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Editors' Preface

It is unusual that an archaeological site, which was previously practically unknown, electrified archaeologists of the Southern Levant and biblical scholars in such a short time and equally made headlines not only in scholarly literature, but also in newspapers throughout the world. The excavations at Khirbet Qeiyafa at the entrance to the Elah Valley, carried out by the Hebrew University of Jerusalem and the Israel Antiquities Authority and directed by Yosef Garfinkel and Saar Ganor, caused sensation from the very beginning. Already in the second year of excavation an inscribed ostrakon was found, which was widely and controversially discussed among scholars. Later, other spectacular finds followed, e.g., the two shrine models discovered at the very end of the 2011 season, which are analyzed in depth in this publication.

The dating of the archaeological remains also created attention, for according to the excavators the settlement, which was enclosed by a wall with two gates, only existed for a relatively short time-span of 50 years during the 10th century BCE – the time of the early Judahite Monarchy. From the moment at which the excavations were associated with the name of David, the first great king of Judah and Israel, Khirbet Qeiyafa was on everyone's lips. Immediately, vigorous debates erupted about the dating of the remains, the biblical identification of the site, and the ethnic allocation of the material culture.

Meanwhile, buses soon brought archaeologically interested tourists to the small parking lot near the foot of the hill, since an excavation with such spectacular and coherent horizontal exposure of an ancient town is rare: walls, gate complexes, dozens of houses one beside the other next to the casemate wall, rooms with indications of cultic activity, plazas and even a small quarry could all be seen at this one site.

The discussions about the finds and findings from Khirbet Qeiyafa among the scholarly community are at times quite heated, not just in Israel. When we invited the members of the Swiss Society for Ancient Near Eastern Studies (SGOA) to a conference on September 6, 2014 with the excavator Yosef Garfinkel and other renowned presenters, it was our aim to facilitate scholarly discussion without undue excitement and at a level at which the main issues could be easily understood. Thanks to the informative and factual contributions, we were able to achieve this aim. The conference participants were able to get a good overview of the significance of the site, the excavations, individual finds and the archaeological and cultural-historical context. Encouraging feedback has led us to make the results of the conference available to the wider public through the series 'Orbis Biblicus et Orientalis'. Even though publications discussing Khirbet Qeiyafa are quite numerous, particularly in Israel and in the English-speaking world, based on its concise layout and content the present volume should nevertheless prove useful to readers. In response to the comprehensive, though naturally condensed, report of the excavator, the con-

tributions of Aren Maeir (Bar Ilan University) and Thomas Römer (University of Lausanne/Collège de France) formulate scholarly questions and comments from various angles and at times also express disagreement. Further contributions continue the discussion about some particular subjects: Benjamin Sass (Tel Aviv University) on the epigraphic corpus of Khirbet Qeiyafa; Stefan Münger (University of Bern) on some details of the material culture; Silvia Schroer (University of Bern) on the iconography of the shrine models. A short epilogue by Ernst Axel Knauf (University of Bern) concludes the present volume.

We want to express our thanks to Yosef Garfinkel for his presence and his considered discussion. We also thank all the colleagues who presented at the conference and later provided these presentations to us in written and edited form. For the co-organization of the conference our thanks go to Dr. Patrick Wyssmann. We gratefully present his bibliography on Khirbet Qeiyafa in an appendix. We would also like to thank Tim Frank for his revision and correction of the language and grammar of the contributions. Nancy Rahn and Myriam Röthlisberger helped us in the preparation of the manuscript.

We thank the executive committee of the Swiss Society for Ancient Near Eastern Studies (SGOA) for the friendly support of the conference and the inclusion in its conference series. We are grateful to the editors of the series 'Orbis Biblicus et Orientalis' and to SGOA for including this publication in the series and for the financial support, respectively.

Bern, August 2016

Khirbet Qeiyafa in its Regional Context: A View From Philistine Gath

Aren M. MAEIR

In light of the excavations and publications of the finds from the two neighboring sites of Tell eš-Šafi/Gath and Khirbet Qeiyafa, I present an assessment of the suggested interpretations of the site of Khirbet Qeiyafa in its broader regional context, and discuss the relationship between the two sites during the early Iron Age.

