Treaty reforms and legislative efficiency in the EU: An interrupted time-series analysis.

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Abstract

This study examines whether a series of EU treaty changes designed to increase the efficiency of the legislative process have had the intended effect. It looks at whether or not the Amsterdam, Nice and Lisbon treaties have successfully increased the speed with which the EU creates new laws. An interrupted time-series analysis approach is utilised to detect the total effects of treaty change on the decision-making process. In using this approach, we avoid issues with endogeneity that may be present in previous studies and therefore draw more reliable causal inferences. The findings presented suggest that the Amsterdam and Lisbon treaties were the most successful at increasing legislative efficiency, while the Nice treaty was less so. Changes to legislative procedures account for most of the total effects of treaty change, but not all of it. Furthermore, we find significant variation in the effects of treaty changes across different types of EU legislation.

Draft paper prepared for the Annual Meeting of the Swiss Political Science Association, Bern, January 30-31, 2014, and the Annual Retreat of the European Politics Group at the Center for Comparative and International Studies (CIS), ETH Zurich, January 27-29, 2014.

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Introduction

The European Union has gone through a series of treaty changes since its inception in 1958. The Single European Act, the Maastricht treaty, and the treaties of Amsterdam, Nice and most recently Lisbon have helped shape the EU into the highly complex international institution that we see today. Of these treaties, Amsterdam, Nice and Lisbon were initiated in order to prepare the EU for the significant eastern enlargements starting in 2004. The aim of these treaties was first to streamline the decision-making process in order to avoid legislative gridlock (Garrett, 1992; Tsebelis and Kreppel, 1998) and second to improve the democratic legitimacy of EU decision-making by increasing the involvement of the European Parliament (Lodge, 1994; Williams, 1990). As a result, these treaty changes have had a profound effect on the character of the Union and the balance of power between the institutions and member states involved in the legislative process. They have aimed to adapt the EU to the challenges of governing Europe in a democratic and efficient manner.

This study aims to assess how successful Amsterdam, Nice and Lisbon have been at addressing the second of these concerns: the efficiency of the decision-making process. It does so by considering how each treaty change has affected the EU's ability to legislate successfully and in a time efficient manner. The dependent variable of interest is the time it took the EU institutions to agree upon a large set of legislative decisions made between 1997 and the end of 2012, and we examine this variable in order to test the claim that treaty changes have increased the legislative efficiency of the EU. Using an interrupted time-series analysis, we show that there is significant variation in the effects that each treaty change has had on legislative efficiency in the EU. This methodological approach allows us to avoid issues with endogeneity that are potentially present in the current literature. The study further adds to the existing literature in this area by estimating the *total* effect of each treaty change on decision-making efficiency across different legislative instruments.

The study is structured as follows. We first explore the existing literature on the effects of treaty changes on the efficiency of EU decision-making, and identify a number of potentially problematic issues that are addressed in the current study. We then proceed to describe the specific changes to the institutional structure brought about by each treaty revision, deriving a

number of theoretical expectations that clarify the effect that these specific treaty changes are expected to have on decision-making efficiency. We then describe the data and methods that are to be used to test these theoretical arguments, and following that present the results of our analysis. We conclude with a discussion of the implications that our findings have for assessing the effectiveness of previous treaty changes and for future treaty reforms at the EU level.

Existing literature

A well developed literature exists that considers the effects of legislative institutions on the capacity of the EU to make decisions, and the effects that changing these institutions have on this capacity. As the tools of analysis utilised in political science have become more and more advanced, the academic debate accompanying each institutional change has become more and more nuanced, moving from descriptive and anecdotal accounts of the effects of reforms to highly sophisticated quantitative analyses of different aspects of the decision-making process.

The question of legislative efficiency in the EU has been of central concern in this literature. Following the implementation of the Single European Act in 1987, a series of studies suggested both that the EU faced significant difficulties in legislating due to Council inefficiencies (Scharpf, 1988), and that the decision-making process was facing increasing legislative gridlock (Wessels, 1991). While these studies pointed to potential problems affecting the efficiency of the legislative process, they failed to systematically and empirically assess to what degree a slow down in legislative efficiency was actually occurring over time.

In response to this lack of systematic investigation, Schulz and König (2000) considered variation in the length of time between a proposal and the final decision outcome for a set of 3,708 legislative decisions made between 1984 and 1994. Utilising an event-history analysis approach, they document a number of findings relating to legislative efficiency. First they find that increasing the use of the qualified majority rule decreases the proposal-decision time lag. Second, they find that increasing the participation of the Parliament through increased use of co-decision increases the duration of the decision-making process. Third, they find that measures pertaining to policy areas that constitute the functional core of the EU have shorter time lags than measures in other issue areas. Fourth and finally, they find that regulations and

decisions have shorter time lags than directives. While this study was an important first step in analysing the determinants of legislative efficiency, it does not directly test for the effects of treaty change. While the treaty changes relating to the introduction of the Single European Act and Maastricht are discussed, a direct test of the total effects of these treaty changes is lacking. This is unfortunate as both treaty changes occurred within the time frame analysed and are likely to have affected legislative efficiency.

In a second study building upon this empirical effort, König (2007) introduces a divergence of preferences measure into the analysis and finds that the larger the distance between the Member States' positions, the longer the EU decision-making process takes. This study also reinforces previous findings that the process of EU legislative integration is significantly slowing down, even though increasing the use of qualified majority voting facilitates the decision-making process. He also finds that increased parliamentary participation through co-decision modestly increases the duration of the legislative process.

Golub (2007) and Golub and Steunenberg (2007) criticise both of the aforementioned studies, arguing that there are serious methodological problems with the event-history analyses they present, rendering many of their conclusions unreliable. They argue that the assumption made by these studies that the effects of the chosen covariates are constant over time is unrealistic in the context of EU decision-making. When one utilises the correct empirical modelling strategy (time-varying covariates), one can conclude that for the most important types of legislation, qualified majority voting (QMV) and EU enlargement have increased decision-making speed, whereas the empowerment of the European Parliament and extreme preference heterogeneity amongst decision-makers have decreased it. Most importantly, they also show that these effects are not constant over time. While some of the findings that emerge from this study appear similar to those in the previously discussed literature, their claim is that one must take significant care when designing an empirical strategy to test such effects.

