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Legislative Effects of Regulatory Impact Assessment:

A Comparative Event History Analysis of Modifications of Law in France, Italy, Hungary and the UK

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Abstract

Regulatory instability, that is frequent modification of adopted laws, is costly for society. Regulatory Impact Assessments (RIAs) are designed to improve the quality of legislation, however, we know little about RIAs' impact on legal stability. Therefore, this paper analyses whether RIAs influence the incidence and frequency of the modifications of laws. Our analysis is based on a complete dataset of more than 2500 enacted laws in France, Hungary, Italy, and the United Kingdom in 2006-2012. We apply a comparative event history analysis to account for both first and subsequent modifications of legislation. We find across-the-board that RIA contributes to legal stability. However, the strength of this effect varies by country in ways calling for further research. RIA is predicted to have the largest effect when political power changes both in terms of seat shares and party ideology, suggesting that the technical and consultative practices engrained in RIA can, to some degree, tame volatile politics.

1. Introduction

The last decades experienced an increasing attention to regulatory reforms that should improve government efficiency and benefit the overall welfare of society. Regulations, if properly designed, "improve societal wellbeing, improve business competition, and enhance environmental outcomes" (OECD, 2019, p.18). But regulations can have unintended negative consequences. Poorly designed, they increase costs for citizens, business, or the public sector and lead to negative welfare outcomes. In order to reduce such risks, regulatory reforms incorporate cost-benefit analyses to justify a proposed regulation based on assumed positive and negative effects. Ideally, a proposed regulation is only pursued if the benefits justify their costs.

While the US is conducting cost-benefit analyses for regulatory proposals since the late 1970s, the EU institutionalized them in the early 2000s through the 'Better Regulation' framework. These regulatory reforms manifest a more general trend towards regulatory delegation to independent authorities and the mandatory application of evaluation techniques (Cingolani and Fazekas, 2019). In practice, this occurs mainly through Regulatory Impact Assessments (RIA). RIAs provide a (quantifiable) cost-benefit analysis for a proposed primary or secondary legislation. Sunstein (2018) has called this institutionalization of quantifiable regulatory decision-making since the late 1970s a 'cost-benefit revolution'; the increasing victory of technocratic rule over political polarization.

While Sunstein limits his claim of a cost-benefit revolution to secondary legislation, primary laws are also often subject to RIA, especially in Europe (Lianos et al., 2016). After all, the longer a law is in effect in its original form, the greater predictability it confers on society. Legal stability is thus a value sought after and RIAs provide the technical means to achieve it. However, in practice, laws frequently change and many laws are modified multiple times. While the political obstacles surrounding the implementation, diffusion and design of RIA received ample attention, little is known about the effects of RIA on the modifications of enacted primary laws. **Hence, this paper analyses whether RIA influences the incidence and frequency of the modifications of laws, that is the impact of RIA on legislative stability.** In order to achieve our goal we situate RIA and its effects on legal stability within the broader political and economic environment (Maltzman and Shipan, 2008).

Our analysis is based on a complete dataset of laws enacted in France, Hungary, Italy, and the United Kingdom from 2006 to 2012, consisting of over 2500 laws. Our unique, large-scale dataset allows for an in-depth yet comparative analysis of the factors driving modification of enacted laws. The selected countries have all embarked on New Public Management-type reforms to different degrees and implemented RIA in different scope and quality while being part of the broader EU regulatory framework (Lianos et al., 2016). The diversity of countries is beneficial for our research design in as much as it supports broader conclusions likely valid across many legal systems, while it also presents challenges for averaging across apples and oranges. Hence, we will keep each country separate in the analysis and draw cross-country conclusions only when we see strong convergence in results.

To study modification of laws over time, we employ comparative event history analysis (or survival analysis) on a month-law resolution, amounting to about 300,000 observations in the dataset. While this method is well-fitted to serve our research goals and is an advanced quantitative tool, our claim for causality has to remain limited. We interpret our findings largely as associations between variables while the time lag between independent and dependent variables do meet some basic criteria of causal interpretation (i.e. Granger causality) (Aalen et al., 2008, ch 9).

We find that, RIA contributes to legal stability across-the-board. However, the strength of this effect varies by country in ways calling for further research. RIA is predicted to have the largest effect when political power changes both in terms of seat shares and party ideology, suggesting that the technical

and consultative practices engrained in RIA can, to some degree, lock in political choices in spite of volatile political changes.

2. Theory

2.1 Better Regulation and the Regulatory State

Regulatory Impact Assessments are very much linked to a more general argument in favor of evidence-based policy making and political delegation. Politicians are assumed to be unable to provide Pareto optimal welfare for society given that they are mainly driven by individual interests, be it office seeking, power, patronage, or public opinion changes (Parker, 2002; Downs, 1957; Buchanan, 1960; Niskanen, 1971). In such an environment, politics is often fueled by special interests that lead to the redirection of resources from the majority to a predefined minority and the redistribution of these resources at significant costs for the wider society (Lohmann, 2003). As politicians operate under the time constraints of electoral cycles (Majone, 1999), the short-termism of politicians will lead to an oversupply of regulations in the form of new laws and the modification of existing rules. Such oversupply "is encouraged by a lack of adequate national accounting for regulatory costs" (Parker, 2002, p.500).

The regulatory state was presented as a solution to the time inconsistency of politicians and potential regulatory costs on a society's overall welfare. Its main tool is the delegation of policy making to independent administrators and the application of technical impact assessment techniques. While the exact definition of the regulatory state varies, all share the same basic understanding of the underlying structural change: the regulatory state displays a structural shift from government to governance and the replacement of command structures (government) with the principle of contracting-out based on cost-benefit analyses (governance) (Majone, 1994, 1996, 1997; Moran, 2000, 2003; Braithwaite, 2000; Levi-Faur, 2013).

The constant demand of European stakeholders for regulatory reform led to the European Commission's 'Better Regulation' framework. Following the New Public Management logic, the framework tries to achieve regulatory goals in an efficient way at the lowest cost based on evidence-based expertise. It is a general standard for improved rule-making that should be applicable to all stages of the policy cycle (Baldwin, 2005; Weatherill, 2007; Radaelli, 2010). The approach is mainly, but not exclusively, based around ex-ante regulatory impact assessments (RIA), ex post regulatory evaluations, and the expertise of field experts and stakeholders in the policy process (OECD, 2019). It is thus an evaluation of how policy options affect the welfare of a target group or the society as a whole; it determines the risks, benefits and costs of policy options; takes into account how compliance can be achieved; and considers the potential impact on business, the opinion of stakeholders, and the requirements for ex post evaluation (Baldwin, 2010, p. 265). Among those, ex-ante impact assessments are the only Better Regulation tool that was institutionalized by every EU member state. Given their outstanding importance for regulatory reform, we center our analysis around these RIAs.