Introduction

The excavations at Khirbet Qeiyafa, directed by Yossi Garfinkel and his colleagues, with the fascinating finds and their very commendable prompt and comprehensive publication (e.g. Garfinkel and Ganor 2009; Garfinkel *et al.* 2014), have added much to what we know about the early Iron Age in the Southern Levant. Already from the initial reports on the finds from the site and their interpretation, Khirbet Qeiyafa has been at the center of some of the most vigorous and lively debates among scholars of the Iron Age Levant. Just about every possible opinion on this has been brought forward – ranging from full acceptance of the excavators' suggestions, through partial acceptance of the suggested ramifications, and to complete denial of any connection between the finds and the early Judahite Kingdom. Without a doubt, if one just would read the studies relating to Khirbet Qeiyafa and its finds – many hours of library work would be needed! In my opinion, whatever one's stance regarding the interpretation of the site – one has to profusely thank Yossi Garfinkel for serving as the “engine” behind all this research activity!¹

In this paper, I would like to discuss the significance and regional context of the finds from Khirbet Qeiyafa from the perspective of the excavations at nearby Philistine Gath (Tell eš-Šafi/Gath), which I believe was the major polity in the southern Land of Israel at the time. I will utilize this as an opportunity to suggest an interpretation of the broader context of the finds at Khirbet Qeiyafa, and in particular, their significance for reconstructing the cultural and political history of the Southern Levant during the late Iron Age I and early Iron Age II.

¹ As the director of the Tell eš-Šafi/Gath Archaeological Project, and as one of the close archaeological “neighbors” to the excavations at Khirbet Qeiyafa (and of Yossi Garfinkel's new excavations at Lachish as well), I am particularly happy that I have been given the opportunity to discuss the significance of the finds from Khirbet Qeiyafa in relation to the finds at Tell eš-Šafi/Gath. This paper is updated as of August, 2015.

Judahite – Israelite – Canaanite: The Debate on the Identity of the Population of Khirbet Qeiyafa

The unique nature of the site of Khirbet Qeiyafa is apparent. The clear evidence of fortifications at the site (including a well-planned casemate wall, and according to the excavators, two classical Iron Age chambered gates), epigraphic and cultic finds, are on their own quite unique, but if one adds to this the short-lived nature of the main architectural features and their dating (late 11th/early 10th centuries BCE), the cultural affiliation as suggested by the excavators (Judahite), and their belief that the finds of the site can be associated with the incipient Kingdom of Judah (in the time of David), both the finds, and their interpretation by the excavators, are quite extraordinary. Needless to say, the excavators' claims have not been accepted by all.

To start with, I must state that I accept the excavators' suggestion that the site is Judahite – if only for the lack of a better-fitting solution. The material culture does show clear connections with early Iron Age Jerusalem – even if little is known about Jerusalem at this stage (see, e.g. Cahill 2003; Cahill West 2008). The well-known inscription (Misgav *et al.* 2009), while one cannot say with total confidence that it can *only* be Judahite (e.g. Rollston 2011) – nevertheless may very likely be so. The various elements which the excavators connect to Judahite material traditions (pottery, architecture, cult, etc. – e.g. Garfinkel *et al.* 2014) – do in fact fit in nicely with what we know of late Iron I and early Iron IIA Judah.

Suggestions of an early Israelite connection (e.g. Kingdom of Saul – suggested by Finkelstein and Fantalkin 2012) in my opinion are hard to accept. This is so both for the general lack of clear evidence of the Kingdom of Saul, which is a separate issue beyond the scope of the present study, but also the lack of any specific material finds at Khirbet Qeiyafa that enable this interpretation. The lack of pig bones at Khirbet Qeiyafa, which the excavators have suggested can help in determining the Judahite identity of the site (e.g. Garfinkel *et al.* 2014; or to the Canaanite – see below), may perhaps be relevant for raising additional problems with the suggested connection with the early Israelite Kingdom. As Sapir-Hen *et al.* (2013) have demonstrated, from the Iron IIA onwards, in Israelite sites there is a rise in pig consumption, while in Judahite sites, for the most part, abstention from pork continues. This being the case, if in fact Khirbet Qeiyafa should be dated to the transition between the Iron I to the Iron IIA, then perhaps the lack of pig bones might indicate that a southern, Judahite orientation is more likely.

