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Evidence for the sources and use of tin during the Bronze Age of the Near East: a reply to J E Dayton

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Evidence for the sources and use of tin during the Bronze Age of the Near East: a reply to J. E. Dayton[1]

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We have joined in writing this critique because we feel that the early history of metallurgy is a problem in technology as well as one related to archaeology and philology. One author of this critique for some six years has been searching out possible geological sources of tin in the Middle East as an aspect of the origins of all metallurgy, using experienced metallurgists and geologists in the undertaking. The other author has surveyed the written texts that illuminate that portion of the Bronze Age from Ur III and Kültepe on. We do not believe Dayton should go uncontested, however refreshing some of his hypotheses. There are a number of points with which we wish to take issue and these can best be treated on an individual basis.

Turkey as a source of minerals in antiquity

Dayton maintains that 'Turkey is not so very rich in minerals by ancient standards' (53); indeed it 'is not particularly rich in copper ores' (54). For this reason, he concludes that the Mesopotamians looked beyond Anatolia to the Balkans and to the Danube for their supplies of metals. And he posits a series of Bohemian copper and tin supply routes to the Aegean (from Troy I on) which were the avenues by which bronze came to be known. These were periodically cut.

We shall not try to document the profusion of mineral ores that are attested to by studies of the *Maden Tetkik ve Arama Enstitüsü* (MTA), the mineral exploration agency of Turkey and reflected some years ago in the compilation of C. W. Ryan's *Guide to the Known Minerals of Turkey* (ICA–MTA, 1957, 1960). Ergani alone, today, produces 17,000 tons of copper per year. There is also substantial production at Küre and Murgol. The record of production of a blister or matte copper at Ergani is carried back to the Kültepe period and earlier (Birgi 1950: 339). But billets of the first industrial production of copper have also been found at Alaça Hüyük, Kültepe, Acemhüyük, and again testify to a most complex metallurgical technology by 2000 B.C., involving the use of matte smelting of sulphide ores.

The fact is that Dayton ignores both known geological facts as well as the now demonstrated interconnections of copper, arsenic, lead, and iron in the evolution of casting and

[1] This joint paper was written in response to a recent article by John E. Dayton, The problem of tin in the Ancient World. *World Archaeology* 3 (1) 1971:49–70. All references given simply in parentheses are to the pages of Dayton's article.

smelting. He speaks of arsenic coppers (50), of pigments and glazes (66 f.), without ever demonstrating the interconnections that are intrinsically involved in all pyrotechnology and especially that having to do with metals. Because of his preoccupation with a novel theory of the origins of bronze metallurgy – whose historical occurrence or lack thereof he documents well (54–8) – he falls into fallacious assertions about the occurrence of copper and ignores the juxtaposition of ores and metals now believed necessary for the birth of an understanding of smelting, impurities, and alloys. This tendency reaches its climax on page 66, on which he, in three subsequent paragraphs:

1 implies that *annaku* may have been a copper ore
2 associates it, by implication, with Anarak in Iran,
3 and casually dismisses, *vis-à-vis annaku*, the ores of Ergani in Turkey as being 'not very rich in copper . . . difficult to smelt and not at all rich by the standards of the ancients. Of course, the richer upper oxide zones could have all been worked out in ancient times.'

These casual associations and assertions are the very heart of Dayton's problem. After long and frequent visits to copper, lead, and other mineral sources in south-western Asia, in company with leading American and local geologists, we can say categorically that Dayton is wrong about Turkey. Not only possessing two copper rich zones (the Black Sea Mountains and the Taurus), it boasts several hundred polymetallic deposits (copper, lead, zinc, iron) that we believe were essential to the identification of minerals and fluxes in smelting. In other articles due shortly for publication, we shall argue that Turkey, as a rich metallurgical crossroad, is a most probable home of both the bronze and iron ages – starting with its own abundant copper ores of all types. This argument takes cognizance of the known fact that the highly organized trade in obsidian in the late neolithic was centred in Anatolia and gave rise to counter currents of all types, involving exchanges of bitumen, and so on (Gary Wright 1969; Hole, Flannery and Neely 1969).

