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## Answers to Critiques

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### PROLOGUE

I welcome this opportunity to deal with a broad spectrum of criticism of the reformulation of my circumscription theory. The comments above represent a formidable array of incisive observations about the formation of chiefdoms and states. In responding to them I am bound to face questions I had not considered before, and fully expect to learn much from a careful consideration of the issues they raise. It remains to be seen if I can successfully defend my theory against the challenges that have been posed. In any event, coping with them as best I can should prove to be a most zestful exercise.

The only instruction I was given in responding to my critics was to make sure I dealt with the remarks of every one of them. This requirement had the salutary effect of making me face squarely criticisms which I might otherwise have been inclined to sidestep.

In organizing my comments two courses seemed open to me. One was to group them into broad categories and to deal with various instances of the same general argument together. And indeed, at first I considered following just this approach. But it was not long before I realized that doing so would entail an inordinate amount of work. Instead, I chose to deal with each critic's comments separately.

Having decided that much, the question still remained as to what order to adopt in discussing the twenty-two sets of comments. No clear line of progression suggested itself since the various sets of comments were often disparate within themselves. In the end, I decided against any sort of clustering, such as dealing with the favorable critics first, to be followed by the unfavorable ones.

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Instead, I have more or less interspersed the two, keeping in this way more of a sense of balance.

Some questions – especially about the determining role of ideology, population pressure, and warfare in the origin of chiefdoms and states – were raised repeatedly by my various critics. Since I had to meet essentially the same objections over and over again, I tried to devise some way of avoiding tiresome repetition. Thus each time I countered what amounted to the same argument, I did so by dealing with a different aspect of it, by employing different wording, or by offering different examples of the same counter-argument. I hope in this way to have minimized repetition and thus helped maintain interest.

### REJOINDERS

**Joyce Marcus** brings to bear on the study of state formation a profound knowledge of the archaeology of Mesoamerica, especially of the Valley of Oaxaca and the Maya lowlands. She is convinced that the process of political evolution, whether in Mesoamerica or elsewhere shows a striking degree of parallel development. '[I]t is now clear', she tells us, 'that societies in many parts of the ancient world arrived at similar solutions to the same problem' (p. 74).

In describing the forces at work in this development, Marcus does not shy away from singling out war as the primary instrument at work. Competition between political units, she says, 'was the social engine that ultimately led to the loss of community autonomy and the emergence of political hierarchies' (p.75).

The evidence for the presence of war in ancient Mexico, she points out, is overwhelming. It can be seen in 'defensive walls or palisades, burned houses and villages, head-taking, and skeletons showing trauma' (p. 75). Less direct indications of war were the measures taken by peoples to secure their safety: 'local populations leave desirable areas ... [and] move to defensible locations far from water sources and good agricultural land and invest in walls or moats' (pp. 75–76).

Marcus, along with Kent Flannery, Charles Spencer, and Elsa Redmond, has presented a detailed and compelling reconstruction of political evolution in the valley of Oaxaca. Between 700 and

100 BC, 'the valley featured three rank societies [chiefdoms]'. Moreover, 'rivalry between these three ... societies was intense' (p. 76). Briefly but vividly she describes how one polity, fighting successfully against its rival, established itself on the summit of Monte Albán. 'What resulted from this and other military victories was a unified Zapotec state' (p. 77). (This, I should note, took place in a valley ringed by mountains. The moment I first set foot in the Valley of Oaxaca in 1960, I was struck by the fact that here was a place of environmental circumscribed *par excellence*. And it was this circumscription, not resource concentration – which Oaxaca lacked – that seems to have been the initial condition that helped propel this valley on the road to state formation.)

Oaxaca provides a convincing case – documented as few others have been – of a state arising through military means. Contrast this view of the matter with archaeological theory as it stood fifty years ago, when it was stubbornly disinclined to see war as an important element in its prehistory. As an example of this dismissal of unpleasant realities, take the case of the 'danzante' figures carved into the walls of Monte Albán. Now generally regarded as humiliated and mutilated war captives, they were first thought to be *dancers*, as the Spanish name for them denotes.

In commenting on the area around Lake Titicaca in Peru and Bolivia, where '[a] state arose through a similar set of processes' (p. 77), Marcus cites the work of Charles Stanish and Abigail Levine, who reached similar conclusions to her own, but were at odds with the views of earlier archaeologists working in this region. Nor was this surprising. As long as archaeologists continued to shy away from the unpalatable fact of recurring warfare (as they did when they kept referring to the 'peaceful Maya') they continued to misinterpret what was under their noses. Lawrence Keeley has written an entire book, *Warfare before Civilization* (1996), spotlighting this failure by archaeologists – including himself! – to recognize the prevalence and importance of war in prehistoric times. However, once they began to reinterpret the evidence their excavations were revealing as a sign of warfare, the whole picture dramatically changed.

Though Marcus firmly believes in the importance of warfare in creating ever-larger societies, in seeking the impulses leading to

this warfare she prefers to see it as stemming from ‘competitive interaction’ rather than from population pressure. A number of her Latin American colleagues, she tells us, ‘detected many cases where chiefly rivalries led to warfare before population growth would seem to be implicated’ (p. 77). This apparent lack of population pressure raises a major issue (which recurs in many other comments on my paper) and as such needs to be faced squarely.

In my ‘Reformulation’ I proposed an explanation of why population pressure may not always be easy to detect among swidden cultivators. Yet, this answer is, in some ways, an evasion. It does not apply to areas of fixed-field cultivation. And it is in such areas that most advances in political development were taking place. However, it is just in these areas that a lack of outright population pressure is most apparent.

Admittedly I have used population pressure as something of a *blunt instrument* in making it the major impetus to conquest warfare. The concept needs further discussion and refinement. First of all, though, let me try another tactic to argue for its presence. Let me suggest another case in which population pressure has been reported as lacking when it may actually have been present.

More than one Egyptologist has stated that there was no population pressure in the Nile valley in pre-dynastic times, a period which saw the early crucial stages of state formation. Consider this possibility: that the *nomes* (political units that later became provinces in the unified Egyptian state) were in fact, in many cases, warring chiefdoms in pre-dynastic times. And consider further that the sparsely settled, if not actually unoccupied stretches of land along the Nile between *nomes* might have thrown Egyptologists off the scent. It might well have been the case that within the *nomes* themselves, people were indeed densely packed. Moreover, this density of human numbers might have been the cause of recurring fighting between adjacent *nomes*, and that one result of this fighting might have been the creation of *buffer zones* – no-man's-lands – between *nomes*. The creation of buffer zones in areas of repeated and intense fighting between rival polities has been reported widely, both historically and prehistorically (LeBlanc 2008: 443–449). Could this state of affairs not have prevailed along much of the Nile in pre-dynastic times, giving the misleading appearance of a *lack* of population pressure? And that certainly might have been

the case if in making calculations of population per square mile archaeologists used *all* the land along the Nile, instead of just settled areas. This might easily create the false appearance of a lack of such pressure.

Let us suppose, though, that Egyptologists *reject* this interpretation and that there really was *no* population pressure in pre-dynastic Egypt. What then? To what 'fall-back' position can I retreat? Simply this: It is to admit frankly that in certain regions of the world, where political evolution was well underway, the impetus for it did not involve population pressure. The conclusion then becomes inescapable: warfare – including conquest warfare – can be caused by factors *other than* population pressure. Or, perhaps, better stated, conquest warfare can begin *even before* population pressure has made its appearance.

Some critics might be quick to affirm that this admission represents a serious challenge to the circumscription theory. But does it? Does it really undermine the basic elements of the theory? I think not. After all, it would not deny the efficacy of war as the instrument, above all others, for surmounting local autonomies and building up larger and larger political units. In fact, it emphasizes *even more conspicuously* the role played by war in the growth of polities. If anything, it gives even *greater weight* to it. (What it also does – as I suggested in my 'Reformulation' – is to raise the question of whether the circumscription theory might better be called by a different name.)

So important do I regard this point that I would press it even further. Let me say again that the absence of population pressure does not affect the *dynamic element* of the theory. Nothing about the lack of population pressure goes against my contention that, *whenever it is present*, population pressure *does* give an added impetus to conquest warfare. Thus, if even in the absence of population pressure warfare leading to state formation can still proceed, *how much stronger* will that impulse be when that pressure is actually present?

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At the beginning of his remarks, **Alain Testart** complains that, as much as I talk about the state, I never actually define it. And he is right. At least not in this paper, although I have done so else-

where (Carneiro 1981: 69): '*A state is an autonomous political unit, encompassing many communities within its territory and having a centralized government with the power to draft men for war or work, levy and collect taxes, and decree and enforce laws*'.

Testart offers his own definition of the state – one patterned after that of Max Weber – the key to which is that a state enjoys a monopoly on the use of force. This definition is by now famous. However, I think it is flawed. For one thing it is rather skeletal, leaving out what might be called the 'internal organs' of the state. Moreover, it fails to apply to polities which, on other grounds, one would want to consider to be a state. For instance, the Anglo-Saxon laws tell us that, for certain offenses, several kingdoms of the Heptarchy permitted individuals to take the law into their own hands (Attenborough 1922). And I believe the same was true of the kingdom of Alor in East Africa, of Tahiti, and no doubt of other states elsewhere in the world. Thus, if we were to choose Weber's definition of the state as the 'official' one, it would fail to encompass a number of polities which we might otherwise want to count as states.

Moreover, the Weber-Testart definition is a negative one. It specifies that a state is a polity in which private parties cannot carry out police functions. Negative definitions, though, are infelicitous. To define a state negatively is like defining a herbivore as an animal that does not eat meat, disregarding all of its positive attributes.

As he continues to wrestle with the question of what characterizes a state, Testart offers the Cheyenne as a society that manifested a certain state-like feature, referring specifically to the practice of their police societies of fining tribal members who went off hunting buffalo prematurely. And in an effort to make the Cheyenne appear more state-like, Testart quotes Robert Lowie to the effect that Cheyenne police societies 'were the state in this case' (p. 107).

It is true that Plains Indian police societies did perform a function that is usually carried out only by full-fledged states. But just as one swallow does not make a summer, a single state function does not come close to making the Cheyenne a state. The fact is, the Cheyenne were a *band* society. Only during the summer – and then only for the purpose of hunting buffalo – did Cheyenne bands aggregate into a *tribe*, and only then did the police society come

into existence. As soon as the buffalo hunt was over, the Cheyenne broke up into individual bands and scattered over the countryside, light years away from being a state.

Testart overreaches when he quotes me as saying that environmental circumscription 'is not absolutely essential to' state formation (p. 107), as if I had thereby undermined my entire theory. I wrote those words to show that resource concentration, and the social circumscription that it often produced, could, under unusual circumstances, bring about the same result as environmental circumscription. Now, to introduce this possibility was one of the reasons I wrote my 'Reformulation' paper. I felt the need to refine and elaborate the theory in such a way as to take account of special cases not fully discussed in my original paper.

Testart insists that 'the state is a political fact' and that 'a political fact can only be explained by political facts', adding that it 'cannot be explained by ecological factors' (p. 107). But that assertion can certainly be challenged. The state arose in certain areas of the world and not in others. How is this to be explained without invoking ecological facts which favored its rise in some regions but inhibited it in others? 'Political facts' do not exist in a vacuum. They are a response to external conditions as well as to internal 'political' necessities.

Let us explore the matter further, as I have already done in my 'Reformulation'. One can hardly make too much of the fact that several native kingdoms – Alor, Toro, Buganda, Ankole, Bunyoro – arose in the interlacustrine region of East Africa. Here was an area of abundant natural food resources, provided largely by its lakes – an ecological fact *par excellence*. It was also an area of partial environmental circumscription provided by the mountains that rose immediately to the west of the lakes. By way of contrast, just east of this region lay an area consisting of open savannas, a landscape lacking concentrated food resources. And as far as political development was concerned, there was nothing in that area above the tribal level. I invite Testart to explain these facts without alluding to ecological conditions.

Testart appears to think that the circumscription theory would be dealt a body blow if archaeologists were suddenly to discover that states had *already* existed in Egypt and Mesopotamia *before* those polities which we now consider its earliest states. Clearly,

though, this is a mistake. Were it true that such supposed earlier states had existed, they would no doubt have arisen for precisely the same ecological reasons as did those that succeeded them!

Speaking of ancient Egypt, I have no reason to disagree with Testart that Egypt, prior to its unification around 3200 BC, already contained a number of petty polities. And it was those political entities that constituted the building blocks out of which a unified Egyptian kingdom was finally erected. These polities must already have evolved to a significant degree when, as the kingdoms of Upper and Lower Egypt, they fought each other in a great final struggle which resulted in the political unification of the country.

Finally, I am pleased that Testart made it a point to distinguish between 'state' and 'civilization'. In my opinion, the word 'civilization' is being used far too loosely, even by anthropologists. Consider, for example, the Upper Xingú region of central Brazil where I have carried out field work and where, in pre-Columbian times, a level of political organization no higher than the chiefdom had been reached. Yet it has been suggested rather casually that a 'civilization' might once have existed here. In my opinion, the term 'civilization' should be reserved only for states with sizable urban centers – in a word, *cities*.

