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Just in Time? A Temporal Analysis of the Initiation of Legislation in Coalition Governments

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Abstract

In this paper, we explore the reproducibility of the [König et al. \(2022\)](#) paper on the timing of bill initiation under coalition governments and validate its scope condition by expanding the analysis to an additional government and country, namely the United Kingdom's Conservative-Liberal Democrat coalition government of 2010 to 2015. We find that [König et al. \(2022\)](#)'s main analysis is robust to reproduction, and that [König et al. \(2022\)](#)'s results do not travel to the UK's typical majoritarian system. Our additional contribution also highlights the potential for future research to further address the endogeneity of legislative institutions to coalition governance, and possible institutional confounders to coalition policing.

KEYWORDS: replication; reproduction; coalition government; Western Europe; Britain; Conservative-Liberal Democrat coalition; circular regression

1 Introduction

This paper presents a reproduction and expansion of [König et al. \(2022\)](#)'s paper 'Agenda Control and Timing of Bill Initiation: A Temporal Perspective on Coalition Governance in Parliamentary Democracies', published in the *American Political Science Review*. In their article, [König et al. \(2022\)](#) present a theory of bill initiation under coalition governments where the perceived *type* of a minister's coalition partners affects the minister's decision on the timing of bill initiation. Specifically, they contend that a minister's perception that their coalition colleagues are *competitive* – as opposed to *cooperative* – partners delays bill initiation. The authors test this theory using circular regression ([Gill and Hangartner 2010](#)) models on a dataset comprising over 25,000 bills from 11 European countries. Their analysis expands the existing literature on agenda control in parliamentary regimes (e.g. [Döring 1995, 2001, Döring and Hallerberg 2004, Tsebelis 2002, Martin 2004](#)), and on the role of legislative institutions under coalition governments (e.g. [Martin and Vanberg 2011, Strøm et al. 2010, Sagarzazu and Klüver 2017, Franchino and Høyland 2009](#)).

In this paper, we both explore whether [König et al. \(2022\)](#)'s analysis is reproducible, and validate its scope condition by expanding the analysis to an additional government and country, namely the United Kingdom's Conservative-Liberal Democrat (LibDem) coalition government of 2010 to 2015. For testing reproducibility, the codes and data published by the authors on the American Political Science Review Dataverse ([König et al. 2021](#)) were used. For our analysis of Britain, government bills and relevant information were identified using the UK Parliament API ([UK Parliament 2023](#)). We applied [König et al. \(2022\)](#)'s models to the UK data to test if these models also held in Britain, whose parliament has been deemed comparatively 'weak' in its policing power ([Martin and Vanberg 2011](#)).¹

We successfully reproduced [König et al. \(2022\)](#)'s main regression results on the authors' dataset. With regard to applying the analysis to the UK, we found that

¹We thank the authors for providing additional coding and clarification on some of the variables used in the original analysis.

[König et al. \(2022\)](#)'s models do not hold. We found that the coefficients for the main explanatory variable, i.e. the length of bill scrutiny, were negative across all models from the original article and not statistically significant. In other words, contrary to [König et al. \(2022\)](#)'s findings in coalitional European parliamentary regimes, a minister who received more scrutiny for their bills was no more likely to delay the introduction of their later bills in the UK's Conservative-LibDem coalition. This is so despite recent emphasis among the UK parliamentary scholarship on incremental reforms in its legislative process to improve legislative scrutiny ([Thompson 2013, 2014, 2016](#), [Levy 2009, 2010](#)), and on the coalitional dynamics under the 2010-15 government (e.g. [Bennister and Heffernan 2012](#), [Martin and Whitaker 2019](#), [Whitaker and Martin 2022](#)). Whilst our non-finding is consistent with [Martin and Vanberg \(2011\)](#)'s seminal theory on the role of 'strong' and 'weak' legislatures under coalition governments, we also highlight some additional considerations that might have been in work in the UK context, but also potentially in other countries as well, namely: (1) smaller payoffs for ministers from late bill initiation, and (2) the increasingly apparent bicameral dynamics in the UK parliament (e.g. [Russell 2010](#)).

