

# Formal Models in Historical Political Economy

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## Abstract

This chapter discusses approaches to formal modeling in historical political economy and leading examples of formal models in this area. I argue that formal models are useful because they can depict causal mechanisms with a high degree of clarity. This in turn enables causal *explanation*, an important complement to causal identification from credible empirical research. Moreover, the clarity of causal mechanisms in models can help us understand when two seemingly disparate historical processes potentially result from a similar mechanism, thus enabling a sort of causal *generalization*.

**Keywords.** Formal modeling, game theory, historical political economy

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# 1 What Do Formal Models Do in Historical Political Economy?

Scholars of political economy have long applied the field's tools to analyze historical events and development. The emergence of historical political economy as a subfield provides an opportunity to reflect on how we accomplish this, and how we can do it better. In this chapter I argue that formal models are an indispensable part of our toolkit, and I provide an incomplete survey of major formal-theoretic work in the field to date.

Consistent with broader directions in political economy, much research in HPE attempts to identify treatment effects based on randomizations or as-if randomizations throughout history. But there is a limit to this kind of knowledge. Credibly identified treatment effects of some  $X$  on some  $Y$  do not explain why an effect occurs. They simply rule out all explanations in which  $X$  is unrelated to  $Y$  (which garden variety observational correlations usually also do), or in which the relationship is driven by a confounder  $W$  (which garden variety observational correlations usually cannot do).

We use models because we want to know more than this. There are two important contributions of models in historical political economy. The first is that we usually wish to *explain why* variables are related. Models represent the social, political, and economic forces that lead to decisions and in turn historical events, *i.e.*, they embed causal mechanisms. Showing that  $X$  causes  $Y$  does not in itself identify the social forces that bring about the effect. A model, and only a model, can explain what those forces are.

A second rationale for modeling is that we usually wish to *generalize causal mechanisms* across specific contexts. Generalizing requires knowing what other scenarios are like the one in which we observed an effect. Models, especially formal models, enable this. That is because formal models define an *equivalence class* of scenarios in which a specific causal mechanism operates. No two social settings are exactly alike; but two settings that are both in a model's equivalence class are similar enough in relevant ways that the same mechanism operates in both. Without a model to relate distinct social settings, we have no way to know if the effect of  $X$  on  $Y$  from one setting should apply in another or not. To be sure, causal generalization by models brings us far short of the identificationist demands of the self-styled credibility revolution, but it is the best causal generalization we can get (Gailmard 2021*b*; cf. Pearl and Bareinboim 2014).

These arguments apply to theoretical models of any kind. The additional benefit of *formal* models is two-fold (cf. Gailmard 2020). First, formalization and the use of deductive methods ensures *consistency* of assumptions and conclusions. With a valid deductive argument, it is not possible to state implications that are incompatible with premises. This provides necessary discipline on those of us who are tempted to see our arguments explain more than they do. Second, formalization brings *clarity* of assumptions and their connection to implications. The real downside of formalization is that it poses a barrier to engagement for scholars unfamiliar with formal techniques. But once the cost is paid to overcome that barrier, formalization requires assumptions to be clearly stated, and deductive arguments show clearly the role they play in

deriving implications.

In HPE, formal models are almost always game theoretic. This is because game theory provides a flexible language to express an extremely wide variety of strategic decision problems, and thus identify how seemingly disparate situations are actually fundamentally alike. These models typically assume canonically rational actors, which has been sharply criticized in previous reviews of formal models of historical events (e.g. Elster 2000, 692). In my view the criticism is overstated. To impute rationality to the agents in a model is simply to model agents who had their reasons to do what they did. One way to understand decisions is to reconstruct what those reasons were, which is simply to build a rational model of them. A commitment to rationality does not eschew emotions or other-regarding preferences as motivations—avoiding embarrassment or pursuing glory can be powerful motivators. To be sure, any given historical event surely admits of non-rational explanations. There is nothing wrong with this, but a scholar interested in the event should be interested in all the explanations for it. If some of them involve strategic rationality, this per force implies an interest in rational models.

Game theoretic models in HPE “explain” historical events in the sense that their equilibria depict the operation of fundamental strategic forces that underlay major decisions in those events. These strategic forces embed some combination of the “library of mechanisms” that political economists draw upon to explain anything: commitment problems, information asymmetries, agency problems, and the like (Guala 2005; Cox 2004), and in some cases models in HPE may add to that library of mechanisms. That said, formal models in HPE cannot succeed unless they thoroughly grounded in the historiography of the place, time, and developments they analyze. Yet successful formal models in HPE cannot simply reproduce the causal arguments already present in historiography; they must make new arguments about the historical process under consideration. In this sense, formal models in HPE are making historical as well as theoretical arguments.

With that background in mind, I next provide an (alas, incomplete) survey of formal models in HPE. I primarily consider models focused on and developed for a specific historical process or context, and exclude models that are in principle applicable to history but not built around any setting in particular (because that would include all models). Following this, I characterize predominant approaches to formal modeling in HPE, and consider the sort of understanding that different types of models offer.

## **2 An Incomplete Survey of Formal Models in Historical Political Economy**

Formal HPE has considered a relatively small number of general substantive topics. On one hand, this tends to be self reinforcing. When there is already a body of modeling literature on a topic, it provides a foothold for building more models and a community of scholars to read them.

On the other hand, the marginal value of formal models may be particularly high on substantive topics where few or none have been written. The portability of models allows communication of major issues to a substantive non-specialists in a short space.

