SUPPLEMENTARY ANALYSIS DOCUMENT

The Policymaking Consequences of Institutional Design Under Alternative Political Contexts: Evidence from Official Revenue Forecasts in the American States

Technical Details of Hausman-Taylor Variant of FEVD Estimation Procedure

Because the institutional venue covariates of central interest to us are often slowly moving variable within each panel (state), care must be given to parceling out statistical relationships. While weakly time−invariant covariates that rarely change in a given panel can be estimated using standard fixed effects, such estimates will not only be inefficient, but these coefficients will also be highly unreliable (e.g., Arellano 2003: Chapter 2; Plumper and Troeger 2007: 127). This creates the dilemma of having to choose between modeling unit effects while forgoing valid estimates of time−invariant covariates that are central to predicting official state revenue forecasting performance, or modeling the time−invariant covariates and improperly handling any remaining unobserved heterogeneity that may exist. In practical applications, this means that one cannot jointly estimate time-varying and time-invariant covariates, alongside unit−specific intercepts without imposing additional model identification assumptions about which subset of regressors are independent of the unit effects (e.g., see Beck 2011: 121−122).

As noted in the text, the estimation approach best suited to this particular statistical modeling problem is the Hausman−Taylor (1981) variant of the FEVD estimation strategy (Plumper and Troeger 2011). First, a standard cross−sectional fixed effect (*within-variance estimator*) model is estimated on only a vector containing time−varying covariates (X_k) to obtain estimates of the unit−specific panel effects ($\hat{\mu}_{it}$):

$$y_{it} - \bar{y}_i = \beta_k \sum_{k=1}^{K} \left(X_{k,it} - \bar{X}_{k,i} \right) + \varepsilon_{it} - \bar{\varepsilon}_i \quad . \tag{1}$$

The unit specific effects obtained from (1) are $\hat{\mu}_i = \overline{y}_i - \hat{\beta}_k^{FE} \sum_{k=1}^{K} \overline{X}_{k,i} - \overline{\varepsilon}_i$. Next, the unit−specific panel effects ($\hat{\mu}_{it}$) are regressed on the vector of time−invariant covariates (Z_m) via pooled OLS:

$$\hat{\mu}_i = \omega + \gamma_m \sum_{m=1}^{M} Z_m + \eta_i \quad , \tag{2}$$

Moreover, random component of the unit fixed effects ($\hat{\eta}_i$) – i.e., $\hat{\eta}_i = \hat{\mu}_i - \omega - \gamma_m \sum_{m=1}^{M} Z_{m,i}$ – is assumed to be independent of the time-varying covariates (Z_m) by assumption for purposes of model identification noted in the preceding paragraph. Equations (1) and (2) are equivalent to estimating the following single-stage panel regression model via pooled OLS that jointly estimates the time−varying covariates (X_k), time-invariant covariates (Z_m), and the random component of unit effects ($\hat{\eta}_i$) where $\phi = 1$:

$$y_{it} = \alpha + \beta_k \sum_{k=1}^{K} X_{k,\,it} + \gamma_m \sum_{m=1}^{M} Z_{m,i} + \phi \hat{\eta}_i + \varepsilon_{it} \quad . \tag{3}$$

The inclusion of ($\hat{\eta}_i$) as a covariate ensures that (3) will produce valid estimates that account for any omitted variable bias attributable to the fact that the time-invariant covariates are assumed to be orthogonal to the unit−specific panel effects by construction. The variance-covariance matrix estimates produced by (3) are generated by using values of both the time−invariant covariates ($Z_{m\,i}$) and unit effects for the time−variant covariates ($\hat{\mu}_i$) as instruments to account for the downward bias that plagues their original variance-covariance matrix estimates (Breusch, et al 2011).[1] Temporal dynamics are modeled via a Prais−Winsten AR(1) serial correlation correction.

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[1] Plumper and Troeger's (2011) revised FEVD variance–covariance formula is: $V_{FEVD}(\beta, \gamma) = (H'W)^{-1} H'\Omega H (W'H)^{-1}$, where $H = [X^*, Z]$, $W = [X, Z]$, and $\Omega = \sigma_\varepsilon^2 I_{NT} + \sigma_\eta^2 I_N \otimes \iota_T \iota'_T$,

In addition, we analyzed these statistical models using alternative estimation approaches. These alternative panel econometric estimation strategies include cross–sectional random effects and time–wise fixed effects, plus also estimate models both with and without first–order serial correlation corrections. Further, we estimate variants of these models that consider institutional venue–political context covariates as endogenous regressors with respect to revenue forecast performance (Instrumental Variable [IV] models). These various robustness checks are discussed at considerable length in this ***Supplementary Appendix*** document that has been jointly submitted with the revised manuscript to JPAM. Although there are some tangible differences among results within models that are to be expected from using sets of ten different estimation techniques, the core conclusions drawn from the evidence are consistent with those reported in the manuscript.

Addressing a Potential Endogeneity Critique

Institutional structure is treated as being exogenous to policy performance in both theoretical and empirical terms in this study. While one may question whether policy (forecasting) performance may also affect the choice of policymaking (forecasting) institution, we cannot relax this exogeneity assumption *a priori* for two reasons. First, there may be coalitional drift so that the original intent of the enacting coalition differs from the intent of the current coalition. If this is the case, then attempts to insulate delegatory institutions from coalitional drift by making it rather difficult to alter them is the solution to this problem (e.g.,

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where I_N is an $N \times N$ identity matrix and ι_T is a $T \times 1$ vector of ones. We gratefully acknowledge both Thomas Plumper and Vera Troeger for providing us with their updated **xtfevd** STATA code.

Horn and Shepsle 1987; Moe 1989; Shepsle 1992). Indeed, it is possible in several states that decisions on proper revenue forecasting venue were made well before our sample period began by coalitions that likely differ substantially from the current dominant coalition. Second, whether a particular institutional venue under a particular political context possesses official revenue forecast responsibility may be directly related to the nature of constitutional powers embodied in the American states. These issues are addressed in the subsequent pair of sub-sections analyzing potential endogeneity bias between institutional venue-political context and revenue forecast performance.

