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Mesopotamian Political History: The Perversities

Abstract: This essay outlines approaches and problems in writing ancient Mesopotamian political history. A brief review of Assyriological studies is contrasted to political history generally. What follows are six points of theory which present problems and opportunities for moving these studies forward, based on a refocus away from the state; the strategic use of ambiguity by political entities; the role of social forgetting; the productive use of absences of evidence; a renewed effort at period history; and an appraisal of environmental explanations of historical change.

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Introduction

Writing Mesopotamian political history has always been assisted by a richly detailed (if unevenly distributed) primary record documenting the emergence of the state and kingship, and the development of those forms through time and space. This primary record inevitably presents limitations on interpretation, of course; but these limits are sometimes also aggravated by our mimicry of them, or by the misapplication of theory, method, and narrative, in some traps and truisms of our thinking. What do we think political history is? Or, better, what do we think it's supposed to *do*? And how do accounts of Mesopotamian political history compare with what other political historians and political scientists (can) do?

This essay will first outline the types of work that scholars of ancient Mesopotamia do when they work on political history: state history; chronological studies; political economy; ideology and rhetoric; the “middle management”

of city and state officialdom; and cultural-historical studies. Here I will try to give some sense, especially for non-Assyriologists, of the scholarly and intellectual strengths and weaknesses of these types of research. Second, I will consider briefly the concerns of political historians and scientists working in other fields and think about the ways in which those concerns often differ from (but also increasingly inform) the six well-established ways in which we already work on political history.

Third, I will advance a series of propositions to move beyond the ways in which we now write the political history of the ancient Near East; to point toward the exit from various theoretical dilemmas we face. These propositions are all counter-intuitive to some degree or another – hence “perversities.” The questions they produce, which ask of antiquity some of the questions we pose for modernity, are hardly tailor-made for our sources, given the anachronisms we must filter out. But they clarify the gap between what we can know and what we might nevertheless legitimately wish to ask in our study of these political societies, as of any other.

In these discussions, I do not pretend to “cover” either 3,000 years of political history or any particular body of historical theory. The field of Mesopotamian political history is too broad to afford anything like a full statement of the questions-of-the-field or an exhaustive review of the relevant secondary literature at the length of an essay. This is, unabashedly and necessarily, a subjective, even personal account of history-as-practice. Nor can the Perversities function as a research agenda; but they give names to a rogue’s gallery of interpretive problems that pervade our secondary literature, ones that deserve to be raised for more focused attention.

Mesopotamian political history: what we do

What is “political history” in Mesopotamian studies? It can be characterized in the six areas of research outlined in the next few pages, especially to orient those less familiar with the architecture of Assyriological scholarship. Before plunging ahead, though, it is crucial to qualify that the nature and availability of ancient Near Eastern sources has always conditioned the types of work which can be done, producing a “peculiar marriage of history and archaeology.” Something like this is of course true for all historical fields, but the superabundance of attention to state and bureaucratic apparatus in Mesopotamian studies is partly a result of several idiosyncrasies: the places in which texts have been found; their specific functions; their patterns of preservation; and “the questions that most easily present themselves based on those preserved

sources.”¹ I do not intend to ignore these irreducible ground conditions in posing other, further questions; nor do I mean to suggest that the study of Mesopotamian political history could or should have developed thus far in very many dramatically different ways. These boundary conditions must be recognized, but it is unhelpful to simply decry them or the terrain so demarcated and stop there; what is important in acknowledging them is to show that more terrain yet lies beyond. As Dickens said of old Marley’s death, “This must be distinctly understood, or nothing wonderful can come of the story I am going to relate.”

So: Mesopotamian political history is first of all dynastic history, the history of states (city-states, territorial states, and empires) and international relations.² This arena still offers the lively action of the discovery of evidence to a degree unlike many other historical fields, since lost cities, unknown kings, and forgotten wars are still being discovered and recovered by cuneiformists and archaeologists on a fairly regular basis. This is all amplified by the fact that, unlike Greek, Roman, and Biblical studies, Assyriology is an “open corpus” field, with hundreds of thousands of documents yet untranslated. This presents an advantage in the sense that Assyriologists may yet reasonably hope to fill in some gaps in historical puzzles about little-known states or the origins of obscure dynasties. It would be cantankerous to deny the pleasures of this adventurous sort of work. On the other hand, some expectations that political history should be *primarily* constituted by this kind of research has had a retarding effect on pursuing more abstract and theoretical questions about the nature of political institutions, actors, and processes. Many are the articles which conclude by demurring from conclusion, deferring answers until we know more of the “basic facts.”

Second, chronological studies are sometimes received as a kind of political history. Most chronographers themselves don’t necessarily make the claim that their work is inherently explanatory or analytic of historical issues; indeed, much of it is considered disinterested of and predicative to such analyses. The most important of such projects is the challenge to reconstruct an absolute chronology of the second millennium BCE – then on to the third! – through sources which maddeningly enough seem to conflict just as often as

¹ I thank one of my anonymous readers for making the foregoing points and for the felicitous language quoted here.

² Relevant surveys of ancient Near Eastern history include Kuhrt (1995), Van De Mieroop (2007), and Frahm (2013). See also Garfinkle (2013) and other relevant essays in that volume.

they agree.³ This is a tempting area of research with potentially very large payoffs: a proper sequence of events would have macro-regional implications for the histories of Egypt, Anatolia, and many other parts of the ancient Near East. Again, however, though the evidence is all “hard” (dendrochronological, calendrical-astronomical, lengths of reigns, etc.), scholars must defer the treatment of most of the signified historical problems (e.g. explaining why it would *matter* when Babylon fell to the Hittites) until final reconstructions are accepted.

Third are economic studies that aim to explain the political relations of institutions, non-state sectors, and the actors who moved between them.⁴ These types of approaches are useful for every major Mesopotamian period in which economic and administrative data are plentiful – perhaps most prominently in Ur III and Neo-Babylonian/Achaemenid studies, with the Early Dynastic and Neo-Assyrian periods somewhat less well represented. Political economy has mostly been examined through the study of archives, a methodology with the advantage of explanation through emic terms and structures. The drawbacks, however, have much to do with our imperfect grasp of these same concepts, the thorny problem of making analogies to modern economic forms, and an inability to assess the relative importance of economic data and information in relation to the scope of the wider economy (in terms of both absent and non-existent documentation).

A fourth area of political history is the study of ideology – usually the royal ideologies of large and durable conquest states.⁵ The official principles of state organizations have not been so difficult to reconstruct, since our sources often articulated them in cartoonishly bombastic language: the king as shepherd of his people, fearless warrior, and wise judge; the state as the locus of order, with enemy lands the site of disorder; dynasties as revivals of primeval orders; and so forth. Attempts to correlate those state ideologies with the tenets of temple religion or the discourse of civil society, however – that is to say, to argue that they were based on broadly-shared ideals – have usually met with less than convincing results.⁶ Thus on the one hand certain concepts are generally accepted: that, for instance, the palace institution was a “household” with the king as *pater familias*; or that the king had a special relationship with certain

³ See, e.g. Gasche et al. (1998) and Roaf (2012); and the ongoing ARCANE project at <http://www.arcane.uni-tuebingen.de>. “Chronology was also the subject of a workshop at the July, 2013 Rencontre Assyriologique Internationale in Ghent, Belgium, organized by K. R. Veenhof and G. Barjamovic.”

⁴ See, e.g. Steinkeller (1987), Yoffee (1995), Renger (2001), and Van De Mieroop (2004).

⁵ See, e.g. Larsen (1979) and Michalowski (2004).

⁶ A few successful and recent analyses of the political dimensions of ancient myths – populated almost entirely by gods and semi-divine heroes – are found in Liverani (2004); see also the brilliant and wide-ranging essay of Larsen (1987).

gods and temples; or that particular economic principles can be discerned in Mesopotamian political speech (contract language, reciprocities, etc.). Yet on the other hand, such connections tend toward the general, retain an artificial feel, and are usually fairly accepting of the premises advanced by state institutions.

