PHILOSOPHICAL TRANSACTIONS B

royalsocietypublishing.org/rstb

Invited reply

Cite this article: Spengler, III R, Oliveira HRC. 2026 Invited reply: Response to: Towards a universal definition of 'domestication'. *Phil. Trans. R. Soc. B* **381**: 20260095.
https://doi.org/10.1098/rstb.2026.0095

Received: 9 March 2026
Accepted: 9 March 2026

Subject Areas:
evolution, genetics

Author for correspondence:
Robert Spengler, III
e-mail: junominerva@gmail.com

The accompanying Comment can be viewed at https://doi.org/10.1098/rstb.2026.0094.

Invited reply: Response to: Towards a universal definition of 'domestication'

Robert Spengler, III[1] and Hugo Rafael Cardoso Oliveira[2]

[1]1 Domestication and Anthropogenic Evolution Research Group, Max Planck Institute for Geoanthropology, Jena, Thuringia, Germany
[2]ICArEHB, University of Algarve, Faro, Portugal

RS,III, 0000-0002-5648-6930

Phil. Trans. R. Soc. B **380**: 20240188 (Published online 15 May 2025). (https://doi.org/10.1098/rstb.2024.0188)

Kosovsky *et al*. have responded to two recent attempts at unifying the scientific community under one definition for domestication—Spengler *et al*. [1] titled: *Seeking consensus on the domestication concept* and Lord *et al*.'s [2] recent paper, titled: *A universally applicable definition of domestication*. We will quickly comment on the main critiques raised by Kosovsky *et al*. [3], which seem focused more on the proposal from Lord *et al*. In both cases, these groups of scholars draw awareness to important issues, but ultimately, we read both of their discussions as demonstrations for the need to unify the multidisciplinary array of scholars currently studying domestication. Without a consensus on the definition, we are all trapped in a semantics skirmish that impedes scientific progress.

The main argument that Kosovsky *et al*. appear to be making is that 'domestication is too consequential an event in the evolution of humanity and the biosphere to be treated primarily as a matter of formal definition' [3, p. 1]. It is important in science that terms are defined, and so without a formal definition, it is unscientific. Setting aside this claim, their secondary argument appears to be that 'claims have emerged that domestication has little evolutionary relevance because it does not lead to speciation, or that it is discussed primarily due to human self-interest'. We are not sure who is making this claim; we would not classify the discussions provided by either Spengler *et al*. or Lord *et al*. this way. Spengler *et al*. open their paper with the statement that: 'Domestication is one of the most important biological phenomena to pull humanity into the culturally modern world' [1, p. 1].

As a quick note, the nod to Vernadsky's teleological Nösphere was an interesting choice. While a hero of the Soviet Union, Vernadsky acknowledged adopting his historicist views from the French priest, mystic, and scientist, de Chardin in 1920. Chardin's Nösphere was more overtly religious, but ultimately still culminating in a singularity for humanity. We direct the interested reader to Spengler [4], who has recently contextualized this topic.

The majority of Kosovsky *et al*.'s paper is a listing of facts about domestication. They discuss the area of agrarian land today, the duration of time that the early domestication process took, percentages of species domesticated, etc. They wrap up by enumerating genetic processes that can lead to domestication, and, of course, we agree with the diversity inherent in these genetic mechanisms—further illustrating that phenotypic expression must be key for defining domestication. While we could nitpick about details in their narrative, we see no fruitful outcome in doing so. Hence, we will spend the rest of our word limit responding to the Lord *et al*. proposal. Kosovsky *et al*. comment that the Spengler *et al*. definition 'effectively eliminates the distinction between domesticated and synanthropic species' [3, p. 1]. We find this statement intriguing and largely correct. Whether it is their intention or not, they do appear to be pitting Spengler *et al*. against Lord *et al*.; hence, we feel justified switching gears and focusing on what Lord *et al*. have proposed.

THE ROYAL SOCIETY PUBLISHING

1. Response to Lord *et al.*

We agree with the majority of what Lord *et al.* state, and we are aligned in the goal of constructing 'A Universally Applicable Definition', specifically one that 'avoids making assumptions about how domestication happens' [2, p. 1]. Further, Spengler *et al.* make the exact same critiques regarding definitions that focus on human utility and control. Despite the similarities in theirs and our attempt at defining domestication, we clearly follow diverging approaches. We find their constructed definition thought-provoking, and we can see its heuristic value; it is from the perspective of interest that we engage here.

Our main critique of their definition is its complexity. We find it hard to believe that many scholars are going to spend the time to learn their system. To maintain scientific utility, a definition, like a scholarly model, in a Popperian sense, must follow the principle of parsimony.

Lord *et al.* complain about a false dichotomy between artificial and natural selection, a point close to our heart, as Spengler [5] has recently published a discussion of exactly this. Nonetheless, we worry that Lord *et al.* fall victim to their own criticisms, as they are constructing a new definition for mutualism when it involves humans. They open by stating that they see domestication as resulting from a mutualism and claim that 'Domestication is simply an evolutionary process resulting from the interaction between two species, one of which is human' [2,p. 1]. Therefore, the processes they are describing are already well-defined by evolutionary ecologists as: not a mutualism, a facultative mutualism, or an obligate mutualism. Constructing new nomenclature when perfectly good ones already exist does more to obscure semantics than it does to clarify anything. Hence, they are constructing a false dichotomy between artificial and natural mutualisms.

