the same way even if the transparency reform had not been introduced.<sup>18</sup> If electronic voting (without public disclosure) had led to a greater increase in attendance during public votes than during secret ones, the treatment effect would be upward-biased (which, in anticipation of a positive average treatment effect, would be the problematic case). This is, however, unlikely since secret votes tend to be more important/contested. One indication of this is on average higher attendance during secret votes even prior to the reform, as I will document in the Results Section.

## 4 Dataset and Descriptives

The data span an entire legislative period commencing in December 2011 and ending in September 2015 (49<sup>th</sup> legislative period). The Upper House meets four times a year during pre-assigned dates in the spring, summer, fall and winter sessions. Each session lasts three weeks. Meetings take place from Monday afternoon to Thursday. Most are in the morning, though sometimes two meetings a day are scheduled. Final passage votes are decided upon almost exclusively on the last Friday of a session. This results in at least 13, and at most 15 meetings per session. All sessions and meetings are chronologically reported in the parliamentary minutes (Amtliches Bulletin). They cover all debates and votes including their aggregate voting outcomes. The type of vote is documented as well, making it possible to assign a vote to the public or secret category.

The data encompass a total of 2,041 votes, as summarized in Table 2. It corresponds to an average of nine votes per meeting. 56% were secret votes, the remainder were public ones. Among

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<sup>18</sup>This follows from de Chaisemartin and D’Haultfoeuille (2017): when the treatment “electronic voting” is equal in both treatment and control group, the estimator collapses to the standard difference-in-differences case.

Table 2: DESCRIPTIVE STATISTICS OF VOTE CHARACTERISTICS

| Category    | Vote type   | Obs. | Percentage |
|-------------|-------------|------|------------|
| SECRET      |             | 1137 | 56         |
| PUBLIC      |             | 904  | 44         |
|             | Final       | 302  | 33         |
|             | Total       | 438  | 49         |
|             | Debt        | 159  | 18         |
|             | Urgent      | 5    | 1          |
| INDEPENDENT |             | 1040 | 51         |
|             | Secret      | 819  | 79         |
|             | Public      | 221  | 21         |
| BLOCKS      |             | 1001 | 49         |
|             | Secret      | 318  | 32         |
|             | Public      | 683  | 68         |
|             | Only secret | 183  | 18         |
|             | Only public | 503  | 50         |
|             | Mixed       | 315  | 32         |

NOTE: Descriptive statistics. Source: Swiss Parliament website [www.parlament.ch](http://www.parlament.ch).

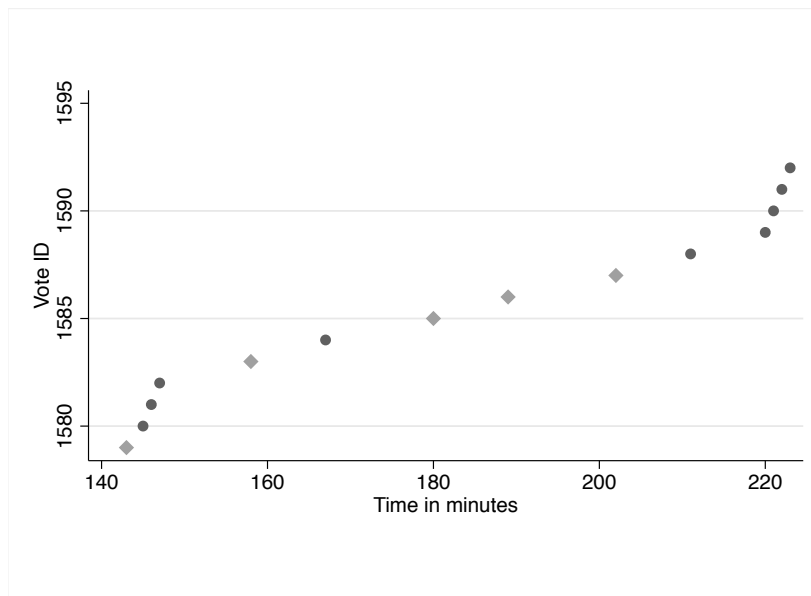
the public votes, 49% were total votes, 33% final passage votes, 18% debt brakes and 1% urgent votes.

Although many votes appear as single, *independent* votes (51%), clusters of several votes in a row are equally frequent. Defining a *block* as consecutive votes without interruption by speeches or debates, there are on average 1.8 votes and up to 29 votes in such a block. An exemplary distribution of votes on a randomly selected day (29 May, 2012) can be found in Figure 3. It shows the occurrence of public votes (points) and secret votes (diamonds) over time in minutes. Nine out of 14 votes were public; the first four votes are executed one after the other in a block. They are followed by six independent votes every nine to 13 minutes. The meeting finishes with a block of four consecutive votes.

Independent votes are most likely to be secret (79%). Public votes (68%) are more likely than secret ones to be taken in blocks. Vote blocks either exclusively comprise secret votes (28%), public votes (50%), or a mix thereof (32%).

Individual attendance within a block is heavily correlated: legislators' presence during the first

FIG. 3: DISTRIBUTION OF VOTES OVER TIME DURING A MEETING



NOTE: Vote patterns on 29 May, 2012. Occurrence of public (point) and secret (diamonds) votes. The x-axis shows time in minutes. The y-axis shows a continuous vote ID.

vote of a block is a good predictor of their presence during the remaining votes in the same block: in 97% of cases legislators attend either all or no votes during the same block. Therefore, two adjustments to the dataset are required. First, I measure average attendance at the level of the voting block instead of the individual vote to ensure that long and short voting blocks have the same weight in the regressions. Second, I exclude all 315 votes taken in mixed blocks. The reason is the possible spill-overs in attendance from public to secret votes, and vice versa. The influence of transparency could not be unambiguously captured. Therefore, all my results are conditional on independent votes or blocks of votes that comprise only public or secret votes. In the interests of completeness and comparison, I also report results without these data adjustments in the Results Section.

All final passage votes are dropped from the analysis: they are taken consecutively in a block on the last session day. Absences are very low on average (2.54%), and are mostly excused (83% of absences). Final passage votes thus constitute a special case regarding attendance, and no impact of monitoring on selective presence would be expected. I also check whether excused absences on the last session day change after the reform compared to all other days: there is no significant change (not reported).

Before the reform, a camera captured the councilors during the voting process. The videos allow me to construct a dataset of individual attendance per vote prior to electronic voting.<sup>19</sup> Legislators are counted as present if they are in the chamber during a vote. They are treated as absent if they have left the chamber. In 769 cases (0.8%) it is impossible to observe legislator presence because not all members were captured on film. From aggregate voting results, it can be inferred in 268 of cases whether a legislator was actually present in the chamber even though he was not captured on camera. Results are the very similar regardless of whether these inferred observations are used or not (Table 15 in the Appendix).

Since 2014, when electronic voting was introduced, all sessions have been recorded on video. The difference with regard to filming lies in the camera capturing one of the electronic boards during the vote instead of the legislators. I use the final voting outcome displayed on the board to code whether a legislator attended a vote or not.

Pre-reform attendance is thus determined based on physical presence in chamber. Post-reform presence reflects voting, i.e. pressing one of the electronic buttons. In theory, it is possible that a legislator is physically present during a vote but does not vote. In the pre-reform data, such legislators would be coded as present, but they would be recorded as absent after the reform; attendance after the reform would on average be lower. In anticipation of a positive estimated treatment effect, this would only downward-bias the coefficient. Moreover, in the pre-reform data it is possible to check whether legislators did not vote though they were present for all cases when yes votes, no votes and absences were fully counted. There are 36 such instances or 0.13% of observations, which suggests that present legislators almost always vote.

There are two special cases regarding the coding of absences. First, the chamber presidents—who are elected members of the Upper House—do not actively participate in votes, though there are a few exceptions. They chair the session, and make all announcements during their one-year term. Second, vote counters performed a special institutional role in the pre-reform period and are thus mostly present in the chamber. I therefore control for the acting chamber president and vote counter in the regressions.

As further control variables, I use vote and individual controls. The vote controls are: time of the vote during the—while some meetings are relatively short, others can last for several hours

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<sup>19</sup>The video records of the Upper House have been used to explore the quality of political representation and voting patterns in this chamber (e.g., Benesch, Bütler & Hofer 2015; Bütikofer 2014; Eichenberger, Stadelmann & Portmann 2012; Hug & Martin 2011; Portmann & Stadelmann 2013; Stadelmann, Portmann & Eichenberger 2013, 2014).

