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Exploring Causality between Public Opinion and the Death Penalty Using Granger Testing

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Abstract

The relationship between public opinion and the death penalty has been explored in depth, but understanding the causal relationships in the death penalty opinion-policy nexus has been limited by adherence to cross-sectional studies. This study explores the causal direction between public opinion and various death penalty legislative actions and consequences (sentencing outcomes, standing legislation, and new/provisional legislative acts) using Granger-causal testing. The results of the tests suggest that public opinion is influenced by legislative acts more than it influences them. These findings support the idea that policy tends to drive public punitiveness rather than the other way around. Recommendations for future research include conducting additional temporal causal tests with larger datasets, with a focus on electoral accountability, and with a wider set of socio-economic variables.

Keywords

Public Opinion, Death Penalty, Electoral Accountability, Capital Punishment, Causal Relationships, Granger Tests, Legislative Actions

"From the beginning, the study of relationships between public opinion and policy has been vexed by knotty, frustrating problems of causal inference. When opinion and policy correspond, it is extremely difficult to sort out whether public opinion has influenced policy, or policy has influenced opinion, or there has been some mixture of reciprocal processes." (Page, 1994: 26)

1. Introduction

1.1. Lack of Time Series Analysis in Public Opinion Research

While ample evidence suggests there is a relationship between death penalty politics and public opinion (Sevenans, 2021; McGregor, 2019; Pickett, 2019; Perrin & McFarland, 2011; Shapiro, 2011; Soroka & Wlezien, 2010; Wlezien & Soroka, 2009; Brace & Boyea, 2008; Burstein, 2003; Monroe, 1998), stubborn adherence to cross-sectional research has limited our understanding of that relationship (Chen et al., 2021; Pickett, 2019; Hofman, Amit, & Watts, 2017). Temporal-causal modeling has only rarely figured in opinion-policy research (Hakhverdian, 2012; Toshkov, 2011; Tan & Weaver, 2010; Nicholson-Crotty, Peterson, & Ramirez, 2009; Soroka, 2002; Hartley & Russett, 1992), and we have been unable to identify a study that has employed such methods on the relationship between public opinion and death penalty legislation specifically. By so observing, we do not wish to portray opinion-policy research as unique in its adherence to mostly cross-sectional research, nor as non-pluralist in its approach or methods outside of that limitation, rather that the neglect of temporal causal tools evinces a larger trend across the non-economic social sciences (Chen et al., 2021; Keuschnigg, Lovsjö, & Hedström, 2018; Hofman, Amit, & Watts, 2017; Blyth, 2009; Cohen, 2008; Aldridge, 1999).

A full treatment of the reasons for the neglect of Granger regression would not be parsimonious. Nevertheless, we outline it here only to contextualize the present study: 1) The marginalist revolution of economics of the 1860s and the associated schism between more "precise" and "pure" analytical tools being employed in economics disconnected more heavily analytical tools developed there from the rest of the social sciences (Blyth, 2009; Cohen, 2008); 2) Historical computing constraints previously disincentivized the use of analytically intensive methods outside of the neoclassical side of social science disciplines (i.e. outside of neoclassical economics where the marginalist revolution made analytical intensity a cultural-disciplinary priority) (Chen et al., 2021; Blyth, 2009; Cohen, 2008); 3) A lack of standardization of causal-predictive approaches across the non-economic social sciences (Hofman, Amit, & Watts, 2017); 4) A failure in the non-economic social sciences to recognize the role of causal-predictive approaches as complementary instead of substitutive relative to other methods (Hofman, Amit, & Watts, 2017); 5) general cultural-disciplinary reticence in non-economic social sciences to engage in predictive endeavors with causation as a foundation (Chen et al., 2021; Hofman, Amit, & Watts, 2017).

Despite these historical and cultural impediments, the last decade has brought increased attention to the use of forecasting, temporal causation, and computational tools in general to non-economic social science disciplines and has been described as a key to progressing those disciplines (Keuschnigg, Lovsjö, & Hedström, 2018). As innovative as this may feel, it is in some ways a return to some of the earliest notions about social science, for example, Auguste Comte's formula of "*Savoir pour prévoir et prévoir pour pouvoir*" (Aldridge, 1999). We argue that, by extension, a neglect of these tools will stall sociology, political science, and the other non-economic social sciences (Lindstedt, 2019).

