

Openness and censorship in the European Union: An interrupted time series analysis

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Abstract

This study examines transparency and censorship in the Council of Ministers of the European Union from 1999 to 2009. We measure transparency by considering the timeliness of record release and the levels of censorship applied to records when (and if) they are released. We show that legislation introduced in 2001 (Regulation 1049) triggered a massive shift towards greater transparency, in line with its intention. However, we also show that the trend towards greater transparency has been interrupted by the enlargement rounds in 2004 and 2007. We attribute this fact to inexperience on the part of the new member states and the resulting need for censorship while these states adjusted to the negotiation styles in the Council.

Keywords

Censorship, Council of Ministers, democratic deficit, Eastern enlargement, legislative transparency

Introduction

Questions relating to transparency and censorship in the legislative process of the European Union (EU) have been a central concern for EU legislators over the last decades. Legislative transparency is attractive to policy practitioners, civil society, and the general public, as it is intimately connected with the opportunity to hold decision-makers to account for their negotiation behaviour and resulting decisions

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(Hall and Taylor, 1996; Naurin, 2007). However, existing research also suggests that transparency can make compromise decisions more difficult to reach (e.g. Cross, 2013a). Much of the debate surrounding transparency in the EU has thus revolved around the trade-off between granting access to legislative records in order to legitimate decisions, and censoring them to protect the decision-making process (Cross, 2013a, 2013b).

The centrality of questions relating to transparency and censorship in the EU is demonstrated by the efforts made to formalize transparency commitments into the EU treaties and secondary legislation. The Maastricht treaty was the first EU treaty to explicitly address the concerns pertaining to legislative transparency, as it demanded significant increases in transparency across all aspect of EU policy-making. Each successive treaty, from Amsterdam through to Lisbon, has further emphasized the need for legislative transparency within its text. The efforts to increase transparency culminated in 2001, when agreement was reached on formal rules regulating how legislative records are released to the public (Regulation (EC) number 1049/2001). This piece of legislation requires that all legislative records be fully released in a time-efficient manner unless they come under one of a series of exceptions outlined in article 4 of the regulation. These exceptions relate to a multitude of issues including the protection of private and public interests, and the protection of the integrity of the decision-making process. As such they provide significant opportunities for legislators to censor legislative records thought to be of a sensitive nature.

Previous academic work on EU transparency has shown that the opportunities to censor legislative records written into regulation 1049/2001 are used extensively to deny public access (Cross, 2013a, 2013b). We add to this literature by (a) assessing whether regulation 1049/2001 has been successful in increasing legislative transparency in the Council of Ministers over time, and (b) examining the effect of subsequent enlargements. To this end, we employ a new dataset capturing the levels of transparency and censorship applied to a total of 31,762 legislative records created by the EU between 1999 and 2009. This undertaking is especially pertinent given the ongoing negotiations within the EU to reform the legislation pertaining to EU transparency.¹

Transparency in the EU

Transparency, defined by Mitchell (1998) as ‘the availability of regime relevant information’, is crucial for evaluating the performance of key actors in a political system. Accordingly, Meijer (2013: 430) defines transparency more specifically as ‘the availability of information about an actor that allows other actors to monitor the workings or performance of the first actor’. Similarly, Grimmelikhuijsen and Welch (2012) define transparency as ‘the disclosure of information by an organization that enables external actors to monitor and assess its internal workings and performance’.

Building upon these definitions, it is also possible to differentiate between different *types* of transparency. Grimmelikhuijsen and Welch (2012) point to three different types of transparency, two of which are relevant here. First, *transparency of the decision-making process* relates to the degree to which the steps taken to reach a decision within a political institution are open to scrutiny. This is the form of transparency most relevant to the current study, which is concerned with whether or not this information is made available in a time-efficient and uncensored manner. Second, *transparency of policy content* relates to the information available about what has been decided, how it will be implemented, and what the implications are for affected groups. This type of information is also present in Council records and we aim to assess the degree to which it is transparent and publicly available. Finally, *transparency of outcome* refers to the provision of information about the implications and effects of policies once implemented. This form of transparency is less relevant to the current study, as we are concerned with the *policy-making* rather than *policy-implementing* part of the policy cycle.

In the EU context, the role of transparency in legitimizing political decisions has been emphasized by H eritier (1999), among others. This is especially important in light of the gap that many observers believe exists between EU legislators and their citizens (e.g. Follesdal and Hix, 2006).² Empirical evidence that transparency is associated with legitimacy and the related concept of trust has been provided by a series of experimental studies (De Fine Licht et al., 2011; Grimmelikhuijsen et al., 2013), lending support to H eritier's claim that the two are intimately connected. Nevertheless, it should also be kept in mind that while transparency is a *necessary* condition for accountability, it is not in itself *sufficient*: when information about the decision-making process is publicly available, this does not imply that this information will be publicized and utilized to hold legislators to account for their policy positions (Naurin, 2007).

Another strand of literature explores the implications of transparency for negotiator behaviour from a formal modelling perspective (Levy, 2007; Meade and Stasavage, 2008; Stasavage, 2003, 2004, 2006). The key mechanism examined in this literature emerges from the idea that legislators have two distinct utility concerns when negotiating legislation. First, legislators gain utility by achieving a favourable policy outcome, as in most decision-theoretic and game-theoretic models of negotiation. Second, legislators' utility also depends on reputational concerns with the principal whom they represent, be that superiors in government or the general public. Transparency enters the picture as it determines the amount of information available to observers about legislator behaviour during the negotiation process. When more information is available, observers are better able to attribute a reputation that accurately reflects legislator performance.

It follows that while transparency is appealing to those who perform well in negotiations, it is less appealing to those who perform poorly. Under conditions of transparency, it takes highly skilled and experienced negotiators to reach compromise decisions while appearing to do a satisfactory job in the eyes of observers. Indeed, succeeding in terms of both concerns may not always be possible.

Accordingly, the end result attained from these formal models is that negotiators' concern with their reputation may interfere with their ability to reach compromise decisions. The mechanism creating this potential adverse effect of transparency has found empirical support in a number of contexts, including deliberations in central banks (Stasavage, 2003), the US Federal Reserve (Meade and Stasavage, 2008), national parliaments (Stasavage, 2004), and the EU (Cross, 2013a).

Cross (2013a, 2013b) considered legislative transparency in the Council of Ministers of the EU from two different perspectives. He first examines how the levels of transparency anticipated by decision-makers affect their position-taking behaviour during negotiations. Drawing from the formal theoretical literature discussed above, he finds that increased levels of transparency are associated with increased polarization of the decision-making process: negotiators have a tendency to grandstand when they believe that legislative records demonstrating how they behaved during negotiations could be made public.

