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Book Filled with Lies

Bob Black

The latest of the Director's ironic indiscretions is his heavy reliance on Edwin Wilmsen's *Land Filled with Flies* to bash the anarcho-primitivists. In *SALA*, Bookchin asserted an affinity between anarcho-primitivism and post-modernism, with sublime indifference to the fact that post-modernism has no harsher critic than John Zerzan. To any reader of Wilmsen not in thrall to an ulterior motive, Wilmsen is blatantly a post-modernist. One of his reviewers, Henry Harpending, is a biological anthropologist who is charmingly innocent of exposure to PoMo. He had "a lot of trouble" with the beginning of the book, which contains "an alarming discussion of people and things being interpellated in the introduction and in the first chapter, but my best efforts with a dictionary left me utterly ignorant about what it all meant." Not surprisingly: the jargon ("interpellation of the subject") is that of Louis Althusser, the structuralist Marxist who went mad and murdered his wife. Other anthropologists, more widely if not better read, have noticed Wilmsen's post-modernism. According to Thomas Headland, Wilmsen-style "revisionism is not just testing and rejecting hypotheses. Partially fueled by postmodernism, it seems to be ideologically driven."

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When it was published in 1989, *Land Filled with Flies* created a sensation, as it was meant to. Not only did it debunk the conventional wisdom, it did so as insultingly as possible. Not only did it furnish startling new data drawn from language, archeology and history in addition to fieldwork, it placed them in a pretentious theoretical apparatus. And it seethed with self-righteousness. By not recognizing the San for what they are — an underclass, the poorest of the poor under comprador capitalism — all other anthropologists were ideologically complicit in their subjugation. Since all anthropologists who have lived with the San are strongly committed to some notion of their rights and autonomy, naturally they were infuriated to be castigated as the dupes or tools of neo-colonialism. Rebuttals were soon forthcoming, and the controversy still rages. But Wilmsen enjoyed a strategic advantage: his quadruple-barreled shotgun attack. His linguistic, archeological, historical and ethnographic researches all converged on the same or on congruent conclusions.

Academics are the timid type in the best of circumstances. By temperament they prefer to be the big fish in a pond however small. The phrase “a school of fish” says as much about school as it does about fish. Specialization is the source and the limit of the academic’s authority. The expert in one subfield, such as ethnography, cannot help but lose self-confidence — something he probably never had very much of — when his certitudes are impeached by researches in three other subfields. He begins to wonder if he can be sure of even the evidence of his own senses (or what he remembers to be such). Wilmsen, by purporting to possess expertise in so many areas, intimidates the experts in all of them — at first, anyway. But scholars have started checking up on Wilmsen, just as anarchists have started checking up on Bookchin, and with similar consequences.

Most of Edwin Wilmsen’s observations of 70’s San are strikingly unlike the observations of all his dozen-odd predecessors in the field. Previous anthropologists had already reported

how abruptly the San foraging life-way was succumbing to pressures ranging from protracted drought to entanglement in counterinsurgency in Southwest Africa to the sedentarizing, nationalizing policies of newly and nominally independent Botswana. Nobody now denies that most of the San have been forced into the capitalist world-order at its very bottom level — and while it was happening, nobody did deny it — but only Bookchin is obscene enough to enthuse over this particular extension of the development of the productive forces. He doesn't care what happens to people so long as he can turn it to polemical advantage.

Most of Wilmsen's fieldwork was done at a waterhole he calls CaeCae, whose inhabitants he labels, according to how he classifies their "principal production activities," as variously "pastoralist, independent, forager, reliant, and client" — a rather elaborate typology for just 16 households, only 9 of which were San. There's almost a category for every San household, which rather defeats the purpose of categorization. In 1975–1976, only two households (both San) consisted of foragers, people deriving over 95% of their food from hunting and gathering; by 1979–1980, both subsisted on a combination of relief and casual wage-labor. As for the "independents," who owned some livestock but derived over half their subsistence from foraging, there were three households in the earlier period, two in the later. Those in the other households did some hunting, but subsisted mainly by other means. Now even if Wilmsen's findings are accurate, they derive from a ridiculously small sample, 2–5 households at the most, of people who were obviously caught up in a process of proletarianization so accelerated that it would have made Karl Marx's head spin.

I read a bunch of reviews of Wilmsen's book, pro and con, before I read the book itself. Nothing prepared me for the sheer, shocking near-nothingness of its ethnographic database. Nothing Wilmsen says he found in the field, even if true, refutes

or even calls into question what previous researchers discovered about far larger groups of San at earlier times and in other places. Wilmsen berates his predecessors for ignoring history (they didn't). But he's the one who has trouble accepting the possibility that, just as the people he studied were living differently in 1980 than they were in 1975, the people that Lee, DeVore, Howell, Tanaka and others studied before 1975 might have in a rather short time come to live differently. The historian himself needs historicizing.