In a recent contribution that builds upon the studies just discussed, Klüver and Sagarzazu (2013) consider the role of actor preferences and ideological conflict between multiple institutional actors in determining the speed of legislative negotiations. In addition to confirming many of the previous findings relating to the influence of the institutional structure, they find that in situations where the Commission, Council and Parliament are ideologically close to one

another, decision-making speed is significantly reduced and vice versa. This further attention to ideological conflict between institutional actors adds support to the previously mentioned idea that policy preferences are important determinants of decision-making speed.

A second set of literature has considered the expansion in informal modes of decision-making that have developed as a result of treaty changes (Farrell and Héritier, 2007, 2003). The expanded use of early agreements that existed before 1999, but were formally introduced when the treaty of Amsterdam came into force, has been flagged as an important institutional innovation affecting the decision-making process (Reh et al., 2011). Early agreements, defined as proposals decided upon through informal agreement at either the first or early second reading stage of negotiations (Rasmussen and Reh, 2013), allow the Commission, Council, and Parliament to avoid many of the more formal procedural requirements by conducting informal trialogue negotiations to reach a decision. Toshkov and Rasmussen (2012) find that while early agreements have significantly reduced the overall duration of the legislative process, the first reading stage generally takes longer for early agreement proposals than for proposals decided upon without early agreement. They also note a trend of increasing duration of early agreement proposals between 1999 and 2009. As a result, they conclude that early agreements reached post-Lisbon look different from those concluded when the procedure was first introduced with the Amsterdam treaty.

While all of the studies discussed above provide important insights into how decision-making rules affect decision-making efficiency, they have some notable limitations. These studies all tend to focus on the formal and informal rules and procedures of decision-making, and usually attempt to either identify their causes or their effects. Proper identification of causal effects may pose a serious challenge in this area, but it has received limited attention in previous studies. In particular, these studies tend to be silent about endogeneity: The possibility that the use of decision-making rules depend on the expected duration of a proposal's legislative processing.

Such a relationship may arise in two ways. First, it may be brought about by the treaties governing the decision-making procedures. It is likely that the prescription of a given legislative procedure varies according to how hard it is to reach an agreement in a given policy area. The rationale for introducing majority voting, for example, may be stronger where agreement is harder to reach. On the other hand, those who stand to be out-voted have incentives to avoid

such procedures being applied. The fact that different rules are used in different policy areas suggests that the rules do vary by some characteristics of the policy areas. Furthermore, the fact that member states are negotiating over which rules to apply, makes it likely that the difficulty of reaching decisions is one such characteristic.

Second, a relationship may be brought about by the application of the treaties to specific legislative proposals. Evidence of mechanism can be seen in the case history of the European Court of Justice, where it is relatively common to see disputes over the relevant decision-making procedure, implying the possibility of choosing a procedural rule, rather than it being pre-determined in the treaties.[1] These choices and the conflict that arise from them occur, because in many situations, multiple treaty bases are relevant to a given legislative proposal. This is problematic, because each treaty base is associated with different legislative procedures and voting rules, and when there is a clash between these voting rules, there is the potential for actors to choose the decision rules that best suit them. The general implication is that analyses focusing on the effects of procedures alone may be undermined by reverse causation where legislative procedures are endogenous.

The broader conclusions that one can draw from this discussion of the current literature is that careful consideration of the empirical strategy one uses to capture changes to the decision-making process over time is required, and one should account for issues of endogeneity that arise from the indeterminacy of decision-making procedure. Furthermore, additional attention should be given to estimating the effects of the formal and informal reforms introduced by each treaty revision in a holistic fashion.

The treaties and their expected effects

In this study, we consider the effects that the institutional changes embodied in the treaties of Amsterdam, Nice and Lisbon have had on the efficiency of the legislative process. There is significant variation in the institutional changes that each of these treaty revisions entails, so

[1] Another examples the finding that the likelihood of early agreement depends both on policy area (i.e committee) and the "absolute distance between national political parties of the rapporteur and the minister presiding over the responsible Council at the time of political agreement" Reh et al. (2011). These are features that are also likely to influence the speed of decision-making, and so questions about endogeneity emerge.

we will discuss each in turn.

Before the introduction of the Amsterdam treaty, the Maastricht treaty was in force. The Maastricht treaty introduced the co-decision procedure that made the European Parliament a co-legislator, and significantly extended the scope of the cooperation and assent procedures in EU decision-making. Under co-decision, the Parliament is thought to have a conditional veto right, which implies that when it is in agreement with the Commission, it is very difficult for the Council to amend proposals (Crombez, 1996, 2000). The Maastricht treaty extended qualified majority voting within the Council to cover most decisions under the co-decision procedure and all decisions under the cooperation procedure. The treaty furthermore established EU competencies in six new policy areas: trans-European networks; industrial policy; consumer protection; education and vocational training; youth; and culture. The Maastricht treaty applies to proposals decided upon in the first period in our dataset and went into force on November 1, 1993.

Negotiations for the treaty of Amsterdam begun in 1995 so as to amend the Maastricht treaty in order to prepare the EU for future enlargements and increase the democratic legitimacy of the decision-making process (Garrett, 1992; Tsebelis and Kreppel, 1998; Lodge, 1994; Williams, 1990). It extended EU competencies in the areas of civil protection and common foreign and security policy. Importantly, it significantly increased the use of co-decision in legislative decision-making, and significantly strengthened the role of the European Parliament in the negotiation process. Co-decision was extended to most of the existing EU competencies. Another salient institutional innovation introduced by the treaty of Amsterdam was the formalisation of the possibility of concluding proposals through early agreement (Farrell and Héritier, 2004; Reh et al., 2011; Rasmussen and Reh, 2013). While significant reforms were included in the Amsterdam treaty, many considered it a failure, as member states failed to fully reform the Commission (in terms of adjusting its composition), and failed to agree on a re-weighting of member states' votes in the Council so as to more accurately reflect member state populations and thus address questions about the democratic legitimacy of decisions emerging from the Council. Despite these alleged shortcomings, the Amsterdam treaty went into force on May 1, 1999.

