Short Notes

Has Authentic *Tĕkēlet* Been Identified?

In a First Identification of Authentic *Tĕkēlet*, (BASOR 265: 25–33), Ziderman argues that a violet dye, a mixture of indigotin (blue) and 6,6′-dibromoindigotin (purple), was derived exclusively from the Mediterranean mollusk species *Murex trunculus* in biblical and talmudic times. Although the title of the article implies that the author will provide compelling evidence for his theory, he cites no new chemical, textual, or historical data, and the arguments largely reiterate those in his earlier articles (*e.g.*, Ziderman 1981; 1983; 1986b).

To date, the chemical underpinnings for Ziderman's conclusions are extremely weak. Of the numerous ancient samples of molluscan origin that have been analyzed by modern instrumental techniques, not a single example has been shown to be a mixture of 6,6′-dibromoindigotin and indigotin (see Saltzman, below). Ziderman refers to Pfister's identification of a mixture of 6,6′-dibromoindigotin and "a second component that resembled indigotin" (p. 29) on a Palmyran textile "some 1700 years old"; but even he admits that Pfister's technique gave "only qualitative results."

It is possible that an ancient sample comprising a mixture of the two dyes will eventually be found and analyzed, since *M. trunculus* extract has precursors for both dyes (Fouquet and Bielig 1971), and shells of that species—which had been preferentially broken to remove the dye-containing gland—occur in ancient middens. Even if an ancient sample were chemically identified, however, the possibility that the mixture of the dyes was obtained from the extracts of other mollusk species, not only *M. trunculus*, cannot be ruled out. For example, photodebromination of the leucobase of 6,6′-dibromoindigotin from *M. brandaris* and *Purpura haemastoma*, converting it to indigotin, occurs very readily on exposure to light (Driessen 1944), to produce a mixture. Ziderman (1986b: 48) has recognized that possibility but believes it improbable that "the ancients could have efficiently converted Tyrian [royal] purple to a leucovat." Yet when discussing our experiments and Doumet's (detailed below), he states "that the dye may be vatted merely by the addition of alkali, the chemical reduction being attributed to the mercaptans released during the dye formation" (p. 28). Whether or not reducing or vat systems were known to ancient dyers, one cannot have it both ways and should at least be consistent about one's assumptions.

Greater or lesser amounts of either dye, and in some instances solely indigotin or 6,6′-dibromindigotin, can also be derived from *M. trunculus*. Thus, Ziderman himself refers elsewhere (1986b: 51) to the findings of Elsner and Spanier (1985) that the extract of live male *M. trunculus* generally yields indigotin, whereas the female extract generally yields 6,6′-dibromindigotin.

Different methods of processing the *M. trunculus* extract can also give varying relative amounts of the dyes. Based on current knowledge of the chemistry of molluscan dyes, we have proposed (McGovern and Michel 1984; 1985) that the 6,6′-dibromoindigotin on the interior of a 13th century B.C. storage jar from Sarepta (Lebanon) could have been derived from *Murex trunculus*, which were the only utilized shell remains in the contemporaneous archaeological context (Pritchard 1980). If the *Murex trunculus* extract solution were kept in the dark in the absence of a reducing system, the indoxyls not substituted in the 2 position, which forms about 90 percent of the indigotin blue, could have been prematurely converted to the dye during prolonged exposure to air and then separated along with the organic residues from the solution containing mostly precursors of the dibromo compound. Consequently, only the precursors of 6,6′-dibromoindigotin would have been

left in solution and then converted to the dye on exposure to air and light. Ziderman opposes this reconstruction of the Sarepta data (p. 30, n. 3), arguing that mercaptans formed from the precursors would act as reducing agents (Elsner and Spanier 1985) and that 6,6′-dibromoindigotin, less soluble than indigotin, would be preferentially deposited on the sides of a vessel. Regarding the first point, the amount of mercaptans available to reduce the dye would be less than the total amount of dye produced by oxidation and photooxidation, and if processing were carried out over an extended time and no oxygen scavenger or additional reducing agent were available, the dyes would certainly be reoxidized. The second argument assumes that 6,6′-dibromoindigotin is less soluble in water than indigotin (which has not been convincingly demonstrated), and that, if so, then the dibromo compound would precipitate and migrate to the sides of the vessel before indigotin formed. In fact, every area of the vessel interiors (rims, sidewalls, and bases) was covered with a purple coloration, implying that the solution was highly concentrated in the dibromo compound. The proposal that indigo might be removed from an extract mixture gains added support from Pliny the Elder (*Historia Naturalis*, book 35, section 27, edition of Rackham 1961, vol. 9: 295), the most reliable ancient authority (below).

