FORUM RESPONSE

A Return to the Dark Ages? Reply to Thornton et al. 2010

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Abstract

A synthetic theory pointing out the central importance of metallurgy in the emergence of Bronze Age civilizations was recently published in the *AJA* ("From Metallurgy to Bronze Age Civilizations: The Synthetic Theory" [2009] 497–519). In reaction, six well-known authors (Christopher Thornton, Jonathan Golden, David Killick, Vincent Pigott, Thilo Rehren, and Benjamin Roberts) have written a rebuttal devoted mainly to defending the current localizationist paradigm challenged by the synthetic theory. In this reply, the epistemological nature of localizationism and its position regarding scientific theories is considered first, then three critical points underlying the synthetic theory are discussed: (1) the external mode of crucible heating, (2) the incompatibility of crucible and furnace smelting, and (3) the prevalence of southern Canaan in the emergence of furnace metallurgy.*

INTRODUCTION

In October 2009, the *AJA* published my article proposing a new theory regarding the origin and development of metallurgy.[1] It was termed the "synthetic theory" because it reconciles within a single conceptual framework two ideas about the origin of metallurgy previously considered mutually contradictory: the existence of one single homeland for metallurgy (diffusionism) and the claim to multiple independent homelands (localizationism). The purpose of my article was to generate debate and discussion of the issue.

Both localizationism and diffusionism are unable to integrate each other's data. In the synthetic theory, these incompatibilities are resolved by distinguishing, for the first time, two modes of production of copper: crucible smelting and furnace smelting. While there is clear evidence that crucible smelting appears independently at many locations (localizationism), it is assumed that furnace smelting emerged at one time in the southern Levant during the fifth millennium B.C.E. and expanded rapidly throughout the ancient world (diffusionism). The replacement of local crucible smelting by exogenous furnace smelting is also linked in the synthetic theory to strong social, economic, and cultural changes.

I proposed the synthetic theory after about 50 years of localizationism, during which time the "open questions" asked by promoters of diffusionism were being gradually discarded. In this context, the publication of a new theory regarding the origin of metallurgy would understandably trigger vigorous opposition. Obviously, it is healthy for a scientific theory to be debated and challenged. In their rebuttal, however, Christopher Thornton, Jonathan Golden, David Killick, Vincent Pigott, Thilo Rehren, and Benjamin Roberts adopt an unusual position. They refer to conclusions reached before publication of the synthetic theory as *arguments d'autorité*: "Archaeological discoveries over the last 50 years have shown that the adoption of furnace metallurgy was not a primary cause of the social and political transformations that occurred during the Bronze Age."[2] They also recommend that the reader refer to previous works that promote a localizationist view: "The interested reader is encouraged to seek out some of the classic synthetic works on the origins of metallurgy."[3] It is unclear, therefore, how the current opinion can be challenged by a new theory. The task of a rebuttal is dismissed when the intent of the authors is to defend five decades of archaeometallurgical research to "the wider archaeological audience."[4] No one is asked to defend archaeological discoveries,

Editors' note: We invite readers to participate in further discussion on the issues examined here by visiting the *AJA* Web site (http://www.ajaonline.org), under "AJA Online Forum."

* I would like to thank Editor-in-Chief Naomi J. Norman for allowing me to reply to the rebuttal. Patrick Jean-Baptiste, Hervé Seligmann, Arieh Zaritzky, Philippe Grosos, Yehudit Gavish, and Beno Rothenberg are acknowledged for their help, advice, and fruitful discussion on this topic. I offer here my gratitude to Susana Lezra for help with the English version of this text and for useful comments, to Jean-Baptiste for drawing figs. 1 and 2, and to the Israel Museum for the photograph of the Nahal Mishmar hoard.

[1] Amzallag 2009.

[2] Thornton et al. 2010, 312.

[3] Thornton et al. 2010, 305.

[4] Thornton et al. 2010, 312.

but everyone is invited to discuss theories built on those discoveries.

First of all, there is an epistemological problem. Localizationism, the theory of multiple independent homelands for metallurgy, was first formulated in response to the diffusionism of Gordon Childe, which assumed the existence of a single, Near East homeland for metallurgy. Localization was based on the discovery of many ancient independent sites of metallurgy,[5] but these findings cannot transform localizationism into a scientific theory.

Scientific theories are mental constructs that emerge from the conjunction of organizing hypotheses formulated to reveal a previously ignored link between factors. An organizing hypothesis (correlation of variations of X and Y) is tested with regard to a null-hypothesis (no correlation in variations of X and Y). A null hypothesis, however, is only a *par défaut* hypothesis. No one "makes science" to demonstrate that nothing is related to nothing. If they did, scientific activity would be reduced to the making of lists of collections and inventories.

Considering localizationism the null hypothesis of Childe's diffusionism modifies its epistemological status. A link between distinct sites of ancient metallurgy may eventually not exist. This negative assertion, however, may be defended only if the current data cannot be integrated in any theory. And as soon as a new theory is formulated in a field dominated by a null hypothesis, all of us have the duty to consider it, to check its premises and its predictions. The attempt of Thornton et al. to "set the record straight for the wider archaeological audience"[6] reveals their misunderstanding of the goals of science and its methods. I address briefly the irrelevant arguments in their rebuttal before turning to the scientific discussion.

THE IRRELEVANT ARGUMENTS

Diffusionism

The synthetic theory is identified in the rebuttal as a new version of the old diffusionist theory of Childe:[7] "It is therefore disquieting to see Childe's technological determinism resurface in Amzallag's synthesis."[8] The synthetic theory, however, is not simply a return to Childe's diffusionism; rather, in the synthetic theory, diffusionism is restricted to furnace metallurgy alone and is combined with the localizationism of crucible metallurgy.[9] Thus, their refutation of Childe's diffusionism alone is not sufficient to negate the main argument of the synthetic theory.