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**Khaled Hakami** hits on a recurring theme when he writes that I need to say more about ideology. I have addressed this complaint in replying to several others as well, but when a fellow cultural materialist like Hakami advises me to do so, I feel compelled to acquiesce once again.

A major part of ideology is of course religion, especially since the societies we are dealing with were all steeped in supernaturalism. The gods played a profound role in the minds of the members of those societies. Sometimes they were appealed to for help, and sometimes they had to be placated. And when the issue was life-and-death, as it inevitably was in war, it was especially important for societies to (in modern parlance) 'get right with God'.

Ideology provided the psychological incentives warriors needed to bolster their courage before going into battle. It spurred them on to strive for victory in the face of great danger and of enemies who were calling on gods of their own. While the lure of political and territorial gain alone might have sufficed for a ruler to go

to war, it was hot blood, not cold logic, that gave impetus to the warrior to do so. And it was he, rather than his sovereign, who was ready to cry, as he went off to war, a favored expression of Theodor Roosevelt, 'We stand at Armageddon and we battle for the Lord!'

Thus, when considering the entire panoply of determinants that led men to fight, Hakami is quite right. Ideology, especially religious conviction, cannot be left out of the picture.

In peacetime, though, the main function of ideology was to promote integration and solidarity within the society, especially a society made up of disparate elements, often antagonistic toward each other. The matter can be put in these terms: Operating within each society we can distinguish two forces, a *centrifugal* one tending to break the society apart, and a *centripetal* one, operating to keep it together. And it was to the latter half of the equation that ideology made its contribution.

There! I think I have paid my dues to ideology.

Turning to a different matter, Hakami is unsure if my later views on how chiefdoms arose are correct. Nor is he the first one to express such reservations. And indeed it is quite possible – as I originally thought – that chiefdoms emerged when the strongest village in a region successively and systematically defeated, subjugated, and incorporated its neighboring villages. This view runs counter to my current thinking which sees chiefdoms arising through the solidifying of an alliance of villages led by a redoubtable war leader. I am frank to admit, though, that I regard these later views as tentative and provisional. The issue – like so many others – can be resolved only by an appeal to empirical evidence.

And here let me voice a complaint of my own regarding my fellow theorists as they try to lay out the most probable course of political evolution. Too often their arguments seem to me abstract, to be resolved by a resort to *excogitation* rather than to the known facts (see, *e.g.*, an account by LeBlanc [2008: 450–452] which he himself labels a 'just-so story'). What we need is more *evidence*. And it seems to me that state formation theorists have not been diligent enough in seeking out and assembling the relevant facts, fugitive as they may be.

Let me cite just two sources which are gold mines of information on chiefdoms but which lie virtually untouched by theorists.

One is *Señorío y Barbarie en el valle del Cauca*, by Hermann Trimborn, which offers a cornucopia of data on the 80-odd chiefdoms that filled the Cauca Valley of Colombia when the Spaniards first arrived on the scene around 1530 (Trimborn 1949). I have drawn heavily on this volume in presenting a panoramic view of these chiefdoms (Carneiro 1991), but as far as I know, no one else has yet tapped this reservoir of information.

The other sources I have in mind are the works of Charles Hudson and his associates at the University of Georgia, who have brought together all the ethnohistoric information available for the Southeastern United States in the 16<sup>th</sup> century. This source contains the most exhaustive compilation available of the data on chiefdoms in that region when the Spaniards arrived (*e.g.*, Hudson 1990).

In this connection, let me interject a few words about *theory*. The function of theory is to explain fact. And the more we immerse ourselves in the facts the better able we will be to formulate sound theory. Indeed, illuminating ideas about how chiefdoms arose will jump out at us the more we surround ourselves with the facts about them. Facts are often nuggets that lie hidden in the ore, and it is our job to extract them and put them to use.

Hakami seems somewhat uncomfortable at my placing so much emphasis on the *individual* in my scenario for the origin of chiefdoms. I even go so far as to christen the protagonist in this drama, giving him a name – *pendragon*. Did this war leader *in fact* really play such a crucial role in giving rise to the polity he then came to dominate? Here I may seem to be backing away from my customary position of stressing the predominance of cultural forces over the individual. Still, when I look at a strong leader like Ujukam among the Achuar of Ecuador, Möawa among the Yanomamö of Venezuela, and Matoto among the Tairora of New Guinea (Carneiro 1998: 28–31) I see each as a dominating figure with the power to intimidate his fellow villagers as well as others around him. Nevertheless, these men still ranked no higher than chief of an autonomous community.

It is apparent to me, though, that men like these, with forceful personalities, demonstrated military prowess, and ambition to match, if given the right conditions could easily have given rise to a chiefdom. Aha! ‘Given the right conditions’. Am I here not falling back on my usual position that (as Herbert Spencer put it), *conditions*

and not *intentions* determine? Perhaps. But rather than try to resolve the issue of just where I stand in this debate, I leave it to others to put me in whatever camp they wish!

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I agree with **Peter Peregrine** that – as I stressed in my ‘Reformulation’ – what we should aim for is a *general* theory of state origins, not a unique causal chain applicable only to each instance of it. In its full particularity, of course, each case of state formation is unique. The Roman Empire and the Inca Empire, for example, were very dissimilar in the way they arose and in the way they functioned. For instance, one relied heavily on slave labor, the other made exclusive use of the *corvée*. Still, underlying the diversity between them, a common set of circumstances united them. They faced and overcame a number of similar problems that all large and successful states had to surmount in order to achieve a dominant position. Thus, the Incas and the Romans each had superior armies that enabled them to ride roughshod over their enemies and to extend the boundaries of their empires almost at will. Another element that allowed them to exercise effective control over conquered territory was an elaborate system of well-maintained highways that extended to the four corners of their empires. And this system of roads contributed as well to keeping their respective empires integrated.

(Let me indulge here in a bit of speculation. We shall never know just how many budding states fell apart before reaching full-blown statehood because they failed to develop the necessary integrative mechanisms.)

Peregrine asserts that ‘if one begins to construct a theory for the rise of the state under the assumption that there can only be a small set of causal factors, that assumption can also become a self-fulfilling prophecy’ (p. 83). Assuming that he is directing that warning to me, let me respond as follows. First of all, the number of factors involved in my theory of state formation, whatever their number, was never an *assumption*. Nothing was taken for granted. The theory was based on empirical evidence on the rise of actual chiefdoms and states. The causal mechanisms involved were never simply *posited*. They were observed *in operation* in a variety of cases reported in the anthropological literature.

A reasonably dense population is always required for a state to arise. The fact that Oaxaca and Mesopotamia are said by some to have given rise to states in the face of a *declining* population needs close examination. It is evidence of something that may not at first strike the eye, namely, that at *some prior time* population was *greater* than it later became. Quite possibly it was large enough to have permitted a chiefdom – or even a state – to develop. (We shall have occasion to look more closely at the case of Oaxaca in a later comment.)

Peregrine seems to concur that competition over resources may have had the same effect as population pressure. That is, it may have led to war, with all its attending consequences. Still, one should be on the lookout for the possibility that warfare waged ostensibly over limited resources may have *masked* an underlying population pressure as a contributing factor.

Let me say again what I said in my comments to Joyce Marcus, namely, that warfare – even warfare for territorial gain – may arise *prior to*, and even *in the absence of*, population pressure.

Like Testart, Peregrine notes that I fail to provide a definition of the state. I refer him to the one given above. It is clear and concise, easy to apply ethnographically and easier to infer archaeologically than (for instance) ‘a monopoly on the use of force’. It is a definition that focuses on the *sinews* of the state and not on its *feathers*. To pursue this analogy further, some definitions of the state seem drawn to its feathers, as in defining an elephant by its tail instead of by its trunk.

Peregrine says that the state is the culmination of a long and continuous process – a ‘continuum’ that begins with a scattering of autonomous villages, passes through successive stages which see the massing of these villages into a chiefdom, and ends with the consolidation of a number of chiefdoms into a state. Clearly, what we have here is a *process*, divisible into *stages*, the two concepts, rather than being antagonistic, implying each other.

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**Alessandro Guidi** cites mounting archaeological evidence – especially from Europe – of the presence and importance of warfare in prehistory. What once was minimized or even denied, has now been overwhelmingly demonstrated. This dramatic reversal in archaeological thinking has been vividly recounted in Lawrence

Keeley's book *War Before Civilization* (1996). It is also traced in Stephen LeBlanc's article *Prehistory of Warfare* (LeBlanc 2005).

Guidi refers to my insistence on the importance of war in human history as 'prophetic'. It is kind of him to credit me with this achievement but I must step aside and point out that, in this regard, Herbert Spencer anticipated me by more than a century.

However, having appreciated the critical role of war in prehistory, it seems anomalous that Guidi should then proceed to cite Antonio Gilman, a Marxist archaeologist, as having shed shafts of light on political evolution in Bronze Age Spain. I say that because to me there is a remarkable inconsistency between fact and interpretation in Gilman's work.

According to Guidi, Gilman cites as major factors in 'the emergence of social stratification in Bronze Age Europe' the introduction of such elements as 'plough agriculture, offshore fishing', as well as 'Mediterranean polyculture and irrigation' (p. 57). All well and good. No doubt these things did contribute in some degree to the development of social complexity in Bronze Age Spain, where Gilman has done most of his work. But what strikes me as odd is what Gilman *does not* say. He virtually ignores warfare as a driving force in the prehistoric developments he is trying to trace. And here lies the glaring inconsistency I alluded to between fact and interpretation.

Among the tangible results of Gilman's archaeological surveys was finding in Albacete province in southern Spain the remains of no fewer than 270 forts (Carneiro 2003: 222)! Guidi seems to accept Gilman's explanation that 'a community engaged in ... time-expensive economic strategies' (that is, tending to their wealth!) 'could not refuse the protection of the warrior elite' (p. 57). But in embracing this explanation Guidi sidesteps the fact that in all likelihood it was this elite, in the role of military leaders, who raised Bronze Age Spain to the level of chiefdoms and made themselves wealthy in the process. Guidi's ignoring of all this seems to me tantamount to turning his back on the importance of war, and thus being inconsistent with his previous recognition of its importance.

As long as we are ignoring things, this seems to be an appropriate place to introduce a subject that has long bemused me. I refer to the fact that Marxist anthropologists have completely ignored the circumscription theory. It is not that I feel slighted; it is just that

I am curious. This failure on the part of Marxists scholars cannot be because they are not interested in the rise of the state. After all, Marxism is all about the origin and evolution of institutions. Thus the circumscription theory, being a prominent enough theory of state origins, deserves their attention. I say to them, accept it or reject it, but do not treat it with total silence. Still, in all the Marxist anthropological literature I have read I have yet to find a single mention of the theory, let alone any assessment of its merits. Why should that be? Let us explore the possibilities.

One reason why Marxists might look with favor on the theory is that it is boldly materialist. What could be more materialist, after all, than the clash of arms? And indeed, one of the favorite words in the Marxist lexicon is *struggle*. Warfare, of course, is nothing if not struggle. Still there is a curious duality in Marxist thinking at this point. Struggle *within* societies – the ‘class struggle’ – is embraced by them as the mechanism of choice when it comes to advancing the cause of social evolution. But struggle *between* societies – war – at least an equally important social dynamic, appears to be frowned upon in Marxist interpretations. It cannot be that the ‘founding fathers’ did not countenance it. Engels, after all, made it very clear that warfare was of paramount importance among the ancient Germans.

I think, though, that we are approaching the real answer. It may very well be that in the circumscription theory the *dialectic* plays no role! And the dialectic seems to be the touchstone above all others applied by Marxists in deciding if a theory – or a theorist – is acceptable or not. In this regard, Stanley Diamond, one of the leading Marxist scholars in anthropology, relegated Leslie White to the lower status of a ‘mechanical materialist’ because even though White – more than Marvin Harris – was the anthropologist who first trumpeted cultural materialism to the profession, he made no use at all of the dialectic (Diamond 1974: 341).

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**David Sneath** brings the steppe nomads into the discussion of state formation and raises the question of how, in a vast expanse of treeless plains – an area which was anything *but* circumscribed – it was possible for a state to emerge. He has no doubt, of course, that the state *did* emerge there, but his point is that my theory is

incapable of explaining it. And I admit that, largely for this reason, I have given central Asia a wide berth! To be more specific, I have not even attempted to wrestle with the problem of how states could have arisen in such a region, so devoid was it of all those features which (according to the theory) are most conducive to the rise of the state. Even in my 'Reformulation' I have paid no heed to the states created by horse nomads. And that, of course, is a shortcoming for any theory that purports to account for the state wherever it arose. Having made this admission, let me face the problem squarely and see what can be salvaged.

Since the steppes of Asia had neither environmental circumscription nor resource concentration, what we are left with is warfare. Warfare is the dynamic element of the theory, and we know for a fact that warfare was prominent here. The galloping hordes of Attila the Hun and Genghis Khan, sweeping across the plains of Eurasia, striking terror and wreaking havoc wherever they went, are vividly inscribed in our imaginations. Thus, even though the *conditions* called for by the circumscription theory were lacking in the steppes, its principal *mechanism* was certainly present and active there. And that surely counts for something.