In an attempt to account for evidence that goes against his theory, Ziderman sometimes misconstrues known dye chemistry. For example (p. 28), he dismisses Doumet's experiment (1980) in which a mixture of mollusk extracts, including *M. trunculus*, produced only red/purple, because the solution was inadequately exposed to light. According to Fouquet and Bielig (1971), however, about 60 percent of the product from *M. trunculus* made in the absence of light would be blue indigotin and 40 percent would be 6,6′-dibromoindigotin. If Doumet had carried out the process in the dark, the end solution should therefore be violet, not red as Ziderman claims.

Even the most detailed account of mollusk dyeing from antiquity, that of Pliny the Elder's from the mid-first century A.D., is too equivocal in its vocabulary and descriptions to allow us to reconstruct exactly the stages of the process (Michel and McGovern 1987). Based on experiments in our laboratory, it seems likely that the mollusk extracts were boiled for ten days in a tin vessel (Bostock and Riley 1855: vol. 2: 446, n. 93; Caley 1971) rather than a lead vessel (Bailey 1929). Pliny uses the word *plumbum*, but does not supply the adjective *album* ("white") for tin or *nigrum* ("black") for lead. The distinction is crucial in deciding whether a reducing/vat system is being described. Additionally, Pliny states that the vessels with the extracts were subjected to "an even and moderate heat," but without knowing what the highest temperature was to which the extracts were heated, we cannot determine that a specific reducing system was or was not operative. For example, reduction to the leuco base occurs slowly with metallic tin in a potash solution at about 90°C. Pliny, however, does not explicitly state that an alkali was used, and the alkalinity of the additives he does mention (urine and salt) would not have been sufficient for reduction to occur. Possibly, that was an oversight in his text, since strong alkalis (potash, soda, and lime) were available to the Romans.

If problems exist in relating Pliny the Elder's descriptions to the known chemistry of mollusk dyeing, then extrapolating the Roman process back into much earlier times is fraught with even more difficulties. Purple was so intimately associated with the Canaanite city-states (Tyre, Sidon, Sarepta, and others) along the Levantine coast that the name by which they called themselves (Canaanites), as well their later historical appellation (Phoenicians), very likely derive from ancient Semitic and Greek roots for "purple" (Speiser 1936; Landsberger 1967). Perhaps as much as the Semitic alphabet or the seafaring by which the latter was transmitted to the West, purple dyeing was a distinctive element of Phoenician culture, and to promote the industry dye factories were set up at colonies throughout the Mediterranean (Reese 1979–1980). With so much importance attached to purple dye, it is surprising that Phoenician texts hardly refer to purple, and that the process itself goes totally unmentioned. The textual omission of mollusk dyeing in Phoenician texts may very well be due to the limited archaeological investigation of Iron Age sites in Lebanon thus far. Biblical texts, which incorporate Iron Age and later traditions, are more informative about the involvement of Phoenician city-states, especially Tyre, in the industry (Ezek 27:7, 16, 24; 2 Chr 2:7, 14). Yet again, no information about the actual process can be gleaned from the texts, as one might anticipate if Israelites were not directly involved in the industry. It should also be

noted that Ziderman does not deal directly with biblical texts, but rather relies largely on secondary sources (*e.g.*, Forbes 1964).