## 2.1 State Development, Capacity, and Structure

Some of the foundational questions in political economy involve why states and state-like organizations come to exist, what they do, and how they operate.<sup>1</sup> Models in historical political economy have provided answers based on extraction and its management, pursuit of trade, and protection of the powerful. Noticeably absent from the list of explanations HPE has offered is the state as maximizer of a broad-based social welfare function. This is probably not an oversight.

Focusing on state institutions in the earliest sedentary societies, Mayshar, Moav and Neeman (2017) develop a foundational model of state institutions that depend on transparency of agricultural production. The model is embedded in a simple but flexible repeated principal-agent framework, and thus takes a state hierarchy as given. The agent produces high output in a period if both its effort is high and conditions are good, and produces low output otherwise. The agent observes local conditions; the principal observes output and a noisy signal of local conditions. The probability of an accurate signal represents “transparency” of the agent’s production process to the principal. The agent’s incentive scheme is a mix of “sticks” (threat of dismissal and replacement by an ex ante identical agent, at a cost to the principal) and “carrots” (bonuses). The key result is that if transparency is low enough, then the optimal contract is a “pure carrot” scheme, but if transparency is high, then the optimal contract is a carrots-and-sticks scheme: the agent receives a bonus when output is high, but is dismissed when output is low and the signal indicates favorable conditions. In either case the agent exerts high effort every period, but the “stick” allows the principal to induce high effort with smaller bonuses. So as production becomes more transparent to the ruler, the ruler extracts more output and keeps agents closer to subsistence wages. Mayshar et al. apply the model to growth of state power in the ancient near east, but the results can be extended easily to colonial contexts.

Dal Bó, Hernández-Lagos and Mazzuca (2022) also focus on statebuilding in the first sedentary societies. They argue that the decision to form these societies required not just material prosperity, but security from outside attackers. They posit a 2-period model in which a settler society has a resource endowment it can consume, invest in future output, or devote to security. In the first period, an outsider can attack the settlement. Success in conflict is determined by a contest function, which depends on the settler’s and attacker’s expenditure on conflict. The winner has secure control over the settlement’s output in period 2. Therefore, high investment by the settler in future output makes it a more attractive target for attackers, and thus requires increased security expenditures to secure prosperity. The settler can achieve “civilization

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<sup>1</sup>See also Dincecco and Wang (2022) in this volume for a review of this area.

potential”—a combination of prosperity and security—only if investment returns are sufficiently high, and security expenditure is sufficiently effective. The key point is that high potential for material growth is not in itself sufficient to bring about civilization; potential for defense is also necessary. However, there is an asymmetry in how these parameters contribute to civilization. Greater investment returns increase the settler’s future prosperity, but also make the settler a more attractive target for attack, so require greater security expenditure. On the other hand, greater effectiveness of security expenditures benefits the settler with no countervailing effects.

Fast-forwarding to early modern Europe (which reveals a massive lacuna in the field), Bates and Lien (1985) present one of the first formal models in HPE that looks recognizably like the contemporary literature. They model the interaction of a ruler and a (homogeneous) mass of subjects in jointly determining taxes, other policies, and output. The ruler and subjects have different preferences over both taxes and other policies. The model makes the simple but powerful point that the ruler is constrained in policy choice by the preferences and consumption of the subjects. Therefore, the equilibrium tax rate is inversely related to other policy concessions from monarch to subjects. Insofar as legislative representation is a natural device to ensure policy concessions, the model explains rulers’ grant of assembly rights in exchange for greater taxes, a now-standard argument about elite bargaining over assemblies and representation.

Gennaioli and Voth (2015) consider state centralization in early modern Europe. They posit a model of states centralizing power over localities in their jurisdictions,<sup>2</sup> which is costly due to local resistance but allows the state to claim more revenue. After deciding how much to centralize, states go to war with exogenous probability. There are two key parameters: first, internal fragmentation within a state. This determines the center’s cost of overcoming local resistance. Second is the financial demand of war, which allows the chance of victory to range from  $\frac{1}{2}$  irrespective of revenue differences, to sure chance of victory for the side with more revenue. When the financial demands of war are low, rulers have little incentives to centralize: it does not help much in winning wars, and so a centralizing ruler may incur the cost of local resistance only to have the gains lost from the flip of a coin. In this case, a greater chance of war reduces centralization. By contrast, when the financial demands of war are high, rulers have strong incentives to centralize: centralization brings more revenue, and thus not only a markedly greater chance of keeping it after war, but of winning the rival’s revenue too. In addition, the centralization of fragmented vs. cohesive states diverge. Rulers in cohesive areas centralize more—the sensitivity of war outcomes to revenue leads them to amplify their advantage—while rulers in fragmented areas centralize less.

In short, “war makes states” (cf. Tilly 1990), but only when the fiscal demands of war are high. Gennaioli and Voth (2015) relate this parameter to the “military revolution” in early modern Europe, which scholars have argued greatly increased the sensitivity of war outcomes to fiscal resources. The model explains why European statebuilding before this period was (*i*)

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<sup>2</sup>Thus unlike Acharya and Lee (2018), Gennaioli and Voth (2015) assume that only one ruler can possibly centralize a given locality.

generally low, and *(ii)* relatively similar for internally homogeneous vs. fragmented states, and why statebuilding after this period was high, especially for internally cohesive states.

One of the signal attributes of governance around the world is a system of sovereignty divided among states that are more or less territorially contiguous. Acharya and Lee (2018) explore the emergence and stability of this system. They posit a “market for governance” for individuals or localities. Each individual is located on an interval and attaches a constant value to “governance”. Two potential suppliers of governance—would-be states—are located at the end points. The states compete for the allegiance of each individual. Each ruler offers a “price” of governance that depends on an individual’s location (all bargaining power rests with the rulers), and incurs a cost for governing any individual. A key assumption is that the cost of governance of an individual by a ruler is increasing in their distance. Prospective rulers offer their price schedules simultaneously, and individuals voluntarily decide which ruler’s governance to accept.