Suggestions to see Khirbet Qeiyafa as a Canaanite site – and connected to a supposed Canaanite “enclave” in the Shephelah, between the Philistines and the Israelites (which has become a very popular explanation in recent years ...) – is a possibility, but I don't see definitive evidence of this. Recently, it has been repeatedly suggested that not only can the Philistine and Israelite/Judahite ethnicities be clearly identified archaeologically, an additional, “Canaanite”

group can be seen in the archaeological record, in the Shephelah buffer zone between the Philistines and Israelites. This has been suggested for the early Iron Age phases at sites such as Beth-Shemesh, Tel Eton and Khirbet Qeiyafa.

Perhaps the most sophisticated of these interpretations has been developed by Bunimovitz and Lederman (2011; Lederman and Bunimovitz 2014), who focus on practices of resistance of the local population in Tel Beth-Shemesh. Accordingly, at the time of the arrival of migrant communities, people at Tel Beth-Shemesh stopped consuming pork and also did not acquire pottery, which they associated with the newcomers. This brings us very close to the emic identification of otherness without falling into the necessity to equate the “others” with a homogenous “ethnic group”. What might have been perceived as a homogenous other by the inhabitants of Tel Beth-Shemesh could have been in fact a very heterogeneous group of migrants and parts of the material culture and social practices of these newcomers were then perceived as being characteristic for all of them by the inhabitants of Tel Beth-Shemesh. This raises the question of who identifies whom as a coherent social group - and that there is even no single “emic” perspective - but competing identifications of the other (and, finally, also oneself) as a group with a joint identity. It should be noted though that the most recent finds at Tel Beth-Shemesh (summer 2015) so far reported only informally, may question this “tight” interpretation, as it appears that in the excavation of early Iron Age contexts a significant amount of Philistine pottery was found, perhaps supporting the possibility that there was a Philistine presence on site.

As opposed to what appears to be a tight, site-specific interpretation which may “hold water”, attempts to formulate an overall definition of ethnic groups living in very clearly defined and bordered regions appear to be hard to justify (e.g. Faust and Katz 2011; Faust 2013). It should be stressed that the very definition of “who is” and “who is not” a Philistine or an Israelite/Judahite is hardly agreed upon. And thus, suggesting to explicitly define the supposedly static ethnic identity of a group living in the contact zone between these groups is fraught with difficulty. The very fact that “Canaanite” (local Levantine) features are seen in Iron Age Philistia and at the same time, a major part of the so-called “early Israelite” culture can be traced to local Levantine (“Canaanite”) origins, makes it difficult to distinguish between a “real” Canaanite – supposedly living in this buffer zone, and a “transformed” Canaanite – who lives in the Philistine and/or Israelite/Judahite regions.

In addition, the suggestion that a Canaanite “entity” existed betwixt the Philistines and the Israelites, may very well be influenced by a modern reading of the biblical text – in particular the mention of Canaanites in this region in the “Tamar and Judah narrative” in Genesis 38 – as there is no clear corroboration of this in contemporaneous Iron Age texts. As very few biblical scholars would date this text to the early Iron Age (e.g. Leuchter 2013), one wonders whether this text in fact reflects a historical reality at all. Can we speak of a

Canaanite group identity in this region during the early Iron Age, and even if so – how can this be identified archaeologically?