In a second contribution, Cross (2013b) finds that the use of censorship in the Council is strongly influenced by the total amount of controversy that arose during negotiations, the content of a particular legislative record, and the number of member states involved in the negotiation process. He also identifies an increasing trend in transparency over time and what appear to be structural breaks in this trend due to EU enlargement. The latter finding is notable as the evidence that enlargement has had an impact on EU politics is mixed (see e.g. Hertz and Leuffen, 2011). At the same time, Cross's findings are based on a small sample of legislative proposals and their associated legislative records, and it is therefore unclear whether they hold more generally. In the conclusion of his study, Cross calls for a more robust analysis of the finding that EU enlargement has negatively affected legislative transparency. It is this call the current study answers through the examination of a more comprehensive dataset of legislative records related to proposals decided upon between 1999 and 2009.

Current rules, key events, and according hypotheses

This study focuses specifically on the application of transparency policy in the Council of Ministers. Our general approach to capturing transparency and censorship in the Council proceeds by considering the manner in which records pertaining to legislative decisions are released. In particular, we consider two distinct aspects of transparency and censorship in the Council: (a) the timeliness of record release, and (b) the manner in which censorship is applied to the records of interest, if it is indeed applied. We further develop our theoretical expectations within a rational-choice institutionalist framework, assuming that political actors attempt to maximize their utility within the institutional constraints that apply in a given institutional context (Aspinwall and Schneider, 2000; Hall and Taylor, 1996).

In the Council, it is the Secretariat that is tasked with deciding on a record-by-record basis *which* records to release and *when* to release them. The rules that constrain the Secretariat when making these decisions are set out in

regulation 1049/2001. As discussed above, this regulation requires that all legislative records be released immediately unless justification for delaying or refusing release can be found in the set of exceptions included in article 4. As can be seen from Figure 1, which lists these exceptions, access to records can be refused for a large variety of reasons. These include concerns relating to the protection of public or private interests, the protection of legal proceedings, or issues relating to security or defence. Perhaps most interesting for the purposes of this study, access to records can be refused in order to protect the integrity of the decision-making process.

Article 4: Exceptions
<p>1. The institutions shall refuse access to a document where disclosure would undermine the protection of:</p> <p>(a) the public interest as regards:</p> <ul style="list-style-type: none">– public security,– defence and military matters,– international relations,– the financial, monetary or economic policy of the Community or a Member State; <p>(b) privacy and the integrity of the individual, in particular in accordance with Community legislation regarding the protection of personal data.</p>
<p>2. The institutions shall refuse access to a document where disclosure would undermine the protection of:</p> <ul style="list-style-type: none">– commercial interests of a natural or legal person, including intellectual property,– court proceedings and legal advice,– the purpose of inspections, investigations and audits, unless there is an overriding public interest in disclosure.
<p>3. Access to a document, drawn up by an institution for internal use or received by an institution, which relates to a matter where the decision has not been taken by the institution, shall be refused if disclosure of the document would seriously undermine the institution's decision-making process, unless there is an overriding public interest in disclosure.</p> <p>Access to a document containing opinions for internal use as part of deliberations and preliminary consultations within the institution concerned shall be refused even after the decision has been taken if disclosure of the document would seriously undermine the institution's decision-making process, unless there is an overriding public interest in disclosure.</p>

Figure 1. Regulation (EC) number 1049/2001; article 4.

This provides the Secretariat with plenty of opportunity to deny access, especially when it can be argued that the decision-making process might be adversely affected.³

Nevertheless, we would expect the 2001 regulation to cause a major shift towards greater transparency, given its intentions and content. The regulation became applicable on 3 December 2001, and we expect a clear shift to be evident in each of the time series of interest from this time onwards. In short, we expect that:

H1a: Regulation 1049/2001 decreased the length of time taken to release legislative records.

H1b: Regulation 1049/2001 decreased the use of censorship in the legislative process.

Our second set of hypotheses relate to how the enlargement rounds in 2004 and 2007 have affected the levels of censorship in the Council. The inclusion of new states in the negotiations has two key implications. First, adding new member states increased the preference heterogeneity in the Council (Naurin and Lindahl, 2010; Thomson, 2009). The new members differed notably from most existing members in terms of economic development, political organization, and other important country-level characteristics. Such heterogeneity increases transaction costs and the potential for conflictual negotiations. This in turn is likely to influence the levels of transparency we observe: when conflict exists, negotiators need the time and space provided by a closed-door setting to try to find a compromise. If legislative records showing conflict were made public before a compromise was found, this could lead to outside pressures from observers disrupting the negotiation process. The Council Secretariat may thus have good reason to invoke article 4.3 of the transparency regulation to censor such records after enlargements.

The second point to note about enlargement is that new negotiators would have to adjust to the consensual way of doing business in the Council (Heisenberg, 2005; Lewis, 2003, 2005). The predominance of consensus norms are widely held to explain the lack of overt conflict between negotiators in terms of negative voting behaviour (Hosli et al., 2011; Mattila, 2004, 2009; Mattila and Lane, 2001). Furthermore, such norms tend to develop in 'normatively dense institutional environments that contain 'a wide range of informal norms, rules and standards of appropriateness which prescribe and proscribe certain behavior and arguments' (Lewis, 2010). The Council is thus a prime example of an environment in which these norms can take hold. Of fundamental importance here, however, is the idea that these norms take time to develop, and that individual actors must be socialized into them. This is consistent with the observation by policy practitioners who were present at Council negotiations around the time of enlargement that newer member states took time to adjust to the prevailing norms (Lewis, 2010).

A final point to note is that norms of consensus should function best in closed-door settings (Lewis, 2010). Indeed, Hayes-Renshaw and Wallace (2006) state that ‘participants [in Council negotiations] often want to speak in unvarnished terms and to deploy arguments that they would not repeat so easily in a more explicit and public form’. In closed-door settings, ‘[m]inisters and their officials build coalitions, exercise leverage and do deals, benefiting from the veil of secrecy that largely cloaks their actions’. On the basis of these considerations the Council Secretariat has good reason to utilize censorship after enlargement, and this leads to the following hypotheses:

H2a: Enlargement increases the length of time taken to release legislative records.

H2b: Enlargement increases the use of censorship in the legislative process.

In light of the discussion above, an interesting question is whether any effect of enlargement can for the most part be attributed to the need for new member states to adjust to the norms of consensus, or simply to increased substantive conflict between negotiators. One key point here is that an increase in preference heterogeneity should give more persistent effects as it takes a long time for preferences to change, while any effect of inexperience on the part of new member states should be temporary and disappear as these states adjust to the prevailing norms of consensus. This offers one way of distinguishing between these mechanisms, and we will discuss this issue again below. Furthermore, if we can measure the level of conflict, we can control for this to test whether it explains the effect of enlargement.

Data and methods

The dataset used to test our hypotheses has been constructed by scraping information from the Council’s online Consilium database. In the first step of the data collection process, a list of all EU consultation and co-decision proposals between 1994 and 2013 was constructed by referencing this database. Using this list, a web crawler was then constructed to collect meta-information for all of the records associated with the proposals of interest. These meta-data contain information on the date that a record was created, the date that a record entered into the Consilium database, and details about whether a record was fully, partially, or not released. This dataset was reduced to focus on negotiations that took place between the beginning of 1999 and the end of 2009, as this time period is most relevant to our analysis. In total, we analyse 31,762 legislative records across 2,170 legislative proposals.