They state: 'Validating the domestication syndrome requires first defining which populations are domestic, so that the syndrome traits can be compared between domestic and wild populations. Using the syndrome to define domestic populations presents a circular reasoning challenge' [2,p. 2]. For zooarchaeologists and archaeobotanists, identification of domestication traits by contrasting domesticates with progenitors is common practice. Consequently, Lord *et al.* who are primarily geneticists are discrediting our research, doing more to splinter the field than to unify it. In either case, their claim of circularity only applies because they reject the study of progenitors, disparagingly calling them an 'unobservable ancestor'. Taking their claim and applying the same reasoning to evolutionary ecology (and to reiterate, they claim that 'domestication is simply an evolutionary process') would suggest that we cannot study evolutionary change in the wild by using the palaeontological record. The study of 'unobservable ancestors' is foundational to evolutionary ecology. Also, behaviour, such as fear responses to humans, is part of the animal's phenotype; therefore, what Lord *et al.* are actually focusing on specific traits of the domestication syndrome.

They state that, 'In the absence of a clear definition for domestication, neologisms have proliferated' [2, p. 2]. Another point where we strongly agree with them, and one that we too made in our paper. They use 'semidomesticated' as their example of this shortcoming, which is the same case that we used. If everyone accepts that domestication is evolution, then there can be no semievolved species; evolution is unidirectional, and contrary to what Vernadsky may imply, nothing in life is teleological. They further, and correctly, state that 'these neologisms often carry an implied value' [2, p. 2]. Still, we contend that they are constructing novel neologisms themselves.

Every definition of synanthrope that we have found emphasizes the organism's 'undomesticated' or 'wild' status (actually, the word anthropophilic may be closer to the definition they wanted); either the authors are using the term erroneously, as a domesticated wild species simply does not make sense or they have constructed a novel definition in order to construct a novel definition. They have redefined synanthropic (removing the wild caveat) to mean something loosely similar to domesticated (a species that evolved into a mutualistic relationship with humans), and then they proceed to attach qualifiers to their new term, creating similar neologisms to the ones they have just critiqued. By discussing the degree of synanthropism, they are in effect categorizing the degree of domestication (or degrees of an evolved mutualism)—i.e. semidomesticated versus fully domesticated. Further, they have attached 'obligate' in front of their redefined synanthrope in the exact same way that most of the scholarly community would discuss an obligate domesticate (for a discussion see [5]).

Further complicating the situation, Lord *et al.* continue to use both words—domesticate and obligate synanthrope—interchangeably throughout their manuscript. If their argument was that 'some might question whether domestication remains a useful concept', as Marshall *et al.* [6, p. 6157] does, then we could sympathize with their frustration, but instead, they double down and construct multiple words for domesticated.

Spengler *et al.* and Lord *et al.* both present a plea to scholars to use the domestication term to refer to a population-scale process; domestication, like all evolution, works on a population not an organism. They then step away from evolutionary ecology, by claiming that all populations fall along a spectrum—we would never claim that humans or dandelions fall along a spectrum of speciosity. While the divisions may be, in some cases, arbitrary, taxonomists lump or split all variation within a population into a single clade. If most domesticated species fall along all or at least half of the nodes in their spectrum, is there really any utility? They seem to be trying to rectify this issue by stating that dogs reside at different rungs on their ladder of evolution, forming 'subpopulations' [2, p. 6], seemingly implying that some dogs are domesticated and others are not, but, as we note below, they counter this approach when discussing horses and pigs. Further, if populations can jump between categories, what service does this new definition or complex set of new definitions do to the study of domestication?

Lord *et al.* use cats as an example, claiming that cats only exist in anthropogenic contexts; when Spengler grew up, in upstate New York, he often saw cats deep in the forests. They were considered a serious threat to songbirds and ground-nesting grouse and acted as 'obligate recluses', running away upon approach. Hiking the same forests today, a feral cat would be a rare sight, as conservation initiatives have caused a boom in the coyote population. Applying the Lord *et al.* definition, the greater the conservation efforts, the more certain species become domesticated. As the coyotes were completely extirpated prior and relied on humans for their continued presence, we wonder where Lord *et al.* would place them. Are blight-resistant American chestnut

trees planted in forests in New England actually wild, despite being intentionally bred? As Lord *et al.* note, the destruction of habitats can make an organism 'domestic'; hence, what do we do with *Bergmansia*, *Ginkgo*, *Encephalartos* cycads, and a long list of other species that are extinct in the wild but could logically be re-established in the wild with conservation—presumably, they are currently domesticated but could become wild again? What do we do with species (especially island endemics) that are threatened by invasive predators but persist owing to proper conservation efforts to constrain the invasive species?