Under natural assumptions, there is a range of locations to which only one state can profitably supply governance, and therefore have the full surplus extracted by that state. However, there may be a range of locations where both rulers can benefit from supplying governance. For these locations, Bertrand competition between the potential rulers threatens to drive down the price of governance. Acharya and Lee show that in a repeated game, the rulers can still sustain full rent extraction for locations in the overlapping market, with the static Bertrand pricing outcome as the off-equilibrium punishment. Thus, the model explains *(i)* territorial contiguity of states, *(ii)* the respect of rulers for the territorial claims of other rulers. The implication, especially of point *(ii)*, is that the territorial state system is simply a cartel in the “market” for governance, for the benefit of states at the expense of individuals in overlapping “markets” for governance.

Greif, Milgrom and Weingast (1994), a foundational model in HPE, explores the institutional foundations of extra-national trade in the medieval world.<sup>3</sup> The issue is that extra-national traders were exposed to expropriation in foreign states, which could threaten valuable exchange. Traders organized in guilds could impose embargoes on hosts that failed to uphold traders’ security. However, this presented a second-order problem: if all guild members observed the embargo, it would be particularly tempting for a trader to violate it and have a city’s trade all to itself. Greif et al. explain the structure and powers of medieval merchant guilds as institutional solutions to those problems. The paper is a landmark both substantively, for demonstrating the institutional foundations of economic exchange, and methodologically, for combining sophisticated, original, and generative theoretical modeling with deep investigation of economic history.

Larson (2017) analyzes the relationship between peer sanctions, cooperation, and social network structure. The model posits a population of individuals interacting over time in pairwise prisoners’ dilemma games. Population members are uniformly randomly matched each period.

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<sup>3</sup>See also Milgrom, North and Weingast (1990) and Greif (1993).

In any period, individuals spread information about misbehavior by their counterpart through an exogenous social network. Information about an individual's past defection induces a punishment phase in which the previous defector is expected to cooperate while their counterpart defects. The key point is that individuals in a peripheral network position cannot spread information very far. Therefore their counterparts can cheat against them with little risk of entering the punishment phase. Larson applies the model to understand the breakdown of social order between white and Chinese settlers in the 19th century American West. Despite the lack of formal institutions, social order among white settlers in boomtowns was often surprisingly stable, even among immigrants. On the other hand, defection against Chinese immigrants quickly shifted from initially low levels to rampant, egregious mistreatment. Larson notes that Chinese settlers held an unusually peripheral and isolated network position, even relative to other immigrant groups, and therefore were uniquely vulnerable to rampant defection.

Egorov and Sonin (2015) model the endogenous emergence of violence in political (non-democratic) succession. The setup is a simple dynamic model: an incumbent ruler faces a challenger in a fight for power, with the result exogenously determined. The winner then decides whether to execute the loser. If the winner spares the loser, the loser challenges again in the next period. If the winner executes the loser, the winner either faces no challenger in the next period, or faces a new challenger, with exogenous probabilities. This simple setup produces a crisp tradeoff: if the winner executes the loser, it has some chance of uncontested rule. But when a ruler with a history of executions eventually loses, the challenger defeating them may have no choice but to execute them. An important implication of the analysis is that violence is fundamentally history-dependent. Leaders with a history of violence are deposed violently, because they pose the gravest threat to their successors if spared. But with a history of non-violent deposition, leaders can alternate in power indefinitely without executions.

In a creative twist, Koyama, Rahman and Sng (2021) focus not on political-economic development, but on military forces—in particular, the development of navies in a competitive environment. In their model competing states choose naval investments in a Stackelberg model. Naval investments determine chance to win a fixed prize via a contest success function. Both states incur a fixed cost of investment. These aspects of the model differentiate naval power from land-based military forces, and produce several stylized facts about naval power throughout history: cycles of investment; higher concentration across states than land power; and periodic arms-race type competitions.

## 2.2 Institutional Persistence

Political-economic development is as much a story of institutional *stasis* as institutional *change*. To this end, an extensive sub-literature focuses on why political and economic institutions persist, particularly in the substantively important and theoretically challenging case when they

are inefficient.<sup>4</sup> A common theme in the models is that inefficient institutions persist because they protect the power of entrenched elites, whereas successors to that power under alternative institutions cannot commit to compensate them sufficiently. Markov games are a natural match for these themes and predominate in this subliterate.

Modeling institutional persistence is one of several strands of formal HPE particularly influenced by Acemoglu and Robinson. Acemoglu and Robinson (2006) first address this issue in a model positing that political and economic power are linked. Moreover, if an economic elite is replaced, the new elite cannot commit to preserve the old elite’s political power. Therefore, elites fearing replacement block efficiency-enhancing economic change to preserve their hold on power. Paradoxically, strongly entrenched elites have less concern for replacement and therefore are more open to efficiency-enhancing change. The paper applies the model to interpret economic modernization in Britain, Germany, Japan, Russia, and the Habsburg Empire.

Acemoglu and Robinson (2008) elaborate on the theme by considering *de jure* political power, such as that conferred by formal institutions, and *de facto* power, which is conferred by capacity for collective organization and action. In this model, elites can offset loss of *de jure* power with investment in *de facto* power. The key implication of this logic is that changes in formal institutions, such as the end of colonialism in Latin America or slavery in the US South, may not alter economic institutions. This is because, facing *de jure* institutional change, elites alter their investment in *de facto* power to preserve their privileges.