Our first dependent variable captures the delay between a record being created and released.⁴ We measure this as the number of days between its creation and the time at which it was entered into the Council database.⁵ Our second dependent variable captures the level of censorship applied to a legislative record.

Table 1. Lag of release (and non-release) by degree of censorship.

	Mean lag (in days)	N
Fully withheld	1.7	2489
Partially released	244.9	788
Fully released	226.0	16,139
Total	198.0	19,416

Note: the numbers refer to process-related documents for proposals created from 1 January 2003 until 31 December 2009. For fully withheld documents, the lag captures the time until a reference to the document appeared in the online database. Such lags are excluded from the analysis below, and provided only for the sake of completeness.

It is constructed as a three-level indicator of whether a record has been made fully available, partially available or is not available. We code non-released records as those that have not been made available, and partially released records as those where the identities of negotiators making interventions have been redacted, but the content of their interventions are present. This classification of document release reflects the way in which the Council applies transparency and censorship policy in practice and is similar to the coding scheme used by Cross (2013b).

To clarify the relationship between our two dependent variables, Table 1 reports a cross-tabulation of the lag of release by the level of censorship applied. For fully withheld records, there is a minimal lag of 1.7 days from the creation of the document until it is added to the database, suggesting that the decision to not release a record takes place almost immediately. As fully withheld documents have no meaningful lag of release to the public, we code these as missing values on the lag variable (although including these cases would not change our substantive results). Turning to the other categories, it is clear that the lag of release is marginally higher for partially released than fully released documents (244 vs 226 days). The fact that the difference is so slight implies only a weak relationship between partial release and longer lags. In other words, these outcomes seem to offer alternative ways of withholding information and may indeed serve different purposes: certain documents may only be sensitive for a given time and can be released with a lag, whereas others may remain sensitive and thus require more direct censorship.

We merge our data with an updated version of the EU policy-making dataset (Häge, 2011), which was extracted from another legislative database of the EU (PreLex). In the updated version of this dataset, we use the length it took to conclude negotiations for a proposal as a measure of conflict. In order to construct this variable, we follow the methodology of Häge (2011), who first identifies the event at the end of the negotiation process (adoption, partial adoption, and withdrawal of legislative proposals are all possibilities), and then extracts the date on which this event took place. The number of days between the end date and the start date (usually the date the Commission makes the formal legislative proposal)

represents the length of time a proposal was under negotiation. This variable serves as a proxy for substantive conflict, as more difficult negotiations take a longer time to resolve.

In our analysis, we also account for the type of legislative record under consideration, as certain record types are likely to be more sensitive than others in terms of containing information about the negotiation process. While we use dummy variables to identify the different types of document included in the analysis, we further differentiate between two broad categories of records: one pertaining to the negotiation process and one to the outcomes. Records related to decision outcomes include the Commission proposal, the final piece of legislation decided upon, and any statement of reasons released by the Council about how it came to a decision. These records are much less likely to require censorship, as they represent the outcome of negotiations at a particular point in the negotiation process and are thus intended to be made public. As such, we would not expect them to contain sensitive information with the potential to stall negotiations. In contrast, records relating to an ongoing negotiation process include progress reports, working documents, information notes, and other records that provide details about negotiations that have yet to be concluded. We are only interested in records that potentially contain sensitive information, so we focus solely on the second category of documents in our main analyses, while we use the first category for a placebo test in our discussion of internal validity.

As explained below, we use a research design that greatly reduces the number of plausible confounders, but we nevertheless include a range of control variables. In particular, we control for the formal involvement of the European Parliament (EP) by including a dummy for the legislative procedure involved. Under consultation, the Parliament is limited to providing a non-binding opinion on a legislative proposal, whereas under co-decision (now called the 'ordinary legislative procedure'), the Parliament must formally approve any legislation with the Council. In negotiations where the Parliament has a formal role, the Council will have incentives to hide any internal conflict within its ranks, so as to present a united front when negotiating with Parliament officials. This is expected to lead to increased levels of censorship of legislative records. We further include a variable capturing whether or not a proposal is new or an amendment to existing legislation, as the latter may be less controversial than the former. As the proposal title consistently identifies amended proposals as such, this variable is constructed using a simple word match algorithm that checks for the word stem 'amend' in the title of a proposal.

Lastly, we control for the policy area that characterizes a proposal under the assumption that certain policy areas are more sensitive than others, and therefore likely to be subject to more censorship. We thus construct a variable identifying the Commission Directorate General (DG) primarily responsible for the particular legislative proposals under consideration. These data are also found on the PreLex website. The assumption here is that each DG within the Commission deals with a different policy area, so using the DG from which a proposal originates is a reasonable proxy for the relevant policy area.

A key motivation of this study is to draw reliable causal inferences by utilizing an interrupted time series (ITS) design (see e.g. Morgan and Winship, 2007). The basic idea behind the ITS design is to use the exact timing of the events we are interested in and examine data on the relevant outcomes before and after these events, or ‘treatments’. The key question is whether we find a discontinuity at the time of treatment. ITS thus resembles the sharp regression discontinuity design (e.g. Imbens and Lemieux, 2008), but with time as the forcing variable. By properly modelling the dynamics in the dependent variable before and after a given event, we can identify the causal effect of this event as the discontinuity in the dependent variable. The assumption is that no other relevant events occurred at the exact same time, as we discuss and test further below.

We conduct our study at two different levels of analysis. First, we consider monthly averages of the dependent variables of interest, allowing us to treat the data as a time series, inspect relevant patterns over time, and develop ITS models in a transparent way, while considering time series diagnostics. We thus generate a monthly time-scale, categorize all records by their date of creation, aggregate our variables by this monthly time-scale, and proceed to analyse the resulting monthly average time series. Having developed appropriate models, we move these models down to the level of individual documents, which allows us to utilize all the available information in the data, and include additional document-level covariates.

To test the impact of each treatment on each dependent variable, we specify a separate time series model for each event of interest. We start by selecting a window of observations around the time of each treatment, thus focusing on the most relevant observations. These windows should be sufficiently wide to allow a reliable analysis, and sufficiently narrow to exclude irrelevant observations and trends that would call for unnecessarily complex models. We select windows covering two years before and after each treatment (a total of 48 months). For each event of interest, we further create a binary ‘treatment indicator’ (T) capturing whether the event had occurred by the time a record was created.

For each specific analysis, we centre the time variable (t) on the time of treatment (so that $t=0$ when T goes from 0 to 1). In the models, we include t as a predictor to capture linear trends, and to account for any change in the trends after treatment, we interact T and t . Our analyses are thus based on the following general model:

$$Y_t = \beta_0 + \beta_1 T_t + \beta_2 t + \beta_3 T_t t + \sum_{m=1}^M \beta_{m+3} X_{mt} + \varepsilon_t \quad (1)$$

where β is a vector of coefficients, X is a matrix of M additional covariates, and ε_t is an error term. The centring of t has the advantage that the coefficient on T (β_1) can be interpreted as the immediate effect of treatment (at $t=0$), which is the effect we are the most interested in.