Another odd caveat, which appears to be an attempt to revert back to their claim of domestication being a population-level process, is that: if a 'subpopulation' is wild, then, in some cases, but not all, the whole clade is wild. They use the horse and pig as examples here, stating: 'In contrast to conventional wisdom, we classify horses as human exploiters and not domestic because horse populations are self-sustaining outside of the anthropogenic niche' [2, p. 3]. While the true 'unobservable ancestor' of the horse no longer exists in its wild range, feral horses do exist in parts of the world where megafaunal predators have been extirpated. Nonetheless, while cats can maintain feral populations, and in some cases even become invasive in parts of the world where predators have been hunted off the landscape, they remain ostracized to the obligate category; although cats seemingly span half of the spectrum in Lord *et al.*'s table, and wild tomatoes even earn two rows. Goldfish released in a pond with no pike are likely to maintain a stable population. Does this mean that all goldfish in the world are actually wild, or does the telescope goldfish constitute a subpopulation? If humans, by removing apex predators or megafaunal ecosystem engineers, can shift species along the spectrum, then, presumably, most species on Earth are already domesticated. If humans have the ability to denude most of the Earth's surface but choose to preserve remnant populations, then is most of life already domesticated, as life already relies on the human choice to preserve it?

Feralization, in their schema, is essentially de-domestication, as the process they are defining can revert backwards—further challenging the idea that 'Domestication is simply and evolutionary process', as evolution can only be unidirectional [2, p. 1].

Kohl [7] has already published a critique of Lord *et al.* and he beat us to one of the more peculiar caveats in their definition—the inclusion of bedbugs and rejection of body lice. Spengler has already discussed the bed bug and body lice topic in considerable detail in *Nature's greatest success* [5]. It is surprising to see that they draw a line between which kinds of anthropogenic environments count as having a domesticating ability (i.e. blankets versus clothing).

Another rather peculiar caveat that they add to their definition is that a species can evolve off of the scale; they use a broiler chicken as an example. They emphasize that once a species reaches an obligate state, it cannot become more domesticated, but then they add yet another rung onto their *scala domi*, claiming that a species falls off the ladder if they become so obligate they no longer breed without human aid.

To summarize, they have redefined domestication by taking a different word (synanthropic) and redefining that word to mean something similar to domestication, then added qualifiers to create a series of neologisms, which duplicate qualifiers already added to the word domestication. Further, they are discussing degrees of mutualistic relationships, but they choose not to use the nomenclature that evolutionary ecologists have established to discuss mutualisms. Then they add a series of illogical caveats to their system. It really just seems like a lot of unnecessary philosophizing to circle back around to a situation nearly identical to the one they are criticizing.

Despite the points of divergence, we are both aligned in our closing statements, as they say, 'if domestication is adaptation to an anthropogenic world, we may face a future devoid of wild populations' [2, p. 8], or as we concluded our paper, 'In the not-too-distant future, most of the life on Earth will be domesticated' [1, p. 11].

Ethics. This work did not require ethical approval from a human subject or animal welfare committee.
Data accessibility. This article has no additional data.
Declaration of AI use. We have not used AI-assisted technologies in creating this article.
Authors' contributions. R.S.: writing—original draft, writing—review and editing; H.R.C.O.: writing—original draft, writing—review and editing
Both authors gave final approval for publication and agreed to be held accountable for the work performed therein.
Conflict of interest declaration. We declare we have no competing interests.
Funding. No funding has been received for this article.

References

1. Spengler RN, Tang L, Dal Corso M, Gillis RE, Oliveira HR, Makhamad BM. 2025 Seeking consensus on the domestication concept. *Phil. Trans. R. Soc. B* **380**, 20240188. (doi:10.1098/rstb.2024.0188)
2. Lord KA, Larson G, Allaby RG, Karlsson EK. 2025 A universally applicable definition for domestication. *Proc. Natl Acad. Sci. USA* **122**, e2413207122. (doi:10.1073/pnas.2413207122)
3. Kosovsky GY, Glazko G, Glazko TT. 2026 A Comment on: Towards a universal definition of 'domestication' (2025), by Lord *et al*. *Phil. Trans. R. Soc. B* **381**, 20260094. (doi:10.1098/rstb.2026.0094)
4. Spengler R. 2026 Historical developments of Anthropocene concepts. *Quat. Int.* **761**, 110172. (doi:10.1016/j.quaint.2026.110172)
5. Spengler RN. 2025 *Nature's greatest success: how plants evolved to exploit humanity*. Berkeley, CA: University of California Press. (doi:10.1525/9780520405844)
6. Marshall FB, Dobney K, Denham T, Capriles JM. 2014 Evaluating the roles of directed breeding and gene flow in animal domestication. *Proc. Natl Acad. Sci. USA* **111**, 6153–6158. (doi:10.1073/pnas.1312984110)
7. Kohl PL. 2025 Domestication is adaptive evolution in response to human use. *Proc. Natl Acad. Sci. USA* **122**, e2413207122. (doi:10.1073/pnas.2518220122)