In response to the perceived failures of the treaty of Amsterdam, negotiations started almost

immediately to further reform the EU. The resulting Nice treaty attempted to address concerns about vote weights in the Council by establishing a double-majority requirement under qualified majority voting such that a majority of both member states and votes cast were required to approve legislation. 62% of the total EU population also had to be represented by any decision, and member states could request verification that this requirement was met before a decision was approved. The Nice treaty also increased the total number of seats in the Parliament once enlargement took place, overturning the cap established in the treaty of Amsterdam. Further streamlining of the decision-making procedures was also included in the treaty in order to prepare the EU for the imminent eastern enlargement. In spite of these reforms, the Nice treaty was also considered a failure, as it did not go far enough in terms of the scope of the reforms it introduced to voting procedures.[2] After the conclusion of negotiations in February 2001, the Nice treaty went into force on February 1, 2003.

The Lisbon treaty finally got around to addressing many of the concerns raised about the previous two treaties. It succeeded in this aim by significantly extending the use of co-decision and qualified majority voting. Co-decision (since Lisbon referred to as the ordinary legislative procedure) was extended to most remaining policy areas and thus significantly empowered the European Parliament. Similarly, qualified majority voting plays a much more important role and now applies to nearly all policy areas including asylum, transport, energy and the EU budget among others, but excluding foreign policy and tax issues. The Lisbon treaty furthermore established a President of the European Council and a High Representative of the Union for Foreign Affairs and Security Policy. The three-pillar system established in the Maastricht treaty was also abolished and the previously separate pillars were consolidated so that the EU now represents a single legal entity. Most observers considered the Lisbon treaty to be a success in terms of reforming the legislative decision-making process, as it concluded the reforms that were initialised when negotiations for Amsterdam were opened. The Lisbon treaty went into force on December 1, 2009. However, its arrangements for decicion-making in the Council will

[2]The ratification process was also considered to be problematic, as the Irish rejected the treaty when it was initially put to a referendum. Once reassurances were given to the Irish that the treaty would not affect Irish neutrality, the treaty was passed in a second referendum, but the drawn-out ratification process raised questions about whether the treaty would have been accepted in other countries were it to be put to a referendum. This undermined the democratic legitimacy of the treaty itself in the eyes of many.

only replace those of the Nice treaty after October 31, 2014, and until March 31, 2017, member states can still request that the old rules be used for specific votes.[3]

It is clear from the above discussion that each successive treaty revision significantly affected the rules that structure the decision-making process, and thus each had the potential to affect the efficiency of legislative negotiations. It is our aim to assess exactly how successful each treaty revision was in increasing decision-making efficiency. As stated above, the Amsterdam treaty was introduced to streamline the legislative process in order to prepare for the forthcoming membership enlargement. Both Nice and Lisbon were follow-up treaties that sought to address issues that the relevant preceding treaty had failed to address. One of the key goals of each treaty reform was increasing legislative efficiency in the face of an expanding membership. As a result, we hypothesise that each successive treaty change has led to an increase in decision-making efficiency: *Each treaty has decreased the time it takes to reach a legislative decision.*

While each treaty change may generally be expected to have this effect, significant differences are also to be expected in how effective the treaties were due to differences in the reforms they entailed. Both Amsterdam and Lisbon contained significant increases in the use of the co-decision procedure and qualified majority voting, while the Nice treaty was less ambitious in its reforms, focusing mostly on a new double majority requirement in the Council and reforms to the Parliament. In fact, the Nice treaty may overall have made it harder to reach decisions in the Council. We may thus expect the increases in legislative efficiency due to treaty change to be larger for the Amsterdam and Lisbon treaties than for the Nice treaty.

Data, methods and the identification strategy

We exploit the time dimension of our data to estimate the overall effects of the treaties and draw causal inferences. More specifically, we study the impact of each treaty taking an interrupted time-series (ITS) approach (see e.g. Morgan and Winship, 2007). The general idea behind this approach is to model the dynamics in the dependent variable before our "treatment" occurs, using this model to make predictions, and compare these "counterfactuals" to the observed values after "treatment". The difference between these values can be given a causal interpre-

[3]For the moment, is is thus only possible to assess the effect of the arrangements that have been in place since 2009.

tation given the assumption that no other relevant changes took place at the same time as the "treatment". In other words, focusing on the exact time that a treaty entered into force greatly reduces the number of potentially confounding variables.

Our identification strategy has several advantages. First, it avoids the aforementioned endogeneity challenge facing existing analyses that focus on decision-making procedures. Looking at new treaties entering into force and using this as a treatment variable, we avoid this issue insofar as it applies to decision-making procedures and voting rules. From our perspective, changes in the mix of decision-making procedures and voting rules being used is the mechanism through which treaties are the most likely to have an impact. However, these rules and procedures may be endogeneous to decision-making speed, and the treaties may also introduce less formalised, but still important institutional changes. In this environment, our approach of abstracting up to the level of treaties also provides a second advantage: It enables us to capture the complete effect of each treaty. Third, by exploiting the time-dimension of the data, we obtain plausible counterfactuals, which enable us to draw fairly strong causal inferences. Lastly, our approach allows for a high level of transparency, as the effects we find are well-suited for graphical illustration. It also allows us to be explicit about the assumptions we inevitably must make in drawing causal inferences, so that the validity of our inferences can be assessed.

We use data from the European Union Policy-Making dataset (Häge, 2011) to test the expectations outlined in the previous section. This dataset contains information scraped from the PreLex database on inter-institutional decision-making procedures. The proposals contained in the dataset were introduced by the Commission between 1975 and 2012 (as updated in the latest iteration of the study at the beginning of 2013). We utilise a subset of these data relating to legislation decided upon between 1995-2012, as this time period corresponds to the time when the treaties of interest came into force. Within this dataset, we have information on the dates when the Commission makes a proposal and the dates when a final decision was reached (i.e. the date of formal adoption by the Council, if such adoption did indeed take place).

We construct the dependent variable as the number of days between a proposal and decision. This variable is heavily skewed, however, as some rare decisions take very long time, producing a right-tailed distribution. We therefore transform this variable, calculating its natural logarithm, which yields an approximate normal distribution. To make the results readily interpretable, we

report our results in separate tables, where the effects are transformed back to the original scale, i.e. number of days (and appropriate p-values are included for our interaction effects).