A presumption that state ideologies were outgrowths of social ones is also falsifiable: where in Mesopotamian royal ideology, for instance, do we find some trace of the social and philosophical pessimism that infuses much of Sumerian and Akkadian literature? What traces of Mesopotamian religious imperatives about sacrifice translate meaningfully into royal literature? How comfortably did royal claims of innovation and historical “firsts” rest among other precepts which held the *past* to be the location of perfect forms, against which change was coded as devolution? It is hardly impossible to give some kinds of answers to such questions, but it must also be admitted that it was in the nature of political ideology to assert its own precepts *over* those of other spheres of society. Full compliance with social ideals and principles was not a desired much less achievable goal for the state (cf. below regarding ambiguity and hypercoherence). That being the case, the ways in which state rhetoric deviated from more strongly emphasized cultural themes are as instructive of how ideology worked as their isometry or interconnection with them.

A fifth aspect of Mesopotamian political history are its studies of institutions and actors at the level of management and mediation – of magnates, assemblies, and scribaria – the non-royal people who made political life work on the ground and circumscribed its in- and out-groups, sociologically speaking.⁷ This is a rich field of study to be sure; institutional and commercial documentation offers us nothing if not a close view of the day-to-day business of the offices and bureaus that made states and cities run. Such texts are highly self-referential and revealing of little outside their immediate concerns. But an even deeper problem in using such texts lies in the *a priori* presumption of their effective instrumentality in forging political relations; this approach uncritically reifies their importance. Yes, perhaps the fact that a grain delivery text shows that official “X” had control of 24,000 liters of grain seems an index of his ability to make and exercise political relations; but without a context of scale for such transactions, even the largest archives will remain impossible to evaluate for importance. How important was 24,000 liters of grain? And therefore official “X”? In fact, how important was “important”? Anyone who has actually done a study of institutional life from a body of cuneiform texts has shared the nagging feeling that almost every historical agent one discusses from the textual realm

7 See, e.g. Mattila (2000), Seri (2006), and Michalowski (2010).

becomes inevitably transformed into an “important” person, or an “elite.” The conclusions that are possible to make about such things are a fragile web of contingent and carefully-reconstructed characterizations.

Sixth and last, we come to political accounts of cultural products: of literature, ornament, scribal training, and the styles of palaces. As in other fields, such themes were once established as discrete historical topics. But since political power has been re-conceived as relations and discourses grounded in all kinds of practices, one finds the political in almost every nook and corner of every text and artifact. These multifarious channels reveal ever more subtle ways in which political values such as loyalty and submission were reiterated and propagated, or dissent and alterity expressed. The past generation has thus seen a bonanza of analyses of the literary qualities of political speech, as the “linguistic turn” affects how we read historical sources.⁸ The diverse applications of these scholarly approaches defy easy definition (though we may feel we “know them when we see them”), but they represent a way of thinking that has breathed new life into imagining ancient political dialogues by extending the subject matter: because, like the giant in the story, the historian should be omnivorous.

In a more pessimistic frame of mind, though, one could also say that there has been, in seeing power everywhere and in all practices, an accumulation of more and more ways to say in effect that political power was propounded through every verbal, figurative, and practical repertoire. This may be so, but without a healthy dose of counter-argument for the autonomy of genres and practices, or for the role they played in forming counter-narratives, having all sources generally accrue to the state aggregates toward the almost certainly erroneous conclusion of its omniscience and omnipotence. To imagine state power as being this ubiquitous and comprehensive, even in this patchwork way, is not much less credulous than the simplistic conception of “Oriental despotism” which it hopes to complicate.

New directions

One could tinker endlessly with heuristics and typologies, but most political histories of antiquity in the Near East could be adequately corralled under one or more of the six aforementioned headings. Now to compare a little: what do other political historians do when they “do” political history? What does their work

⁸ See, for instance, Michalowski (2011, esp. chs. 2 and 4), Van De Mieroop (1999), Cooper (2001), and Tadmor (2011).

offer ancient historians, and what steps have been taken to follow up on those opportunities? Modern political history, of course, has had the luxury of solving most informational questions (the precise dates of events, location of places, sequence of rulers) and to some degree now consigns those types of inquiries to the realm of pedantry (though still necessarily dependent on and engaged in them). In this way, the ancient historian, still struggling after basic facts at the same time that he wishes to examine their significance, works amidst a majority scholarly culture in which many of his intellectual concerns may seem small-minded.

Despite its durability in practice,⁹ the disciplinary identity of political history is hardly as straightforward as it might seem. To make a contrast, while political *science* is rich in reflective “state-of-the-discipline” self-appraisals and organized as a distinct academic unit and scholarly subject, political *history* has a fairly low disciplinary profile, usually submerged within individual national-historical traditions – where political historians of Victorian England or post-war Japan work first within the framework of British and Japanese history. In this respect, Mesopotamian political history is hardly different. Book titles in political history, while still addressing the familiar terrain of modern states and their affairs, also include a bewildering array of topics under the heading. These might seem to the Assyriologist to cover virtually any- and everything: there are political histories of friendship; of wealth, of welfare, of fatherhood; of languages, humor, and religion; even other disciplines like medicine and sociology. Thus as a rubric “political history” remains astoundingly productive at the academic and popular level of historical publishing even while, as an independent discipline, it is hardly as coherent as one might imagine.

The variety of political history, however, reflects one great lesson for ancient historians: the benefits of expanding analysis into areas which do not valorize the state as the sole arena of the political – into domestic space, civil society, and material culture. In this, we detect the hand of social history directing political analysis away from elites; of a literary arm, arguing for “discourses” distinct from “ideologies”; of anthropology, seeing polities and civil societies as mutually structuring; of sociological influences of the “New

⁹ See, however, the May, 2001 special issue of the American Historical Association’s *Perspectives on History* devoted to political history; Pillarisetti Sudhir, writing in the introduction “Plural Perspectives on a Protean Culture” notes that “Even as numbers of the self-professed aficionados of other subfields changed over the years, perhaps reflecting shifting historiographic inclinations, the number of AHA members who declared their primary field to be political history stayed steady in the past [35] years.” Self-identified political historians held steady between 3.7% and 3.9% of all members between 1975 and 2010.

Institutionalism.”¹⁰ Political historians today think about unofficial cultural discourses and contexts as not merely shedding light on political life, but as forms of political life in and of themselves. In these developments, political history has enjoyed extensive and fruitful cross-pollination with other fields, if paying for it with some consequent dilution of its disciplinary identity.

Many political scientists, by contrast, have become occupied with demography, statistical analysis and the model-building that permits prediction for decision-making, policy, and behavior (especially voting behavior); theorists in political science stand at the disciplinary edges of their field.¹¹ That is an enormous generalization, but I make it to call attention to two areas of political science in which theory plays a larger role: comparative politics and historical geography. Both of these fields are interested in political life outside of sovereignty models, in forms of politics neither bounded by nor centered on the state. The important dimension of comparative politics, as both method and discipline, is its focus across structural levels, concerning itself with different scalarities of non-, sub-, and supra-state level political institutions and phenomena. Historical geography, meanwhile, despite its long history as the handmaiden of cartography and colonialism, has undergone a re-invention: now the conceptual and social impacts of geographic and landscape awareness are the true subjects.

