Pereltsvaig claims that my phrasal pied piping analysis of Semitic NPs (Lingua 114:1465–1526) is inadequate since it fails to account for (i) the distinct behavior of light and heavy adjectives, (ii) the position of DP and PP complements of the noun and (III) the correlation between agreement in definiteness with the position of the modifiers. She also finds my work “theoretically inelegant” and “non-minimalist”.

I’ll be brief and stick to the three substantive claims. The rhetorical ones require no response.

Claim 1: Light adjectives appear in inverse order; heavy adjectives appear in canonical order. Shlonsky can’t account for this. Pereltsvaig can “in a more direct and elegant way”.

The claim is undermined by a fatal methodological error and benefits from no empirical support. Pereltsvaig’s own analysis is perhaps ‘elegant’ but insufficient.

Consider the error. Pereltsvaig’s examples of DPs with heavy adjectives (her (12) and (13)) only show that a heavy adjective follows a light one. To show anything about adjective ordering, one needs examples which are independent of heaviness. In other words, one crucially needs a test case of a DP containing at least two heavy adjectives. Then, one could determine whether they are ordered ‘canonically’ or ‘inversely’. “Unfortunately,” writes Pereltsvaig, “this … cannot be tested, since strings with two heavy adjectives in Hebrew appear to be degraded, regardless of the order …”. Unfortunate indeed.

Relative clause modifiers are also heavy, must follow light adjectives and, unlike heavy adjectives, there may be more than one. Indeed, when there are two or more relative clauses in a DP, they are freely ordered among themselves. This looks relevant but Pereltsvaig, rather than attempting to state a generalization here, denies that one is possible, stating that relative clauses “have some independent reason for their placement”. She never deigns to inform the reader what this reason is.
The relevant generalization, however, is easy to state: heavy constituents, including full and reduced relative clauses and adjectives with phrasal or PP complements (Pereltsvaig’s ‘heavy’ adjectives) appear to the right of light ones.

This generalization is, to be sure, not an explanation. That would require a theory of ‘heaviness’, but given the notorious but little understood tendency of phonologically heavy constituents to follow light ones, such a theory is needed above and beyond the Hebrew facts.

A heavy adjective in Hebrew can follow a genitive PP. The genitive in (1a) – a variation on Pereltsvaig’s (12b) – is sandwiched between the light adjective and the heavy one. The contrast with (1b) is sharp; the genitive simply cannot follow the heavy adjective

\[
(1) \quad \begin{align*}
&\text{ha-psalim} & \text{ha-yafim} & \text{šel matis} & \text{ha-yoter govhim} \\
&\text{DEF-sculptures} & \text{DEF-beautiful of} & \text{Matisse} & \text{DEF-more tall} \\
&\text{mi-david} & \text{šel} & \text{mikelandzelo} \\
&\text{from-David} & \text{of} & \text{Michelangelo} \\
\end{align*}
\]

‘the beautiful sculptures of Matisse taller than Michelangelo’s “David”’

\[
(1) \quad \begin{align*}
&\text{ha-psalim} & \text{ha-yafim} & \text{ha-yoter govhim} \\
&\text{DEF-sculptures} & \text{DEF-beautiful} & \text{DEF-more tall} \\
&\text{mi-david} & \text{šel mikelandzelo šel matis} \\
&\text{from-David} & \text{of} & \text{Michelangelo of Matisse} \\
\end{align*}
\]

Pereltsvaig’s example (17) purports to show that a heavy adjective cannot follow a possessor. While the grammaticality judgement is sound, the acceptability of (1a) proves that the general statement about the order of heavy adjectives and possessors is false. Nobody, as far as I know, has come up with a general explanation of the factors which determine the relative ordering of genitive PPs and noun modifiers in Hebrew, since the data appear to point in contradictory directions.

The acceptability of (1a) is, however, fatal for Pereltsvaig, who crucially needs genitive PPs to remain inside NP (although she fails, throughout her paper, to distinguish between the position of complement and adjunct PPs). If, as she claims, the heavy adjective in (1a) sits in the specifier of some projection and N has adjoined to some head above it, there is simply no way to derive the order in (1a).