RIAs can be broadly defined as "a systematic and mandatory appraisal of how proposed primary and/or secondary legislation will affect certain categories of stakeholders, economic sectors, and the environment" (Radaelli and De Francesco, 2010, p.280). They should ensure consistency, accountability, transparency, and openness in the regulatory process and outcome (Kirkpatrick and Parker, 2004; Baldwin, 2010). In order to achieve these goals,

ex ante assessment of costs, benefits and risks should be quantitative whenever possible. Regulatory costs include direct costs (administrative, financial and capital costs) as well as indirect costs (opportunity costs) whether borne by businesses, citizens or government. Ex ante assessments should, where relevant, provide qualitative descriptions of those impacts that are difficult or impossible to quantify, such as equity, fairness, and distributional effects (OECD 2012, p.13)

But not all laws need to go through such ex-ante impact assessments. There can be exceptions for low impact laws when the cost of conducting an impact assessment exceeds the potential benefits of the law itself. Other exceptions are linked to laws that are emergency measures, that implement international law, questions of national security and public order, as well as laws that clarify the operation between different federal levels of government (OECD, 2019, p.76).

2.2 National Variation in RIA use

There is strong cross-country variation in the design and quality of impact assessment procedures in Europe which is also reflected in our 4-country sample (OECD, 2019; Lianos and Fazekas, 2014). A large number of countries do not quantify regulatory impact assessments in a systemic matter and identify benefits of a proposed legislation qualitatively (OECD, 2019). Ex-ante RIAs that are based on a purely qualitative assessment eliminate a crucial feature of the ‘better regulation’ approach: the justification of regulations based on the quantifiable ratio between estimated benefits and costs.

France, Hungary, Italy and the United Kingdom, among others, conduct quantitative cost-benefit analyses to inform the development of laws. All four of these countries provide written advice to regulators that allows for a monetization of costs and benefits in a consistent manner. Yet despite this common denominator, the four countries differ significantly in the specificities of their RIA approach. The United Kingdom implemented one of the most comprehensive impact assessment regimes worldwide and took a leading role in the scope and quality of its better regulation framework (OECD 2010a). RIAs should strengthen economic competition and increase productivity through a reduction of regulatory “overburden” on the economy. This economic rationale remained the most important driver for RIAs over time (OECD 2002, OECD 2010a, OECD 2019). Italy and France, in contrast, apply RIAs to reduce regulatory inflation, modernize an over-regulating state, tackle regulatory complexity and reduce legal instability. Impact assessments were introduced to reduce the bureaucratic burden on economic productivity, simplify policy making and increase legal predictability (OECD 2013, 2010b). Similarly, Hungary implemented RIAs to streamline regulation, increase legal credibility and improve policy effectiveness (OECD 2014).

In terms of RIA quality, the UK is leading international standards by nurturing a policy culture that applies RIA as a mean to increase the efficiency beyond the individual legislation. It benefits from a distinct ideational approach that sets it apart from the other three countries. Rather than perceiving RIA as a way to improve individual laws, each RIA contributes to a policy learning process that should further improve the entire policy making system. This has consequences for every aspect of the RIA regime. In terms of accountability, each RIA is signed-off by both the chief economist and the minister of the department conducting the impact assessment. Through such ‘double signing’, economists and ministers basically “vouche” for the predicted monetized impact of a law. The quality of RIAs is further strengthened by a political system that assigns a strong role to cabinet committees and subcommittees where legislative proposals are further scrutinize before they are put to vote in the house. These (sub)committees rely heavily on better regulation tools and impact assessments to scrutinize legislative proposals. Moreover, the inclusiveness of the consultation process is broad and goes beyond business groups and unions. The UK’s leading role also extends to ex-post evaluation procedures. Approximately three years after a law was enacted, the ultimate outcome is evaluated and contrasted to the assumed effects in the ex-ante RIA (OECD 2010a).

A significant improvement of the French RIA framework occurred with the constitutional reform in 2008. Not only did the reform implement a more systematized application of RIAs but it also affected consultation practices and quality monitoring. France broadened the scope of consultation stakeholders beyond the previous corporatist model and strengthened independent RIA oversight. The General Secretariat of the Government (SGG), for example, provides technical training and ministers are required to contact the SGG whenever a legislation is drafted to ensure high RIA standards.



Nonetheless, the overall content of RIAs still varies significantly in quality and tends to be less detailed regarding the economic implications of a proposed legislation (OECD 2010b).

Italy, similar to France, streamlined its RIA approach in 2008 and introduced ex post evaluations in 2009. The Department of Legal and Legislative Affairs (DAGL) monitors and evaluates the quality of ex ante assessments and ex post evaluations. If a RIA is deemed insufficient in quality, it can express a negative opinion before legislation is presented to the Council of Ministers (OECD 2019). The scope of consultations is smaller than in France and it lacks formal structure. As a result, consultations are oftentimes conducted informally by the various ministries and ex post evaluations lack systematization. Moreover, the 2008 and 2009 regulations on RIA led to a relative large number of RIAs compared to the pre-2008 period and this increase in RIA quantity came at the expense of the quality of content and the quality of monitoring procedures (OECD 2013). The combination of high RIA quantity with insufficient RIA quality can increase legislative amendments and create the very opposite of what the 'better regulation' approach intends to achieve.

Hungary is a negative outlier in most aspects. In terms of RIA oversight, Hungary has no independent body to oversee the quality and content of RIAs, while the other three countries have established the oversight body through legally-binding law. These oversight bodies can not only review RIAs but also request a revision in cases when the quality does not comply with established standards. In Hungary, the quality review of a RIAs is done by a political body rather than an independent authority. This can have repercussion for the quality of all RIAs which, in turn, may affect the stability of laws more general. Lower quality RIAs could create adverse effects and may even lead to an increase in ex-post legal modifications.

All four countries updated aspects of their RIA framework over time but the relative quality differences remained over time. Table 1 summarizes the scope of RIAs in France, Hungary, Italy, and the United Kingdom on primary laws.

Table 1. Scope of RIA. The table refers to all laws with a RIA (Source: OECD Indicators of Regulatory Policy and Governance (IREG))

| Impact assessment on: | France | Hungary | Italy | United Kingdom |
|-------------------------------------|---------------|--------------|-------------------------|----------------|
| Macroeconomic costs | For some RIAs | Never | Never | For all RIAs |
| Financial costs | Never | Never | Never | For some RIAs |
| Budget | For all RIAs | For all RIAs | For major laws with RIA | For all RIAs |
| Public sector | For all RIAs | Never | For major laws with RIA | For all RIAs |
| Competition | For some RIAs | For all RIAs | For major laws with RIA | For all RIAs |
| Distributional Effects | For all RIAs | Never | For major laws with RIA | For all RIAs |
| Environmental | For all RIAs | For all RIAs | For major laws with RIA | For all RIAs |
| Trade | For some RIAs | Never | For some RIAs | For all RIAs |
| Market Openness | For some RIAs | Never | For some RIAs | For all RIAs |
| Small Businesses | For all RIAs | For all RIAs | For major laws with RIA | For all RIAs |
| Specific regional areas | For all RIAs | Never | For major laws with RIA | For all RIAs |
| Non-Profit Sector (incl. charities) | For some RIAs | Never | For some RIAs | For all RIAs |
| Foreign jurisdiction | Never | Never | For some RIAs | For all RIAs |
| Sustainable development | For all RIAs | For all RIAs | For some RIAs | For all RIAs |
| Innovation | For some RIAs | Never | For some RIAs | For all RIAs |
| Specific Social Groups | For all RIAs | Never | For some RIAs | For all RIAs |
| Gender equality | For all RIAs | Never | For some RIAs | For all RIAs |
| Poverty | For all RIAs | Never | For some RIAs | For all RIAs |
| Social goals | For all RIAs | For all RIAs | For major RIAs | For all RIAs |
| Income inequality | For all RIAs | Never | For some RIAs | For all RIAs |

While the UK assesses the impact of every dimensions for almost all laws with a RIA, the impact assessments in France and Italy tends to vary in scope.³ Yet most notably, Hungary generally excludes

³ The notable exception in the UK are financial costs that only need to be identified for some laws with an attached RIA.

a wide range of dimensions. These areas include distributional impacts that could indicate who may benefit from a law or not, gender equality, income inequality, trade and market openness, as well as the effects of a law on non-profit actors, innovation, and poverty.

