

Retrospective Voting in the Pre-Modern World: The Case of Natural Disasters in the Roman
Republic

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Political scientists frequently debate how “blind” retrospective voting is in the modern world. We test these arguments in a radically different time and place: the Roman Republic. We then leverage the exogenous nature of river flooding as well as the random assignment to office location in the Roman Republic to estimate the causal effect of flooding on those randomly assigned to be in the city when floods occurred. Analyzing 272 second-tier executive officials in the Republic, we find that significant flooding substantially increased the ability of those randomly placed in the city to achieve the Republic’s top executive office in the near future. Among those randomly assigned to be outside of the city, we find no evidence of any effect on short-term career advancement. These results indicate that floods were important opportunities to perform for voters, with no evidence of “blind” punishment for natural disasters.

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The retrospective nature of voter behavior is a recurring subject in political science. In recent years, scholars have debated the extent to which modern voters are retrospective and even “blindly” retrospective, punishing officials for unavoidable events such as natural disasters. Thus far, these papers have been limited to the modern context (overwhelmingly in America) and feature exogenous assignment only to the flooding itself, not the ability to respond – an important secondary feature of disasters. In this paper, we use a novel empirical setting – the middle Roman Republic – to obtain plausibly exogenous assignment to both the disaster and the capacity to respond. We leverage the exogenous nature of floods as well as the random assignment to location of service of Republican officials to estimate the causal effect of both disaster *and* the opportunity to respond. Analyzing the subsequent political careers of Roman praetors – second-tier executive officials – between 218 and 166 B.C.E., we find no evidence of “blind retrospection” by Roman voters. Instead, we find that floods were opportunities for very visible performance in front of Roman voters for those randomly assigned to be in the city rather than the provinces. In flood years, city-based praetors enjoyed a 45 percentage-point increase in the likelihood of obtaining the Republic’s chief political office within five years compared to non-city praetors.

In addition to addressing the causality issue, we also move the study of retrospective voting behavior into a new pre-modern context. Few, if any, people ever made as many collective voting decisions on their high-level state executives as the electorate of the Roman Republic. Voting-eligible Roman citizens elected multiple executives every year for almost five centuries. Roman elections were not entirely dissimilar to our own, featuring candidates and campaigns, electioneering, campaign money, family dynasties, and sloganized advertisements plastered to walls. It stands to reason that if there are near-universal aspects of human elections,

they should be found in classical Rome as well. Yet, we have little systematic empirical knowledge of how these elections worked, especially in light of theories developed about electoral behavior in the modern political science literature. We contribute to this understanding by taking a modern test from the literature and applying it to this pre-modern context.

Voting and Floods

Floods have become a “cottage industry” in political science, motivated by scholars’ desire to test several theories of voters’ choices. One theory, articulated by Achen and Bartels (2016) posits that voters are “blindly retrospective,” unable to observe and evaluate official behavior, to connect behavior to outcomes, or reasonably differentiate between things an official can and cannot influence. The result is “irrational” punishing and rewarding for (dis)contentment unrelated to job performance. While Achen and Bartels’s (2002, 2016) most infamous example is anti-incumbent responses to shark attacks (but see Fowler and Hall 2018a, but see Achen and Bartels 2018, but see Fowler and Hall 2018b), a more expansive test concerns the impacts of droughts and floods. These events cause substantial destruction, but are largely out of the control of any official. Achen and Bartels find that incumbents are punished for these events in ways that would not correspond to “rational” candidate evaluations (but see Gailmard and Patty 2019 and Ashworth et al 2018).

Other researchers advance a different perspective, that voters are at least capable of discerning response to disasters, and rewarding those who appear to “do something” about them. Rather than blindly punishing when hurt, voters see and reward those who help them. Examples include Healy and Malhotra (2009, 2010), Gasper and Reeves (2011), Bechtel and Hainmueller (2011), Fair et al (2017), and Heersink et al (2017). Others still argue that it is not the disaster or response that matter, but the symbols and narratives that develop around them (see the overview

in Rubin 2018). The results in this literature vary across time and place of analysis. The most recent and exhaustive analyses imply a negative impact of flooding on incumbent success, but one that can be mitigated. In some contexts, this mitigation can come to exceed the penalty of flooding, giving officials the opportunity to perform for voters and “credit claim” (Bechtel and Hainmueller 2011). Yet these results sometimes fail to replicate in new contexts and are sensitive to modeling choices (Bovan et al 2018). In the American context, this mitigation often falls short (Heersink, Peterson, and Jenkins 2017).

