This monograph is described as an “outgrowth” of a Ph.D. dissertation submitted to the University of Michigan, which seems to be something different than simply a revised form of the original dissertation. I am uncertain who the intended audience is; the book seems to be designed for a more popular audience, on the one hand, since it does not go into depth on a number of points. However, it presumes a specialist knowledge in order to work with the data presented.

The main thesis is that ethnographic evidence from diverse cultures shows a probability that complex chiefdoms immediately preceded state formation and that this is likely to have been the case in the highlands of Palestine in the twelfth–eleventh centuries before the emergence of the state in the tenth-century BCE (xiv).

In chapter 1 the author states that the highlands “constitute a self-contained ethnicity on stylistic grounds” (2) and were the direct antecedents of Iron II Israel and, thus, can be called “Proto-Israel,” regardless of what they called themselves. Even more, since the Merneptah Stela records the name of at least part of this community as Israel and archaeology establishes a continuity between Iron I and Iron II in the region, the “proto” label can be dropped and the Iron I complex chiefdom(s) can be called Israel.
In chapter 2 Miller introduces and justifies his use of the anthropological model of complex chiefdom as a starting point and source of generation of questions to be asked. Since models summarize current thought or help raise new questions, in his opinion, it is appropriate to begin with a model. He argues that if scholars want to reject typological analogy as legitimate, they must do so for reasons other than lack of proximity. Models are not true but useful, meant to explain a significant body of data.

The model is introduced in chapter 3. Miller acknowledges that Israel would have been a secondary rather than pristine complex chiefdom; kingship had already existed in the area in the Late Bronze period. Three traits are recognized: tribute mobilization, cycling, and sacralization. The paramount chief’s role is the mobilization of goods from self-sufficient local units and not redistribution. He does not control the means of production but receives tribute in the form of consumable goods and locally made exotic crafts, which he repays through sacral powers and activities such as intercession. The crafts are then traded with elites of neighboring societies, and the received goods enhance the power and rank of the chief, who amasses them as the fountainhead of the society’s wealth and distributes some to his subchiefs and they to the ordinary people. Cycling involves a fluctuation between two and three levels of control, where the paramount chief tries to circumvent his subchiefs and occasionally, where the paramount chief disappears, leaving the subchiefs in control, until one manages to make himself a new paramount chief.

In chapter 4, the archaeological correlates of the complex chiefdom in the Iron I highlands are presented. As a preliminary, Miller argues that the archaeology and its interpretation will define the “boundaries” of Israelite settlement in Iron I, so no effort is made to decide which sites are Israelite before assembling the data. He then asserts that, archaeologically, a distinct settlement occurred between Jerusalem and the Jezreel Valley in Iron I. Six site attributes are cited for the complex chiefdom, taken from Pebbles and Kus: ascriptive rank, differences in scale and wealth between households, settlement hierarchy, an abundance of resources near settlements, and monumental architecture or similar constructions that would require conscripted labor, and evidence of warfare or defense. Of these only the final one is acknowledged to be found in other forms of organization.

The Gravity Model is to be used to study interaction between sites because it has been argued by one scholar to be particularly applicable to chiefdoms, A single sentence explains this model. It should yield midlevel sites between smaller sites and the main center in the case of a complex chiefdom. Miller then notes that the Gravity Model requires that all sites in a region be included to be useful, which cannot be done currently since the coverage of the surveys that have been conducted is not 100 percent. Missed sites would have a severely detrimental impact on the results. He also acknowledges that
reliability depends on a firm knowledge of a site’s size in the given period, which is almost impossible to establish from survey alone. He proposes to overcome the latter problem by using size-based classes, and he assigns sites to these classes on the basis of a “nonmathematical examination of modern size, percentage of pottery, position in the depositional sequence, erosional nature, duration of occupation, time since abandonment, and the like” (26). He also notes the problems in identifying Iron I pottery and the problem of residuality of forms into later strata.

In chapter 5 the correlates are applied to the central highlands in the twelfth–eleventh centuries B.C.E. Four administrative systems are identified in the heartland, centered at Khirbet Seilun (Shiloh); Tell Balatah (Shechem), which is the largest; Tell el-Farah North (Tirzah); and Tell Dothan (Dothan). Miller indicates that size alone cannot be the determining factor in identifying centers of administrative control; residential hierarchy and monumental architecture (fortification walls in most instances) must be given preference. He is forced to this position because Tell Marjame should be a center given its size, but no architectural remains have been found dating to Iron I. He admits that without the Gravity Model, “one would see only close clusters of sites and other isolated ones, and so reconstruct each system as covering only single ecological zones alongside a few autarkic sites” (40). Foreign goods are found at villages as well as centers. A survey of highland economy by ecological zone ensues, which is followed by a consideration of destructions within the Iron I at certain sites, the limited mortuary evidence available, differences in scale and wealth between households, and protective walls at various sites. The chapter ends with a discussion of boundaries.