Noting this gap in applying temporal causal methods outside economics, this paper attempts a modest contribution toward addressing that deficiency by applying what is, to our knowledge, the first temporal-causal analysis of public opinion and the death penalty. Using information from the Death Penalty Information Center and General Social Survey, we created a data set examining public punitive opinion and death-penalty legislation and outcomes in the United States between 1974 and 2021.

1.2. Theoretical Conceptualization

The conceptual orientation of this research is primarily socio-economic and draws upon elements of the social-morphogenetic approach, socio-spatial signaling, and bounded rationality. Under this lens, we take public opinion and death penalty outcomes, respectively, as analytical dualism (Archer, 2010), bidirectional signals (Eskridge Jr., 1994; Ten Eyck & Christensen, 2012) that are "ceaseless and essential both to the continuation and further elaboration of the system (with) subsequent interaction (being) different from earlier action because (it is) conditioned by the structural consequences of that prior action" (Archer, 2010: 228). We also conceptualize collective opinion and policy signals as resistant to goal trade-offs and therefore stable unless the collective calculus of costs and benefits is imbalanced sufficiently for one to mobilize the other. This can also be conceptualized as a collective-level extension of the endowment effect (Kahneman, Knetsch, & Thaler, 1991), constructing collectives as requiring a lower-than-expected (under rational assumptions) cost-benefit ratio to abandon the currently held orientation than to adopt an equally net-beneficial new one.

The causal pathways we propose are depicted in **Figure 1**.

The current body of (cross-sectional) research is inconclusive about the causal direction of these relationships, or whether there is a causal relationship at all. Varyingly, these studies have posited that: opinion does have an impact on sentencing outcomes (Pickett, 2019), that executions decrease support for the death penalty (Jacobs & Kent 2007 in (Pickett, 2019)), that executions do not change

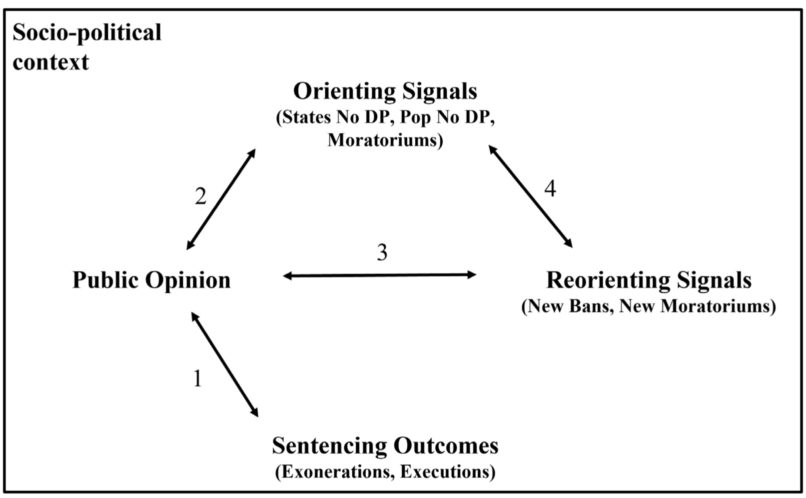

Figure 1. Conceptualization of morphogenetic collective signals.

support for the death penalty ((Norrander, 2000) in (Pickett, 2019)), that opinion constrains elected officials through electoral accountability (Shapiro, 2011), that some representatives are not informed relative to their constituents' opinions (although those who are may be more likely to respond to it) (Butler & Nickerson, 2011), that opinion can be mobilized to constrain policymakers (Baum & Potter, 2019; Druckman & Leeper, 2012; Shapiro, 2011; Jones, 2002; Kiser, 1999), that novel stimuli can act as motivating signals which destabilize weaker opinion or reinforce stronger opinion (Druckman & Leeper, 2012), and that it is doubtful there is a causal relationship at all (Beckett & Sasson, 2004; Roberts et al., 2003; Zimring & Johnson, 2006 in (Pickett, 2019)). Especially emergent from this plurality of literature (and of interest to this study) is the question of whether public opinion drives policy or if elites use policy to drive public opinion. Under current scholarship, the answer to this question has been obscured through adherence to cross sectional research as noted by Pickett (2019). To elucidate the temporal causality between public opinion and policy, we require new methods not previously embraced by opinion-policy scholarship generally.