Assessing Potential Endogeneity via Within Sample Period Changes of Institutional Venue

In the first set of analyses of the potential endogeneity critique, we analyze whether revenue forecasting performance affected changes in the institutional venue responsible for making official revenue forecasts in the American states observed during our sample period. This was implemented through a series of direct tests to determine whether forecast outputs might have *systematically* influenced a change in forecasting institution by making comparative state-level assessments regarding the revenue forecasting environment prior to the institutional venue change. The first test presupposes that a given state's economic volatility – measured as the three-year lagged standard deviation in state *i*'s real gross state product growth (*Economic Growth Volatility*) – is significantly higher compared to all other states for the years prior to the change in policymaking venue. The second test pertains to *Revenue Shocks* that account for the unanticipated component of revenues that deviate from a long run trend of general fund revenues collected relative to income (Crain 2003: 74-75). Specifically, we expect that higher unanticipated negative (positive) revenue shocks for a given state relative to other states will be most (least) apt to cause a change in policymaking venue for the state in question. The third and fourth tests directly inspect a state's revenue forecasting performance relative to those of other states in the year preceding a change in policymaking venue in terms of *Forecast Accuracy* and *Forecast Conservativism*, respectively. Logic suggests that states which change institutional venue responsible for making official revenue forecasts will have generated both less accurate and conservative revenue forecast errors relative to all other states during the period preceding this institutional venue change. That is, the endogeneity critique would contain some merit if states with relatively inaccurate or optimistic forecasts compared to other states moved policymaking authority to a more politically insulated institution.

A summary of our statistical findings appear in **Table SA−1**. Eleven states changed their policymaking venue within our sample period where we have available data to assess policy environment and performance effects to assess potential endogeneity.[2] Not surprisingly, given the trend toward Consensus Group' independent commissions over the past few decades, most of the changes were from either the legislative branch or the executive branch to a consensus group independent commission. Only a single state (Louisiana) shows support for both dimensions of forecasting environment (*Economic Growth Volatility* & *Revenue Shocks*), but does not do so with respect to actual forecasting performance (*Forecast Accuracy* & *Forecast Conservatism*). In all but two of the remaining states that changed institutional venue during our sample period (Maine and Vermont), only one of these four dimensions appear to be consistent with the logic

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[2] South Carolina changed their Consensus Group structure with all partisan members to one containing some nonpartisan members in FY 1989. However, since these structures are combined in our analysis, this change institutional change is omitted from the subsequent analysis.

of endogenous institutional change. Most of this modest evidence is derived from *Revenue Shocks*, and only two states does this emanate from differences arising from *Forecast Conservatism* (Kentucky and New York). Under no circumstance did a state change its institutional venue in response to less accurate revenue forecasts compared to other states in the period preceding the institutional change (*Forecast Accuracy*). There are also other reasons for well-founded skepticism regarding the potential for endogeneity bias relating to the analysis of institutional venue change. In three of four cases where a state moved from an executive branch forecast to a consensus group, it came in the year after a governor's lame duck year, implying a change possibly was made due to poor forecast performance. Yet, in these three instances, the loss of executive branch authority over official revenue forecasts transpired during unified party government. In turn, this implies that the change in institutional venue from the governor to the consensus group was probably not driven by a desire to rebuke the governor for manipulating revenue forecasts. While it is not possible to entirely rule out bias arising from an endogeneity problem in this particular analysis, the bulk of the evidence suggests that changes in revenue forecast institutions were not chosen in response to poor forecast performance in terms of either forecast accuracy or forecast conservatism.

[Insert Table SA−1 About Here]

Assessing Potential Endogeneity via Instrumental Variables

In addition, we consider potential endogeneity bias between institutional venue-political context and revenue forecast performance that accounts for all fifty American states (where 39 states possessed fixed institutional arrangements with respect to official revenue forecast responsibility during the sample period). Addressing potential endogeneity in this manner requires us to rely on constitutional and institutional features as instrumental variables that suitably predict institutional venue-political context, but remain uncorrelated with the residual term in the revenue forecast error 'structural' equations. We offer three candidates for viable exogenous instruments. The first instrument, *Governor Full Budget-Making Powers*, is a binary variable that is coded 1 if the governor exercises unilateral control over a state's budget formulation, 0 if this policymaking responsibility is shared with other governmental institutions (Mean = 0.79, SD = 0.41).[3] The idea underlying this instrumental variable is simple. In order to provide a balance of fiscal policymaking powers, states whose governors exercise full budget formulation authority will be less likely to have responsibility for making official revenue forecasts. Put another way, legislatures and independent commissions (Consensus Groups) will be more likely to control official revenue forecasts when the governor exercises unilateral control over constructing the state government's budget. Relatedly, this 'balancing of power' logic means that the likelihood of the executive branch controlling the official revenue forecast will be considerably lower in those states where the governor is not subject to term limit restrictions compared to those states where they face this type of electoral constraint. A second instrumental variable that assesses executive branch constitutional powers is *Independent Elected Executive Branch Fiscal Officials* that is measured as a count variable of the number of independently elected executive branch officials with fiscal policymaking responsibilities (sans the governor). This variable ranges from 0 to 4 (Mean = 1.03, SD = 0.66).[4] Higher (Lower)

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[3] These data come from the table "***The Governors: Powers –Budget Making Power***" in *The Book of the States* (1986−2008).

[4] The relevant state-level executive branch fiscal officials accounted for in this measure are as follows: Treasurer, Comptroller, Financial Officer, and Revenue Officer. 20.33% of cases are coded "0", 56.06% are coded as "1", and 23.61% of the cases are coded as "2". These data come

values indicate a greater diffusion (concentration) of power residing within the executive branch. Under such circumstances, diffuse executive fiscal powers' states should result in a higher probability of the executive branch being afforded responsibility for official revenue forecasts compared to when power is more heavily concentrated in the hands of the governor's office. The third and final instrument, *Non−Delegation Doctrine*, focuses on the legislature's constitutional capacity to delegate policymaking authority to other institutions (Mean = 2.26, SD = 0.69). This variable is coded as an ordinal measure that equals 1 when the legislature experiences considerable latitude for delegating policy tasks to the executive branch (*weak restrictions*), equals 2 when they possess moderate discretion for delegating policy tasks to the executive branch (*moderate restrictions*), and equals 3 when they possess very limited ability to delegate policy tasks to the executive branch (*strong restrictions*).[5] Legislatures operating under increasing delegation restrictions will be more likely to make official revenue forecasts since they are less capable of delegating these tasks to either the executive branch or independent commissions.[6]

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from the table "***Selected State Administrative Officials: Methods of Selection***" in *The Book of the States* (1986-2008).

[5] The relevant breakdown of this variable by state−year observations is as follows: *Weak Restrictions*: 14.04%, *Moderate Restrictions*: 45.94%, and *Strong Restrictions*: 40.02%. These data come from Rossi (1999: 1201, Table 1). We thank Gbemende Johnson for generously sharing her data with us.

[6] Our initial choice of instruments was based upon a belief that theoretically they make sense. We also evaluate this empirically as suggested in the text above. We have evaluated the quality of the instruments in other ways. First, we verified the instruments themselves are exogenous.