Perhaps then, one should prefer to look at the transition between the Philistia-Shephelah-Central Hills, as a region in which boundaries did exist, but they were “fuzzy” and constantly changing. While there is no question that during the early Iron Age there were peoples that identified themselves separately – perhaps as “Philistines” (and they resided mainly in Philistia) and as Israelites/Judahites (and they resided mainly in the Central Hills) – and for the arguments sake – perhaps even “Canaanites” (residing in the Shephelah), it would be very hard to define, at any given time, based on the available archaeological data, the cultural/ethnic affiliation, and more than that – the exclusive group identity – of the inhabitants of a given site in the border zones. Thus, simplistic interpretations of the archaeological correlates for identifying “ethnic” Philistines as opposed to other groups in the Iron Age Levant warrant caution. Similarly, attempts to identify a unified “Philistine identity” may be problematic as well. Not only are the Philistines of a very mixed origin (various foreign components “mixed” in with local ones), as noted above, there are discreet regional differences between the material culture at various Philistine sites. Add to this the fact that the Philistines themselves, as far as we know from the available epigraphic materials from Philistia, defined themselves based on their cities or origins – and not necessarily as “Philistines” in general.

Yehuda Dagan, who conducted extensive surveys (and some excavations) in this region (e.g., Dagan 2010; 2011), has questioned the very dating of the remains from the site. Dagan’s views (2009) are simply unacceptable, since telltale remains that are picked up in survey, even if of various periods, cannot override the results of extensive excavations. Thus, even if Dagan found sherds from many periods, the excavations have clearly and definitively shown that the primary architectural features at the site date to a relatively short time frame.

Lily Singer-Avitz (e.g. 2010; 2012) and Israel Finkelstein and Eli Piasezky (e.g. 2010; see as well Finkelstein and Fantalkin 2012) have challenged the dating proposed by Garfinkel *et al.* While the latter would date the site to ca. 1025–975 BCE, and see it as representing an early Iron Age IIA pottery horizon, Singer-Avitz believes that the pottery is more likely to be late Iron Age I, while Finkelstein and Piasezky believe that the radiocarbon dates that have been published so far from the excavation do not enable such a close dating; they believe that as of yet, the dates can be set only as somewhere between ca. 1050 BCE and no later than 915 BCE – similar, in their opinion, to the dating of various late Iron Age I sites in the Levant. While clearly these views have relevance for understanding the exact role of this site, I believe that by and large, they do not change much in the importance, and character, of the site. Even if one argues that the material culture from the site should be classified as late Iron Age I and not early Iron Age IIA, the character of the site (fortified and of relatively short duration) and its cultural affiliation (not Philis-

tine/coastal), argue quite clearly that the founding and construction of this site should be related to a polity that existed to the east of Philistia. Whether this is a polity that derives from the Central Hills (i.e., the early Judahite and/or Israelite kingdoms) or if from an as-yet unidentified polity that existed in the Judean Shephelah at the time (e.g. Lederman and Bunimovitz 2014), the evidence at present is not sufficiently definitive; I, personally, as mentioned above, prefer in this case, an “Occam’s Razor” approach – and opt for the possibility that it is related to the incipient polities in the Central Hills – what later will be known as the Israelite, and then Judahite kingdom.

Nadav Na’aman (2008a; 2008b)² originally questioned the cultural affiliation of the site, believing that it is not to be seen as Israelite, but rather as Philistine, and in fact, sees it as a satellite site of the Philistine kingdom of Gath (do note that he has more recently opted for a Canaanite identity for the site). This though can hardly be accepted. The stark differences between the pottery from Khirbet Qeiyafa and that of both late Iron Age I and early Iron Age IIA Tell eṣ-Şafi/Gath can hardly justify a claim that these two sites are closely affiliated. Initial evidence of the palaeodiet, even if one should relate to this issue with caution (e.g., Sapir-Hen *et al.* 2013; Maeir and Horwitz 2015), as well as other aspects, only strengthens this claim. If at all, the rather strong similarity to the partially published late Iron Age I/early Iron Age IIA pottery from Jerusalem, strengthens a claims for an inland affiliation of this site.