The lack of pre-Classical documentation for the process originally prompted us to seek an answer to the apparent contradiction between the presence of 6,6′-dibromoindigotin on the interiors of pottery vessels at Sarepta and the fact that only broken *M. trunculus*, which should yield a mixture of indigotin and the dibromo compound, were found in the vicinity. The archaeological evidence from Sarepta is particularly significant because it is the earliest instance of a dyeing facility in the eastern Mediterranean. A spouted vat with 6,6′-dibromoindigotin on its interior was interpreted as a processing container by which liquids could be drained and solid organic residues scooped off the surface. Since the dyeing facility was in the midst of a large group of pottery kilns (Khalifeh, in press) in which pottery was fired to a temperature at least above 500°C, possibly the fresh extract mixture was exposed to temperatures near 100°C, which could have deactivated the enzyme purpurase or aided reductive reactions under appropriate conditions.

We should stress that, without more definitive textual or archaeological evidence, our hypothesis of how purple dye might have been derived from *M. trunculus* cannot be proven. Further excavation at Sarepta, for example, could lead to the discovery of a pile of utilized *Murex brandaris* or *Purpura haemastoma*, from which pure 6,6′-dibromoindigotin could be obtained more directly. The primary function of a hypothesis, where interpretation is based on a very narrow data base, should be not to prove something but to provide a framework for carrying out additional investigation. In the case of the Sarepta dye industry, specific chemical experiments and archaeological test soundings at the site are planned to test our hypothesis. Ziderman's thesis should have a similar intent; it also should be subjected to testing procedures. Given what is presently known, a violet, combining 6,6′-dibromoindigotin and indigotin, need *not* have been obtained solely from *M. trunculus* in biblical and talmudic times. Depending on the process used, alternative chemical pathways might have been followed to obtain either pure indigotin, pure 6,6′-dibromoindigotin, or various mixtures of the two dyes from any of the mollusk species. Ziderman's statement that "*T. trunculus* was employed for *tĕkēlet* at Sidon and Sarepta" (p. 28) is not supported by the archaeological data—only samples of purple have been recovered. Until more exact textual descriptions of the industrial process are forthcoming or until at least one ancient sample containing a mixture of indigotin and the dibromo compound has been found and chemically verified, the "*trunculus* theory" will remain divorced from the evidence.

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ADDENDUM

With reference to the Ziderman article on the nature of *tĕkēlet* (1987a: 25–33), I have two comments.

First, no one has yet presented proof of his theory. Only an unambiguous analysis of a sample of yarn dyed with the secretions from *M. trunculus* can begin to give that proof. A sample of dyed filter paper that Ziderman gave me when he was at the University of California at Berkeley in 1984 contained only 6,6′-dibromoindigotin, according to spectrophotometric analysis. I gave the results of my analysis, a description of the analytical method employed, and representative curves of mixtures of 6,6′-dibromoindigotin and indigotin from other sources to Ziderman *before* he presented his paper at the American Chemical Society in 1984 and published articles in *BASOR* and *Review of Progress in Coloration* in 1986. In each of the latter instances, he fails to mention that my analysis of dyed material from *M. trunculus* did *not* contain a mixture, and therefore did *not* conform with his theory. I do not question the work of Fouquet and Bielig (1971); they undoubtedly found the precursors for indigotin and 6,6′-dibromoindigotin in extracts from *M. trunculus*; but the presence of both dyes in an ancient or modern dyed yarn has not been verified. As I have also stated as a necessary precaution in an earlier paper (1978), "while the dyed yarn cannot contain any material which was not in the plant (except for mordants which may be used), there is much in the plant which may alter a curve but which will not dye the plant [*sic*; yarn is the correct word]. The same is true of insect or other animal material."

Second, the "facile technique for directly identifying a mixture of indigotin and 6,6′-dibromoindigotin" (p. 29) is misleadingly credited to "M. Saltzman and I. Ziderman, unpublished." I and I alone developed the analytical method, and Ziderman has cited it without my permission. One of the above-mentioned curves of a mixture of indigotin and 6,6′-dibromoindigotin, which I gave to Ziderman, was also published without my consent (1986b: fig. 2). The possibility exists for a simple semiquantitative method of analysis of a mixture of the two dyes; but the concentrated sulfuric acid solutions, as well as those of other solvents that have been tried, are not stable for more than 10 to 15 minutes. Consequently, the method is still being refined, and I have not published my results.