A notable feature of our data is that a clear seasonal pattern is present, reflecting the annual legislative calendar in the EU. Legislative activities in the EU show significant drops around Christmas and in the summer months of July and August, as negotiators take holidays during these periods. We therefore include a set of dummy variables in all our models to capture monthly average differences. Turning to the diagnostics of our models, these are generally acceptable: the residuals are significantly stationary for all our models and there are few signs of serial correlation. In this light, we do not attempt to model further dynamics, but stick to models based on equation (1), adding monthly dummies, and, in some instances, further controls.

Developments in transparency and censorship: 1999–2009

We start by examining the developments in openness and censorship over the whole period in question. Figure 2 shows the average lag in the release of EU records to the public from January 1999 until December 2009. The figure also includes four vertical lines representing the events that we focus on in this paper: two lines show when Regulation 1049/2001 (a) was adopted and (b) became applicable, and two further lines represent the first and second enlargements (1 May 2004 and 1 January 2007, respectively).

As expected, a major shift took place in relation to the 2001 regulation, lending support to *H1a*. For records created before this regulation became applicable, the average lag could be as high as 1,500 days or more, while it afterwards settled at about 250 days. The large drop in Figure 2 appears to occur before the treaty became applicable, but this is partly a matter of coding: the records in question have been categorized by their date of creation rather than their date of release. Once the treaty became applicable, a large number of records were released that had been created much earlier, resulting in a decline that appears to precede the regulation. However, the regulation does also seem to have had an actual effect before becoming applicable. If we look at the volume of released documents by their release date (which is not shown in the figure), this volume increased in two distinct steps: it first increased in June 2001 suggesting that the Council Secretariat started releasing more records as soon as the regulation had been adopted (on 30 May 2001). The volume then increased further once the regulation formally became applicable in December 2001. Overall, the main point to notice about Figure 2 is that, as expected, regulation 1049/2001 thoroughly transformed the release of EU legislative records to the public.

However, there are also further trends and events worth noting within the new regime of openness starting with the 2001 regulation. First, there is a generally declining trend in the lag of release, continuing towards the end of the series. The efforts to increase public access to records in a timely fashion also appear successful in this sense. In 2000 the average lag of release was 1,115 days ($N=864$), in 2002 this was down to 279 days ($N=2,223$), and in 2009 the lag had further declined to 123 days ($N=2,231$). However, temporary increases in the lag are also evident,

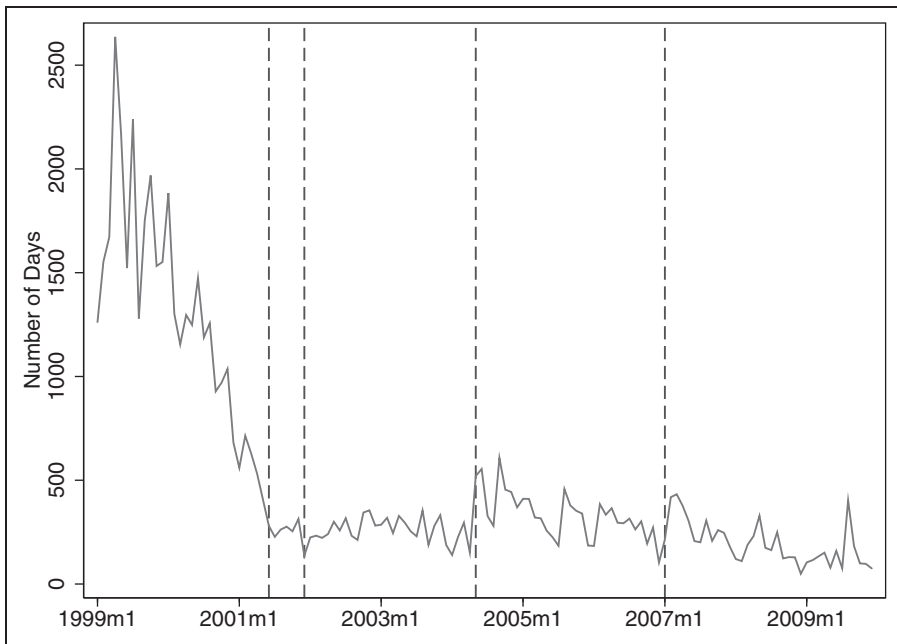


Figure 2. Lag in the release of EU documents to the public over time.

Note: the plotted lag represents an average for the documents produced each month. The first two vertical lines show when the 2001 regulation regarding public access to EU documents was adopted and became applicable, the third line represents the first batch of the Eastern enlargement, while the fourth represents the second batch.

occurring in relation to the 2004 and 2007 enlargements. This lends some preliminary support for *H2a*, which we test more thoroughly below.

Figure 3 shows the development in partial and full censorship of records from 1999 until 2009. It includes the same vertical lines as Figure 2 to identify the events of interest. Again, we see a major shift in relation to the 2001 regulation, providing support for *H1b*. Before the regulation became applicable, release of records to the public was rare; if we coded records by the time of their release instead of their creation, this would be even clearer: no records were released before July 2000. This is almost certainly due to the fact that the database we are examining did not exist publicly before this point in time. From then on, the share of released records increased, especially in the last months before the 2001 regulation became applicable. Of the records created in 2002, only 12 percent were fully withheld from the public. Thus, again, we can see how regulation 1049/2001 transformed public access to EU records and increased legislative transparency. As this effect appears somewhat gradually, yet is so clearly evident in the data, we do not carry out an ITS analysis for regulation 1049/2001, but rather focus on the effects of enlargement.

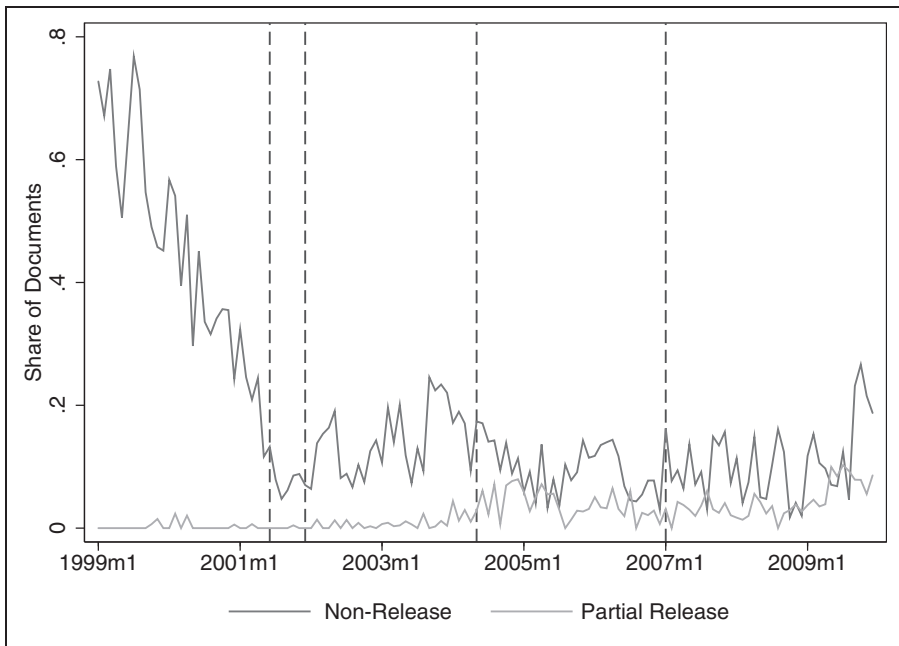


Figure 3. Degree of public access to EU documents over time.