The Levantine Paradigm

Thornton et al. consider the synthetic theory as a new formulation of an old concept that they call the "Levantine paradigm." It assumes that metallurgy emerged in the Levant from the early working of native copper, followed by the smelting of pure copper oxides.[10] The Levantine paradigm is obviously a conceptual chimera, since native copper has never been found in Canaan. It actually is the single region where copper metallurgy appears independently of the working of native copper. This singularity is one of the fundamental principles of the synthetic theory[11] that should, therefore, be considered the antithesis of the Levantine paradigm.

Smelted vs. Native Copper

It is not always easy to see a clear-cut distinction between native and smelted copper (i.e., produced from ore) by analyzing artifacts, as Thornton and his coauthors note.[12] A point of origin for smelting activity, however, is required to elaborate a theory about the emergence of metallurgy. In the synthetic theory, this zero point is associated with the first identification of slags. Clearly, one may criticize this criterion; but if so, another criterion should be proposed in its stead—if not, the working of native copper becomes confused with the smelting. This is precisely what happens in the rebuttal. On one hand, the authors state that "there is as yet no actual evidence that native copper was melted and cast prior to the invention of smelting."[13] On the other hand, there is a mix-up between metallurgy and the working of native copper when they write, "Contrary to Amzallag's assertion that the Anarak mining region of Iran displays the earliest evidence of copper metallurgy, the first use of native copper in the world is actually from sites in eastern Anatolia."[14] It is difficult to discuss a theory about the emergence of metallurgy in such a context.

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[5] Renfrew 1973.

[6] Thornton et al. 2010, 305.

[7] Childe 1934; Wailes 1996.

[8] Thornton et al. 2010, 312. To minimize the novelty of the synthetic theory, the authors even reduced it to a skimpy version of the diffusionism of Childe (Thornton et al. 2010, 311–12): "The key difference, of course, is that Childe identified the Near East at large, rather than the southern Levant, as the region from which metallurgical innovation (and, indeed, 'civilization') spread to the rest of the Old World."

[9] Amzallag 2009, table 1.

[10] Thornton et al. 2010, 305: "This model [the Levantine paradigm] presents a historical narrative in which the early use of native copper led to the smelting of 'pure' copper oxides."

[11] Amzallag 2009, 502.

[12] Thornton et al. 2010, 306.

[13] Thornton et al. 2010, 306.

[14] Thornton et al. 2010, 310.

Circumstantial Errors

Thornton et al. claim they have identified errors in my article, but it would have been useful for them to distinguish between circumstantial and essential errors. Circumstantial errors, when identified and corrected, improve a theory, while essential errors refute it. There is no room for circumstantial errors in a rebuttal, its goal being the refutation of a theory. This is why their long discussion about the emergence of Thai metallurgy is irrelevant.[15] The same is true of the discussion of the earliest date of crucible smelting in the Balkans and Iran[16] and the so-called errors noticed in the maps.[17] For the same reason, the criticism about the first use of fluxes in furnace smelting need not be debated here.[18]

Challenging Explanations

A new theory is appreciated by the answers it provides to open questions. It therefore becomes depreciated by explanations revealing that the so-called open questions were already resolved. To be relevant in a rebuttal, however, the current explanations should not be more speculative than the ones proposed by the new theory. This criterion enables the following criticisms made by Thornton et al. to be discarded:

1. In the synthetic theory, the presence of sulfides is intimately related to crucible smelting.[19] In the rebuttal, the following speculative explanation is proposed: "The presence of sulfides in crucibles does not necessarily suggest the intentional mixing of oxidic and sulfidic ores. . . . Instead, sulfides may have entered the crucible or furnace unintentionally as remant phases in the oxide/carbonate gossans that form over sulfide deposits."[20]
2. In the synthetic theory, the lack of a gradual transition from crucible to furnace points toward basal differences between these two modes of smelting. In the rebuttal, this lacuna is sometimes interpreted as a "missing link" in the spontaneous evolution between crucibles and furnaces: "In regions where transitional installations are not yet known, it is likely that this is because of the lack of systematic surveys and the limited chronological resolution provided by archaeological methods at these early periods."[21] The other case invoked, that of "identified" examples of transition between crucible and furnace, is discussed below, under "Gradual Transition Between Crucible Smelting and Furnace Smelting."

These speculative explanations do not challenge the fundamental principles of the synthetic theory. At best, they may serve as ornaments for a null hypothesis.

Distorted Criticism

The authors claim in their rebuttal that many archaeological data were misinterpreted and distorted in my paper.[22] To begin with, Thornton et al. should be careful to report accurately the claims they disapprove. Below are seven examples of distorted criticism from Thornton et al.:

1. The authors criticize the representation of crucible smelting in the synthetic theory. They write, "crucible smelting would result in a very low yield of copper metal per 'charge' (i.e., the combination of ores, fuels, fluxes, and anything else added together within a reaction vessel)."[23] This claim contradicts the absence of fuel in crucible charge assumed in the synthetic theory.[24]
2. Contradicting their previous claim, the authors assert that I suggested that "no charcoal was being added to the ore charge within the crucible because of its small size."[25] The synthetic theory actually claims the contrary: it is not the small size of a crucible that prevents the introduction of charcoal within, it is the external heating that limits the size of the crucible.[26]
3. They claim I argued "that crucible smelting was an inefficient process disliked by ancient metalworkers."[27] The original sentence actually states that "furnace smelting, as soon as it came into being, immediately replaced crucible smelting."[28]
4. The authors distort the synthetic theory when they claim, in its name, that "[t]he larger volume and improved facilities for slag production provided by furnaces allowed for much higher yields of