It should be stressed that despite the clear differences between the finds at Khirbet Qeiyafa and Tell eṣ-Şafi/Gath, it would be rash to claim that one can define a clear border between these two sites, with Khirbet Qeiyafa representing the westernmost position of an inland polity and culture. Dagan (2010: 195–201) recently published a late Iron Age I/early Iron Age IIA tomb that was discovered ca. 4 km to the northeast of Khirbet Qeiyafa, further within the hilly region of the eastern “High Shephelah.” The tomb appears to have quite a few “Philistine” type vessels, and in fact it is quite similar to the pottery found at Tell eṣ-Şafi/Gath, seemingly somewhat different from the assemblage at Khirbet Qeiyafa. This being the case, it would appear that the users of the tomb may have been affiliated with the Philistine culture, and perhaps with the Philistine polity of Gath to the west, rather than to the occupants of Khirbet Qeiyafa – despite the fact that the tomb is located further inland. This can perhaps be seen as indicating that the cultural borders in this region were quite fluid – and perhaps, one should not talk of distinct cultural and/or political boundaries during this period between the coastal plain and Philistia and the inland (Judahite/Israelite?) regions.

This latter suggestion should not come as a surprise, both in light of what is known of the shifting character of border zones between cultural units, and, at the same time, the biblical description of the fluidity of the relationship be-

² Note that he later changed his mind on this issue, and now believes the site is Canaanite in nature (e.g. Na’aman 2010).

tween, and the definition of, the peoples living with the Philistine and the Israelite/Judahite spheres of influence (for an extended discussion of this issue, see Maeir and Hitchcock in press).

The Relationship between Tell eš-Šafi/Gath and Khirbet Qeiyafa

But what can the finds at Tell eš-Šafi/Gath tell us about the relationship between Gath and Khirbet Qeiyafa? In addition to the fact that the material cultures at these two sites are quite different, I believe that the status of Gath throughout the Iron I and Iron IIA can help us understand the role, function and status of Khirbet Qeiyafa during the brief time of its existence during the Iron Age. As we know now (that is including the 2014 season of excavations), Gath was a large Philistine site from the very beginning of the Iron Age (ca. early 12th century BCE) up until the late 9th century BCE – when Hazael destroyed the site (e.g. Maeir 2012; 2013; 2016). In fact, during the late Iron I and early Iron IIA, it appears that Gath was of a particularly large size, ca. 45 to 50 hectares – including an expansive lower city. In addition, throughout the Iron I and early Iron IIA (until the Hazael destruction), the site continued to flourish without any evidence of destructions and or change in cultural and/or political orientations. Thus, it can be safe to assume that the city of Gath served as the primary polity in this region, particularly during the late Iron I and early Iron IIA (e.g. late 11th through late 9th century BCE).

This being the case, the existence of this polity would limit the possibilities of a westward expansion of the incipient Judahite polity on the one hand – and of a southern expansion of a northern early Israelite polity (in light of Finkelstein and Fantalkin's [2012] suggestion mentioned above). Attempts to “explain away” the importance of Gath at this stage and see it as an anomaly – which existed in a “bubble” while the early Judahite Kingdom expanded into the Shephelah unhindered (as Faust 2014 and Ussishkin 2014 seem to believe) – is rather hard to accept. It is now clear that the site was fortified at this stage, as seen both on the upper tell and in the lower city,³ to which can be added its large size (ca. 50 hectares), lack of destructions, and evidence of inter- and intra-regional connections, does not enable one to suggest that this was a site which had not political “clout” – and the city of Gath was not the center of the largest kingdom in the region with a strong influence on neighboring polities and cultures – and their ability to expand into the regions under the control of the Kingdom of Gath.

In this light, as argued in the past (e.g. Maeir 2012), I believe that the site of Khirbet Qeiyafa was a short lived attempt of the Judahite polity to extend its influence to the west, but that this attempt was ephemeral and was quickly

³ In particular, the results of the 2015 season have demonstrated the existence of impressive fortifications in the upper and lower city during the Iron Age IIA. Thus, Ussishkin's (2014) claim to the contrary can be disregarded.

crushed by the Kingdom of Gath. This would explain why Khirbet Qeiyafa is destroyed after a short period – and does not serve as a “base” for the further expansion of the Judahite influence in this area. When Judah does expand into the Shephelah, whether before or after the conquest of Gath by Hazael,⁴ the focus of this expansion is at other sites, such as Lachish. In any case, even if Judah did expand in the Shephelah prior to the late 9th century BCE, this would have only been towards the southwestern Shephelah (the regions south of Lachish).