Note: the plotted lines represent the shares that each type of public release or non-release make up of the total number of documents produced each month. Documents that are neither partially released nor withheld from the public (non-release) are fully released. The vertical lines represent the same events as those in Figure 2.

A few more points are worth noting about Figure 3. In general, the proportion of withheld records has remained fairly stable after it settled at its new level, thus providing little support for *H2b*. However, the figure also shows that the share of partially released records has increased over time, in particular after the 2004 enlargement. This is consistent with *H2b*, which we test more thoroughly below.

The 2004 enlargement

To test *H2a* more explicitly and assess if the 2004 enlargement had the expected impact on public access to records, we specify a set of time series models based on equation (1) above. The first of these examines the impact of the enlargement on the lag in the release of records to the public, and is reported in Table 2. Model 1 is a time series model and the results presented add credence to the impression of the enlargement having a large impact, as illustrated in the corresponding Figure 4. It is also worth noting the steeper decline of the trend after enlargement, which suggests that the impact was temporary, as can also be seen from Figure 2 above.

Table 2. Impact of the 2004 enlargement on lag of release.

	Model 1	Model 2	Model 3	Model 4
Level of Analysis	Months	Documents	Documents	Documents
Link Function	None	Neg. Binom.	Neg. Binom.	Neg. Binom.
Enlargement (<i>T</i>)	279.717*** (47.349)	1.744** (0.302)	1.773*** (0.308)	1.872*** (0.329)
Time (<i>t</i>)	-4.988* (1.877)	0.983* (0.008)	0.982* (0.008)	0.981* (0.008)
Enlargement × Time	-5.508 (3.289)	1.030* (0.013)	1.020 (0.013)	1.015 (0.012)
Length of Negotiation			1.867*** (0.143)	1.863*** (0.147)
Co-Decision				0.656** (0.088)
Amendment by Commission				0.902 (0.095)
Constant	172.075*** (46.156)	19.991*** (8.768)	0.571 (0.340)	0.855 (0.521)
FE, Month	Yes	Yes	Yes	Yes
FE, Policy Area	No	Yes	Yes	Yes
FE, Doc. Type	No	Yes	Yes	Yes
Dispersion		3.066*** (0.060)	2.996*** (0.060)	2.986*** (0.060)
Observations	48	8280	8280	8280
Window	48	48	48	48
R^2	0.642			
R^2 , adj.	0.490			

Note: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The reported estimates for Models 2, 3 and 4 are IRRs. The aggregate (monthly) time series model is weighted by the number of observations per month. For this model, the constant refers to the month closest to the overall average for the period included in the analysis. For the document-level models, the standard errors are clustered by legislative proposal.

To examine this result further, we estimate additional models at the document level, which allows us to include more controls. To account for the count nature of the dependent variable at this level, we employ negative binomial models (the significant dispersion-parameters confirm that the variable is overdispersed). Furthermore, our models at this level include a set of dummy variables to control for policy area, the most plausible confounder for the effect in question: if the enlargement caused more legislative proposals in politically sensitive areas, this

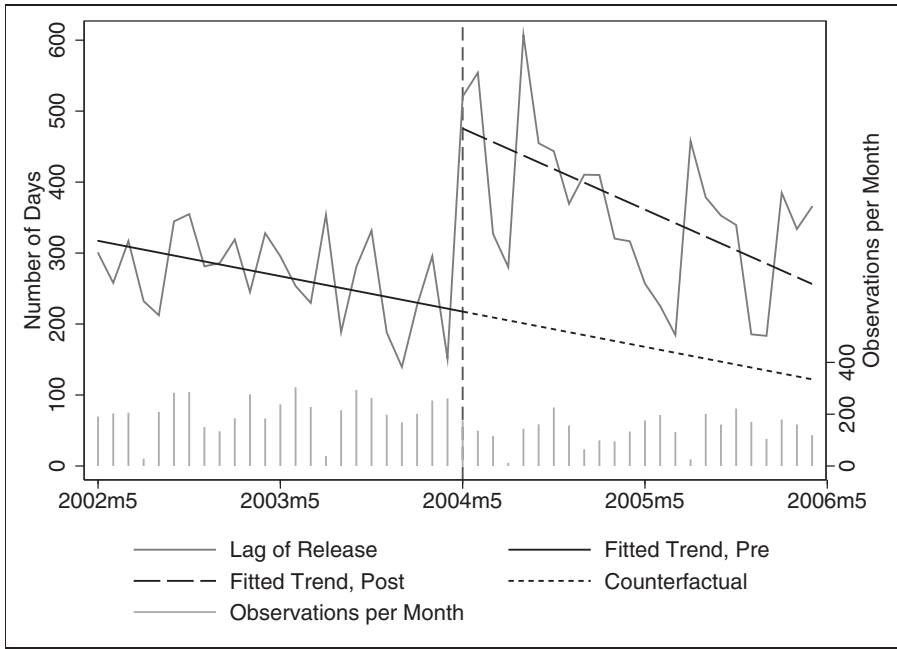


Figure 4. Impact of the 2004 enlargement on the lag of release to the public.
 Note: the plotted lag of release represents an average per month. The fitted lines are weighted by the number of documents contributing to each monthly average; months with few observations are given less weight and may appear as outliers.

might explain the pattern, and call for a different interpretation of the enlargement effect. Lastly, the models include dummy variables capturing document type, in case enlargement increased the relative production of sensitive documents.

Model 2 thus replicates Model 1, but with documents as the unit of analysis, rather than months, and including the mentioned set of dummy variables. In Model 2, the incidence rate ratio (IRR) for the effect of enlargement is 1.74, implying that enlargement increases the expected lag of release by a factor of about 1.74, or 74 percent, all else equal – a very large and statistically significant effect. As discussed above, this effect could be due to increased controversy and substantive conflict. As a first step in assessing this possibility, we plot the length of negotiations over time in Figure 5. As we might expect, the figure shows a clear increase at the time of enlargement, consistent with the assumption that controversy increased. To assess whether this accounts for the effect of enlargement on the lag of release, we include the log of this variable in Model 3. However, while the lag of release is indeed greater for longer negotiations, this control does not reduce the effect of enlargement, thus offering little support for the notion that increased conflict accounts for the effect. Lastly, Model 4 includes the indicators of Parliament involvement and whether a proposal is an amendment or not.