(particularly long meetings might induce legislators to leave earlier); the number of votes in a voting block; an indicator for Monday since legislators arrive on that day; and a linear session time trend. Next to controls for chamber presidents and vote counters, I use the following individual controls: an indicator for legislators' membership in the committee responsible for the bill as a sign of the legislator's expertise on the subject matter; an indicator for the authorship of the bill, i.e. the legislator who initiated the bill; an indicator if the councilor's party or another member thereof proposed the bill. All three controls could be expected to have a positive influence on presence during votes.

Presence accounts for 87% of individual observation, i.e. legislators miss one in eight votes. On average, five legislators were absent during votes, one of whom was excused. The share of votes missed at individual level, however, varies considerably. While some legislators miss as few as 3% of all votes, others are absent for up to 27% of votes. Table 3 shows the descriptive statistics.

Table 3: DESCRIPTIVE STATISTICS OF CONTROL VARIABLES

| Variable        | Mean  | Std. Dev | Min.  | Max.  |
|-----------------|-------|----------|-------|-------|
| Hour            | 2.259 | 1.276    | 0.017 | 6.392 |
| Monday          | 0.215 | 0.411    | 0     | 1     |
| Votes per block | 1.477 | 2.283    | 1     | 29    |
| President       | 0.022 | 0.146    | 0     | 1     |
| Counter         | 0.024 | 0.154    | 0     | 1     |
| Committee       | 0.233 | 0.420    | 0     | 1     |
| Own bill        | 0.003 | 0.053    | 0     | 1     |
| Own party       | 0.046 | 0.208    | 0     | 1     |

NOTE: Descriptive statistics of control variables. Based on 52,693 individual-level observations from voting blocks.

Data sources are the following. I received the complete video recordings of all meetings covering the full 49<sup>th</sup> legislative period from Parliamentary Services. Information on excused members comes from official attendance registers. Causes of absences were provided by the Parliament office. The time of a vote was coded from the videos. The remaining control variables are from the parliamentary minutes.

## 5 Results

### 5.1 Main Results

The baseline results are reported in Table 5. All regressions are run with ordinary least squares. Standard errors are clustered at legislator level since individual decisions are likely to be correlated.<sup>20</sup> In specifications (1)–(5) observations are collapsed to voting blocks so that the dependent variable measures the share of votes missed in a block.<sup>21</sup> In columns (6)–(7) observations are not collapsed and the dependent variable is 1 if a legislator was absent, and 0 otherwise. Blocks including both public and secret votes are dropped in columns (1)–(6). The full sample is used in column (7). The upper part of the table reports estimates while dropping excused absences. In the lower part excused absences are included.

The main coefficient of the interaction term *Reform*  $\times$  *Public* is positive and significant throughout all specifications. After the reform, attendance during observable votes increases by 2.3 percentage points compared to attendance during secret votes (column (1)). This corresponds to an average reduction in absences by 12% from pre-reform absence levels of 18.5%, or roughly 1 legislator per vote.

Controlling for legislator fixed effects (column (2)) does not change the estimated effect; controlling for season fixed effects (column (3)) slightly increases the estimated effect to 2.4 percentage points. Time-invariant legislator characteristics as well as seasonalities over the year seem not to affect the average treatment effect. When control variables are included, the treatment effect becomes larger (0.026 in column (4)). Estimating the full model suggests an average increase of attendance during monitored votes by 2.6 percentage points (column (5)). Taken together, covariates and fixed effects have some explanatory power for absences. This will be the main specification (referred to as the baseline) for the remainder of the paper.

Comparing the baseline regression (5) to the same estimation but using data without collapsing observations to blocks in (6), yields a coefficient larger by an order of magnitude of one half. Not

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<sup>20</sup>In Table 16 in the Appendix, standard errors for the baseline specification (5) are estimated with different clustering schemes. Clustering at the block level—i.e. assuming that observations in a voting block are correlated across legislators—leads to slightly higher standard errors. But the effect is still significant at the 5% level. Two-way clustering at the legislator and block level does not change the standard error. Moreover, estimating Jackknife standard errors dropping one legislator at a time yields a significant treatment effect, suggesting that results are not driven by single legislators or outliers.

<sup>21</sup>Some controls need adjustment as well: *Committee* is one if the legislator was on the committee related to at least one vote in a block; *Own bill* and *Own party* indicate whether any of the votes in a block was proposed by the legislator or his party. However, few observations are concerned since in 86% of cases only one bill per block is voted upon and controls would not vary within a block.

collapsing the data gives excessive weight to blocks with multiple consecutive votes, and would likely underestimate the true effect. This comparison shows the value of vote-level data in contrast to monthly/annual averages. Consequently, such precise control over the timing and correlations among votes would not be possible. In (7) observations from all votes are used. If the mixed block, i.e. those encompassing both public and secret votes, are not discarded, the coefficient is lower: it drops from 0.038 to 0.028. The treatment effect is smaller in mixed blocks than among non-mixed blocks or independent votes. I will return to this point at the end of this subsection.

Including all observations from excused absences has a limited effect on the estimated treatment effect: it becomes marginally larger in two specifications (columns (4)–(5)), smaller in (7), or does not change (columns (1)–(3), (6)). The result is not necessarily surprising since excused absences are relatively rare and it is difficult to exhibit strategic behavior depending on vote types when missing an entire day in parliament. The only way to strategically select between public and secret votes would be to miss only days with predominantly secret votes but be present on days with many public votes. While the excused category “maternity leave” is non-strategic in nature, other excused absences could be motivated by strategic considerations. Although there is no anecdotal evidence of legislators in Switzerland using sick leave to shirk their duties, it is possible in theory. Running a regression with the probability of being excused on a certain day as a dependent variable while controlling for individual fixed effects shows that there is no significant change in excused absences since the reform (cf. Table 17 in the Appendix). For the remainder of the paper, I report results excluding excused absences since selective absences are more likely to capture the dynamics of differential monitoring.

Before the reform, attendance during public votes was significantly lower than during public ones (by 5.6–8.5 percentage points), as indicated by the coefficient of *Public*. It confirms the notion that secret votes are typically more contested than public ones. It also indicates that legislators are able to discriminate between the vote types. After the reform, presence during secret votes tends to increase: the *Reform* coefficient is always positive and significant (with the exception of columns (4)–(7) when excused absences are not dropped). The significant change in the control group can be attributed to various causes. Potentially, the composition of bills differed after the reform for reasons unrelated to electronic voting and not controlled for by covariates. Alternatively, but not incompatibly, electronic voting has amended the way votes are being taken irrespective of whether a vote is subsequently published or not. Legislators might believe it more likely to cast a valid vote under electronic voting, and put more effort into returning for the votes. The positive reform

coefficient furthermore speaks against optimizing time by shifting absences from public to secret votes.

The signs of the control variables can be explained intuitively (reported in Table 15 column (2) in the Appendix). The longer a meeting takes, the lower presence is. On average, absences are lower on Mondays than on the other days of the week, suggesting a timely arrival of legislators in the chamber.<sup>22</sup> Absences become less likely if more votes take place during a voting block. This supports the interpretation that if many votes take place one after the other, legislators are more likely to return to the chamber. The linear session time trend is negative but insignificant. Owing to their special functions, chamber presidents and vote counters are always present, as are the legislators who are behind the proposed bill. Curiously, absences are higher, though insignificantly, when votes are held on bills proposed by members of the legislator's party (other than himself).

In short, the increase in attendance during public votes from 81.1% to 88.5% can be rationalized by the effect of improved monitoring. While 2.6 percentage points can be attributed to the reform, the remaining increase in presence was also observed in the control group.

Next, I return to the analysis of the timing of votes either in blocks or independently. Column (1) repeats the estimation from column (7) using all data for better comparison. In (2) only independent votes are included, whereas in (3) only votes taken in blocks are used. The treatment effect in the subsample of votes taken in blocks (0.036) is almost twice as large as in the subsample of independent votes (0.021). Legislators seem to adjust presence less when only one vote at a time is taken. Potentially, it is more difficult to arrive on time for a single vote than for a voting block, which, by definition, takes longer. Alternatively, legislators optimize by strategically choosing to be present more often during blocks, which drives up attendance rates more efficiently than being present at the same number of independent votes.<sup>23</sup>

In the subsample of votes taken in blocks exclusively made of either secret or public votes (column (4)) the increase in attendance during public votes amounts to 6.3 percentage points (using collapsed data the coefficient is somewhat smaller with 0.049). In contrast, there is virtually no change in absences in mixed blocks of both secret and public votes (column (5)). These blocks are discarded from most of the analysis because attendance during a block is highly correlated such

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<sup>22</sup> In an interview, one former legislator mentioned that some parties organize their own meetings before the Monday afternoon session. Thus, legislators are obliged to arrive on time.