[15] Thornton et al. 2010, 310–11.
[16] Thornton et al. 2010, 310.
[17] Thornton et al. 2010, 305 n. 3.
[18] Thornton et al. 2010, 307–8. The lack of deliberate addition of fluxes in the Near East before the Middle Bronze Age is tenable, but only with difficulty, in light of the unexpectedly consistent chemical composition of slag in early southern Canaanite metallurgy (Merkel and Rothenberg 1999, 162).
[19] Amzallag 2009, 501.
[20] Thornton et al. 2010, 308.
[21] Thornton et al. 2010, 309.
[22] "We feel it important to provide a rebuttal to set the record straight for the wider archaeological audience" (Thornton et al. 2010, 305). Here, again, a confusion is introduced between circumstantial and essential errors.
[23] Thornton et al. 2010, 306.
[24] Amzallag 2009, fig. 1.
[25] Thornton et al. 2010, 306.
[26] Amzallag 2009, 501.
[27] Thornton et al. 2010, 306.
[28] Amzallag 2009, 500. The sentence is quoted by Thornton et al. 2010, 306 n. 11.

copper metal and the production of copper alloys (e.g., arsenical and antimonial copper) for the use of polymetallic sulfide ores."[29] According to the synthetic theory, however, alloys are also produced by crucible smelting, due to the use of sulfide ores as a reducing agent.[30]

5. The authors claim, in the name of the synthetic theory, that sulfide ores were used in furnaces only after roasting: "The presence of sulfides in crucibles does not necessarily suggest the intentional mixing of oxidic and sulfidic ores (i.e., 'cosmelting'), nor does the presence of sulfides in furnaces suggest prior roasting steps."[31] This is not true. In the synthetic theory, the use of sulfide ores in furnaces is assumed long before their roasting.[32] The rebuttal creates here an artificial conflict between the synthetic theory and the current state of our knowledge.
6. In criticizing the synthetic theory, they write that "the old idea that copper sulfides occurred only rarely in prehistoric crucible smelting is no longer tenable, based on evidence from a number of regions in the Old World."[33] But according to the synthetic theory, sulfides are essential for cosmelting copper in a crucible. Here again, they promote an artificial conflict between the synthetic theory and current knowledge.
7. The authors of the rebuttal write, "[Amzallag] argues that Levantine metalworkers who were skilled in furnace smelting then migrated throughout the Old World (from Ireland to Japan, Thailand to sub-Saharan Africa)."[34] This is a caricature, since the process of expansion of furnace metallurgy is infinitely more complex in the synthetic theory. It involves both centrifugal and centripetal modes of expansion, their complex interaction, and the isolation of whole metallurgical domains from the Near East.[35]

THE SCIENTIFIC DISCUSSION

The rebuttal challenges three fundamental principles of the synthetic theory: (1) the external heating of crucible, (2) the incompatibility between crucible and furnace smelting, and (3) the central importance of Canaan in the emergence of furnace metallurgy. These three criticisms are considered in this section.

External Heating of the Crucible

According to the synthetic theory, crucible and furnace smelting differ in their mode of heating. While charcoal is mixed with ore in a furnace, it is not included in the charge of a crucible. An external mode of crucible heating is suggested by three considerations. First, crucibles are always small, a characteristic that fits the thermal constraints of external heating. Second, metal purification is performed in crucibles. This remelting process cannot occur when living charcoal is introduced in the crucible; acting as an oxygen trap, it would prevent oxidation of the impurities. And third, in the case that the crucible is heated from within, large amounts of charcoal fragments and ash would be carried by liquid copper during casting. This situation is not observed either in molds or in artifacts.

Ignoring these arguments, Thornton et al. postulate that "all early metallurgical crucibles studied over the last half-century or so were found to have been fired from above or inside, using charcoal as an integral and substantial part of the charge."[36] However, the reliability of this statement should be questioned. Crucible fragments with both internal slags (rich in copper oxide and silica but devoid of wood ash) and external slags (caused by reaction between wood ash and clay) have been reported for a long time.[37] The finding at Los Millares (Spain) of a circular dip (diam. 1 m) with evidence of intense heat points to crucible heating from the outside.[38] The practice of externally heating crucibles is also attested (for copper melting) by paint from third-millennium B.C.E. tombs in Egypt and archaeological findings from Mesopotamia (figs. 1, 2).

Thornton et al. argue that externally heating the crucible is a more recent, Iron Age technique.[39] They also assume (without providing any reference) that at the earliest phases of metallurgy, the walls of an externally heated crucible would have collapsed long before the metal was reduced.[40] The authors conclude that, in early antiquity, crucibles had necessarily to be heated from the inside. This argument is surprising, given the archaeological evidence. Generally, it is observations that point to flaws in theories, not the opposite.

This view of the crucible is stated both in the rebuttal[41] and in a paper recently published by Thornton and Rehren in which they argue that "[a] major reason

[29] Thornton et al. 2010, 307.
[30] Amzallag 2009, 501.
[31] Thornton et al. 2010, 308.
[32] Amzallag 2009, 503.
[33] Thornton et al. 2010, 308.
[34] Thornton et al. 2010, 309.
[35] Amzallag 2009, 510–12.
[36] Thornton et al. 2010, 306.
[37] Tylecote 1976, 19.
[38] Craddock 1995, 133–34.
[39] Thornton et al. 2010, 306.
[40] Thornton et al. 2010, 308.
[41] "[A]lmost all early crucibles were made from poor clays that could not maintain their material and structural integrity

Fig. 1. Evidence for crucible external heating in the third millennium B.C.E., detail from mastaba of the Mereruka tomb at Saqqara, Sixth Dynasty (ca. 2400 B.C.E.) (drawing by P. Jean-Baptiste).