Now, just how warfare was able to weld together peoples as fugitive 'by nature' as pastoral nomads – a job akin to the proverbial task of herding of cats – I leave to specialists in the area to determine. From general principles, though, I am confident that it can be done. Indeed, a solid theory may already exist, or at least can be hammered together from facts already known.

On another matter, I must say that Sneath's failure to regard the chiefdom as a useful intermediate stage between autonomous villages and the state is something I find very puzzling. All the more so, since everything he had said up to that point sounded eminently reasonable. As a distinctive political unit, the chiefdom has become so well known from ethnographic and ethnohistorical accounts that Sneath's rejection of it surpasses understanding.

Sneath also attacks the idea of evolutionary stages, the existence of which I am staunchly prepared to uphold. He may be one of those modern theorists who enthrone 'process' and decry 'stages' as an outmoded concept. That, however, is far from the truth. 'Process' and 'stages' are by no means antithetical to each

other. Stages in social evolution are nothing more than distinctive and significant way stations along the road societies have traveled in their journey from simplicity to complexity. As such, in working out the course of evolution, I regard them as indispensable.

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Unlike certain others who chose to comment on my article, **Ludomir Lozny** focuses on the circumscription theory as a whole instead of picking at certain elements of it. He understands that my aim was to formulate a *universal* theory, applicable to all cases of state formation – or at least to a large preponderance of them. Using his own special set of categories, Lozny analyzes the theory into its various components. He also recognizes that what I have set forth purports to be a *unitary* theory, combining several causal elements into a single composite whole. Not every commenter has taken cognizance of this, which is a new element in my exposition of the theory, not having been part of my original article.

On the basis of his own observations of early state formation in north-central Europe, Lozny agrees with my contention that ‘warfare ... is critical in the evolution of political systems’ (p. 72). Altogether, then, I have very little to say about Lozny's comments other than to indicate that I am ready to embrace him as an ally.

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A recurring criticism of my paper revolves around my failure to treat religion as a factor in the rise of the state. And **Fred Spier** can be added to that hearty chorus.

First of all, I must remark that Spier appears to be a sympathetic critic of the circumscription theory, being favorably impressed by my reformulation of it. He especially welcomes the emphasis I now place on resource concentration. At the same time, though, he appears disappointed at my ‘strong emphasis on rejecting religion as an important aspect of early state formation’ (p. 101). Commenting on my failure to give religion its due, he cites his own work on Andean religious beliefs, which, although it deals mostly with the contemporary Quechua, still has something to say about ‘the rise of early states in that region’ (p. 101).

I was unable to consult Spier's work on Peru, but I am familiar with Geoffrey Conrad's description of the growth of the Inca empire. And I suspect that Conrad tells much the same story as Spier

does. At any rate, Conrad makes a convincing case for religion having been a powerful motivating force in Inca wars. Thus, I am sure, Spier would find Conrad's account very much to his liking. And so I will proceed to quote relevant passages from it.

Starting out as a petty state centered around Cuzco, it took the Incas less than two centuries to make themselves masters of thousands of square miles of territory and of a population estimated to number some 12 million people. How were these impressive results achieved? A first-rate fighting force must surely have been required. But Conrad thinks this was not enough. He believes that to inspire the 'fanatical imperialism' of the Incas 'a strong and pervasive ideological motivation' was required. And that this 'ideological motivation was ... the key to the Incas' success'.

Prominent in the Incas' religious cult were 'the mummies of dead Inca rulers' which were 'believed to be the divine sons of the "sun god" Inti'. Those mummies were regarded as 'the embodiment of the gods on earth', as well as 'the visible manifestations of the fertilizing forces of nature'. And 'these sacred corpses ... were believed to play central roles in the state agricultural production ...'. Accordingly, 'the Incas' well-being depended on maintaining the prosperity of the royal mummies', which were treated 'as if they were still alive'. These religious ideas had a direct bearing on Inca militarism, for the 'proper treatment of the royal mummies *in perpetuity* was required for the Incas' very existence' and beyond that, the welfare of their mummies was 'made dependent on Inca victories'.

Added to this was the notion that the dead Incas 'retained all the property they ever owned, while the living emperor was forced to conquer new territories in order to ensure his own eternal support' (Conrad 1992: 162–163).

Thus, aside from the purely economic and political gains obtained through territorial conquest, there was a double motive for launching a military campaign: (1) satisfying the insistent demands of the dead Incas, and (2) insuring the future well-being of the current living one. Altogether, then, it is undeniable that religious beliefs must have played a significant role in the success of Inca arms.

These extended passages, quoted approvingly from the writings of the Peruvianist Geoffrey Conrad, should satisfy Spier that I would not deny the role of religion in the triumphant expansion of the In-

cas. Nevertheless, having made this acknowledgement, there is still more to be said on the subject.

What has been documented is the salient role of religion *at the later stages* of the expansion of the Inca Empire. But one can hardly expect that these elaborate beliefs were imparted to the Incas full blown. There is little doubt that they must have developed gradually out of inchoate beginnings. The remaining question, then, ... the question whose answer most interests me, is ... what role did religion play *at the very outset of Andean political development*? Having established that it grew to be of great significance, what we want to know is: did it exist ... was it *essential* to ... the very early stages when the first efforts were made to surmount local autonomy and create supra-village polities?

We know that religion goes far back in Peruvian prehistory, that fact being attested to by the great pilgrimage center of Chavín de Huántar, which flourished as far back as 1000 BC. But Chavín itself, it would appear, is a cut or two beyond the initial steps in state formation. What was religious ideology like when autonomous villages were taking the first faltering steps toward a higher level of political organization? And what did it contribute to that process? My surmise would be that whatever religious beliefs then prevailed, they were much less central, much less instrumental, in providing an impetus to warfare.

Having agreed with Spier's notions of the role religion played in at least the later phases of Inca conquests, it may be that Spier, for his part, may actually agree with me about what things were like when political evolution was first getting started. That this might in fact be true is suggested by his remark on the limited role of the 'oracular religion' of the Incas, adding that, in his opinion, early Andean societies 'would never have been able to transform into early states without recourse to organized violence' (p. 103). And that, he continues, in the unfolding of this process, 'resource concentration and ecological circumscription would have played the major role' (p. 103).

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**Blair Gibson** is correct in asserting that Karl Wittfogel was not particularly concerned with just how the state came into existence. In fact, only a single paragraph of *Oriental Despotism* can

be construed as proposing a theory of state origins. His focus throughout the book was on how managing a large-scale irrigation system was a principal factor in giving rise to several elements of the machinery of the state.

In Gibson's opinion, Henri Claessen is not an evolutionist at all but someone 'not really grounded in ... any sort of physical reality' (p. 52). Rather, he thinks that Claessen's way of dealing with human society is best regarded as a 'cognitive approach'. I am not convinced that Claessen himself would be ready to accept this characterization of his work, but I tend to agree with it – at least when Claessen dons the mantle of theorist.

Turning to more concrete matters, Gibson contends that Uganda's population was not hemmed in. But that depends on what part of Uganda one is considering. The interlacustrine basin of that country certainly had a denser population than any other region, and was indeed partially hemmed in by the various lakes and by the mountains that rose to the west of them. Moreover, it was precisely these conditions – I again insist – that permitted several states to emerge here. In striking contrast, other parts of Uganda, being uncircumscribed and lacking resource concentration failed to produce any political structure above the tribe.

In holding that Bunyoro, Toro, and Ankole made no significant use of the fish that abounded in Lake Victoria and the region's other lakes, Gibson is, I think, very wide of the mark. That the people of these states did not avail themselves of so valuable a food source when it was so readily available is hard to imagine. I am ready to wager, in fact, that Lake Victoria drew sizable populations to its banks well before agriculture had come on the scene, drawn there by a greater variety of cichlid fish than existed anywhere else on earth.

To counter the idea that population pressure was instrumental in giving rise to the state, Gibson quotes Richard Blanton, Stephen Kowalewski, and Gary Feinman – three Mesoamerican archaeologists – as saying that in the Valley of Oaxaca 'leaps in political complexity were associated with population *declines* rather than advances' (p. 52). However, can we take this statement as definitive? In a recent article dealing with political development in Oaxaca, Elsa Redmond and Charles Spencer, archaeologists who have worked in Oaxaca for more than three decades, paint a rather different picture.

The period 300–100 BC in Oaxaca's prehistory, they tell us, witnessed 'Monte Albán's successful transition from complex chiefdom to state'. This phase of the archaeological sequence, we learn, 'saw the development of a much more hierarchical regional administration'. And as for its area and size, Monte Albán 'grew to extend over an area of 442 ha, and its estimated population tripled to more than 17,000 persons' (Redmond and Spencer 2012: 33, 35). This hardly suggests that a 'leap in political complexity' was 'associated with population declines'!

Still, a decline in population in a previously more densely peopled area does not necessarily prevent the simultaneous growth of societal complexity. Redmond and Spencer note that the advances in political organization in Oaxaca were closely tied to heightened military activity. The increased frequency and intensity of war, which they indicate fostered the rise of Monte Albán, occurred at the expense of the other chiefdoms in the valley, whose towns and temples they burned and whose people they killed. The decimation that Monte Albán caused its enemies might easily have decreased their numbers to such an extent that it brought about a decline in population *for the region as whole* but not where a budding state was evolving.

This scenario, which seems plausible to me, should be examined carefully by anyone ready to assert that a decline in population would surely forestall political development in the valley of Oaxaca. It would certainly counter any attempt to give the death knell to a theory of social evolution that relied heavily on population pressure.

At another point in his remarks, Gibson cites Norman Yoffee's claim that chiefdoms never existed in Mesopotamia prior to the Sumerian states. I find this hard to believe. Autonomous villages never achieve statehood at a single bound. Some intermediate phase is always passed through in going from autonomous villages to a state. And if this intermediate phase was not a chiefdom, what was it? It is up to Yoffee and Gibson to tell us what these anomalous, unnamed political entities were, and what to call them, if not chiefdoms. At the very least, they must have been multi-village aggregates of some sort, and they must have had some kind of political leadership. Already we have here two of the major ingredients of a chiefdom!

What could possibly lie behind Yoffee's and Gibson's desire to eliminate from Mesopotamian prehistory a stage which has manifested itself everywhere else in the world where archaic states have arisen?

While acknowledging that 'geography, demography, and ecology ... are important variables' in the process, Gibson concludes his remarks by saying: 'One cannot discount the importance of tradition and culture [ideology?] as significant players ... in processes leading to increased political complexity' (p. 54). Once more let me say that while I recognize the stabilizing and integrating function of ideology, I simply leave that subject for others to pursue. My concern is with ferreting out the dynamic elements that drove forward the process that turned small and simple villages into large and complex states. And these seem to me to be the overwhelmingly tangible and material.

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From his remarks it is clear that **Herbert Barry** is uncomfortable with the notion that war has played a leading role in welding autonomous villages into chiefdoms and chiefdoms into states. Indeed, he is one of those anthropologists who, in the words of Lawrence Keeley, would 'pacify the past'. Let us see how he argues his case.

First of all, I find it hard to understand how the statistics he cites from a study by Melvin and Carol Ember contradict my belief in the prevalence of war among preliterate peoples. Barry tells us that of 186 societies in the Embers' sample, 105 with a history of external warfare 'had definitely or probably not been pacified', presumably at the time they were studied (p. 31). The implication is that the other 81 societies which *had* been pacified nonetheless had engaged in war at some prior time, although they no longer did so. Does this not imply that *all* the 186 societies in the Embers' sample had participated in war at some time in the past? On the face of it, then, these figures would appear to *lend weight* to my belief in the near-universality of war.

Contrary to another of Barry's arguments, being related by kinship is no barrier to war between autonomous communities. The enemies of a Yanomamö village, for example, are usually members of another Yanomamö village somewhere in the vicinity, who are, in many cases, related by blood to the village they are attacking.

Moreover, this is commonly the case in primitive society. You fight not with some unrelated group a thousand miles away, but with neighboring villagers, some of whom are very likely to be your kinsmen.

Hardly an autonomous village exists today which does not have a history of having split over some quarrel among its members. And these splits often involved violence. As such instances demonstrate, not even close kinship is sufficient to prevent villages from fissioning. Nor does a split always prevent further violence between the two resulting halves. I myself once witnessed an occurrence of this among the Yanomamö of Hasuböwei-teri village.

'Favorable conditions' for state formation, says Barry, 'can be described for peaceful integration of independent communities' (p. 32). No doubt they can. But is that evidence that the earliest chiefdoms and states were actually formed in this way?

Quoting Blair Gibson, Barry cites the ancient Germans (among others) as having 'embodied a mixture of coercion and volunteerism' in their rise above autonomous communities (p. 33). True enough. But one has only to read the accounts of Caesar and Tacitus to see that warfare among various Germanic tribes and chiefdoms was the most salient facet of their history.