Both political history and political science call us back to a better question: not just “what is” political history; but what does political history *do*? What do we *need* it to do? What contingencies and counterfactuals will tell us about the political questions and problems the ancients themselves faced? The goal becomes one of using political history not merely to make a checklist of political institutions and actors for description, but as a way to expose the processes in which they engaged. This alters a conception of political history from an ideal type, something to be “done properly,” into an instrument of explanation for what is by definition unknown. Certainly we want to flesh out dynastic details, define legal procedures, and finalize a second millennium chronology. But – just to unfairly pick on one particular issue for the sake of illustration – were we to finally discover *when* the first Sealand monarch ruled, it should take us about 3 minutes to turn to the questions we *really* want to have answered from that

10 The term has come to cover studies of the influence of institutions on a wide variety of social practices, behaviors, and human cognition; historical institutionalism, however, has mostly been overshadowed by the prevalence of this approach in economics (especially through the work of Douglass North) and sociology.

11 See the centennial volume of the American Political Science Association, *American Political Science Review* 100 (2006), entitled *The Evolution of Political Science*.

information: why *then*? And *how* then? From the time we spend focused on apparently obscure and detailed problems, it is hardly clear to other scholars that we want to know more than it sometimes appears we do. Being explicit about what we ultimately want of our studies, as far as those wants might be from our means, would be a good first step in communicating the stakes of our projects.¹²

The reader might by this point have gained the impression that Mesopotamian studies are out of touch with contemporary political history; that they are well beyond traditional and positively hidebound. This is hardly the truth: the past decade has actually been a lively one for fresh work on Mesopotamian political life. I think here of Dan Fleming's (2004) attention to subscription and alterity as components of political communities; of Adam Smith's discussion of sovereignty as "assemblage";¹³ of Norman Yoffee's (2005) emphasis on the heterogeneity of archaic states; of Seth Sanders' (2009 and 2012) arguments about polities as "grounded in communicative form;" of my own work on "presumptive states," persuasion, and the production of political membership.¹⁴ The Topoi project in Berlin has given priority to issues of "governed space";¹⁵ Gebhard Selz (2002) to conflict management; Mario Liverani (2010) to political rhetoric and audience; Henry Wright (2006) to the early state as experiment. There is excellent new work on the politics of urbanism, diaspora, and the pristine state; on governance and territoriality; and a seemingly endless stream of anthropological studies of the early state.¹⁶ One could go on and on: there are many to whom we are grateful; it is in fact a great time to be working in Assyriology if one likes to think about such things.

But structural-theoretical problems have kept this work from having the impact it could have. Let me name two obstacles: first, Assyriology is a cottage-industry in supposing it can and should do all of its own social sciences work. We have Assyriologists who *work on* economics, demography, political science, and so forth, but few of us – myself included – really have any training in social scientific disciplines. Second, and more importantly, we continue to talk about political forms as ideal types more than we admit, and we think of the job of political history to be one of explaining how things work. That's why we

¹² See the excellent summary of issues in Van De Mieroop (2013).

¹³ Smith (2003 and 2006), but especially (2011); and his forthcoming 2013 Rostovtzeff lectures delivered at the Institute for the Study of the Ancient World.

¹⁴ Richardson (2012a, 2010a, and 2014a), respectively.

¹⁵ See the research agenda of Baltrusch et al. (2011).

¹⁶ See, e.g. Bouchard (2011) and Bondarenko (2008); for a view from economics, see Mayshar et al. (2011).

focus on officials and assemblies and cities and tribes, because we think if we can make an account of the cogs and wheels which aren't the king and the state, we'll figure out how everything worked. But as anybody who knows anything about politics knows, political life is just not this well-organized. Not every historical actor who *affects* political life is *involved* in political life (e.g. non-state actors), and vice versa; not everything important emerges toward systematic homestasis; no ideal type comes close to existing on the ground. Not everything works.

Not everything works: what if we took that idea seriously, as a point of theory? What if we recognized it as more than commonplace cynicism and used it in a productive way? What would a "political history of things not working" look like? The purpose of the rest of this essay is to chase that rabbit down the hole and see where it goes. Some of the following observations are about theory, some about method; I have organized them as sorts of slogans, which seems appropriate to the political. They focus on how political life didn't work, didn't fit in the boxes and flowcharts we want them to, perversely going against our instinct to explain, define, and understand (and not to imply their perversion of any stable paradigm in antiquity itself).

This little bottle of toxins, all inversions and reversals, I call "The Perversities." These anti-principles won't lead us to Truth or Heaven, but they can add some dimensionality; widen the historical frame; test with counterfactuals; and bring new subjects onto the menu of what we consider "political." The Perversities are:

1. No more state fetishism.
2. Power likes ambiguity.
3. History is also about forgetting.
4. Incompletes, opposites, and absences are evidence.
5. Allay the *longue durée*.
6. Environment is not explanation.

The Perversities

No more state fetishism

A prominent critique of mainstream political history of the past generation has been the insufficiency of the state as topic, framework, or explanatory model; many have sought to rebuild a political history focused on behavioral and non-institutional categories. The robustness of these challenges is reflected in the

existence of an entire counter-movement to “bring the state back in,” a now decades-old debate which has hardly run out of steam.¹⁷ I do not mean to entirely dislodge states as important frames of study, but since Mesopotamian political history is already overabundantly built around their edifices, it is vital to introduce the counter-arguments; and these require a little more discussion than the other, following points.

Nation-states are creatures particular to the modern period, many historians argue, and the state as a transhistorical form is a questionable proposition. There are both progressive and regressive forms of this critique. The progressive and neo-liberal forms cluster around watchwords such as globalism, cosmopolitanism, and trade diaspora – all the transnational phenomena of an emergent, supposedly post-national world.¹⁸ Focus on these issues stresses the state as a retarding factor on flowering political communities formed as communicative networks rather than sovereign entities; communities with identities of class, project, or mobility, extending across an unfolding world of opportunity and free movement. Although such actors and groups are obviously primarily associated with – or even unique to – late capitalism, they have not failed to remind ancient historians of the merchants, mercenaries, and magnates of antiquity whose reference groups were geographically-distributed networks, rather than local residential members of territorial states.¹⁹

Then there are regressive critiques, such as world-systems theory, a postulate which presupposes global inequality and the asymmetric distribution of wealth and power.²⁰ Other regressive “failed state” approaches focus on sub-state phenomena like demographic pressure, the factionalization of elites, or ethnic conflict, privileging fault-lines in state unity as political demarcations just as important as the state itself. Scholars following these subjects look warily at a post-national world made up of warlords, narco-syndicates, kleptocrats, and rogue states²¹ instead of the more optimistic neo-liberal world of jet-setting capitalist elites and multilingual global citizens – where our violent future

¹⁷ See Vu (2010); cf. Baltrusch et al. (2011: 5), on the revival of the civilizational approach.

¹⁸ See the review article of Michel Bouchard (2011).

¹⁹ See, e.g. Baltrusch et al. (2011: 4–5): “...the focus [of new, transdisciplinary concepts] has shifted from national and Eurocentric perspectives toward transnational and global ones – most conspicuously since 1989.”

²⁰ This dark core of world-systems theory is too often forgotten when ancient studies casually use the term in celebratory fashion, mistaking it for its neo-liberal analogues.

²¹ In some ways, Charles Tilly led the way in focusing on these forms *as* political societies; to choose but two recent examples (of many), see Ahram and King (2012) and Byman and King (2012).

looks like the worst imaginable version of the deep past, but amplified by weapons technology and structures imitative of state organization.

The Fund for Peace's "failed states" index, which tracks individual countries slipping out of statehood, counts as many as two billion people in the world today (about 28% of humanity) living under failed political orders.²² What began as the observation of a seemingly temporary problem of the post-colonial world has become a subject in and of itself, as huge patches of the earth have remained outside of state organization. It has become apparent that state failure is not a transitory phenomenon; this in turn requires modeling "failure" itself as a valid (if undesirable) mode of political interaction.²³ Even if we were to object that the dystopian futures these regressive images provoke are paranoid fantasies, we would have to admit that paranoid fantasies can still have very real effects on political life (e.g. through imperial military adventurism).