Technological history is *not* casual. There *is* a reason why the oldest archaeological finds of native copper are in Anatolia or on its borders: Çanönü Tepesi, Shanidar, Çatal Hüyük, Can Hasan; or why the first industrial age of copper appeared in Turkey (at Kültepe) and was followed by the age of iron. Both evidence and logic rule out the loose associative arguments of Dayton and force one to return in the search for the earliest tin to the granites of the Black Sea mountains and of the upper Zagros.

Troy and Ur: the date of the introduction of tin bronze and the use of Bohemian tin

Central to Dayton's thesis is the belief that, with the exception of the Royal Cemetery at Ur, the use of tin bronze did not develop in the Near East before the end of the third millennium B.C. Therefore Troy II is described as a Copper Age culture (62, 63) as was the time of Sargon of Akkad (60, 64), though Dayton is willing to admit that there is some evidence for the use of tin bronze by the end of Troy II (59). Still, 'It is significant that during the Kültepe period, *c.* 2100–1950 B.C., tin bronzes first appear in the Levant' (62). This came about because, 'By this time the Bohemian branch of our "Finno-Ugrian-Hurrians" had discovered the secret of tin bronze in Bohemia' (69).

The date of the introduction of the use of tin bronze is still not clearly established. The main reason for this is the lack of analyses. There are still far too few analyses of copper and bronze objects from the Ancient Near East. (However, see now the important new evidence published by Moorey and Schweizer 1972.) Unfortunately Dayton has overlooked much of the evidence which does exist. His discussion of Troy II ignores the analyses published by Kurt Bittel (1959), showing that two separate alloys, one a copper-tin, the other a copper-arsenic alloy, were in use at the same time though never in the same object. This same combination of tin-alloy and arsenic-alloy is found in Central Anatolia, as shown by the analyses from Ahlatlibel, Alaça Hüyük, Büyük Güllücek, and Mahmutlar (Koşay and Akok 1966). The analyses from Horoztepe show that the same situation existed there (Özgüç 1964). The Early Bronze II period in the Cyclades shows that exactly the same two alloys were found at this time in the Aegean (Renfrew 1967; Renfrew 1972: 314; Bossert 1967).

Unfortunately, Dayton does not utilize any of these studies. His knowledge of the situation in Anatolia is apparently based upon the work of Ufuk Esin (1969), but the conclusions drawn from this work are open to question (see below). Dayton does list the recent collection of analyses by Hans Günter Buchholz (1967) in his bibliography, but the text of his article does not really utilize any of the material brought together by Buchholz. The evidence for the early use of tin bronze is much greater than Dayton apparently realizes, and it is steadily increasing: witness the recently found tin bronze from Tepe Yahya in southern Iran (Lamberg-Karlovsky 1971), and from Non Nok Tha in northern Thailand (Solheim 1972a, 1972b).

What about the question of Bohemian tin and the possibility of trade connections between Bohemia and Ur? Here things become rather involved. Dayton has much to say about migrations involving Kurgan peoples and Bell Beaker Folk but, in all cases, these movements involve people with only a copper metallurgy and they have nothing to do with uses or possible sources of tin (55 f., 61 f.). Dayton speaks of an elaborate trade network, running from Spain to Ur, which developed after the destruction of Troy I. This all has a background of copper metallurgy, as all of these regions are, in the Troy II period, copper-using cultures. Yet Dayton can also say that what links the entire area, from Spain to Ur, is 'The single thread of the presence of tin' (69).