2. Method

2.1. Dataset

To test these relationships, we gathered data from the Death Penalty Information Center and General Social Survey, which together contained the variables needed to test the temporal-causal relationships described in our theoretical framework. The Opinion variable was coded as percent opposed to the death penalty, in response to the question: *Do you favor or oppose the death penalty for persons convicted of murder* (Smith, 2019)? All death-penalty variables came from the Death Penalty Information Center.

2.2. Imputation, Transformation, and Covariate Stationarity Testing

Due to irregular data collection and reporting, missing values were imputed using an iterative Markov Chain Monte Carlo method, Fully Conditional Specification (Van Buuren, Brand, Groothuis-Oudshoorn, & Rubin, 2006), with the fifth iteration retained.

Variables were tested for stationarity using both the Augmented Dickey-Fuller (ADF) (Mushtaq, 2011) and Kwiatkowski–Phillips–Schmidt–Shin (KPSS) (Kokoszka & Young, 2016; Hobijn, Franses, & Ooms, 2004) tests for stationarity. Any that were not stationary were transformed in the following priority order: 1) First-order differencing; 2) Three-year moving average; 3) Second-order differencing; 4) First-order difference of three-year moving average. Variables that were not stationary with any of these transformations were retained in the most-stationary transformation and checked for covariate stationarity in the combinations in which they would be Granger tested. **Table 1** displays descriptive statistics for raw variables and **Table 2** for transformed variables. While not all

Table 1. Descriptive statistics (untransformed).

	N	Mean	SD
Opinion	31	29.25	6.00
Sentencing Outcomes			
Exonerations	32	3.34	2.41
Executions	29	27.79	22.52
Orienting Signals			
Population No Death Penalty	33	68.90	36.59
States No Death Penalty	33	16.09	4.68
State Moratoriums	33	0.64	1.10
Reorienting Signals			
New Bans	31	1.10	4.41
New Moratoriums	32	0.09	0.29

Table 2. Descriptive statistics of transformed variables.

	Min	Max	Mean	SD	Skew	Kurtosis
Opinion (D2)	−5.11	90.93	28.53	14.88	1.27	6.31
Exonerations	0.00	9.00	3.77	2.61	0.11	−1.13
Executions (D1)	−211.37	102.50	−18.40	61.62	−0.88	2.63
Pop No DP (MA, D1)	−135.41	259.50	−2.06	63.11	1.69	6.01
States No DP (D1)	−33.17	17.23	−6.83	9.07	−0.38	1.27
State Moratoriums (D1)	−4.73	2.81	−0.85	1.60	−0.81	0.48
New Bans (D1)	−7.08	3.00	−0.82	2.31	−1.15	1.09
New Moratoriums	0.00	1.00	0.42	0.50	0.35	−1.96

variables were individually stationary (**Table 3**), Augmented Dickey-Fuller tests demonstrated covariate stationarity for all transformed variables in **Table 2** in the combinations in which they would be Granger tested, with $p < 0.01$ for all tests.

2.3. Granger-Based Temporal Causal Modeling

Before proceeding to a presentation of the test results, we first briefly discuss Granger-based temporal causal modeling, the analytical method of this study. Temporal causal modeling (TCM) is a technique based on Granger testing (Granger, 1969) that designates variables as targets, inputs, or candidate targets and inputs, then identifies top model systems using fit metrics such as R^2, AIC, and BIC. As Granger regression is the basis of these tests, we make a few notes on that form of analysis as well. Pierce (1977) summarized Granger testing by observing that, "a variable X 'causes' another variable Y if Y can be better predicted by from the past values of X and Y together than from the past of Y alone,

Table 3. Stationarity tests of transformed variables.

	Stationarity test (p-value)	Truncation lag
Opinion (D2)	ADF (0.01)	3
Exonerations	ADF (0.01)	3
Executions (D1)	ADF (0.04)	3
Pop No DP (MA, D1)	ADF (0.30)	3
States No DP (D1)	KPSS (0.01)	3
State Moratoriums (D1)	KPSS (0.05[a])	3
New Bans (D1)	ADF (0.30)	3
New Moratoriums	KPSS (0.01)	3

a. Unrounded value = 0.48.

other relevant information also being used in the predictions" (p. 11).

In a slightly more technical presentation of the method, Arnold, Liu, & Abe (2007) proffered, "x is said to 'Granger cause' another time series y, if and only if regressing for y in terms of both past values of y and x is statistically significantly more accurate than doing so with past values of y only" (p. 68). The mathematical specifications of Granger testing are as follows.