Iyigun, Rubin and Seror (2021) extend Acemoglu and Robinson (2006) to consider another channel of entrenchment of elites: culture. They model elites’ capability to provide a public good as a function of the proportion of nonelites that share a cultural attribute with them. This production technology then influences cultural transmission: parents socialize children to share that cultural attribute so that they can consume larger levels of the public good. Yet this socialization perpetuates elites’ hold on power, which allows them to block threatening economic changes even when they are efficient. The paper applies the model to understand “cultural revivals” in the Jim Crow South and the Gülen movement in early 20th century Turkey.

Similar themes abound. Besley and Persson (2009) argue that elites may obstruct state capacity development to prevent rivals from having effective means of taxation when they take power. Acemoglu, Vindigni and Ticchi (2010) show that state weakness can prevent the future emergence of a powerful military with which the elites would be forced to share rents. Acemoglu, Ticchi and Vindigni (2011) argue that when elites anticipate democratization, they have an incentive to retain an inefficient public administration based on patronage politics. Patronage enhances elite control over democratic politics and thus can limit redistribution. Acemoglu, Robinson and Torvik (2020) focus on barriers to efficient state centralization. In their model, centralization allows more efficient public goods provision and regulation, but it also allows non-elites to identify common interests, in turn confronting and making demands on the state

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<sup>4</sup>See Acharya, Blackwell and Sen (2022) in this volume for a review of this area.



in new ways. Decentralization, though inefficient, enables elites to pursue a sort of “divide and conquer” strategy over non-elites and thus control the political agenda. And Dippel, Greif and Treffer (2020) argue that elites use their political power to inhibit development of a productive informal sector: this reduces workers’ outside options, and thus their bargaining power over wages in the formal sector that elites control.

Robinson and Torvik (2016) analyze emergence of presidential vs. parliamentary regimes. Their key assumptions are that (i) parliamentarism grants stronger protections to minorities (in the form of some chance to claim agenda setting power), and (ii) presidentialism grants presidents stronger control over their political coalition than parliamentarism grants to prime ministers. Therefore, while political leaders generally prefer presidentialism, their supporters may not because it erodes their influence within their coalition. At the same time, when an empowered faction fears losing agenda control to another coalition, then both leaders and followers prefer presidentialism. The model is applied to interpret the emergence of presidentialism, especially transitions to it from parliamentarism, in Latin America and Africa.

### 2.3 Colonialism and Imperial Governance

Empires have been historically important vehicles for dispersing structures of both state organization and political coercion around the globe. Besides its substantive importance, imperial governance is strategically interesting: it requires projecting power, preserving order, and ensuring compliance at distance and with limited information at the center. For these reasons, imperial governance has been a significant focus of formal modeling in HPE.<sup>5</sup>

Principal-agent problems are among the most salient in imperial governance. Sng (2014) presents a model of a central government that delegates tax collecting to an agent. The central government sets the legal tax rate, but the agent can extract larger rents from peasants. The center wishes to maximize tax rates, but is constrained by the possibility of local revolt if effective tax rates are too high. The center can monitor the agent and punish peculation, but the efficacy of monitoring declines exogenously in state size. Agents extract more from peasants in larger states (because monitoring is weaker); therefore, to prevent unrest, the center sets lower official tax rates. Sng uses this model to explain high corruption and low revenue collection from distant provinces in late imperial China.

Padró i Miquel and Yared (2012) develop a dynamic moral hazard model to analyze the problem of indirect control that was ubiquitous in imperial governance. In their model a principal can either use an agent to control a disturbance in the agent’s territory through unobservable effort, or intervene to control the disturbance itself. The intensity of intervention is chosen by the principal, and greater intensity is costly to both principal and agent. Further, the principal cannot commit to future intervention decisions, and the agent cannot commit to future effort after intervention subsides. Due to limited commitment, the optimal dynamic incentive scheme

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<sup>5</sup>See also Guardado (2022), this volume, for a review of this area.

exhibits periods of delegation alternating with occasional costly interventions by the principal. The paper applies this model to early imperial Rome, where indirect governance of imperial dominions through client states was occasionally punctuated by incursions from the Roman legions to preserve order.

In a series of papers, Gailmard (2017, 2019, 2021) explored the development of New World institutions under English colonial rule. These models all turn on agency problems between the English crown and colonial governors. The general thrust is that these agency problems determined English imperial institutions, and set the stage for American institutions that borrowed from them. In particular, Gailmard (2017) argues that colonial governors had incentives to extract rents from colonists even when costly to the crown, and the cost of replacing governors limited the crown's discipline. In response, the crown supported assembly rights in colonies: assemblies could restrain rent extraction when the crown itself could not.

However, English colonial assemblies proved adept at shifting power to themselves away from the crown's colonial agents. The crown then faced another agency problem with colonial governors: how to induce them to stand up to assembly encroachments on the crown's prerogative. Gailmard (2019) argues that review of colonial legislation by royal bureaucrats in England helped with this effort. While designed to mitigate inconsistencies between colonial and English law, legislative review could also reveal capitulation by governors to assembly demands, and thus cause governors to resist assemblies. Yet Gailmard (2021*a*) explains why these approaches were ultimately unsuccessful at restraining assembly power in the English colonies. In bargaining over colonial revenues, the crown was typically in a weak position because it usually desired more revenue than assemblies did. At the same time, governors typically had superior information about exactly how much revenue an assembly would grant.