How quickly a proposal they can be decided upon depends on the type of legislative instrument under negotiation. The most important legislative instruments in the EU are decisions, regulations and directives, and there are significant differences in the scope and application of these legislative instruments (Craig and De Búrca, 2011). Decisions are EU laws that are relevant only to those to whom they are addressed, regulations are EU laws that enter into effect immediately and apply to all member states, and finally directives apply to all and must be transposed into national law by member states and usually require that a member state achieves a particular policy goal. We analyse legislative instruments separately, as they differ notably in the average length of negotiations. Figure 1 shows our logged dependent variable over time for each of the legal instruments in question. The figure uses a quarterly time-scale to facilitate interpretation, and the treaties in question are marked as dashed vertical lines.

A key point to notice from figure 1 is how long it takes to reach a decision on directives. During the period from 1995 to 2012, it took 483 days on average to conclude negotiations. This is problematic for our analysis, as many directives will be affected by a treaty change while being processed (after the Commission has introduced the proposal, but before the final decision has been made), and these cases are likely to blur any effect of the treaties.[4] This issue is less problematic for decisions and regulations, as these are decided upon in a more timely fashion. Between 1995 and 2012, the average time required to reach a decision on regulations was 125 days, while the equivalent for decisions is 183 days. Regulations and decisions are thus normally processed in 4-6 months. Another issue is that directives are much more rare than the two other types of instruments, providing fewer cases in the first place. Together, these two features make directives unsuited for our analytical approach, which relies on fairly rapidly occurring effects. In our analyses, we therefore focus on decisions and regulations. Looking at figure 1, it should be noted, however, that while the Amsterdam and Nice treaties may have reduced decision-making time for directives, such effects appear to have been temporary.

[4]A possible solution would be to exclude from the analysis all cases for a certain period after a treaty change, e.g. the average duration for all cases. However, when reaching a decision takes as long as for directives, this would leave few cases for analysis, occurring long after the treaty change, thus undermining our research design. We discuss our treatment of such cases further below.

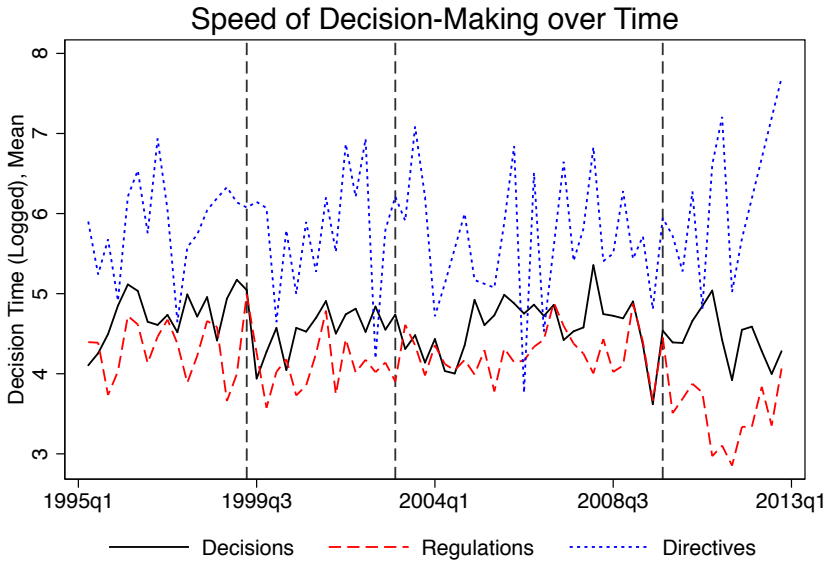

Figure 1: Speed of EU Decision-Making by Legal Instrument over Time.
Note: The dashed vertical lines represent the treaty changes investigated in this paper: Those agreed upon in Amsterdam, Nice and Lisbon, respectively.

For our analysis, we create a monthly time-scale, classifying decisions by their end-date (i.e. date of formal adoption by the Council). To obtain time-series, we aggregate our variables over this time scale (but note that our main models are not aggregated, as will be explained below). We analyse the impact of each treaty separately, selecting a window of observations around the time when the treaty entered into force. A key question is how wide to make these windows; they should be wide enough to yield enough observations and allow each treaty to take effect. We thus select windows covering 2 years before and after a treaty entered into force (a total or 48 months). It should be noted that the exact size of this window makes no substantial difference for our reported results.

For each treaty, we further create a binary "treatment indicator" (T) capturing whether the treaty was in force when a decision was made. As mentioned, some decisions will be in an intermediate category, being initiated before a treaty entered into force, but ending after. We still code these as treated, which is a conservative option, as a treaty's chance to influence these cases will be limited.[5] This choice means that the effect of a treaty may appear gradually, but

[5]The option of creating a separate indicator for these cases involves a risk of inflating our estimated effects, as the remaining series of treated cases would have an artificially short legislative duration: Cases starting after a treaty entered into force and finishing before a given date will by construction have a shorter duration than the period between these dates; the closer the given date is to the treaty change,

our focus on instruments with relatively short processing times limits the relevance of this issue.

For each analysis, we centre the time variable (t) on the time of entry into force (i.e. $t = 0$ when T goes from 0 to 1). In the models, we include t as a predictor to avoid mistaking a linear trend for the effect of a treaty. To capture gradually appearing effects (and other changes in the trends after treatment), we include an interaction between T and t. Our analyses are thus based on the following general time-series model:

$$Y_t = \beta_0 + \beta_1 T_t + \beta_2 t + \beta_3 T_t t + \varepsilon_t, \tag{1}$$

where β_0 is a constant, β_1, β_2, and β_3, are coefficients, ε_t is an error term. With this specification, the centring of t has the advantage that the coefficient on T (β_1) can be interpreted as the immediate effect of treatment (at $t = 0$).