The article will proceed in the following structure. The next section reports on the reproducibility of the [König et al. \(2022\)](#) paper, together with an overview of the original analysis. The article will then proceed to present and discuss the results from our replication on the UK data. We conclude the article by summarising the take-aways from our replication effort, and by discussing room for future research on legislative institutions and coalition governance.

2 Reproducibility

[König et al. \(2022\)](#) is an impactful addition to the rich literature on the roles played by legislative institutions in coalition governance (e.g. [Martin and Vanberg 2011](#), [Strøm et al. 2008, 2010](#)). Their theory adds an important element to existing studies on coalition governance, namely the timing of bill initiation, and utilises the relatively new method of circular regression in testing their theory ([Gill and Hangartner](#)

2010). The authors investigated whether the timing of bill initiation under coalition governments was correlated with the length of scrutiny ministers had received from coalition partners, drawing on an extensive dataset on government bills from 11 European parliamentary democracies – over 25,000 bills from Belgium, Czech Republic, Denmark, Estonia, Finland, Germany, Hungary, Latvia, the Netherlands, Norway and Poland. The method of circular regression enabled the authors to take into account the ‘circular’ nature of the data (König et al. 2022, 238), which is fitting given that the cyclical metaphor has often been employed in coalition studies (e.g. Strøm et al. 2008).

We tested the reproducibility of König et al. (2022)’s results using the code and data on König et al. (2021). The original article analysed three main models, each corresponding to the authors’ hypotheses (König et al. 2022, 236-237). Their first hypothesis states that ‘[t]he more scrutiny the [*sic*] minister’s bills have experienced, the later in the term she initiates subsequent bills’ (König et al. 2022, 236). Hence, the model is specified by the following formula, with *experienced scrutiny* as the main explanatory variable and with five control variables (*minister’s party size*, *coalition policy divergence*, *policy saliency*, *opposition policy divergence*, and *government duration*):

$$Bill\ timing = \mu_0 + g^{-1}(\beta_1\ Experienced\ scrutiny + X\phi + \epsilon)$$

μ_0 represents circular intercepts and $g^{-1}(\cdot)$ the transformation function $2arctan(\cdot)$ (Gill and Hangartner 2010), whilst X is a matrix of controls and ϕ stands for the parameter estimates vector (König et al. 2022, 240).

The second model takes into account the policy divergence between coalition partners and states that ‘[t]he higher the policy divergence between the coalition parties, the stronger the positive effect of experienced scrutiny on late initiation of

bills' (König et al. 2022, 236). The model is specified as follows:

$$\begin{aligned}
 \text{Bill timing} = & \mu_0 + g^{-1}(\beta_1 \text{ Experienced scrutiny} \\
 & + \beta_2 \text{ Coalition policy divergence} \\
 & + \beta_3 \text{ Experienced scrutiny} * \text{ Coalition policy divergence} \\
 & + X\phi + \epsilon)
 \end{aligned}$$

The third model considers the 'power' of ministers and states that '[t]he more powerful the minister is, the weaker the positive effect of experienced scrutiny on late initiation of bills' (König et al. 2022, 237). The authors operationalised this relative power of ministers by the following specification:

$$\begin{aligned}
 \text{Bill timing} = & \mu_0 + g^{-1}(\beta_1 \text{ Experienced scrutiny} \\
 & + \beta_2 \text{ Minister's party size} + \beta_3 \text{ Minister median party} \\
 & + \beta_4 \text{ Experienced scrutiny} * \text{ Minister's party size} \\
 & + \beta_5 \text{ Experienced scrutiny} * \text{ Minister median party} \\
 & + \beta_6 \text{ Minister's party size} * \text{ Minister median party} \\
 & + \beta_7 \text{ Experienced scrutiny} * \text{ Minister's party size} \\
 & * \text{ Minister median party} + X\phi + \epsilon)
 \end{aligned}$$

We re-ran these three models with the original parameters (100,000 iterations and 1,000 burn-ins), and produced the results shown in Table 1.