Ma and Rubin (2019) model another facet of the principal-agent problem in imperial administration: absolutist rulers who are well-informed of what agents take from subjects cannot commit not to expropriate their wages. This degrades the incentives of provincial agents to administer imperial taxation. Ma and Rubin contend that low investment by the ruler in administrative capacity makes the ruler ignorant of the stakes of confiscation. In turn it gives administrators incentives to implement imperial taxes. Ma and Rubin apply this model to explain the low wage/low revenue equilibrium of imperial China under the Qing dynasty. They further argue that limited rulers can avoid the problem of expropriating wages *de jure*, and thus implement a high wage/high tax equilibrium.

Garfias and Sellars (2021) analyze the imposition of direct rule by the Spanish Empire in colonial Mexico. The Spanish initially governed much of Mexico through indirect rule by local potentates (*encomenderos*). Garfias and Sellars argue that the potentates were generally better at preserving local order than state agents, but they extracted excessive revenues from the crown. In their model, a region generates revenue proportional to its population, and is exogenously split between the crown and the potentate. The crown can attempt to impose direct rule,

thereby keeping all revenue for itself. Following this, the potentate makes two choices: first, how strongly to resist direct rule; second, whether to guard his region against local unrest. Generally, resistance against the crown and local protection are complements for the potentate. In particular, if regional population (thus income) falls below a parametric threshold, guarding against rebellion is not worth the potentate’s cost, and the potentate’s resistance to direct rule falls as well. In turn, the crown’s benefit from direct rule increases, both because the potentate is less useful when not guarding against unrest, and because the potentate’s resistance to the crown declines. Garfias and Sellars argue that this dynamic drove imperial centralization in Mexico. Specifically, indigenous demographic collapse in the 16th century caused declines in regional incomes, weakened *encomendero* resistance, and facilitated imposition of direct rule.

Garfias and Sellars (2022) extend this analysis to consider the subtle effects of statebuilding on peasant rebellion. In particular, centralization can weaken both the power and the loyalty of local potentates who mediate between peasants and the state. This creates fragile authority structures in which local resource or climate shocks can result in widespread peasant unrest, which reduces the central state’s ability to punish elite defectors, thus reinforcing the defection. This positive feedback loop reveals why even mild shocks that, while easy for the government to control in times of strength, can threaten the regime’s survival in times of weakness. Garfias and Sellars use evidence from late colonial Mexico to argue that tax centralization interacted with resource shocks to cause greater peasant unrest.

Departing from the principal-agent approach, Arias and Girod (2014) model the origins of extractive institutions in the Spanish empire. A colonizer encounters an indigenous leader with a given “technology”—essentially, a social organization—for extracting output from indigenous laborers. The colonizer can either retain the indigenous leader to “mediate” with local laborers, and share output from the leader’s technology, or replace the technology at some cost. Technologies correspond to the degree of hierarchy in indigenous labor processes. The key assumptions are that for any technology, the leader’s mediation raises output; but the difference between mediated and unmediated output is smaller for higher levels of indigenous labor hierarchy. This means that the indigenous leader’s bargaining power is lower when there is already a strong hierarchy. In this case, the colonizer obtains a high share of the output in bargaining with the leader, and the leader and the existing labor hierarchy remain in place. By contrast, when the existing indigenous labor hierarchy is low, the colonizer’s bargaining power is lower. In this case, the colonizer’s decision depends on local resource endowments. When they are small, the cost of removing the leader and remaking the labor technology is not worth the cost, so the leader remains in place and the colonizer accepts unfavorable bargains. But when local resources are very valuable and indigenous hierarchy is low, the colonizer removes the leader altogether and builds a hierarchy at its own cost. In short, the model implies that colonizers practiced strongly hierarchical organization of indigenous labor in regions where either (*i*) indigenous labor organization was already strongly hierarchical, or (*ii*) indigenous resources were very valuable.

Taking a macro perspective, Grossman and Iyigun (1997) analyze the end of European colonialism in Africa and Southeast Asia. In their model an indigenous population allocates time between productive activities (which the colonizer exploits) and subversive activity (which hinders exploitation). The key result is that increasing population causes an increase in the return of indigenous people to subversive action, and thus reduced the gain to European states of holding colonies. By this result the paper argues that population increase contributed to the end of colonialism in these regions.

Gartzke and Rohner (2011) focus on a related development: the decline in territorial acquisitiveness by militarily powerful states. Their model incorporates several key factors to simultaneously explain cycles of colonial acquisition and the secular decline of acquisitiveness. Technological shocks in one state allow sufficient military power to expropriate resources from another—to colonize it. But shocks decay over time, which causes recession in specific territorial claims. In addition, increasing capital abundance makes empire increasingly costly over time, such that nations use power to control terms of trade rather than to expropriate resources for production. This accounts for a general decline in territorial acquisition over time. Moreover, when the most powerful states transition away from territorial acquisition, it means that less powerful states need not hold their own colonies to keep up, thus sparking a wave of decolonization.