Finally, Barry cites the formation of the UN as evidence of political entities in modern times having joined together peacefully for a common purpose. But that event, which took place a scant few decades ago, has precious little relevance to how chiefdoms and states arose during the preceding millennia. Moreover, despite its peaceful intentions, how many wars has the UN been successful in averting?

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**Edward van der Vliet** correctly notes that I reject Elman Service's – or anyone else's – managerial theory of the origin of the state. The simple reason for doing so is that the agricultural system that prevailed among autonomous villages at the moment they began to fuse into chiefdoms did not require any overall supervision to function adequately. No manager had to tell the simple cultivators when to plant their fields or when to harvest their crops. They had been doing so quite successfully for untold generations.

But what about large-scale irrigation? It is at this point that the story begins to change. Once an overarching political structure is

in place and has enabled separately irrigated fields to be integrated into a larger system, the need arose for bureaucratic control and management of the entire system. Who could draw off how much water from the system? What hours could he be allowed to do so? All that and more required regulation. And by that time something like the state was already in place, having been brought into being by military specialists, not agricultural ones.

Van der Vliet seems to attribute the rise of 'intermediate or transgenerational' societies (by which he seems to mean chiefdoms) not to warfare at all but to 'dominant personalities and feasting and debts incurred as a consequence of the organization of feasts and ceremonial activities and social obligations' (p. 111). These activities, he thinks, played a far greater role than warfare. Now, as far as his 'dominant personalities' are concerned, I am willing to accept them as one in a constellation of factors that gave rise to the chiefdom. And a recognition of the role of 'dominant men' is one of the new elements in my 'Reformulation'. Ceremonial feasting, however, I would discount as having played much of a role in the process. And here allow me to suggest to van der Vliet that fuller acquaintance with the chiefdom literature would make him less ready to dismiss warfare as being central to chiefdom formation.

In van der Vliet's opinion, only 'centralized political communities and sophisticated military organization' can 'guarantee expansion and state building' (p. 111). Here, in citing 'military organization', he seems to be coming over to my side of the argument. But a military organization, especially a 'sophisticated' one, does not arise full blown. Yet, how could it come into existence if not through some kind of 'apprenticeship' in war? And this, van der Vliet seems disinclined to grant it. But again, he insists that a 'certain degree of social complexity is necessary' before warfare can be effective in state formation. So here we are left with a quandary.

But it is more than a quandary. Here, it seems to me, van der Vliet has impaled himself on both horns of a dilemma. He is unwilling to accept war as the principal instrument causing communities to begin their fusion into successively larger polities. At the same time, though, he says that a 'sophisticated military organization' is required to 'guarantee ... state building' (p. 111). Again, how can this 'sophisticated military organization' just spring into being? Does it not require a long period of development, necessar-

ily beginning with wars involving village against village? Could it be that the unpalatable nature of war, at whatever level, has led van der Vliet – as it has so many others – to turn his back on the harsh reality of war when the entities who are waging it are nothing more than simple villages? Does he feel that small, simple, autonomous villages are too ‘nice’ to fight one another?

Influenced by the remarks of the noted contrarian Norman Yoffee, van der Vliet believes that ‘the concept of chiefdom ... is highly problematic’ (p. 111). I discuss the matter elsewhere, but here let me just caution van der Vliet that when it comes to the subject of chiefdoms, Norman Yoffee is a Pied Piper not to be followed!

Appearing to choose an alternative (or at least an adjunct) to environmental circumscription as a catalyst in the process of state formation, van der Vliet points to ‘networks of communication’ as being ‘at least as important [as circumscription] as a starting-point of state formation’ (p. 112). But a ‘network of communication’ strikes me as a system for coordinating and integrating a state once it is in place, not an instrument for its emergence.

After reading his remarks on the matter, we *know* pretty well how it is that van der Vliet thinks the state did *not* arise: namely, through warfare. But it is hard to form a clear notion of just how he thinks it *did* arise.

At one point van der Vliet makes a very significant statement. He says that ‘the origin of the state in South Mesopotamia ... apparently was strongly stimulated by environmental constraints *caused by the changing environment*’ (emphasis mine. – R. C.) [p. 112]. (Here let me note that van der Vliet seems to be holding hands with the circumscription theory, if not actually embracing it. (But let that pass.) What I would focus on is the *changes in the environment* that van der Vliet alludes to. How might such changes affect the circumscription theory?

This is a question I have never touched on in print. Nonetheless it deserves more than passing notice. The full effect that changes in the environment can contribute to the circumscription theory was first brought home to me by Prof. Wu Wenshan, a Chinese geologist. The essence of his argument, as I understand it, is that climatic changes in certain parts of China – especially the onset of drought conditions – acted to *narrow* the area of arable land,

thus *tightening* the degree of circumscription around the available arable land, thereby *accelerating* the rate at which state formation can occur. This I think is an important consideration to bear in mind when studying the operation of the circumscription theory wherever climatic change seems to have taken place.

Van der Vliet denies any appreciable role to warfare in the rise of the Greek city-states. How then would he account for their formation? How was the autonomy of each agricultural community overcome as the city-states began to take form? (Remember my contention that the voluntary surrendering of sovereignty, by even a small political entity, has never been historically documented.) The ‘predatory raiding’ that van der Vliet mentions for ancient Greece is described by him as ‘oversea ... battling for land’ (p. 112). But one engages in overseas ‘raids’ for booty, not for land. He is ready to allow that the ‘only example of military expansion and conquest here [*i.e.*, ancient Greece] is Sparta’ (*Ibid.*), but if Sparta expanded its territory in later Greek history through military action, as van der Vliet concedes, why not entertain the possibility that the same means were employed by other Greek city-states at an earlier period, *before* there was a Thucydides around to record it?

The political integration of the Athenian polity van der Vliet sees as being ‘achieved by ritual, as well as by symbolic and ceremonial means’ (p. 112). But this was *after* the *polis* had been established. So again I ask, how was the autonomy of the component villages that came together to form Athens overcome? What, in van der Vliet’s mind, brought about the fusion of these once-independent villages into a *polis*? (And let me remind van der Vliet that as far back as we can go in Greek history – with the semi-legendary account of the *Iliad* – warfare was already a conspicuous element.)

I know I am belaboring the point, but we are dealing with a fundamental axiom of the circumscription theory. The transcending of local autonomies and the fusion of villages into multi-village polities – in a word, the formation of *chiefdoms* – is the question of questions when dealing with the initial stages of state formation. If van der Vliet proposes to accomplish this by peaceful means, I think he will find that history – *world history* – is solidly against him.

At the end of his remarks van der Vliet quotes approvingly the statement by Paul Bohannon, which I used to begin my 'Reformulation': 'We know that we cannot answer questions about the "origin" of the state because the factual evidence is buried deep in the unrecorded past'. After quoting this remark I boldly assert: 'Today, ... neither Bohannon nor anyone else would be inclined to utter these words'. But evidently I was mistaken. Even the best theories of state formation, van der Vliet concludes, 'are hypotheses which we cannot confirm by hard means' (p. 112). A most pessimistic assessment, I would say, given the mountains of relevant evidence bearing on political evolution that has been accumulating over the last several decades.

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At the outset, I must say that I find it difficult to understand **Alexander Ganzha** and **Evgeny Shinakov** when they deny that war is *the* mechanism by which smaller polities were forged into larger ones. If that was not the case, what alternative means do they propose to achieve that result? Or do they even *agree* that transcending local autonomies is the first problem that any theory of political evolution has to resolve?

One of the sources on which Ganzha and Shinakov seem to lean heavily in their opposition to considering warfare as of paramount importance in political evolution is Jonathan Haas. Haas, after first turning the spotlight on the prevalence of warfare in the American Southwest, has more recently backed away from war as being essential to chiefdom and state formation. And a very specific reason lies behind his having done so. The reason is Caral.

Caral is an important archaeological site on the north coast of Peru containing impressive architectural remains. It was the site of a well established chiefdom that flourished some four thousand years ago. Its most striking characteristic, though, is that, according to Haas, who has worked at the site, it lacks any trace of having been involved in warfare. This, of course, runs counter to my views on chiefdom formation. Now, is there really no evidence of warfare at Caral? Did warfare really play no role whatsoever in Caral's rise to the level of chiefdom?

What are the alternatives? Could it be that evidence for warfare was once there but has been eroded away? Or might it be that it is

buried under tons of sand? Or was such evidence obliterated by Caral's inhabitants once their valley had been politically unified and there was no longer any need for the fortifications that might once have defended one part of the valley from another? Those all strike me as possibilities, although Haas denies them.

But let us assume that Haas is right and that warfare was indeed absent from Caral, even at its initial stages. That admittedly would pose a serious challenge to the circumscription theory's claim to being universal. Since, of course, for any theory purporting to account for every known instance of a phenomenon, even a single genuine exception is damaging. First though, I need to be convinced beyond the point I am now that warfare never existed at Caral.

Now, when a theory has proved itself successful in a very substantial number of cases, any apparent exception to it must be scrutinized very carefully to make sure it is, in fact, an exception. That, I feel, has still to be done in the case of Caral. That, however, is an issue between Jonathan Haas and me. For the present, Ganzha and Shinakov are entitled to hold Caral against the circumscription theory – until such time as I can prove otherwise. On another matter, I quite agree with them that the center for the study of social evolution – or at the very least, for the dissemination of the results of such studies – has moved to Russia. And the best evidence of this is the existence of the present journal, which is, far and away, the leading journal of its kind in the world.

Ganzha and Shinakov seem to regard it as 'a step backward' on my part that I argue for a *unilinear* theory of state formation. They would look with greater favor on a *multilinear* theory. So this may be the place to clarify what I consider to be the difference between these two forms of evolution.

If we take every archaic state into account, we can say, with some assurance, that in *some* respects they all evolved along similar lines – even if in no other respects than in having evolved the characteristics that *define* a state. However, in a variety of *other ways* these states surely diverged from each other in their evolutionary pathways. Now, as long as our aim is to extract the maximum amount of regularity, of universality, from every historical instance of state formation, we are pursuing *unilinear* evolution.

Of course, though, there is no telling in advance how many similarities they will share. One cannot *posit* the answer, it is a purely empirical question.

However, if we choose to focus on those lines of development in which societies *differed* from one another, then we are doing *multilinear* evolution. Again, we cannot tell in advance in how many such lines these states will have diverged. Elsewhere in these comments, for example, I have cited the difference between the Roman and Inca empires in their respective ways of mobilizing labor: the Romans relied on slaves, the Incas on the *corvée*. And of course, there were other ways in which they differed.

To be sure, from a unilinearist perspective, one can always say: the Romans and the Incas *converged* here as well in that they both achieved the same objective, namely, the large-scale mobilization of labor. For without the government-organized mobilization of labor, no political entity would qualify as a state.

Speaking more theoretically, I once expressed the difference between unilinear and multilinear evolution as follows: In pursuing unilinear evolution we start by examining a wide spectrum of instances of state formation (or whatever) and attempt to extract from those sequences all the regularity we can find. The results of such an endeavor – assuming we are successful – is a unilinear evolution. Now, from the unilinearist's perspective, *multilinear* evolution can be regarded as the *residue* left over after we have squeezed out all the unilinearity we can, and found exceptions.

Or, to put our evolutionary search somewhat rhetorically, we can say that actual history, in its full particularity, offers us a rich and dense tapestry, and that we carefully examine its many threads in search for those that form a distinctive pattern.

It is unclear to me if Ganzha and Shinakov are citing Frederick Engels in support of me or against me. What I came away with from *The Origin of the Family, Private Property and the State* was that Engels had a clear notion of the importance of warfare among the ancient Germans (Engels 1942). Consequently, Marxist anthropologists need not shy away – as they seem to – from invoking warfare as an active agent in political evolution.

Finally, a word about theory in general may not be out of place here. If it is ultimately to be successful, *theory* must be based on *fact*. After all, the function of theory is to *explain* fact ... to ac-

count for something in the external world with a general proposition. It is not to build elegant castles in the air. That endeavor can happily be left to the metaphysician.

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At the outset of his remarks, **David Small** tells us that ‘we’ (anthropologists?) have failed to meet the challenge of offering a better explanation of warfare's relation to state formation than I do. My essay, he says, is ‘too outdated to be of current use’ (p. 93), but he fails to cite any alternate way in which the autonomy of local communities could have been transcended. And that is the critical first step which any theory of state formation has to account for. Thus, I invite Small to offer evidence that any autonomous village ever voluntarily gave up its sovereignty. I maintain that only through coercion – war, essentially – did that happen.

In an effort to spread the blame for this failure on the part of anthropologists (presumably including himself) to come up with a better theory, Small admits that ‘we have not been developing models of state origins to compete with the circumscription theory’. Why not? Could it be that it is difficult to find a worthy enough challenger to it ... that, perhaps, the circumscription theory is not so bad after all?

If Small finds my thinking out of date is it because it is not ‘modern’ enough? Perhaps, not *postmodern* enough? One cannot help wondering, though, if a postmodernist theory of the state could really pose a serious challenger to the circumscription theory. For one thing, has anyone even *heard* of a postmodernist theory of the state? Or could it be that postmodernists have more serious issues to concern themselves with? That they have bigger fish to fry? Might it be that they are contemplating such more compelling concepts as *personhood*, *mimesis*, and *alterity*?