Eventually perhaps, proper samples dyed with extracts of *M. trunculus* under conditions consistent with ancient technology will be shown to contain a mixture of indigotin and 6,6′-dibromoindigotin. Yet to date, I believe, on the basis of my forty years of experience in the analysis of colorants, that Ziderman's theory has not been proven.

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Response

The analytical method Saltzman describes was developed because I had explained to him the necessity for a technique to identify a *mixture* of indigotin and 6,6′-dibromoindigotin (DBI), the characteristic composition of *tĕkēlet* (hyacinthine purple). I accordingly proposed that he demonstrate that this could be achieved using his method (1978) for identifying the *separate* constituents. Saltzman mailed to me the resulting spectral curves in camera-ready artwork and on a projection slide for the expressed purpose (his letter August 9, 1984) of my presenting them in my paper at the American Chemical Society Meeting in Philadelphia, which I did with his consent. While this artwork arrived too late to be fitted in the prepared text of the lecture as later published (Ziderman 1986a), I managed to incorporate it in the review article (1986b: fig. 2). Joint accreditation (Ziderman 1987a: 29) of Saltzman's work was, regrettably, misleading, and is hereby retracted.

Pfister Misquoted

To prove an injudicious point, McGovern and Michel have garbled both Pfister's findings and my reference to them (1987a: 29). Pfister (1937: 11) obtained the "mixture of DBI with a second component that resembled indigotin" from live *Murex trunculus*! The "Palmyran textile some 1700 years old" was specimen #L. 41, woolen weft yarns colored bluish-violet, which Pfister competently analyzed (1937: 23) and found to contain indigotin and DBI, just as expected for an authentic *tĕkēlet* dyeing with *M. trunculus*. He also verified the presence of bromine.

Dubious Debrominations

Contrary to the statements of McGovern and Michel, Driessen (1944) did *no* experiments on "*M. brandaris* and *Purpura haemastoma*," nor did he "produce a mixture." He merely converted synthetic DBI to indigotin. Without experimental demonstration of the occurrence of a controlled *partial* photodebromination of DBI to produce a *mixture* of DBI with indigotin, there is no justification for doubting that *M. trunculus* is the source of *tĕkēlet*.

There is accordingly no "inconsistency" in my assumptions regarding the ability of the ancients to convert Tyrian purple to a *leuco*-vat. In my previous article (1986b), I expressed caution regarding the assumption that they could have consistently achieved a *completely efficient* conversion, "just as it can be done today using synthetic sodium dithionite" (p. 48); and, in that same article, I added that Elsner and Spanier "discovered that the dye could be vatted by addition of alkali alone. . . . *To*

prevent premature oxidation, the authors added either oil to create an impervious floating film or D-glucose" (p. 51). I distinguished thereby between the *probable* state-of-the-art in antiquity and what is technically *feasible* when exploiting today's scientific insight and hindsight. Similarly, the tin vat hypothesis (Doumet 1980) lacks archaeological evidence in the form of an appropriate tin vessel coated with DBI, and Bailey (1929: 155) has refuted the textual evidence presented by McGovern and Michel (above; 1987).

Individual variations in dye composition between male and female *M. trunculus* would have been quite ineffectual when tens of thousands of snails were collected for use in a single vat of over 100 pounds of dye (Pliny *Nat. Hist.* IX: 133–38). The result would obviously have been a typical *tĕkēlet* composition of DBI with indigotin (Bouchilloux and Roche 1955; Fouquet and Bielig 1971).

A Pre-Phoenician Pliny?

In regard to their "hypothetical scenario" of how indigotin was supposed to be removed from the Sareptan dye-vat, McGovern and Michel (1984: 70) state: "This points to an earlier experimental stage of dye processing, when the importance of a reducing environment and, by implication, of the vat method, was not well understood." But in their note here, they cite Pliny the Elder as "added support" for that proposal, implying anachronistically that a Roman could have been familiar with primitive pre-Phoenician experimentations.