for the use of internally-heated crucibles during the earliest phases of metallurgy was the lack of suitable refractory ceramics."[42] This claim should be reconsidered, since faience (thermally resistant ceramic) is encountered in Egypt, Canaan, Mesopotamia, and the Indus Valley as early as the fifth millennium B.C.E.[43] It was produced by transformation of steatite into enstatite at a temperature of at least 1,100ºC. In their paper regarding faience in the fifth millennium, Bar-Yosef Mayer et al. even suggested that "[s]uch use of pyrotechnology may in fact be part of an 'experimental package' associated with emergence of metal production."[44]

Strikingly, the work of Bar-Yosef Mayer et al. is also mentioned in a recent paper by Thornton and Rehren: "Such mineral processing, producing a paste of crushed steatite with a clay binder and water, is known from Chalcolithic beads from the Peqi'in cave in Israel and elsewhere, including Mesopotamia, India and Pakistan (Bar-Yosef Mayer et al. 2004 and literature therein)."[45] Thus, two of the coauthors of the rebuttal acknowledge that people knew how to produce heat-resistant crucibles from the fifth millennium B.C.E.

The rebuttal also refers to the discovery at Tepe Hissar of a steatite-made crucible dated to 3600 B.C.E. and mentioned in the article by Thornton and Rehren.[46] In the rebuttal, this crucible is considered to

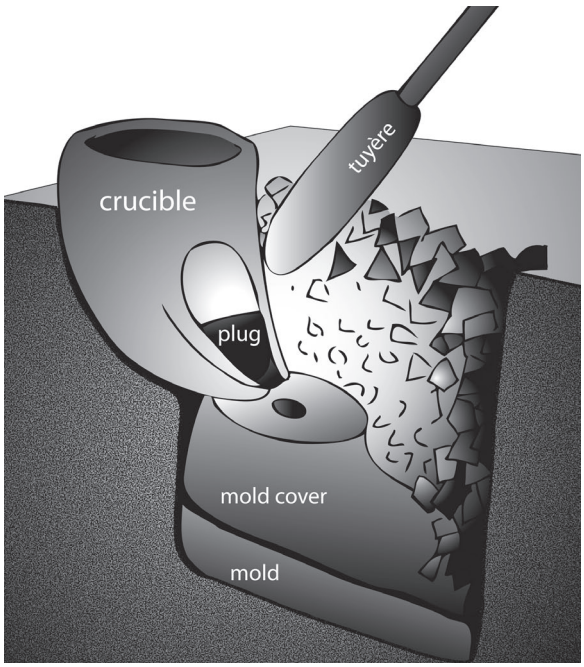

Fig. 2. The casting process at mid third-millennium B.C.E. Mesopotamia. The similarity in crucible shape in Egypt and Mesopotamia confirms a communal sharing of metallurgical knowledge (drawing by P. Jean-Baptiste; adapted from Davey 1988).

in the presence of high temperatures and reactive chemicals" (Thornton et al. 2010, 308).

[42] Thornton and Rehren 2009, 2701.

[43] See Bar-Yosef Mayer et al. 2004 (with references).

[44] Bar-Yosef Mayer et al. 2004, 499.

[45] Thornton and Rehren 2009, 2711.

[46] Thornton and Rehren 2009.

be the first externally heated metallurgical crucible identified in a prehistoric context.[47] Thornton and Rehren, however, argued in their earlier article that "[t]his crucible was not a mistake or an experimental product—it was a well thought-out and highly specialized tool for a specific purpose. As such, there must have been a tradition leading to its production and use, and other examples should be sought in this region."[48] Therefore, at least two of the coauthors of the rebuttal acknowledge that externally heated crucibles existed and imply that they were used during the fourth millennium B.C.E. They also agree these crucibles were produced by a technique widely known during the fifth millennium B.C.E. This is enough to reject the first criticism.

Incompatibility Between Furnace and Crucible Smelting

According to the synthetic theory, furnace and crucible smelting processes differ markedly. The mixing of copper ore and charcoal (i.e., furnace smelting) allowed the production of large reactors for smelting. It also enabled the addition of fluxes improving the yield of the smelting process and the use of a wide range of copper ores (even of relatively low grade). These advantages of furnace smelting over crucible smelting engender an incompatibility between the two processes, so that the crucible was replaced by furnace smelting as a simple consequence of the extent of its domain. This dynamic, though supported by much historical evidence,[49] is contested by the coauthors of the rebuttal. They argue that (1) a gradual transition exists between crucible smelting and furnace smelting, suggesting a spontaneous evolution of smelting techniques; and (2) crucible and furnace smelting processes coexisted at the same sites, even in the southern Levant.

This latter point is especially intriguing. Why would metallurgists continue to smelt copper in crucibles when furnaces were already in use? The most compelling answer is to assume that crucible and furnace smelting do not essentially differ, so that the size of the reactor fits each specific need. In the rebuttal, this explanation is compatible with the claim that "the strict dichotomy between crucible and furnace smelting is entirely unsupported by archaeological research."[50] Later, however, the same authors conclude that they "have shown that crucible and furnace smelting methods are not in fact as different as the author suggests."[51] The six authors never mention what is, to their mind, the difference between furnace and crucible smelting. However, at the beginning of the rebuttal, they seem to agree with the existence of functional differences noted for the first time by the synthetic theory when they state, "the author is correct in saying that crucible and furnace technologies have often been conflated in the literature."[52] In the absence of any satisfactory response concerning the common use of crucible and furnace for smelting, I examine the two above-mentioned objections evoked in the rebuttal.