(I admit, though, that this is nothing more than an *obiter dictum*. I really have no reason to place Small in the camp of the postmodernists.)

To me, Small's attitude toward warfare strikes me as equivocal and perplexing. On the one hand, he says that war in a circumscribed environment does not necessarily and automatically result in state formation. Well, that is true. For one thing, the process is not instantaneous ... it takes *time*. Anyway, the theory does not

require that environmental circumscription *has* to give rise to the state. It says only that being environmentally circumscribed gives an area *a particular boost* in that direction. Thus, other factors being equal, a state will arise in such an area *more readily* than it will in an uncircumscribed area.

However, when Small comes to discuss the state that emerged in the Titicaca basin, he seems to change the direction of his argument. He says that archaeology offers convincing evidence of the prevalence of warfare in that region, especially during the later phases of state formation. He cites the conclusion of Charles Stanish and Abigail Levine, archaeologists who worked there, that ‘some sort of warfare through territorial expansion was correlated with the rise of states within this [the Titicaca] valley’ (p. 94).

But Small is not satisfied with this ‘correlation’. In order to understand state formation correctly he contends that we need to ‘isolate structural models of cultures’, which would be based ‘on the isolation of institutions and their contexts in the past, and a close look at the social strategies of actors within these contexts and in the creation of new institutions and contexts’ (p. 93). (Unfortunately, he fails to provide us with an English translation.)

Warfare raises its head again when Small brings up the Greek city-states. These polities, he says, ‘rarely engage[d] in territorial expansion’ (p. 92). But if so, it was not for lack of trying. Those polities were frequently at each other's gates, and if one of them did not conquer and incorporate the other, we must look for those conditions that kept them from doing so. I venture to suggest that it was due in part to the broken nature of the Greek terrain. Just as the sharply circumscribed nature of the many small valleys in the Peloponnesus made it relatively easy to unify a valley politically, it also made it difficult for a *polis* to conquer its neighbor in an adjacent valley. As a result, what we find over the course of centuries of Greek history is a series of alliances aimed at maintaining – however, precariously – a ‘balance of power’ among the various city-states.

But let us grant that the Greek city-states were not always after each other's territory. Nothing in the circumscription theory *requires* that they should. The theory states only that the degree of political consolidation achieved in ancient Greece (or elsewhere)

had been achieved, not by the voluntary acquiescence of the villages involved, but at the point of a sword. And since this happened in Greece *before* written history was around to record it, one would have a hard time finding evidence to the contrary – especially since that evidence, in my opinion, simply did not exist.

Agency theory seems to make its appearance when Small complains that my conception of prehistory is one of ‘past societies, but without people’ (p. 92). He supposes that I deny the fact that ‘people ... act ... through leaders who impact social units in definite ways’ (p. 93). Yet, how can he assert that I ignore the individual's role when I take pains to show that it was the *ad hoc* war leader – the *pendragon* – who played such a central role in the rise of the chiefdom?

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First of all, it gives me great satisfaction to have once ‘corrupted’ **Paul Wason!** Not many persons, I am sure, can claim that distinction. And another source of satisfaction is that he has a firm grasp of the argument I am making. It feels good to be understood.

Wason declares that ‘ideology ... may not be essential for the *origins* of the state, however important it will be for [its] maintenance’ (p. 116). Again, this pretty much reflects my thoughts on the matter.

Wason is one of the few commenters on my paper who took special notice of my attempt to show that a *composite* theory of state origins, embracing a variety of causes, can still be a *unitary* one, not one proposing a number of separate theories. This aspect of my argument was not part of my 1970 article, being entirely new to my ‘Reformulation’. And I readily admit that I had to wrestle with the thorny issue of ‘multiple causation’ before I felt I had adequately expressed my ideas about it.

In a related observation, Wason touches on the question of ‘uniqueness’ as regards the way states arose. Let me respond as follows. If we look at every instance of state formation, in its full particularity, it is, of course, unique. No other state arose in quite the same way. Still, a number of elements connect the rise of each state with the rise of every other. The matter could be represented by a series of Venn diagrams. There would be dozens of overlapping circles, each one representing a particular state, with no two

circles covering exactly the same area. In the center of the diagram, though, there would be a 'roundish' area common to all instances of state formation. And this inner core would contain the essential factors needed to give rise to any state. It is *these* essential features that my theory sought to identify and describe.

Now it is certainly possible – and no doubt fruitful – to study the *differences* among the overlapping circles, and thus to get at the peculiarities in the development of each state. It would be illuminating to see the variations in all the causal factors at work and in their intricate interplay. Environmental factors would be among the ones that varied most. Different cultural traditions would likewise come into play. And we cannot forget 'historical accidents', so dear to the hearts of timid theorists who quake at the thought of there being any universal regularities! An examination of how these various factors work together, in varying proportions, to produce a state would be an exercise in *multilinear* evolution – an approach espoused by Julian Steward but never actually practiced by him (Steward 1953).

Wason is in sympathy with my call for a science of culture vigorous enough to risk making real and robust predictions. And still another point of agreement between us is his feeling that in describing the steps and stages by which societies go from autonomous villages to states we need more refined categories than those we now have on hand. Indeed, it is almost unseemly, given the enormous spread between autonomous villages and states, that we have only one category – the 'chiefdom' – to span that gap. Elsewhere I have proposed recognizing three levels of chiefdom, which I labeled *simple*, *compound*, and *consolidated* (Carneiro 1992: 36–37). And this is not to say that even finer gradations in the overall evolutionary trajectory leading to the state would not be useful.

Whenever we are dealing with a continuum – as we surely are in this case – it never hurts to seek out and designate a number of *way stations* to give greater detail to the course being followed. Named stages actually *invite* a study of the process that gave rise to them. I have long argued that 'process' and 'stages' – thought to be antithetical by some – are in fact mutually reinforcing (Carneiro 2000).

But now here at last we have a disagreement! Though in general Wason sees warfare as underlying the formation of chiefdoms, he appears to question my contention that chiefdoms are *always* the result of war. The question is a purely empirical one. If Wason can bring forth a chiefdom which, at its very roots, was formed *in some other way* than through military action, I will be forced to back away from my blanket statement.

In a related issue, Wason points to Stonehenge as showing no evidence of warfare at any phase of its construction. That may well be true, but consider the following argument. Once a large, well-integrated society had established itself in that part of England, peaceful conditions might have reigned for miles around in a sort of *Pax Stonehengica*. But this peace might well have masked the fact that only after repeated clashes of arms was it possible to establish such a *Pax* over the Salisbury plain. And only then did conditions prevail that permitted Stonehenge to be built.

In passing, I should note that my emphasis on the militarism involved in the building up of states has kept me from paying much attention to the years of peace prevailing within even a militaristic society *between* wars. It is a 'vacuum' I acknowledge but that I invite others to fill.

The construction of Stonehenge suggests another important question. Can a leaderless society successfully carry out such an enterprise? Can ordinary individuals cooperate successfully in such a major endeavor without some strong authority directing them? Colin Renfrew recognizes a type of chiefdom he calls *group-oriented* which he thinks lacked a classical paramount chief. One of the reasons he created such a category was the discovery of apparent chiefdoms whose burials lacked the kinds of luxury goods usually found in the graves of illustrious paramount chiefs, suggesting that there were none. This is not to say that Renfrew identified the builders of Stonehenge as having belonged to such a chiefdom, but it is a possibility to be considered.

So let us consider it. Was it, in fact, such a group-oriented chiefdom that built Stonehenge? Did the members of that society work cooperatively, each one doing his part of the work, without a leader telling them what to do and seeing to it that they did it? Again, this is an empirical question, difficult to solve from archaeological data alone. But ethnohistorical evidence can be found that might throw

light on the question. Take for example Polynesia. Is there any evidence attesting to the building of a large stone *marae* there without a paramount chief giving the orders? If so, then we would have an actual case of such an accomplishment which, at first glance, appears highly unlikely.

However, that still leaves open the question of whether, if faced by a real emergency – an attack by a powerful enemy, let us say – a society could defend itself effectively with everyone a private and no one an officer. Here I think the answer would have to be ‘no’. Warfare engenders strict military hierarchy and discipline, and if such did not exist beforehand, the exigencies of the situation would have surely brought it into being.

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In his remarks, **Toon van Meijl** suggests that in formulating a theory of state origins I should follow Karl Popper's dictum of constantly being on the lookout for *falsification*. I should have focused more, he says, on *counter examples* of state formation to mine, instead of on any ideas that fold nicely into the circumscription theory. I have always wondered, though, is that the way scientists actually proceed? Or is it a reflection of Popper's underlying – if unexpressed – distaste for science?

Now, to propose counter examples of state formation to the one I actually offer is a role I expect my critics to perform. It is a role I feel no obligation whatever to fulfill. Indeed, I cede it to van Meijl or any of my critics with undisguised enthusiasm. Having said that, though, I should add that I have always had an eye out for counter examples – exceptions to my theory that seemed to have some element of plausibility. However, so far I have been singularly unsuccessful in my quest!

Let me point to one exception, though. It is not a flaw in the theory, but rather a seeming failure of one corner of the world to conform to it. The place I have in mind is an area which might well have given rise to a chiefdom but did not. This is the Apa Tani valley in northeastern India which, being a bowl entirely ringed by mountains, presents a classic example of environmental circumscription. The valley has a dense population too, so it has all the elements that should have led it to become politically unified, thus spawning a chiefdom. But no such chiefdom existed there. Its several villages remained autonomous (Furer-Haimendorf 1962).

Not only was the Apa Tani valley densely populated, it also had the distinction of containing the largest autonomous village I know of. Previously I had never found an autonomous village with a population exceeding 3,000, but in the Apa Tani valley there was a village which numbered some 7,000 persons.

Now, when something exceeds one's theoretical expectations, there is always the inclination to ask, why? Why did this encircled and densely populated valley not develop a chiefdom? One possibility, of course, is that long ago, before modern observers were there to record it, it once had had a chiefdom but that it later broke down into its component parts. I readily admit, though, that this is merely a hopeful speculation in attempt to do away with a troublesome exception. For now, though, it must stand as an exception to expectations.

However, this is a somewhat superficial analysis of the case. Let us examine the matter more deeply. The first thing to keep in mind is that the circumscription theory does not say that a circumscribed area *must* give rise to a chiefdom or a state. It only specifies that being environmentally circumscribed is a *great facilitator* for an area to give rise to a chiefdom. The presence of other auxiliary conditions will determine whether it actually does so or not.

Van Meijl himself cites counter examples – if such they actually be – of polities created, not at the point of a sword, but through the action of an irenic *ideology*. These cases turn out to be the very examples I myself quoted from among those offered by Jan Vansina and Henri Claessen. I say 'if such they actually be', then, because they were not offered as *documented* instances of state formation through peaceful means, but only as *alleged* instances of it. And, as I stated in my paper, I am disinclined to accept them as such.

Now, that an archaic state would use ideology to 'explain and justify violence and hierarchy' (p. 81), as van Meijl contends, can hardly be denied. Ideology may indeed have strengthened the arm that wielded the sword that gave rise to chiefdoms and states. But was it not the hard edge of the sword that actually won the battle? And it was a succession of those hard-won victories that ultimately brought large polities into existence, and not just *thinking* about it.

Van Meijl insists that ideology 'must no longer be considered as a passive reflection of socio-political relations' (p. 81). And again I am ready to accept it as an active element in the process. It

can certainly serve to ‘stiffening the sinew, summon up the blood’, as Shakespeare put it, thus impelling the warrior on to great deeds.

Take for example the Arabs who, early in the eighth century, swept across North Africa, overran Spain, and knifed deep into the heart of France. Did they not have in their heads the idea of  *Jihad* , a doctrine which spurred them to wage war against any infidels who refused to accept Islam? And were the Arabs not better fighting men for it?

But at the same time, consider this. Ideology is not self-generating. It is a response to a broad spectrum of circumstances. And it is on these circumstances, rather than on ideology itself, that I choose to focus. Going back to the previous notion, were not the militant  *suras*  that found their way into the Koran reflections of a martial spirit that already existed among the tribes of the Arabian peninsula  *before*  any religious texts had been composed?

But let me return for a moment to Henri Claessen, who is such a central figure in these discussions. I would note that I once heaped praise on him for his fine-grained description of early French history. Indeed, I described it as a ‘brilliant, illuminating, and persuasive piece of work’. However, it also exhibited the sharp contrast between Claessen the theorist and Claessen the historian. While asserting that ‘it seems improbable that war should be considered the, or even a, prime mover behind the evolution of socio-political forms’, in his substantive contribution to the same volume – a chapter on the evolution of the Frankish state – Claessen provided what I called ‘a ringing demonstration of the controlling role played by warfare in early state formation’. Thus, what Claessen had denied in theory, he illustrated in fact (see Carneiro 1987: 766).