What seems to be the cause of the trouble is the presence of tin bronze in the Royal Cemetery of Ur, a presence confirmed by the analyses recently published by Moorey and Schweizer (1972) which show the same combination of tin bronze and copper-arsenic alloy both at Ur and at Kish. Dayton is prepared to go so far as to suggest that the material from Ur may actually date to the Kültepe period (65), which would certainly create a bit of a problem with respect to the history of Ur. Of one thing Dayton is certain: that the development and the use of tin bronze represents a movement which went from west to east and not vice versa (57 f.). Who was responsible for this spread in the use of Bohemian tin, from west to east? None other, it seems, than the *porteurs de torques* of C. F. A. Schaeffer. However, in the reconstruction of Mr Dayton, these torque bearers have become Hurrians, related to the Ugro-Finns of Hungary (62 f.).

Once again the argument is somewhat confused. Dayton suggests that the Sumerians buried in the Royal Cemetery may be related to these 'Finno-Ugrian-Hurrians', as the Royal Sumerians had ox carts and as both Hurrian and Sumerian may be Finno-Ugric

languages (64 f.). Now if this is to be accepted, then the *porteurs de torques* should have already made contact with the tin fields of Bohemia as the Royal Cemetery has bronze. Yet everybody outside of Ur only has copper, according to Dayton. Troy II (the EB 4 period according to Dayton) was a Copper Age and, furthermore, many of the torques themselves upon analysis turn out to have been made of copper (63).

A puzzling situation. How is it to be explained? Dayton assumes that there are different groups of torque bearers and that only by the beginning of the Kültepe period (*c.* 2100 B.C.) had the torque bearers, or 'Finno-Ugrian-Hurrians', definitely discovered the secret of Bohemian tin (69). Before then had come the Indo-European Kurgan invasions of *c.* 2300 B.C. which 'cut off the Mediterranean from the copper ores of the Balkans and Central Europe' (61). But how does this explain the presence of tin bronze in the Royal Cemetery at Ur? Leaving aside the question of tin for the moment, it must be recognized that Mr Dayton has made some rather extreme statements regarding the history of the ancient world in the third millennium B.C. Can they be justified? One must begin with the Sumerians and the Royal Cemetery of Ur.

There have been a number of discussions in recent years concerning the complex question of the dating of the material from the Royal Cemetery (Maxwell-Hyslop 1971: 1 f.). The material seems to be roughly contemporary with that from the later phases of Troy II and with the royal graves from Alaça Hüyük, giving a date around the middle of the third millennium B.C. (Bass 1970; Renfrew 1972: 121 f., 196 f.).

But what does this material have to do with Bohemia? Mr Dayton is much interested in ox carts, but he cannot decide whether they came from the west (64) or from the east (65). An examination of the recent study by Stuart Piggott (1968) would have been helpful here. Dayton feels that the urnfield at Osmankayasi must be Hurrian instead of Hittite because it consists of cremation burials with the bones of horses (63). This is truly extraordinary as both cremation and the horse have long been considered the very hallmarks of the Indo-Europeans, and both are specifically part of a Hittite royal burial as described in Hittite funerary texts (Otten 1958). The question of Sumerian origins is a vexed problem (cf. Jones 1969) as is that of the Hurrians, but we can be reasonably certain that illumination will not come from Bohemia.

In short, there is tin in Bohemia, but Mr Dayton has presented no evidence for its use in the Ancient Near East. The evidence, such as it is, still seems to represent a movement from east to west. Dayton has rejected the one body of evidence which tells us the most about ancient tin, namely the textual references to *annaku*. Seen in its proper light, this documentary evidence indicates a tin trade going from east to west, from Assur to Kültepe and from Susa to Mari by way of Eshnunna and Sippar (Leemans 1968; Dossin 1970; Malamat 1971). The archaeological evidence also indicates that tin bronze appears first in Mesopotamia, in the early third millennium B.C., and then later in Syria, again a movement from east to west (Morrey and Schweizer 1972: 194 f.).