In the subsequent instrumental variable−'reduced' form regression analysis, we employ these three exogenous instruments to arrive at the best joint prediction of the institutional venue-political context under consideration based on a Wald χ^2 exclusion test.[7] Individual binary Probit equations are estimated that predict the absence or presence of a particular institutional venue-

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The only instrument that we examined that we felt completely confident was exogenous was the 'non-delegation' doctrine trichotomous indicator. Second, we evaluated each of the three instruments adopted here by implementing a simple "reverse causation" test (Angrist and Pischke 2009) and then re-estimated the models described above including only the instruments that this method suggests are truly exogenous based upon these test results. This involves estimating models that regress the institutional venue variables on the instruments and the instruments at t+1. If the coefficient on the instrument at t+1 is significant, then this suggests that the instrument may be endogenous to institutional venue-political context. We reestimated the HT-ar(1) and HT models including only those instruments that this method suggested were exogenous and the results generally confirm what is described in the text except that in two cases the IV models exogeneity tests rejecting the null hypothesis that the *Institutional Venue-Political Context* covariates are exogenous to forecast performance approach conventional levels of statistical significance ($p < 0.07$; models corresponding to Table SA−5, HT and SA−6, HT). The full estimates are available upon request from the authors.

[7] By selecting the covariates that provide the strongest set of instruments, we attempt to ensure that the chosen instruments are best for assessing endogeneity bias subsequently estimated in the structural-outcome equations using the 2SRI method discussed in the next section.

political context measured as covariates appearing in *Models 1−4* displayed in **Table 2**.[8] As the results in **Table SA−2** reveal, the set of instruments chosen vary in each Probit equation, and their predictive content also varies by equation as evinced by the Wald χ^2 exclusion tests. **Model A** shows that both a governor's budget making powers is positively correlated with the probability of the legislature possessing official revenue forecast policymaking authority although the coefficient is imprecise. **Model B** reveals that governors' exercising unilateral control over budget formulation are less likely to obtain the power to issue official revenue forecasts consistent with the 'balance of powers' logic noted earlier. The strength of this inverse relationship is much greater when the governor can be elected to an unlimited number of terms (**Model C**) compared to when they are subject to term limits (**Model D**). **Models G**, **H**, and **J** suggest that states with governors exercising unilateral control over budget formulation are also more likely to have legislatures conducting official revenue forecasts. Legislatures with greater restrictions on delegation are estimated to be less likely to delegate forecasting responsibility to the executive branch with the odd exception of states experiencing periods of split branch government.

[Insert Table SA−2 About Here]

The consequences of endogeneity bias on the institutional venue-political context estimates are addressed in the next section which offers a comparative analysis of the robustness of our findings using alternative estimation techniques. In short, while the instruments used to predict institutional venue are often 'strong' predictors as noted in **Table SA−2**, accounting for

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[8] The *Independent Elected Executive Branch Fiscal Officials* is omitted from these IV−Probit model specifications since it is either not exogenous to Institutional Venue-Political Context based on the reverse causality test, or results in an inferior model fit.

this potential source of endogeneity bias often (thought clearly not always) fails to improve model performance based on both 1) the Hausman exogeneity test results and 2) the sizable efficiency loss from these two-stage residual inclusion (2SRI) model specifications (see next section for technical description of the 2SRI method). In several instances (**Models 3 & 4**: *CSRE-ar(1)*, *CSRE*, and *TWFE*), this efficiency loss is relatively minor and the corresponding Hausman exogeneity tests rejecteding the null hypothesis that the *Institutional Venue-Political Context* covariates are exogenous to forecast performance. Yet, in these particular instances, the findings from the 2SRI method reveal mixed no clear patterns in terms of relative coefficient differences produced by the HT-ar(1) estimation method reported in the manuscript (as indicated by Wald Coefficient restriction tests appearing on the bottom portion of **Table SA−5 & Table SA−6**). In some instances, these relative coefficient differences reported in the manuscript are more conservative than those generated by these relatively 'efficient' IV models (e.g., those involving various legislative branch and executive branch term limit lame duck differences), and in other instances they are less conservative (e.g., those involving various legislative branch & executive branch: no term limit differences). These issues are discussed in greater detail in the next section comparing the revenue forecast error models across several different estimation methods.

Robustness Checks Accounting for Alternative Estimation Techniques

To address potential endogeneity bias between institutional venue-political context covariates and revenue forecast performance, we utilize the two−stage residual inclusion (2SRI) method for handling potential endogeneity bias. The 2SRI method simply involves estimating the structural/"outcome" equation comprised of endogeneous regressors and exogenous covariates as if endogeneity were to be ignored, plus adding the predicted residual probability for each relevant endogenous regressor in the reduced−form/instrumental variable estimated Probit equations appearing in **Table SA−2**. In this manner, endogeneity bias is treated as an omitted variable problem that is properly accounted for by inclusion of these residual probability covariates relating to the endogenous regressors. The 2SRI method is desirable in our empirical application for three reasons. First, the 2SRI technique produces consistent estimates when either the endogenous regressor(s) or outcome variable (regressand) are measured as a discrete or limited dependent variable (Terza, Basou, and Rathez 2008).[9] Second, this technique allows one to treat multiple binary endogenous regressors "*As Is*", as opposed to relying on continuous measures of institutional venue-political context which are inconsistent with the discrete, mutually exclusive concepts analyzed here. We can thus make direct comparisons between IV and non-IV model specifications. Finally, the 2SRI method allows one to test endogeneity bias as a restriction within the confines of the original structural equation of interest (Hausman 1978).

Besides dealing with endogeneity corrections, we also utilize alternative estimation techniques that either correct [ar(1)] or do not correct for first-order serial correlation, plus use cross-sectional random effects (CSRE) and timewise fixed effects (TWFE) methods as an alternative to the Hausman-Taylor/FEVD [HT] based estimates reported in the manuscript. The first set of analyses cover the **Model 1** specification in **Table 2** which considers the legislative branch (LB) and executive branch (EB) control over official revenue forecast responsibility, with consensus group independent commissions (CG) captured in the intercept term as the baseline

[9] The 2SRI technique has been used in many applications involving various types of limited endogenous or dependent variables (e.g., Blundell and Smith 1989; Newey 1987; Rivers and Vuong 1988; Wooldridge 2002).

category. These results appear in **Table SA−3**. The HT-ar(1) results reported in the manuscript yield coefficients which are larger in magnitude, but estimated less precisely, relative to the CG baseline than compared to other non−IV estimation approaches [HT, CSRE-ar(1), CSRE, and TWFE]. Yet, the Wald coefficient differences between LB and EB are more modest in the reported model [HT-ar(1)] than in the other non-IV estimation approaches [HT, CSRE-ar(1), CSRE, and TWFE]. The IV model estimates [HT-ar(1)*, HT*, CSRE-ar(1)*, CSRE*, and TWFE*] are estimated with considerable imprecision, and thus are highly inefficient – a fact further corroborated by the failure to reject that the residual probabilities from the LB and EB Probit equations are jointly different from zero via the Hausman IV exogeneity test. Therefore, endogeneity bias does not appear to be a problem in the various **Model 1** specifications, and accounting for it as omitted variable bias results in highly inefficient estimates that falsely obscure differences between legislative and executive branch revenue forecasts.