Gradual Transition Between Crucible Smelting and Furnace Smelting. It is mentioned in the rebuttal that "the local transition from crucible smelting to furnace smelting has been well documented,"[53] and three sources are given:

1. An article by Golden, Levy, and Hauptmann, in which the coexistence of crucibles and furnaces at the Chalcolithic site of Shiqmim (southern Canaan) is reported, but the article makes no attempt to reconstruct any temporal sequence of events and techniques.[54] Should the reader of this paper guess the claims unexpressed and the data not provided by Golden?
2. An archaeological report by Helwing; if this report contains the first evidence of a gradual transition between crucible smelting and furnace smelting, Helwing is asked to publish these results as soon as possible in an easily accessible journal, as they may provide the "missing link" Thornton et al. are looking for.[55]
3. An article by Shimada and Wagner, which is devoted to pre-Columbian metallurgy. This should not be used to rebut an argument for the origin of furnace metallurgy in the Old World.[56]

Thus, I contend that the articles cited by the authors in support of their rebuttal do not provide relevant evidence for a spontaneous transition between crucible and furnace smelting.

Coexistence of Furnace Smelting and Crucible Smelting at the Same Site. The coauthors of the rebuttal refer to the existence of "metallurgical installations that are transitional from crucible to furnace, such as those presented from the Levant or the Aegean—both areas cited by Amzallag but misrepresented or misunder-

[47] Thornton et al. 2010, 307.
[48] Thornton and Rehren 2009, 2710.
[49] Amzallag 2009, figs. 2–5.
[50] Thornton et al. 2010, 309.
[51] Thornton et al. 2010, 311.
[52] Thornton et al. 2010, 306.
[53] Thornton et al. 2010, 309.
[54] Golden et al. 2001.
[55] Helwing 2005.
[56] Shimada and Wagner 2007.

stood by him."[57] Let us consider those sources. They mention the "smelting crucibles from Abu Matar"[58] and cite the work of Shugar, who analyzes refractories from the Chalcolithic site of Abu Matar (Beer Sheba).[59] The bimodal distribution of size, shape, and thickness of the slagged ceramics there clearly reveals the occurrence of two distinct reactors used for copper production: crucibles and furnaces. At first sight, this finding confirms the coexistence of furnace smelting and crucible smelting at the same site. A comparative analysis of the chemical composition of the slags from crucibles and furnace fragments, however, challenges this preliminary interpretation (table 1).[60] Shugar concluded, therefore, that "[t]he results of these studies provided a new understanding of Chalcolithic metallurgy in the Southern Levant. In addition to the known metallurgical processes that occurred underground at Abu Matar, it was established that pyrotechnological processes occurred above ground in a two refractory, two stage process in which a proper shaft furnace was used for smelting and crucibles were used for remelting selected copper prills."[61] Since Thornton et al. mention the work of Shugar at Abu Matar as a case of crucible smelting, it seems that they, too, are satisfied with the preliminary interpretation. They specifically quote two pages of Shugar's work, and not its final conclusions. But even here, Shugar steps away from the preliminary misleading conclusion and writes that "[t]hese findings indicate that in the current assemblage, crucibles were used for remelting of copper for casting, but not for smelting."[62]

What should be concluded here? I assume that the authors disagree with the conclusions made by Shugar and that they only mention his findings to dress them with their own interpretation. If this is the case, they cannot claim in their rebuttal that I have "misrepresented or misunderstood" the data about crucible smelting in the Levant.[63]

There is another problem. Rehren, one of the coauthors of the rebuttal, is thanked by Shugar in his work.[64] If he failed to convince Shugar to modify his interpretation, why did Rehren not publish his own view on such an important problem before mentioning the smelting crucibles from Abu Matar in the rebuttal he coauthored?[65] And why is this work of Shugar mentioned 13 times in a recent paper by Rowan and Golden[66] without any criticism regarding its content and conclusions?

Shugar's argument has introduced two fundamental assumptions in archaeometallurgy: (1) the finding of slags in crucible fragments is not enough to draw a conclusion about crucible smelting, and (2) a two-step process of copper production (furnace smelting followed by crucible remelting) exists from the Chalcolithic period. Accordingly, the coauthors of the rebuttal are invited to argue their interpretation concerning Shiqmim[67] and Tepe Hissar[68] by discussing the composition of crucible and furnace slags beyond just their occurrence. In the meantime, the synthetic theory continues to account for this reality.

The Central Importance of Canaanite Metallurgy

In the synthetic theory, furnace metallurgy spontaneously emerges in southern Canaan in the fifth millennium B.C.E. and then expands everywhere from this area. To challenge this premise, the authors of the rebuttal write that "Amzallag grounds much of his argument on controversial data from this region [the Levant]."[69] But the existence of controversial data does not prevent them from adopting an oracular, imperious, almost decalogous style when they mention ancient Canaanite metallurgy.

Thornton et al. assure us that, in southern Canaan, copper metallurgy first appeared only in the fourth millennium: "These installations or 'protofurnaces' likely date no earlier than 3800 B.C.E., and there is no

[57] Thornton et al. 2010, 309. The references cited are Golden et al. (2001) for the Levant and Pryce et al. (2007) for the Aegean. The latter deals exclusively with furnace smelting, so it does not provide any evidence against the synthetic theory.

[58] Thornton et al. 2010, 306.

[59] Shugar 2000.

[60] The data analysis and conclusions in table 1 are from Shugar 2000, 184–209. Conclusions about the lack of charcoal within the crucible are mine.

[61] Shugar 2000, 253.

[62] Shugar 2000, 100.

[63] Thornton et al. 2010, 309.

[64] "I must also thank Professor Thilo Rehren for his willingness to find the time to discuss many issues related to my work, and also for his advice, assistance and editing during this project" (Shugar 2000, 26).

[65] Thornton et al. 2010, 306.

[66] Rowan and Golden 2009.

[67] Golden et al. 2001. Shiqmim is a site contemporary with Abu Matar, ca. 20 km from Beer Sheba. It would be unusual to find there something very different from the metallurgy performed at Abu Matar. Curiously, Golden seems to foster, at least partly, Shugar's interpretation of the two-stage metallurgical process concerning the findings from Shiqmim: "During this incipient stage of metallurgical technology [furnace smelting], the smelting product would be composed of a mass of slag, partly decomposed ore, and small copper prills. The manually extracted copper prills might then be re-melted in a crucible for several reasons" (Golden et al. 2001, 960). This is another example of the contradictory views of Golden.