Van Meijl seems to suggest that the ‘constructivist approach’, with its ‘focus on practice and agency’ has superseded a ‘scientific approach’ in studying the rise of the state (pp. 79–80). No question about it, ‘agency theory’ is very popular these days. But those who trumpet it as an  *open sesame* , unlocking all doors, seem to disregard the fact that people – agents – behave according to the ‘values’ they have imbibed from the surrounding culture. Where else could these ideas come from? Certainly, individuals have to  *act* , or nothing would get done, but what makes them act in one way rather than another?

Agency theory is often depicted as being opposed to 'process'. To be sure, invoking 'process' is not in itself an explanation, but only the framework of one. Within this framework elements have to interact. Just as I used a *pressure cooker* analogy before, let me introduce a *blender* analogy here. A blender is very effective at mixing the contents within it, but it is never enough by itself. It must have *ingredients* to work with. From the point of view of an individual, 'process' is the interactive stream of cultural elements that are churning within, resulting in his overt behavior. On a larger scale (as Joyce Marcus put it) 'processes represent the amalgamated behavior of multiple agents'.

Contrary to what van Meijl appears to suggest, I certainly do not explain war by reference to biology or genetics. As I have previously affirmed, war is *situational*, an expression of a particular set of circumstances. It is *not* the overflow of an innate pugnacity (Carneiro 1994: 8).

Finally, van Meijl bids me 'shop across the spectrum of theoretical paradigms' (p. 81). I have already done so. And what I have found is that 'competing paradigms' often offer little more than filmy speculations. To be genuinely competitive, what they must do is to deal with hard-edged ideas, based on tangible evidence, and to show how they all fit together in a solid structure. To any such theories I would be unreservedly receptive.

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**Henri Claessen**, a leading figure in these discussions, declares that I misconstrue him as saying that ideology is the only factor required for the state to exist. Not so. What I *do* say is that while he accepted the fact that other factors may be required, he claims that without ideology, no state could arise, and certainly, if it did, could not survive. It is with this statement that I disagree.

Let me try to make my position clear. Ideas were certainly in the minds of men engaged in the train of events that led to the emergence of the state. These ideas no doubt inspired them, nourished their ambition, and gave them the courage to go into battle. A valuable adjunct, yes, but without the military component to go with it, is not enough.

So warfare remains the *sine qua non* of state formation. Nothing else could have broken down the political autonomies of

smaller political units, without which they could not have been welded into a larger, complex structure.

Claessen correctly identifies the various factors missing among the Yanomamö that kept them from becoming a chiefdom. Among these factors he listed the lack of an ideology 'in support of a more developed type of leadership' (p. 36). True enough, but only part of the story. Mōawa, the headman of the village of Mishimishimabōwei-teri, had the force of character required of a paramount chief. Through the strength of his personality he was able to dominate and intimidate his fellow villagers. But also lacking among the Yanomamö were other features, and their absence made Mōawa's formidable personality – his 'ideology', as Claessen might call it – insufficient to bring about a chiefdom.

I continue to maintain that 'success in war' is the primary means by which an autonomous village could spearhead its way to a chiefdom. But war alone was not enough. It was only war *under certain conditions* that could bring it about. And these conditions were not present among the Yanomamö or the Mae Enga, and so neither group was able to take the next evolutionary step. But here is the point. Since they lacked those other essential conditions, their lack of the appropriate ideology to go with them made no difference. Let me try to make the point with an analogy.

If I lack the leg muscles required to excel in the long jump, it hardly matters that I *also* lack the desire to do so. On the other hand, if I *did* have the desire – even the *burning* desire – to excel at the long jump, but still lacked the necessary leg muscles, the desire itself would not bring me any closer to excelling in that event.

Claessen claims that states arose in sub-Saharan Africa without the element of circumscription, population pressure, or warfare having been involved. While he does not name them outright, the states he has in mind may well be the very ones cited by Vansina and quoted by me. Perhaps there are states in sub-Saharan Africa today in which warfare and the other factors cited by Claessen are presently lacking. But that is *today*, centuries after these states came into being. What we are concerned with here, though, is *history* – with how those states arose some time in the past. Can Claessen and Vansina provide hard historical evidence that these states actually arose *without* war having played a part? We have Vansina's conjecture that they were formed *in the mind* before they

took shape on the ground. But we also have L'vova's assertion to the contrary, that the oral histories of these people tell a very different story. They tell of war leaders and conflict having played a critical role in their development. I continue to choose L'vova's oral histories over Vansina's conjectures.

Claessen speaks at some length of the Kachin of highland Burma, noting the intricacy of their kinship-based society. However, the Kachin's loose political bonds did not amount to a *bona fide* chiefdom, so even if warfare was not in their immediate past – and there is reason to doubt this – their present state of affairs of relative peace does not require that my theory be able to explain it.

In a related matter, Claessen puts forward the 'earth priest' as a central figure in the political development of certain unnamed African states. I do not question that such priests existed, nor that they exercised considerable influence or even authority. But I would still argue that it was not *they*, but *military leaders*, who brought about the rise of such states. And whenever there was a clash between the priest and the military leader, it was the latter who generally got the better of it. (For a dramatic instance of just such an encounter, see E. A. Ritter's stirring account of Shaka and the witch doctor in *Shaka Zulu* [Ritter 1955: 111–125].)

Claessen presents the hypothetical scenario (based largely on the contentions of the like-minded archaeologists David Grove and Susan Gillespie) that chiefdoms, and even states, in Mesoamerica arose through the control of the people by the priests, who cowed them into unquestioning obedience by invoking the threatening presence of the awe-inspiring gods. Yet, this reconstruction – smacking as it does of the now-repudiated 'peaceful Maya' – can be challenged. Indeed, the evidence can be interpreted quite differently. Claessen quotes Richard Diehl and Michael Coe to the effect that some representations in stone of what appear to be supernatural beings 'indicate some kind of powerful, all-pervasive and almost certainly centralized theological control over large parts of Mesoamerica during the Early and Middle Formative' (p. 40). Perhaps. But to set against this surmise we have the passage I quoted from Coe himself telling of the intensive warfare over choice levee lands which he is convinced took place among the Olmec.

For a very different picture from that of Grove and Gillespie of what was transpiring in Mesoamerica, consult the archaeological

work by Flannery, Marcus, Spencer, and Redmond in the valley of Oaxaca. They present overwhelming evidence (cited above) of the presence and central role of warfare in the step-by-step process that led, over time, to the rise of the Zapotec state (Marcus and Flannery 1996).

Claessen states that ‘the occurrence of fertile land surrounded solely by inhospitable regions is fairly rare’ (p. 40). Irrespective of their rarity, however, it was this very set of conditions – the defining feature of environmental circumscription! – that was *precisely* the conditions most conducive to the rise of archaic states. It did so in Egypt, Mesopotamia, the Indus valley, the valley of Mexico, the coast of Peru, and elsewhere.

Proceeding to another point raised by Claessen, it is true that warfare and conquest may not always solve the problem of a shortage of land due to population pressure, the problem that often gave rise to warfare in the first place. But it may alleviate the problem by putting the conquered population to work, and by imposing on it high taxes, forcing them to extract more from the land than they otherwise would have.

Was there really population pressure in the sharply circumscribed valley of the Nile? Claessen quotes Egyptologists as denying this. But the case may not be as clear cut as they allege. In my comments on Joyce Marcus I have already dealt with some of the subtleties involved here.

Though I do not think that religion was a prime mover in forging the structure of the state at its beginnings, I readily admit that it assumed a progressively greater role once the state was in existence. In fact, the state and the church often formed a very tight bond. Thus, as I have said before, I do not mean to minimize the role of religion as an integrative mechanism, which is the point Claessen insists on. Every archaic state we know about had developed an elaborate religious system that undergirded its political structure – the ‘state-church’, as Leslie White liked to call it. Nor was this religious system a parallel but separate institution; it was virtually part and parcel of the state itself. As an example of this relationship, White quoted the historian Ralph Turner as noting that in ancient Sumer ‘church and state were so bound together that the exercising authority formed a theocracy, functioning, on the one hand, religiously and on the other – secularly’ (White 2007: 394).

As an example of this close relationship, we need only cite the fact that, as a rule, the monarch of ancient kingdoms was at least chief priest, when not actually a god incarnate.

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**Robert Carmack** is generally supportive of my views on state formation except for my failure to include 'cultural factors, especially those of a religious nature' in the process (p. 35). And in his feeling that I could have paid greater attention to the role of religion he is not only correct but in good company. In my previous comments I hope to have rectified this shortcoming. However, I will make a brief attempt to do so here as well, directing myself more specifically to Carmack's concerns.

Regardless of the underlying causes of war (and among them I continue to find ecological ones the most compelling) men need a warm, emotional incentive to impel them to fight. Human beings, after all, are not by nature belligerent. They are biological organisms and as such are built to avoid death, not to court it. And if nothing else, war is a deadly business. Thus, men must be prepared, by a complex set of cultural means, to risk dying on the battlefield. And so societies that habitually go to war have developed means of instilling in warriors ways of overriding their fears as they go to face the enemy. Courage-instilling ceremonies are, therefore, a common feature of such societies.

These kinds of ceremonies often involve imbuing warriors with a profound hatred of the enemies they are about to face, a sentiment not hard to bring to the surface in someone if the enemy has raped his wife or killed his father. Thus, such motives were probably among the earliest to be instilled in the minds of men who were about to fight. And they remained such until war began to involve massed armies, where the identity of the individual you were about to face was unknown to you.

It was probably not long, though, before religious elements began to enter the picture. Venerated ancestors were calling for revenge for wrongs they had suffered years before. Then the gods became involved. They desired blood. They *needed* blood to be properly nourished. And so warriors, as they went forth, knew they were carrying out a divine mandate as well as responding to a more personal incentive. And, of course, the more highly moti-

vated a warrior, the more redoubtable a fighting man he was likely to be. Religion thus became combined with less exalted motives for undertaking war.

And what were those ‘less exalted motives’? Political ambition, including territorial gain, may have loomed large in the mind of a paramount chief or a king, not to mention the enhancement of his personal power and prestige.

So the reasons societies go to war are multiple and complex. Yet, the outcome of war is often much simpler: triumph and territorial aggrandizement for one side, defeat and loss of land and independence for the other. Viewed from a broad perspective, the overall effect was striking and profound. It was a reduced number of autonomous political units in the world and an increase in their size. Such has been the relentless march of social evolution.

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The comments of **Jianping Yi** are, by all odds, the most substantive and suggestive of all the ones I have received. Rather than criticizing some narrow aspect of the theory, Yi's remarks serve to advance and strengthen the theory as a whole. Specifically, Yi adds important evidence, based on recent archaeological work in China, which permits a further expansion and elaboration of the theory.

First, though, at the time of my original article, I knew so little about China that I classed the rise of the early state there as somewhat aberrant in that it arose in an area which seemed to lack environmental circumscription. But in the intervening years I have learned better. The place where one of the earliest Chinese states arose – the inverted ‘T’ around the confluence of the Wei and Yellow rivers – was indeed environmentally circumscribed. Mountains rising 5,000 feet or more stood behind either bank of these two rivers.

More noteworthy yet, Yi's comments called my attention to the fact that in at least three other regions of China parallel but apparently independent developments were taking place. In these areas, dozens of autonomous communities were brought together into chiefdoms, and in some areas into states. As evidence that warfare was involved in the aggregation and consolidation of these settlements into chiefdoms and states, Yi points to the 70-odd walled villages and towns found in these regions, dating back to Neolithic and Bronze Age times.

While similar to each other in a number of respects, these developments differed in certain others. And these differences are worth close examination. Yi cites three types of region in China in which major steps in political evolution were taking place. The most significant differences between them, from our point of view, was their varying degrees of environmental circumscription.

The most tightly circumscribed of these regions cited by Yi was the Chengdu Plain of Sichuan province in central China. Here rivers that eventually flowed into the upper Yangtze were flanked by hills and mountains, creating a number of circumscribed pockets. And in this region environmental circumscription was supplemented by resource concentration as factors facilitating the development of successively larger polities. Here, Yi tells us, archaeology has revealed 'a complete series of political evolution ... [that is] from an egalitarian village to chiefdom and to a state' (p. 122). So in Sichuan province one finds exemplified what Yi calls my 'static model' of state formation – the one described in the article of 1970 – in which environmental circumscription predominates over all other factors.

The second type of region, where a considerable degree of political development also took place, was marked by a lesser degree of environmental circumscription. Being less tightly hemmed in, the population living there was better able to move.

The third type of region distinguished by Yi was 'even more open and thus, more accessible' to movement by its inhabitants (p. 124). At the same time, though, the people living here were more susceptible to being impinged on by adjacent populations, so that social circumscription was more prominent here as a causal factor.

Yi notes – as we might have expected – that 'only in the areas under the first and second categories [do] we find complete series of political evolution. In the areas under the third category the highest social stage found was the chiefdom' (p. 124). And, he further notes, 'most of the areas in China, where chiefdoms and pristine states arose, did not have tightly constructed environments, and thus were not able to prevent the vanquished from fleeing to other areas' (p. 126). Again, not unexpectedly.