[Insert Table SA−3 About Here]

The various estimation approaches for **Model 2** appear in **Table SA−4**. The institutional venue-political context covariates are estimated with greater precision in the alternative non−IV models, but have coefficients which are considerably smaller in magnitude than compared to the ones reported in **Table 2** based on the HT/FEVD-ar(1) technique. Once again, the IV model methods are highly inefficient given their much larger standard errors compared to non−IV models, and also corroborated by the failure to reject the null of exogeneity displayed in the Hausman IV exogeneity test statistics. Moreover, the Wald coefficient different tests among the various institutional-venue-political context covariates reveals that the reported results based on the HT/FEVD-ar(1) method are more conservative relative to other non−IV models. The failure to reject coefficient differences between the institutional venue-political context covariates in the

IV models is a manifestation of the highly inefficient nature of these set of statistical estimates. As a result, the IV model estimates are neither suggestive of endogeneity bias nor produce superior estimates.

[Insert Table SA−4 About Here]

The comparison of estimation procedures for **Model 3** appears in **Table SA−5**. There are some similarities to the patterns apparent in **Tables SA−3** and **SA−4**. Specifically, the IV model estimates are often estimated with poor precision. That said, the Hausman IV exogeneity test restrictions are rejected at conventional levels of statistical significance in several of the models. The Wald coefficient differences between various institutional venue-political context variables tend to be more conservative based on the HT-ar(1) estimates relative to the non−IV models based on alternative panel estimation strategies. However, in those instances where -endogeneity bias appears to be a tangible problem based on the significant Hausman IV exogeneity test statistic, it is driven by the predicted residual probability corresponding to when the legislature is not subject to term limits (*Legislative Branch: No Term−Limit*).[10] The differences between both legislative branch scenarios (term limit restrictions and no term limit restrictions) and a governor not subject to term limits, as well as the distinction between governor not subject to term limits and those that are subject to such term limit restrictions but are not lame−ducks, are no longer significant in the models based on instrumental variables. Interestingly enough, the difference between legislatures not subject to term limit restrictions and governors that are term limited

[10] There are three exceptions to this general pattern: *Executive Branch: Term Limit−Lame Duck* = *Executive Branch: No Term Limit* [EBRTLD = EBUT]; Legislative Branch: Term Limit = *Executive Branch: No Term Limit* [LBTL = EBUT]*; Legislative Branch: No Term Limit =* *Executive Branch: No Term Limit* [LBNTL = EBUT].

lame ducks becomes significant once one accounts for endogeneity bias. Nonetheless, these various results from these alternative robustness checks are consistent with the main findings reported in the manuscript. Specifically executive branch revenue forecasts in states with gubernatorial term limits are generally indistinguishable from legislative branch forecasts, and when they differ they indicate that the legislature not subject to term limit restrictions will produce more conservative revenue forecasts than a lame duck governor completing their tenure in office.

[Insert Table SA−5 About Here]

The final set of analysis comparing results from **Model 4** across different estimation strategies appears in **Table SA-6**. In the HT*, CSRE-ar(1)*, CSRE*, and TWFE* models, the Hausman IV exogeneity tests are clearly rejected by the data. For consistency purposes, we present the HT-ar(1) model results in **Table 2** of the manuscript. Moreover, in general, the Wald coefficient differences between various institutional venue-political context variables tend to be more conservative based on the HT-ar(1) estimates relative to the IV models, especially those where endogeneity bias appears to be a tangible problem based on the significant Hausman IV exogeneity test statistic. There are a handful of cases where the Wald tests after the HT-ar(1) models identify a statistically distinguishable difference in coefficients but these disappear in the IV models.[11] Nonetheless, across all ten model estimation methods the statistical results

[11] Specifically, *Legislative Branch: Unified Government* = Legislative Branch: Split Branch−Unified Legislature [LBUG = LBSB-UL] (HT*; CSRE-ar(1)* , and CSRE*), *Legislative Branch: Divided Legislature* = *Executive Branch: No Term Limit* [LBDL = EBUT] (HT-ar(1)* , HT*), and , *Executive Branch: No Term Limit* = *Executive Branch: Term Limit-No Lame Duck* [EBUT = EBRTNLD] (TWFE*).

consistently show that executive branch actors not subject to term limit restrictions (EBUT) produce more conservative revenue forecasts than when the legislature controls this policymaking responsibility under times of unified party government (LBUG). Moreover, the evidence across these ten models consistently demonstrates that one cannot distinguish revenue forecasts conservatism between legislatures under either unified party government (LBUG) or split partisan branch-unified legislature (LBSB: UL) controlling revenue forecasts from those instances when governors control the revenue forecast and are serving in their lame duck-term in office (EBRTLD). Finally, in keeping with a major finding of this study reported in the manuscript, the IV estimation−based results support the claim that legislative branch forecasts are more conservative in the presence of divided partisan legislatures than unified party government. As a matter of fact, this particular finding becomes stronger or robust when accounting for endogeneity bias. In turn, these pair of key findings corroborates a major point of this study −− that understanding the policy consequences of delegation decisions requires understanding the political context in which institutional actors exercise policymaking responsibility. To unequivocally declare that the best solution for arriving at conservative revenue forecasts is for the legislature to delegate this policymaking responsibility to the executive branch is erroneous. Rather, determining which policymaking institution is most apt to offer conservative revenue forecasts requires a nuanced understanding the political incentives and pressures facing each political institution, and how these characteristics predictably vary across the American states.