[68] Thornton et al. 2010, 309.

[69] Thornton et al. 2010, 305.

Table 1. Comparative Analysis of Slags from Crucibles and Furnaces from the Chalcolithic Site of Abu Matar (Beer Sheba) (after Shugar 2000).[a]

Component in Slags	Condition of Formation	Occurrence in Furnace Slags	Occurrence in Crucible Slags	Comments	Crucible Process
Fayalite (Fe_2 SiO_4) and pyroxene (Fe, Mn, Mg) $CaSi_2O_6$	coreaction with CaO from ceramic body and fuel ash	abundant	relatively rare	angular inclusions of fayalite in CS,[b] suggesting origin from FS[c]	remelting, no charcoal (source of ash) inside
Magnetite (Fe_3O4)	reaction of iron silicate (included in ore or added as fluxes) during smelting process	abundant, associated with fayalite	rare	–	no smelting
Cuprite (Cu_2O)	partially oxidizing conditions	rare, in the FS surface	typically found	in FS, cuprite appears after charcoal is totally consumed	remelting, no charcoal (oxygen trap) inside
Metallo-sulfide complexes (matte)	use of sulfide-rich copper ore	matte, sulfide droplets	matte only	–	remelting
Copper ore fragments	smelting process	partially reacted ore	very rare	ore found at outer edge of CS is incidentally present	no smelting
Iron content in copper prills	Fe solubilized in copper during smelting process	ca. 0.97% Fe	ca. 0.33% Fe	Fe content reduced by oxidizing conditions	remelting, no charcoal (oxygen trap) inside

[a] With author's own conclusions about the lack of charcoal within the crucible
[b] CS = crucible slags
[c] FS = furnace slags

evidence for more developed furnaces in this region for centuries thereafter."[70] Since furnaces are said to exist in the northern Euphrates at the end of the fifth millennium, this claim inevitably leads to the conclusion that furnace metallurgy cannot originate from southern Canaan.

To strengthen this point, the coauthors characterize early Canaanite metallurgy as being very primitive and conservative. In their opinion, the southern Canaan metallurgical installations "betray a rather primitive technology (relative to other contemporaneous regions of the Near East such as Anatolia, the Caucasus, and Iran) that in no way represents the 'invention' of the furnace."[71] They also introduce a time lag of four centuries between the first appearance of copper artifacts in Canaan and the first evidences of smelting: "it has been well established that there is no copper in southern Levantine sites prior to ca. 4200 B.C.E."[72]

A picture arises from pooling these tenets: Canaanite metallurgy was introduced early in the fourth millennium (northern Euphrates, Iran, and Anatolia being potential sources) after four centuries of exogenous supply of copper artifacts and copper ingots. This representation of the early southern Canaanite metallurgy is, however, unfounded. In comparison with the other ancient homelands, the early southern Canaanite metallurgy is neither primitive nor underdeveloped and lately emerging for the following reasons:

[70] Thornton et al. 2010, 310.
[71] Thornton et al. 2010, 310.
[72] Thornton et al. 2010, 310.

1. Primitiveness. The southern Canaanite Chalcolithic copper industry can hardly be considered primitive. The Nahal Mishmar hoard, discovered near the Dead Sea,[73] is a collection of more than 400 outstanding artifacts dated from the early fourth millennium B.C.E. (fig. 3).[74] They were produced by the highly complex technique of lost-wax casting of copper alloys. A similar level of achievement of copper artifacts is not found elsewhere at this time.[75]
2. Rate of development. The southern Canaanite metallurgy is not characterized by a low rate of development. Rather, it rapidly developed toward large-scale production during the fourth millennium B.C.E. A protoindustrial "metallurgical chain" including multiple phases of remelting and purification has been identified at Feynan (Punon) during the Early Bronze Age. According to Levy et al., it was "the largest Early Bronze Age (c.3600–2000 BC) metal manufactory in the ancient Near East."[76] This large-scale smelting was not devoted to local use only. It was exported far away,[77] and thus confirms the centrality of the early Canaanite metallurgy in the emergence of an extended network of trade.[78]
3. Ancientness. In southern Canaan, the smelting of copper does not start ca. 3800 B.C.E. but long before. The radiocarbon analysis of seven samples from furnace basins discovered at the Chalcolithic site of Abu Matar (Beer Sheba) provided calibrated dates between 4306 and 4028 B.C.E.[79] In southern Canaan, the radiocarbon dates of samples from the earliest stratigraphic contexts associated with metal artifacts indicates their occurrence from the 45th century B.C.E.[80] Accordingly, the radiocarbon date of Timna site 39 (domestic site and ancient furnace of archaic shape) at 4460 B.C.E.[81] fits the context of emergence of the earliest furnace metallurgy in southern Canaan.[82] Thornton et al. write, "New and more empirical evidence is needed before the proposed early date of the Timna furnaces will be accepted by most archaeologists."[83] The authors cite Craddock against the date of the early furnaces from Timna.[84] However, they fail to mention Rothenberg's[85] reply and the distortions he identifies in Craddock's paper. One thing is certain: denying the existence of fifth-millennium southern Canaanite metallurgy engenders an infinite series of artificial controversies.

Fig. 3. Chalcolithic metallurgy in southern Canaan: artifacts from the Nahal Mishmar hoard. The differences in color among artifacts are due to the copper alloying with arsenic, antimony, silver, and nickel at various ratios (Collection of the Israel Antiquities Authority; © The Israel Museum, Jerusalem).

Labeling some data "controversial" is an excellent expedient for undermining the premises of the synthetic

[73] Bar-Adon 1980.