The study of elections and individual voting behavior has a rightful place as one of the most significant pursuits of political science. Yet, research has been overwhelmingly focused on the modern, Post-War period. The advent of modern polling, programs like the American National Election Studies, and the recent availability of granular election data make electoral behavior before the 1960s seem like a sparsely charted wilderness. Yet, elections are not recent phenomena. Americans have conducted elections for centuries, and humans for millennia. Indeed, the mechanics of popular selection were likely operative even in the earliest human societies. Theories of “voting behavior,” inductively formed by observing the behavior of late 20th century Americans, and then empirically tested on the behavior of early 21st century Americans, may be narrower in scope than is regularly appreciated. Testing these theories in other times and places helps reveal to what extent these theories capturing something timeless about human voters rather than something specific about the modern, American voter.

Defining an Empirical Test in the Roman Republic

Given that it is difficult to obtain detailed, computationally useful election results from Kansas in 1986, it should be unsurprising that the whims of history have not left for posterity voluminous details on the voting behavior of Roman citizens 2,200 years ago. However, we

identify one period in which sufficient data are available: the years 218-166 B.C.E., which we call the “Livy Years,” as they are detailed in the extant books of Titus Livy.

For the Livy Years, starting with the Second Punic War and ending in 166 B.C.E., we know four crucial sets of facts that enable empirical analysis. First, we know (almost perfectly) the set of people who were praetors of the Roman Republic. Praetors were the second-tier office holders of the Roman Republic, which elected four to six every year during this period. The praetorship was an important office because it conferred on its holder *imperium* – the power to command, both militarily and civically. Though a praetor’s normal task was something loosely comparable to a judge, the position was incredibly flexible because of this underlying grant of *imperium* (Brennan 2000). This forms our unit of analysis: 272 individual praetorships. Second, for each praetor, we know if (and when) they became one of Rome’s chief executives, the consuls. In the Republic, voters elected two consuls annually, almost exclusively from candidates who had previously served as praetors. This becomes our dichotomous dependent variable: whether each praetor **Became Consul** within five years, generally the amount of time within which a politically successful praetor would become consul. The underlying math of the Republican Constitution – four to six praetors and only two consuls each year – created the variation we need for analysis. Fewer than half of all praetors ever became consul.

Third, we know the jurisdiction of each praetor. Each praetor was allotted a region over which to exercise their *imperium* from a set of locations important to the Republic at the time. These included Sardinia, Sicily, and Gaul, among others. Specifically, we focus on the **City Praetors** – the *praetor urbanus* and the *praetor inter peregrinos* – as one of our independent dummy variables, taking the value “1” when the praetor was a city praetor, and “0” otherwise. The *praetor urbanus* was responsible for legal disputes in the city between Romans, while the

praetor inter peregrinos was responsible for disputes involving foreigners.¹ Unlike other praetors, who would take their position and leave the city, not to be seen again by voters until the end of the year, the two city-based praetors would, in fact, spend a considerable amount of time as the most senior officials in town: their only superiors – the two consuls – were consistently out of the city fighting the Republic’s many wars in almost every year of our time period. These positions were perceived to be more valuable than other positions (Brennan 2000). They provided an opportunity to perform notable functions right in front of the voters, rather than sitting deciding cases among provincials who could not vote.

If, as some argue, voters blindly punish those in power when outcomes are bad, being around when the Tiber overflows and floods the city could be bad for a politician. Thus, this specific position among other praetors poses exactly the test we desire. Some praetors were in office, but not able to respond because they were far away, while others were able to respond because they were in the city. This response would likely be very personally connected to the praetors themselves, as the Republic was a very limited state, and disaster response and public functions and construction were often funded by officials themselves in a form of *noblesse oblige*. And conveniently, the assignment to this position was nominally random. Each year, the relevant set of provincial assignments were put into a “sortition,” which randomly assigned the year’s praetors to a specific geographic jurisdiction (Brennan 2000).²

¹ Brennan (2000) notes that, of the two city praetors, the *praetor inter peregrinos* was more likely to be pulled away from the city on military commands – especially during the early part of the Livy Years. Our analyses hold even if we use only the *praetor urbanus*, however are strengthened by the inclusion of the *praetor inter peregrinos* because this position was still much more likely to be in the city at any given point in time than praetors with provincial commands.