[Insert Table SA−6 About Here]

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Table SA−1:

Summary Analyzing Potential Endogeneity:
Changes In Forecasting Institution Related to Forecasting Performance

State	Date of Change	Original Format	Revised Format	Economic Growth Volatility	Revenue Shocks	Forecast Accuracy	Forecast Conservatism
Colorado	1989	EB	LB	ND	**CS**	*WS*	*WS*
Kentucky	1994	EB	CG	ND	ND	*WS*	**CS**
Louisiana	1988	LB	CG	**CS**	**CS**	WS	WS
Maine	1993	EB	CG	ND	*WS*	ND	ND
Michigan	1990	LB	CG	ND	**CS**	*WS*	ND
Mississippi	1993	EB	CG	ND	**CS**	*WS*	ND
Nevada	1996	LB	CG	*WS*	**CS**	ND	ND
New York	1996	LB	CG	*WS*	*WS*	*WS*	**CS**
Rhode Island	1991	EB	CG	*WS*	*WS*	ND	ND
Tennessee	1993	EB	CG	*WS*	**CS**	*WS*	ND
Vermont	1996	LB	CG	ND	ND	ND	ND

Note: **CS** (*Correct Sign*) refers to forecasting difficulty being significantly greater in state *i* prior to format change relative to other states. **ND** (*No Difference*) refers to no significant difference in forecasting difficulty in state *i* prior to format change relative to other states. **WS** (Wrong *Sign*) refers to forecasting difficulty being significantly less in state *i* prior to format change relative to other states. **EB** refers to Unitary Executive Office. **LB** refers to Legislative Branch. **CG** refers to Consensus Group. **Economic Growth Volatility** refers to lagged standard deviation in state *i*'s state growth product from the preceding three years. **Revenue Shocks** refers to the unanticipated component of actual general fund revenues (Crain 2003: 74-75). **Forecast Accuracy** refers to [(|actual state general fund revenues – official projected state general fund revenues|) / actual state general fund revenues]*100. **Forecast Conservatism** refers to [(actual state general fund revenues – official projected state general fund revenues)/actual state general fund revenues]*100.

TABLE SA−2: Instrumental Variable 'Reduced-Form' Probit Regression Results for Institutional Venue-Political Context Covariates

(Only Instrumental Variable Results Reported)

	Model A: LB	**Model B: EB**	**Model C: EB-UT**	**Model D: EB-RTLD**	**Model E: EB-RTNLD**	**Model F: LB:NTL**	**Model G: LB:TL**	**Model H: LB:UG**	**Model I: LB: SB-UL**	**Model J: LB: DL**
Governor Full Budget-Making Powers	0.43 (0.38)	-0.52* (0.29)	-0.82** (0.37)	-0.39 (0.27)	________	________	0.30 (0.30)	0.26 (0.33)	________	0.58* (0.31)
Non-Delegation Doctrine	________	________	-0.34 (0.29)	________	-0.46* (0.26)	0.06 (0.25)	________	0.32 (0.25)	-0.41* (0.23)	0.35* (0.20)
Joint χ^2 ~(k) Test: Instrument Exclusion	1.26 [0.26]	3.18* [0.07]	5.22* [0.07]	2.18 [0.14]	3.07* [0.08]	0.06 [0.81]	0.99 [0.32]	2.85 [0.24]	3.40* [0.07]	6.07** [0.05]
Pseudo R^2	0.37	0.21	0.40	0.24	0.25	0.28	0.15	0.43	0.44	0.19
Effective Sample Size	1099	1099	1099	1099	1099	1099	1099	1099	1099	1099

Notes: ***Model A: LB*** (Dependent Variable: = 1 if Legislative Branch has official revenue forecast responsibility, = 0 otherwise); ***Model B: EB*** (Dependent Variable: = 1 if Executive Branch has official revenue forecast responsibility, = 0 otherwise); ***Model C: EB: UT*** (Dependent Variable: = 1 if Executive Branch - No Term Limit Restrictions has official revenue forecast responsibility, = 0 otherwise); ***Model D: EB: RTLD*** (Dependent Variable: = 1 if Executive Branch – Term Limits, Lame Duck has official revenue forecast responsibility, = 0 otherwise); ***Model E: EB:RTNLD*** (Dependent Variable: = 1 if Executive Branch – Term Limits, Non-Lame Duck has official revenue forecast responsibility, = 0 otherwise); ***Model F: LB: NTL*** (Dependent Variable: = 1 if Legislative Branch - No Term Limit Restrictions has official revenue forecast responsibility, = 0 otherwise); ***Model G: LB:TL*** (Dependent Variable: = 1 if Legislative Branch – Term Limit Restrictions has official revenue forecast responsibility, = 0 otherwise); ***Model H: LB: UG*** (Dependent Variable: = 1 if Legislative Branch – Unified Government has official revenue forecast responsibility, = 0 otherwise); ***Model I: LB: SB-UL*** (Dependent Variable: = 1 if Legislative Branch – Split Partisan Branches-Unified Legislature has official revenue forecast responsibility, = 0 otherwise); ***Model J: LB: DL*** (Dependent Variable: = 1 if Legislative Branch – Divided Partisan Legislatures has official revenue forecast responsibility, = 0 otherwise). Standard errors appear inside parentheses. Probability values appear inside brackets.

* $p \leq 0.10$ ** $p \leq 0.05$ *** $p \leq 0.01$.

TABLE SA-3: *Model 1* Alternative Estimation Results
(Only Institutional Venue Results Reported)

	HT-ar(1)	HT	HT-ar(1)*	HT*	CSRE-ar(1)	CSRE	CSRE-ar(1)*	CSRE*	TWFE	TWFE*
Legislative Branch	-3.39** (1.49)	-2.62** (1.00)	-5.81* (3.20)	-3.10 (2.79)	-2.41** (0.80)	-1.80** (0.64)	−0.86 (1.92)	-0.46 (1.63)	-1.64** (0.56)	0.45 (1.53)
Executive Branch	-0.71 (1.59)	-0.47 (1.03)	-3.68 (5.02)	-3.86 (3.98)	0.31 (0.83)	-0.004 (0.66)	1.26 (2.94)	-0.10 (2.42)	0.15 (0.57)	-1.65 (2.21)
Hausman IV Exogeneity Test	______	______	1.19 [0.30]	0.66 [0.52]	______	______	1.45 [0.49]	0.77 [0.68]	______	1.25 [0.29]
H_O: LB = EB	2.31 [0.13]	3.52* [0.06]	0.11 [0.75]	0.02 [0.89]	5.14** [0.02]	5.89** [0.02]	0.32 [0.57]	0.01 [0.91]	7.80** [0.01]	0.55 [0.46]
R^2	0.17	0.17	0.18	0.17	0.07	0.09	0.07	0.09	0.08	0.08
Effective Sample Size	1042	1097	1042	1097	1097	1097	1097	1097	1097	1097

Notes: ***HT-ar(1)****:* Hausman-Taylor/FEVD estimation with AR(1) serial correlation correction; ***HT****:* Hausman-Taylor/FEVD estimation; ***HT-ar(1)*****:* Hausman-Taylor/FEVD estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue* covariates; ***HT*****:* Hausman-Taylor/FEVD estimation and endogeneity correction for *Institutional Venue* covariates; ***CSRE-ar(1)****:* Cross-Sectional Random Effects estimation with AR(1) serial correlation correction; ***CSRE****:* Cross-Sectional Random Effects estimation; ***CSRE-ar(1)*****:* Cross-Sectional Random Effects estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue* covariates; ***CSRE*****:* Cross-Sectional Random Effects estimation and endogeneity correction for *Institutional Venue* covariates; ***TWFE*****:** Timewise Fixed Effects estimation; ***TWFE*:*** Timewise Fixed Effects estimation and endogeneity correction for *Institutional Venue* covariates. Standard errors appear inside parentheses. Probability values appear inside brackets.