Among Wilmsen's most controversial claims is for long-standing social stratification among the San and between the San and Bantu-speaking peoples. Since his ethnographic evidence is paltry, he relies mainly on evidence of inequality embedded in the languages of the San and their Bantu neighbors, such as the Herero. Unfortunately for Wilmsen, one of his reviewers, Henry Harpending, actually knows these languages. Wilmsen claims that a word the Herero apply to the San they also apply to their cattle, implying that the San are their chattels. However, the Herero apply the same word to the Afrikaansers, and nobody would say that the Afrikaansers are the Herero's property. The Herero word implies antagonism, not ownership, just as I do when I say that Freddie Baer is a cow. According to Harpending, Wilmsen derives sociological conclusions from bad puns: "This all, and much more, is fanciful drivel. It is like saying that the people of Deutschland are called 'Germans,' meaning 'infected people,' from the word 'germ' meaning a microorganism that causes illness. Almost every foray into linguistics appears to be entirely contrived, created from nothing, even when there is no reason to contrive anything." Yet another "bizarre analysis," this one drawn from San kinship terminology, Harpending characterizes thusly: "It is as if I were to claim that the English word grandmother refers to a custom whereby old people stay at home and grind wheat for the family bread and that grandmother is really a corruption of grindmother. Of course, if I were to write such

According to Wilmsen, the records left by European traders confirm their commercial activity in the Dobe area. But not according to Lee and Guenther. Repeatedly, the diaries and maps cited by Wilmsen to place these Europeans in or near the Dobe area actually place them hundreds of kilometers away. In fact, the Europeans say that they went well out of their way to avoid the area. It was unmapped — all the maps Wilmsen refers to display the Dobe area as a big blank spot — its commercial potential was limited, and its inhabitants, who were mostly the then-numerous San, were known to be hostile to intruders.

The chicanery doesn't end there. Wilmsen's linguistic flim-flammy, previously noted, isn't confined to obscure African languages where he might hope to get away with it. He mistranslates German too. One of his most highly-hyped findings is in a German-language source which, he claims, identifies "oxen" at an archeological San site. The German word quoted actually means onions, not oxen. Lee and Guenther also adduce other mistranslations which even I, whose German is scanty, found fishy. In self-serving ways Wilmsen inserts words which clearly have no counterparts in the German originals, usually for the purpose of faking evidence of ethnic stratification.

Revisionism in the extreme form espoused by Wilmsen is untenable, but nothing less extreme debunks the primitive-affluence thesis as Bookchin has caricatured it. The reader will by now be weary of !Kung calorie-counting and kindred esoterica: and Bookchin is counting on it. He deploys an argument almost as persuasive as the argument from force, namely, the argument from boredom. Anything you say, Murray, just don't say it to me! Anyone ever involved with a leftist group knows the school where Bookchin learned "process." Bookchin's perverse paradise is precisely this pathology generalized. The winner of every argument is the guy who won't shut up, the Last Man Grandstanding.

like Wilmsen in addition to its trendiness. When he contradicts the ethnography of a dozen predecessors, they are inclined to retort that either conditions changed or Wilmsen is wrong. It wouldn't be the first time an anthropologist with an ideological agenda went into the field and saw what he wanted to see. But if Wilmsen was a latecomer, perhaps a too-latecomer to the field, he was almost a pioneer in the archives where time is on *his* side. If the others point to the 1960's, he can point to the 1860's. Take that! But there is a crucial disadvantage too. There is no returning to the ethnographic 1960's, but the archival 1860's are available for others to visit. Wilmsen's critics did research his sources, as I researched Bookchin's, and with the same devastating results.

Richard B. Lee and Mathias Guenther sought out the traders' and travelers' diaries (in English, German and Afrikaans), the maps, the letters and the other sources on which Wilmsen relied to prove that the remote arid region of the Kalahari where the Lee/DeVore anthropologists found foraging San a century later was a major trade crossroads in the mid-nineteenth century. The Dobe area, according to Wilmsen, "pulsed" with commercial activity in which Europeans, Bantus and San were all heavily involved. On this account the San, however, were herders, not hunters — they were the serfs of the Bantus whose cattle they tended — and when disease decimated the cattle in the late nineteenth century, the San lost their livelihoods and were forced into the desert to forage ("literally devolved, probably very much against their will," in Bookchin's learned words). Even *a priori* there was reason to doubt this remarkable discovery. As Harpending writes: "There is more trade through Xai Xai than anywhere in South Africa! Yet Xai Xai is perhaps the most remote isolated place I have ever visited. I am ready to believe that the occasional trader showed up at Xai Xai, but I am not ready to believe that it was ever a hub of major trade routes."

nonsense it would never be published. Editors and referees would laugh me out the door because they would be familiar with English. But hardly anyone in Europe and North America is familiar with !Kung and Otjiherero."