In chapter 6 the author discusses the Merneptah Stela as the only relevant textual evidence for Israel at the beginning of the Iron Age, since it dates to the end of the Late Bronze period. He asserts that it puts Israel in its proper geographical location. Chapter 7 is a social history of highland Palestine in the Iron I, based on the previous analysis of chapter 5 and very much steered by the model of the complex chiefdom sketched in chapter 3. Most of the villages were self-sufficient, but they presented tribute in cash crops or conscripted labor to higher levels of economic centers, although without specialized administrative apparatus. The people lived in nuclear families, but often in clusters of houses around a common courtyard with their relatives. The average nuclear family had two or three children who survived infancy. The herders would take animals kept in a room on the first floor of the house out in the morning up to ten to fifteen kilometers away. The larger towns likely had town elders belonging to one family or a single chief and his noble family. They were legally the owners of all the land and likely wore distinctive clothing and intermarried with members of their own extended family or with families of other towns’ elders. The four main polities (Shechem, Shiloh, Dothan, and Tirzah) controlled the north central highlands, while a complex network of simple
chiefdoms controlled walled cities to the south in Benjamin, which nevertheless shared a common regional storage facility at Tell en Nasbeh. The five were of a single ethnicity (98–99, 102).

In chapter 8 Miller proposes that we use the model of the complex chiefdom to describe how such a society textualizes sociopolitical reality, if at all. He then turns to taphonomy, or the process by which items go from active use to deposition in the ground, whence they can become excavated artifacts, as the correct way to approach the problem. He surveys adjoining regions for likely complex chiefdoms and finds only Moab to fit the bill, but finds no written texts anywhere. He argues on analogy that the actual literature of complex chiefdoms probably resembled Tahitian dramas and long family traditions and that the standard approaches that biblical scholars have used to try to deduce possible source material dating to or reflective of the Iron I period in the biblical text is backwards; one needs to go from the active use forward to its deposition, which he then notes is impossible, since we do not have knowledge of every stage of the process.

In chapter 9 Miller examines selected stories or “paintings” of Iron I in the book of Judges against the model of complex chiefdom and concludes that they are consistent with what would be expected. He equates Tell el-Ful with Gibeah and notes that Shiloh, Shechem, and many of the Benjaminitite sites figure in the narrative in Judges but not Tirzah and Dothan. He suggests that 1 Sam 8–31 might reflect the destruction of Khirbet Raddana ca. 1025 by an unknown assailant, which facilitated the renaissance of Gibeah, Similarly, Judg 20 might reflect events ca. 1150 or 1125, when Gibeah and Bethel were destroyed, possibly leading to the overcrowding of Ai and Khirbet Raddana (118). In his brief conclusion to the chapter, he states that Judges might provide information about the Iron I and that it would be most difficult to prove that it does not, but then goes on to state that the biblical text cannot be used as a direct source for reconstructing cultural concepts, nor can archaeology, since the ideological dimensions of artifacts are not known.

This study is bold in its vision but problematic in its application. The author has worked with the model of the complex chiefdom but has not informed us what other models might be possible and why this one was chosen over those others. He also has not discussed how many of the six characteristics of the complex chiefdoms are found in other forms of organization. He notes one is not unique, but none of the six is. More problematically, he has used the Gravity Model to plot interrelations between Iron I highland sites in a situation where he cannot succeed; it is certain that the present surveys have not identified all the Iron I sites, since they have not included 100 percent coverage on the ground. In addition, population sizes are supposed to be used for establishing hierarchies, but instead the author uses site sizes that cannot be determined without reasonable doubt unless extensive excavation is undertaken at each site. We are given no
detailed explanation as to how the various gravity-based systems presented in chapter 3 were determined, how the outlying sites were linked back to the center, or how their limits were determined. Consequently, we are unable to make an informed decision about whether we agree with Miller’s reconstruction or not; we are expected simply to accept it. His stated assumption that all sites were settled simultaneously during the two hundred years of the Iron I goes against common archaeological and anthropological working models and is contradicted by his observation that, according to its excavators, Tell Balatah was not occupied in the eleventh century (35).