In sum, the UK is an exception on almost all dimensions and we therefore use it as the baseline to which the other three countries are compared to. France and Italy take a middle position with relatively well established systems of impact assessment, while Hungary, due to the lack of transparency combined with low levels of RIA oversight, displays the least developed RIA framework (OECD, 2019; Lianos and Fazekas, 2014). These relative differences between the four countries allow us to directly test for the legislative effects of Regulatory Impact Assessments.

2.3 Politics of RIA

While RIAs should be a technical approach towards better regulation, the variation in design among the four countries indicate that the political 'battle' for regulation never disappeared. As Wegrich writes,

in an ideal world, the selection of one regulatory option over others would be based on a comparison of the costs, benefits and potential side effects of various discrete options. However, impact assessment systems are not situated in an ideal world, and the history of RIA is littered with disappointment concerning the effects of RIAs on actual policy choices and contestation concerning the design and operation of RIA regimes (Wegrich, 2011, p.397)

As a consequence of this, most of the existing research looks at the design and diffusion of RIA, its adoption and implementation, as well as the resulting economic effects (Dunlop and Radaelli, 2016). Findings are thereby derived from cross-country comparisons with some research placing RIA in the context of international organisations and the European Union (Dunlop et al., 2012; De Francesco, 2012; Radaelli, 2020; Renda, 2016). Turnpenny et al. (2008) shows that the implementation of RIAs depend on a variety of country specific features that include political commitment, legal requirements, support of powerful stakeholders, public opinion, and the institutional framework of a country. Hertin et al. (2009) build on these findings to determine the effects of impact assessment on the design of a regulation. Their evidence confirms that the actual application of RIA is shaped by the political context. Even if the intention behind the implementation of RIAs was to determine the best regulatory option, the political context can result in decisions that have not necessarily been promoted in the RIA. Moreover, if context-specific political factors are perceived to constrain the policy choices available, then officials are less willing to channel resources and conduct RIA.

But the political context has an even broader effect. Countries tend to learn from each other when it comes to the implementation of RIA. While De Francesco (2012) found support for transnational networks as a reason for governments to adopt RIA, such cross-country policy learning is not necessarily independent from regulatory specificities on the national level. National regulators adjust RIAs to their national context which lead to a "hybridisation of IA practices" among countries (De Francesco, 2016). There is further empirical evidence suggesting that several types of diffusion can co-exist which makes any streamlining of RIA across different countries rather difficult (Lianos et al., 2016).

The national regulatory set-up often takes a center stage in RIA research. Lianos and Fazekas (2014) show that it matters how expert knowledge is linked to politics and, thus, the prevalent 'evidence ecosystem' of a country; that is, the country-specific practices and institutional processes that allow for the creation and utilization of evidence for RIA. Hence, several scholars have focused on administrative features for the quality of regulatory impact assessments. Radaelli (2005), for example, shows that quality variation in RIA can occur in a context where the government questions the neutrality of those actors that conduct the impact assessment. But even if political neutrality is assumed, power struggles

between centralized RIA supervision and the supervisee can determine RIA variation (Wegrich, 2011). Moreover, the quality of RIA can be effected by administrative resistance due to the lack of administrative capacity to conduct rigorous RIA in departments and missing commitment by ministers (Caroll, 2010). RIAs may thus have only symbolic value without influence on the actual law (Radaelli, 2010). Properly trained administrators within the government are after all a crucial precondition for the quality of RIA (Kirkpatrick and Parker, 2004). Staronova (2010) confirms this in her analysis of RIA implementation and design among Central and Eastern Europe countries where administrative training, political support and a strong central oversight determine the ultimate quality of RIA implementation. The literature provides extensive insight into the determinants that underlie cross-country variation of RIA regimes. Nonetheless, the question remains whether or not RIAs, and cross-country variation in RIA regimes, have an *ex-post* effect on legal stability?

2.4 Hypotheses

While the presence of an impact assessment procedure should foster legal stability, arguably RIAs operate in a political context. Furthermore, law-specific characteristics and exogenous conditions, like macroeconomic performance, naturally intervene. Thus, we want to analyse how regulatory procedures, political factors, legislative features and economic performance jointly determine the probability whether and when a law is modified.

RIAs have a particular function in the context of political delegation. Their broad, transparent and technical design limits the influence of special interest politics which increases the probability that a new political majority will not change the already enacted law. These characteristics of RIAs are expected to lead to the durability of laws.

Hypothesis 1 *The presence of Regulatory Impact Assessment is expected to reduce the likelihood of modifying already enacted laws.*

As Thom and An (2017) argue, the durability of policies and programs more generally depend on their ultimate success. This argument seems also plausible in the context of RIA. While the technical nature of RIA should lead to less modifications of the law, they should still be modified if the expected positive net effect of a law does not materialize post-enactment. The *ex-post* evaluation stage of RIA can serve as a "fire alarm" (Radaelli, 2010) that signals policy makers if the outcome of a law deviates too strongly from the *ex-ante* predictions. It should thereby limit the economic damage of a regulation on the welfare of the target group (Kirkpatrick and Parker, 2004). While cost-benefit analysis can never account for all possible unintended regulatory effects, the detailed multi-stage process of RIA should uncover most such effects hence also diminish the likelihood of multiple modifications.

Hypothesis 2 *The presence of Regulatory Impact Assessment is expected to reduce the likelihood of modifying laws after the first modification.*

Several studies from the US have shown that the presence of a powerful government leads to less legal modifications. Focusing purely on the time of enactment, a unified government will be able to draft more coherent laws with more flexibility for the executive and thus allow a law to adjust to changing circumstances which, in turn, should lead to more legal stability (Maltzman and Shipan, 2008). Following up on this, Ragusa (2010) tested this claim by extending the focus beyond first modifications of laws. Ragusa found that, once a law is enacted, the long-term chances of the law being modified decreases if the law was passed under a divided rather than a unified government. While this contradicts to some extent previous findings, it does not consider the effects of RIA on the link between government power and legal stability nor does it explicitly incorporate politics beyond the time of implementation. Nevertheless, it seems plausible that RIA is utilised as a technical tool against volatile politics subsequent to enactment. In other words, RIAs can safeguard laws from volatile politics

because of their extensive ex-ante assessment procedures, the broad inclusion of stakeholders and the dominant role of the permanent bureaucracy rather than politically aligned ministries. RIAs could, therefore, serve as lock-in which makes subsequent legal changes more costly because the well-articulated and quantitatively estimated impacts, the incorporation of the views of a wide stakeholder group, and the relative absence of obvious legal and administrative errors. These features should reduce the chances for hijacking the law post-enactment by politicians and special interest groups for their short-term interests. We thus argue that RIA reduces the effect of changes in government power on legal modifications.