The Hittites and the tin trade

To make the case against Anatolia as a possible home of the Bronze Age, Dayton not only argues against the presence of significant copper deposits in Turkey. He also tries to demonstrate from analyses that a true bronze age followed a 'rich copper period' in the

last centuries of the third millennium B.C., but was terminated in the later Hittite period when the 'Hittites cut the trade route to the Bohemian tin fields' (60). Central to his argument is the fact that analyses of artefacts at Alaça, Horoztepe, Mahmutlar, and Alişar show a fairly consistent early record of arsenic copper, later contain tin, then seem to weaken in tin content. He cites the comprehensive study of Ufuk Esin, showing tin to 'be present in many of the artefacts, *and equally absent in others*' (his italics) as if in proof of the late and uncertain shift from a copper to a bronze age.

Nowhere in our reading of Ufuk Esin (1969), an interesting and important compilation, can we find statistical support for the assertion that Horoztepe represents the Anatolian watershed between copper and bronze; and that Alişar represents a reduction in tin brought about by the cutting of the trade routes to the tin fields by the Hittites. The analytical data cannot be read that way, and Esin makes no such assertion.

What Esin's analyses do show – as do those of Selimkhanov, Buchholz, Burton-Brown, Bittel, Renfrew, Branigan, Moorey, and others – is a growing pattern of impurities, with arsenic gradually gaining ascendancy, accompanied by a complex of other impurities such as antimony and bismuth. Tin often shows a tentative presence among these impurities; and one gets the impression that arsenic at about 3–5% is a deliberate additive. Where tin approaches 8–10% in quantity, however, the other impurities drop off, bespeaking a clear knowledge that tin yielded the superior alloy and was available in quantities.

Little can be said at all about the availability of tin in this period. We can presume that it was scarce, but the arsenic was often a perfectly acceptable substitute, much more available; and that arsenic in any case preceded tin in common use (Tylecote 1962 and 1970). To read 'trade routes' into the statistical data about tin bronzes is nonsense, given the variety of metals that took part in the smelting and alloying operations and the fact that the great majority were found in tempting juxtaposition in the region of the Black and Caspian Seas. This is not to deny that trade routes existed for both metals and metal ores, as the Kültepe and Mari tablets show (Garelli 1963; Sasson 1966), or that trading in tin probably very rapidly followed the natural route of the Danube to Hungary and Bohemia.

Thus there is no evidence to suggest that the arrival of the Hittites had anything to do with any eclipse in the use of tin or that they severed any trade routes with Bohemia. Indeed, according to James Mellaart (1968), the Hittites did all they could to open and to maintain the trade routes bringing Bohemian tin into Anatolia. But neither Dayton nor Mellaart are able to present any convincing evidence for the use of Bohemian tin. Mellaart's arguments are based upon the assumed western Anatolian origin of a series of objects found in Europe which everyone else considers to be of Aegean origin or inspiration (Branigan 1970).

The translation of *annaku*

Dayton argues that Sumerian AN.NA, Akkadian *annaku* designate not tin or even lead, but something else, perhaps a copper ore (66) or a 'tin-rich ingot-torque' (66, 68). He further maintains (59) that 'The great controversy which has raged over the meaning of the word *annaku*, translated as either "lead" or "tin", appears to have been based on the existence of tin in the Caucasus, and on Frankfort's conviction that this area was the home of metallurgy.' As neither assumption can be substantiated, Dayton believes that the controversy should be abandoned since both translations now have no meaning.

As a number of recent studies have tried to point out, the translation of Sumerian AN.NA, Akkadian *annaku* (or *anāku*) is basically a problem in lexicography. The reasons for the translation 'tin' have been well stated by Benno Landsberger (1965), but it seems necessary to stress once again two main points.