* $p \leq 0.10$ ** $p \leq 0.05$ *** $p \leq 0.01$.

TABLE SA-4: *Model 2* Alternative Estimation Results
(Only Institutional Venue Results Reported)

	HT-ar(1)	**HT**	**HT-ar(1)***	**HT***	**CSRE-ar(1)**	**CSRE**	**CSRE-ar(1)***	**CSRE***	**TWFE**	**TWFE***
Legislative Branch	-3.85**	-3.30***	-5.62*	-3.23	-2.38***	-1.79**	0.86	-0.39	-1.67***	0.50
	(1.57)	(1.10)	(3.34)	(2.97)	(0.78)	(0.62)	(1.94)	(1.68)	(0.55)	(1.57)
Executive Branch:	1.95	1.29	5.39	3.76	2.03	2.11**	3.20	3.71	2.16**	3.02
No Term-Limit	(2.62)	(1.77)	(5.00)	(4.02)	(1.28)	(1.01)	(2.84)	(2.42)	(0.89)	(2.18)
Executive Branch:	-4.52*	-4.82***	-10.45*	-12.88**	-1.63	-1.44	-0.66	-2.52	-1.09	-3.34
TL: Lame-Duck	(2.33)	(1.85)	(5.72)	(5.00)	(1.09)	(0.91)	(4.16)	(3.52)	(0.83)	(3.22)
Executive Branch:	-2.48	-2.77	5.90	10.92	-0.66	-0.16	3.55	3.56	-0.34	3.12
TL: No Lame-Duck	(2.51)	(1.97)	(7.75)	(8.29)	(1.06)	(0.87)	(3.91)	(3.24)	(0.78)	(2.86)
Hausman IV	———	———	0.90	1.09	———	———	3.42	3.20	———	1.18
Exogeneity Test			[0.47]	[0.36]			[0.49]	[0.52]		[0.32]
H_O: LB = EBUT	4.95**	6.75***	3.68	1.99	10.55***	13.15***	1.57	2.17	16.33***	0.98
	[0.03]	[0.01]	[0.06]	[0.16]	[0.00]	[0.00]	[0.21]	[0.14]	[0.00]	[0.32]
H_O: LB = EBRTLD	0.10	0.86	0.42	2.36	0.42	0.14	0.00	0.27	0.46	1.02
	[0.76]	[0.35]	[0.52]	[0.13]	[0.52]	[0.71]	[0.97]	[0.61]	[0.50]	[0.31]
H_O: LB = EBRTNLD	0.37	0.09	1.71	2.40	2.19	2.97*	0.92	1.04	2.43	0.57
	[0.55]	[0.76]	[0.19]	[0.23]	[0.14]	[0.09]	[0.34]	[0.31]	[0.12]	[0.45]
H_O: EBUT = EBRTLD	5.43**	9.13***	4.33**	6.10***	5.47***	7.77***	0.53	1.88	8.12***	2.36
	[0.02]	[0.00]	[0.04]	[0.01]	[0.02]	[0.01]	[0.47]	[0.17]	[0.01]	[0.13]
H_O: EBUT =	2.33	3.73*	0.00	0.72	3.06*	3.38*	0.01	0.00	5.16**	0.00
EBRTNLD	[0.13]	[0.05]	[0.95]	[0.40]	[0.08]	[0.07]	[0.94]	[0.95]	[0.02]	[0.98]
H_O: EBRTLD =	2.55	2.98*	2.73*	5.00***	0.67	1.42	0.43	1.32	0.56	1.88
EBRTNLD	[0.11]	[0.08]	[0.10]	[0.03]	[0.42]	[0.23]	[0.51]	[0.25]	[0.46]	[0.17]
R^2	0.16	0.17	0.18	0.18	0.08	0.09	0.08	0.09	0.09	0.09
Effective Sample Size	1042	1097	1042	1097	1097	1097	1097	1097	1097	1097

Notes: ***HT-ar(1)****:* Hausman-Taylor/FEVD estimation with AR(1) serial correlation correction; ***HT****:* Hausman-Taylor/FEVD estimation; ***HT-ar(1)*****:* Hausman-Taylor/FEVD estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; ***HT*****:* Hausman-Taylor/FEVD estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; ***CSRE-ar(1)****:* Cross-Sectional Random Effects estimation with AR(1) serial correlation correction; ***CSRE****:* Cross-Sectional Random Effects estimation; ***CSRE-ar(1)*****:* Cross-Sectional Random Effects estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; ***CSRE*****:* Cross-Sectional Random Effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; ***TWFE*:** Timewise Fixed Effects estimation; ***TWFE***:** Timewise Fixed Effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates. Standard errors appear inside parentheses. Probability values appear inside brackets. * $p \leq 0.10$ ** $p \leq 0.05$ *** $p \leq 0.01$.

TABLE SA-5: *Model 3* Alternative Estimation Results
(Only Institutional Venue Results Reported)