Where environmental features did not offer a 'classic' example of circumscription, it was left to social circumscription to bring about

that degree of population pressure that occurred here. As a result, warfare was unable to bring about an early and easy consolidation of villages, since 'there were still enough "leakages" for those who wanted to leave' to do so (p. 127). And while Yi himself expresses some doubt as to why political evolution in this region did not proceed beyond the level of chiefdom, this 'leakage' would seem to provide at least part of the answer.

Yi proposes to distinguished two forms of state formation: a 'static model', which applies to areas of tight environmental circumscription, and a 'dynamic model', so called because it applies to more open areas where populations were freer to move about and thus more difficult to subjugate and unify. In such areas states tended to rise more slowly (if at all), and to fall apart more readily.

Since China is a land interlaced with rivers and dotted with lakes, Yi is surely right in pointing out that this richness in riverine and lacustrine food resources 'might have played an important role in population growth ... ultimately increasing population pressure' (p. 127).

Here then is another facet of the process. To judge from Yi's observations about the occurrence of resource concentration, the pre-history of China may provide actual examples of what I rather tentatively proposed when comparing the Olmec area and the valley of Oaxaca. I said then: 'It might even be the case that under special circumstances – albeit unusual ones – resource concentration may actual *trump* environmental circumscription in giving rise to chiefdoms and states'. Yi makes it appear that, far from being that unusual, this scenario may indeed have played itself out with some frequency in ancient China.

Speaking of resource concentration brings to mind the case of the Harappan civilization in the Indus valley and what role that factor might have played in its political development. Being flanked on either side by deserts, and thus environmentally circumscribed, the Indus valley was a propitious region for an early state to develop. But it strikes me that this must not have been the only factor involved. Might the Indus River – just as did the Niger River in West Africa – not have contributed significantly to the subsistence base on which the Harappan state was reared? In fact, the fish in this river might have attracted a sizable population to its banks even

before agriculture began to be practiced there – just as I suggested for the interlacustrine area of East Africa.

Curiously enough, all the archaeological accounts of Harappan civilization I have consulted failed to say a single word about the riverine food resources of the Indus! Only in the zoological literature have I found any mention of the fish that abounded in that river. And that being the case, one can hardly imagine that fish would not have contributed significantly to Harappan subsistence, and therefore to cultural development along the Indus.

One additional remark remains to be made about ancient China. It is this: A region best suited for a state to arise may not be best suited for its further development. The foremost example of this rule known to me is provided by the Shang civilization. Its antecedents can, I venture to say, be traced back to the big bend of the Yellow River, where environmental circumscription must have played a large part in its early development. But only when the successors of this early state had moved downstream to the extensive and fertile plain of the lower Yellow River did the great Shang civilization reach its florescence.

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**Gary Feinman** contends that my reformulation of the circumscription theory has taken ‘small steps ... away from deterministic ... thinking’ (p. 45). I completely disagree. What it *has* done is to make determinism more *intricate*. But I assure him that causality still reigns.

I am at a loss to understand Feinman's remark that ‘the ubiquity of warfare in the human career has served as a potent critique of the circumscription model’ (p. 45). Does he mean that if warfare is present everywhere then why is it that state formation occurred in some places and not in others? But does he not see that the whole thrust of my argument is to *specify those conditions* that promoted state formation in some areas of the world and discouraged it in others? Moreover, warfare is not simply warfare. It may be ubiquitous, but its immediate causes and ultimate outcomes vary tremendously depending on the circumstances that brought it on.

Feinman, among others, argues against my continued reliance on population pressure as a major component of the circumscription theory. He goes so far as to suggest that in embracing this

determinant I was ‘more guided by habit and faith ... than by rigor’ (p. 46). I find this statement puzzling. Years of scanning the anthropological literature is what led me to its formulation, not ‘habit’ or ‘faith’.

I am ready to concede, though, that the most serious critique raised against the circumscription theory centers on the question of whether population pressure was a necessary element in state formation. I have already dealt with this issue at length in my comments to Joyce Marcus and will not do so here.

Feinman expresses his belief that for further insights into the rise of chiefdoms and states we need ‘a conceptual reframing that shifts the modeling from the construction of law-like propositions and deterministic, unitary sequences toward an approach that examines processes and mechanisms, the interrelationships between key variables that may lead to varied outcomes’ (p. 46). In the interstices of this remark I think I can detect a multilinearity bordering on historical particularism.

Feinman brings up the subject of cooperation, suggesting that I failed to pay it sufficient heed. He may be right. I certainly do not deny its existence in the process that led to the rise of the state. But in my scenario the most prominent element is *competition*. This is so because my focus is on the *struggle* between societies which ultimately saw one overcoming another, the process being repeated countless times, resulting in larger and larger polities. This is competition *in spades*. At the same time, it is undeniable that in order to compete successfully, a society must have the close *cooperation* of its members. And this too can be called cooperation *in spades*. Thus, competition and cooperation may be seen as opposites, but in the actual unfolding of events, they imply each other.

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It is, of course, gratifying to have one's work described as ‘built with a high degree of logical correctness’ by a leading philosopher of science (p. 86). Thus, I welcome **Nikolai Rozov's** remarks and am ready to take his criticisms very seriously.

Rozov does not concern himself with peripheral aspects of the circumscription theory, but seizes on the central elements of it and subjects them to careful analysis following strictures laid down by the philosopher Carl Hempel. This does not mean, of course, that

he overlooks other elements of state formation to which he thinks I should have devoted greater attention. One such element is fragmentation. Time and time again chiefdoms and states spring up only to break down into their component units. In fact, this is a normal and important aspect of the overall course of political evolution. Julian Steward was aware of this when, in his groundbreaking article, *Cultural Causality and Law* (Steward 1949), he proposed a stage in political development which he called 'Cyclical Conquests'. In this stage, recurring wars caused successive breakdowns among polities, only to see them reassembled, but often with different constituent units.

My failure to discuss this aspect of the process is not because I regard political evolution as rectilinear and irreversible. Far from it. It is simply that in both my original article and in its reformulation my concern was with *synthesis* rather than with its opposite ... with evolution rather than dissolution. To be sure, disintegration happened repeatedly. Indeed, there is little doubt that in actual history the breaking down of chiefdoms and states occurred at least as often as their consolidation. However, exploring this phenomenon is something I leave to others (*e.g.*, Tainter 1988).

A related aspect of political evolution is that of stability, perhaps, better referred to as *stasis*. By this I mean the failure of some societies to evolve beyond a certain point. As Rozov notes, 'some polities may cease conquest ... and remain at the stage of chiefdom' (p. 87). I may have given the impression now and again that I regard political evolution as a steady progression, forever upward and onward ... that once having emerged, chiefdoms were inevitably destined to become states.

Such a development was true in some cases, but by no means in all. Indeed, if we take the years before 1500 as marking a period during which aboriginal conditions prevailed throughout the world, we have good evidence that many more chiefdoms existed than did states. But almost all of them were later truncated by the arrival of Europeans before they had a chance to evolve any further. It is true that powerful forces were at work pushing polities to greater heights ... for chiefdoms to grow into states. But this was by no means true of each chiefdom. Many of them, even under pre-contact conditions, stopped short of going beyond that level.

While I was aware of this in a general way, I became particularly conscious of it after learning something about the prehistory of the Cauca Valley of Colombia. Archaeological evidence suggests that the chiefdom level of political organization, which was in full swing here when the Spaniards first entered it around 1530, had already existed there for about a thousand years.

It is true that Guaca, at the northern end of the Cauca valley; and Popayán, at the southern end, seemed to be on the verge of becoming states. The rest of the valley, though, contained some 80-odd chiefdoms that had evidently remained at that level for centuries. Moreover, it is significant that, as described by Spanish chroniclers, warfare in the Cauca Valley was aimed not at territorial conquest so much as at exacting revenge on enemy chiefdoms (Carneiro 1991). Thus, the Cauca Valley impressed me as had few others that not every chiefdom was hell-bent on conquering its neighbor's territory and pushing ahead to statehood.

Now let me rephrase Rozov's remarks about what is involved when a chiefdom turns into a state. I have argued that a paramount chief achieved his status primarily through his strength of character and his military exploits. Most conspicuous at this stage then was not the *office* of paramount chief but the *individual* who embodied it. Paramount chiefs usually attained their office not by election but by *imposition*. If Louis XIV could proclaim 'L'état c'est moi', a paramount chief could equally proclaim that *he* was the chiefdom. Linguistic evidence would back up his assertion, too. Many chiefdoms – Coosa in northern Georgia, to cite but one – were named after the chief who either created it or ruled it.

Once a certain threshold was crossed, though, once a chiefdom was well on its way to becoming a state, the political leader generally obtained his office by *succeeding* to it. No longer was the office the personal possession of one man; it was becoming a socially recognized and formally transmitted position. From then on we can say that the political leader governed by right instead of by might ... by law rather than by force. Nevertheless, beneath it all, one could still discern the iron fist inside the velvet glove.

Turning now to Rozov's 'black box', I find this an intriguing and useful device. A black box, as I understand it, encloses – and to some extent obscures – the workings of the process going on inside it. Within the black box (one can say) are the gears that are

meshing and grinding, their functions to perform. It is the precise workings of this machinery, not immediately visible, that we seek to ascertain.

A black box may also be thought of as the place where impersonal forces are transmuted into personal motives. Here, abstract causes are inculcated into flesh-and-blood human beings who then venture into the world as actors in the unfolding drama of political evolution.

As far as population pressure is concerned, Rozov's remarks suggest one way of posing a problem and suggesting solutions. The pressure of human numbers on the land can be thought of as giving rise to two different kinds of directed forces: *internal* and *external*. If the problem to be solved is a shortage of food one internal response may be to make cultivators work harder. And if this response is insufficient, an attempt may be made to bring new land under cultivation through irrigation or terracing.

However, if these *internally directed* responses fall short of the desired goal, then *external* measures may be tried. First and foremost among them is war. The taking of land from a neighbor is an obvious, recurring, and immediate response. It is from this point on that I have tried to trace in some detail what ensues politically when societies take up arms against each other. From this point on, military successes leading to conquest after conquest led to the rise of states and even to empires.

Rozov concludes that my theory 'almost completely lacks ... cultural [ideological] components'. And, as others have done, he expresses the view that 'the state like any social institution can be sustained and preserved for generations if only being accompanied with a complex of sacred symbols' (p. 91). I have already responded to this criticism in several of my previous comments. Here, therefore, let me simply restate that my *ignoring* of something does not necessarily imply my *denying* it. Or even *minimizing* it. It just means that the focus of my work has been on the more concrete, tangible, and immediate elements of state formation. Ideology, then, including religion, I relegate to a subsidiary and less conspicuous role, which I leave to the attention of others.

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**Donald Kurtz** affirms that in the reformulation of my theory I make resource concentration and social circumscription of 'equal

importance' with environmental circumscription. That is not quite true. They each play somewhat different roles and can be assigned differential weights in the process of state formation. To recapitulate briefly, resource concentration draws people to certain areas where there is a concentration of food resources. Here populations tend to cluster, eventually, as they grow in number, giving rise to social circumscription. This is another form of population pressure produced, not by the constricting effect of surrounding mountains, but by people occupying the peripheries of a region and encircling and impinging upon others held firmly in the middle. Either way, when people feel themselves compressed beyond a certain point, war is the likely outcome, with a familiar train of events to follow.

While warfare is the immediate response to such conditions, considered more broadly, it can be seen as the *mechanism* of political expansion. It comes into play when certain *conditions* reach a certain threshold. Although a variety of conditions may lead to warfare, they may not all produce the same effect. The type of war most conducive to chiefdom and state formation is war brought about by the effects of environmental circumscription. Here populations are more constricted and less able to move away in response to attack. We have, then, what in my 'Reformulation' I referred to as the pressure cooker effect. And it is for this reason that Kurtz is a little wide of the mark when he says that in my current formulation, environmental circumscription is no longer first and foremost among the causes leading to state formation.

I am not at all sure, as Kurtz believes, that Elman Service 'deflated the idea of "prime movers"' (p. 67). I like to think that prime movers still exist, but that they are not necessarily *single factors*, acting in perfect isolation and in perfect unison. Rather, a prime mover may be a *composite set of causes*, closely conjoined to produce a particular effect. If a compact set of causes of this sort far surpasses any other set in bringing about a given effect, then I see no reason for not regarding it as a prime mover.

Warfare, Kurtz says, is not an 'independent variable'. That is quite true in the sense that warfare, wherever it occurs, may be generated by a complex set of circumstances. Nevertheless, warfare remains the engine that drives political evolution onward, and once underway, it acquires a momentum of its own. Indeed, once unleashed, warfare becomes a force hard to contain or reverse. It is

not easy to imagine, for example, that once the Inca armies had conquered much of the Andean highlands they were not going to march irresistibly against the polities of the coast.