Although "every area of the vessel interiors was covered with purple coloration," only a single square centimeter of sherd was analyzed (McGovern and Michel 1985). *Tĕkēlet* is also a purple shade. It seems quite unjustified to conclude (McGovern and Michel, above; also 1984; 1985) that this sherd is necessarily representative of all other fragments. The absence of indigotin from the vessel is therefore not rigorously demonstrated.

My statement that "*M. trunculus* was employed for *tĕkēlet* at Sidon and Sarepta" must be read in its context of the "production centers in antiquity utilizing the *species locally available*" (1987a: 27–28), and it is properly supported by the archaeological data on *M. trunculus* debris at those sites. One square centimeter of sherd from Sarepta with proven DBI encrustation cannot invalidate this statement.

In Defense of Doumet

I do not dismiss Doumet's research (1980), nor does it go against my theory: both are misconstrued above. Let me quote: "*M. trunculus* gives a red color with a slight violet tinge. *M. brandaris* gives a very delicate violet-pink. *Purpura haemastoma* gives a violet color" (Doumet 1980: 47), which McGovern and Michel render erroneously, thus: "a mixture of mollusk extracts, including *M. trunculus*, produced only red/purple"! Since Doumet *did not mention* what illumination he used, it was purely speculative to suspect that "omission of proper exposure to sunlight may *also* explain why Doumet obtained a red color from *M. trunculus* instead of the violet found by other investigators" (Ziderman 1987a: 28). McGovern and Michel are mistaken to ascribe dye compositions (60 percent indigotin + 40 percent DBI) to Fouquet and Bielig, who quantitatively analyzed only the indoxyl *precursors*, from which, "*besides indigo and DBI*, the analogous indirubins and other known red 2,3′-coupled indigoid dyes are formed" (Fouquet and Bielig 1971). The yield of those red constituents was evidently enhanced in the particular experimental conditions of Doumet's work (1980), including sex of *M. trunculus* used, temperature, aeration, illumination, etc. Similarly, a dye composition of indigotin with indirubin (no bromine derivatives), as found by Malaczkiewitz (1967), is expected as the typical result for *M. trunculus* males, just as it is found (Ziderman 1986c; 1987b) for plant indigo (see Elsner, below). The same anomalous result for *M. trunculus* was previously reported *inter alia* by Derrien (see Bouchilloux and Roche 1955), and can be explained if the snails were gathered in early spring when males predominate. Seasonal variation of population gender (O. Elsner, personal communication, August 1987) suggests the possibility that males may change sex to female, as occurs in another shell fish, the common Atlantic slipper shell *Crepidula formicata*.

The Elusive Indigotin

The single substantive issue raised by McGovern, Michel, and Saltzman concerns absence of indigotin from a filter paper and a sherd, presumably dyed with *M. trunculus* extracts.

The paper that Saltzman tested for me was not *dyed* at all, but merely had been stained while I

was filtering a chloroformic solution of dye made from *M. trunculus* (Ziderman 1986a: 193). The lack of indigotin was evidently due to its absence from the extract that was made from snails gathered in the summer, when females predominate. Even if any indigotin had been present, colloidal DBI would have been selectively retained by the filter. And even were this DBI dissolved, it evidently had a greater affinity for paper cellulose than any indigotin present. Saltzman himself even stated that outcome "as a necessary precaution" (above; 1978). It certainly cannot discredit the known dye chemistry of *M. trunculus* (Elsner and Spanier 1985; Elsner, below).

In regard to staining of the Sareptan sherd by DBI, McGovern and Michel (above; 1987) consider that the "the fresh extract mixture was exposed to temperatures near 100°C." At that heat, the indoxyls substituted at the 2 position are converted to DBI in the *absence* of light (Baker and Sutherland 1968; Christophersen *et al.* 1977). Furthermore, dye formation is evidently a chain reaction (Christophersen *et al.* 1977), that would in any case indiscriminately transform the various indoxyls present to dye. "Current knowledge of the chemistry of molluscan dyes" thus invalidates "the proposal that indigo might be removed from an extract mixture," McGovern and Michel's "reconstruction of the Sarepta data." In fact, if anything, it is the DBI that has been removed from the extract mixture, crystallizing on the vessel walls. Because its reduction potential (*ca* −600 mv) is higher than indigotin (*ca* −350 mv), DBI will be oxidized and deposited first (Elsner and Spanier 1985). I do not assume that DBI "would all precipitate and migrate to the sides." While notably absent from sherds, however, the *tĕkēlet* composition (DBI + indigotin) *has* been found in ancient textiles.