² Historians disagree as to how perfectly random the sortition was (see Brennan 2000, 760). We are mindful of this debate and include controls for family prestige – the dimension on which some historians expect the sortition results were modified. This does not alter our results.

Fourth, during the Livy Years we also know when disasters struck the city: specifically, **Significant Floods**, our second important independent variable, which takes the value “1” when a significant flood is recorded, and “0” otherwise. For its entire history, the Tiber River has had a tendency to spill over its banks, flooding the low-lying areas between the city’s famous hills. These floods could be significant and cause widespread disruption, as many of the city’s public buildings and commercial spaces were built in this floodplain. The havoc caused by these diluvial disasters is well recorded in Roman history (Aldrete 2006). Contemporaneous authors commented on Tiber flooding – the primary way that we can date specific floods. Pliny (“the Younger”), writing several centuries after the Livy Years, described:

“Those who live in highlands out of the reach of these terrible storms have witnessed, here, the household paraphernalia and weighty furniture of the wealthy, there, the simple tools of the farm, over there, oxen, plows, and the plowmen themselves, here, herds set free and straying, jumbled among the trunks of trees, or the beams and roofs from villas, and all of it floating about randomly and widely.”³

Pliny’s comments evoke modern images of flooded main streets, a random assortment of unrelated debris comingling in the water. Undoubtedly, these experiences were just as damaging and traumatic to the Romans as they are to modern people, if not more so.

In total, we have a “denominator” group of second-tier Roman officials, of which some achieved the premier office. Each praetor was randomly assigned to either be based in the city or based in an external province out of public view. And, exogenously from this system, Tiber floods occurred in some years. Collectively, these data provide an interesting opportunity to estimate both the causal effects of flooding, as well as the causal effect of the opportunity to respond to floods, on a Roman praetor’s ability to obtain higher office.

³ *Letters of Pliny the Younger* 8.17. Quoted and translated in Aldrete (2006).

Plausible Explanations of Electoral Success and Control Variables

Though the preceding variables are plausibly exogenous, we also include a variety of relevant control measures that might also determine whether a praetor would become a consul within five years. These variables fit within four categories: family prestige, national security, economy, and temporal variation. A full summary of all control variables is provided in Table 1, and summary statistics are presented in Table A1 in Appendix A.

Variable Name	Measurement	Source
Family		
Patrician	“1” if praetor was a member of a known Patrician family; “0” otherwise	Broughton (1951), Brennan (2000)
Consular	“1” if one of praetor’s direct ancestors (or the praetor himself) had been consul before praetorship, “0” otherwise	Brennan (2000)
Family Prestige	Each office in the Republic is credited with a number of points based on its prestige. All points are summed (and logged) for offices accumulated by the praetor’s <i>gens</i> in the 11-year period ending in their praetorship.	Original construction by authors (further detailed in Appendix B) based on listing of office-holders in Broughton (1951).
Security		
War	“1” if the Republic was engaged in significant military conflict in the year of the praetorship, “0” otherwise.	Venning (2011)
National Insecurity	Estimate of the percentage of the Republic’s territory threatened by warfare in the year of the praetorship.	Original construction by authors (detailed in Appendix B), based on chronology of Venning (2011).
Economic		
Economic Output	Lead content stored in ice cores corresponding to the year of the praetorship. Lead was a byproduct of mining and coin-minting, and serves as an indicator of injection of wealth into the economy.	McConnell et al (2018). ⁴ The full time series is presented in Figure A1 in Appendix A.
Time		
Praetor Reform	“1” if the praetorship fell in a period of six praetors; “0” if the praetorship fell in a period of four praetors.	Brennan (2000)
Time Trend	The year of the praetorship	Brennan (2000)

⁴ The year 218 B.C.E., is missing in the McConnell et al (2018) data. We linearly impute this value based on data from 217 B.C.E. and 220 B.C.E.. This decade of output is highly stable.

Because our outcome is dichotomous, we estimate logistic regressions. The first model contains the two exogenous variables – City Praetorship and Flooding – as well as an interaction of the two. In the second model, we add year fixed effects, which capture any unobserved factors impacting the Republic in the year of the praetorship. In the third model, we add the control variables detailed in Table 1. Finally, in model 4, we again add year fixed effects, and retain the control variables with within-unit variance. We cluster standard errors for each annual slate of praetors. Abbreviated results of these models are presented in Table 1, while the full models with control variables are presented in Table A2 in Appendix A.