Regarding my statement that ideas are not uncaused causes, Kurtz replies that 'ideas may be causal when they are materialized ... in ... [such ways as] irrigation' (p. 68). Now is he saying, not that in and of themselves ideas are causal, but that *only when embodied* in material objects can they be so regarded? I am not sure this way of putting it would be to his liking. I think he would be more comfortable with the proposition that, once generated by material conditions, ideas set off on their own and, as liberated entities, are now capable of becoming causal agents. If he is ready to accept this way of expressing the issue, then I am ready to go along with him. I do so because, in effect, he would be admitting that ideas must come from *somewhere*. That they are not immaculately conceived but must be *generated*, and that the material conditions of existence provide the most propitious seedbed for their germination.

Still I am not sure that Kurtz would like to see ideas as materialized as he seemed to indicate. I suspect he would rather see them causative simply *as ideas in people's heads* regardless of their source. Well, then, let me put the matter in the way I have responded to other comments. Men never go to war – or do anything else – without ideas being inside their heads, impelling them to act. Kurtz goes on to say that he believes ideas and material conditions interact 'more than he [Carneiro] credits' (p. 68). It would be more accurate, though, to say 'more than I *express*' rather than 'more than I *credit*'. So let me repeat again that my stressing the material side of the equation does not mean that I completely disregard the existence or importance of the other.

The circumscription theory would indeed be more convincing, says Kurtz, if bolstered by a broad and systematic array of evidence. To be sure! The positive archaeological data provided above by Jianping Yi is a shining example of what new evidence can bring to the table. But let me remind Kurtz that what I presented here is merely an *article*, not a *monograph*. Needless to say, I would welcome an ampler study that brought together bushels of ethnographic, ethnohistorical, and archaeological evidence, carefully assembled and rigorously analyzed. Let me be so bold, though, as to venture that such evidence would serve to solidify the circumscription theory rather than undermine it.

Kurtz is on the right track when he points out the need to understand the process which transformed an *ad hoc* war leader into the ruler of a firmly established polity. Focused as I have been on the successive aggregations and integrations of villages into larger and larger political units, I have slighted the process that sees the political leader evolving into something light years above the lowly village headman out of which he grew.

One more word about the chiefdom and its head. I have expressed the belief that in its earliest stages the chiefdom is something *imposed* on a group of villages by a powerful and successful military leader. And, as the chiefdom he created continued to grow, it took on more of the character of something lasting, rather than something ephemeral and transitory. Increasingly, then, the structure of the chiefdom came to be regarded by its members as fitting and proper. What started out merely as *de fact*, became accepted in time as solidly *de jure*.

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In his comments, **Stephen Kowalewski** chooses to ignore the circumscription theory altogether. He narrows his focus and trains his microscope on the elemental building blocks – autonomous villages – out of which larger political units were constructed. The problem is that he *denies* that any such units already existed! *All* villages, Kowalewski says, are part of some larger network, thus preventing their being considered autonomous. As an archaeologist, Kowalewski has apparently never lived in an autonomous village. As an ethnologist, I have.

To be sure, the Kuikuru village in central Brazil, in which I did field work, does not exist in splendid isolation. It maintains contact with neighboring villages. It trades with them, intermarries with them, and at certain times of the year it participates in joint ceremonies with them. However, when it comes to *political matters* the Kuikuru village is completely autonomous. In making political decisions it does not consult with other villages, nor does it have to answer to them in any way. Again, it is *autonomous*.

Moreover, in this regard most Indian villages in South America are in the same position. Even a Yahgan community which sometimes consists of fewer than a dozen people, was basically autonomous. To be sure, when a whale was stranded on shore, several communities gathered together, set aside their petty animosities,

and made sure no scrap of the whale was left unconsumed. But what we see here is not autonomy being *transcended*, but autonomy being temporarily *suspended*.

Kowalewski regards the pressure cooker analogy that I use as a way to better understand the build up of population pressure as a *metaphor*. I disagree. A *metaphor* and an *analogy* are not the same thing. The latter involves a more direct relationship, a closer tie between the entity being explained and the parallel being used to explain it. Here one concrete physical process is being compared to another. No vague poetical similarity – as metaphors often suggest – is being implied.

I have never said that the autonomous village was the original condition of mankind. Every anthropologist knows that the *nomadic band* preceded the autonomous village by countless millennia. I simply choose to begin my reconstruction of political evolution at the point in human history when agriculture has appeared on the scene and nomadic bands have transformed themselves into settled villages.

Kowalewski asserts that in South America ‘chiefdoms were more common in pre-Columbian times than they were in the ethnographic present’ (p. 65). That is certainly true. The arrival of Europeans truncated – when it did not decimate – most of the chiefdoms they encountered. Now, it is not at all clear to me if Kowalewski is offering this statement as somehow constituting proof of the *absence* of autonomous villages on that continent in pre-Columbian times. If that is the case, then, given that he accepts the former existence of chiefdoms, out of what socio-political units, one may ask, does he think they were constructed?

It is quite true that individual chiefdoms may cycle up and down the evolutionary track. At some point they may fragment into their constituent units (autonomous villages) only to be reconstituted at some later date, quite possibly with different villages. It is the chiefdom as an *institution* – as an evolutionary *stage* – that endures. Unless of course, as in parts of central Mexico and Andean Peru, chiefdoms evolved into states.

Finally, Kowalewski brings up complexity, a subject to which I devoted little attention in my ‘Reformulation’ but which certainly deserves to be explored. As Kowalewski says, complexity manifests itself not just as a vertical hierarchy but also with a horizontal dimension. The best example of this is the proliferation of occupa-

tional specialties that occurs as societies grow larger and continue to diversify. Thus, the *Wycliffe Bible Dictionary* lists no fewer than 93 occupational specialties that were practiced in the Holy Land during Biblical times (Pfeiffer *et al.* 1961: 1222–1244). More than two thousand years ago, then the increasing heterogeneity so characteristic of social evolution had already shown itself to a high degree.

### EPILOGUE

Facing the comments of twenty-two scholars with a more-than-passing knowledge of one's own field has meant either successfully defending a position against informed criticism, or else abandoning it and seeking higher ground. As the preceding pages attest, I have had to do both – although hopefully more of the former.

Let me begin with some observations about the current status of social evolution. The most salient fact, it seems to me, is that once again social evolution is being warmly embraced and actively pursued. As a valid and fruitful approach to the history of human society it can now be said to occupy an inexpugnable position.

With but a single exception, those critics who have offered comments on my 'Reformulation' paper have accepted the fact that seeking the causes behind the rise of the state is an endeavor that can look forward to being crowned with success. Thus, despite its recrudescence among postmodernists and their ilk, anti-evolutionism in anthropology seems to be a thing of the past.

For the half-century or more that it flourished, however, anti-evolutionism did much to harm anthropology. It stunted its growth if it did not actually truncate it (Carneiro 2003: 75–98). It kept us from tackling some of the great evolutionary questions, such as the one we are dealing with in these pages, namely, the origin of chiefdoms and states. Indeed, the circumscription theory, proposed in 1970, should have been propounded in the 1920s, if not earlier. Enough was known of the relevant facts even then to have allowed this theory to be put forward. In fact, tucked away in a few pages of his *Principles of Sociology*, Herbert Spencer had presented the kernel of the circumscription theory back in the 1870s. But such was the climate of opinion after Spencer's day that no one picked up on his lead and the idea was quietly forgotten.

With their fine-grained knowledge of the native polities of Africa, British social anthropologists were in a favorable position as far

back as the 1930s to advance a credible theory of state origins. Almost every gradation of political structure, from autonomous villages to full-fledged states, were there to have suggested a reasonable series of steps through which villages had passed on their way to becoming states. But so strongly were their sails set against evolution that the British Africanists failed even to try.

Today, though, things are very different. We can now pursue evolutionary reconstructions, not only respectably but with every expectation of success. The fact that a major journal now exists which devotes itself entirely to social evolution is a testament to the vigorous resurgence of this approach. And the further fact that this issue of that journal is so largely devoted to the great question of how the state arose shows beyond doubt that evolutionism has retaken a field which once lay firmly in enemy hands.

One happy trend in modern-day social evolutionism is the joining hands of ethnology and archaeology. When, after half a century of somnolence, evolutionism again sprang to life, its resurgence was sparked largely by ethnologists – Leslie White, Julian Steward, and Elman Service in particular. But archaeologists, whose professional activity has always been the study of change over time, soon came to assume their proper place in this endeavor. Indeed, one can argue that today archaeologists have seized the reins from ethnologists in driving the chariot of evolutionism.

However, as an ethnologist, I would still argue that while the practiced eye of an archaeologist can look at a long-abandoned site and discern that a chiefdom once flourished there, for a full interpretation of how that chiefdoms came to be, the ideas of the ethnologist cannot be dispensed with.

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Having reaffirmed my staunch allegiance to evolutionism, I would like now to turn to more specific remarks about a few of the issues highlighted in the foregoing discussions.

In what ways have I had to accommodate my thinking to the arguments set forth by my critics? First and foremost, I was forced by an insistent chorus of voices to take up the role of *ideology* in the process of state formation, a subject I had previously preferred to keep at arm's length. In a half dozen comments in which I have expressed my opinion on the subject, the tenor of my remarks has been, not to deny or belittle the role of ideology, but to assign it what might be called a *parallel* role. Thus rather than calling it *de-*

*terminative*, I regard it as *supportive*. My concession – if that it be – may be encapsulated in these words: Swords do not wield themselves, and certain ideas, transmuted into motives, must be inside the heads of those who do.

At the same time, though, I continue to insist that ideas do not spring from nothing. They are the products of particular circumstances, and that the weightiest of these circumstances are those most closely linked to subsistence and security, to competition and survival. And the ideas that spring from these considerations exert the most powerful effect on people's actions.

Another issue that came up frequently in these discussions was the role of population pressure as driving territorial expansion and state formation. Arguments and evidence adduced by my critics have forced me to entertain second thoughts on this score. True enough, population pressure often does trigger conquest warfare. But in certain instances such warfare can ensue *before* population pressure has fully manifested itself.

What does this mean for my overall argument? It entails no great change, I think. Like a fencer backed into a corner, but with his saber still flashing, I believed I have fought my way out of this concession without losing important ground. After all, no matter what its causes, warfare still stands as the mechanism *par excellence* for overcoming local autonomies and welding small polities into larger ones. And this, after all, is a major element of the circumscription theory.

Finally, while by no means clasping 'agency theory' to my bosom, I nevertheless have tried to show how the behavior of flesh-and-blood human beings can be *interwoven* with the cultural forces that surround and impinge upon them in bringing about major structural changes in society. This combination of cultural forces and individual action is, perhaps, best exemplified, as I have tried to show, in the process that gave rise to the chiefdom.

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Now, what lies ahead in our study of the rise of chiefdoms and states? What line of investigation appears to be the most promising? To me, the most rewarding avenue is clear: theorists should declare a *moratorium* on reading each other's conjectures and turn instead to a consideration of the *facts*. Solid evidence is the bedrock on which successful theories are built. It should replace the *excogitation* to which so many theorists are inclined. And the solid

evidence I have in mind here consists of the factual sources which ethnology, archaeology, and ethnohistory can contribute to the enterprise.

Let me suggest just two such sources. Some years ago I came across several articles by R. A. L. H. Gunawardana (*e.g.*, 1982) containing detailed accounts of early political developments on Sri Lanka. Gunawardana's work struck me then as eminently worth pursuing as holding many answers to our questions.

An area of the world which was caught in mid-transit from autonomous villages and chiefdoms by the arrival of Europeans was New Zealand. The ethnohistorical sources on the Maori, supplemented by much recent archaeological work, should provide a cornucopia of actual instances of chiefdom formation.

Two things in particular about Maori prehistory caught my eye. One was the astonishing number of hill forts (called *pa*) that were located in New Zealand – some 7,000, if memory serves – attesting to the frequency and intensity of fighting among the Maori. The other interesting detail I have retained was that the most advanced Maori chiefdoms were located on the ‘big toe’ of North Island, a narrow spit of land jutting northward into the Pacific. This was easily the most nearly circumscribed part of New Zealand – just where our theory would have expected political evolution to have proceeded the furthest!

And there are surely other such areas waiting to be discovered, seized upon, and mined. Once reduced, their ore should reveal further particulars of the early stages of political evolution. These other sources, however, I leave for my fellow theorists to unearth for themselves!

I should emphasize here that it is the transitional phase between autonomous villages and the earliest chiefdoms – a transition I have called the ‘flashpoint’ – that is the least known in the entire trajectory of political evolution. Thus it is the one most in need of study. Also, it may be argued, it was the most difficult one for human beings to achieve, since it took some two million years to accomplish. And it is also the most difficult one to pin down. I myself have changed my mind about how it took place, and would thus particularly welcome additional data to either verify my current views or overturn them.

We also need to learn more, in minute detail, of how chiefdoms evolved into states. And in our effort to do so we can turn to the rich coffers of written history. In an earlier passage I cited how,

in his role as historian, Henri Claessen had presented a brief but illuminating account of the rise of the Frankish state. It is this type of account that we are looking for, unencumbered by historical minutiae and focusing instead on broad-gauged structural changes in society.

Finally, in the last half century we have learned a great deal about the origins of chiefdoms and states. And in the train of these accomplishment, we are poised to learn even more.

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