"Let $\{x_t\}_{t-1}^{T}$ *be lagged variables of x and* $\{y_t\}_{t-1}^{T}$ *for y, and let* $\overrightarrow{x_t}$ *denote, in general, the vector* $\{x_t\}_{t-1}^{T}$. *Then, the Granger test is performed by first conducting the following regressions:*

$$yt \approx A \cdot \overrightarrow{y_{t-1}} + B \cdot \overrightarrow{y_{t-1}} \quad (1)$$

$$yt \approx A \cdot \overrightarrow{y_{t-1}} \quad (2)$$

and then applying an F-test (*or some other similar test*) *to obtain a p-value for whether or not* (1) *results in a better regression model than with* (2) *with statistically significant advantage* (Arnold, Liu, & Abe, 2007). *It should also be noted that x is only said to cause y if x and y are more statistically significant predictors of y than of x* (Arnold, Liu, & Abe, 2007: 68)."

The transformed variables were tested in two phases: first in pairs with each death penalty variable against the public opinion variable (with both variables in each pairing specified as candidate input and target variables), then as a Granger-based temporal causal model (TCM) with all variables along the orienting signal/public opinion/reorienting signal path (2-3-4 in **Figure 1**) to quantify relative causal strength of path 2 compared to 3-4 and 3 compared to 2-4 (the direct and indirect paths through which public opinion and legislation might impact each other). Hereafter, the former will be termed "pairwise" testing and the latter "multivariate" or temporal causal model testing, even though all models were run in SPSS using temporal causal model forecasting with Year identified as the period field.

Running pairwise and multivariate models provided additional insight into the robustness of significant causes by treating the pairs as subsets within multivariate models and observing which pairwise Granger causes remain in the presence of additional variables. To put it differently, a significant relationship in pairs testing may not be significant in a larger model, indicating that other variables negate the predictive power of that variable in a larger system of predictors. This test design was guided by the idea of evidential pluralism, which has been advocated for temporal causal research (Moneta & Russo, 2014). While we hope future studies will challenge or confirm our findings, providing *between-study* evidential pluralism, we hoped that exploring multiple models would provide a sort of *within-study* effect as well. Given that testing variables in multiple combinations might raise questions about which is most valid, and noting that this study aims to contribute to the knowledge of causal relationships beyond the data in our models, we took as a maxim that attention to AIC and BIC fit measures would provide more helpful information than R^2 measures due to their ability to speak to fit beyond the present data and models.

3. Results

Statistically significant results from the pairwise and TCM tests are reported in **Table 4**.

Table 4. Statistically significant Granger effects between opinion and legislative signals pw = pairwise test, TCM = temporal causal test of the 2-3-4 pathway, p < 0.1 + 0.05* 0.01** 0.001***.

	Full model metrics			Pair metrics	
	AIC	BIC	R^2	β (p)	Lag
Sentencing Outcomes					
Exonerations → Opinion (pw)	89.45	108.83	0.33	−2.14 (0.046)*	1
Orienting Signals					
Pop No DP → Opinion (pw)	244.63	264.01	0.83	0.10 (0.023)*	2
Opinion → States No DP (pw)	187.58	206.96	0.45	0.25 (0.091)+	3
Moratoriums → Opinion (pw)	241.00	260.37	0.85	−3.79 (0.029)*	1
Moratoriums → Opinion (pw)	241.00	260.37	0.85	5.40 (0.006)**	2
Moratoriums → Opinion (pw)	241.00	260.37	0.85	−4.19 (0.030)*	3
Reorienting Signals					
New Bans → Opinion (TCM)	240.68	286.47	0.92	−4.88 (0.015)***	2
Causation Between Orienting and Reorienting Signals					
StatesNoDP → NewMoratoriums (TCM)	−70.56	−24.77	0.77	−0.05 (0.001)**	1
StatesNoDP → NewMoratoriums (TCM)	−70.56	−24.77	0.77	0.05 (0.020)*	2

While the paired models can be presented straightforwardly through tables, impact diagrams assist in visualizing larger temporal causal models. Figure 2 presents the impact diagram of the model with the lowest BIC score as an illustration of the 2-3-4 path indicated earlier in Figure 1.