(a) The various other metals which played an important role in the ancient world have all been securely identified in the Ancient Near Eastern sources as follows:

	Sumerian	Akkadian	Hittite	Egyptian
gold	KÙ.GI	*ḫurāṣu*	(KÙ.GI)[1]	*nbwj*
silver	KÙ.BABBAR	*kaspu*	*ḫarki-*(?)	*ḥḏ*
iron	AN.BAR	*parzillu*	*ḫapalki-*	*bi3*
lead	A.BÁR/GAR$_5$	*abāru*	*šuli(ya)*	*dḥtj*
copper	URUD(U)	*erû*	*kuwanna(n)-*	*ḥmtj*
bronze	UD.KA.BAR	*siparru*	*ḫarašu-*	*ḥsmn*
What remains is:				
tin	AN.NA	*annaku*	*dankui-*	*ḏḥw*

(b) The usage of the word *annaku* certainly indicates a meaning 'tin'. In numerous texts from the third through the first millennium B.C., *annaku* is combined with *erû* in order to produce *siparru*. In other words, tin plus copper results in bronze (for *erû* and *siparru* see Zaccagnini 1971). Also, the proportions of tin to copper mentioned in the texts, in general from 1:6 to 1:10, match the known ratios attested in ancient bronzes. Thus, as early as the pre-Sargonic texts from Lagash (mid-third millennium B.C.), we find a text (RTC 23) giving a proportion of $13\frac{1}{3}$ shekels of tin (AN.NA) and 80 shekels of copper (URUDU), this being a ratio of 1:6 (Hallo 1963: 139). A somewhat later text (UET III 429) gives a ratio of 1:7:

'5 shekels of tin (AN.NA)
$\frac{1}{2}$ mina, 5 shekels of pure copper (URUDU.LUḪ.ḪA)
for one sheep-killing knife.'

While a related text (UET III 368) gives a ratio of 1:8:

'45 shekels of tin (AN.NA)
6 minas of copper from Meluhha (URUDU.ME.LUḪ.ḪA) . . .'.

The ratio of 1:6 was still being used in the Neo-Assyrian period, as Sennacherib speaks of:

> 'four columns of bronze in which (copper) was mixed with tin in the ratio of (one part of tin to) six parts (of copper) . . .' (*CAD*, s.v. *balālu*, 41a; s.v. *dappu*, 106a).

It is difficult to see how these passages could be understood in any other way. *Annaku* is 'tin'.

Mr Dayton makes much of the assumption that tin, as a metal, was not known in the Near East before Phoenician times (58 f.). Therefore the tin bronzes from the Near East could only have been manufactured by adding a 'tin-rich ingot-torque' to copper or copper-scrap. It is such a 'tin-rich ingot-torque' which is designated in our texts as *annaku* (66, 68).

[1] The Hittite word for gold is unknown as the texts always employ a Sumerian ideogram.

The argument that tin was not known as a metal in the Near East because no objects of tin have survived is a very dangerous one. If Assyriologists were asked to confine their translations to the material culture recovered through excavation they would be in serious trouble. The Assyrian king Sennacherib says that:

> in order to (be able to) draw well water every day, I had wire cables of bronze and chains of bronze made and I placed (trunks of) giant trees and date palms over the wells instead of the (usual) poles (*CAD*, s.v. *būrtu*, 335b).

To our knowledge no such objects have ever been found. Are we, therefore, to deny that Neo-Assyrian technology was incapable of manufacturing them?

More important, the translation of annaku as a 'tin-rich ingot-torque' is simply impossible. The one thing the Mesopotamian texts tell us about the source of *annaku* is that it came from two different mountains (*CAD*, s.v. *annaku*, 129b). We do not know where these mountains are to be located, but such a source hardly suggests a 'tin-rich ingot-torque'. *Annaku* does come in the form of 'bricks' (Sumerian SIG_4), 'talents' (Akkadian *lû*), 'sheets' (Akkadian *ruqqu*), and 'sticks(?)' (Akkadian *uppu*) (*CAD*, s.v. *annaku*, 130a; Wiseman 1967). The texts also refer to 'blocks' of *annaku* (Akkadian *šibirtu*) (*CAD*, s.v. *ittu*, 308b) or *šuklu* (*CAD*, s.v. *zakû*, 24b; Veenhof 1972). Again, how can all of this be reconciled with an 'ingot-torque' shape?

