Dear Dr. Kruuk,

Thank you for reviewing our manuscript and for giving us the opportunity to address the points raised by you, Dr. Slocombe, and the two reviewers.

An overarching concern was whether our paper addresses fundamental biological principles. In this revision we clarify how our study is, indeed, rooted in biology. Our paper addresses the fundamentally biological question of whether the human aptitude for vocal imitation is important for explaining the evolution of language. We have substantially edited the introduction to more properly situate our work for the audience of Proceedings B. For example, we present language in the broader context of primate vocal communication, and note aspects of language that distinguishes it from these other forms of communication.

We also address each point made by the reviewers below.  
  
**Referee 1:**

*1. In the introduction the authors write (80-81): does the imitation of a particular water-splashing sound become, over generations of repeated  imitation, a better label for the more general category of water-splashing sounds? What does this mean in linguistic terms? Do the authors refer in any way to the iconicity of language?*

Indeed. This work is situated within the general topic of linguistic iconicity.

On the view that category labels are arbitrary, there is no sense in which one label is any better or worse than any other. We challenge this view by demonstrating that otherwise equal labels may be easier or harder to learn and generalize to new category members. Thus, in this experiment, our measure of what makes one label “better” than another is not derived from linguistic features, but from behavioral measures. We argue that labels that are learned faster and generalized to new category members more easily are de facto better category labels.

Our approach in looking to behavioral measures for determining how closely aligned a particular word form is with its potential meaning very much fits with current views on iconicity in language, as the reviewer notes. We now introduce iconicity earlier in the manuscript, including reference to iconicity in signed languages, and we pose our research questions as aiming to determine whether similar principles might apply in the formation of new spoken words.

*2. In “Collecting vocal imitations”, 111-112, “Participants were allowed to listen  
to each target sound multiple times” How many times were allowed to listen to each target? Was there a maximum number of repetitions allowed to each participant?*

There was no maximum number of repetitions allowed to each participant. The quoted text now reads:

Participants were allowed to listen to each target sound as many times as they wished, but were only allowed a single recording in response.

*3.  Figures in the text, please, the graphs must include the headings on the x and y axes*

We are sorry for this. There was an error in the conversion of our document into the online manuscript management system. A support request was submitted during our initial submission but was not addressed before the manuscript was sent out. Our submission now appears correctly online as it does offline.  
  
**Referee 2:**

*1. My first general issue concerns what these findings tell us. The paper shows that modern English-speaking humans show biases, that are present in their language, in the specific task. Namely, they can imitate sounds (and we have plenty examples of onomatopoaies in English as well across languages), and their imitations become more word-like (and there are plenty indications from previous studies using iterated learning - see point 2 below) that through generations language-like properties emerge in this paradigm. Thus, the findings are not surprising.*  
  
*Still, unsurprising findings can be informative about language evolution. Here, however, I also have a problem. In addition to the usual difficulty in making any sort of claim concerning language origin and evolution starting from modern humans (who already know a language), the present findings do not allow us to claim that imitation in vocalizations is a sine qua non, nor that they have been the first step. They do not falsify the possibility that gesture came first, nor that a combination of gesture and vocalization was key. They simply show that imitation in vocalization might have played a part. Thus, I find the argument proposed in the paper not to be fully justified on the basis of the findings.*

We take the Reviewer’s point that our hypothesis about the formation of conventional words from vocal imitations is not new. We do believe we are among the first to attempt to document the transition from vocal imitations to conventional words in a lab study. Our study is also, to our knowledge, the first to show that simple repeated imitation can produce better category labels.

We agree with the reviewer that these findings do not directly falsify other hypotheses about language evolution, namely the role of gesture. In light of these points, we have qualified our main argument as a specific test of the minimal conditions under which vocal imitations might give rise to conventional words. In the introduction, we describe our research question as follows:

Thus, converging evidence suggests that people can use vocal imitation as an effective means of communication. At the same time, vocal imitations are not words. If vocal imitation played a role in the origin of some spoken words, then it is necessary to identify the minimal conditions under which vocal imitations can give rise to more word-like vocalizations that can eventually be integrated into a vocabulary of a language. In the present set of studies we ask whether vocal imitations can transition to more word-like forms through sheer repetition — without an explicit intent to communicate. To answer this question, we recruited participants to play an online version of the children’s game of “Telephone”. …

We also included a concluding paragraph discussing the implication of our results for theories of language evolution that emphasize the role of gesture, leaving .

Notably, our hypothesis that vocal imitation may have played a role in the origin of some of the first spoken words does not preclude that gesture played an equal or more important role in establishing the first linguistic conventions (e.g. Fay, Arbib & Garrod, 2013; Goldin-Meadow, 2016; Kendon, 2016). What the present results make clear is that the transition from imitation to word can be a rapid and simple process: the mere act of repeated imitation can drive vocalizations to become more word-like in both form and function while still retaining some resemblance to the real world referents.