<sup>23</sup> A reduction (an increase) in the share of public (secret) votes taken as independent votes around the reform would bias the coefficient upwards. The share of independent votes increases for public votes and decreases for secret ones, and both changes are insignificant (p-values are 0.111 and 0.310 respectively). Thus, the coefficient would be more likely downward biased.

that the distinction between treatment and control group would be blurred. A look at descriptive statistics provides some intuition as to the direction of spill-overs between the two groups of votes. Before the reform, legislators were absent during 8% of secret votes if they were taken in a non-mixed block, and during 9% if they occurred in mixed blocks. Although the difference is statistically significant (t-test p-value: 0.019), it is small. The two respective values for public votes are 19% and 10%, suggesting significantly higher attendance for mixed blocks (t-test p-value: 0.000). While presence during public and secret votes is markedly different when votes are taken in non-mixed blocks, it is similar in mixed blocks. In particular, attendance during public votes adjusts to levels similar to those for secret votes. The generally higher attendance rates during secret votes seem to spill over to public votes even when attendance is unobserved, as was the case before the reform.

Based on the findings of the separate analysis by independent votes and blocks, it can be concluded that the institutional setup defining the timing of votes can impact voting outcomes. In the context of the Swiss Upper House, grouping together secret and public votes had led to positive spillovers in attendance rates, and this even before the introduction of monitoring.

Table 4: MAIN RESULTS

|                          | (1)<br>No controls   | (2)<br>MP FE         | (3)<br>Season FE     | (4)<br>Controls      | (5)<br>Baseline      | (6)<br>Not collapsed | (7)<br>All votes     |
|--------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| WITHOUT EXCUSED ABSENCES |                      |                      |                      |                      |                      |                      |                      |
| Reform x Public          | 0.023**<br>(0.009)   | 0.023**<br>(0.009)   | 0.024**<br>(0.009)   | 0.026***<br>(0.009)  | 0.026***<br>(0.009)  | 0.038***<br>(0.013)  | 0.028**<br>(0.011)   |
| Public                   | -0.057***<br>(0.008) | -0.056***<br>(0.008) | -0.056***<br>(0.008) | -0.069***<br>(0.008) | -0.068***<br>(0.008) | -0.085***<br>(0.010) | -0.062***<br>(0.008) |
| Reform                   | 0.050***<br>(0.009)  | 0.047***<br>(0.009)  | 0.051***<br>(0.009)  | 0.043***<br>(0.011)  | 0.042***<br>(0.012)  | 0.043***<br>(0.012)  | 0.040***<br>(0.012)  |
| Observations             | 50,778               | 50,778               | 50,778               | 50,778               | 50,778               | 62,885               | 77,046               |
| Adjusted R <sup>2</sup>  | 0.011                | 0.036                | 0.012                | 0.036                | 0.059                | 0.063                | 0.058                |
| ALL OBSERVATIONS         |                      |                      |                      |                      |                      |                      |                      |
| Reform x Public          | 0.023***<br>(0.009)  | 0.023**<br>(0.009)   | 0.024**<br>(0.009)   | 0.027***<br>(0.009)  | 0.027***<br>(0.009)  | 0.038***<br>(0.013)  | 0.027**<br>(0.011)   |
| Public                   | -0.054***<br>(0.008) | -0.054***<br>(0.008) | -0.054***<br>(0.008) | -0.067***<br>(0.008) | -0.066***<br>(0.008) | -0.083***<br>(0.010) | -0.059***<br>(0.008) |
| Reform                   | 0.053***<br>(0.011)  | 0.049***<br>(0.011)  | 0.053***<br>(0.011)  | 0.015<br>(0.022)     | 0.023<br>(0.021)     | 0.023<br>(0.022)     | 0.019<br>(0.021)     |
| Observations             | 51,776               | 51,776               | 51,776               | 51,776               | 51,776               | 64,065               | 78,431               |
| Adjusted R <sup>2</sup>  | 0.010                | 0.043                | 0.011                | 0.036                | 0.065                | 0.068                | 0.064                |
| MP FE                    | NO                   | YES                  | NO                   | NO                   | YES                  | YES                  | YES                  |
| Controls                 | NO                   | NO                   | YES                  | NO                   | YES                  | YES                  | YES                  |
| Season FE                | NO                   | NO                   | NO                   | YES                  | YES                  | YES                  | YES                  |

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. In (1)–(5) observations are collapsed at block level, and the dependent variable is the share of attended votes in a voting block. In (6)–(7) observations are not collapsed and the dependent variable is 1 if a legislator was present during a vote, and 0 if he was absent. In (1)–(6) mixed blocks are excluded; in (7) they are included. There are no controls in (1), legislator fixed effects in (2), season fixed effects in (3), covariates in (4), and everything is controlled for in (5)–(7). In the upper part of the table all excused absences are dropped; they are included in the lower part.

Table 5: VOTING BLOCKS

|                         | (1)<br>All           | (2)<br>Independent   | (3)<br>Blocks        | (4)<br>Blocks<br>Not mixed | (5)<br>Blocks<br>Mixed |
|-------------------------|----------------------|----------------------|----------------------|----------------------------|------------------------|
| Reform x Public         | 0.028**<br>(0.011)   | 0.021**<br>(0.008)   | 0.036***<br>(0.012)  | 0.063***<br>(0.019)        | 0.001<br>(0.007)       |
| Public                  | -0.062***<br>(0.008) | -0.057***<br>(0.008) | -0.077***<br>(0.010) | -0.143***<br>(0.019)       | -0.010**<br>(0.004)    |
| Reform                  | 0.040***<br>(0.012)  | 0.042***<br>(0.012)  | 0.041**<br>(0.016)   | 0.034<br>(0.023)           | 0.036*<br>(0.021)      |
| Observations            | 77,046               | 44,429               | 32,617               | 18,456                     | 14,161                 |
| Adjusted R <sup>2</sup> | 0.058                | 0.058                | 0.070                | 0.096                      | 0.070                  |

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Observations are not collapsed and the dependent variable is 1 if a legislator was present during a vote, and 0 if he was absent. Legislator fixed effects, season fixed effects and covariates are always controlled for. In (1) all observations are used. In (2) only votes taken independently are included, and only blocks in (3). In (4) only blocks which are not mixed are included, and in (5) only mixed blocks are used.

## 5.2 Falsifications

In this part, I consider alternative candidate explanations other than the transparency reform for the increase in attendance, and provide evidence that they are unlikely to have influenced the results: anticipation effects, the introduction of electronic voting, and election cycles.

### 5.2.1 Anticipation Effects

The transparency reform was accepted one year before it was technically launched. I check for anticipation effects by running placebo regressions defining *Placebo* to take on value 1 since the final passage vote in spring 2013, and 0 before (results are in Table 6). I find no significant change in attendance for public votes as compared to secret ones that were held around the time when the transparency bill was passed (column (1)). Similarly, restricting the sample to pre-reform data yields no significant effect. The placebos speak against legislators anticipating future monitoring for certain vote types in a way that would explain the negative main effect.

Table 6: FALSIFICATION: ANTICIPATION

|                         | (1)<br>Placebo spring 2013 | (2)<br>Pre-reform data |
|-------------------------|----------------------------|------------------------|
| Placebo x Public        | 0.011<br>(0.009)           | -0.012<br>(0.013)      |
| Public                  | -0.063***<br>(0.009)       | -0.064***<br>(0.009)   |
| Placebo                 | -0.020*<br>(0.010)         | -0.006<br>(0.014)      |
| Observations            | 50,778                     | 27,941                 |
| Adjusted R <sup>2</sup> | 0.058                      | 0.063                  |

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Mixed blocks are excluded, and observations are collapsed at block level. The dependent variable is the share of attended votes during a block. Legislator fixed effects, season fixed effects and covariates are always controlled for. *Placebo* takes on value 1 for all votes taken since spring 2013 when the transparency bill was accepted, and 0 before. In (1) all data are used, in (2) observations are restricted to pre-reform votes (i.e. until 2013).

### 5.2.2 Effect of Electronic Voting

The introduction of electronic voting can potentially have an impact on the probability of being present if it increases the likelihood of the legislator returning to the chamber on time to cast a valid vote. Higher post-reform attendance in the control group is a possible indicator that this is actually happening. A main identifying assumption states that this “pure” effect of electronic voting on absences should not be greater for public votes than for secret ones because this would upward-bias the estimated treatment effect. Note that an identical impact of electronic voting on public and secret votes is uncritical for identification. Exploiting the seating arrangement in the chamber allows for a way of testing whether this might be the case.