We don't have to think too hard to see the benefits of an ancient political history that makes an account of actors uninterested in state legitimacy: antiquity had its fair share of warlords and rogue rulers, and its own "28%" living under them. Communities organized under such authorities – however little we might think of them, however loudly ancient states may have deplored them – worked because they provided a semblance or simulacrum of the security and services states did.²⁴ Agents need not have expected or even aspired to possess the trappings of state power (e.g. religious justification, monumental architecture, dynastic descent, and historical presentation) to have been politically effective or important; indeed, probably no state is ever invested in every available mode of legitimation at once anyway.²⁵

To return the benefits of such an analysis to the political life of *groups* requires the end of fetishizing the state as the sole arena of politics in practice. As it stands, we exclude from political analysis *a priori* certain kinds of actors

²² Ghani and Lockhart (2008).

²³ Offe (2013).

²⁴ We do not even need to look to extreme examples to locate these para-political mechanisms in operation, e.g. the Italian *mafia*, Brazilian *favelas*, Mexican drug cartels, Hezbollah in Lebanon, and so forth. Such organizations are deemed illegitimate by some or all states since their authority is effected through unarbitrated or extrajudiciary violence. But that critique does not preclude the subscription to group membership in them by millions of people living today due to their routine performance of governance functions and participation in other legitimate structures. Somali pirates, for instance, have invested millions of dollars in the Kenyan real estate market; Hezbollah collects garbage and runs hospitals; Afghan warlords have proven effective in such governance tasks as restricting local violence, counternarcotics, and the building of infrastructure (Mukhopadhyay 2009, with literature).

²⁵ On "little kingdoms," see Richardson (2012a: 16–25).

and groups because they seem not to aspire to traditional power. We might, for instance, contingently privilege Yaminite and Sim'alite tribes as politically engaged when their ambitions intersected with the occupation of royal thrones;²⁶ but the political life of tribes less integrated in the life of the state, such as the contemporary Rababi or Aḥlamû, has not been a subject of much scholarship. What do we make of the “predynastic” centuries of Kassite tribal presence in Babylonia? Is our task only to reconstruct a trajectory that brought them from a stateless tribe into chieftaincies under princes and eventually into a dynasty? Is a postulate of aspiration to a Sumerian or Babylonian style of state power sufficient to describe their political role in the eighteenth/seventeenth century BC?

Kibitzing about what to label political forms is not a worthwhile goal; disimagining the state has to take a historically-specific form to become productive. I will try to give shape to this point with examples from my own work on the collapse of the First Dynasty of Babylon, which asks how the first place on earth to have adopted civilization also became the first place on earth to give it up. A non-state view has helped answer the question by formulating four different questions.²⁷

First: was the First Dynasty of Babylon a “failed state”? The Babylonian state's problems included a loss of control over space, constituencies, and discourse, meeting some of the “failed state” criteria. What's more, over more than a century, Babylon could look in every direction and see only non-state societies. This was something like reverse secondary-state formation, since “failure” too was produced in comparative terms. Second: was Babylon lost to “non-state actors”? Not in a single event, no; but sustained low-power competitions between warlords, tribes, merchants, and soldiers created the conditions whereby those groups were better able to deliver on guarantees of land, production, and transit than the state. The state's monopoly of power was thus visibly falsified to its constituents.

Third: was a non-state identity imagined? I think here about Babylon's asymmetric “borderlands,” communities which didn't just practice alterity in contrast to a central norm, but places for which liminality *was* an identity. There was the Euphrates corridor, where we certainly had warlords; the southern marches settled by diasporic city elites and new tribal dynasties; and a rural zone ruled as domain land by the Babylonian king, jurisdictionally distinct from his rule over his cities. The Babylonian state became an island in a sea of

²⁶ For an excellent discussion of these tribes and their relationship to the state of Mari, see Fleming (2009).

²⁷ Richardson (2002).

de-centralized authority, with kingship gradually emulating warlordism itself. Fourth: what happened to Babylonian state identity? Here, I think about ideological disenchantment. The epiphenomena of the late Old Babylonian period included changes in ritual *techné*, scribal training, titles, personal names, glyptic, royal inscriptions, year-names, and literary production. Together, these were severe alterations to the semantic field of power, changes extensive enough to destabilize the rules which legitimated authority. This was not just a loss of faith in this or that state, but a disenchantment in the state idea altogether, in favor of other identities.

It is not necessary to imagine wildly utopian or dystopian variations on the state to construct the alternatives that make politics, as a matter of multilogue, meaningful; a rich fund of topics exists between the extremes. We need better studies of minor kingship, for instance. We prefer to concentrate on big states, but minor kingdoms were numerically significant. What do we make of the many ephemeral rulers attested only in single inscriptions – in the inscriptions of others – or named only in lines of ancestry? Are we to conclude that the political record documenting a majority population of “little kings” merely describes failures and also-rans? Or is there some profit to reconstructing a modal way of politics revolving around “small kingdoms”? Small polities often had enduring political relations with other states without always having the exclusive capacity to act domestically as “legitimate organizational authorit[ies] to arbitrate and resolve disputes.” This distinction supposes an environment in which even the political authority of larger states was a “power *to*” organize clients rather than a “power *over* ... organizations.”²⁸ Thus political analysis should not be limited to entities with competing claims of legitimacy, but also work cross-scale between those needing legitimacy and those less and disinterested in it; concerns for legitimacy need not have been present or absent, but have existed situated in a hierarchy of other concerns. A focus on the pervasiveness of “small warfare,” on small-scale and cross-scale conflict, would similarly develop our understanding of political life beyond ideal types. Another fruitful area of study are vernacular or popular political discourses, forms of speech which challenged or were corrosive to authority,²⁹ even down to the level of family and individual disputes. These, too, are a politics of a kind, though it entails no necessary presumption that sovereignty had a role to play. And even when we do study states, a sort of hyper-particularism helps; to inquire of every object and practice whether or not it is (or is part of) the state.

²⁸ On an anarchic view, see Angelbeck and Grier (2012: 547, 551).

²⁹ Lincoln (1995).

A real opportunity exists for rapprochement with mainstream political history through attention to political life outside of sovereign authority. The theoretical affinities between accounts of the world before the emergence of states and those of the post-national future are promising, even beguiling, and should be developed by turning away from states as the central objects of study, perhaps for a generation. At this juncture, antiquity has much to say to post-modernity, and its historians are eager to listen.

As a final point, I will plead for a theory of incipience. I have argued elsewhere for an approach to states as “incomplete,” as forms in a state of becoming.³⁰ This accords with much contemporary thought about states as unfinished projects, about a pervasively “incomplete” modernity. This is an approach to historical states as continuously emergent, at no point fully accomplished – a point perhaps nowhere so important to establish as at the prehistory/history divide which ancient history guards with such vigilance. States did not just appear in the Early Dynastic period with an intact apparatus of territorialism, membership, legal authority, economic coextension, in a system among and of other homologous states.

No: the political issues we think of as prehistoric were still being worked out long into the historic period. Ancient states were not legally and spatially integrated; they were networks of local and professional actors with closely circumscribed goals and interests. In practice, Mesopotamian states had discontinuous powers – yet routinely claimed authority over spheres of action in which they could not effect or even did not want to accept actual problem-solving responsibility. This may seem counter-intuitive to us until we think of the claims themselves as conceptually real *desires* of the state – that they *wished* they could exercise such powers. In this sense political rhetoric was more erotic than documentary. But one still must account for eros: at the point when the desire for power was more potent than its actual exercise, wants influenced the course of political history more than abilities. The state was important as a developing form; but to fetishize it as the finished form *par excellence* is to have acquired, by purchase, a bridge of great dimension.