Table 1. Praetors Randomly Assigned to City Service Were More Successful After Flood Years

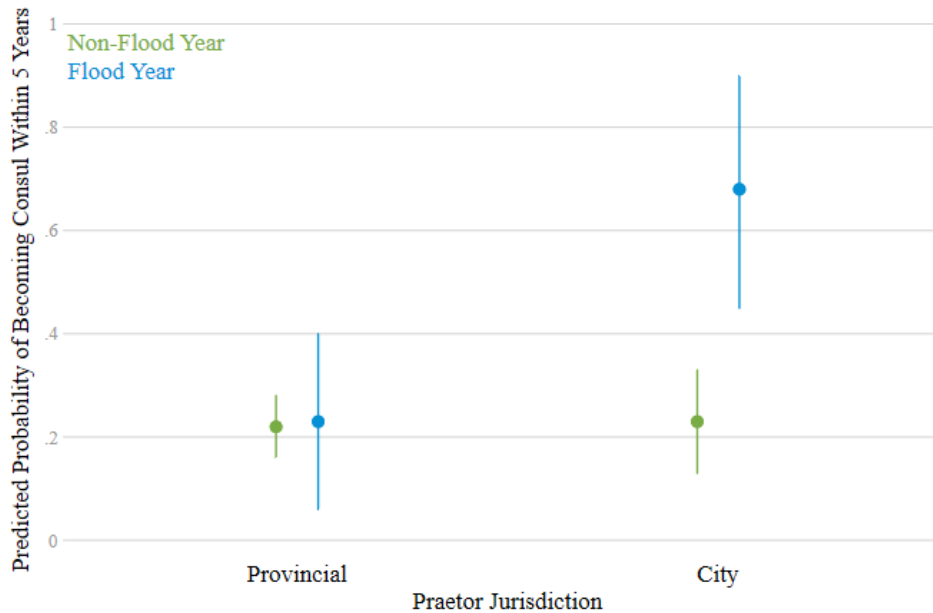
<i>DV: Became Consul (<=5 years)</i>	(1)	(2)	(3)	(4)
<i>City Praetorship</i>	0.03 (0.31)	0.06 (0.43)	0.05 (0.34)	0.16 (0.46)
<i>Flooding</i>	-0.16 (0.51)		0.05 (0.61)	
<i>City Praetorship X Flooding</i>	1.91 (0.71)	2.88 (1.45)	2.21 (0.73)	3.27 (1.33)
<i>Clustering</i>	53 Years	36 Years	53 Years	36 Years
<i>Controls</i>	None	None	All	Within-Year Varying
<i>Fixed Effects</i>	None	Year	None	Year
<i>N</i>	272	186	272	186

Note: Numbers in cells are logistic regression coefficients, with standard errors in parentheses.

We interpret these results by answering two questions. First, was there a general effect of flooding in years of praetorships on the short-term political success of praetors? An account of blind retrospection may imply a negative effect by unhappy Roman voters, but, in general, floods were not associated with a positive or a negative effect on future political careers. Second, did the floods impact career success for those praetors randomly assigned to be in the city during the year of a flood? The data consistently support the conclusion of a positive effect. Though effect sizes vary by model, in Model 3, we estimate a 45 percentage-point increase to the likelihood of obtaining the consulship within five years for those assigned to be in the city in a flood year

compared to praetors assigned to provincial posts. We plot the predicted probabilities (based on Model 3) of becoming consul within five years in Figure 1.

Figure 1. Impact of Flooding on the Likelihood of Becoming Consul, by Praetor Jurisdiction



Discussion and Conclusion

The positive effect we find does not match the world of electoral behavior described by theories of blindly retrospective voters. We find no evidence that Roman voters blindly punished officeholders for devastating, but uncontrollable, floods. Instead, our results better fit another familiar assessment of voters: that they make rationally self-interested choices based on limited information. The opportunity to respond to a crisis, to spend one's own resources to help the city recover, and to be observed doing something to benefit voters was a massive political resource in a city-state empire. The achievements of candidates outside the city were of reduced importance to voters, and less likely to be known than what city praetors did in response to disasters in Rome itself. And while the normal judicial work of a praetor was as likely to create

as many enemies as friends, responding to a disaster provided an opportunity to achieve universal acclaim. This description best matches the data we analyze.