Final remarks

I would also like to address several other points that Yossi Garfinkel has suggested regarding Khirbet Qeiyafa:

1. The suggestion to identify the site as Shaaraim (Hebrew *sha'arayim*) – although technically a possibility – cannot be accepted *per se*. The fact that the site has two gates does not prove this point – both due to the fact that the “ayim” ending does not necessarily indicate ‘double’ (as already pointed out by others), as well as the fact that the original claim that only this site has two gates has now been disproven by Garfinkel himself at Lachish!
2. Garfinkel’s suggestion (e.g. Garfinkel *et al.* 2012) to see the early Judahite kingdom as being based at three sites during the time of David – Jerusalem, Qeiyafa and Hebron – is hard to accept. First of all, it is hard to see why a site which is on the very western periphery of the Judahite polity would be chosen as a central site. In addition, as discussed above, it is destroyed after a very short period. And most importantly, from all the excavations that have been conducted in ancient Hebron (Tell Rumeideh; e.g., Eisenberg and Nagorski 2002; Chadwick 2005), there is so far no evidence of a substantial late Iron I/early Iron IIA presence on this site – let alone an indication that it was one of the major sites of the Judahite polity! So besides Jerusalem – and even there the remains at this stage are not that impressive (see Cahill 2003) – there is virtually no evidence for this three-pronged urban settlement pattern that has been suggested.
3. Likewise, I don’t agree with Yossi Garfinkel’s suggestions regarding the understanding of the biblical text – and the methodologies currently employed to analyze it – in light of the excavations at Khirbet Qeiyafa. Due to the fact that he believes that evidence for the Kingdom of David has been found at the site, he questions much of the currently accepted methods and interpretations of modern biblical research. Without going into too much detail, this is very problematic. On the one hand, as noted above, the ar-

⁴ See e.g., Faust 2014 who argues for an earlier date, as opposed, e.g., to Koch 2012; Sergi 2013; Lehmann and Niemann 2014, who argue for a later date of the Judahite incursion into the Shephelah.

chaeological interpretation of the site in a simplistic and straightforward manner in relationship to the biblical text is hard to accept. And no less important – the tools and methods of modern biblical scholarship are not something that can be brushed aside based on this or that find - from this or that site! The complex nature of the biblical texts – and for sure those dealing with the “Davidic cycle” – cannot be collapsed into a monolithic, “Sunday School” understanding of early biblical history! This would seem to be completely obvious – and one can only state quite simply that the very evidence from Khirbet Qeiyafa does not support this view! For if we accept a monolithic understanding of the David story – one would assume that the Israelites defeated the Philistines after the David and Goliath confrontation – and the Philistines barely escaped from the field! If this was in fact what happened in reality, one would expect that Khirbet Qeiyafa, as one of the three major sites of the Davidic Kingdom would continue to exist throughout David’s rule and his control over Philistia would be manifested at sites in Philistia (as the biblical text would lead us to believe). But in fact, the very finds from Khirbet Qeiyafa indicate that the picture is very different. Even if one accepts this site as being Judahite (which I believe is the case), the story of the site’s existence and role hardly fits in with a “simplistic” reading of the biblical text.

To summarize, while I am in awe at the finds that Yossi Garfinkel and his colleagues have discovered at the site, am very impressed with the swift and comprehensive publications, and agree in part with some of the suggested interpretations by Garfinkel and his team (in particular that the site is most likely Judahite), other aspects of the interpretive framework which they have suggested – and in particular what it can tell us about the early Judahite Kingdom and the supposed veracity of the biblical texts about this stage in Judahite history – I find hard to accept.

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