A key question that arises when engaged in time-series analysis is how to specify the remaining dynamic component in the dependent variable. Key criteria for choosing a specification are time-series diagnostics (for a discussion of time-series modelling, see e.g. Kennedy, 2008). Our main concern, however, is to specify models that appropriately captures the causal effect of our treatment indicators. If the trends are not linear, for example, this may need to be taken into account. The challenge is that a non-linear specification may also pick up random fluctuations and make the analyses less reliable. Thus, rather than including non-linear trends, we choose to analyse fairly narrow windows, and assume that the underlying trends are approximately linear within these windows (before and after treatment).[6] We do, however, control for seasonal patterns in the data, by including a set of dummy variables identifying each month.

We test our time-series models for first-order auto-regressive auto-correlation using the Breusch-Godfrey test (referred to as B-Godfrey), while we test for autoregressive conditional heteroskedasticity using Engle's Lagrange multiplier test (referred to as ARCH-LM). We furthermore use the Breusch-Pagan test to check for heteroskedasticity conditional on fitted values (referred to as B-Pagan). For all these tests, our tables report the p-values of violation of the assumption of spherical disturbances (i.e. independent and identically distributed errors, "i.i.d.").

the shorter the duration must be for the selected cases. In other words, the cases in the beginning of this series would be selected by their short time processing time, biasing our estimates.

[6] Our plots suggest that is a valid assumption.

We also test for non-stationary errors, using the Dickey-Fuller test (referred to as D-Fuller). For this test, we report the p-values of non-violation (stationary residuals).

The time-series diagnostics are generally unproblematic, especially for the models where there does appear to be effects (re-specifying the other models to improve their diagnostics would not alter our main findings). This means our models involve little dynamics. In fact, it is feasible to implement our models at the level of decisions (rather than aggregating by month), and this is preferable as it retains more information, and allows for controls to be included at the most appropriate (i.e. the lowest) level of analysis. We therefore implement our final analyses at this level (without otherwise changing the models). However, for each reported model, we still report time-series diagnostics based on equivalent time-series models of aggregate data. We also include plots based on such aggregate time-series models to illustrate our results.

In certain model specifications we also include dummies for each type of legislative procedure. This is done in order to assess to what degree the effects of treaty change can be explained by changes in the use of procedures alone. The procedures we include in the analysis are consultation, cooperation, co-decision, agreement, and "no procedure", and the data used to identify each procedure is found in the Häge dataset. As data on the relevant voting rule in the Council is not available for each of the proposals in the dataset, it is not possible to include voting rules in this test. This is unfortunate, as some changes in the voting threshold applicable in each policy area were included in each treaty and it is reasonable to expect that such changes affect decision-making efficiency. Nevertheless, in accounting for the full effect of treaty changes, we capture the influence of voting reforms alongside the other legislative reforms. Our analysis is thus also informative in this respect.

To further ensure that our estimates are not driven by changes in the substantive content of legislation enacted before and after a treaty change, we control for policy area (using a set of dummy variables to capture fixed effects). Policy area is captured using the field of activity data in the Häge dataset, which has been scraped from the PreLex database. In some cases, more than one field of activity is found for a proposal. We assume that the first field of activity mentioned is the most salient one for the proposal under consideration, and thus ignore situations where proposals have more than one field of activity. By controlling for policy area fixed effects, we estimate averages of the effects of treaty changes within each policy area.

Analysis

The Amsterdam treaty

Tables 1 and 2 report the results for the effects of the Amsterdam treaty on legislative efficiency. The first column of table 1 reports a model of the time required to make a decision, and the estimates imply a considerable, immediate effect of the treaty. This effect is illustrated in figure 2, which confirms the picture of an immediate effect, retaining its impact over time. The plot includes a counterfactual line, extending the linear pre-treaty trend to the post-treaty period.

If we transform the results back to the original scale, as in table 2, the average number of days required to make a decision went from 154 to 92 exactly when the treaty entered into force. This is a reduction of approximately 40%, and lends strong support to our expectation that Amsterdam increased legislative efficiency for legislative decisions. To assess the possibility that there is a delayed (or temporary) effect, the second and third columns of table 2 report estimated effects two years after the treaty change. Such estimates require extrapolation of the pre-treatment trend, to obtain relevant counterfactuals. To show be transparent in this regard, and highlight any uncertainty, the estimates are based on a constant and a linear trend, respectively. Assuming a linear trend, the effect after two years has the same size as the immediate effect, while a constant trend would give a somewhat smaller estimate.

A key mechanism through which we would expect the treaty to have effect is a change in the use of various decision-making procedures. In model 2 of table 1, we therefore include indicators for the main procedures used (as they are captured in our data): consultation, co-operation, co-decision, agreement, and "no procedure". This does indeed reduce the estimated effect of the treaty, rendering it statistically insignificant, suggesting that procedural change is indeed a key explanation of the effect. However, it is also worth noting that part of the immediate effect appears to remain, even if it is not significant. This suggests that the procedures do not account for the full effect of the Amsterdam treaty change.

If we turn to regulations, in model 3 of table 1, the Amsterdam treaty does not appear to have had an effect on legislative efficiency. The positive effect estimate is largely due to intermediate cases – proposals starting before and ending after treaty change. Excluding these, this pattern would disappear, but no lasting reduction in processing time would emerge. This

suggests that there are qualitative differences between decisions and regulations interacting with the potential efficiency-improving effects of the Amsterdam treaty.