Thicker arrows represent more statistically significant causal effects. The statistically significant path from States No DP to New Moratoriums appeared earlier in the table. However, when showing the relationships at three levels, State Moratoriums was revealed as a statistically significant cause of Opinion. What is notable is that, among statistically significant causes in this system, there is a broken path: from State Moratoriums to Opinion, then from States No DP to New Moratoriums. To summarize, the impact diagram illustrates two key findings: a death penalty variable as a Granger cause of opinion (rather than the other way around) and a broken causal path in a low-BIC larger temporal causal model.

3.1. RQ 1: What Is the Causal Relationship between Sentencing Outcomes and Opinion?

Exonerations Granger caused Opinion in lag 1, and the association was negative, indicating that an increase in exonerations reduces opposition to the death penalty. There was no statistically significant Granger-causal effect between Executions and Opinion.

3.2. RQ 2: What Is the Causal Relationship between Orienting Signals and Opinion?

When considering the transformed variables, changes in the number of people living in restrictionist states had a small Granger-causal effect on public opinion over two years, and opinion had a small positive effect on the number of states that prohibit capital punishment over three years. These imply, respectively, that

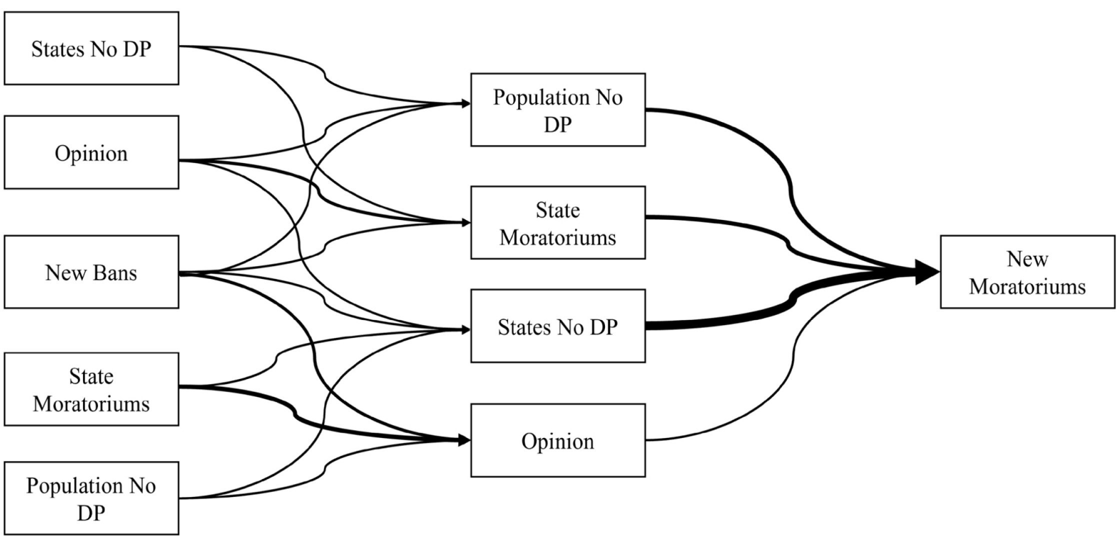

Figure 2. Impact diagram of the larger temporal causal model with New Moratoriums as target variable.

orienting signals (such as the share of the population in restrictionist states) do impact public opinion, and that opinion does have the capacity to constrain legislative action. However, this was limited to one lag, and this was the only statistically significant instance of opinion impacting death penalty outcomes (rather than the other way around), as will be discussed later.

Given the transformed variables, Moratoriums was a strong and robust predictor (across three lags) Granger cause of Opinion, but with alternating coefficients. The transformation of both variables makes straightforward interpretations somewhat challenging, but the results indicate that changes in the number of moratoriums are useful in forecasting future changes in the percentage of the population that oppose the death penalty.

3.3. RQ 3: What Is the Causal Relationship between Reorienting Signals and Opinion?

There were no statistically significant Granger-causal relationships between Reorienting Signal variables and the Opinion variable in any pairwise Granger test. Granger-based TCM testing was conducted with all variables on the 2-3-4 pathway (**Figure 1**). All were designated as candidate input and target variables and iterative Granger-causal testing determined which combination of inputs and targets maximized model fit (Arnold, Liu, & Abe, 2007). In this model, the first-order difference of New Bans had a statistically significant negative Granger effect on the second-order difference of those who opposed capital punishment. Noting that this relationship was not present in pairwise Granger testing, this result is present with caution, emphasizing that it was only present when considering a system of variables on the 2-3-4 pathway.