Power likes ambiguity

My second perversity follows on the first: power likes ambiguity. In the first states, we already see the application of a principle to camouflage the locus of power. Early states, like later ones, were more than eager to specify and police

³⁰ Richardson (2012a: 3–4, 28, 36, 44–45).

the terms of legitimate practice for the ruled, while reserving an exemption from accountability for the rulers. This camouflage can be a difficult point to explicate precisely because of the variety of state representations. Mesopotamian states developed multiple modes of political speech, from epic to annal to charter; through stele and sealing and statuette; to the piety of hymns and the bluster of justified violence. These rhetorical modes often have unexpected foretexts, from the epithetic quality of Middle Assyrian royal inscriptions, to the casuistic format of Hammurabi's law code, to the performative background of Ur III royal hymns. Though restrained by tradition, there was almost endless innovation in how Mesopotamian states spoke to their audiences: theme was complicated by genre and voice, and the nature of power thus blurred.

We tend to hear these voices cumulatively, as ever-expanding repertoires, broadening over time, with military accounts added to building accounts; then in pietistic, idealist, juristic, custodial, and munificent voices. The addition of these voices sounds to us like the expansion of powers in a fairly arithmetic progression.³¹ But I'd resist the urge to concatenate or reify these voices:³² instead of reconstituting ancient states according to claims of integrated powers they never completely realized, I propose that we are looking at an ambiguation of voice which deliberately hid the location and modest extent of political power. As in a carnival fun-house of mirrors, states spoke in multiple registers to achieve two political goals.³³ The first was to perpetuate a sense of omniscience, that states were everywhere and did everything; this we know already. But the corollary goal was defensive: to shield state authority from any external rationalizing analysis, a principle of concealment already encoded in Jeremy Bentham's idea of the panopticon, and one which brings to the forefront the question of state autonomy.³⁴ States constituted subjecthood by establishing criteria for membership; for *itself* the state preferred to be understood only so far as recognition made clear its dominance, but not so far as to be objectively

31 Baltrusch et al. (2011: 3) speak of the "diverse symbolic and concrete forms of governance" as the key to the success and stability of "politico-spatial orders"; but also acknowledge the role of consciousness of how these forms were constituted as the crucial analytic. Perhaps I am more pessimistic in positing that a deliberate blurring of forms was itself a strategy of governance.

32 Even though one of the intended effect of this "generic layering" was to buttress the state's claims through a *simulated* referentiality: on "generic layering," see Richardson (2014b: 437, 463, 504).

33 For a brief and highly entertaining account of this notion, see Battaglia (1997); see also Baltrusch et al. (2011: 3).

34 Mann (1984); cf. Bondarenko (2008).

apprehended as an entity like other entities.³⁵ States aimed to obtain maximum authority with minimal accountability; to avoid exclusive meanings which permitted the falsification of that authority, the visible precipitate of which was rebellion.

Looking at it this way, we'd be fools to try to find any "real" or "true" or "core" nature of the Ur III, Neo-Assyrian, Old Babylonian, or any other state. States put across polyvalent instead of exclusive identities to speak to multiple constituencies, each of which understood its relationship to the center in different terms. It seems at once facile to suggest this of ancient Mesopotamian states without a thorough presentation of evidence; yet impossible to prove. But an accretion of attributes seems to fit what we see: Sargon of Akkad perfected themes of military dominance and introduced temple patronage and cult of personality; Šulgi of Ur added knowledge arts and historical authority; Old Babylonian kings piled on jurisprudence and economic regulation; and so forth. The projection of these supposed grounds of authority meant that different audiences got different messages from the same states.

What I suggest is a way of reading the evidence not as fragments of a larger reality which should be reconstituted and rationalized by the historian as once-coherent ideologies, but as fragments of a *reality of dissonance*, of ideological pastiches created with materials at hand – a little religion, a little power, and a little justice – with states throwing ideas at the wall to see what stuck, and relying on the ambiguation of the nature of power to forestall the disenchantment of its audiences. For those living under this emerging early state "governmentality," ambiguation produced a sense of omnipresence and omniscience because the unbounded claims of the state were so hard to verify.³⁶ For scholars working today, the problem is very similar: since a presumption that powers and attributes were either full or fully integrated is methodologically

35 An analogous disposition lies behind theological conundrums, demanding of the believer a suspension of disbelief and sublimation through the miraculous.

36 Rose and Miller (1992: 272–73) term this the "mythical abstraction" through which the state was governmentalized, transforming it beyond a mere institution; cf. Baltrusch et al. (2011: 8). See also Adam Smith's forthcoming 2013 Rostovtzeff lectures, in which discussion of political communities runs as follows: "...sovereignty demands the reproduction of (at least) three key conditions: 1) a coherent public defined by relations of inclusion and exclusion that are materially marked and regulated; 2) the figure of a sovereign, cut away from the community by an apparatus of social and martial violence; 3) an apparatus capable of formalizing governance by transforming the polity itself into an object of desire, of care, and of devotion." The coherence desired and demanded by authority, however, had no bearing on its reservation of ambiguity for itself – but this itself is perfectly commensurate with the positions of the observed and the (concealed) observer in the panoptical model.

unsound, how can we avoid mimesis? How can we disprove unbounded power? This question wants a *method*, something more than just the suspicious disposition of the reader.

States also achieved the resilience of ambiguity internally, by creating “Chinese walls” between their own bureaus, within their own structures; the camouflage was not just for external audiences. Kings received information and expected action through different channels – from priests, diviners, generals, viziers, and vassals – not in spite of the fact that royal authority was impossible to fully collocate, but because of it. States thrived on the competition between the branches of its apparatus, in which a perceived ability by cadres and factions to attain dominance within the state created continuous categorical blurrings of genre, office, and institution. This perception was usually illusory: the absence of precisely-defined power boundaries also allowed the state to maintain its preëminence by cloaking its structure within as without.

An ambiguation principle is a theoretical tool appropriate to the study of many cultural discourses; that for a form of power to render a fully coherent account of itself – hypercoherently – instantly introduces the possibility of its undoing. Were one to clearly and fully explain how to make a cult image, for instance, one would just as authoritatively admit the categories of unmaking and improper making.³⁷ For a political example, consider the syncretic manipulations of theology of the late eighth to the late sixth centuries BC, which problematized as much as they ever solved the exclusive justifications of imperial kingship in cult. Or, to choose another, the principle of secrecy that surrounded divinatory procedures in Old Babylonian and Neo-Assyrian courts provided a cover of ambiguity for political decision-making through privileged spaces, offices, and technocrats.³⁸

So, perversity No. 2 is: the political entities we study preferred ambiguity; they avoided hypercoherence.³⁹ The problem for historical analyses that aim to explain political life and institutions, to make their nature and purpose clear, is that the subjects themselves tried to *never* operate or present themselves in clear terms. Thus accounts of political life must at least take ambiguity and coherence on as subjects in and of themselves.

³⁷ On “hypercoherence”, see Richardson (2012b: 234, 248–49): “The variety and discontinuity of cultural forms and practices permit them to limit or buffer social conflict because their heterarchical locations contain and diffuse those conflicts. Rendered unambiguous and rational, coherence is their undoing.”

³⁸ Richardson (2010b).

³⁹ For a cogent but unpersuasive counter-argument that transparency aided early state power by enabling rational public subscription to it, see Mayshar et al. (2011).

History is also about forgetting

We look at how the ancients reconstructed imagined pasts, accentuating the role of social memory. We have identified their emulation of dynastic heroes; their veneration of ancestor-kings; the legitimization of cult through antiquarianism and pious fraud; and historical claims over the control of time, space, and knowledge (e.g. Assyrian *Distanzangaben*; the constructed idea of a once-and-future *māt Aššur*; and the reception of ritual and scientific literature through ancient sages, respectively). Assyriology's secondary literature is shot through with brilliant threads of commentary about real and imagined historical rememberings.