The author has relied upon standard interpretations and challenges little. For example, he identifies Tell el-Ful as Gibeah (117), in spite of the compelling objections raised against this by J. M. Miller in 1975. He also accepts the dubious reconstruction of a four-towered fortress at that site in the Iron I period (77) and the dating of the smallest of about two hundred silos at Tell en-Nasbeh to the Iron I (69). He has accepted the dates for destruction layers at Beitin, Tell el-Ful, Khirbet Raddana, and et-Tell (66–68, 118) that were proposed by their excavators as firm and has postulated shifts in population and power based on them. All such dates are approximate, since they are based on pottery chronology alone and can be shifted thirty years or so up or down.

No accounting is made of anomalies that do not easily fit the complex chiefdom model. Why would Ai have been able to survive as such a large but unwalled settlement when so many other smaller sites in the region required walls? Why are there no housing remains at Marjame and Tell en-Nasbeh to go with the Iron I pottery? Is it likely that wooden structures were used at both sites? How plausible is it to suggest Tell en-Nasbeh served as a regional storage facility (69) when Benjamin is otherwise described to have been the home to a number of simple chiefdoms (66)? The four identified chiefdoms in Mount Ephraim and the Samarian hills do not correlate with road systems going from the highlands to the coast (34), yet trade items make it clear that there were regular contacts with the lowlands. What is to be made of the locally made Philistine bichrome pottery found at unfortified stratum IV Tell en-Nasbeh?

The apparent assumption that similar material cultural remains over a geographical region mean a common ethnic identity is problematic. So is Miller’s proposed reconstruction, which has Iron I Israel, an ethnic unity, divided into different types of political groupings. The Benjaminite region cannot be neatly sorted into gravity systems like Mount Ephraim and the Samarian hill country, yet it is still included within the larger ethnic unit, identified as Israel. After analysis, Miller proposes that Israel consists of two complex chiefdoms and two simple chiefdoms in the latter regions and a series of simple chiefdoms in Benjamin (82). Since trade went on between the elites of adjoining chiefdoms in the hill country as well as with neighboring lowland regions, how can we
decide the limits of ethnic Israel from the material remains alone, without texts? Contrary to his assertion, the Merneptah Stela does not allow us to locate Israel geographically in the central hill country at the end of the Late Bronze Age—this is a hypothesis only—and his qualification that at least a segment of the Iron I hill-country population can be identified with this Israel allows for the counterassertion that Israel should be equated with one of his two gravity systems that he thinks represent complex chiefdoms, or with one of the simple chiefdoms. The author includes Gibeon within the common material culture he associates with Iron I Israel, yet he subsequently suggests on the basis of biblical tradition that it might not have been Israelite (118–19).

The suggestion that texts in the book of Judges could and perhaps should be correlated with destruction phases dated to ca. 1150–1125 and 1025 B.C.E. is extremely tenuous, as is the intimation that these texts might reflect real events in the Iron I period because they are consistent with features associated with the complex chiefdom model. All six correlates he gives in chapter 4 are equally applicable to the model of kingship and many to simple chiefdoms. In addition, the presumption that fortification walls are evidence of conscripted labor is possible but not the only option for understanding how such “public” structures get built. If defense is a priority, a community can voluntarily work together to build a protective wall that will benefit all of them.

On a more positive note, Miller has gathered together the results of excavations in the central hill country where Iron I levels have been exposed to any degree and has presented a full account of items that are not likely to have been produced locally, consulting unpublished excavation notebooks in his quest to be thorough. He has also included the results of analyses of clays used in pottery production when they have been done, which have yielded some surprising results. As another plus, he has provided a catalogue of all sites currently identified as having Iron I pottery in the central hill country. He has consistently applied the model of complex chiefdom to the material finds, being guided by the model’s explanations and correlates in his interpretation of the artifacts.

This volume demonstrates for me how severely limited our knowledge of the central highlands in the Iron I period is; of 453 sites, less than twenty seem to have been excavated, and the exposure is extremely limited at some sites, such as Beitin and Tell el-Ful, or the site was problematically dug, such as el-Jib. As a result, I think it is still premature to attempt to establish the political configurations that existed at this time in this region, in the wake of the devolution of the Late Bronze city-states, not to mention the ethnic affiliation(s) of the locals. I have reservations about using sociopolitical models to fill in the many gaps in our data and knowledge, particularly when the archaeological correlates being used are not unique to a single type of political control. It is always
useful to consider possibilities, but it is also necessary, under the present circumstances where so little information exists, to consider a range of possibilities and not lock ourselves into a single model. The author’s analysis unintentionally demonstrates this, as he is led from his gravity-generated systems to propose that simple chiefdoms and possibly dimorphic chiefdoms existed in addition to complex chiefdoms in the Iron I central highlands. Why exclude the survival or regeneration of some city-states, especially in the Samarian hills, or the establishment of some tribally based villages?