Our results contribute to this literature in multiple ways. First, we test theories of voting in a novel new context. Second, we have plausibly exogenous assignment not only to disaster, but also to the ability to respond, which in previous studies has been an endogenous element. Finally, we also place pre-modern voters within a discussion of how voters behave. There were elections before democracy, and those also merit consideration when we discuss “how voters decide.” Based on our findings, as well as others within this expanding literature, our theories of voting are likely time- and place-dependent and subject to the nuances of the political environment.

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Appendix A – Summary Statistics and Full Model Results

Table A1. Summary Statistics

Variable	Mean	Median	S.D.	Min	Max
Became Consul (≤ 5 years)	0.24	0	0.43	0	1
City Praetorship	0.31	0	0.46	0	1
Flooding	0.13	0	0.34	0	1
Patrician	0.36	0	0.36	0	1
Consular Family	0.61	1	0.48	0	1
Family Prestige	2.78	2.48	1.01	1.79	5.44
War	0.70	1	0.46	0	1
National Insecurity	0.10	0.01	0.16	0	0.71
Economic Output	3.39	3.06	1.75	1.25	11.19
Six-Praetor Norm	0.69	1	0.46	0	1
Year	-189.72	-189	14.88	-218	-166

Figure A1. Economic Output in the Republic over Time

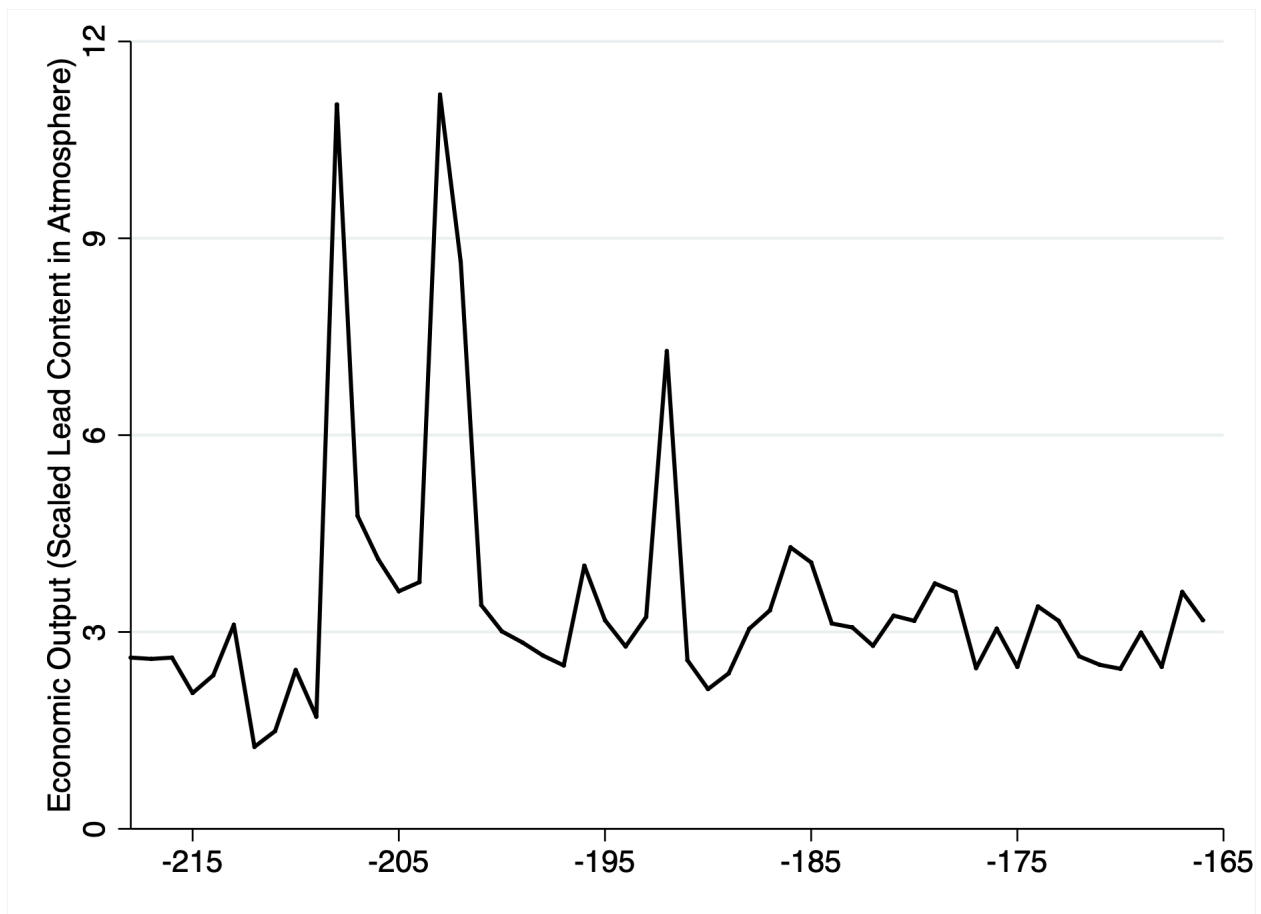


Table A2. Model Results

<i>DV: Became Consul</i>	(1)	(2)	(3)	(4)
<i>City Praetorship</i>	0.03 (0.31)	0.06 (0.43)	0.05 (0.34)	0.16 (0.46)
<i>Flooding</i>	-0.16 (0.51)		0.05 (0.61)	
<i>City Praetorship X Flooding</i>	1.91** (0.71)	2.88* (1.45)	2.21** (0.73)	3.27* (1.33)
<i>Patrician</i>			-0.50^ (0.30)	-0.67 (0.41)