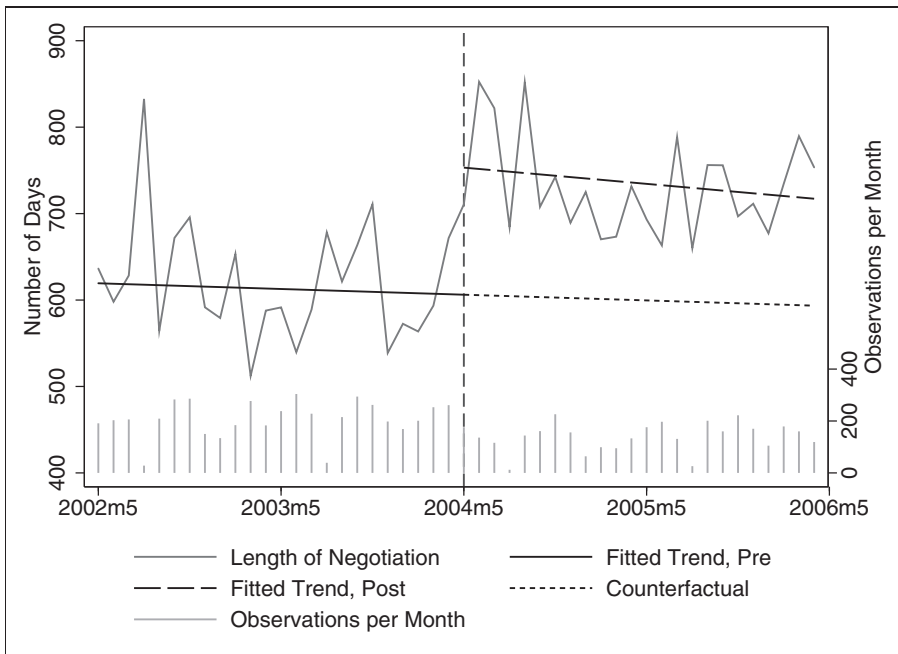


Figure 5. Impact of the 2004 enlargement on the length of negotiations.
 Note: the plotted length of negotiation represents an average per month. The fitted lines are weighted by the number of documents contributing to each monthly average; months with few observations are given less weight and may appear as outliers.

These controls do not reduce the estimated effect of enlargement, which remains large and highly significant. The IRR of 1.87 implies that a document with an expected lag of 200 days before the enlargement would have an expected lag of about 375 days after the enlargement, thus providing strong support for *H2a*.

Table 3 reports similar analyses of the share of fully and partially censored records (Models 1 and 2, respectively). Contrary to what was expected according to *H2b*, Model 1 suggests that the 2004 enlargement did not have an effect on the share of fully censored records. In contrast, Model 2, which analyses the share of partially released records, does detect a notable impact of enlargement, as illustrated in Figure 6. This effect may also appear somewhat transient, although Figure 3 also suggests some of the effect may have remained for an extended period.

As before, we examine this result in more detail at the document level, adding further covariates. We employ a logit link function to account for the nature of the dependent variable at this level.⁶ Model 3 replicates Model 2 at the document level, while Model 4 controls for the length of negotiation. Both models show clear effects of enlargement, although the statistical significance is somewhat reduced in Model 4 (the t-value is still 1.86, which is significant at the 5% level in a

Table 3. Impact of the 2004 enlargement on censorship of documents.

	Model 1 Non-Rel.	Model 2 Partial Rel.	Model 3 Partial Rel.	Model 4 Partial Rel.	Model 5 Partial Rel.
Level of Analysis	Months	Months	Documents	Documents	Documents
Link Function	None	None	Logit	Logit	Logit
Enlargement (<i>T</i>)	-0.066 (0.038)	0.044** (0.013)	0.972* (0.447)	0.889 (0.477)	0.867 (0.460)
Time (<i>t</i>)	0.003 (0.002)	0.000 (0.000)	0.089*** (0.026)	0.097*** (0.027)	0.098*** (0.027)
Enlargement x Time	-0.004 (0.002)	-0.001 (0.001)	-0.103** (0.033)	-0.112** (0.035)	-0.113** (0.036)
Length of Negotiation				0.640* (0.297)	0.628* (0.285)
Co-Decision					0.094 (0.403)
Amendment by Com.					0.195 (0.439)
Constant	0.192*** (0.045)	0.016 (0.009)	-6.895*** (1.383)	-10.681*** (2.239)	-10.734*** (2.225)
FE, Month	Yes	Yes	Yes	Yes	Yes
FE, Policy Area	No	No	Yes	Yes	Yes
FE, Doc. Type	No	No	Yes	Yes	Yes
Observations	48	48	9552	9552	9552
Window	48	48	48	48	48
<i>R</i> ²	0.380	0.689			
<i>R</i> ² , adj.	0.116	0.557			

Note: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The aggregate (monthly) time series models are weighted by the number of observations per month. For these models, the constant refers to the month closest to the overall average for the period included in the analysis. For the document-level models, the standard errors are clustered by legislative proposal.

one-tailed test). For a typical document, the effect is reduced about nine percent from Model 3 to Model 4, providing only limited support for the notion that increased substantive conflict explains the results. Model 5 further introduces the same controls as Model 4 in Table 2, which makes no practical difference for the results. The effect in Model 5 is still considerable: for a reasonably typical document with a 1.3 percent predicted probability of being partially released before the enlargement, the equivalent after enlargement is 3.1 percent: about 2.4 times higher.⁷ While enlargement did not make it more likely that documents would be completely withheld from the public, it did increase the

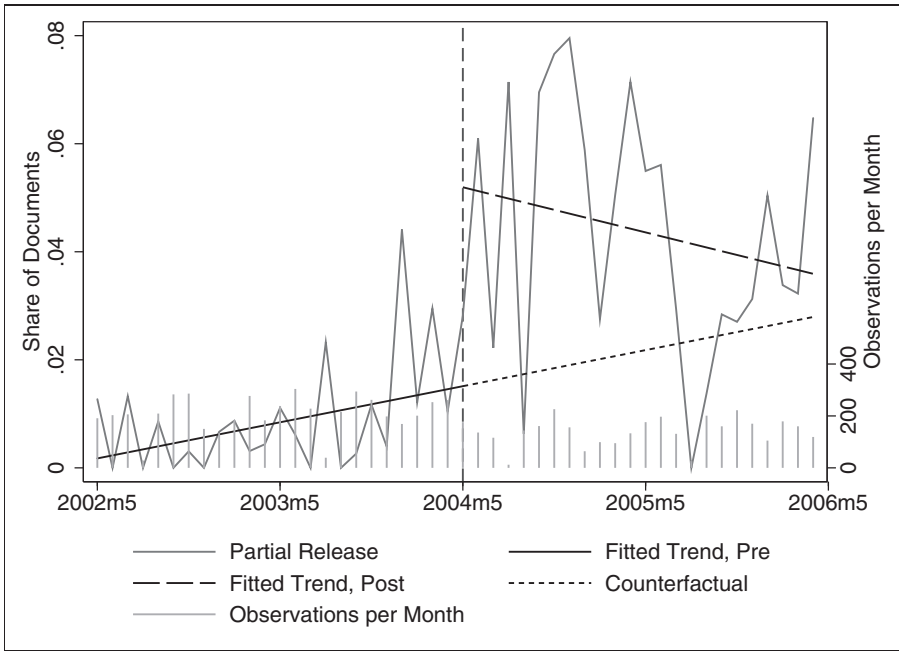


Figure 6. Impact of the 2004 enlargement on the probability of partial censorship. Note: the fitted lines are weighted by the number of documents contributing to each monthly average; months with few observations are given less weight and may appear as outliers.

likelihood that they would be partially censored before their release, lending partial support to *H2b*.