The results we produced across the three models were almost identical to the results presented in the original article (König et al. 2022, 241), except for the parochial difference in the statistical significance of the minister median party variable in Model 2. We further investigated the robustness of these results by running Heidelberger and Welch's convergence diagnostics, which the authors conducted in their Appendix E (König et al. 2021, 11-12). The results of the convergence diagnostics for each chain of each model are shown in Tables 2, 3 and 4. As shown in

Table 1: Main reproducibility results

	Model 1	Model 2	Model 3
Experienced scrutiny	0.49	0.36	0.58
	[0.46; 0.52]	[0.33; 0.40]	[0.52; 0.63]
Experienced scrutiny * coal pol divergence		0.31	
		[0.25; 0.37]	
Exp scrutiny * min party size * min med party			-0.37
			[-0.49; -0.25]
Minister median party			-0.65
			[-1.03; -0.29]
Minister's party size	-0.03	-0.02	0.07
	[-0.05; -0.01]	[-0.05; 0.00]	[0.02; 0.12]
Coalition policy divergence	0.19	-0.04	0.23
	[0.16; 0.21]	[-0.08; 0.01]	[0.20; 0.25]
Policy saliency	-0.19	-0.21	-0.16
	[-0.21; -0.16]	[-0.23; -0.18]	[-0.18; -0.13]
Opposition policy divergence	-0.07	-0.09	-0.08
	[-0.09; -0.05]	[-0.11; -0.07]	[-0.10; -0.06]
Government duration	-0.26	-0.26	-0.27
	[-0.29; -0.24]	[-0.28; -0.23]	[-0.29; -0.24]
Experienced scrutiny * minister's party size			-0.15
			[-0.22; -0.09]
Experienced scrutiny * minister median party			0.49
			[0.31; 0.69]
Minister's party size * minister median party			0.17
			[0.11; 0.23]
Intercept	0.22	0.22	0.41
	[0.18; 0.25]	[0.18; 0.25]	[0.30; 0.53]

Note: the dependent variable is the temporal location of bill initiation in each term. We ran 100,000 MCMC iterations with 1,000 burn-ins.

these tables, we could not reject the null hypothesis that the chains converge, and thus successfully reproduced the robustness of the results.

To summarise our tests on the reproducibility of the main results and the convergence diagnostics, we successfully reproduced the estimated parameters for the circular regression models in the original analysis and the convergence diagnostics. In the next section we turn to the verification of the models' scope condition by replicating the authors' analysis to a coalition government bills in a typical majoritarian legislature, namely the UK's Conservative-LibDem coalition government of 2010 to 2015.

3 Expansion to the United Kingdom

König et al. (2022)'s theory on the timing of bill initiation draws heavily on the assumption that coalition parties use legislative institutions such as committees

Table 2: Heidelberger and Welch’s convergence diagnostic for Model 1

	Stationary test	p-value	Halfwidth test	Mean	Halfwidth
b0_chain	passed	0.730	passed	0.2149	1.82E-04
kp_chain	passed	0.989	passed	0.5170	6.23E-05
bt_chain.1	passed	0.752	passed	0.4883	4.51E-04
bt_chain.2	passed	0.471	passed	0.1885	2.35E-04
bt_chain.3	passed	0.564	passed	-0.1870	3.84E-04
bt_chain.4	passed	0.616	passed	-0.0294	1.93E-04
bt_chain.5	passed	0.934	passed	-0.0722	1.63E-04
bt_chain.6	passed	0.727	passed	-0.2639	2.71E-04
zt_chain.1	passed	0.754	passed	0.2891	2.32E-04
zt_chain.2	passed	0.474	passed	0.1186	1.45E-04
zt_chain.3	passed	0.563	passed	-0.1177	2.36E-04
zt_chain.4	passed	0.615	passed	-0.0187	1.23E-04
zt_chain.5	passed	0.935	passed	-0.0459	1.03E-04
zt_chain.6	passed	0.729	passed	-0.1643	1.61E-04
mu_chain	passed	0.730	passed	0.2149	1.82E-04