Now for its opposite: just as coherence suggests undoing, an idea of historical memory would also require integrating the blemishes of the past, a problem solved through social forgetting. Where, for instance, was the Neo-Assyrian account of the dark days of the eleventh century? Certainly its scribes had access to the types of information that would beg explanation beyond the fully mythologized account of the Erra Epic. Why was the fall of the First Dynasty of Babylon recalled only in the most roundabout ways? The event was certainly remembered, but only in the baroque allusions of tales about Marduk and in one cramped, seven-word, interlinear insertion of a chronicler (from the size of which, one sees a conflict between suppression and the fidelity of the copyist). And how, after four episodes of Marduk's removal from Babylon, could any Babylonian believe that the god's sixth-century statue was really the same as the eighteenth century one? Why should Babylonians have been so consumed by lamenting the destruction of Akkad and Ur but not, later on, of Babylon itself? When Gutians and Elamites were called forth onstage to enact the destruction of civilizations, in the place of what problematic memories did they stand?

If we are to have a historical “remembering,” we should think, too, about historical “forgetting.”⁴⁰ Psychologists and historians have paid a good deal of attention to “programmatic amnesia” or “motivated forgetting” – ways of re-narrativizing and rescripting painful or inconvenient pasts. In the collective as well as in the individual model, the function of forgetting can be either therapeutic or problematic. The interpretive or methodological difficulty lies in the fact that forgetting is accomplished *through* the rescripting of the historical narrative. That is: programmatic obliviousness is located in precisely the same source material we think of as “historical,” and “forgetting” is thus hard to distinguish from “remembering.”

40 See Richardson (2014b: 494–500), with literature cited there.

What is useful for the ancient historian about “forgetting”? For political history, I see at least two opportunities arising. One has to do with demonstrable forgetting. By this I mean the obvious relegation of painful subjects to the edge of historical consciousness *despite evident recourse to historical records*. I think of the cool way the later Larsa rulers shrugged off the question of dynastic legitimacy; I wonder why no clear indication of the location of Akkad is left in the cuneiform record; I think of Adam Smith’s (2003: 1–5) proposal of an “amnesiac shock” produced by the live inhumations in the royal graves at Ur, an erasure of the creation of subjecthood. Many subjects have promise here: what is being kept at the edges by political institutions, suppressed, in the dark – and why? The answers to these questions should help us sharpen what we know about politics as a process and dialogue because they illuminate the anxieties which complicated ideals.

A second opportunity has to do with what I call “replacement” – the process of putting old practices in abeyance as new ones arise, a “mothballing” of the past, when new cultural imaginaries required the demise of old forms. The new political imagination of Sargonid Assyria, for instance, came with a putting-away of royal paternities and genealogies; venerating ancestors and old inscriptions; the Assyrian King List; traditional literature about heroic kings of the ancient past. Where Old Babylonian Nippur, for instance, produced almost five hundred compositions about ancient kings, the entire Neo-Assyrian corpus has given us only fifty, even though its scribes copied massive quantities of other older literature.⁴¹ I don’t view this as a statistically valid observation so much as the heuristically valid sort of patterned absence I describe below.⁴²

One doesn’t just make political innovations *ex nihilo*, one has to correspondingly and programmatically subordinate older traditions to do so. What does the disappearance of forms tell us about the appearance of new ones? What old principles have been disenchanted to accommodate new ones? History-telling is often a form of inventory, of telling what arises and changes, and assuming its novelty; but proper inventories account for depletions, too.

Incompletes, opposites, and absences as evidence

Seeing as my points above depend to some degree on deviations from or inversions of evidence, I’ll take a detour into methodological perversity and say a little about incomplete, opposite, and absent evidence. We, as other historians, generally see the absence of evidence as a poor basis for scholarship

⁴¹ Richardson (2014b: 485–94).

⁴² Baltrusch et al. (2011: 4).

(or: the basis of poor scholarship); that deductions from lack of evidence and argument *e silentio* usually denote a thinly-disguised wishful thinking on the part of the historian. The fundamental principle of scholarly doubt is that valid knowledge requires demonstrable proofs. On the other hand, we have an “escape clause”: that an absence of evidence is not (demonstrably) always evidence of absence. Thus the arena in which the competing imperatives of presence and absence play out is one of proof versus disproof. The problem, however, is again that we lack a methodology that would permit meaningful distinctions between the absent and the *conspicuously* absent. One potentially useful tool is comparative history, when done right; but too often comparativism turns out to mean an emphasis on the comparisons of similarities and not of dissimilarities. In any event, comparanda are often not available for our material, and we must rely only on the sources before us.

With political sources, more to the point, the historian faces an even “softer” standard of reality, since they are primarily evidence of *ideas* rather than events. The tangibility of ideas as evidence presents obvious challenges, as problems of intellectual history. Even more problematic, if political speech is aspirational – persuasive rather than descriptive – then its underlying reality anyway is not any objective truth (or lie) of what it describes, only (the equally objective truth) that its saying is evidence of a desire-for-something-to-be-so. The political is a transcript of incipience. It is not a record of the frequency or integrity of its own truth capacity, but a frame or boundary for political *questions*. Are ideas “real things”? Sort of: in this gossamer instantiation, we at least catch a glimpse of what the ancients really *wanted* to be true. That is a subject of interest in itself, tempered by the fact that political rhetoric needs to touch base with enough objective reality, or risks losing its persuasive capacity. Thus our task in assessing incipience is not only to differentiate between realistic truth as against the aspirational content of documents, but to elucidate the contingencies which required the sublimation from the one to the other.

Taking this a little farther: what do claims do? They disprivilege other claims; they establish the principle to declaim authoritatively in the spheres of action they describe; and most importantly they *respond* to other claims, to other (real) ideas. It would not be hard, for instance, to say that Hammurabi’s claim in his law code to have treated Larsa with “mercy” was untrue in moral terms. But to think it merely a “lie” would also miss why it was important enough to carve it into a massive, expensive stone and erect it for all to see. Its presence already tells us something of the counterfactual: that Hammurabi hoped, for instance, to *govern* Larsa and the south. We know this turned out to be a short-lived hope; but it tells us that Hammurabi was making a claim to juridical authority in a geo-political context (which was metaphorically innovative); it tells us that enough of Hammurabi’s contemporaries held the “mercy” claim to be untrue enough to warrant

answering them; and it tells us that both the claim and its opposite formed a discourse constitutive of precepts of good and bad kingship. To choose another example, I have elsewhere argued that Assyrian royal inscriptions used different metaphors to describe different kinds of rebellion and that the metaphors of violated contracts, animals out of control, or unruly children all denoted different *kinds* of political contention. This menu of metaphors confirms the existence of qualitatively different political dialogues, though leaving only traces of the variety of challenges to power.⁴³

Perhaps; this all may want a stronger methodology to prove its usefulness, though its truth seems apparent to me. So does every claim of every king always need a full consideration of its potential opposites? To distinguish its incompleteness as against its emptiness of truth? Does it need to have its tactical motives questioned to this degree? Is every political claim a sort-of-true platitude meant to soothe an equally sort-of-true anxiety?

Yes. I don't think one needs to conclude that every particle of political speech is a half-truth of this kind; it's not a theoretical paradigm. But it is a methodological one: the questions need to be asked, the alternatives they present seriously considered, the counterfactuals and counter-arguments exposed as the contingencies which formed a Mesopotamian politics. Not to do so bears not so much on the question of credulity versus skepticism, but on missed opportunities to narrativize a record of political speech which is otherwise depressingly monovocalic. Bearing in mind Marc Bloch's enduring analogy of the historian to the architect, such questions must precede – rather than anticipate – answers; too aware of the restricted nature of our evidence, we too often permit ourselves not to pose intellectually legitimate questions because we know (or think we know) that they cannot be proven or disproven.