Hypothesis 3 *The presence of Regulatory Impact Assessment is expected to reduce the likelihood of modifying already enacted laws especially when the shifts in political power brings about instability.*

As we argued before, RIAs operate in a wider political and economic context. We discuss these additional factors in detail to motivate their inclusion in the regression models. To adapt these findings to the European context, we focus on the share of seats in parliament as a proxy for powerful government. We thereby assume that if the leading political party has a higher proportion of the seats it might be easier for them to pass new legislation and change existing ones. In order to account for post-enactment changes, we add changes in government power to our model.

First, partisanship at the time of enactment and changes thereof over time should have an effect. One argument is the effect of 'legislative inheritance' which should lead to legal modifications under a new parliament due to divergent preferences based on changes in partisanship over time (Berry et al., 2010). Yet the legislative inheritance argument could also hold without changes in partisanship. Therefore, we test the impact both of changes and stability in partisanship over time on the modification of laws.

Second, we look at the process of passing the law through parliament. We argue that the timing of discussing and enacting a law should play a role. The longer a law is in parliament before enactment the more it should be contested by diverse stakeholders. This suggests that the longer a law is in parliament before it is enacted the less likely are future modifications. However, a lengthy parliamentary procedure may also signal that the law itself is contested hence increasing the likelihood of modification post enactment.

Third, laws vary in their complexity and scope both of which influence the likelihood of amendments. In simple terms, the longer the law the more likely it is that it will be amended post enactment as it may cover more policy areas and affect more interest groups.

Lastly, we control for economic performance because there is yet no agreement on the direction of the effect when it comes to the amendments of laws (Ragusa, 2010). While some argue that good macroeconomic performance will increase policy activism, others argue that it is mainly under poor economic performance that laws and policies are terminated or changed (Binder, 2003; Heitshusen and Young, 2006; Berry et al., 2010). This ambiguity is crucial as our observation period spans through the 2008 financial crisis.

3. Methodology and Data

3.1 Data

The data on legislative procedures, laws and modifications in the 4 countries has been obtained from official parliamentary websites directly (Table 2). For France, Hungary, and the UK, we directly scraped the parliamentary websites, that is collecting the text appearing on those websites in structured as well as unstructured forms with the help of computer algorithms. The downloaded data was organised into a single structured database combining information from all sources making unified variables on where

it was warranted by the similarities in the source information.⁴ In the case of Italy, this laborious task of mapping, scraping and structuring legislative data was done by the Italian Law-Making Archive (ILMA) project from which we could simply download the structured data in a single file (Mesiti et al, 2015).

Table 2. Sources of legislative data by country

| Country | sources |
|---------|--|
| France | http://www.legifrance.gouv.fr/initRechTexte.do |
| Hungary | http://www.parlament.hu/iromanyok-lekerdezese http://www.parlament.hu/iromanyok-elozo-ciklusbeli-adatai |
| Italy | https://www.normattiva.it/ |
| UK | http://www.legislation.gov.uk/ http://services.parliament.uk/bills/ |

3.2 Methods

Rather than looking for the factors that influence the general probability of law modifications, we are interested in the drivers that effect the hazard of modification which a law faces throughout its life. We therefore use Cox proportional hazards model approach (Aalen et al, 2018). Our analysis started by looking at the first modification, incorporating a range of control variables; then examining how an array of factors influence the hazard of modification when all amendments are considered (for overview of variables used see Table 3).

For the first purpose of the analysis the data is organized the following way: Each law is observed for a series of months beginning in the month it was published and ending in December 2012 when our observation period ends. In order to analyse both first and multiple modifications we created two dependent variables. The first one is initially put to a value of zero and stays zero until the law is first modified. At the time of transition the law takes on a value of 1 and is dropped from the sample. This is used in Table 4, Table 5, and Table 8 as well as in Figure 1, Figure 2 and Figure 3. The second formulation of the dependent variable equals 1 every month the law is changed, otherwise it is 0. This variant is used in Table 7 and Figure 2. In this setup all the laws are censored from the right, they are only dropped from the sample at December 2012, when they were last observed. We run models both on the pooled sample including country fixed effects and also models by country in order to both explore the average effect across the whole sample and to highlight the heterogeneous effects by country.

The main independent variable of interest is whether the enacted law was subject to a prior RIA or not. As the exact meaning of what a RIA is depends to a large degree on the country, while it can also differ case by case (Lianos and Fazekas, 2014); our simple yes-no formulation of RIA treatment averages over different qualities of RIA. Hence, all our findings should be interpreted in this frame, pooling together very simple as well as advanced RIAs to estimate an average effect. The rate of RIA use, at least according to public records, also differs per country, ranging from 2.5% in Hungary to 48.1% in the UK. This difference across countries and the generally low level of RIA publication is surprising on its own considering that RIA is mandatory or at least recommended for most laws in the 4 countries (OECD, 2010a, OECD 2010b, OECD 2013, OECD 2014). As the assignment of RIA to laws is non-random, we cannot claim causal effects; instead, we identify conditional probabilities while controlling for a host of crucial factors for determining the hazard of modifications. Moreover, we rely on theoretical arguments as well as prior empirical research in interpreting the effects identified in the models and argue for the plausibility of the impact mechanisms.

⁴ The only notable deviation between these national datasets is the high missing rate for time spent in parliament in the French dataset. Hence, we run 2 models with and without this variable for France to test comparability of results.

Regarding the set of independent variables characterising political conditions, first, we look at government power which is measured by the share of seats occupied by the largest government party in the parliament. A higher proportion of the seats should make it easier for a government to pass new legislation and change existing ones. Second, in order to account for the change in government power, we add a variable that captures how the share of seats in the parliament changed each month compared to the time the law was enacted. Third, partisanship is a dummy variable that takes on the value of 0 if the government in a given country is considered left (or center-left) and 1 if it is considered right (or center-right). There are two sets of the partisanship variables; the first captures the conditions at the time when a law was enacted and takes on the same value for the whole observation period and a second set that constitutes its time varying counterparts which are updated in every month the law is observed. By controlling for all possible interactions of partisanship we can capture how changes compared to the initial conditions affect the hazard of modification.

Regarding the set of legislative variables, we capture the complexity of laws by the number of words the legislation contains (in thousands) and the depth of legislative work through the natural logarithm of the number of days a bill is the parliament for debate, before its enactment.

Lastly, we control for the broader macroeconomic environment via quarterly per capita real GDP growth suggesting that economic volatility, especially economic decline might make it easier to revise laws to adapt the legal framework to unwanted circumstances.