[74] On the basis of new AMS dates, Aardsma (2001) even argues that portions of the mat on which the hoard was placed may date to as early as ca. 4350 B.C.E. This information is reported in Rowan and Golden 2009, 45. After assuming the exogenous origin of the Nahal Mishmar hoard for a long time, these artifacts are now definitely considered as being locally produced (Goren 2008). From the highly complex technology it requires, Goren concludes that "the chalcolithic technology of mold construction for the lost wax casting technique was well established and performed by specialists" (Goren 2008, 390).

[75] This stimulated Moorey (1988, 187) to ask, "how and why did such a sophisticated copper industry emerge in the villages of the northern Negev of Israel in the earlier fourth millennium BC?"

[76] Levy et al. 2002, 425. Curiously, this paper is mentioned in Thornton et al. (2010, 310 n. 61) as a confirmation of the primitiveness of the southern Canaanite metallurgy.

[77] Gophna and Milevski 2003.

[78] Muhly 1995.

[79] Shugar 2000, 58, 71.

[80] Shugar and Gohm 2006, fig. 1.

[81] Rothenberg 1990; Rothenberg and Merkel 1998.

[82] See references in Amzallag 2009.

[83] Thornton et al. 2010, 310.

[84] Craddock 2001.

[85] Rothenberg 2002.

theory in the absence of more cogent arguments. This strategy should be used in moderation, however, as otherwise a self-conflicting situation rapidly emerges. In 2009, Golden published, together with Rowan, a work entitled "The Chalcolithic Period of the Southern Levant: A Synthetic Review."[86] In this paper, he begins the section on "metallurgy" by saying that "[t]hough copper first appears during the Neolithic elsewhere in the ancient Near East (Iran and Anatolia), by the Chalcolithic the metallurgical techniques of the southern Levant are on a par with, if not surpassing, those of other contemporary peoples."[87] Southern Canaanite metallurgy is not deemed "primitive" in Golden's work. Moreover, it has an earlier origin than stated in the rebuttal. Reporting the multiple archaeometallurgical findings unearthed in area A of Abu Matar, Rowan and Golden conclude, "Excavation of Area A also produced an in situ furnace, yielding charcoal radiocarbon dated to c. 4200–4000 BC."[88]

Where does Golden speak the truth: in his recent work[89] or in the current rebuttal?[90] To clarify this point, it may be useful to refer to a recent article by Thornton.[91] Here again, early Canaanite metallurgy is not considered primitive, for he writes that "[d]espite the advanced nature of Iranian metallurgy relative to the Levant, the lost-wax cast, polymetallic alloys [from the hoard of Nahal Mishmar] have no parallel either stylistically or technologically in Iran; thus, their origin must lie elsewhere."[92] Here he cites Goren, but Goren's work has shown (through chemical analysis of the residual clay from the internal mold) that the Nahal Mishmar artifacts were locally produced. So the use of the vague expression "elsewhere" cannot justify downplaying early Canaanite metallurgy. When considered together, all these inconsistencies suggest that the representation of the earliest Canaanite metallurgy made in the rebuttal is misleading. There is no need, therefore, to refute it further.

THE HIDDEN REALITY

This reply has revealed how the coauthors of the rebuttal have attempted to drown the synthetic theory in a flood of irrelevant arguments and how they contradict their own opinions in several independent instances (as published in recent papers) to undermine its premises. What may drive such an unscientific attitude?

The authors of the rebuttal encourage readers "who wish to know more about the origins of metallurgical technologies in various regions . . . to read the new syntheses presented in recent editions of the *Journal of World Prehistory* and *Antiquity*."[93] The papers from the *Journal of World Prehistory* were first presented in a workshop organized by Roberts and Thornton, in which the other coauthors of the rebuttal were invited to participate.[94] The goal of the workshop was apparently to reach "a more holistic understanding of interactions between metals and societies."[95] The rebuttal, then, recommends the reading of a series of papers written by its authors, published at about the same time as my article in the *AJA*, and discussing the same topic.

The article mentioned in *Antiquity*, by Roberts, Thornton, and Pigott, espouses a new theory about the origin of metallurgy.[96] This means that two distinct theories about the origin of metallurgy appeared simultaneously at the end of 2009, after 50 years of localizationism. This coincidence presents us with the opportunity to compare the value of each theory with regard to the current data and the open questions. This, however, has not occurred. Instead of a fruitful confrontation, Roberts, Thornton, and Pigott choose to denigrate the scientific legacy of the synthetic theory by defending localizationism. But is it not contradictory to defend localizationism against the synthetic theory and at the same time propound another theory that challenges localizationism, the null hypothesis of Childe diffusionism? Juxtaposing the *Antiquity* article with the rebuttal they coauthored, however, helps to clarify their position.

Origin of Metallurgy

Roberts, Thornton, and Pigott assume that "[t]here is no evidence to suggest that metallurgy was independently invented in any part of Eurasia beyond Southwest Asia."[97] They suggest that metallurgy emerged in

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[86] Rowan and Golden 2009.

[87] Rowan and Golden 2009, 41.

[88] The work mentioned here by Rowan and Golden is again Shugar 2000.

[89] Rowan and Golden 2009.

[90] Thornton et al. 2010.

[91] Thornton 2009a. Thornton cannot be accused of "canaanophily." His paper (Thornton 2009a) reveals, even from its title, that a part of his doctoral dissertation (Thornton 2009b) was devoted to freeing his mind from the Levantine paradigm, a conceptual chimera imagined for the circumstance. This is expressly mentioned in the rebuttal where the Levantine paradigm is labeled a "conceptual chimera—with no explicit exemplars in the published literature" (Thornton et al. 2010, 305).

[92] Thornton 2009a, 319 (citing Goren 2008).

[93] Thornton et al. 2010, 310.