Four Chemical Identifications of Tĕkēlet

I concur in requiring evidence for the presence of the *tĕkēlet* composition (DBI + indigotin) on ancient yarns as archaeological proof of the *M. trunculus* hypothesis. Four such specimens have been identified so far, all analyzed by independent investigators (who notably were uninvolved in the *tĕkēlet* issue).

Pfister's classical analysis of DBI and indigotin on the woolen specimen from Palmyra is cited above. Using Saltzman's own spectrophotometric method (1978) and thin-layer chromatography (TCL), Daniels (1985) has shown the presence of both DBI and indigotin in two textile fragments from Enkomi, Cyprus, each dated to the *first century* B.C. by radiocarbon accelerator.

Tĕkēlet *from Israel*

The fourth specimen is from Israel, a blue weave of woolen yarn from Gihon's excavation of the seventh century A.D. site, ʿEn Boqeq, which Masschelein-Kleiner has determined by her TLC technique (1970) to contain DBI and indigotin, and has confirmed its content of bromine by X-ray fluorescence spectroscopy (Sheffer 1987).

In view of the lack of Phoenician documentation of purple dyeing, which we may attribute to industrial secrecy, I should indicate some biblical references to Israelite involvement in this craft. Comparison of v. 23 with v. 25 of Exodus 35 reveals that the Israelites spun into yarn the *tĕkēlet*, which they received as dyed woolen tops (Ziderman 1987a: n. 7). The tribe of Zebulun "shall suck . . . the hidden treasures of the sand" (Deut 33:19), which is rendered "they shall gather snails to dye *tĕkēlet* from the mucus for the tassels of their garments" (Targum Jonathan ben Uziel *l.c.*); this was their traditional occupation (Sifrei *l.c.*). King Hiram of Tyre sent to King Solomon a *tĕkēlet* expert of mixed Tyrian-Israelite parentage, to work with Israelite craftsmen in preparing *tĕkēlet* for the Temple in Jerusalem (2 Chr 2:13). Talmudic references have been reviewed (Ziderman 1986c; 1987c), only *tĕkēlet* specially dyed by Jewish dyers being ritually fit for the biblical *tĕkēlet* commandment (Num 15:38). On Semitic roots of the terms Canaan, Phoenician, and purple, consult Astour (1965).

Tĕkēlet *in Pliny's Text*

Dedekind (1898) understood Pliny as referring to two hyacinthine purples, one a simple dyeing (= *tĕkēlet*) with *M. trunculus*, the other an artificial concoction of a mixture of shell species. The text he cites (*Nat. Hist.* XXI: 22), however, only mentions a violet dye named amethyst, which is also called ianthine or hyacinthine purple.

The source of Dedekind's statement is an earlier passage (Pliny *Nat. Hist.* IX: 127–40), where the

two classes of mollusk dyes are termed *purpura* and *conchylia*. *Purpura* was made with mixtures of two shellfish species, *bucinum* and *pelagia* (= sea-purple = *murex*); the latter existed in several varieties. One of those *purpura* mixtures gave an amethyst color, another gave Tyrian purple. The other dyeing, *conchylia*, was made without *bucinum* and had a paler shade: Pliny implies that this also was amethyst.

I interpret his description as follows: One amethyst color was a *purpura*, made by mixing *bucinum* and *pelagia*; the paler amethyst was *conchylia*, made solely from *pelagia*. Dedekind (1898) evidently says just this, adding the equation: *conchylia* = amethyst = hyacinth = *těkēlet*, which was prepared solely from a *pelagia* that he identifies as *M. trunculus*. Consideration of the morphological and ecological features that Pliny describes has indicated that *M. brandaris* is also a variety of *pelagia* (probably *dialutense* ("mud purple"?), which he designates as the best source for the dye), and that *Thais haemastoma* is *bucinum* ("trumpet-shell"). *M. trunculus* would probably be the "pebble purple" that Pliny states is remarkably suitable for dyeing (Ziderman 1986c).