	HT-ar(1)	**HT**	**HT-ar(1)***	**HT***	**CSRE-ar(1)**	**CSRE**	**CSRE-ar(1)***	**CSRE***	**TWFE**	**TWFE***
Legislative Branch: No Term-Limit	-5.45*	-6.47**	2.54	2.80	-2.23***	-1.69***	2.55	2.94**	-1.36**	2.69**
	(2.95)	(2.40)	(2.99)	(2.41)	(0.80)	(0.64)	(1.57)	(1.32)	(0.57)	(1.22)
Legislative Branch: Term-Limit	-6.30*	-7.27**	-11.60	-11.54	-3.44**	-2.72**	-5.23	-3.81	-2.17**	-1.07
	(3.39)	(2.75)	(15.79)	(13.73)	(1.44)	(1.13)	(9.92)	(8.41)	(1.03)	(7.69)
Executive Branch: No Term-Limit	1.54	0.52	4.63	2.84	2.02	2.08**	2.46	2.95	2.21**	2.41
	(2.74)	(1.93)	(7.28)	(4.16)	(1.28)	(1.03)	(2.83)	(2.40)	(0.89)	(2.16)
Executive Branch: TL: Lame-Duck	-4.52**	-4.30**	-7.86	-10.55*	-1.60	-1.45	-2.99	-4.81	-0.95	-5.59*
	(2.28)	(1.84)	(6.58)	(5.58)	(1.09)	(0.92)	(4.42)	(3.71)	(0.83)	(3.39)
Executive Branch: TL: No Lame-Duck	-2.48	-2.27	4.63	9.32	-0.63	-0.15	1.65	1.80	-0.24	1.95
	(2.45)	(1.95)	(7.28)	(7.91)	(1.06)	(0.87)	(3.64)	(2.98)	(0.78)	(2.65)
Hausman IV Exogeneity Test	———	———	0.95	2.16*	———	———	9.43**	11.28**	———	2.54**
			[0.45]	[0.06]			[0.03]	[0.05]		[0.03]
H_O: LBNTL = LBTL	0.29	0.34	0.88	1.15	0.76	0.92	0.57	0.60	0.64	0.04
	[0.59]	[0.56]	[0.35]	[0.28]	[0.38]	[0.34]	[0.45]	[0.44]	[0.42]	[0.84]
H_O: LBNTL = EBUT	4.62**	8.68***	0.20	0.00	9.64***	11.72***	0.00	0.00	13.78***	0.01
	[0.03]	[0.00]	[0.65]	[0.99]	[0.00]	[0.00]	[0.98]	[0.99]	[0.00]	[0.90]
H_O: LBTL = EBUT	4.73**	8.55***	1.17	1.22	9.09***	11.27***	0.59	0.64	11.58***	0.03
	[0.03]	[0.00]	[0.28]	[0.27]	[0.00]	[0.00]	[0.44]	[0.42]	[0.00]	[0.86]
H_O: LBNTL = EBRTLD	0.08	0.88	1.54	3.79*	0.30	0.02	1.14	3.19*	0.22	4.39**
	[0.78]	[0.35]	[0.21]	[0.05]	[0.59]	[0.89]	[0.29]	[0.07]	[0.64]	[0.04]
H_O: LBTL = EBRTLD	0.23	1.21	0.04	0.00	1.17	0.87	0.04	0.01	0.95	0.59
	[0.63]	[0.27]	[0.85]	[0.95]	[0.28]	[0.35]	[0.84]	[0.92]	[0.33]	[0.44]
H_O: LBNTL = EBRTNLD	0.73	2.64	0.07	0.61	1.88	2.59	0.07	0.16	1.67	0.08
	[0.39]	[0.11]	[0.80]	[0.43]	[0.17]	[0.11]	[0.80]	[0.69]	[0.20]	[0.77]
H_O: LBTL = EBRTNLD	1.00	2.96*	0.71	1.37	2.72*	3.58*	0.40	0.37	2.40	0.01
	[0.32]	[0.09]	[0.40]	[0.24]	[0.10]	[0.06]	[0.53]	[0.54]	[0.12]	[0.92]
H_O: EBUT = EBRTLD	4.48**	5.30**	2.05	3.13*	5.35***	7.48***	0.96	2.69*	7.68***	3.46*
	[0.03]	[0.02]	[0.15]	[0.08]	[0.02]	[0.01]	[0.33]	[0.10]	[0.01]	[0.06]
H_O: EBUT = EBRTNLD	1.79	1.74	0.00	0.50	2.97*	3.14*	0.03	0.10	4.95**	0.02
	[0.18]	[0.19]	[0.97]	[0.48]	[0.09]	[0.08]	[0.86]	[0.76]	[0.03]	[0.89]
H_O: EBRTLD = EBRTNLD	2.56	2.96*	1.88	4.27***	0.67	1.47	0.51	1.52	0.50	2.46
	[0.11]	[0.09]	[0.17]	[0.04]	[0.41]	[0.23]	[0.48]	[0.22]	[0.48]	[0.12]
R^2	0.18	0.17	0.16	0.17	0.08	0.09	0.08	0.08	0.09	0.09
Effective Sample Size	1042	1097	1042	1097	1097	1097	1097	1097	1097	1097

Notes: ***HT-ar(1)**:* Hausman-Taylor/FEVD estimation with AR(1) serial correlation correction; ***HT**:* Hausman-Taylor/FEVD estimation; ***HT-ar(1)****:* Hausman-Taylor/FEVD estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; ***HT****:* Hausman-Taylor/FEVD estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; ***CSRE-ar(1)**:* Cross-Sectional Random Effects estimation with AR(1) serial correlation correction; ***CSRE**:* Cross-Sectional Random Effects estimation; ***CSRE-ar(1)****:* Cross-Sectional Random Effects estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; ***CSRE****:* Cross-Sectional Random Effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; ***TWFE*:** Timewise Fixed Effects estimation; ***TWFE**:** Timewise Fixed Effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates. Standard errors appear inside parentheses. Probability values appear inside brackets.

* $p \leq 0.10$ ** $p \leq 0.05$ *** $p \leq 0.01$.

TABLE SA-6: *Model 4* Alternative Estimation Results
(Only Institutional Venue Results Reported)