Table 3. Overview of variables used in the analysis, month-law level dataset (N=230,412)

| Variable name | Variable role | Values | Mean |
|---|-----------------------------------|--|-------|
| First modifications of laws | Dependent var. | 0=no modification 1=modification | 0.08 |
| Modifications of laws, multiple modifications | Dependent var. | Number of modifications | 0.24 |
| RIA dummy | Main independent var. of interest | 0=no RIA 1=RIA | 0.19 |
| Share of largest governing party in parliament at enactment | Independent var. | % share of seats | 51.21 |
| Change in government power | Independent var. | % share of seats change | -0.45 |
| Partisanship at enactment | Independent var. | 0=left 1=right | 0.61 |
| Partisanship after enactment - time varying | Independent var. | 0=left 1=right | 0.62 |
| Log time in parliament | Independent var. | Log(number of days) | 4.37 |
| Word count of the law (1000s) | Independent var. | Number of words | 9.27 |
| Real quarterly GDP growth | Independent var. | % change compared to same quarter in previous year | 0.19 |

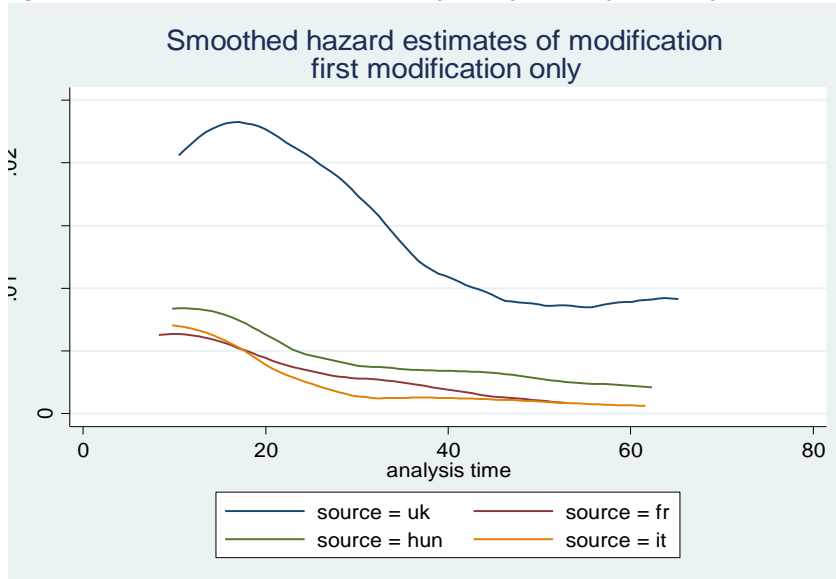
4. Results

4.1 RIA and the first modification (H1)

In order to gain insight into the variability for the first modification of laws in each country, we start with a simple bivariate set-up: plotting hazards by month. Figure 1 depicts the smoothed Kaplan-Meier estimates of hazard by country. The functions can be interpreted as the probability that a law will be modified in a given month, provided that it has not been modified yet.

A few observations are due (Figure 1): i) Modification hazard is largely downward sloping in all 4 countries albeit at varying rates. It appears that laws face the highest hazard of modification within the initial 20 months of their publication in every country, then hazards decline steeply to flatten out at the right end of the scale. ii) Interestingly, the acts produced in the UK face over twice as high risk of first modification in any given month as in the other countries (Moran, 2003). Less pronounced, but still noticeable, the hazard estimates are also slightly higher for Hungary than France or Italy. Such similarities and differences imply that our country-wise regressions are comparable as long as the different country average hazards are taken into account which is warranted by the regressions run either including a country fixed effect or separately country by country.

Figure 1. Smoothed hazard estimates of modification: first modification, 2006-2012, France, Hungary, Italy, and the UK



First, we investigate the impact of RIAs on the pooled sample of all 4 countries; including country fixed effects (i.e. allowing different intercepts) but assuming the same coefficient across the whole sample; then we move on to disaggregate findings by country (i.e. also allowing for regression coefficients to differ by country).

Table 4. shows the results of our regression models across all countries for RIA while controlling for political, legislative as well as economic factors. The model parameters are reported as hazard ratios which implies that coefficients with hazard ratios lower than one can be interpreted a decrease in the likelihood of subsequent modifications while coefficients greater than one correspond to an increase. The successive models include additional control variables on top of country fixed effects.



Table 4. Event history analysis of first modifications of laws, 2006-2012, pooled analysis

| VARIABLES/Model | Model 1 | Model 2 | Model 3 | Model 4 |
|---|----------------------|----------------------|----------------------|----------------------|
| RIA dummy | 0.581*** (0.0770) | 0.581*** (0.0835) | 0.625*** (0.0914) | 0.609*** (0.0885) |
| Share of largest gov. party in parl. at publication | | 1.045*** (0.0105) | 1.047*** (0.0107) | 1.049*** (0.0107) |
| Change in government power | | 1.023** (0.0098) | 1.021** (0.0101) | 1.026** (0.0102) |
| Partisanship at enactment & time varying interaction | | | | |
| BASELINE: left (at enactment) *left (time varying) | | | | |
| left*right | | 1.415** (0.226) | 1.394** (0.228) | 1.482** (0.250) |
| right*left | | 0.817 (0.238) | 0.717 (0.219) | 0.786 (0.244) |
| right*right | | 1.039 (0.167) | 1.075 (0.178) | 1.130 (0.193) |
| Log time in parl. | | | 1.061 (0.0571) | 1.060 (0.0563) |
| Word count of the law (1000s) | | | 1.008*** (0.0014) | 1.008*** (0.0014) |
| Real quarterly GDP growth | | | | 0.959** (0.0164) |
| Country | | | | |
| BASELINE: UK | | | | |
| France | 0.230*** (0.0295) | 0.148*** (0.0246) | 0.346*** (0.0647) | 0.328*** (0.0615) |
| Hungary | 0.321*** (0.0362) | 0.146*** (0.0302) | 0.182*** (0.0423) | 0.160*** (0.0373) |
| Italy | 0.304*** (0.0403) | 0.266*** (0.039) | 0.355*** (0.0583) | 0.314*** (0.0543) |
| Number of observations | 90,780 | 89,588 | 74,279 | 74,279 |
| Number of laws | 2660 | 2657 | 2277 | 2277 |
| Number of modifications | 591 | 587 | 559 | 559 |
| Pseudo R ² | 0.0137 | 0.0199 | 0.0280 | 0.0287 |

Note: Coefficients reported as hazard ratios; Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; All models were estimated with the *stcox* routine in STATA 12 using the Breslow method for ties.

In all our pooled models, we find an unequivocal, substantial and statistically significant impact of RIA on legal stability, whereby the presence of an ex ante RIA lowers the risk of the first modification subsequently. Largely independent of the set of control variables, the hazard ratio effect size ranges between 0.58 and 0.63, that is having conducted a RIA decreases the rate of modification by 42-37% (1-0.58 and 1-0.63) compared to the baseline.

Table 5 shows the results of our regression models in each country for RIA while controlling for political, legislative as well as economic factors. Regressions for France are included in two variants because the 'Time in parliament' variable was missing for a relatively high number of laws. Considerably modifying the picture revealed by the pooled analysis, we can identify a heterogeneous impact of RIA on legal stability in each of the 4 countries. Our models for France and Italy show a statistically significant coefficient in line with our expectations and the findings of the pooled analysis, RIA decreases the subsequent hazard ratios by 0.23-0.51, that is the risk of modification decreases by 77-49%. However, in the UK, RIA has no significant impact on first modifications; while in Hungary, RIA

has a statistically significant positive effect. The similarity between France and Italy in line with our main expectations lends empirical support to our theory. However, the lack of clear relationship in the UK - which is a frontrunner in implementing high quality RIA across a wide set of laws – seems counter-intuitive. Moreover, the fact that in Hungary RIA increase the likelihood of first modification is similarly counter-intuitive, even though there are only 31 identified RIAs for a total of 1137 laws.

There are 2 inter-related interpretations for these findings which nevertheless are only tentative and are in need of further investigation: RIAs are not assigned randomly to laws rather a complex political and technical process leads to the decision whether a RIA is conducted or not. This may mean that laws with wide ranging impacts and many impacted stakeholders are selected for RIA which naturally imply that a subsequent modification is more likely. On the other hand, RIA can become part of political games in which it may be used to neutralize opposition in politically controversial cases; however, political conflict surrounding the process of passing a law may also imply that as the balance of power shifts, modifications are more likely.