3.4. RQ 4, Comparison of Pairwise and Multivariate Models

Seeing that the comparison between models yields additional information about the causal mechanisms between public punitiveness and legislative signals, we compare model differences to observe the robustness of pairwise effects.

In paired models, several capital-punishment signals were Granger causes of opinion, however when the reverse was observed, opinion was rarely a Granger cause, marginally predicting only one variable, changes in States No DP.

When considering the top models among systems of variables on the 2-3-4 path, Granger effects were only present for two relationships: New Bans as a Granger cause of Opinion and States No DP as a Granger cause of New Moratoriums. The model in which the latter relationship emerged had low AIC and BIC numbers, suggesting that it is a powerful model in terms of its ability to forecast beyond the data.

Considering all models, what is robust is that: 1) Public opinion is constrained by legislative signals more than the other way around and 2) New moratoriums are impacted by precedent (number of states without the death penalty) rather than opinion.

4. Discussion

4.1. Emergent Framework

The results of the Granger tests provide a first look at the temporal causal relationships between public opinion and death penalty legislation and outcomes. The results from this exploratory study tip the debate about whether opinion constrains or shapes legislation in favor of the latter, except in the case of electoral accountability. These findings support observations by Shapiro (2011) in specific and, more generally, seasoned sociological notions of the power elite (Mills, 1958) in the sense that the masses are shaped by powerful structures more than they shape them.

Considering these results, we propose the following framework revision as depicted in **Figure 3**.

Before exploring practical applications, this subsection elaborates on the way data results impacted the proposed theoretical revision given in **Figure 3**. In keeping with the exploratory nature of this study, we consider the results of all models hoping to provide a broader base for evidential pluralism (Moneta & Russo, 2014).

We now summarize the results from which the proposed causal paths in this framework were derived.

1) Consistent with the nomenclature of our initial framework (**Figure 1**), the number of people living under capital punishment prohibitions and the number of moratoriums appear to orient public opinion, though only mildly in the case of the latter.

2) Changes in the number of bans and the number of exonerations appear to shift public opinion as well, and may appropriately be thought of as reorienting signals, also in keeping with our initial framework.

3) A major change to the initial conceptualization is that changes in the number of states without the death penalty do not fall under the same causal direction as the otherorienting signals as originally depicted in **Figure 1**. It should be noted that the statistical strength of Opinion → States No DP was weak at 0.091 with a 95% confidence interval spanning across zero (−0.04, 0.53), indicating a

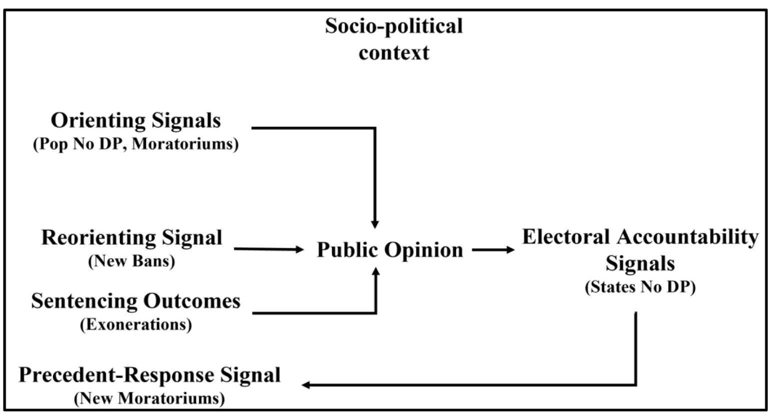

Figure 3. Emergent theoretical framework.

weaker predictor. However, seeing also that the (marginally) statistically significant effect is lagged three years, it may be possible to conceptualize this relationship as a manifestation of *electoral accountability* as noted in the literature (Shapiro, 2011). Bringing the pairwise findings together, it might be tempting to posit that changes to the population living under death penalty prohibitions mobilize opinion, and opinion constrains elected representatives more during election cycles (every two years in the case of midterm elections), with the legislation taking effect the following year. Notably, this effect existed in the pairwise but not the multivariate models, emphasizing the need for further testing to determine the robustness of the effect in the presence of different variables.

4) When considering multivariate temporal causal models, precedent appeared to constrain elected officials more than opinion, as evidenced by the number of states without the death penalty as a more significant cause than opinion. Though opinion was a marginally statistically significant cause in one pairwise model, it was not a statistically significant cause of any death penalty legislation in the multivariate temporal causal models.