<i>Consular Family</i>			1.34** (0.36)	1.60** (0.54)
<i>Family Prestige</i>			0.42* (0.18)	0.44^ (0.26)
<i>War</i>			0.62 (0.39)	
<i>National Insecurity</i>			-0.96 (1.50)	
<i>Economic Output</i>			-0.15 (0.13)	
<i>Six-Praetor Norm</i>			-1.25 (0.81)	
<i>Year</i>			0.04^ (0.02)	
<i>Clustering</i>	53 Years	36 Years	53 Years	36 Years
<i>Controls</i>	None	None	All	Within-Year Varying
<i>Fixed Effects</i>	None	Year	None	Year
<i>N</i>	272	186	272	186

Note: Numbers in cells are logistic regression coefficients, with standard errors in parentheses.

^=p<0.10; * = p<0.05; ** = p<0.01

Appendix B – Further Explanation of Original Variable Construction

Family Prestige

In addition to having markers of the family’s deep historical lineage (patrician status) and achievement (consular status), Roman families were also retail politics shops, complete with client networks of bound voters and a system and apparatus of electioneering and political influence that could help ensure the future electoral success of family members. They were brands of accomplishment, competence, and success that younger members carried with them on election day. To account for this additional – and much more variable – aspect of family prestige, we measure each family’s recent acquisition of offices and honors in the Republic, counting all offices held by someone of the same *gens* in the year of a praetorship and the ten previous years. We award each office a number of points based roughly on the prestige conveyed by the Romans themselves through the number of Lictors (ceremonial guards) assigned to each position. We present the schedule of values in Table A3.