Table 5. Event history analysis of first modifications of laws, 2006-2012, country-wise analysis

| VARIABLES/Model | FR(full) | FR(restr) | HU | IT | UK |
|---|----------------------|----------------------|------------------------|----------------------|----------------------|
| RIA dummy | 0.562 (0.215) | 0.381** (0.151) | 4.601*** (1.303) | 0.228*** (0.0747) | 1.422 (0.379) |
| Share of largest gov. party in parl. at publication | 0.884* (0.058) | 0.855** (0.0654) | 0.629*** (0.107) | 1.446 (0.663) | 1.362 (0.751) |
| Change in government power | 0.944 (0.0813) | 0.877 (0.0836) | 0.850*** (0.0177) | 1.546 (0.71) | 0.776 (0.129) |
| Partisanship at enactment & time varying interaction | | | | | |
| BASELINE: left (at enactment) *left (time varying) | | | | | |
| left*right | N/A | N/A | 65.47*** (26.49) | 3.926 (3.476) | 0.134** (0.109) |
| right*left | 0.336 (0.319) | 0.373 (0.371) | N/A | 0.337 (0.271) | N/A |
| right*right | 0.815 (0.805) | 0.915 (0.959) | 35,258*** (119.670) | 3.1 (2.742) | 1.536 (4.239) |
| Log time in parl. | 0.96 (0.102) | | 1.912*** (0.188) | 0.606*** (0.0667) | 0.937 (0.0865) |
| Word count of the law (1000s) | 1.034*** (0.0071) | 1.042*** (0.0069) | 1.008*** (0.0021) | 1.030*** (0.0039) | 1.008*** (0.0022) |
| Real quarterly GDP growth | 0.925* (0.0417) | 0.938 (0.0418) | 0.968 (0.0283) | 0.986 (0.0353) | 1.072 (0.0455) |
| Number of observations | 11,739 | 25,493 | 36,089 | 20,486 | 5,965 |
| Number of laws | 369 | 692 | 1137 | 549 | 222 |
| Number of modifications | 106 | 106 | 237 | 100 | 116 |
| Pseudo R² | 0.0626 | 0.0884 | 0.0458 | 0.0852 | 0.0416 |

Note: Coefficients reported as hazard ratios; Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; All models were estimated with the *stcox* routine in STATA 12 using the Breslow method for ties.

Regarding control variables, country FE effects reflect what has been shown by Figure 1, namely that all the 3 other countries have lower modification risks with Hungary displaying somewhat higher risk

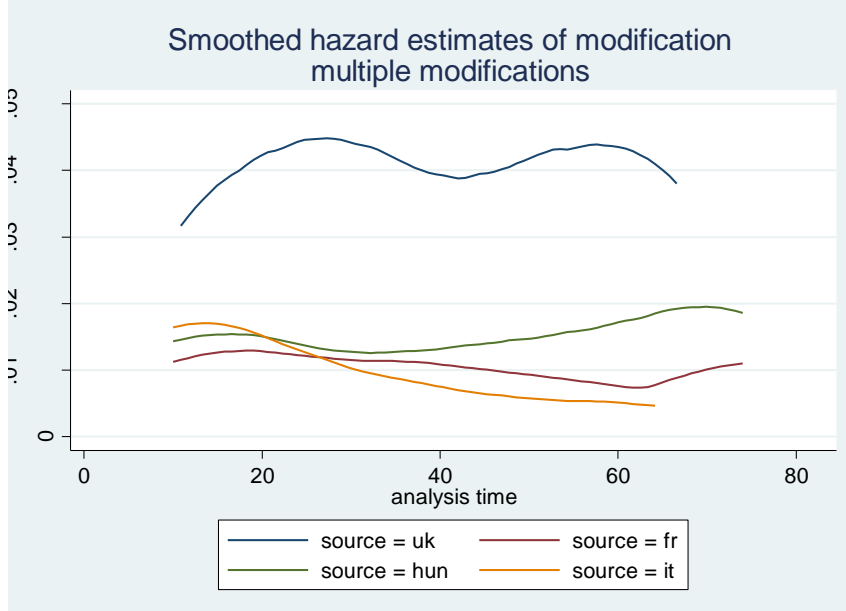
than France and Italy. Political power distribution appears to play an important role in determining the endurance of laws, both at the outset of a law and throughout its life-cycle. In the pooled analysis, stronger majorities tend to make less durable laws, albeit this effect seems to be largely driven by Italy and the UK. While changing the share of the governing party further increase the likelihood of modification (i.e. increase in the government's seat share is associated with higher modification hazard). When the, arguably, simple left-right leaning of the government and the change thereof is considered, we find that in particular the left to right shift in government is associated with a higher hazard ratio. Although, it must be noted that the effect is particularly pronounced in Hungary (David-Barrett & Fazekas, 2019) and we observed no such shift in France in 2006-2012.

Characteristics of the law itself are also strong predictors of modification hazard. The amount of time spent in parliament seems to have an ambiguous effect on modification risk. While, the complexity of the legal text (i.e. its word count) appears to clearly contribute to higher hazard ratios both in the pooled analysis and in each country. When it comes to economic environment, we find a largely negative relationship, that is when quarterly GDP growth is high the risk of modification drops, even though the relationship is not always significant on the country-wise samples.

4.2 RIA and repeated modifications (H2)

In order to gain an insight into the variability in repeat modifications of laws in each country, we first have a look at it in a simple bivariate set-up: plotting hazards by month. Figure 2 depicts the smoothed Kaplan-Meier estimates of hazard by country. The functions can be interpreted as the probability that a law will be modified in a given month.

Figure 2. Smoothed hazard estimates of modification: repeat modifications, 2006-2012, France, Hungary, Italy, and the UK



A few observations which are largely overlapping with the previous section: i) The acts produced in the UK face over twice as high risk of modification in any given month as in the other countries (Moran, 2003). Less pronounced, but still noticeable, the hazard estimates are also slightly higher for Hungary, especially towards the end of the period than France or Italy (note, Hungary had a change of government in 2010 which lead to a flurry of legislative changes). ii) Modification hazards are largely flat in all 4 countries albeit at varying rates. Such similarities and differences imply again that our countrywise as well as pooled country-fixed effects regressions are comparable.

Table 6 shows the results of our regression models across all countries for RIA while controlling for political, legislative as well as economic factors. The model parameters are again reported as hazard ratios and successive models include additional control variables on top of country fixed effects just like in the preceding section.