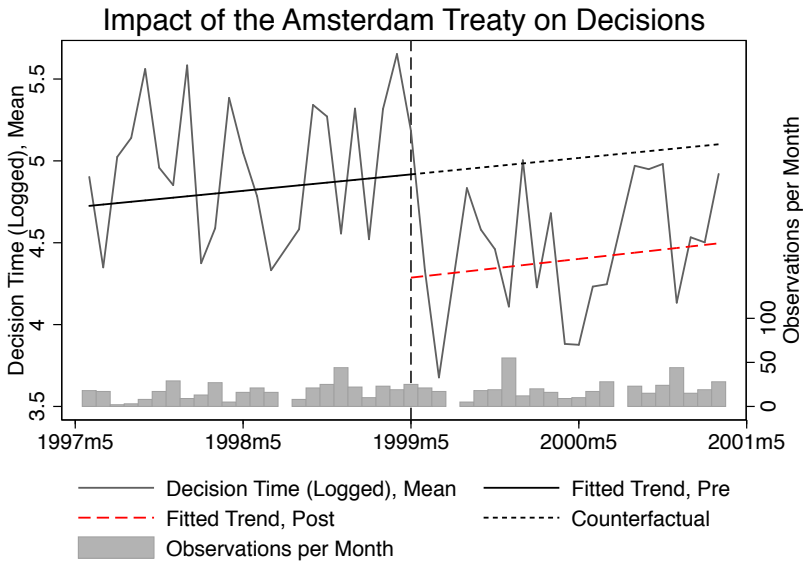

Figure 2: Impact of the Amsterdam Treaty on Decisions.
Note: The plotted decision time represents an average for the decisions made each month; the fitted lines are weighted by the number of decisions contributing to each monthly average. The weighting yields a model more closely reflecting those reported in the tables, and accounts for any apparent discrepancies between the plotted average time and the fitted lines. The lines are predictions for the month closest to the overall average for the period included in the analysis.

The Nice treaty

Tables 3 and 4 report the results for the Nice treaty. Model 1 in table 3 is an analysis of decisions. The estimated immediate effect involves a reduction in processing time of 24%, with a p-value of .086, as shown in table 4. It would thus be significant at the 10% level in a two-tailed test and the 5% level in a one-tailed test. For the estimated effect two years after the treaty change, assumptions about the counterfactual trend become crucial. Decision-making time was already decreasing before the treaty entered into force. If we assume this downward trend would continue linearly, the estimates imply no effect after two years. However, if rather we assume the speed of decision-making would remain stable at the level at which it was when the treaty entered into force, there is a notable and statistically significant effect. The treaty may thus have had an effect, but we cannot be confident in such a conclusion. In other words,

Table 1: Impact of the Amsterdam Treaty on Decision-Making Time

	Model 1	Model 2	Model 3
Instrument	Decisions	Decisions	Regulations
Treaty	-0.517^{**}	-0.271	0.182
	(0.173)	(0.145)	(0.180)
Time	0.008	-0.003	-0.008
	(0.010)	(0.008)	(0.009)
Treaty x Time	0.001	0.018	0.000
	(0.012)	(0.010)	(0.012)
Consultation		-1.078^{***}	
		(0.155)	
Co-Operation		-0.438	
		(0.428)	
Co-Decision		-0.102	
		(0.935)	
Agreement		-1.993^{***}	
		(0.156)	
No Procedure		-2.319^{***}	
		(0.150)	
Constant	5.551^{***}	6.954^{***}	5.659^{***}
	(0.294)	(0.270)	(1.104)
FE, Month	Yes	Yes	Yes
FE, Policy Area	Yes	Yes	Yes
Observations	813	813	745
Window (mo.)	48	48	48
R^2	0.228	0.481	0.455
R^2, adj.	0.183	0.447	0.421
D-Fuller (p)	0.000	0.000	0.000
B-Godfrey (p)	0.085	0.516	0.984
ARCH-LM (p)	0.079	0.523	0.521
B-Pagan (p)	0.933	0.466	0.841

Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The reported models are at the level of individual proposals/decisions, while the time-series diagnostics (last four rows) are based on data that are aggregated by the month in which decisions are reached. The diagnostic time-series models do not include policy area fixed effects, as this would not be feasible. The reported diagnostics are p-values. The null-hypothesis of the D-Fuller test is violation of the model assumption (stationarity), while the null-hypotheses of the remaining tests are non-violation (aspects of spherical errors).

Table 2: Effects of the Amsterdam Treaty: Days between Initiation and Adoption

	Decisions			Regulations		
Time	0	2 years	2 years	0	2 years	2 years
Counterf. Trend	–	Constant	Linear	–	Constant	Linear
Wihtout Treaty	153.7	153.7	183.7	40.7	40.7	33.8
With Treaty	91.7	112.5	112.5	48.8	40.8	40.8
Difference	−62.0	−41.3	−71.2	8.1	0.1	7.0
Prop. Diff.	−0.40	−0.27	−0.39	0.20	0.00	0.21
p-value	0.003	0.054	0.162	0.312	0.989	0.579

Note: For the 2-year effect estimates, the p-values are based on two-tailed t-tests using analytical standard errors, calculated as: $\sigma = \sqrt{var(\hat{\beta}_1) + t^2 var(\hat{\beta}_3) + 2t \times cov(\hat{\beta}_1, \hat{\beta}_3)}$, where the coefficients are labeled as in equation 1, and $t = 23$ (as $t = 0$ is the first month subject to our treatment). For a discussion of interaction effects and their variances, see Brambor et al. (2006). The effect estimates with a constant counterfactual are based on models with separate trends before and after treatment, to make the constant at $t = 0$ the baseline for the other coefficients. All predictions refer to the month and policy area closest to the overall average for the period included in the analysis.

the hypothesis of increased efficiency does not receive clear support in this case, but it can neither be clearly rejected.

Model 3 in table 3, which analyses regulations, also implies negative immediate and long-term effects, but, as shown in table 4, neither estimate is statistically significant. Excluding intermediate cases (proposals starting before and ending after treaty change) would remove a peak occurring after treaty change, and thus yield a clearer immediate effect, but no lasting effect would emerge.

The Lisbon treaty

Tables 5 and 6 report the results for the Lisbon treaty. Model 1 in table 5 is an analysis of decisions. The results lend no support to hypothesis that efficiency increased.[7] In contrast, Model 2 of table 5, which analyses regulations, finds a clear effect. There is both a significant immediate reduction in processing time, and a further reduction over time. This effect is

[7]While this analysis fails to find an immediate effect, it appears to find a delayed effect in the opposite direction of what we would expect. This is the result of a downward trend before the treaty entered into force. If we assume this trend would continue in the absence of the treaty, the treaty appears to have a strong and significant effect, increasing the time it takes to reach a decision. If, on the other hand, we assume the speed of decision-making would remain constant at the level it was at when the treaty entered into force, this effect would be weaker and insignificant. If we exclude intermediate cases (proposals starting before and ending after treaty change), the "effect" would largely disappear, as the speed of decision-making would remain fairly constant.