To follow these points to their conclusion, it is important to consider the possibility that some forms – short-lived or peripheral – were simply failures. A functionally-biased model of human social interaction which privileges successful emergence toward holism as its pre-set explanation for observables seems woefully ill-equipped to make an account of the political arena, in which failure was and is a fairly normal outcome in vernacular, epistemological, and historical terms.⁴⁴ If history – political history included – is fundamentally

⁴³ Richardson (2010a: 1–12).

⁴⁴ See Baltrusch et al. (2011: 10). By way of analogy, Chao (1999: 524–28) has observed of ritual studies that: “[w]ithout theorizing transformative rituals and failed rituals, analysts eliminate from their depictions of social arenas many forms of agency that contest and resist power.” Another example emerges from medical methodologies which take time to analyze “morbidity and mortality,” errors in research and care. Compare with Selz (2002: 110–11) on the “epistemological gap” created by the selectivity of historians in working on forms that succeed.

about documenting and understanding *change*, then modeling the theoretical space of failed trajectories is as much as half (if not more than half) of our job.

To think that things we can't see or prove can't be evidence means that we have blinkered ourselves to a historical record of debates and problems of the greatest importance: the ones that almost succeeded, but didn't. Should we chase after postulates we can't prove and the ideas that found no home?

Obviously, I think we should. It is one kind of limitation, yes, to see things that aren't there and believe in them – at the far end of that spectrum lies psychosis. But it is a different kind of perceptive defect to never develop the basic object permanence that infants acquire in their first year: the understanding that objects continue to exist after they can't be observed. Discerning absent and opposite evidence requires a *methodology* of patterned lacunae;⁴⁵ it entails a taking-seriously of the Mesopotamian concern for the binary construction of observable phenomena with their counterparts. These opposites were the silent interlocutors of the perceptible world – because politics was nothing if not dialogic.

Allay the *longue durée*

The *longue durée*: how crucial was this concept for historians of the modern period! Easily absorbed in seeing events as the very fabric of narrative, focused on the seemingly endless details of archives, historians of modernity were at first unconcerned with deeper rhythms of time, the environments which shaped these ephemeral events. But then the longer temporal framework promoted by the Annalistes forced historians to account also for the durable pasts of tradition, landscape, folk life, and material culture. This was a necessary corrective for modernists, whose craft could be as self-absorbed as modernity itself.

But millennial trends are not, to begin with, something ancient history generally neglects. Our stodginess runs more to declarative sentences about the immutability of substrate forms like kingship or households, arching over centuries of history. It is the capacity for transformative change that our sources do not emphasize and which we do not enough pursue. For Mesopotamianists, *la longue durée* fossilizes the very politics we need to breathe life and movement into by declaring them essential and unchanging – when just what needs to be emphasized are the unique and local little shifts and revolutions.

⁴⁵ Baltrusch et al. (2011).

Yet some of our histories can also run to the opposite extreme, with attention to very close sequences of temporal activity. The opportunity for the ancient historian to showcase the incredible detailing made possible by certain archives⁴⁶ in combination with the thick web of philological commentary usually necessary – one feels at pains to ground and explain many, many of the terms one discusses – often leads to an expositional and descriptive form of prose. This situation emerges in part from the corpora themselves. Economic and administrative texts are rich in data but generally unreflective and unexpressive about the world outside of what they describe. Even the epistolary corpora which have politics as their primary concern – from Mari and from Nineveh – are still in some stages of having sorted out, of necessity, their major apparatus – prosopographies, geographies, and dossiers – before much can be said conclusively about them. Only the few who have mastered these dimensions can credibly produce histories of their subjects, and these tend to be closely detailed, pointillist accounts. This reflects not so much the fact that we have thoughtfully developed histories-of-events as a form (especially as we cannot produce them for 95% of Mesopotamian history), only that we have maintained and extended a fidelity to narrativizing particular sets of cuneiform texts. Here too the “archival context” remains the hegemonic mode of analysis.

But the true poor stepchild of ancient Near Eastern history is neither the history of epochs nor the history of moments, but the middle-range, the time-span of generations. One might think this corresponds to doing “period history,” which is still a fairly dowdy, handbook-type of enterprise in our fields. Even worse, the Annaliste term between histories done as *longue durée* and *événementielle* is “*conjoncturelle*,” which brings unwelcome implications of circularity, suggesting already that intermediate periods were simply reiterative cycles within larger spirals of time.

Mesopotamian periodization is anyway much too uneven to be a good fit for the histories of generations. Some periods are determined linguistically, others stylistically, most politically; some are as short as 75 years, while others span as much as 400 years. Our modern framing, however, cannot constrain the probability that the political imagination of Mesopotamians was not only and always bound up in the historical senses of precedent and consequence of antediluvian kings, or of what had happened last week. Political accounts should also take up the terms of decades, of the three or four generations that made up the memorial horizon of households – indeed of most *archives*. What is missing is an attempt

⁴⁶ The paradigm case is without a doubt the rich fund of detail from the letters of the Middle Bronze Age Mari archive; for a history of events told from these sources, see especially the monumental (Charpin 2004).

to write the sort of agnostic histories that evoke the challenges of the times with reference to the times of fathers and grandfathers.

And these reference points of lived life should bring us to renewed focus on change *within* periods. We look to the middles of periods for paradigms; we look to their ends for changes. We feel somehow that states in full bloom “work” in some essential way, while rising and collapsing states display metastatic changes – inverse, alternate, exterior – outside of true paradigms.⁴⁷ But not only should we question the determination of political by dynastic and linguistic criteria, as we do; a productive reconception of periodicity itself is in order, one which privileges the nomenclature of problems rather than ideal types. To say that politics is “dialogic” is merely a dry and stuffy way of pointing out that it is about argument; politics is about fighting. So the first point in a reconstruction is to think about periods as laboratories of dispute instead of as perfect incarnations of ideologies. Thus one might think more usefully of the twenty-first century BC as a time when the state struggled to emerge into hegemonic status over and against other institutions; or that a tension between cultural novelty and authenticity was the hallmark of politics ca. 730–539 BC. I think of the century ca. 1820–1720 BC as something like Mesopotamia’s own “Warring States” period, when the question was not so much (mechanically) which state would emerge to regional dominance, so much as whether or not regional dominance was workable at all (it wasn’t, yet). However we denominate politically meaningful spans of history, the criteria have to raise the *questions and contentions* to primary, denotative importance.

Let me return to my “Fall of Babylon” example. The crucial political changes I see transpiring over the course of that dynasty had as much to do with contrasts with the past as the contests of the present, i.e. the comparisons the seventeenth century BC made with its eighteenth century self (much as eighteenth century Babylonia compared itself, probably unfavorably, to Ur of the twenty-first century).⁴⁸ Babylonian political thought in the last century of the Middle Bronze was to a great degree comparative of the warrants laid down by Hammurabi. The collapse of the dynasty in the Late Old Babylonian in this sense wasn’t “caused” by Kassites or shifting canals or debt cycles any more than by any other proximate cause. The Fall of Babylon was about what *didn’t* happen; it was about Babylon’s failure to generate the political will to deal with problems by rendering a credible

⁴⁷ See Hall (2010: 204–205).

⁴⁸ Richardson (2002).

account to itself of its own past. This repression only compounded in the historiography of the event, since Mesopotamian stories about the past always had implications for the present and future through the suggestive allusions of the ominous.

Obviously, such an account, as for any other in which the subject is discourse, is hard to document. But my reconstruction would run like this: the terms by which the dynasty had established itself were too big to be forgotten: juridical control, territorial conquest, and economic regulation. The visibility of the gaps between the state's premises and its real circumstances inevitably produced a political and historical dialogue internal to the time. The disenchantment of reduced circumstances was bad enough; but against this, *mutatis mutandis*, new forms also began to present themselves as alternatives: the conversion of kingship to a domain state; the emergence of non-state actors in official capacities; a surprisingly creative re-tooling of scholarly material. Yet all these changes would be invisible to us were we to frame them by periodic or dynastic nomenclature: they arose while there was still a king of Babylon, who venerated Marduk, issued debt remissions, and whose year-names adorned every document. In external political terms, this was still the "Old Babylonian period," though much was changing beneath the surface. Political coherence by state, dynasty, or city is epiphenomenal, not essential.