	HT-ar(1)	HT	HT-ar(1)*	HT*	CSRE-ar(1)	CSRE	CSRE-ar(1)*	CSRE*	TWFE	TWFE*
Legislative Branch: Unified Government	-8.04** (3.25)	-10.00*** (2.79)	-9.13** (4.59)	-9.70** (4.01)	-3.08*** (0.97)	-2.83*** (0.83)	-4.57 (2.59)	-3.24 (2.19)	-2.55*** (0.78)	-2.97 (2.03)
Legislative Branch: Split Branch -- Unified Legislature	-5.41 (3.31)	-7.04** (2.84)	-6.85 (4.95)	-7.69* (4.35)	-1.14 (1.01)	-1.01 (0.86)	-2.81 (2.17)	-1.51 (1.87)	-1.04 (0.81)	-1.88 (1.70)
Legislative Branch: Divided Legislature	-5.64* (2.98)	-7.22*** (2.49)	5.95 (6.75)	4.38 (6.01)	-1.22 (1.04)	-1.43 (0.88)	10.20*** (4.43)	9.88*** (3.84)	-1.28 (0.85)	10.77*** (3.52)
Executive Branch: No Term-Limit	1.30 (2.83)	0.08 (2.18)	6.36 (5.48)	4.48 (4.93)	2.17* (1.23)	2.08** (1.02)	3.77 (2.83)	4.31* (2.42)	2.18** (0.93)	3.60* (2.18)
Executive Branch: TL: Lame-Duck	-4.69** (2.30)	-4.97*** (1.90)	-11.53** (5.61)	-13.59*** (5.15)	-1.34 (1.065)	-1.44 (0.91)	-2.22 (4.19)	-4.05 (3.55)	-1.21 (0.87)	-4.55 (3.24)
Executive Branch: TL: No Lame-Duck	-2.56 (2.47)	-2.83 (2.00)	6.42 (8.09)	10.22 (9.09)	-0.42 (1.03)	-0.18 (0.87)	5.86 (4.05)	5.28 (3.40)	-0.19 (0.82)	5.73* (3.02)
Hausman IV Exogeneity Test	———	———	1.25 [0.28]	2.08* [0.05]	———	———	11.02* [0.09]	12.79** [0.05]	———	2.66** [0.02]
H_O: LBUG = LBSB-UL	4.17** [0.04]	6.36** [0.01]	0.41 [0.52]	0.42 [0.52]	2.75* [0.10]	3.10* [0.08]	0.31 [0.58]	0.42 [0.52]	2.27 [0.13]	0.19 [0.66]
H_O: LBUG = LBDL	3.72** [0.05]	5.94** [0.02]	4.16** [0.04]	4.40** [0.04]	2.96* [0.09]	2.05 [0.15]	6.34** [0.01]	6.72*** [0.01]	1.74 [0.19]	8.73*** [0.00]
H_O: LBSB-UL = LBDL	0.03 [0.87]	0.02 [0.88]	2.99* [0.08]	3.25* [0.07]	0.00 [0.99]	0.16 [0.69]	6.76*** [0.01]	7.05*** [0.01]	0.06 [0.81]	10.47*** [0.00]
H_O: LBUG = EBUT	6.99*** [0.01]	11.86*** [0.00]	4.36** [0.04]	4.35** [0.04]	14.11*** [0.00]	16.45*** [0.00]	4.79** [0.03]	5.43** [0.03]	17.86*** [0.00]	4.94** [0.03]
H_O: LBSB-UL = EBUT	3.61* [0.06]	5.88** [0.02]	3.06* [0.08]	3.03* [0.08]	5.74** [0.02]	6.17** [0.01]	3.52* [0.06]	3.73* [0.05]	7.75*** [0.01]	4.04** [0.05]
H_O: LBDL = EBUT	4.32** [0.04]	7.29*** [0.01]	0.00 [0.96]	0.00 [0.99]	5.71** [0.02]	7.82*** [0.01]	1.67 [0.20]	1.68 [0.20]	8.77*** [0.00]	3.34* [0.07]
H_O: LBUG = EBRTLD	0.88 [0.35]	2.91* [0.09]	0.10 [0.75]	0.37 [0.54]	2.23 [0.14]	1.57 [0.21]	0.23 [0.63]	0.04 [0.85]	1.63 [0.20]	0.17 [0.68]
H_O: LBSB-UL = EBRTLD	0.04 [0.84]	0.48 [0.49]	0.37 [0.54]	0.83 [0.36]	0.00 [0.99]	0.14 [0.70]	0.02 [0.90]	0.39 [0.53]	0.03 [0.87]	0.51 [0.48]
H_O: LBDL = EBRTLD	0.08 [0.78]	0.68 [0.41]	3.36* [0.07]	4.68** [0.03]	0.00 [0.99]	0.00 [0.99]	3.45* [0.06]	5.97** [0.02]	0.00 [0.95]	8.70*** [0.00]
H_O: LBUG = EBRTNLD	2.18 [0.14]	5.37** [0.02]	2.15 [0.14]	3.21* [0.07]	4.96** [0.03]	5.87** [0.02]	4.31** [0.04]	3.99** [0.05]	5.24** [0.02]	5.14** [0.02]

H_O: LBSB-UL = EBRTNLD	0.58 [0.45]	1.81 [0.18]	1.54 [0.22]	2.55 [0.11]	0.52 [0.47]	0.54 [0.46]	2.93* [0.09]	2.47 [0.12]	0.63 [0.43]	3.82* [0.05]
H_O: LBDL = EBRTNLD	0.77 [0.38]	2.35 [0.13]	0.00 [0.97]	0.24 [0.63]	0.53 [0.47]	1.22 [0.27]	0.64 [0.42]	0.99 [0.32]	1.04 [0.31]	1.46 [0.23]
H_O: EBUT = EBRTLD	4.16** [0.04]	4.72** [0.03]	5.15** [0.02]	5.61** [0.02]	5.42** [0.02]	7.54*** [0.01]	1.25 [0.26]	3.32* [0.07]	8.07*** [0.00]	3.82* [0.04]
H_O: EBUT = EBRTNLD	1.58 [0.21]	1.44 [0.23]	0.00 [0.99]	0.36 [0.55]	3.03* [0.08]	3.28* [0.07]	0.20 [0.65]	0.06 [0.80]	4.21** [0.04]	0.38 [0.54]
H_O: EBRTLD = EBRTNLD	2.78* [0.10]	3.20* [0.07]	3.27* [0.07]	4.38** [0.04]	0.67 [0.41]	1.39 [0.24]	1.48 [0.22]	2.87* [0.09]	0.96 [0.33]	4.39** [0.04]
R^2	0.17	0.18	0.17	0.19	0.08	0.09	0.09	0.11	0.09	0.10
Effective Sample Size	1042	1097	1042	1097	1097	1097	1097	1097	1097	1097

Notes: ***HT-ar(1)****:* Hausman-Taylor/FEVD estimation with AR(1) serial correlation correction; ***HT****:* Hausman-Taylor/FEVD estimation; ***HT-ar(1)******:* Hausman-Taylor/FEVD estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; ***HT******:* Hausman-Taylor/FEVD estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; ***CSRE-ar(1)****:* Cross-Sectional Random Effects estimation with AR(1) serial correlation correction; ***CSRE****:* Cross-Sectional Random Effects estimation; ***CSRE-ar(1)******:* Cross-Sectional Random Effects estimation with AR(1) serial correlation correction and endogeneity correction for *Institutional Venue-Political Context* covariates; ***CSRE******:* Cross-Sectional Random Effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates; ***TWFE*****:** Timewise Fixed Effects estimation; ***TWFE******:** Timewise Fixed Effects estimation and endogeneity correction for *Institutional Venue-Political Context* covariates. Standard errors appear inside parentheses. Probability values appear inside brackets.

* $p \leq 0.10$ ** $p \leq 0.05$ *** $p \leq 0.01$.