Table 3: Impact of the Nice Treaty on Decision-Making Time

	Model 1	Model 2	Model 3
Instrument	Decisions	Decisions	Regulations
Treaty	−0.269	−0.183	−0.228
	(0.157)	(0.135)	(0.193)
Time	−0.012	−0.007	0.007
	(0.008)	(0.007)	(0.010)
Treaty x Time	0.010	−0.001	−0.004
	(0.011)	(0.009)	(0.013)
Consultation		−0.582***	
		(0.166)	
Co-Decision		−1.794**	
		(0.669)	
Agreement		−1.245***	
		(0.166)	
No Procedure		−1.901***	
		(0.154)	
Constant	3.592***	5.328***	5.030***
	(0.409)	(0.382)	(0.522)
FE, Month	Yes	Yes	Yes
FE, Policy Area	Yes	Yes	Yes
Observations	925	925	591
Window (mo.)	48	48	48
R^2	0.212	0.428	0.448
R^2, adj.	0.164	0.390	0.402
D-Fuller (p)	0.000	0.000	0.000
B-Godfrey (p)	0.608	0.825	0.036
ARCH-LM (p)	0.962	0.643	0.972
B-Pagan (p)	0.000	0.256	0.174

Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The reported models are at the level of individual proposals/decisions, while the time-series diagnostics (last four rows) are based on data that are aggregated by the month in which decisions are reached. The diagnostic time-series models do not include policy area fixed effects, as this would not be feasible. The reported diagnostics are p-values. The null-hypothesis of the D-Fuller test is violation of the model assumption (stationarity), while the null-hypotheses of the remaining tests are non-violation (aspects of spherical errors).

Table 4: Effects of the Nice Treaty: Days between Initiation and Adoption

	Decisions			Regulations		
Time	0	2 years	2 years	0	2 years	2 years
Counterf. Trend	–	Constant	Linear	–	Constant	Linear
Without Treaty	92.0	92.0	69.2	109.9	109.9	129.8
With Treaty	70.3	66.0	66.0	87.5	94.8	94.8
Difference	−21.7	−26.0	−3.2	−22.4	−15.1	−35.0
Prop. Diff.	−0.24	−0.28	−0.05	−0.20	−0.14	−0.27
p-value	0.086	0.020	0.869	0.239	0.403	0.387

Note: For the 2-year effect estimates, the p-values are based on two-tailed t-tests using analytical standard errors, calculated as: $\sigma = \sqrt{var(\hat{\beta}_1) + t^2 var(\hat{\beta}_3) + 2t \times cov(\hat{\beta}_1, \hat{\beta}_3)}$, where the coefficients are labeled as in equation 1, and $t = 23$ (as $t = 0$ is the first month subject to our treatment). For a discussion of interaction effects and their variances, see Brambor et al. (2006). The effect estimates with a constant counterfactual are based on models with separate trends before and after treatment, to make the constant at $t = 0$ the baseline for the other coefficients. All predictions refer to the month and policy area closest to the overall average for the period included in the analysis.

illustrated in figure 3. Again, we include counterfactual predictions based on the pre-treaty model. As we show in table 6, the model implies that regulations took 89 days to process before the treaty, and 47 days after – an immediate reduction of 47%. After two years, the processing-time was down to 21 days, which implies a reduction of 83% if we assume a linear counterfactual trend, and 75% if we assume a constant counterfactual. If we include controls for the procedures used, as in model 3 of table 5, the immediate effect disappears, while a part of the delayed effect remains. This is consistent with the interpretation that changes in the use of these procedures accounts for most of the effect. In sum, the hypothesis that the 2009 implementation of the Lisbon treaty increased legislative efficiency receives strong support with regard to regulations, but none with regard to decisions.

Discussion and conclusions

Our results show that the Amsterdam, Nice and Lisbon treaty reforms have had some effect on the legislative efficiency of the EU, but that these effects vary depending upon the treaties and the legislative instruments in question. Furthermore, while it is true that changes to legislative procedure seem to be important determinants of decision-making efficiency, this may not be the whole story. Each treaty change included more than just reforms to legislative procedures, and

Table 5: Impact of the Lisbon Treaty on Decision-Making Time

	Model 1	Model 2	Model 3
Instrument	Decisions	Regulations	Regulations
Treaty	0.107	-0.633^{*}	0.120
	(0.212)	(0.251)	(0.210)
Time	-0.024^{*}	0.016	0.001
	(0.011)	(0.012)	(0.009)
Treaty x Time	0.033^{*}	-0.050^{**}	-0.031^{*}
	(0.015)	(0.017)	(0.014)
Consultation			1.814^{***}
			(0.198)
Co-Decision			2.718^{***}
			(0.590)
Agreement			0.850^{**}
			(0.275)
No Procedure			-0.036
			(0.190)
Constant	4.097^{***}	4.435^{***}	3.530^{***}
	(0.271)	(0.289)	(0.273)
FE, Month	Yes	Yes	Yes
FE, Policy Area	Yes	Yes	Yes
Observations	658	392	392
Window (mo.)	48	48	48
R^2	0.158	0.234	0.580
R^2, Adj.	0.134	0.201	0.557
D-Fuller (p)	0.000	0.000	0.000
B-Godfrey (p)	0.031	0.748	0.978
ARCH-LM (p)	0.956	0.446	0.239
B-Pagan (p)	0.724	0.359	0.282

Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The reported models are at the level of individual proposals/decisions, while the time-series diagnostics (last four rows) are based on data that are aggregated by the month in which decisions are reached. The diagnostic time-series models do not include policy area fixed effects, as this would not be feasible. The reported diagnostics are p-values. The null-hypothesis of the D-Fuller test is violation of the model assumption (stationarity), while the null-hypotheses of the remaining tests are non-violation (aspects of spherical errors).