Thus my second point is that a reconstruction of meaningful political time has to account for the fact that political thought is historically as well as contemporaneously (e.g. socially and geographically) comparative. Antiquity's political dialogues, like ours, were with its past as well as its present arguments, and the temporal framework for them should be measured in the few generations of the family archive, more than in the epochs and immediacies that often occupied the attention of most documents. We could look forward to the renovation of period history as something like the "life-history" of political issues, and not as variations on endless and unchanging themes. Of course some great historical sensibilities and thematics *did* over-arch the three millennia of Mesopotamian political thought – not to say they didn't. But the intention of some of the tendentious stylings in the sources to that effect was to collapse time, to import the glorious past into its present, and to hide and erase the changes, ruptures, and divisions we need to know to reconstruct a politics. At the least, given the frames of time we are working with, and the disposition of our sources to present the past as eternal, we need not worry about adding more *longue durée* to the mix; there is plenty of it already there.

Environment is not explanation

Finally, a word about the environment, which we sometimes endow with an agentive capacity for historical change, especially with reference to the rise and fall of states. Much of the work that's been done on Mesopotamian landscape and environment has made serious, even game-changing contributions to our historical understanding of the culture.⁴⁹ But it has also coincided with contemporary popular historical thinking on man and his environment in a way that is not always very helpful. Present concerns about the state of the planet's health have led to soul-searching all across the academy, and both popular historians and archaeologists have looked to Mesopotamia as a proving ground for theories about human maladaptation and environmental degradation. Thus, although Mesopotamia not so long ago stood in the public imagination as a place – even *the* place – in which man first mastered his environment, it is rapidly turning into the place where he first screwed it up, or where it was first revealed that humans are hard-wired to screw it up – becoming only a historical “first” in that it becomes the first moment when we can see mankind's inherent maladaptiveness in some detail.

These are highly questionable propositions. They put human agency in a position secondary to nature, or even do away with it altogether. Surely, at the least, this is not good social scientific explanation: political history insists on the social rather than the natural world as the locus of change, both good and bad. I am in fact deeply sympathetic to attempts to understand the interaction of populations and landscapes, but I reject historical explanations which make their ultimate recourse to desertification, shifting riverbeds, salinization of the soil, and so forth, to establish causation.

This is not to dispute that early societies were capable, within their limits, of the greediness and sloppiness at the heart of their particular modes of production (every unhappy family being unhappy in its own way, as it were). Nor even is it to challenge the idea that economic production does not inherently require maladaptive disutilities; I believe it does. But I do not believe that ancient societies had the technologies or aggregate bio-energy necessary to alter the physical environment so far past its sustaining boundaries as to engender the deep shocks and setbacks sometimes proposed.⁵⁰ Reciprocally, I seriously doubt that any natural catastrophes were by themselves capable of dismantling any

⁴⁹ See now especially Wilkinson, Gibson, and Widell (2013), with literature; also Adams (2008) and Liverani (1995). Note the forthcoming proceedings of the Oriental Institute Seminar of March 2014 entitled “The Early/Middle Bronze Age Transition in the Ancient Near East: Chronology, C14 and Climate Change,” to be edited by Felix Höflmayer.

⁵⁰ Most prominently, Diamond (2005); cf. McAnany and Yoffee (2009).

entities that were not already ill-equipped to deal with political turbulence for other reasons; states, in short, which had already lost resilience.

Are disasters possible? Of course. Did they happen? Absolutely. But these were not pre-Cambrian era die-offs of the entire biosphere; that's the wrong model. Even if one conceded that the natural world had such a determining power in any given situation, simply connecting environmental problems to state or social collapse is a woefully simplistic abbreviation of historical explanation, if only because environments are not our subject. It is our job as historians to explain how and why societies reacted to pressures and catastrophes in the way that they did. As Guillermo Algaze (2005: 26) has put it:

environmental and geographical factors are only permissive, not prescriptive. Whether individuals and groups react to environmental changes and take advantage of geographical possibilities, and how they do so, are always constrained by culturally determined perceptions of opportunities and threats at any one time.

To point to conditions of the natural world is only description, not explanation.

Nor is it acceptable to foist the extreme and shocking kinds of environmental abuse we commit on a daily basis today as behaviors rooted in the ancient past. Global warming, pollution, and rainforest deforestation remain, I think, clearly and obviously new and modern problems. Yet some recent popular historical books have made the case that looking deep into pre- and early history, neurology, and human evolution will explicate a “big history” or a “deep history” of man and environment leading up to our times.⁵¹ These accounts seem sure that present impacts will find past analogues. As an extreme example – really, a caricature – of the popular historical notion that modern environmental impacts are similar in nature to ancient ones, one reads with grim amusement a 1979 white paper from the US Strategic Air Command. The report patiently explains that the environmental effects of its first-strike against the Soviet Union would be much less severe than the deleterious impacts to the planet of the agricultural and land management practices in continuous use since the Chalcolithic:

No weight of nuclear attack which is at all probable could induce gross changes in the balance of nature that approach in type or degree the ones that human civilization has already inflicted on the environment. These include cutting most of the original forests, tilling the prairies, irrigating the deserts, damming and polluting the streams, eliminating certain species and introducing others, overgrazing hillsides, flooding valleys, and even preventing forest fires.

⁵¹ See, e.g. Smail (2008), anticipated by his 2005 article; cf. Cook (2009).

The plan to which it referred entailed the use of 3,423 nuclear weapons to kill an estimated 80–220 million people.⁵²

But parking our problems deep in the human past, as consequences of enduring social practices and human nature, avoids identifying the unique scale and nature of the powers and problems we have recently acquired, and the commensurately large changes we now need to make. A recognition of the “Anthropocene” is emerging in answer to the gradualist position, a new and distinct geological age in which humans have become geological agents; unsurprisingly, the epoch is held to have begun ca. 1750 AD, i.e. at the beginning of the Industrial Age, and not 1750 BC.⁵³

Our explanations for political problems must go beyond concluding: “Oh, the river shifted”; because we’re missing the opportunity for the human story; because if there is a real contribution ancient studies *can* make to the urgent debate about environment, it is one about resilience and adaptation. We ought to be able to emphasize the deeper history of the *flexibility* of political structures in the face of challenges, and the radically different ways there have been to organize political societies. It may give some superficial comfort to think, as we often do of the ancients: “oh, they were just like us.” And this banality sounds harmless – except insofar as it forecloses on a vastly more valuable lesson that the study of ancient societies, or really the study of any societies, can bring: about how *different* we are capable of being; that change is possible; that we can be smarter than our problems.

Conclusions

It can be hard for us to admit that our kings and states were less powerful than they said; less important than they said; that they might not have done the things they did or meant what they said. It can be uncomfortable to feel that the veracity of our documents shifts like sand underfoot and that we cannot confine the vitality of ancient life in the box of declarative sentences about what “they thought” and “they believed.” It can be taxing to devise methodologies when our real subjects are questions and not answers. Bringing these Perversities to the forefront may seem to make an overly suspicious account of things, but they undo no unquestionable truths and set no rational world on its head. Quite the opposite: their application should bring to our thinking about ancient political history

⁵² Schlosser (2013: 206, 353–54).

⁵³ See Chakrabarty (2009: 207–12, 218).

the same gritty, invested, and animated trepidations with which we greet each morning's newspaper: how exactly, we wonder, will things not work today?

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