The 2007 enlargement

To further test our hypotheses and see if the smaller enlargement in 2007 had an impact on transparency and censorship in the Council, we specify another set of time series models, reported in Table 4. Model 1 examines the impact of the enlargement on the share of withheld records, and, contrary to what was expected, the model fails to find an effect. Similarly, Model 2 analyses the share of partially released records without finding any effect. Model 3 analyses the lag in the release of records to the public. The estimated effect of the enlargement is substantial, but not statistically significant. However, moving to the document level in Model 4 increases the statistical power of our test and uncovers a significant effect. The IRR of 1.58 implies that enlargement increased the expected lag of release by 58 percent, all else being equal. This is slightly less than the effect of the 2004 enlargement, but nevertheless remains a considerable effect. Furthermore, introducing our measure of conflict in Model 5 does little to reduce the effect, offering limited support for the notion that the effect is due to

Table 4. Impact of the 2007 enlargement on lag of release and censorship of documents.

	Model 1 Non-Rel.	Model 2 Partial Rel.	Model 3 Lag of Rel.	Model 4 Lag of Rel.	Model 5 Lag of Rel.	Model 6 Lag of Rel.
Level of Analysis	Months	Months	Months	Documents	Documents	Documents
Link Function	None	None	None	Neg. Binom.	Neg. Binom.	Neg. Binom.
Enlargement (<i>T</i>)	0.047 (0.028)	0.002 (0.007)	76.245 (38.263)	1.582* (0.286)	1.500* (0.274)	1.573* (0.279)
Time (<i>t</i>)	-0.001 (0.001)	-0.001* (0.000)	-3.480 (2.441)	0.971** (0.010)	0.975* (0.010)	0.977* (0.010)
Enlargement × Time	-0.002 (0.002)	0.001** (0.000)	-5.486 (2.778)	0.981 (0.013)	0.982 (0.013)	0.981 (0.012)
Length of Negotiation					1.296** (0.110)	1.346*** (0.113)
Co-Decision						0.558*** (0.084)
Amendment by Com.						1.033 (0.098)
Constant	0.075 (0.043)	0.019** (0.006)	258.486*** (35.407)	8.330*** (3.401)	2.043 (1.286)	2.769 (1.765)
FE, Month	Yes	Yes	Yes	Yes	Yes	Yes
FE, Policy Area	No	No	No	Yes	Yes	Yes
FE, Doc.Type	No	No	No	Yes	Yes	Yes
Dispersion				3.072*** (0.070)	3.062*** (0.069)	3.043*** (0.070)
Observations	48	48	48	8550	8550	8550
Window	48	48	48	48	48	48
R ²	0.355	0.589	0.696			
R ² , adj.	0.082	0.414	0.567			

Note: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The reported estimates for Models 4, 5 and 6 are IRRs. The aggregate (monthly) time series models are weighted by the number of observations per month. For these models, the constant refers to the month closest to the overall average for the period included in the analysis. For the document-level models, the standard errors are clustered by legislative proposal.

increased substantive conflict. Lastly, Model 6 introduces our full set of controls, with the effect remaining as strong as before.

Overall, *H2b*, which claims that censorship increased after enlargement, does not receive support in this case. The 2007 enlargement only appears to have influenced the lag of release, and somewhat less than the 2004 enlargement did. This is not very surprising, however, given that the 2007 enlargement only involved two new member states as compared to 10 in 2004, so the need to adjust transparency policy would have been more moderate.

Internal validity

Before we conclude, we will briefly discuss the internal validity of this study. The key strength of the ITS design is that it greatly reduces the number of plausible confounders: by identifying discontinuities at the time of treatment, the only relevant confounders are those that would show a similar discontinuity at the same point in time. This is the reason why we do not include an indicator for change in Commission leadership, for example. Such changes have been argued to influence various aspects EU policy-making, and could also therefore be relevant in this context. However, the first Barroso Commission took office on 22 November 2004, which could hardly explain the abrupt increase in censorship at the time of enlargement on 1 May 2004. Similarly, the new European Parliament that convened on 20 July 2004 does not offer a convincing explanation for the observed shift in May.

In short, a plausible explanation to undermine our conclusions would have to exhibit a clear shift around the time of enlargement. Such an explanation would most likely be related to the enlargement itself, but reflect an alternative causal mechanism. One possible mechanism could be related to the administrative capacity to deal with translation issues: it might take longer to make documents publicly available when these have to be translated into new languages. At this point, however, another feature of our design comes into play, namely the use of two outcome variables. While increased translation demands might explain a delay in release, it cannot explain the increase in partial censoring of documents, which reflects deliberate censorship on documents already in existence. The translation argument is therefore inconsistent with the evidence presented above.

Furthermore, we conduct one additional test to increase our confidence in the results. As explained above, we distinguish between documents related to the process of negotiation and those related to the final outcome. For documents of the latter kind, which we left out of the analyses above, we would not expect an increase in censorship, as they are intended for the public. In this sense, the observed outcomes for these documents can be seen as ‘placebo outcomes’, offering a straightforward placebo test, where finding an effect would undermine the credibility of our research design. This offers an additional opportunity to test rival explanations, such as those relating to translation, as an increase in translation demands should apply to outcome-related and process-related documents equally.

Figure 7 shows the lag of release for outcome-related documents over the relevant period for the 2004 enlargement. As can be seen, there is no increase at the time of enlargement. In fact, when a model similar to those reported above was fitted, it yielded a slightly negative and statistically insignificant estimate. Doing the same for partial release is not possible, as there are *no* cases of censorship for these documents (and thus no increase after enlargement). These findings are fully consistent with our argument and increase our confidence in the results.