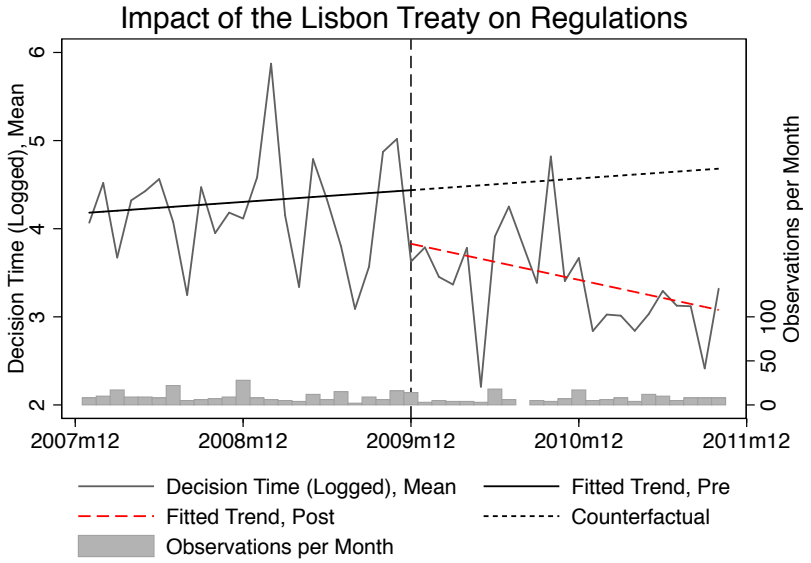

Figure 3: Impact of the Lisbon Treaty on Regulations.
Note: The plotted decision time represents an average for the decisions made each month; the fitted lines are weighted by the number of decisions contributing to each monthly average. The weighting yields a model more closely reflecting those reported in the tables, and accounts for any apparent discrepancies between the plotted average time and the fitted lines. The lines are predictions for the month closest to the overall average for the period included in the analysis.

Table 6: Effects of the Lisbon Treaty: Days between Initiation and Adoption

	Decisions			Regulations		
Time	0	2 years	2 years	0	2 years	2 years
Counterf. Trend	–	Constant	Linear	–	Constant	Linear
Without Treaty	73.3	73.3	42.5	88.5	88.5	127.0
With Treaty	81.5	102.0	102.0	47.0	21.2	21.2
Difference	8.2	28.7	59.6	−41.5	−67.3	−105.8
Prop. Diff.	0.11	0.39	1.40	−0.47	−0.76	−0.83
p-value	0.615	0.105	0.026	0.012	0.000	0.000

Note: For the 2-year effect estimates, the p-values are based on two-tailed t-tests using analytical standard errors, calculated as: $\sigma = \sqrt{var(\hat{\beta}_1) + t^2 var(\hat{\beta}_3) + 2t \times cov(\hat{\beta}_1, \hat{\beta}_3)}$, where the coefficients are labeled as in equation 1, and $t = 23$ (as $t = 0$ is the first month subject to our treatment). For a discussion of interaction effects and their variances, see Brambor et al. (2006). The effect estimates with a constant counterfactual are based on models with separate trends before and after treatment, to make the constant at $t = 0$ the baseline for the other coefficients. All predictions refer to the month and policy area closest to the overall average for the period included in the analysis.

any attempt to assess their effect should take this into account.

We find that the treaty of Amsterdam has had a large and persistent effect on the time needed to reach decisions and that changes to the legislative procedure seems to be the most important explanation of this effect. That being said, legislative procedure alone does not account for the full size of the effect, suggesting that the other reforms included in Amsterdam also had an impact on legislative efficiency. In contrast to its effect on decisions, Amsterdam was not successful in speeding up the legislative process for regulations, however. It is hard to explain exactly why decisions were affected, while regulations were not, but future research should focus on this question.

Interestingly, all of our estimated effects for the Nice treaty are negative, implying reductions in decision-making time, as hypothesised. However, these estimates are generally not statistically significant. While the Nice treaty may have had some effect on both decisions and regulations, its impact was not sufficiently strong and clear for us to confidently conclude that there was one. The conclusion that the Nice treaty's impact was limited is in line with the observation that it was not very successful in reforming the legislative process. It seems that commentators who argued that the Nice treaty failed to achieve its goals may have been right.

Finally, the Lisbon treaty does seem to have had a considerable effect on decision-making efficiency for regulations, but no effect for decisions. Again, we can attribute most of this effect to changes in procedure, as the immediate effect is much reduced and no longer significant when the procedure is controlled for. That being said, procedure alone does not account for the entire effect, suggesting that other changes embodied in the treaty were also important.

It is clear from figures 2 and 3 that there are significant differences between how persistent the effects of treaty change are on different legislative instruments. This suggests that Golub and Steunenberg (2007) and Reh et al. (2011) were correct in their claim that the decision-making process has evolved over time, and adds further justification to our modelling approach that directly addresses time-series aspects of the data. Future research should further investigate how the decision-making process has changed over time rather than providing a static snapshot of the decision-making process for a given period. As more and more data becomes available in this field, such studies are becoming more feasible.

Overall, we can conclude that in some cases, treaties have been very successful in increasing

legislative efficiency, while in others they appear less successful. However, it is worth noting that over the whole period under investigation, decision-making time has generally decreased or remained stable, while EU membership has increased significantly. The treaties may have been successful in stopping any further deterioration in legislative efficiency, which in itself would be a positive outcome. Furthermore, while changes to legislative procedure have an important impact on legislative efficiency they may not entirely explain the effects observe. Future research should look into what accounts for any remaining improvements in legislative efficiency.

These findings have significant implications for future treaty reforms. If efficiency were to be the only salient issue when designing treaty reforms, then continuing the push towards the use of co-decision, early agreements, and qualified majority voting seems to be an effective way to proceed. It must, however, be acknowledged that the efficiency of the decision-making process is but one of the ways in which we can assess the impact of treaty changes on the decision-making process. Others include looking at the democratic legitimacy of decisions reached, the transparency of the decision-making process, or the effectiveness of policy outcomes agreed upon when such legislation goes into force. These aspects of treaty change are as, if not more, important than considerations about legislative efficiency, and deserve careful attention. It must furthermore be acknowledged that some of the goals that treaty reforms try to achieve may contradict one another and have mutually confounding effects. Stasavage (2004) and Cross (2013) for instance find a tension between increasing legislative efficiency on one hand and legislative transparency on the other. What is clear is that all of these goals are intimately related. As a result, careful consideration of reform priorities is necessary when thinking about how the EU treaties should be reformed in the future.

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