Table 3: Heidelberger and Welch’s convergence diagnostic for Model 2

	Stationary test	p-value	Halfwidth test	Mean	Halfwidth
b0_chain	passed	0.772	passed	0.2161	1.96E-04
kp_chain	passed	0.952	passed	0.5253	6.37E-05
bt_chain.1	passed	0.433	passed	0.3644	7.56E-04
bt_chain.2	passed	0.403	passed	-0.0391	9.94E-04
bt_chain.3	passed	0.579	passed	-0.2059	3.40E-04
bt_chain.4	passed	0.796	passed	-0.0242	1.77E-04
bt_chain.5	passed	0.233	passed	-0.0908	1.88E-04
bt_chain.6	passed	0.372	passed	-0.2571	2.76E-04
bt_chain.7	passed	0.429	passed	0.3071	1.29E-03
zt_chain.1	passed	0.439	passed	0.2224	4.25E-04
zt_chain.2	passed	0.403	passed	-0.0249	6.32E-04
zt_chain.3	passed	0.578	passed	-0.1293	2.07E-04
zt_chain.4	passed	0.796	passed	-0.0154	1.12E-04
zt_chain.5	passed	0.233	passed	-0.0576	1.19E-04
zt_chain.6	passed	0.370	passed	-0.1602	1.65E-04
zt_chain.7	passed	0.425	passed	0.1895	7.52E-04
mu_chain	passed	0.772	passed	0.2161	1.96E-04

to ‘police’ their ministerial partners, a theory articulated by the seminal work of [Martin and Vanberg \(2011\)](#). [Martin and Vanberg \(2011\)](#) used the distinction between ‘strong’ and ‘weak’ legislatures (e.g. [Lijphart 1984](#)) in building their theory: Whilst the strength of legislative institutions are endogenous to the polity’s coali-

Table 4: Heidelberger and Welch’s convergence diagnostic for Model 3

	Stationary test	p-value	Halfwidth test	Mean	Halfwidth
b0_chain	passed	0.6158	passed	0.4138	1.06E-02
kp_chain	passed	0.1679	passed	0.5324	8.05E-05
bt_chain.1	passed	0.1476	passed	0.5750	2.13E-03
bt_chain.2	passed	0.1231	passed	0.2287	2.71E-04
bt_chain.3	passed	0.0907	passed	-0.1559	3.97E-04
bt_chain.4	passed	0.1495	passed	0.0721	1.73E-03
bt_chain.5	passed	0.0775	passed	-0.0764	2.56E-04
bt_chain.6	passed	0.1040	passed	-0.2658	4.75E-04
bt_chain.7	passed	0.1012	passed	-0.1515	2.49E-03
bt_chain.8	passed	0.0726	passed	0.4871	2.08E-02
bt_chain.9	passed	0.0994	passed	0.1742	2.46E-03
bt_chain.10	passed	0.6480	passed	-0.3705	1.15E-02
dt_chain	passed	0.5942	passed	-0.6716	3.47E-02
zt_chain.1	passed	0.1456	passed	0.3320	1.02E-03
zt_chain.2	passed	0.1229	passed	0.1431	1.64E-04
zt_chain.3	passed	0.0913	passed	-0.0985	2.47E-04
zt_chain.4	passed	0.1487	passed	0.0458	1.09E-03
zt_chain.5	passed	0.0775	passed	-0.0486	1.62E-04
zt_chain.6	passed	0.1037	passed	-0.1654	2.82E-04
zt_chain.7	passed	0.1001	passed	-0.0956	1.55E-03
zt_chain.8	passed	0.0574	passed	0.2867	1.05E-02
zt_chain.9	passed	0.0989	passed	0.1097	1.52E-03
zt_chain.10	passed	0.6410	passed	-0.2252	6.35E-03
mu_chain.Reference	passed	0.6158	passed	0.4138	1.06E-02
mu_chain.minister_median_party	passed	0.5796	passed	-0.2578	2.47E-02

tion experience, only ‘strong’ legislatures are able to play a role in addressing intra-coalition tensions and minimising the principal-agent issue. [Martin and Vanberg \(2011\)](#) tested this theory by examining legislative processes in five countries: three countries with ‘strong’ legislatures – Denmark, the Netherlands and Germany – and two countries with ‘weak’ legislatures – Ireland and France. They found that intra-coalition tensions played a limited role in legislative deliberations in the latter two countries. [König et al. \(2022\)](#)’s case selection is also based on such ‘strong’ legislatures with a relatively common coalition frequency – Belgium, Czech Republic, Denmark, Estonia, Finland, Germany, Hungary, Latvia, the Netherlands, Norway and Poland.