Annaku is itself used to fashion specific objects. What about a necklace consisting of beads of silver, gold, copper, and *annaku*? Or a tablet of *annaku*, a utensil of *annaku*, or a tiara of *annaku* (*CAD*, s.v. *annaku*, 129b–130a). There are also references to bracelets of *annaku* (Civil 1964: 3, 8). Such references simply cannot be reconciled with the interpretations of *annaku* given by Mr Dayton.

These references do not in themselves demand a translation of *annaku* as 'tin', but surely they do refer to some specific metal. The absence of actual objects made of metallic tin from excavations in Mesopotamia is a problem, but not a serious one. Tin was quite a valuable metal and any not in use would have been melted down and re-used. There are actually very few bronze objects from Mesopotamia, as they were also melted down and we know that smiths and workers were held strictly accountable for the metal and tools that they used (Moorey 1971).

Annaku and *Immanakku*: Babylonian glazes and the use of tin

The only point which needs to be discussed here is that involving the Mesopotamian glass texts.

Mr Dayton places great emphasis upon the argument that *annaku* cannot be 'tin' because *annaku* appears as an ingredient in the texts relating to the manufacture of Babylonian glazes and his analyses have shown that these glazes contain no tin (49, 66, 68)

It must be made clear that these texts do not call for the use of *annaku* or tin and that, therefore, the assumed absence of tin in Babylonian glazes has nothing to do with the translation of *annaku*. They do use lead (Gadd and Campbell Thompson 1936) and a type of stone called *immanakku*. No one knows exactly what kind of stone this is. A text from Sultantepe says:

> the name of the stone the structure of which is like river silt dotted with pebbles is *immanakku*-stone (*CAD*, s.v. *immanakku*, 127b)

It was used in the manufacture of imitation lapis lazuli, as in the text also cited by Dayton (66):

> if you are going to make artificial lapis-lazuli you pulverize separately 10 minas of *immanakku*-stone 15 minas of alkali ashes and one and two-thirds minas of Ú.BABBAR (*CAD*, s.v. *immanakku*, 127b).

Regarding this passage Dayton says (66):

> Therefore in this context at least *annaku* must mean a copper ore, as one could not 'pulverize' metallic copper.

But the text does not mention *annaku*; it mentions *immanakku*. How can such a passage be used to justify a translation of *annaku*? There is simply no support for the assertion (68) that *annaku* is one of the possible spellings of *immanakku*.

It is true that Assyrian medical texts have recipes involving the mixing together of tin (*annaku*), lead, and a frit-like glass known to the Assyrians as *anzaḫḫu* (*CAD*, s.v. *abāru*, 37a), but this has nothing to do with the manufacture of any sort of glass or glaze. Dayton claims that (68 f.):

> . . . the epigraphical evidence is very confused, and in many cases based upon faulty geological, metallurgical and ceramic knowledge and assumptions . . .

Surely it is not the epigraphical evidence which is based upon such false assumptions; rather the modern interpretations of such evidence are what is at fault.

The problem of *immanakku* has now been discussed in an authoritative fashion by A. Leo Oppenheim (Oppenheim *et al.* 1970). Oppenheim offers a general translation of 'sand' (1970: 90) and argues that its use in the various 'glass' recipes indicates that *immanakku* supplied the silicates for the manufacture of various glasslike substances (1970: 74).

In his attempts to actually manufacture 'glass' by following the instructions provided by the Mesopotamian texts Robert H. Brill assumed that *immanakku* designated various types of quartzite pebbles such as could be collected from a river bed (Oppenheim *et al*, 1970: 109 f.). The source of these pebbles suggested to Brill a probable explanation for the possible etymological connection between *immanakku* and *annaku*, alluded to by Oppenheim (1970: 74, 85):

> One might also argue that the connection with tin-ore . . . may be through a common source where both minerals were collected, for tin ore also could have been collected in river beds or gravels rather than having been mined (*op. cit.*, 1970: 109).