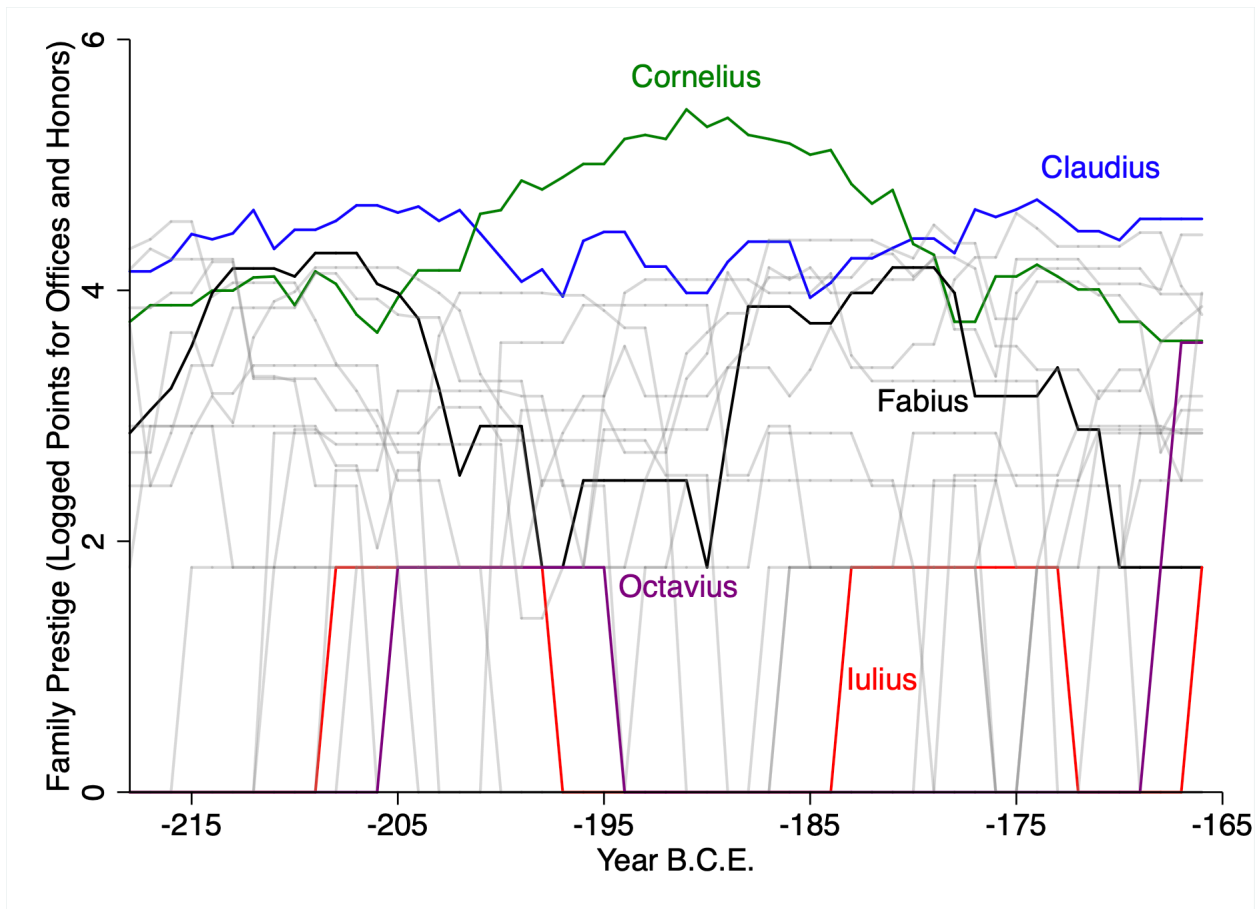
Table A3. Schedule of Points for Various Positions and Honors within the Roman Republic

Position or Honor	Points
Triumph	30
Dictator (non-election)	24
Ovation	15
Censor	15
First Consul	12.5
Second Consul	11.5
Suffect Consul	10
Master of Horse	6
Praetor	6
Dictator (election)	4
Master of Horse (election)	1

For each family-specific eleven-year points total, we log the value to form a measure of **Family Prestige**. This is both a measure of their current “brand” for competence and

achievement as well as their political resources and skills. This measure indicates that 102 different families held senior positions or received honors in the 53-year period we study. In Figure A2, we present a plot of the most prominent families in Rome during the Livy Years, with several notable families highlighted. Two families (the Claudii and the Cornelii) were consistently dominant, while the fortunes of all other families waxed and waned.

Figure A2. Family Prestige Over Time, with Five Highlighted Families



(Note: Zero values artificially induced, given the log scale. No zero values are included in the paper as a praetor’s own office guarantees a non-zero score for his family.)

State Insecurity

As the Second Punic War illustrates, not all wars are created equally, so we also include a measure of **State Insecurity**, which is rough estimate of the percentage of Roman territory in each year threatened by violence and disorder, based on Venning (2011). We define regions – Italia, Hispania, and a combined Greece/Dalmatia – and use standardized discounts on security based on specific entries in the chronologies. For example, for each year in which border raiding and incursions are noted, we deduct 0.1 from that region. More significant invasions yield 0.25 and higher deductions depending on modifying adjectives (for example, “significant” or “devastating”) in Venning (2011). These numbers decay as territory is recovered and belligerents are expelled. These are obviously blunt measures, but capture the disorder caused by violence, and differentiate between smaller conflicts on the border and large incursions and invasions. Ultimately, we weight Italy at 90% of the Republic’s score and Spain and Greece/Dalmatia at 5% each.

This measure takes quite small values other than the years of the Second Punic War. If scared Romans, worried about “Hannibal at the gates,” reflexively blamed officials in charge for the state of things, we would expect those who served in these darker dark times to do poorly in the near-term electoral pursuits. We present the Republic’s total score for each year in Figure A3.

Figure A3. Insecurity in the Republic Over Time