From the perspective of the heaviness constraint, it comes as no surprise that (2), which contains a heavy genitive, is significantly better than (1b). From Pereltsvaig’s perspective, this contrast is completely unexpected.

\[
(2) \quad \begin{align*}
&\text{ha-psalim} & \text{ha-yafim} & \text{ha-yoter govhim mi-david sel} \\
&\text{DEF-sculptures} & \text{DEF-beautiful} & \text{DEF-more tall of-David of} \\
&mikelandzelo šel ha-aman ha-corftai ha-post-impresionisti matis \\
&\text{Michelangelo of} & \text{DEF-artist DEF-French DEF-post-impressionist Matisse} \\
\end{align*}
\]

‘the beautiful sculptures of the French post-impressionist artist Matisse (which are) taller than Michelangelo’s “David”’.

The type of contrast that (1b) and (2) illustrate is not only unsurprising, it is blandly familiar. The pair of sentences in (3), for example, suggest that heaviness is correlated with the rearrangement of post verbal constituents.

\[
(3) \quad \begin{align*}
&a. & ??\text{John gave to Mary the article.} \\
b. & \text{John gave to Mary Skosh Lurny’s profoundly boring article about noun phrases in Middle High Jebusite.} \\
\end{align*}
\]

\[
(3) \quad \begin{align*}
&\text{John gave to Mary Skosh Lurny’s profoundly boring article about noun phrases in Middle High Jebusite.} \\
\end{align*}
\]
By parity of reasoning, heaviness in the DP drives some kind of extraposition, heavy NP shift or remnant NP movement. I do not claim to understand the nature of these transformations but I do insist that if one wants to have a special theory for Hebrew heavy adjectives, as opposed to a general theory of heaviness, one should justify one’s decision. Pereltsvaig fails to do so.

Claim 2: Shlonsky’s “remnant phrasal movement analysis faces insurmountable problems as far as PP complements of the nouns are concerned”.

This claim rests on several egregious misrepresentations. My paper argues against remnant movement, not in favor of it. In note 20, for example, I write: “there seems to be no analytic justification for remnant movement in the syntactic domain studied in this paper”.

If distortion were not enough, Pereltsvaig proceeds to attribute to me an untenable set of assumptions regarding event nominalizations.

“Under Shlonsky’s assumptions,” she writes “the order of (24a) is derived by merging the external argument ... as the sister of the head noun and moving the constituent containing the noun and the external argument over the adjective”.

Anyone reading section 9 of my paper (esp. p. 1515) would be struck by how different my actual analysis is from Pereltsvaig’s caricature. For example, I explicitly adopt the conventional view of the formation of derived nominals by head movement (of V to a nominalizing head). At no point in my paper do I argue, pace Pereltsvaig, that head movement is “both theoretically problematic and empirically unnecessary”. It is astonishing and deeply troubling that the reviewers who recommended publication of Pereltsvaig’s paper failed to see all of this.

Pereltsvaig proceeds to discuss what my alleged analysis would say about PP (free state) adnominal complements. She notes, in passing, that “the placement of PP arguments is part of a more general problem (not considered in detail by Shlonsky himself)”. The truth – available to anyone who read my paper – is not that I do not discuss these “in detail” but rather that I hardly discuss them at all.

Indeed, with the exception of a couple of footnotes, I do not have anything to say about PP arguments. This may be a serious shortcoming of my analysis, but from the alleged invalidity of a non-existent analysis of non-discussed data, one cannot draw any conclusions about the existing, published analysis. This kind of straw man argumentation should be vetted out of scientific exchanges rather than encouraged.

Claim III: Shlonsky’s analysis of agreement internal to the DP (concord) uses AgrPs to encode the correlation between agreement with the position of the modifiers.

This is not the essential point of my section 6. What I tried to defend is a configurational approach to concord, modeled on the mechanism standardly used to represent subject-verb agreement. I did not make use of the mechanism of AGREE, which, in retrospect, would have greatly simplified the analysis.

But Pereltsvaig does not make this point. Rather, she centers her discussion on agreement in definiteness, claiming that I did not “notice the of all agreement features, definiteness is the best predictor of the modifier’s position” and goes on to justify the centrality of definiteness agreement on the basis of fabricated facts. Her (43) alludes a contrast between pre and postnominal demonstratives. The sad thing is that Hebrew only has postnominal demonstratives. One could go on and on, pointing to the errors and distortions which appear on practically every page of Pereltsvaig’s paper. Clearly, the reviewers of this paper glossed over much of its content, a fact which is highly disturbing, given Lingua’s commitment to serious scholarship.