5) In this study, evidence supports the idea that public officials are constrained more by precedent than opinion, as supported by the Granger relationship between States No DP and New Moratoriums. While there was no direct Granger-causal path between Opinion and New Moratoriums, there was a strong causal path between the number of states without the death penalty and new moratoriums, supporting the idea that elected officials take cues from preeminent standing legislation nationwide than the prevailing public punitiveness.

6) The impact diagram in the temporal causal model with the lowest BIC revealed a broken path between variables, suggesting the need to cast a wider net in the inclusion of socio-political variables in future studies.

Taken together, these findings suggest an opportunity for evidential pluralism as future studies either support or challenge the idea that, with the possible exception of an electoral effect, opinion does not appear to constrain policymakers, but policy does appear to constrain opinion in various ways.

4.2. Practical Applications

This evidence that favors the idea that the role of opinion in democratic contexts may be limited mostly to electoral accountability (Shapiro, 2011) tips the debate about whether politicians respond to or attempt to drive public opinion (Sevenans, 2021) in favor of Baum & Potter (2019: i) when they noted: "Democratic publics have always been at a disadvantage when it comes to constraining their elected leaders' independent foreign policy preferences," and that the nature of policy-making can lead "to information asymmetries that disadvantage average citizens in favor of governing elites".

If borne out in future studies, this has several applications for social movements, special interest groups, political action committees, politicians, campaigns, judges, and everyday civilians because these findings illustrate that, even in democratic contexts, the power to constrain legislators is limited for the average person. By extension, the boundary critique of the opinion-policy relationship might be evaluated by all parties: How is democracy being framed across ideologies? What values are suggested by those frames? And, especially, what underlies the gap between idealized and realist notions of democracy.

5. Conclusion

5.1. Summary of Findings

Employing Granger-causal tests to the question of public punitiveness revealed support for the idea that high-powered individuals in the United States drive opinion more than they are constrained by it. The exception to this, as evidenced in the test results, was the electoral accountability effect, with public opinion Granger causing a change in the number of states without the death penalty in a three-year cycle (more accurately, the effect was present with three lags but not one, two, four, or five). There was also a causal relationship between the number of states without the death penalty and new moratoriums but not between opinion and new moratoriums, supporting the idea that precedent (in the form of standing legislation and its consequences) plays a more active role in provisional legislation than does opinion. What was most revelatory to us overall was that, while a considerable faction of cross-sectional literature suggests that opinion plays an important role in constraining legislation, we observed opinion as a temporal causal effect in only one bivariate model, and the statistical power was marginal. This supports another faction of literature that has doubted that there is a causal effect (Beckett & Sasson, 2004; Roberts et al., 2003; Zimring & Johnson, 2006 in (Pickett, 2019)) beyond electoral accountability (Shapiro, 2011).

5.2. Limitations

This study had a number of limitations that may have impacted the results. Due to the (lack of) availability of more frequent death penalty information used in the study and the relative infrequency of legislative action, there was a moderately high percentage of missing data points (33.9%). Even with transformations, Opinion and Pop No DP had higher-than-optimal kurtosis levels, though some scholars have proposed values in the range of −7, 7 as acceptable (Demir, 2022). The overall number of observations in the study is low as well due to the irregular and infrequent reporting of data points and the generally irregular and infrequent nature of legislative action. Even with annual data and legislative changes, it would take a hundred years to get as many data points in the dataset. Innovation in obtaining a dataset with more points would likely yield more statistical power and robust findings. Additionally, we endeavored to keep our analysis and, consequently, our interpretations focused on nation-level collective signals. Future studies might explore causal effects at different scales of organizations: community action groups, (super) political action committees, social movements, state-level (vs. national) trends, as well as causal networks in different interpersonal groups (from everyday citizens to elected officials and other elites). Finally, we limited our study to variables directly related to a direct opinion-policy-outcome line. Replicating the analyses here with a wider net of variables including the impact of crime rates (Pickett, 2019) and broader socio-political variables may prove a fruitful future direction for studies on public punitiveness.

Overall, we take the findings of this study as indicating considerable promise for the inclusion of more temporal causal methodology in the future of opinion-policy research and in helping to flesh out the mechanisms at play in a broader system of policy and punitiveness.

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Conflicts of Interest

The authors declare no conflicts of interest regarding the publication of this paper.

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