Legislators sit in three semicircular rows. The back row is closest to the two symmetrical entrances to the chamber on either side.<sup>24</sup> Before the reform, legislators arriving late would enter the chamber while already raising their hand for the vote. After the reform, legislators need to arrive at their desk to press the electronic button. If electronic voting had a differential effect on absences during secret and public votes, the strongest effect could be expected for legislators sitting in the back row: they are closest to the doors and therefore may be more likely to arrive in time for the vote. I estimate the effect separately by row (cf. Table 7). The main coefficient is positive throughout the rows, but smallest and insignificant in the back row (0.013 column (3)). In an interacted model in column (4) with the indicator *Row 3* for the back row, the treatment effect in this row is 2.1 percentage points smaller than in the front and middle rows—though the respective coefficient of *Row 3 x Public x Reform* is insignificant.

In sum, the effect is not driven by legislators in the back row who should have gained most in terms of the probability of casting a valid vote due to the electronic voting system. The test provides some suggestive evidence for the validity of the identifying assumption.

### 5.2.3 Election Cycle

There is evidence that legislators change their behavior with approaching elections (Lindstädt & Vander Wielen 2011). Potentially, they might be preoccupied with electoral campaigns and have less time to attend voting sessions. Alternatively, they might want to be viewed as particularly dutiful and thus appear at more votes. If this kind of reasoning applies more to public than secret votes, and for reasons exogenous to vote monitoring, it could affect the estimated effect.

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<sup>24</sup>Chamber president, vice president and the vote counters sit at the front of the chamber, and are thus coded as sitting in row 1.

Table 7: FALSIFICATION: ELECTRONIC VOTING

|                         | (1)<br>Row 1         | (2)<br>Row 2         | (3)<br>Row 3         | (4)<br>Interacted    |
|-------------------------|----------------------|----------------------|----------------------|----------------------|
| Reform x Public         | 0.025*<br>(0.014)    | 0.042**<br>(0.016)   | 0.013<br>(0.019)     | 0.033***<br>(0.011)  |
| Public                  | -0.060***<br>(0.012) | -0.066***<br>(0.011) | -0.083***<br>(0.020) | -0.063***<br>(0.009) |
| Reform                  | 0.052**<br>(0.022)   | 0.039<br>(0.023)     | 0.040**<br>(0.016)   | 0.042***<br>(0.012)  |
| Row 3 x Public x Reform |                      |                      |                      | -0.021<br>(0.021)    |
| Row 3 x Public          |                      |                      |                      | 0.001<br>(0.019)     |
| Row 3 x Reform          |                      |                      |                      | -0.018<br>(0.021)    |
| Observations            | 19,586               | 15,717               | 15,475               | 50,778               |
| Adjusted R <sup>2</sup> | 0.078                | 0.050                | 0.050                | 0.059                |

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Mixed blocks are excluded, and observations are collapsed at block level. The dependent variable is the share of attended votes during a block. Legislator fixed effects, season fixed effects and covariates are always controlled for. In columns (1)–(3) the baseline specification is estimated separately by seating row in the chamber. In (4) the baseline model is interacted with an indicator *Row 3* which is 1 for legislators sitting in the third row, and 0 for all others.

Results from several robustness checks are given in Table 8 and, for comparison, the baseline is reported in column (1). Parliamentary business might differ from year to year and affect attendance.<sup>25</sup> Controlling for year fixed effects has no impact on the main coefficient (column (2)). Replacing the overall time trend with individual legislator time trends even slightly increases the coefficient (0.027, column (3)). Controlling for linear time trends separately for public and secret votes increases the main effect even further to 0.028 but the coefficient becomes marginally insignificant (the p-value is 0.134). In specifications (5)–(6) the election year 2015 is dropped; in (6) only observations from the two years adjacent to the reform are considered. In both cases the coefficient becomes even more positive (0.031 and 0.039 respectively). The 2015 election year thus cannot explain the main result. Rather, the effect stems from the year that directly followed the

<sup>25</sup> The Upper House secretary suggested in personal communication that more votes might take place in the first and last years of a legislative period: in the first one large bills like fiscal and legislative planning are debated; in the last one the parliament tries to finalize as many bills as possible.

start of improved monitoring. The evidence provided corroborates the notion that election cycles are unlikely to explain the main result.

Table 8: FALSIFICATION: ELECTION CYCLE

|                         | (1)                  | (2)                  | (3)                      | (4)                 | (5)                  | (6)                  |
|-------------------------|----------------------|----------------------|--------------------------|---------------------|----------------------|----------------------|
|                         | Baseline             | Year FE              | Individual<br>time trend | Time trend          | 2011-2014            | 2013-2014            |
| Reform x Public         | 0.026***<br>(0.009)  | 0.026***<br>(0.009)  | 0.027***<br>(0.009)      | 0.028<br>(0.018)    | 0.031***<br>(0.010)  | 0.039***<br>(0.013)  |
| Public                  | -0.069***<br>(0.008) | -0.068***<br>(0.008) | -0.069***<br>(0.008)     | 1.190<br>(14.907)   | -0.070***<br>(0.008) | -0.080***<br>(0.012) |
| Reform                  | 0.055***<br>(0.008)  |                      | 0.040***<br>(0.011)      | 0.042***<br>(0.011) | 0.052***<br>(0.008)  | 0.044***<br>(0.008)  |
| Time trend<br>secret    |                      |                      |                          | 0.006<br>(0.006)    |                      |                      |
| Time trend<br>public    |                      |                      |                          | 0.006<br>(0.009)    |                      |                      |
| Observations            | 50,778               | 50,778               | 50,778                   | 50,778              | 41,155               | 23,129               |
| Adjusted R <sup>2</sup> | 0.059                | 0.059                | 0.039                    | 0.059               | 0.062                | 0.064                |

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Mixed blocks are excluded, and observations are collapsed at block level. The dependent variable is the share of attended votes during a block. Legislator fixed effects, season fixed effects and covariates are always controlled for. (1) shows the baseline for comparison. In (2) year fixed effects are controlled for. In (3) linear individual time trends are controlled for. In (4) separate linear and quadratic time trends for secret and public votes are controlled for. In (5) and (6) the sample is restricted to the years 2011–2014 and 2013–2014 respectively.

### 5.3 Channels

On average, attendance during monitored votes has increased since the reform. In the following, I explore two potential (non-excludable) mechanisms. The first one relates to the dependance on the political career. The second ones takes a closer look at the vote choices legislators take from the perspective of the theory on voting in committees. In a last step, I look at alternative measures of effort analyze whether the increase in attendance compromises other legislative activity.

#### 5.3.1 Dependence on Political Career

##### Full-Time Politicians

Legislators' career options outside politics may influence how they react to monitoring. If absences are negatively perceived, legislators who depend more on their political careers should react more

strongly. In contrast, legislators with alternative careers either in the market sector and those close to retirement, might react less strongly. I compare treatment effects for those with an outside occupation (i.e. moonlighting) and those who are full-time politicians, and depending on the number of their declared interest groups. Both legislators with other occupations and with many interest groups might have better career options should they fail to be reelected.

There are 14 full-time politicians in the Upper House, while the others simultaneously hold an outside occupation.<sup>26</sup> Before the reform, full-time politicians missed significantly fewer votes (public: 15.5%; secret: 10.7%) than moonlighting legislators (public: 21.8%; secret 17%). This tends to corroborate the idea that some legislators have dedicated their careers to politics and thus put more effort into floor voting.<sup>27</sup>

Regressions results are given in Table 9. The reform effect for full-time politicians is significantly positive (column (1)): full-time legislators increase their presence during public votes by 3.7 percentage points. In contrast, the treatment effect among moonlighting legislators is smaller (0.021, column (2)). In an interacted model (column (3)), the difference in treatment effects between full-time and moonlighting politicians indicated by the coefficient of *Fulltime x Reform x Public* is positive as expected but insignificant. Potentially the relatively low number of full-time politicians does not allow to estimate this difference more precisely.

## Interest Groups

Interest groups have an impact on legislative voting (e.g. Giger and Klüver (2016) in the Swiss context). Some explicitly want to impact legislators' votes and they all demand time and effort. Legislators in the Swiss parliament are legally obliged to disclose their interest groups at the beginning of each calendar year. The information is publicly available online in PDF format. The variable *Interestgroups* measures the individual stock of officially declared interest groups per member of the Upper House in 2012.<sup>28</sup> Fixing the variable at the beginning of the legislative term makes it possible to discard potential strategic adjustments in the number of interest groups. I will deal with such adjustments below. While many legislators have few interest groups, some declare as many as 30. The average and median are both around 8 (cf. Figure 5 in the Appendix for a

<sup>26</sup> An analysis based on legislators' outside wages would be preferable to a dichotomous distinction between full-time politicians and moonlighters. While the declaration of moonlighting wages is mandatory in some countries, e.g. in Germany or Italy, this is, however, not the case in Switzerland.

<sup>27</sup> These descriptives are compatible with Fedele and Naticchionis' (2015) finding that "public-fit" politicians are more likely to be present during votes than "private-fit" ones.