If [König et al. \(2022\)](#)’s theory is valid, we expect the theory should not apply to coalition governments in countries with infrequent coalitions. We test this scope condition of [König et al. \(2022\)](#)’s theory by replicating their analysis on the UK’s Conservative-LibDem coalition, which was in office between 2010 and 2015. The UK is a typical ‘majoritarian’ democracy, with the executive often being regarded

as dominant in the legislative process (e.g. [Lijphart 1984, 1999](#)). Indeed, the UK ranked lowest in the parliamentary policing index of [Martin and Vanberg \(2011, p.52\)](#), heavily influenced by its ad hoc legislative committees and their weak scrutinising powers. Although some recent studies have underlined the role of recent parliamentary reforms in improving the effectiveness of legislative scrutiny in the UK ([Thompson 2013, 2014, 2016, Levy 2009, 2010](#)), the UK's legislative committees remain weak comparatively, and the assessment of these reforms has been mixed among scholars ([Begley et al. 2019, Bochel 2022, Leston-Bandeira and Thompson 2017](#)).²

In order to replicate [König et al. \(2022\)](#)'s analysis of coalition politics on Britain, we collected data on all government bills that were introduced during the Conservative-LibDem coalition, including the mover ministry of each bill and the party affiliation of responsible ministers ([UK Parliament 2023](#)). The position of each party on each policy area and policy saliency were coded following the same procedures as [König et al. \(2022\)](#), using the same version (2019a version) of Comparative Manifesto Project Dataset as the original analysis ([Volkens et al. 2019](#)).³ In total, there were 122 government bills introduced during the Conservative-LibDem coalition government, 121 of which became Acts of Parliament.⁴ 15 bills that had zero values for the main explanatory variable, i.e. those that were introduced first under the coalition government, were eliminated from the subsequent analysis. The descriptive statistics of the data used are shown in [Table 5](#), and the distribution of the

²The most important reform was perhaps the introduction of evidence-taking sessions for public bills in 2006, which were not taken to account in [Martin and Vanberg \(2011\)](#)'s policing index. We also hasten to add that the inherent 'weakness' of the UK's legislative committees does not exclude the usage of other legislative institutions, such as questions, for policing the coalition's potential principal-agent issues ([Martin and Whitaker 2019, Whitaker and Martin 2022](#)).

³We gratefully acknowledge the authors' additional clarification on the coding of some of the variables. Importantly, a π was subtracted from the temporal location of bills in order to run circular regression on the data, which the authors clarified they did in their original analysis (correspondence with the authors revealed that a footnote noting this variable transformation was erroneously removed from their manuscript during the publication process). The median party variable was excluded as neither of the coalition parties held the median position in parliament for their ministerial portfolios. The party size of the responsible minister was transformed into a dummy variable. The government duration period was coded as per the [PPEG \(2022\)](#) database.

⁴The one bill that was withdrawn by the government after introduction was the House of Lords Reform Bill ([Watt 2012](#)). A total of 153 Acts of Parliament, including non-government bills, were passed during the term.