Table 6. Event history analysis of modifications of laws, multiple modifications, 2006-2012, pooled analysis

| VARIABLES/Model | Model 1 | Model 2 | Model 3 | Model 4 |
|---|----------------------|----------------------|-----------------------|-----------------------|
| RIA dummy | 0.505*** (0.0811) | 0.515*** (0.0845) | 0.643*** (0.102) | 0.633*** (0.0996) |
| Share of largest gov. party in parl. at publication | | 1.067*** (0.0120) | 1.073*** (0.0122) | 1.073*** (0.0121) |
| Change in government power | | 1.020*** (0.0063) | 1.027*** (0.00690) | 1.029*** (0.00683) |
| Partisanship at enactment & time varying interaction | | | | |
| BASELINE: left (at enactment) | | | | |
| *left (time varying) | | | | |
| left*right | | 1.438*** (0.165) | 1.258* (0.158) | 1.374** (0.185) |
| right*left | | 0.786 (0.182) | 0.678* (0.156) | 0.747 (0.177) |
| right*right | | 0.941 (0.196) | 0.893 (0.185) | 0.964 (0.206) |
| Log time in parl. | | | 1.026 (0.0626) | 1.025 (0.0622) |
| Word count of the law (1000s) | | | 1.009*** (0.00110) | 1.009*** (0.00110) |
| Real quarterly GDP growth | | | | 0.960*** (0.0113) |
| Country | | | | |
| BASELINE: UK | | | | |
| France | 0.266*** (0.0400) | 0.157*** (0.0320) | 0.411*** (0.0948) | 0.401*** (0.0916) |
| Hungary | 0.332*** (0.0430) | 0.122*** (0.0271) | 0.178*** (0.0423) | 0.162*** (0.0380) |
| Italy | 0.343*** (0.0655) | 0.321*** (0.0688) | 0.548*** (0.128) | 0.491*** (0.114) |
| Number of observations | 108,657 | 107,445 | 91,108 | 91,108 |
| Number of laws | 2670 | 2670 | 2287 | 2287 |
| Number of modifications | 1763 | 1759 | 1654 | 1654 |
| Pseudo R ² | 0.0144 | 0.0231 | 0.0414 | 0.0419 |

Note: Coefficients reported as hazard ratios; Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; All models were estimated with the *stcox* routine in STATA 12 using the Breslow method for ties.

In all our pooled models, we find - again - an unequivocal, substantial and statistically significant impact of RIA on legal stability: the presence of an ex ante RIA lowers the risk of the modifications subsequently. Largely independent of the set of control variables, the hazard ratio effect size ranges between 0.51 and 0.64, that is having conducted a RIA decreases the rate of modification by 49-36% (1-0.51 and 1-0.64) compared to the baseline. This suggests that H2 is supported by empirical data,



RIA lowers the likelihood of repeated modifications, hence its "fire alarm" function is most likely less pronounced than its initial impact on the quality of the law and the legislative process.

But, then again, for this interpretation to hold, all our cases need to have a well-defined ex-post evaluation process that can in fact serve as a fire alarm to adopted regulations that do not serve their initial aims. But most countries do not have an effective way to monitor regulations post-enactment. The report by the OECD (2019) showed that it is, in fact, only the UK's 'Better Regulation' framework that considers the ex-post evaluation of RIA on legal modifications.

Given that only the United Kingdom has a clearly defined framework for the ex-post evaluation of RIAs, our interpretation based on country-fixed effects may differ when we take look at our four cases. Table 7 thus displays the results of our regression models in each country for RIA while controlling for political, legislative as well as economic factors. Regressions for France are again included in two variants.

We can see that the country-wise regressions modify the picture considerably compared to the pooled analysis and we can identify a heterogeneous impact of RIA on legal stability in each of the 4 countries. The models for France and Italy show a statistically significant coefficient in line with our expectations and the findings of the pooled analysis, RIA decreases the subsequent hazard ratios by 0.19-0.56, that is the risk of modification decreases by 81- 44%. However, in the UK, RIA has no significant impact on subsequent modifications; while in Hungary, RIA has a statistically significant positive effect. In terms of political, legislative and economic control variables, we find a largely similar picture to the previous analysis.

Table 7. Event history analysis of modifications of laws, multiple modifications, 2006-2012, country-wise analysis

| VARIABLES/Model | FR(full) | FR(restr) | HU | IT | UK |
|---|----------|-----------|----------|----------|----------|
| RIA dummy | 0.601* | 0.562* | 3.467*** | 0.186*** | 1.266 |
| | (0.163) | (0.172) | (0.868) | (0.0681) | (0.229) |
| Share of largest gov. party in parl. at publication | 0.817*** | 0.810*** | 1.044 | 3.253 | 1.515 |
| | (0.0452) | (0.0544) | (0.196) | (8.232) | (0.565) |
| Change in government power | 0.844*** | 0.829*** | 0.767*** | 3.075 | 0.784*** |
| | (0.0484) | (0.0507) | (0.0177) | (7.829) | (0.0457) |
| Partisanship at enactment & time varying interaction | | | | | |
| BASELINE: left (at enactment) *left (time varying) | | | | | |
| left*right | N/A | N/A | 347.0*** | 10.77 | 0.125*** |
| | | | (146.4) | (50.32) | (0.0386) |
| right*left | 0.481 | 0.424 | N/A | 1.892 | N/A |
| | (0.439) | (0.431) | | (1.412) | |
| right*right | 2.042 | 1.836 | 2.072 | 10.61 | 2.632 |
| | (1.895) | (1.887) | (7.756) | (48.06) | (4.979) |
| Log time in parl. | 0.798** | | 1.930*** | 0.490*** | 1.214*** |
| | (0.073) | | (0.237) | (0.0634) | (0.0849) |
| Word count of the law (1000s) | 1.036*** | 1.042*** | 1.010*** | 1.023*** | 1.008*** |
| | (0.005) | (0.0047) | (0.0024) | (0.0021) | (0.001) |
| Real quarterly GDP growth | 0.954 | 0.952 | 0.967 | 0.959** | 1.108*** |
| | (0.0317) | (0.0338) | (0.025) | (0.019) | (0.0247) |
| Number of observations | 15,476 | 29,230 | 41,146 | 24,017 | 10,469 |
| Number of laws | 371 | 694 | 1141 | 550 | 225 |
| Number of modifications | 304 | 304 | 631 | 308 | 411 |
| Pseudo R² | 0.0939 | 0.123 | 0.0506 | 0.154 | 0.0588 |

Note: Coefficients reported as hazard ratios; Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; All models were estimated with the stcox routine in STATA 12 using the Breslow method for ties.

The UK and Hungary deviate from our hypothesised association between RIAs and repeated modification. This strengthens the case for further analysis of the UK and Hungarian cases. One could argue that the RIA coefficient above 1 for the UK may be due to the ex-post evaluation framework. Given the high quality of the entire better regulation set-up, adopted laws should in most cases remain unchanged. But political and societal circumstances change and even laws that displayed a strong benefits in an ex-ante evaluation, may lead to unintended negative costs later on. Thus, in such a



system, changes are indeed more likely. But again, the insignificant coefficient requires further exploration. Hungary, in contrast, has neither an ex-post evaluation technique, nor an independent agency to oversee RIAs. Hence, RIAs may generally just be a tool to convince stakeholders and parliamentary opposition for a law. Yet, once passed, the lack of independent checks on RIAs in Hungary puts a law's durability at the mercy of the government.

4.3 RIA and swings in political power (H3)

Finally, we look at the impact of RIA on legislative stability in conjunction with swings in political power. For simplicity, we only look at first modifications on the pooled sample (Table 8). In line with our expectations, Table 8 shows that RIA has the largest impact on legal stability in the wake of swings in political power. Model 2 focuses on the association between RIA and changes in government power. As mentioned before, we account for these changes by capturing how the share of seats in the parliament changed each month compared to the time the law was enacted. We can see that a change in government power has largely no effect on the risk of first modification when the law was subject to an ex ante RIA while the impact is large and positive without a RIA. In other words, without a RIA, changes in government power increase the hazard ratio by 1.032 which means that power swings increase the risk of first modifications.