<sup>28</sup> The legislator leaving the Upper House early is allotted his 2011 value, and the two legislators joining the Upper House in 2014 their respective 2014 values.

Table 9: CHANNEL: OUTSIDE OPTIONS

|                                  | (1)<br>Fulltime      | (2)<br>Moonlight     | (3)<br>Interaction   | (4)<br>All           |
|----------------------------------|----------------------|----------------------|----------------------|----------------------|
| Reform x Public                  | 0.037**<br>(0.014)   | 0.021*<br>(0.012)    | 0.020*<br>(0.012)    | 0.054***<br>(0.016)  |
| Public                           | -0.062***<br>(0.014) | -0.071***<br>(0.010) | -0.070***<br>(0.010) | -0.090***<br>(0.015) |
| Reform                           | 0.042*<br>(0.023)    | 0.042***<br>(0.014)  | 0.048***<br>(0.013)  | 0.054***<br>(0.016)  |
| Fulltime x Reform x Public       |                      |                      | 0.019<br>(0.018)     |                      |
| Fulltime x Reform                |                      |                      | -0.020<br>(0.015)    |                      |
| Fulltime x Public                |                      |                      | 0.004<br>(0.017)     |                      |
| Interestgroups x Reform x Public |                      |                      |                      | -0.003**<br>(0.002)  |
| Interestgroups x Reform          |                      |                      |                      | -0.001<br>(0.002)    |
| Interestgroups x Public          |                      |                      |                      | 0.003**<br>(0.001)   |
| Observations                     | 14,682               | 36,096               | 50,778               | 50,778               |
| Adjusted R <sup>2</sup>          | 0.039                | 0.056                | 0.059                | 0.060                |

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Mixed blocks are excluded, and observations are collapsed at block level. The dependent variable is the share of attended votes during a block. Legislator fixed effects, season fixed effects and covariates are always controlled for. In (1) and (2) only observations from legislators without and with outside occupations are used respectively. (3) shows an interacted model, and *Fulltime* indicates whether a legislator was a full-time politician or not. In (4) *Interestgroups* measures the individual, officially declared number of interest groups in 2012.

histogram and density plot).

For the regressions, the variable *Interestgroups* is interacted with the rest of the DiD model to find heterogeneous effects depending on the declared number of interest groups. Legislators without interest groups significantly increase attendance by 5.4 percentage points (column (4)). Per additional interest group, the probability of presence during public votes decreases by 0.5 percentage points. The effect is robust to a log specification (Table 18).

Before the reform, legislators without interest groups had on average 9 percentage points higher presence during public votes than during secret ones (coefficient of *Public* in column (4)). Politicians

who had no interest groups reduced absences during public votes to a level similar to legislators with more interest groups. Having many interest groups might indicate that the legislator has less time to invest in voting. Potentially, these interest groups even demand the politicians' presence in the chamber to be able to influence political outcomes. Legislators with few interest groups, on the other hand, have the capacity to increase their presence in the chamber.

Could any of the above be due to an adjustment in the number of interest groups? Assuming a negative "pure" reform effect of monitoring, the result might be rationalized by an adjustment in the number of interest groups, which would counteract the monitoring effect. For example, if legislators with many interest groups report even more groups and this caused them to have less time for voting, the total marginal reform effect of monitoring and adjustment of interest groups might be zero. Any such adjustments would be expected between the reported numbers of interest groups between 2013 and 2014 because the 2013 number was reported before the transparency bill was ratified.

I run a t-test of the number of interest groups reported in 2013 and 2014 by year. Although the average number increases from 8.6 to 9.9, the change is insignificant.<sup>29</sup> I also ran the regression from column (4) using the declared number of interest groups from 2013, 2014, and 2015 respectively (results are in Table 18 in the Appendix). The treatment effect for legislators with no interest groups is always significant and positive.

Full-time politicians not only were more likely to be present before the reform, but they also reacted more to the changed incentives induced by monitoring. Having devoted their professional lives to politics, their career options in the case of unsuccessful reelection are less promising than of those who also have alternative careers alongside their political duties.

## **Accountability towards voters**

The literature documents the differential behavior of legislators contained by reelection prospects (Barro 1973). There is (partial) evidence for shirking towards the end of the political career by voting against constituencies' interests and (robust) evidence for reduced presence in parliament (e.g., Rothenberg & Sanders 2000). Politicians wishing to continue their political careers have to cater to their voters' interests. In contrast, legislators who do not face reelection constraints should

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<sup>29</sup> Considering the entire legislative period, the number of interest groups reported is significantly higher after the reform. This trend might be due to many reasons like time effects, new legislators acquiring more interest groups or might even occur randomly. The really interesting change is therefore the one from 2013 to 2014, i.e. around the time of the reform.

be less susceptible to monitoring. If this rationale applies, monitoring should have a lesser effect among legislators terminating their political careers at the end of the legislative period. In total, 35 members of the Upper House were standing for reelection, while a further 11 decided to step down. Observations from the two members leaving the Upper House before 2014 are dropped because there is no retirement decision involved.

A crucial assumption to identify a difference in treatment effects between retiring and non-retiring legislators is that the retirement decision is independent of the transparency reform (as in terms of timing the reform precedes the official retirement announcement). Retirement can take two forms: voluntary or in anticipation of a potential defeat at the approaching elections. Some legislators devote their lives to political work, while others utilize political office as a stepping stone for their non-political career.<sup>30</sup> In the case of the Swiss Upper House, the notion of career politicians seems to dominate. Office-holders not running in the 2015 election were between 62 and 69 years old in 2014. On average, they were ten years older than legislators running for the next term, speaking in favor of voluntary retirement. There are also several reasons why it is unlikely that legislators decide to retire because of greater voting record transparency. First, I check who retired depending on initial support for the transparency bill. A t-test shows that supporters and opponents of the transparency bill were equally likely to retire in 2015. Second, the number of retiring legislators in 2015 is very similar to the 2011 elections (11 and 12 respectively). Last, one of the interviewed legislators confirmed that his retirement decision was independent of his rejection of the electronic voting system.

In the subsample of retiring legislators, the treatment effect is insignificant and half the size of the baseline effect (column (1)). Either these legislators do not react to monitoring. Alternatively, it measures a net effect: retiring legislators would have liked to increase absences towards the end of the term but monitoring kept them from doing so. In contrast, incumbents standing for reelection significantly increased their attendance by 2.9 percentage points (column (2)). Consequently, politicians exposed to the threat of not being reelected drive the average result. Testing for difference between the two groups, however, yields a positive but insignificant coefficient *Candidate  $\times$  Reform  $\times$  Public*.

The effect size is in line with the expectation that politicians facing reelection constraints are more susceptible to monitoring. In contrast, legislators who end their political careers are not

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<sup>30</sup>Mattozzi and Merlo (2008) make this distinction between what they call “career politicians” and individuals with “political careers”.

Table 10: HETEROGENEOUS EFFECTS: RUNNING FOR REELECTION

|                             | (1)<br>Retiring      | (2)<br>Candidate in 2015 | (3)<br>Interaction   |
|-----------------------------|----------------------|--------------------------|----------------------|
| Reform x Public             | 0.013<br>(0.013)     | 0.029**<br>(0.012)       | 0.016<br>(0.012)     |
| Public                      | -0.074***<br>(0.013) | -0.066***<br>(0.011)     | -0.077***<br>(0.012) |
| Reform                      | 0.026<br>(0.018)     | 0.044***<br>(0.014)      | 0.038**<br>(0.015)   |
| Candidate x Reform x Public |                      |                          | 0.013<br>(0.017)     |
| Candidate x Reform          |                      |                          | 0.001<br>(0.016)     |
| Candidate x Public          |                      |                          | 0.013<br>(0.016)     |
| Observations                | 12,304               | 37,239                   | 49,543               |
| Adjusted R <sup>2</sup>     | 0.037                | 0.060                    | 0.057                |

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Mixed blocks are excluded, and observations are collapsed at block level. The dependent variable is the share of attended votes during a block. Legislator fixed effects, season fixed effects and covariates are always controlled for. In (1) and (2) only observations from legislators retiring and running for reelection in 2015 are used respectively. (3) shows an interacted model, and *Candidate* indicates whether a legislator was a candidate in the 2015 election or not.

concerned with punishment by their constituencies. In their last term they behave like lame ducks and do not react to incentives related to reelection prospects.

