

## Reply to Blair

Michael L. Kunz  and Robin O. Mills

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*Blair provides a thorough review of data he claims stands in opposition to our narrative concerning the origins of IIA40/Early Blue glass trade beads and their presence in arctic Alaska prior to Columbus' initial voyage. He employs three lines of evidence: historical and archaeological data, Instrumental Neutron Activation Analysis, and radiocarbon dating. Our reply addresses his application of these data sets, clarifying his use of our data to arrive at his conclusions. While we continue to disagree with Blair, we do wish to acknowledge his time spent on debating the issue thereby furthering all of our understanding on this topic.*

**Keywords:** IIA40 / Early Blue glass trade beads, Paternostri guild, Late Prehistoric Eskimo, radiocarbon dating, instrumental neutron activation analysis, fifteenth century, sixteenth century, seventeenth century

*Blair proporciona una revisión exhaustiva de los datos que, según él, se oponen a nuestra narrativa sobre los orígenes de las perlas comerciales de vidrio IIA40 / Early Blue, y su presencia en el Ártico de Alaska antes del viaje inicial de Colón. Emplea tres líneas de evidencia: datos históricos y arqueológicos, análisis de activación de neutrones instrumentales y datación por radiocarbono. Nuestra respuesta aborda su aplicación de estos conjuntos de datos aclarando su uso de nuestros datos para llegar a sus conclusiones. Si bien seguimos en desacuerdo con Blair, deseamos reconocer el tiempo que dedicó a debatir el tema y, por lo tanto, ampliar nuestra comprensión sobre este tema.*

**Palabras clave:** IIA40 / Cuentas comerciales de vidrio azul temprano, gremio Paternostri, esquimal prehistórico reciente, datación por radiocarbono, análisis de activación de neutrones instrumentales, siglo 15°, siglo 16°, siglo 17°

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Blair (2021) comments that the invention of Venetian drawn, hollow, cane manufacturing is less muddled than we present, pointing to approximately AD 1470 as its inception. We accept that this timing occurred in the decades prior to the founding of the Paternostri guild (AD 1486). Blair cites archaeological data in Venice and the New World, indicating the complete absence of the *a speo* method and IIA40 beads before AD 1560. Nonetheless, as with Punyik Point, pre-1560 Early Blue / IIA40 beads were also recovered during excavations at Nueva Cádiz (Jeffrey Mitchem, personal communication 2021), a Venezuelan

Spanish pearl fishing station active from AD 1515 to 1541, though their presence is unreported in publications (Deagan 1987). We posit that if present this early, then why not earlier?

Blair (2021) points out that after the establishment of the Paternostri guild, drawn beads were “primarily” finished by grinding rather than by a *speo* heat rounding during the late fifteenth and “much of” the sixteenth (presumably ~1560) centuries, citing archaeological evidence. Our narrative has been based on beads that could have been made during a development period prior to regular production by the *a speo* method, a point supported by Francis (1988),

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who states that Ila40 / Early Blue beads were among the earliest of the drawn beads. We are not arguing that the *a speo* method was the sole or primary method of finishing beads prior to the mid-sixteenth century. Nothing presented by Blair changes the fact that scholars are uncertain of the exact timing of *a speo* development. Radiocarbon and other dating from tightly controlled proveniences associated with these beads, and more documentary research, will narrow down this timing.

Although Blair considered this an unfortunate methodology choice, we had the Punyik Point beads analyzed by instrumental neutron activation analysis (INAA) in order to compare them directly to earlier INAA studies of the same bead variety (Hancock et al. 1994). We produced the same graphic during our analysis that Blair (2021:Figure 1) did, and we stand by our original conclusion: the results are inconclusive regarding temporal assignment, given that the arctic beads' data overlap all three of the Hancock and colleagues' periods. More work is obviously needed.

Blair also states that both of the Punyik Point dates in our report are "problematic" and that we ignore a suite of other assays from the site that date to later centuries. We have explained in detail the old wood effect associated with the Beta-193802 assay (Kunz and Mills 2021). We are not "dismissive" of this issue. In fact, our understanding of it, in the context of Punyik Point, brings this date into alignment with the bead-associated twine date (Beta-201353). Blair correctly points out that in Kunz (2005), the Beta-201353 dated material is labeled "sinew." The sinew label was an unfortunate typographical error that was not discovered until after publication. The  $^{13}\text{C}/^{12}\text{C}$  stable isotope ratio for this assay, shown in Kunz (2005) and Kunz and Mills (2021), is consistent with terrestrial plants, not animals, and certainly not marine mammals. Less than 1% of the  $2\sigma$  calibrated temporal probability of that date is more recent than AD 1488, and we stand by its accuracy.

Blair implies that earlier publications by Kunz (2005, 2009) indicate that the Late Prehistoric Eskimo occupation of Punyik Point occurred primarily during the sixteenth and seventeenth centuries. His Figure 2, used to support this

position, is misleading. The figure points out, correctly, that there are multiple Late Prehistoric occupations there. The site is a palimpsest of repeated occupations over thousands of years. Of the 45 visible house and cache pits on site, most are probably Late Prehistoric in age, and only five houses have been dated: two from the fifteenth century and three from later centuries. Semi-subterranean houses indicate winter use, and the simultaneous occupation of only a few houses would completely deplete the site's willow-patch fuel source. Consequently, the locale would be abandoned for extended periods while the willows replenished—a cycle that was repeated over and over. In fact, there were likely *multiple* sixteenth- and early seventeenth-century occupations at the site, but Ila40 beads have not been found in any of their features. In our report, we present only two of the 21 radiocarbon dates associated with the site *because* they are the only two that relate to the presence of Ila40 beads.

As explained in our report, the association between the Lake Kaiyak and Kinyiksugvik dated materials and the beads is not as secure as those outlined for Punyik Point. We regard the Lake Kaiyak bead-associated assays as supporting a credible fifteenth-century occupation at the site. The association between the Kinyiksugvik bead and its adjacent dated material is the poorest of all the dates, and it may not even be associated with the Ila40 bead at all in this midden context. Assays from these two sites may also represent examples of the "heirloom effect," a cautionary point also made by Deagan (1987:162) for some early glass beads from the southeastern United States.

In sum, we stand by our original conclusions that the most parsimonious explanation of available data is that Ila40 beads arrived in arctic Alaska in the latter fifteenth century, and that they traveled east from Venice over routes currently unknown. We urge further historical research into both *a speo* and drawn cane manufacturing. We hope our report stimulates academic interest in and reporting of fifteenth- to seventeenth-century European glass trade beads in Asia.

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*Data Availability Statement.* No original data were presented.

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