Conclusions

Purple research is properly conducted in close conjunction with experimental dyeing with fresh murex (see Elsner, below). Otherwise, incautious hypothesizing is encouraged. Evidence from chemical analysis of archaeological specimens is properly evaluated within the overall context of evidence from other disciplines (Ziderman 1986b; 1986c; 1987a; 1987b) rather than as isolated data. This is particularly true when it is "based on a very narrow data base" that is subject to the uncertainties inherent to much archaeological evidence.

Considering the polemic tone that McGovern, Michel, and Saltzman take, it is most significant that no *alternative* hypothesis is mooted there for the nature of antique *těkēlet*. Chemical analyses of several ancient yarns containing the characteristic *těkēlet* composition (DBI + indigotin) are herewith presented, as they have stipulated, to corroborate the *M. trunculus* theory of the historical source of hyacinthine purple (= *těkēlet*) (Bizio, in Grimaud de Caux and Gruby 1842; Dedekind 1898; Fouquet and Bielig 1971; Edelstein 1987). The ultimate verification will be the discovery of an ancient ritual tassel (Num 15:38) dyed with the indigotin + DBI composition.

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ADDENDUM

I have to confess that I do not know about any investigation of ancient Tyrian purple dyed fabrics for the presence of indigo. Also in my work we have not considered such an analysis since the presence of indigo is for us obvious in dealing with dyeing by *Murex trunculus* extracts. Already Bouchilloux and Roche (1955) found a brom-free precursor. There is some discrepancy between the work of Malaszkiewicz (1967) and Fouquet-Bielig (1971). Malaszkiewicz prepared the dyes by acid hydrolysis only and did not find a bromo-derivative. Accordingly, he concluded that *M. trunculus* yields mostly indigo and indirubin. Our investigations show that *M. trunculus* provides both brom and brom-free precursors, but so far we can say the ratio of both does change and we are now studying the problem.

Very often we consider ourselves much wiser than our predecessors were, but maybe the opposite is true. With very limited means they achieved marvelous things, *i.e.*, nice colors varying from blue to reddish purple using the same mollusk. They could do such things on the basis of years of observations and accumulation of expertise. One of the possibilities, in which I believe, was the regulation of what we today call pH of the dyeing bath. We have shown that by regulating the pH we could obtain various colors in the range of purple. For example, I have prepared two samples of woolen yarn dyed one after the other in the same bath of *M. trunculus* extract at various pH. One is purple, the other is blue; the blue is indigo. Since indigo was in the dye bath together with 6,6′-dibromoindigo, it would be illogical to strip the dye and analyze it to show that it is really indigo (if no other blue dye was present in the bath, and the dyeing was carried out in the absence of direct solar irradiation, which may cause debromination).

Regarding the results of Malaszkiewicz versus those of Fouquet-Bielig, it is interesting that

Malaszkiewicz got only brom-free dyes, whereas Fouquet, by similar acid hydrolysis, got both dyes.

In our experiments, enzymatic and acid hydrolysis gave differences mainly in the by-products, such as indirubin, etc., but we always got both bromoindigo and indigo, both at lower yields. Even in the male glands the bromo precursor is present. Maybe our present investigation will bring some clues to help resolve this question.

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Concluding Response

Rather than take up the individual points Ziderman raises in his reply, involving details of indigoid dye chemistry, we would simply stress that the major chemical tenet of the *tĕkēlet* theory remains to be proven, viz., that *tĕkēlet* was produced exclusively from *Murex trunculus* extracts in antiquity. We concur with O. Elsner when he writes in his addendum that " . . . colors varying from blue to reddish purple" can be produced "using the same mollusk." An equally important, unresolved issue is whether *tĕkēlet* is to be translated "violet" rather than "blue" in biblical and talmudic texts.

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