### 5.3.2 Voting Decisions

The preceding analysis shows that legislators depending more on political careers tend to attend more votes once they get monitored. One question is whether attendance during votes should be viewed as a measure of legislators' effort, or whether it captures a strategic component. In theory, non-voting might be optimal if a legislator is uncertain about what vote choice is best. By not voting he can delegate the decision to more competent legislators (Feddersen & Pesendorfer 1996). Under secret voting, absences and abstentions are substitutes as they both do not affect the final vote outcome. However, under transparent voting, abstaining counts towards a positive attendance record, while being absent does not. Such a rationale applies more for uninformed legislators than

experts: uninformed agents will be more likely to vote yes or no under transparency in order to prevent being identified as uninformed. Informed agents generally have the incentive to vote rather than abstain or not attend, irrespective of whether votes are being monitored (Mattozzi & Nakaguma 2017). Consequently, looking at the probability of abstention can help understanding whether the increase in attendance should be viewed purely as an increase in effort, or whether vote incentives change as well. I also look at the probabilities of voting yes or no, and deviation from party line where the party line is defined as the party majority choice.

The sample is reduced to all observations for which the voting decision is known: while attendance is almost always observed, vote choices are not due to imperfections with the filming of the pre-reform votes. This leaves 47,226 observations in the sample. Moreover, I keep only legislators who were members of the four largest parties (SP, CVP, FDP, SVP). The small parties are excluded because a party line based on one or two legislators cannot be defined meaningfully.

The main results are in the upper part of Table 11. Column (1) confirms that attendance during public votes increases by 3.2 percentage points in this restricted sample. Conditional on being present during a vote, the probability of abstention is reduced by 2.5 percentage points (column (2)). The results suggest that not only are legislators more present, they moreover opt more frequently for one of the alternatives instead of abstaining. The average probability to vote yes does not change (column (3)). However, voting no becomes 3.4 percentage points more likely (column (4)). This changed voting behavior corresponds to a reduction in deviation from party line by 1.5 percentage points on average.<sup>31</sup>

I rerun the above regressions interacted once interacted with an indicator for *Uninformed* and once for *Expert*. A legislator is defined as an expert if he was member of the committee that was dealing with the bill. For better overview, the lower part of Table 11 shows only the treatment effects for the two subgroups and the interaction testing for a difference between the subgroups (the full Tables 19 and 20 are in the Appendix).

In the group of uninformed legislators (i.e. those not being members on the committee responsible for the bill), results are very similar to the average effect. Their behavior fits with the hypothesis that legislators will be more likely to vote in contrast to abstaining or being absent if this might be viewed as a signal for incompetence. Fewer deviations from party line might either suggest that legislators vote more for their ideology, or that parties impose pressure on their members to present

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<sup>31</sup> In their analysis of final passage votes, Benesch, Bütler and Hofer (2015) similarly find a decrease in deviations from party line.

a more united front once votes become public.

In the group of experts, presence increases significantly by 4.7 percentage points. The only other significant change is a lower probability of abstention (column (2)). In sum, experts turn out more to vote and rather choose to vote yes or no. Though both probabilities are positive (columns (3)–(4)), coefficients are insignificant. None of the differences between uninformed legislators and experts is significant.

Part of the evidence provided here can be rationalized by incentives arising from voting in committees. But transparency does not have the expected effect on experts. In theory experts have an incentive to vote (rather than abstain or being absent) even if voting is intransparent. A potential issue is that expertise is proxied by committee membership which maybe does not capture the level of information in the intended way.

### 5.3.3 Alternative Measures of Effort

Floor voting attendance is used as one measure of legislative effort in the literature. Legislative activities, however, are not restricted to voting but include bill (co-)sponsorship, speeches and committee work (Dal Bó & Rossi, 2011). I check whether other legislative activities also changed around the time of the reform as a consequence of increased attendance. One could even think about legislative effort in terms of optimization under a time restriction. Then, different measures of effort might actually serve as substitutes. For example, legislators could shift activities to those reaping the highest returns.<sup>32</sup>

The data make it possible to consider the following effort measures: the number of speeches, bill signing (of bills proposed by other legislators), and bill sponsorship. The latter category can be disaggregated by type of bill introduced. These are (ranked from little to high complexity): question, interpellation, motion, postulate, and parliamentary initiative. The data were received from the Parliamentary Services. All variables are measured before and after the reform per legislative session and at legislator level. This results in two observations per legislator. I run legislator fixed effects regressions, which compare individual effort around the time of the reform. These estimations should be interpreted as descriptive evidence, owing to the fact that they do not necessarily lend themselves to a causal interpretation since a control group is not available.

Results are given in Table 12, with standard errors clustered at individual level. For comparison, the first column reports the change in attendance measured in the same way as the other effort

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<sup>32</sup> A full analysis is not feasible for reasons of data availability because it would require observations of the complete range of politicians' activities.

Table 11: EFFECT OF MONITORING ON VOTE CHOICES

|                          | (1)<br>Present       | (2)<br>Abstain       | (3)<br>Yes          | (4)<br>No            | (5)<br>Deviate       |
|--------------------------|----------------------|----------------------|---------------------|----------------------|----------------------|
| Reform x Public          | 0.032**<br>(0.015)   | -0.025***<br>(0.004) | -0.009<br>(0.010)   | 0.034***<br>(0.008)  | -0.015**<br>(0.006)  |
| Public                   | -0.082***<br>(0.012) | 0.022***<br>(0.003)  | 0.341***<br>(0.006) | -0.362***<br>(0.005) | -0.094***<br>(0.012) |
| Reform                   | 0.049***<br>(0.015)  | 0.011**<br>(0.005)   | 0.021**<br>(0.009)  | -0.032***<br>(0.007) | 0.011<br>(0.007)     |
| UNINFORMED               |                      |                      |                     |                      |                      |
| Reform x Public          | 0.028*<br>(0.016)    | -0.023***<br>(0.005) | -0.013<br>(0.012)   | 0.036***<br>(0.009)  | -0.016**<br>(0.007)  |
| EXPERT                   |                      |                      |                     |                      |                      |
| Reform x Public          | 0.047**<br>(0.022)   | -0.030***<br>(0.006) | 0.008<br>(0.015)    | 0.022<br>(0.014)     | -0.012<br>(0.012)    |
| Reform x Public x Expert | 0.019<br>(0.023)     | -0.007<br>(0.008)    | 0.021<br>(0.017)    | -0.015<br>(0.017)    | 0.004<br>(0.012)     |
| Observations             | 47,226               | 41,378               | 41,378              | 41,378               | 41,378               |

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Mixed blocks are excluded, and observations are at vote level. Restricted to legislators from the four largest parties (SP, CVP, FDP, SVP). The dependent variables are indicators taking on value 1 if a legislator (1) was present; (2) abstained; (3) voted yes; (4) voted no; (5) deviated from his party line. Legislator fixed effects, season fixed effects and covariates are always controlled for. The line for *Uninformed* (*Expert*) shows the average treatment effect for legislators who are not (who are) members on the responsible committee.

measures (using only public votes). Presence increases significantly by 6.9 percentage points. The size exceeds the main effect of 2.6 percentage points. The difference is due to the lack of a control group and the impossibility of controlling for covariates at vote and individual level. Legislators exert significantly more effort after the reform in terms of speeches, bill co-sponsorship and in one bill category (columns (2)–(3), (5)). They give around one more speech per session, they sign one more bill every two sessions, and they propose roughly one more interpellation per session. Effort around the time of the reform does not change significantly in terms of the other bill categories and the total number of bills (columns (4), (6)–(9)). This descriptive evidence suggests that other legislative activities—at least the measurable ones—do not suffer from increased attendance and some of them even increase. Most likely, the time required for more voting is not so much as to

compromise other parliamentary work.

Table 12: FALSIFICATION: ALTERNATIVE MEASURES OF EFFORT

|                     | (1)<br>Present      | (2)<br>Sign         | (3)<br>Speak      | BILLS INTRODUCED  |                    |                   |                   |                   |                  |
|---------------------|---------------------|---------------------|-------------------|-------------------|--------------------|-------------------|-------------------|-------------------|------------------|
|                     |                     |                     |                   | (4)<br>Quest.     | (5)<br>Interp.     | (6)<br>Motion     | (7)<br>Postul.    | (8)<br>Ini.       | (9)<br>Total     |
| Reform              | 0.069***<br>(0.012) | 0.439***<br>(0.157) | 0.920*<br>(0.478) | -0.007<br>(0.018) | 0.085**<br>(0.042) | -0.008<br>(0.029) | -0.015<br>(0.033) | -0.000<br>(0.010) | 0.055<br>(0.064) |
| Obs.                | 88                  | 88                  | 88                | 88                | 88                 | 88                | 88                | 88                | 88               |
| Adj. R <sup>2</sup> | 0.568               | 0.747               | 0.718             | 0.286             | 0.819              | 0.451             | 0.564             | 0.748             | 0.821            |

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions with legislator fixed effects. Based on subsample of 44 legislators with pre- and post-reform observations. The dependent variables are: (1) attendance during public votes; number of: (2) signed bills; (3) speeches; (4) questions asked; (5) interpellations; (6) motions; (7) postulates; (8) parliamentary initiatives; (9) bills proposed in total as a sum of (4)–(8).