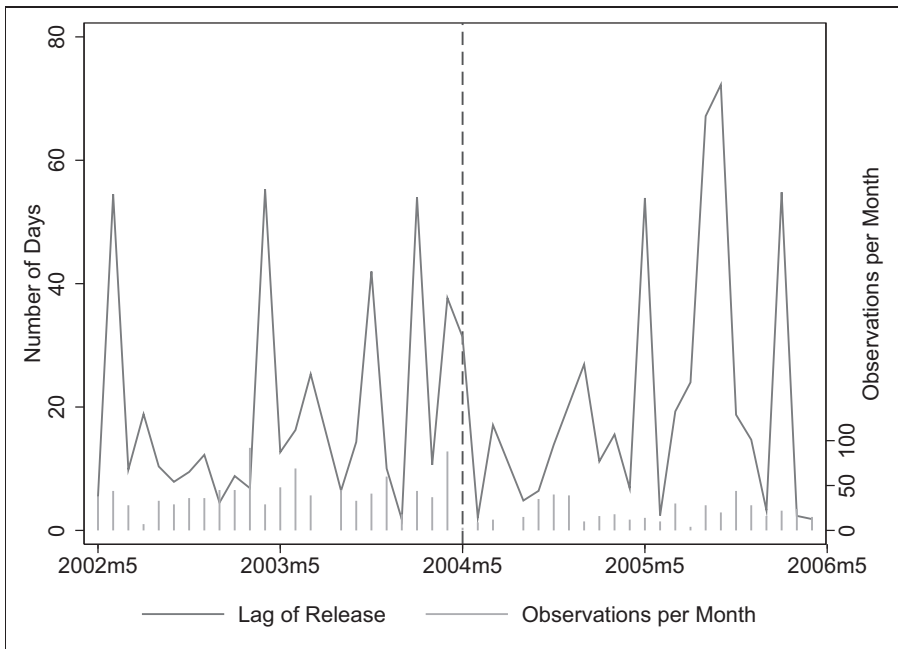


Figure 7. Placebo test looking at the lag of release for outcome-related documents. Note: the plotted lag represents an average for the documents produced each month.

Discussion and conclusion

A number of conclusions can be drawn from this study about legislative transparency in the Council. First, the introduction of regulation 2001/1049 had a major impact on the amount of information available about Council negotiations. Before its introduction, the standard approach was not to release documents relating to negotiations, while after its introduction, most documents are eventually released to the public. Furthermore, transparency has gradually increased over the period, at least in terms of the timeliness of record release. Regulation 1049 has thus been very successful in its stated aim of increasing legislative transparency.

However, when one delves deeper into how these records are released to the public, a more mixed picture of legislative transparency emerges. Questions still arise about whether records are made available to the public in a time-efficient manner. The timing of the release of records is important, as it relates to outsiders' ability to observe negotiations before a decision has been reached. We found that although there have been significant reductions in the time taken for process-related records to enter the public database, at the end of our series there is still an average lag of 123 days, which in many cases may be enough time to reach compromise decisions.

Furthermore, despite the broader trends in decreasing censorship, two distinct structural breaks were identified in our time series, due to the enlargement rounds in 2004 and 2007. The first and most ambitious enlargement caused a significant increase both in the delay of record release and the levels of censorship applied to released records. The smaller enlargement in 2007 triggered a lesser, but still significant increase in the lag of release. Our placebo test using outcome-related documents show no such pattern, suggesting that censorship indeed took place during negotiations, and that the exceptions outlined in article 4 of the 2001 regulation were seeing frequent use in these periods.

We have noted that two related mechanisms are likely to cause censorship following enlargements. The first is increased conflict due to greater preference heterogeneity, calling for censorship to facilitate the search for compromises. A sharp discontinuity in the length of legislative negotiations after the 2004 enlargement is consistent with the notion that substantive conflict increased, but our analysis offers little support for the idea that this explains the increase in censorship. Furthermore, an increase in preference heterogeneity should have lasting effects, as preferences change slowly, while the enlargement effects appear to be short-lived. This is more consistent with our other proposed mechanism: newer member states took time to adjust to the norms of consensus in the Council, leading the Secretariat to censor records to protect the decision-making process and supply the necessary space for new member states to adapt. Overall, the evidence appears to be mostly consistent with the latter mechanism, although both may have been at work.

Future research should investigate to what degree negotiations were affected by enlargement, and to what degree new negotiators take time to adjust to the style of Council negotiations. Such research has the potential to inform us about the effects that significant changes in membership can have on negotiations within political institutions more generally, and on the levels of transparency surrounding such negotiations. Future research could also examine in more detail some of the results we have paid less attention to here, for instance that involvement of the Parliament in the legislative process may decrease the transparency of Council negotiations.

While our results demonstrate that transparency within the Council has improved in important ways over the last 15 or so years, it must also be kept in mind that we have considered only one form of transparency. How the decision-making process actually functions and how different parts of the Council interact with one another to produce policy remains opaque. We are provided with descriptions of how this process works in theory on EU websites, but it is almost impossible to get a complete view of Council politics from the highly atomized manner in which records are presented through online databases. It is also difficult to assess the degree to which the provision of records translates into outside actors *accessing* and *utilizing* said information to monitor EU policy-making. It seems clear that the provision of Council records has been a boon for interested parties with motivations to delve into the records, and when one looks at the annual reports produced by the Council secretariat, one can see such groups highly represented in the user

information presented. However, this does not necessarily translate into a broader public engagement with EU politics and increased understanding of how the EU makes decisions. For non-experts, the decision-making process remains byzantine, and the provision of access to legislative records has done little to ameliorate this fact. This is perhaps the most serious threat to the legitimacy of the Union.

Lastly, our results show that further improvements in transparency are possible, which is something for legislators to keep in mind when negotiating the proposal introduced by the Commission in 2008 reform the EU's transparency policy. The inherent trade-off between increasing transparency on the one hand and the adverse effects that these efforts can have on the negotiation process on the other should be a central concern in these negotiations. Judgements have to be made about what the appropriate levels of transparency are, and, given differing national-level approaches to transparency in politics (compare, for instance, the rather secretive French approach to politics and the more open Scandinavian approach), these questions are sure to engender much controversy. We leave answering these questions to the policy-makers, but hope that our analysis can inform the ongoing debate by showing the trends in legislative transparency since 1999, and, in particular, the effects of the 2001 regulation and the Eastern enlargements.

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The authors have contributed equally and are rotating the order of authorship across several articles.

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Notes

1. Records related to these ongoing negotiations can be found at http://ec.europa.eu/prelex/detail_dossier_real.cfm?CL=en&DosId=196983.
2. There is, however, a strikingly strong relationship between the pace of European integration and the views of EU citizens over time (see Bølstad, 2015).
3. To be clear, we do not claim that this is necessarily a bad thing, as in many cases the exceptions are completely justified. Here, we are more concerned with how the regulation is being implemented. This in turn can inform the debate about whether or not the current regulations should be reformed.

4. In 102 cases (0.38% of the total), this results in a negative figure. We believe this is the result of human error in the process of entering the data into the Council database. Most of these negative values (59 of them) are -1 , while the second most common value is -2 . There is also an instance of -364 . These numbers appear likely to have resulted from typing errors and we have recoded these cases to give them a 0 lag. Excluding these cases would not affect the results noticeably.
5. In a recent update of the Consilium website, the structure of these pages has changed so as not to include the archive date any longer.
6. To save space, we focus on partial release, and report a logit model rather than a multinomial logit model. The reported model predicts the probability of partial release versus either full release or non-release. Excluding non-release from the analysis would not noticeably affect the results, and neither would a multinomial model give substantively different results.
7. The effects related to Models 3 and 4 were calculated for a note regarding energy and transportation, created in June with an average duration of negotiation. The probabilities pertaining to Model 5 were additionally calculated for a note related to a proposal that is not an amendment, but subject to co-decision.

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