Wilmsen claims that archeology demonstrates — well, let's let Bookchin say it in his own inimitable way — "The San people of the Kalahari are now known to have been gardeners before they were driven into the desert. Several hundred years ago, according to Edwin Wilmsen, San-speaking peoples were herding and farming [Wilmsen never says they were farmers, an ecological impossibility], not to speak of trading with neighboring agricultural chiefdoms in a network that extended to the Indian Ocean. By the year 1000, excavations have shown, their area, Dobe, was populated by people who made ceramics, worked with iron, and herded cattle ..." These conclusions the Director serves up as indisputable facts. That they are not.

Karim Sadr has recently taken up Richard Lee's exasperated proposal for independent review of all of Wilmsen's controversial claims. Sadr addresses only the archeological claims, and concludes that they are unsupported by what little evidence is available so far. Wilmsen's ally Denbow, as Sadr has recently related, "says that his model is based on over 400 surveyed sites and excavations at 22 localities. The 400 or more surveyed sites, however, provide no relevant evidence. The model is really based on a dozen of the excavated sites, and of these only three have been adequately published."

One does not have to be an expert to notice how forced and foolish some of the Wilmsenist arguments are. Rock paintings of uncertain age depicting stick figures, supposedly San, alongside cattle are claimed to be evidence that the San at some indefinite past time herded cattle. From this premise — even if true — is drawn the illogical conclusion that the San were working for Bantu bosses who owned the cattle (why the San were incapable of owning and herding their own cattle is not disclosed). As Sadr says, "the stick figures may be herding or steal-

ing the cattle, or the Bushmen may have received the cattle in fair trade. To stretch the point, maybe the paintings represent wishful thinking. One alternative is as speculative as another.”

The main evidence cited to show San “encapsulation” by Iron Age Bantu speakers from the sixth to eleventh centuries is cattle and sheep remains found at San sites in the Kalahari. The proportions, however, are extremely small, like those found in the Cape area where there were no Iron Age chiefdoms to encapsulate foragers. The evidence of all kinds is scanty and inconclusive. San might have been encapsulated at certain times and places, dominant at others. Nothing rules out the possibility “that they may very well have retained their autonomous hunting and gathering way of life until historic times.” Wilmsen claims that when Europeans perceived hunter-gatherers (in 19th century parlance, “savages”), they were constructing them as such in accordance with ideological preconceptions. But when Herero pastoralists, refugees from a vicious German military campaign in Southwest Africa, passed through the Kalahari in 1904 and 1905, they, too, saw only San who lived entirely by foraging. It is unlikely that these Bantus were readers of Jean-Jacques Rousseau, Lewis Henry Morgan or Friedrich Engels. It is almost as if the San would have been foragers even if there had been no Europeans to construct them.

Which brings us to the strictly historical content of Wilmsen’s case. He made more, and more systematic use, of archival evidence than any previous ethnographer of the Kalahari. Identifying these sources and emphasizing their importance may well be his only lasting accomplishment. What he made of them is something else again. Travelers reported seeing “Bushmen with cattle somewhere in the Kalahari in the nineteenth century,” but since nobody ever doubted that Bushmen have long been in contact with cattle-raising Bantu, this does not prove anything about the Bushman way of life. Wilmsen denounces the classical social evolutionists and also

those he derides, with questionable cause, as their latter-day inheritors. But he shares with them the assumption that upon contact with the higher, more complex systems of society, the lower, simpler systems are subsumed or else wilt and wither away. To Wilmsen, as to Bookchin, it is unthinkable that foragers might hold their own against herders or farmers. They are, by definition, inferior! Exposure to a higher level of social organization is like exposure to pathogens to which the savages have no immunity. Trade or any other interaction necessarily subordinates them to those with a higher, more sophisticated form of society.

The only thing wrong with this assumption is everything. It begs the question. For all anybody knows, foragers might have dealt with their neighbors from a position of strength. If you look at the situation from a purely military perspective, for instance, the foragers had definite advantages over the sedentary Bantu herders. The Bantus permanently occupied villages whose locations were easy for an enemy to ascertain. The San often moved their campsites, taking their scanty personal property with them. The Bantus mainly lived off their cattle, whose whereabouts were easily known, and which could be stolen or killed. The San lived off of wild game and gathered plant food which no enemy could destroy or despoil them of. The Bantus could probably mobilize more manpower for war than the San, but to do what? There’s no reason to think that Bushmen and Bantus have, or ever had, some cause of chronic conflict. Wilmsen’s own argument holds otherwise. The peoples had some incentive to interact, perhaps some incentive to avoid each other otherwise, but no known incentive to wage permanent war on each other.

It is above all with history that Wilmsen seeks to overawe the anthropologists. His book is very much part of the historical turn the discipline has taken in the last twenty years. “People without history” nowhere exist. Berating other anthropologists as ahistorical possesses a strategic advantage for someone