## 6 Concluding Remarks

Do individuals react to incentives from differences in monitoring technologies? Using the example of legislators voting under varying monitoring, this paper provides evidence that this is the case. If legislators' attendance rates can be tracked, they are on average more likely to vote compared to situations when their attendance is not disclosed to the public. Moreover, effects differ according to several individual characteristics. Generally, legislators who are more dependent on reelection display greater changes in their behavior: candidates running for reelection, full-time politicians, and those with few interest groups, which potentially indicates fewer career options outside the world of politics. This paper also shows that higher attendance impacts vote choices and contributes to higher party discipline.

My research also contributes to the understanding of effects of parliamentary voting procedures on political outcomes. While the predominant goal of electronic voting was the eradication of counting mistakes during votes, it also changed the way legislators vote. Institutional changes might therefore have (undesired) side effects.

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# Appendix

## Data sources

Table 13: OVERVIEW OF VARIABLES AND DATA SOURCES

| Variable                                         | Description                         | Source(s)                                 |
|--------------------------------------------------|-------------------------------------|-------------------------------------------|
| Attendance                                       | Votes before 2014                   | Videos provided by Parliamentary Services |
|                                                  | Public votes since 2014             | PDFs provided by Parliamentary Services   |
|                                                  | Other votes since 2014              | Videos provided by Parliamentary Services |
| Gender, birthday, party, party group, canton     | Obligatory MP personal information  | Parliament homepage                       |
| Academic title, civil status, number of children | Voluntary MP private information    | Parliament homepage                       |
| Interest groups                                  | Number of disclosed interest groups | Register of interest groups 2012–2015     |
| Profession                                       | Self-reported profession            | Register of interest groups 2012–2015     |

NOTE: This table provides an overview of the variables used with a short variable description, and the source from which it was retrieved.

Academic title, military title, and civil status could vary over time. These variables were collected in September 2015 and thus reflect the status at this point in time. Civil status and number of children are voluntary information given by legislators.

## Member mutations

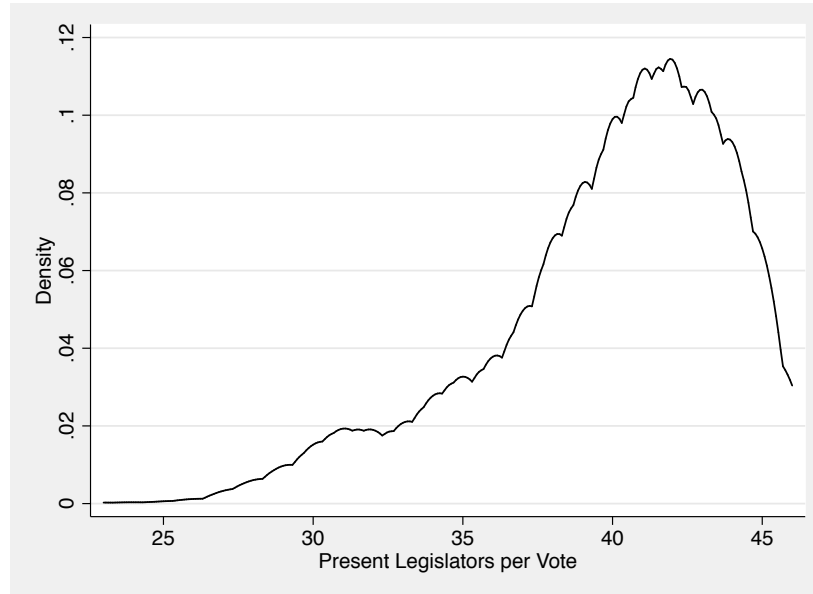
Table 14: MP MUTATIONS OVER TIME

| Name             | Date exit         | Replaced by       | Date entry    |
|------------------|-------------------|-------------------|---------------|
| Berset, Alain    | 31 December, 2011 | Levrat, Christian | 29 May, 2012  |
| Jenny, This      | 13 February, 2014 | Hefti, Thomas     | 3 March, 2014 |
| Freitag, Pankraz | 5 October, 2013   | Hösli, Werner     | 16 June, 2014 |

NOTE: Source: Parliament homepage.

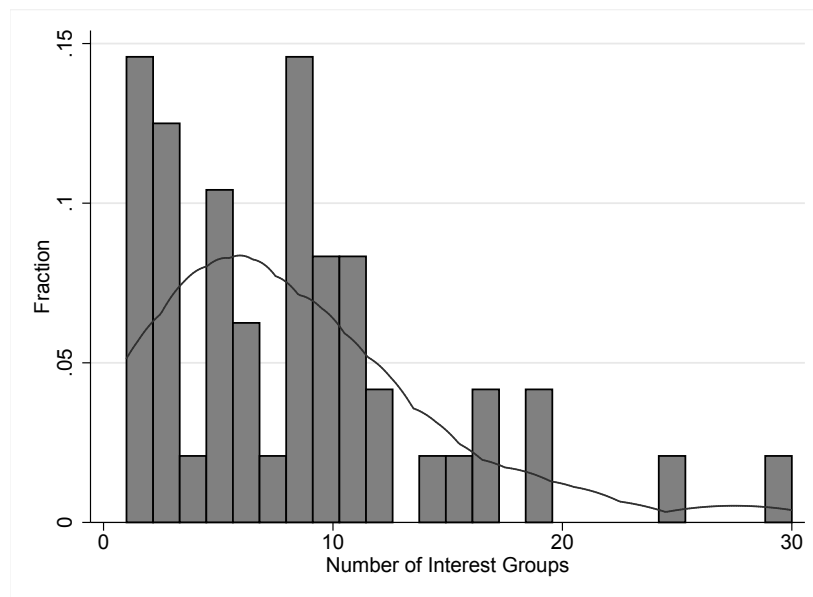
## Figures

FIG. 4: DENSITY PLOT LEGISLATORS VOTING



NOTE: Kernel density plot of the number of present legislators per vote.

FIG. 5: NUMBER OF INTEREST GROUPS



NOTE: Distribution of yearly declared number of interest groups per legislator in the Swiss Upper House. The y-axis depicts the fraction of observations. Source: Official declaration of interest groups available on the Swiss Parliament website [www.parlament.ch](http://www.parlament.ch).

## Tables

Table 15: ROBUSTNESS CHECKS

|                                | (1)                     | (2)                  | (3)                  | (4)                  |
|--------------------------------|-------------------------|----------------------|----------------------|----------------------|
|                                | Without<br>quorum votes | Transparency<br>vote | Observed<br>on video | Controls             |
| Reform x Public                | 0.044***<br>(0.013)     | 0.018<br>(0.017)     | 0.024**<br>(0.009)   | 0.026***<br>(0.009)  |
| Public                         | -0.093***<br>(0.010)    | -0.070***<br>(0.007) | -0.067***<br>(0.009) | -0.068***<br>(0.008) |
| Reform                         | 0.044***<br>(0.012)     | 0.054***<br>(0.015)  | 0.041***<br>(0.012)  | 0.042***<br>(0.012)  |
| Transparency x Reform x Public |                         | 0.012<br>(0.021)     |                      |                      |
| Transparency x Reform          |                         | -0.013<br>(0.018)    |                      |                      |
| Transparency x Public          |                         | -0.000<br>(0.015)    |                      |                      |
| Committee                      |                         |                      |                      | 0.053***<br>(0.005)  |
| Own bill                       |                         |                      |                      | 0.145***<br>(0.014)  |
| Own party                      |                         |                      |                      | -0.015*<br>(0.009)   |
| President                      |                         |                      |                      | 0.144***<br>(0.024)  |
| Counter                        |                         |                      |                      | 0.164***<br>(0.016)  |
| Hour                           |                         |                      |                      | -0.023***<br>(0.003) |
| Monday                         |                         |                      |                      | 0.040***<br>(0.004)  |
| Votes per block                |                         |                      |                      | 0.008***<br>(0.002)  |
| Time trend                     |                         |                      |                      | 0.002<br>(0.001)     |
| Observations                   | 58,656                  | 45,579               | 50,620               | 50,778               |
| Adjusted R <sup>2</sup>        | 0.065                   | 0.060                | 0.059                | 0.059                |

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors clustered at legislator level in parentheses. Ordinary least squares regressions. Mixed blocks are excluded, observations are collapsed at block level, and the dependent variable is the share of attended votes in a voting block. Covariates, individual fixed effects and season fixed effects are always controlled for. Excused absences are excluded. In (1) quorum votes are excluded. In (2) *Transparency* is 1 for legislators who accepted the transparency bill in the final passage vote and 0 else. In (3) Only absences observed on video are included, and inferred data dropped. (4) is the baseline reporting coefficients for control variables.