## 2.4 Democratization, Franchise, and Representation

Acemoglu and Robinson (2000) initiated a sea change in political economy with a dynamic model of economic redistribution and institutional change.<sup>6</sup> Their model begins with a non-democratic state, with elites in control of redistribution. In each period, the threat of collective resistance by the masses is stochastically drawn. The elite can respond to this threat in any period either by redistributing resources within the period but retaining future power for themselves, or by changing institutions and granting the masses political power in the future. The key result is that when the masses are likely to pose a collective threat in every period, elites redistribute a lot whenever the threat materializes—but they also retain future power for themselves, and stave off revolution. When the masses are unlikely to pose a collective threat, elites respond by granting the masses formal institutional power when the threat materializes. The reason is that when the chance of collective threat from the masses is low, elite promises of future redistribution are not credible—everyone knows the elites will not redistribute in periods when they face no threat. Thus in the rare case that the masses pose a high collective threat, they must cement it into formal political power or lose out on redistribution for a long time. In turn, the elites face a stark choice between accepting a revolution of the masses, in which the elites lose everything, or voluntarily ceding political power, in which the masses partially redistribute. In equilibrium

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<sup>6</sup>See also Hanlon (2022) and Stasavage (2022), this volume, for reviews of HPE literature on suffrage and democracy.

elites expand political power to the masses and thereby commit to future redistribution. This grant of power to the masses can be accomplished through a variety of institutions; Acemoglu and Robinson focus on suffrage expansion. They compare 19th century suffrage expansion in England and Germany. In Germany, mass organization through unions was strong, and elites responded with routine redistribution—but kept political power. In England, the masses were not so well organized, but when threat arose, elites responded by expanding political power.

Lizzeri and Persico (2004) construct an alternative explanation for franchise expansion, tied specifically to 19th century Britain. Their model removes the revolutionary threat and focuses entirely on elite incentives. The model involves Downsian competition between two parties, which can promise either public goods or targeted redistribution. With franchise restricted to elites, a winning platform can be constructed to redistribute from disenfranchised masses to a subset of elites; thus, the public good is under-provided. But with broad franchise, platforms of public good spending always win. The logic is that with restricted franchise, fewer votes are needed to win, so targeted redistribution to elites succeeds. But with broad franchise, more voters need to be “bought off,” and public goods are better suited for this. When public goods are sufficiently valuable (due, e.g., to rising urbanization), even most elites prefer public goods programs to targeted redistribution. Thus, elites use expanded franchise as a tool to commit parties to broad-based public goods expenditures.<sup>7</sup>

Llavador and Oxoby (2005) present a third major argument for franchise extension and its connection to economic growth. In their model population consists of elites, exogenously divided between capitalists and landowners, and workers, endogenously divided between skilled (industrial labor) and unskilled (agricultural). Political parties associated with each elite faction choose policy and voting institutions to build a winning electoral coalition with politically unattached masses. In equilibrium, when the landowners’ party is *ex ante* weak and there is a sufficiently large group of industrial workers, political competition results in both franchise expansion to workers (because it will cement the power of industrial elites relative to landowners) and economic growth (because elites choose productivity enhancing policy). When these conditions are not met, the equilibrium entails either autocracy or democracy but not economic growth. Llavador and Oxoby find support for these implications from qualitative investigations on growth and franchise expansion across 11 countries in the 19th century.

Dower et al. (2018) build on the Acemoglu-Robinson model, focusing on 19th century imperial Russia. In this case institutional liberalization consisted of local representative institutions that gave voice to the poor, rather than on suffrage in national legislative elections. Formally, they use a continuous version of the Acemoglu-Robinson model, where elites choose a degree of power for the masses rather than a binary choice (democratize or not). This both avoids some technical complications of equilibrium characterization in the Acemoglu-Robinson model, and maps more naturally into their historical setting.

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<sup>7</sup>However, Chapman (2018) argues that when the median pre-reform voter is middle class rather than poor, democratization can actually reduce public goods expenditure.

Galiani and Torrens (2019) invert the Acemoglu-Robinson logic in the context of the American revolution, to explain why British Whigs opposed solving the crisis by granting Parliamentary representation to the Americans.<sup>8</sup> The issue is, once again, a commitment problem. This time, it lies with the unrepresented actors: Galiani and Torrens argue that the colonists, if represented in Parliament, would have been tempted to align with the democratically-inclined British opposition, which would have threatened the ruling Whigs' power in their own backyard. Thus the optimal choice for the ruling British Whigs was to prevent Parliamentary representation for America, and risk revolution.

Looking to deeper foundations of assemblies, Leon (2020) argues that England opened the long path to democratization with the Norman conquest. To recruit collaborators against baronial revolts, William I extended elite status and legal rights to a relatively large group. Leon models elite status as a partially rival club good. Compensation by expanding the elite was a self-reinforcing strategy for the king, because the proportional cost of admitting new members declines with the number of members. Crucially for Leon's account, elite status brought expanded legal rights and remedies, which eventually formed a core of its democratization. Thus, path dependence following the Norman conquest led to expanding rights in England.

In a creative study, Penn (2009) analyzes the effect of democratic representation on the formation of collective identity (thus taking representation as the explanatory rather than dependent variable). In Penn's model, individuals can identify with either the nation or their community (e.g., their state), and this identification depends on the well-being of each unit. In equilibrium, identity choices determine a policy, which induces the identity choices that produce the said policy. Penn shows that when state-level interests are correlated with population, then large groups can dominate small ones in policy making under fully proportional representation. Further, members of dominant groups may be unwilling to choose identities in common with members of dominated ones, so proportional representation can inhibit formation of a common identity. Malapportionment, by providing for some representation at the group level, mitigates group-based domination in policy making. Therefore, federated representation can better promote formation of collective identity than fully proportional representation.

## 2.5 Political Reform and Development in Party Systems

Models of franchise extension typically sidestep the issue of whether elections are fair or corrupt. Several scholars have addressed the institutionalization of "clean" elections, again focusing on 19th century Britain. Eggers and Spirling (2014) model the decision of candidates to engage in corrupt election practices as a function of the quality of post-election monitoring. In their model, corrupt electioneering is costly for candidates, but increases their chance of winning. Corruption is punished after the fact by imperfect monitors. The key result is that electoral

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<sup>8</sup>See Defigueiredo, Rakove and Weingast (2006) for a model of how this crisis emerged, emphasizing incompatible beliefs of the British and Americans.

corruption declines in the quality of monitoring. Eggers and Spirling map this model onto a change in British election monitoring in 1868: up to that time, monitoring was performed by tribunals drawn from Parliament, but subsequently was delegated to courts. Through a variety of ingenious indirect measures, they show that (i) Parliamentary tribunals had substantially higher error rates than judicial ones; (ii) petitions declined but convictions rose after judicialization. This second result is exactly the pattern one would expect if judicialization actually deterred corruption.