[94] At the 73rd meeting of the Society for American Archaeology in Vancouver, Canada, 26–30 March 2008. This event is also mentioned at the beginning of Thornton et al. 2010.

[95] Thornton and Roberts 2009, 182.

[96] Roberts et al. 2009.

[97] Roberts et al. 2009, 1019.

one single area and then expanded everywhere in the Old World. This diffusion of knowledge is explicitly formulated in their theory: "The movement of communities possessing metallurgical expertise to new ore sources and into supportive societies led to the gradual transmission of metallurgy across the Eurasian landmass."[98]

This view obviously contradicts the five decades of archaeometallurgical data supporting localizationism that they intended to defend in the rebuttal. It also invalidates their (nonrelevant) criticism against the synthetic theory as being a remake of Childe's diffusionism: "Although Childe is rightly honored for his pioneering investigations of the Bronze Age social and political changes, his hypotheses about the hyperdiffusion of metallurgy, and its supposedly transformative effects on ancient societies, have long been discarded."[99]

Surprisingly, the diffusionism of Childe is not discarded by Roberts, Thornton, and Pigott but rather improved: "The transmission of this metallurgical expertise did not simply involve the intrepid wanderings and migrations of independent metal smiths as influentially envisaged by the great prehistorian V Gordon Childe, but it did involve the movement of metalworkers, perhaps in broader social groups, who were able to access the necessary resources."[100] Their theory appears, therefore, as a "communitarian" version of Childe's theory[101] pointing to a single origin of metallurgy in southwest Asia. It should therefore be considered neo-diffusionism. The actualization of Childe's theory, however, is not a simple affair. To succeed, the authors have to deny the spontaneous emergence of crucible smelting as documented by 50 years of localizationism and related by the synthetic theory. The autonomous development of crucible smelting in Spain, for instance, is challenged by Roberts, Thornton, and Pigott when they write, "The evidence for independently-invented metallurgy in southern Iberia is fragmentary and the dating unreliable (see Roberts 2008)."[102] One can be, however, unsatisfied by a refutation founded on the self-generated opinion of one of the authors of a theory challenging localizationism. Even more, many other cases of spontaneous emergence of crucible smelting remain to be refuted,[103] and that list may even grow in the future.

The Linear Narrative

The authors of the rebuttal allege that the synthetic theory lacks a baseline epistemology, reducing it to the status of narrative.[104] Though this accusation remains enigmatic in the rebuttal, it requires a clear-cut distinction between theories that should be discussed scientifically and others that should be discarded immediately. Here again, there is a conflict between the rebuttal and the neo-diffusionism of Roberts, Thornton, and Pigott. According to this theory, diversification of the metallurgical techniques and achievements results from an active interaction between the migrating metallurgists and the host population: "The consequence is a process, not only of metal adoption, but also of metal innovation, as metal objects and production techniques were shaped to reflect specific community standards and desires."[105] This assertion is problematic because the authors never explain how to identify the "standards and desires" except by the metal artifacts produced by the migrant copperworkers. In such a situation, this assertion becomes a circular argument, introducing a tautology for a central issue of their theory. It may explain everything, but at the same time, it says nothing that was not already known.[106] For this reason, the neo-diffusionism of Roberts, Thornton, and Pigott should be considered a narrative rather than a scientific theory.

CONCLUSION

Because of its epistemological flaw and its inability to integrate the current data about local emergence of crucible smelting, neo-diffusionism has low scientific value. Nevertheless, after 50 years of localizationism, it becomes attractive again because of its intent to integrate a large amount of disconnected data in a common framework.[107] Its legacy, however, is challenged by the existence of scientific alternatives.

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[98] Roberts et al. 2009, 1019.
[99] Thornton et al. 2010, 312.
[100] Roberts et al. 2009, 1018.
[101] Childe 1930.
[102] Roberts et al. 2009, 1015; see also Roberts 2008.
[103] Amzallag 2009, table 1.
[104] Thornton et al. 2010, 305.
[105] Roberts et al. 2009, 1018.
[106] As another example of an epistemological flaw, Roberts et al. (2009, 1018) also argue that "there are sufficient differences in the necessary thermal and atmospheric conditions required to suggest that being proficient in metal production would require verbal instruction and visual demonstrations under experienced individuals or groups for a successful transfer of knowledge." With such a trivial claim, the neo-diffusionist theory should not be concerned about criticism. But here again, this explanation has no scientific value.
[107] In the introduction, Thornton and Roberts (2009, 181–82) express clearly this need they intend to satisfy: "invitations were sent to fifteen scholars of early metallurgy who had previously demonstrated an ability to combine archaeometrical analysis, archaeological fieldwork and anthropological interpretation to reach a more holistic understanding of interactions between metals and societies."

The situation clarifies. Three coauthors of the rebuttal published a neo-diffusionist theory at the same time that the synthetic theory appeared in the *AJA*. To refute it, they recruited three other authors to elaborate together a contradictory rebuttal. Its objective was, as they said, to debunk the synthetic theory, its scientific legacy, its fundamental principles, its predictions, and even the questions it asks.[108]

To achieve this goal, the coauthors distorted the content of the synthetic theory, the work of other authors, and even their own opinion. This suggests that the six authors of the rebuttal understand that the synthetic theory, by its simple existence, challenges not only 50 years of localizationism but also their just-born neo-diffusionism.

Their stated desire to "set the record straight,"[109] however, is disturbing in light of the methods they use to discredit undesirable theories and in their attempt to prevent future publication in scientific journals supporting them. The rebuttal of Thornton et al. has little scientific value, but it does seem to announce the coming of a new "dark age" in archaeology.

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[108] "In this rebuttal, we have focused on debunking Amzallag's inaccurate comparison of crucible and furnace smelting and his arguments for the development of these technologies in different parts of the Old World" (Thornton et al. 2010, 311).

[109] Thornton et al. 2010, 305.

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