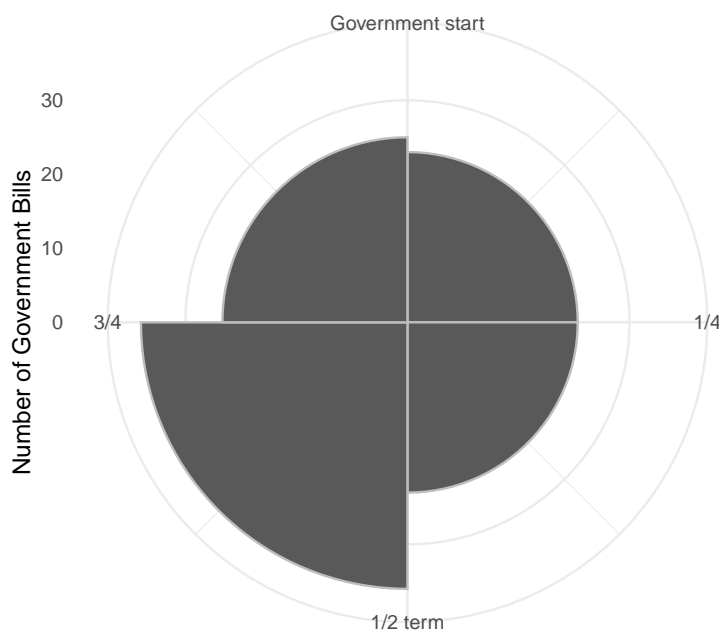
dependent variable, i.e. the temporal location of bills, is shown in Figure 1. We find that the distribution of the bills' temporal location across the coalition government's tenure is similar to those countries analysed in the original article (König et al. 2022, 238).

Table 5: Descriptive statistics of UK government bills, 2010-15

	Mean	SD	Min	Max	<i>N</i>
Bill temporal location	3.27	1.63	0.22	6.06	107
Experienced scrutiny	85.75	42.13	11.50	231.00	107
Coalition policy divergence	0.41	0.62	0.03	3.89	107
Policy saliency	2.15	0.15	1.79	2.37	107
Opposition policy divergence	1.22	1.40	0.24	9.20	107
Minister's party size	0.81	0.39	0.00	1.00	107

Note: Zero values for *experienced_scrutiny* were excluded. The statistics of bill temporal location is before the subtraction of a π , and the statistics of policy saliency is before log transformation.

Figure 1: Distribution of the dependent variable



The main results of the circular regression on the data from the UK are shown in Table 6. We found that, across all models, the coefficient of the main explanatory variable (*experienced_scrutiny*) was negative, albeit not statistically significant.

Thus, contrary to [König et al. \(2022\)](#), ministers who experienced prolonged scrutiny by parliament were no more likely to introduce bills later in the term.

Table 6: Expansion to Britain, 2010-15

	Model 1	Model 2	Model 3
Experienced scrutiny	-0.95	-1.08	-0.34
	[-2.51; 1.34]	[-2.39; 1.33]	[-1.85; 1.10]
Experienced scrutiny * coal pol divergence		0.55	
		[-1.38; 2.15]	
Minister's party size	-0.62	-0.74	0.26
	[-4.54; 1.58]	[-3.71; 1.32]	[-5.29; 1.88]
Coalition policy divergence	-0.21	-0.51	-0.53
	[-1.81; 1.94]	[-1.84; 1.67]	[-1.64; 1.37]
Policy saliency	0.35	0.47	0.67
	[-1.23; 1.64]	[-1.02; 1.69]	[-0.33; 1.65]
Opposition policy divergence	0.25	0.32	0.47
	[-1.74; 1.66]	[-1.48; 1.61]	[-1.18; 1.77]
Experienced scrutiny * minister's party size			-1.41
			[-3.15; 0.45]
Intercept	2.19	2.31	-2.05
	[-3.11; 3.26]	[-3.11; 3.23]	[-3.05; 2.94]

Note: the dependent variable is the temporal location of bill initiation in each term. We ran 100,000 MCMC iterations with 1,000 burn-ins. Zero values for *experienced_scrutiny* were excluded.

Although it may be the relatively small number of the sample bills that may be partly driving these results, we also consider two potential factors that might have contributed to these contrasting results in the UK, in addition to the UK's 'weak' legislative institutions. Specifically, we highlight: smaller payoffs for ministers from late bill initiation and the increasingly relevant bicameral dynamics in Westminster (e.g. [Russell 2010](#)).