Table 16: PROBABILITY OF ABSENCE - DIFFERENT CLUSTERING SCHEMES

|                         | (1)<br>Robust        | (2)<br>MP            | (3)<br>Jackknife     | (4)<br>Block         | (5)<br>2-way         |
|-------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| Reform x Public         | 0.026***<br>(0.007)  | 0.026***<br>(0.009)  | 0.026**<br>(0.011)   | 0.026**<br>(0.013)   | 0.026***<br>(0.009)  |
| Public                  | -0.068***<br>(0.005) | -0.068***<br>(0.008) | -0.068***<br>(0.009) | -0.068***<br>(0.011) | -0.068***<br>(0.008) |
| Reform                  | 0.042***<br>(0.006)  | 0.042***<br>(0.012)  | 0.042***<br>(0.011)  | 0.042***<br>(0.015)  | 0.042***<br>(0.012)  |
| Observations            | 50,645               | 50,645               | 50,645               | 50,645               | 50,645               |
| Adjusted R <sup>2</sup> | 0.057                | 0.057                | 0.032                | 0.057                | 0.057                |

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Ordinary least squares regressions. Mixed blocks are excluded, observations are collapsed at block level, and the dependent variable is the share of attended votes in a voting block. Covariates, individual fixed effects and season fixed effects are always controlled for. Excused absences are excluded. Standard errors in parentheses: (1) robust standard errors; (2) clustered at legislator level; (3) clustered at block level; (4) two-way clustered at legislator and vote level; (5) Jackknife standard errors with legislator clustering.

Table 17: EXCUSED AB-  
SENCES

|                         | (1)<br>Excused    |
|-------------------------|-------------------|
| Reform                  | -0.003<br>(0.005) |
| Observations            | 9,191             |
| Adjusted R <sup>2</sup> | 0.048             |
| MP FE                   | YES               |

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors 2-way clustered at legislator and day level in parentheses. Ordinary least squares regressions with individual fixed effects.

Table 18: CHANNEL: OUTSIDE OPTIONS—ROBUSTNESS CHECKS

| VARIABLES                        | (1)<br>Logs          | (2)<br>2013          | (3)<br>2014          | (4)<br>2015          |
|----------------------------------|----------------------|----------------------|----------------------|----------------------|
| Reform x Public                  | 0.086***<br>(0.024)  | 0.064***<br>(0.016)  | 0.061***<br>(0.018)  | 0.052**<br>(0.020)   |
| Reform                           | 0.051**<br>(0.023)   | 0.043**<br>(0.017)   | 0.038**<br>(0.016)   | 0.042***<br>(0.015)  |
| Public                           | -0.111***<br>(0.025) | -0.101***<br>(0.017) | -0.101***<br>(0.018) | -0.097***<br>(0.018) |
| Interestgroups x Reform x Public | -0.032***<br>(0.012) | -0.004***<br>(0.001) | -0.004**<br>(0.002)  | -0.002<br>(0.002)    |
| Interestgroups x Reform          | -0.005<br>(0.012)    | -0.000<br>(0.001)    | 0.000<br>(0.001)     | 0.000<br>(0.001)     |
| Interestgroups x Public          | 0.023**<br>(0.011)   | 0.004***<br>(0.001)  | 0.003**<br>(0.001)   | 0.003**<br>(0.001)   |
| Observations                     | 50,778               | 50,778               | 50,778               | 50,778               |
| Adjusted R <sup>2</sup>          | 0.059                | 0.059                | 0.059                | 0.059                |

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Mixed blocks are excluded, and observations are collapsed at block level. The dependent variable is the share of attended votes during a block. Legislator fixed effects, season fixed effects and covariates are always controlled for. In (1) the log of officially declared interest groups as of 2012 is used. In (2)–(4) the officially declared number of interest groups in 2013, 2014, and 2015 are used respectively.

Table 19: EFFECT OF MONITORING ON VOTE CHOICE: EXPERTS

|                          | (1)<br>Present       | (2)<br>Abstain       | (3)<br>Yes           | (4)<br>No            | (5)<br>Deviate       |
|--------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| Public x Reform          | 0.028*<br>(0.016)    | -0.023***<br>(0.005) | -0.013<br>(0.012)    | 0.036***<br>(0.009)  | -0.016**<br>(0.007)  |
| Public x Reform x Expert | 0.019<br>(0.023)     | -0.007<br>(0.008)    | 0.021<br>(0.017)     | -0.015<br>(0.017)    | 0.004<br>(0.012)     |
| Public                   | -0.082***<br>(0.013) | 0.023***<br>(0.004)  | 0.359***<br>(0.008)  | -0.382***<br>(0.006) | -0.091***<br>(0.012) |
| Reform                   | 0.063***<br>(0.016)  | 0.009*<br>(0.005)    | 0.024**<br>(0.011)   | -0.033***<br>(0.009) | 0.010<br>(0.007)     |
| Expert                   | 0.075***<br>(0.008)  | -0.000<br>(0.002)    | 0.071***<br>(0.014)  | -0.071***<br>(0.015) | 0.013<br>(0.008)     |
| Public x Expert          | -0.005<br>(0.018)    | -0.004<br>(0.005)    | -0.067***<br>(0.018) | 0.071***<br>(0.017)  | -0.011<br>(0.010)    |
| Reform x Expert          | -0.046***<br>(0.010) | 0.006<br>(0.004)     | -0.015<br>(0.016)    | 0.010<br>(0.017)     | -0.000<br>(0.010)    |
| Observations             | 47,226               | 41,378               | 41,378               | 41,378               | 41,378               |
| Adjusted R <sup>2</sup>  | 0.060                | 0.012                | 0.146                | 0.150                | 0.062                |

NOTE: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Mixed blocks are excluded, and observations are at vote level. Restricted to legislators from the four largest parties (SP, CVP, FDP, SVP). The dependent variables are indicators taking on value 1 if a legislator (1) was present; (2) abstained; (3) voted yes; (4) voted no; (5) deviated from his party line. Legislator fixed effects, season fixed effects and covariates are always controlled for. *Expert* takes on value 1 (0) for legislators who are (who are not) members on the responsible committee.

Table 20: EFFECT OF MONITORING ON VOTE CHOICE: UNINFORMED

|                              | (1)<br>Present       | (2)<br>Abstain       | (3)<br>Yes           | (4)<br>No            | (5)<br>Deviate       |
|------------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| Public x Reform              | 0.047**<br>(0.022)   | -0.030***<br>(0.006) | 0.008<br>(0.015)     | 0.022<br>(0.014)     | -0.012<br>(0.012)    |
| Public x Reform x Uninformed | -0.019<br>(0.023)    | 0.007<br>(0.008)     | -0.021<br>(0.017)    | 0.015<br>(0.017)     | -0.004<br>(0.012)    |
| Public                       | -0.087***<br>(0.017) | 0.019***<br>(0.004)  | 0.292***<br>(0.014)  | -0.310***<br>(0.015) | -0.102***<br>(0.017) |
| Reform                       | 0.016<br>(0.015)     | 0.015**<br>(0.007)   | 0.008<br>(0.013)     | -0.023*<br>(0.013)   | 0.010<br>(0.010)     |
| Uninformed                   | -0.075***<br>(0.008) | 0.000<br>(0.002)     | -0.071***<br>(0.014) | 0.071***<br>(0.015)  | -0.013<br>(0.008)    |
| Public x Uninformed          | 0.005<br>(0.018)     | 0.004<br>(0.005)     | 0.067***<br>(0.018)  | -0.071***<br>(0.017) | 0.011<br>(0.010)     |
| Reform x Uninformed          | 0.046***<br>(0.010)  | -0.006<br>(0.004)    | 0.015<br>(0.016)     | -0.010<br>(0.017)    | 0.000<br>(0.010)     |
| Observations                 | 47,226               | 41,378               | 41,378               | 41,378               | 41,378               |
| Adjusted R <sup>2</sup>      | 0.060                | 0.012                | 0.146                | 0.150                | 0.062                |

NOTE: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors in parentheses are clustered at legislator level. Ordinary least squares regressions. Mixed blocks are excluded, and observations are at vote level. Restricted to legislators from the four largest parties (SP, CVP, FDP, SVP). The dependent variables are indicators taking on value 1 if a legislator (1) was present; (2) abstained; (3) voted yes; (4) voted no; (5) deviated from his party line. Legislator fixed effects, season fixed effects and covariates are always controlled for. *Uninformed* takes on value 1 (0) for legislators who are not (who are) members on the responsible committee.