Camp, Dixit and Stokes (2014) analyze the decline of political parties’ use of “agents” to recruit votes in 19th century Britain—analogous to American political machines. They argue that agents were increasingly ineffective late in the century, but parties were locked in a prisoners’ dilemma over campaign strategy so continued using them. Their model implies that, given declining benefits of agents to parties, the penalties imposed by the 1883 Corrupt and Illegal Practices Act catalyzed parties’ exit from this prisoners’ dilemma.<sup>9</sup>

Kam (2017) presents a bargaining model between candidates and voters in the market for votes. The key to the model is that a market exists only if the gain to candidates from voter bribery exceeds a voter’s opportunity cost of voting. This intuitive finding has the important implication that a secret ballot can collapse the market for votes, by decreasing candidates’ assessment of the efficacy of vote buying. This causal mechanism explains Kam’s finding that bribe prices declined sharply with the introduction of the secret ballot.

Miller and Schofield (2003) shift the focus to partisan electoral realignment, considering the case of the US. They present a two dimensional spatial model to argue that party realignment is effectively a slow-moving manifestation of coalitional instability in multidimensional space. The key assumptions that generate equilibrium platforms in any period, and make the instability slow moving, are that (i) voters vary in the salience they attach to various dimensions, and (ii) candidates need the support of activists. Activists endogenously cluster according to policy preferences, including dimensional salience. Therefore, in equilibrium platforms candidates choose platforms that appeal to activists on their high salience dimension, but outflank them on low salience dimensions to appeal to disaffected voters. Miller and Schofield apply this model to explain changing party positions in the US from 1896 to 2000, specifically how the main cleavage between parties shifted from social to economic and back to social again.<sup>10</sup>

### 3 Taking Stock of Formal Models in HPE

The survey above indicates that the formal HPE literature has made important contributions in several substantive areas. At the same time, few strands of formal work in HPE have produced

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<sup>9</sup>Camp et al. argue that the act was a *catalyst* of electoral reform, in the sense that it accelerated the decline of machines, but not a *cause* in the sense of a necessary condition. However, the act is a “cause” in the sense of the potential outcomes model of causation.

<sup>10</sup>See also Schofield and Miller (2007) and Schofield, Miller and Martin (2003).

a cumulative research agenda about specific historical events or processes (the chief exceptions being persistence of inefficient institutions, and franchise extension). Most formal research in this area develops a bespoke model applied to a specific process or event rather than building on other formal models in HPE.

In my view this state of the literature reflects an implicit consensus about the role of formal models in historical analysis. Scholars use formal models to depict and analyze a causal mechanism that explains how specific historical events unfolded. By definition, all formal modeling in HPE involves some attempt to connect a model to historical evidence: a pure theory paper with no historical referent is not an HPE paper. The best use of models in HPE must remain closely tied to historical evidence, and probably heavily tied to historiography written by historians. To contribute to HPE, it is not much use to study and critique the formal models in HPE as models. It is necessary to understand the historical process that previous models are trying to capture, and to adduce historical evidence that these models ignore or contradict.

Despite some apparent consensus, there are distinct approaches to connecting formal models and historical evidence. These approaches can be placed on a continuum corresponding to how systematically they engage historical evidence before constructing a model. At one pole are models developed with no particular reference to specific historical events. Typically the modeler engages with historical evidence by searching out cases consistent with the model. This is used primarily as “proof of concept” for the model, i.e. a demonstration that its logic has arguably applied somewhere and at some time in recorded human history. However, engagement with historical evidence tends to be superficial.

The polar opposite approach is to start with an existing and known body of historical evidence, and produce a model to explain it. Scholars following this approach essentially “reverse engineer” a strategic environment, the equilibrium of which corresponds to the known evidence.<sup>11</sup> These models obtain their value from demonstrating an underlying causal logic, and thus helping us to understand how important events or institutions in the world came about. They may not even be intended to ever be applied to any other empirical setting. Clear examples of the “reverse engineering” approach are Greif, Milgrom and Weingast (1994); Lizzeri and Persico (2004); Gailmard (2017); Gailmard (2019); Gailmard (2021*a*); Koyama, Rahman and Sng (2021); and Peña (2021).

Most formal research in HPE falls somewhere between these poles: models are specified from some historical evidence, but other evidence is set aside (or identified after model building) to use for evaluation. A typical approach is to take a body of historical evidence and partition it into two parts. One part describes a “decision environment”—an institutional configuration, a sequence of actions, and evidence about preferences. The other part describes historical

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<sup>11</sup>Reverse engineered models are the same as the dreaded “just so story” (Elster 2000). Just so stories are supposed to be problematic because they may not generalize to other events. But if one’s objective is to explain a specific event, it is not clear why it matters that the explanation is *sui generis*. Sometimes specific events have specific explanations.