The central claim of [König et al. \(2022\)](#) is that late bill initiation reduces time for parliamentary actors to scrutinise and amend the bill, thus providing a payoff for the responsible minister. Yet this theory does not elaborate on the possibility that parliaments can 'kill' government bills by forcing the government to withdraw its bills. In fact, one of the most contentious bills under the UK's Conservative-LibDem coalition among the coalition partners, namely the House of Lords Reform Bill, was withdrawn by the Liberal Democrat Deputy Prime Minister Nick Clegg following intense rebellion by Conservative backbenchers ([Watt 2012](#)). Indeed, it is not only in the UK where government ministers may risk the passage of their bills from late initiation. The seminal book of [Martin and Vanberg \(2011\)](#) on coalition governance

and legislative institutions begins its introduction by an anecdotal account of how the German Social Democrats prolonged the scrutiny of a children protection bill and eventually forced their grand coalition partner Christian Democrat minister Ursula von der Leyen to withdraw that bill. Whilst we believe that not including this potential negativity of late bill initiation to the authors' theory may well be justified on the grounds that it can produce a more conservative estimate for the authors' analysis, we think it is theoretically important to note that parliamentary actors may resort to this ultimate tactic of forcing bill withdrawal.

The contrasting results for the UK may also be due to other institutional confounders, such as the UK's increasingly assertive upper house. It is well documented in existing studies on recent developments in the UK's parliamentary process that its unelected House of Lords has become an increasingly relevant policy actor ([Russell 2010](#), [Russell and Cowley 2016](#), [Russell et al. 2016, 2017](#), [Kalitowski 2008](#)). These studies have shown that although the government may possess procedural prerogatives such as guillotine motions to control the legislative process, the upper house can and does force amendments on the government, especially when there is support for its amendments among backbench MPs in the Commons (*ibid*). These studies consistently point to the role of the upper house's 'anticipated reactions' or their 'preventive influence' in shaping the government's legislative strategy, implying that these parliamentary actors may influence the government even *before* the introduction of its bills and in a way that may not be *prima facie* discernible from quantitative data on the legislative process (e.g. [Russell et al. 2016](#), 290). Such a type of influence naturally confounds the theory of late bill initiation articulated by the authors, as the data only takes into account the length of scrutiny in the lower house. Although each country may have a specific constitutional arrangement on the relative powers of legislative chambers, in light of the recent findings on the bicameral dynamics in the UK's 'majoritarian' parliament, we deem further investigation is necessary with regard to how bicameralism affects the timing of government bill initiation, and how such an effect could potentially interact with

coalition governance, not just in the UK but also comparatively (ibid, see also [Garwe et al. 2022](#)).

In summary, we used the UK's exceptional coalition government of 2010 to 2015 to validate the scope condition of [König et al. \(2022\)](#)'s analysis on the timing of bill initiation under coalition governments. As expected from [Martin and Vanberg \(2011\)](#)'s theory on the role of legislatures in coalition governance, we found that the results from [König et al. \(2022\)](#) do not travel to the UK's coalition government of 2010 to 2015, where the legislature was ill-equipped to resolve intra-coalition tensions. We also noted potential additional factors that may have contributed to the contrasting results for the UK, namely parliament's power to 'kill' government bills and the 'preventive influence' of the upper house. These factors beg further comparative investigation into how legislative institutions affect the timing of bill initiation in parliamentary democracies.

4 Conclusion

In this article, we tested the reproducibility of [König et al. \(2022\)](#) and validated its scope condition by replicating their analysis on the UK's Conservative-Liberal Democrat coalition of 2010 to 2015. When testing the reproducibility of [König et al. \(2022\)](#), we were able to reproduce the authors' findings on the same data. We also run the authors' analysis on Britain's coalition government of 2010 to 2015. As expected from existing theories on the legislative institutions and coalition governance (e.g. [Martin and Vanberg 2011](#)), we found that [König et al. \(2022\)](#)'s models do not travel to a country with infrequent coalition experience. We also considered two factors that may also have led to the dissimilar results for the UK: (1) parliament's power to force bill withdrawal, and (2) the role of the upper house. This finding suggests that future research may investigate other institutional factors that affect the timing of bill initiation in parliamentary regimes.

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