In a similar vein, Model 4 considers the association between RIA and the change in government partisanship after the law is enacted. The results are thereby even more pronounced. If a law did not go through a regulatory impact assessment prior its enactment, then a change in partisanship leads to a hazard ratio of 1.66. To put it differently, in the absence of RIAs, the risk of first modifications is one-and-a-half times higher if there is a change in government partisanship. But if RIA was conducted before a law passed, the results change significantly. The association between the presence of an impact assessment and changes in government power lead to a decrease in the hazard ratio by 0.462. In this context, RIAs decrease the risk of legal instability by more than 53%.



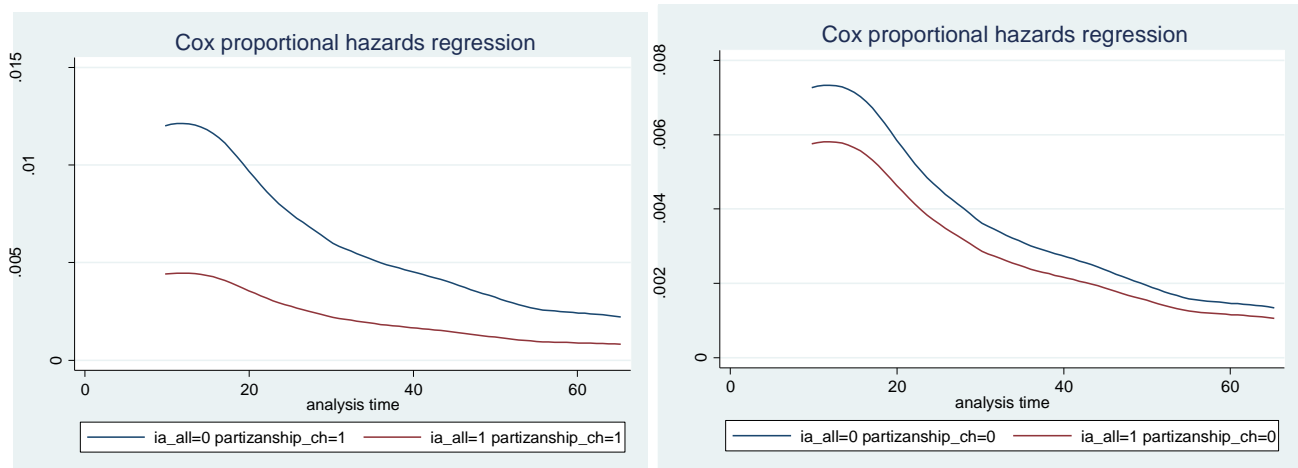
Table 8. Event history analysis of first modifications of laws, 2006-2012, pooled analysis, RIA-politics interactions

| VARIABLES/Model | Model 1 | Model 2 | Model 3 | Model 4 |
|---|----------------------|----------------------|----------------------|----------------------|
| RIA dummy | | 0.603*** (0.0908) | | |
| Share of largest gov. party in parl. at publication | 1.055*** (0.0105) | 1.050*** (0.0107) | 1.042*** (0.0065) | 1.047*** (0.0069) |
| Change in government power | 1.032*** (0.0100) | | | |
| Change in government power*No RIA | | 1.032*** (0.0109) | | |
| Change in government power*RIA | | 0.972 (0.0325) | | |
| Partisanship at enactment & time varying interaction | | | | |
| BASELINE: left (at enactment) *left (time varying) | | | | |
| left*right | 1.248 (0.198) | 1.317 (0.240) | | |
| right*left | 0.651 (0.198) | 0.816 (0.250) | | |
| right*right | 0.987 (0.162) | 1.129 (0.192) | | |
| Change in government partisanship | | | 1.273** (0.139) | |
| Change in government partisanship*RIA | | | | |
| BASELINE: NO ch.in govt. Partisanship*NO RIA | | | | |
| NO ch.in govt. Partisanship*RIA | | | | 0.791 (0.131) |
| ch.in govt. Partisanship*NO RIA | | | | 1.660*** (0.211) |
| ch.in govt. Partisanship*RIA | | | | 0.462*** (0.120) |
| Log time in parl. | 1.040 (0.0565) | 1.055 (0.0559) | 1.043 (0.0575) | 1.065 (0.0564) |
| Word count of the law (1000s) | 1.008*** (0.0014) | 1.008*** (0.0014) | 1.008*** (0.0014) | 1.008*** (0.0014) |
| Real quarterly GDP growth | 0.964** (0.0164) | 0.957** (0.0163) | 0.977 (0.0158) | 0.972* (0.0159) |
| Country | | | | |
| BASELINE: UK | | | | |
| France | 0.391*** (0.0707) | 0.332*** (0.0622) | 0.410*** (0.0673) | 0.335*** (0.0577) |
| Hungary | 0.174*** (0.0403) | 0.159*** (0.037) | 0.267*** (0.0438) | 0.206*** (0.0351) |
| Italy | 0.341*** (0.0578) | 0.331*** (0.0581) | 0.372*** (0.0579) | 0.345*** (0.0538) |
| Number of observations | 74,279 | 74,279 | 74,279 | 74,279 |
| Number of laws | 2277 | 2277 | 2277 | 2277 |
| Number of modifications | 559 | 559 | 559 | 559 |
| Pseudo R ² | 0.0271 | 0.0291 | 0.0249 | 0.0277 |

This notable difference in the impact of RIA on legal stability in conjunction with political power swings is also visually represented in Figure 3. It depicts the smoothed Kaplan-Meier estimates of hazard by RIA application and partisanship. We can interpret it as the probability that a law will be modified in a given month, provided that it has not been modified yet. The left side considers a partisanship change

from right-wing to left-wing government where the blue line is associated with having no RIA and the red one indicating a law passing through an impact assessment. The right side considers a partisanship change in from a left- to right-wing government and its associated with the absence (blue) and the presence (red) of a regulatory impact assessment.

Figure 3. Predicted hazard ratios over time, 2006-2012, pooled sample, Model 4 in Table 8. Left-hand panel is with partisanship change, right-hand panel without partisanship change



In the absence of a RIA, we can observe that the modification hazard is largely downward sloping for both types of partisanship changes. Confirming also previous findings, a laws faces the highest hazard of modification within the initial 10 to 20 months of their publication. Afterwards, the hazards decline steeply. Once RIA is present, the modification hazard is lower in both cases. This confirms our regression findings that RIAs can reduce the effect of partisanship changes in government on legal instability.

5. Conclusions

Our extensive data collection and simple data analysis has contributed to the growing body of literature performing quantitative analysis of legislative outputs and processes. Making use of a unique, large-scale dataset on all laws enacted in France, Hungary, Italy, and the UK throughout 2006-2012, we explored the impact of RIA on legal stability while trying to hold major political, legislative, and economic factors constant. While our analysis could only unearth correlations among key variables of interest rather than identify causal impacts, we gathered valuable evidence in support of the postulated theories.

Our findings show that the legislative effect of increasing technocratic rule-making can have stabilizing consequences on primary laws. First, RIA seems across-the-board to contribute to legal stability. However, the strength of this effect varies by country in ways calling for further research. Second, RIA is predicted to have the largest effect when political power swings both in terms of seats and party ideology, suggesting that the technical and consultative practices engrained in RIA can to some degree tame volatile politics.

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