“outcomes,” which the process is supposed to explain. The modeling and historical assessment works as follows. First, historical evidence on the decision environment is used to construct a formal model: specifying utilities, action sequences, and information sets that seem “reasonable” in light of historical evidence on the decision environment. Second, the model is solved. Third, the solution is assessed against historical evidence on outcomes. Clear examples are Eggers and Spirling (2014) and Garfias and Sellars (2021). The explanation consists of a model to depict a causal process, and empirical test of the correspondence between the model and data. When the correspondence between model and data is strong (e.g., estimated parameters take the hypothesized sign), it is held as evidence that the causal mechanism in the model is a reasonable depiction of the historical process.

Despite differences in process, in each approach, the models depict a causal mechanism, and the evidence shows the consistency of this mechanism and historical observation. This is held as a reason to believe that the mechanism depicted in the model actually operated in the case. No approach shows that its causal mechanism is the only reasonable depiction of a historical process, and no approach shows that its causal mechanism applies to any processes other than the ones around which it was designed and evaluated. Indeed, no approach can do these things (cf. Gailmard 2021*b*).

However, in any of these approaches, a benefit of formal models is that they allow some basis for causal generalization across social and historical settings. Roughly speaking, all models have two elements: an “analytical core” and “contextual information.” The analytical core is an entrant in the “library of mechanisms” that political economists draw on to explain events—collective action problems, coordination problems, dynamic inconsistency, principal-agent problems, and the like. Contextual information makes claims about relevant actors, preferences and decision rules, information sets, and sequences of action. This element is more situation-specific and almost always based on some historical evidence; it may not generalize at all beyond a fairly circumscribed class of situations or even one particular event.

The analytical core of a model provides a bridge to show how superficially distinct social and historical events may be driven by the same causal mechanism. That is, two situations may have very different contextual factors (time period, place, actors involved, etc.), but if models explaining them have the same analytical core, they are strategically or theoretically similar. In this sense, theory provides a basis—I would argue the only available basis—for generalizing findings from a specific case to others. However, even when empirical findings in a single case are rigorously causally identified, we should resist the temptation to infer that theoretical generalization implies the same empirical credibility in other cases.

Though all approaches share these deliverable payoffs, the reverse engineering approach seems subject to particular skepticism or criticism. This criticism overlaps with criticism of “analytic narrative” (AN; Bates et al. 1998): reverse engineered models are often models of a single case or small number of cases, which coincides with at least one definition of AN (Defigueiredo, Rakove

and Weingast 2006).

One type of criticism is that there is nothing to learn from such an exercise (e.g. Clarke and Primo 2012), because any one case can be explained perfectly by some model (usually a variety of models). In my view this reflects a misunderstanding of what we do with models, which is not simply to explain or predict outcomes, but to depict causal mechanisms. Any single case can be perfectly explained by some model(s), but not by every model. Therefore, when a model is put forward that can explain it, we learn something about both the set of candidate explanations for the case, and about the reach of whatever causal mechanism is embedded in that model. These seem like valuable types of knowledge.

Reverse engineering also runs afoul of the supposedly scientific prohibition on observing outcomes before developing theory. Again, in my view this prohibition is misguided. If one's objective is to understand the range of causal mechanisms that can produce an outcome, it makes no difference whether the outcome is observed before the mechanism is developed theoretically. All that matters is that the mechanism is consistent with the empirical observation; if it is, the mechanism is a candidate explanation.<sup>12</sup>

Whatever the process of interfacing models with historical evidence, the explanation in a model is only as good as its assumptions. In game theoretic models, these amount to statements about the decision environment (actors, actions, information sets) and motivations of actors. In rational models—which predominate in HPE to date—motivations come down to maximizing a consistent preference relation in light of correctly derived beliefs about uncertain variables. When a model makes far-fetched assumptions about a decision environment, it is usually easy enough for critics with subject-matter expertise to pinpoint them. Thus it is usually the behavioral postulates of rationality that are highlighted as particularly problematic or unreasonable (e.g., Elster 2000). After all, haven't we known for a long time that people are not rational?

An issue with this line of argument is that rationality is a property of decisions (more specifically, sets of decisions), not of people. Sometimes our choices are rational, sometimes they are not. If a rational model accounts for a set of decisions, it seems odd to set that explanation aside because in some other decision problems, some other people have behaved irrationally. Methodologically, a search for rational explanations for historical events is useful because rational decisions are always intelligible to a reasoning observer—possibly tragic or frustrating, but intelligible nonetheless. And making the world intelligible is a recognized goal of both scientific and humanistic inquiry.

In short, formal models are built in HPE through a variety of processes. The fundamental similarities across these processes are that (i) at least some historical evidence is used to specify the model, (ii) models depict causal mechanisms that, insofar as they imply relations

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<sup>12</sup>It may be objected that simply proposing a candidate explanation is insufficiently ambitious, and we would like to isolate a single causal mechanism that accounts for case evidence. However, because there are essentially always multiple explanations for a finite body of evidence, this is probably impossible. Even the most rigorous causal identification does produce not a single reason why one variable causes another; it simply rules out confounding as a reasonable explanation.

between variables that are observed in historical processes, are candidate explanations for those processes; (iii) models supply *reasons why* treatment effects exist that cannot be supplied by empirical identification alone. *Formal* models are particularly useful because they depict mechanisms with unusual clarity; they ensure logical consistency of arguments; and they enable causal generalization across cases that are unified by the operation of a specific theoretical mechanism.

## 4 Conclusion

Historical political economy should exist because political economy provides powerful theoretical tools to understand decisions and institutions, and therefore the reasons why the world has developed as it has in social, political, and economic terms. Formal models are important in HPE because they offer unmatched precision and clarity for formulating and communicating those causal explanations. In addition, HPE is important for formal modelers because history furnishes cases of important dilemmas in governance and political decision making that can expand the library of mechanisms that political economists can deploy to understand the world.

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