*2. There is by now an important tradition within language studies in using the method presented here in Experiment 1 (and multiple variations on this methods). This method is referred as iterated learning has been introduced by Simon Kirby and colleagues (Kirby et al., 2008). It is rather puzzling that the present paper does not make any reference to the previous papers, to the name of the paradigm and, importantly, to the learning mechanisms that have abundantly discussed in the literature as underscoring the processes simulated with the use of this paradigm. This, in my mind, is especially important. On line 419, they write “Our results show that through simple repetition...”, which seems to me to trivialise what is going on in the study. In fact, they don’t really address what is going on – what the learning mechanisms may be that give rise to rendering imitations more word-like through generations. It seems strange to me to use a very particular and well-discussed experimental paradigm and then not discuss at all why you used it or what the effects of it are.*

Not mentioning iterated learning—a literature that is quite familiar to us—was an oversight. We have included a new paragraph in the introduction that describes the relationship between our paradigm and previous uses of iterated learning paradigms in language evolution research. In brief, we are hesitant to call what we observed in our experiments as “learning” because imitators are unguided in their repetition of the acoustic messages, and thus no learning is required. However, what is similar between previous research on iterated transmission and the current study is that the constraints on faithful repetition of the signal are what drive the observed change. In this case, the aspects of an acoustic signal that cannot be repeated by subsequent generations will not survive the transmission to the next generation, leaving only the acoustic elements that are easier for different individuals to recreate. It is this tendency to move toward more reproducible forms that drives what was once an imitation of a specific acoustic event to become a more general representation of a family of related acoustic events.

*3. The authors call ‘imitative’ what, in the literature is more commonly referred as ‘iconic”. They do not define what they mean by ‘imitative’ nor whether ‘imitative’ is still a correct label to use for the word-like productions that they observe in later generations (where the vocalizations are indeed less imitative). This is an important point if we want to understand the mechanisms that underscore the effects found.*

We agree with the reviewer that it is important to be as precise as possible in our use of the terms ‘imitative and ‘iconic’. These terms overlap in meaning, but, as the reviewer notes, they are not entirely interchangeable. In general, we use the term ‘imitative’ (and derivatives) when referring to cases when a vocalization results from an attempt to produce an accurate, high-fidelity representation of a sound. For example, in our instructions, participants were asked to record themselves “imitating” the sound by “recreating it as accurately” as possible. In comparison, we use ‘iconicity’ to refer more broadly to resemblance between the form of a signal (e.g. a vocalization or gesture) and its referent or meaning. Words that bear a recognizable resemblance are ‘iconic’ (which includes words for non-sound concepts, like *teeny*). Iconic words may or may not have been created by an attempt to imitate a sound. In the revision, we aim to be careful with this distinction, and we discuss iconicity more explicitly, for example, on p. 3 referring to “a growing recognition of the importance of iconicity in spoken languages.

*4. I am unsure about the reasoning behind experiment 3. They argue that if vocalizations are more word-like, then they should more likely refer to categories rather than specific exemplars. I am always uneasy when talking about categorization levels as we do not know, really, whether the specific sounds within each category might already be categories standing for various tokens of the same type. That having been said, I know that some people have argued that iconicity is at the level of categories, not exemplars (e.g., Monagham, 2012). However, we also know that speakers may have more or fewer specific labels within a category (e.g., Eskimos have 10 different words for snow, English speakers only have one) depending upon whether it is important to differentiate or not. Hence one can argue that there is a bias toward category-level labels just because making finer distinctions is not so relevant to the task.*

We have clarified the rationale behind Experiment 3. Categorical reference is one of the key design features of language. All words (with the possible exception of proper nouns) denote categories, though the breadth of the category varies, as the reviewer points out. “Insect” denotes a broader category than “wasp”. “Liquid” denotes a broader category than “water”. But despite this, even highly specific words are categorical in that they abstract away from details of individual tokens/exemplars.

One way to think about the relationship between iconicity and categorical reference is that an iconic word-form evokes more specific exemplars while a word-form is highly effective for evoking a category (Edmiston & Lupyan, 2015; Lupyan & Thompson-Schill, 2012). This is what forms the basis for our prediction in Experiment 3. We reason that to the extent that the imitations are becoming more word-like, they should be abstracting more from the particulars of individual exemplars (e.g., a particular water splashing sound), and act as better labels for the category (e.g., the category of water splashing sounds).

With regard to Monaghan et al’s work on what they have referred to as systematicity or non-arbitrary links between phonology and lexical class—we note that these authors do not dispute the existence of non-arbitrary links between forms and meanings of the kind we are investigating here, e.g., (Dingemanse, Blasi, Lupyan, Christiansen, & Monaghan, 2015). That is, non-arbitrariness at the level of lexical-class-to-wordform does not preclude non-arbitrariness at the level of word-meaning -to- wordform.

*5. The different results for spoken and written labels that they got in Experiment 3 may be task related: in the spoken version, there were three questions, only 2 in the written version. This alternative account needs to be addressed.*

*6. Related to 5, the issue of differences in difficulty between questions in Experiment 3, I think, is not sufficiently addressed.*

We believe the Reviewer is referring to the results of Experiment 2 (Fig. 5), and the fact that the “True seed advantage” – the increase in performance when the true seed was present in the options – decreased over generations when matching imitations to seed sounds, but not when matching written transcriptions back to seed sounds.

We admit that the difference between the results for matching vocalizations to the environmental sounds and transcribed vocalizations to the original sounds is unclear. However, we do not believe the difference is due to the number of questions. The reason is that question type was assigned between-subject, and so each line in Fig. 5 comprises responses from an independent sample of observers. Because responses to each question are independent in both versions of the study, there is no way for the specific match questions to impact estimates of the other question accuracies. However, since we are fitting our results with hierarchical models, our estimates may still be influenced by the inclusion of the third question type. We now report a new analysis that compares the two versions of the experiment without the specific match questions. The decrease in the true seed advantage is unaffected by the exclusion.

We have also clarified our explanation of these findings based on the differences in question difficulty.