This is an interesting argument and raises a very important point, for Dayton (56 f.) has maintained that alluvial tin, being inconspicuous, would have escaped the attention of early man. This thesis is directly contrary to the opinion expressed in almost every modern discussion of this question, for all other scholars have assumed that alluvial tin was the only source of tin known to early man (Hedges 1964: 13). It was certainly known to Pliny the Elder who, describing what the Greeks called *kassiteros*, says that:

> It is now known that it is a product of Lusitania and Gallaecia found in the surface-strata of the ground which is sandy and of a black colour. It is only detected by its weight, and also tiny pebbles of it occasionally appear, especially in dry beds of torrents (Rackham 1952: 241).

Pliny describes this tin as consisting of 'black pebbles mottled with small white spots and of the same weight as gold' (*calculos nigros paullum candore variatos, quibus eadem*

gravitas quae auro) (Rackham 1952: 241). Surely what we have here is alluvial tin.

It is true that a passage in Diodorus Siculus (V. 22), quoted by Dayton (56 f.) does seem to refer to tin mining in Cornwall. However, Dayton's citation of this passage is incorrect and his translation very misleading. A more reliable translation would be:

> They [the inhabitants of Britain] it is who work the tin, treating it in an ingenious manner. This bed, being like rock, contains earthy seams and in them the workers quarry the ore, which they melt down and cleanse of its impurities (Oldfather 1938: 157).

What this shows is that some sort of tin mining was being undertaken in Cornwall, at least by the Hellenistic period. Yet such a reference cannot be used to deny the use of alluvial tin in antiquity. Alluvial tin exists in Cornwall and was still being utilized in the Medieval and Early Modern periods, as revealed by documents relating to the history of Cornish tin mining (Lewis 1906).

Dayton does correctly maintain (57) that a tin stream receives its tin from a major source in granite rock and that, though the stream itself has been exhausted by ancient workings, the pegmatites of the source should still be there for modern geological identification. Hence it is incorrect to say that all evidence for the identification of an ancient tin stream could have long since disappeared. Therefore, says Dayton, since such pegmatites have not been found either in Iran or in Turkey, these countries could not have served as sources of alluvial tin in antiquity. We would agree with the assertion, but not with the conclusion. It seems more reasonable to suggest that, in the case of Iran and Turkey, we have just not been looking in the right places up to now and that the pegmatites remain to be located.

Archaeologists, like nature, abhor a vacuum. Tin is a major vacuum in the understanding of the transition from the age of stone to the age of high energy plastic materials such as metals. It is so, not because we are ignorant of how tin appeared on the metallurgical scene, for that is relatively clear, but because the geology of its early exploitation remains to be explained. The data are consistent and they present us with a geological vacuum at the present time. The authors have combed the available data on tin, visiting alleged sites as distant as Kuhbanan in southern Persia and Mokur in Afghanistan. There are enough analytical evidences available at present to warrant the belief that in the early Bronze Age the Black Sea mountains and the Zagros mountains north of Hamadan afforded alluvial tin much as they afforded alluvial gold – and that the tin was quickly exhausted. In relatively quick order, tin was found in more distant sites, stretching from Hungary and Spain and possibly to Thailand. But there is no evidence at present to suggest that such exotic sources played any role in the trade and metallurgy of the Near Eastern Bronze Age.

We have tried, in this paper, to present the evidence that we feel must be considered in future studies of this question. The problem of tin in the ancient world is still with us today and there are no signs of an impending solution.

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Abstract

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Evidence for the sources and uses of tin during the Bronze Age of the Near East: a reply to J. E. Dayton

Contrary to the assertion of J. E. Dayton, Turkey is comparatively rich in mineral ores, especially copper, lead, zinc and iron, and provided adequate resources for ancient metallurgists; although he is correct in saying that tin is not yet known there. Dayton misinterprets the evidence for early metal-working traditions in the Near East, ignores the linguistic and textual evidence which supports the identification of the Akkadian term *annaku* as tin, and distorts the pattern of the mineral trade as revealed in the Kültepe and Mari tablets.