**General comments to editor and reviewers:**

Major changes:

1. The entire paper has been reorganized for clarity by subdividing the paper into sections related to the types of dynamics that can affect our ability to predict parallel adaptation after domestication bottlenecks/selection.
2. Table 1 and its accompanying information in the paper was removed in response to the comments of Reviewers 2 and 3.
3. Convergence/parallel clarified in response to Reviewers 1 and 3
4. Two figures added: one clarifying convergence and parallelism; the other defining different ways we can predict post-domestication parallel adaptation.
5. Emphasis has changed from “grasses” to “cereals”, a change influenced by the comments of Reviewer 2.

Minor changes:

1. Table 2 (now Table 1) reorganized based on the comments from Reviewer 1, and we now specify “parallel” or “unknown” in Column 8 in order to reflect the clarified definition of convergence/parallel

We also would like to emphasize that we didn’t create the main table from whole cloth but took it from Lenser and Thiessen, 2013 and adapted and reorganized it to include more genes, and added Columns 1, 5, and 8. Many of the genes from the original table were left off, and about a third of the genes in our table were added by us, including all the “adaptation” genes and Gdh7.

**Response to reviewers:**

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| Referee: 1  Comments to Author(s) Woodhouse and Hufford structure their review to contrast parallel and convergent adaptation resulting from domestication and adaptation and then discuss why and how these forces may have operated on the same or different genes. The core information for this discussion is summarized in a table (Table 2). I am not an expert in Maize or grasses more broadly and so cannot say whether this is comprehensive, but it seems they have done a useful service to the evolutionary community by combining this information into a single table.   This review is impressive in its scope and information content.  While in many respects this is a good thing, some simplification would make the review more digestible. In particular, there are some statements that seem a bit peripheral to the main points and some of these confuse or distract from the main points. I point to several of these in my specific comments below, but would also suggest that the authors read back through the manuscript to check whether there are other bits that could be simplified.  A comment to both authors and editor: I suspect that one of the greatest challenges of editing this issue is the need to have consistent definitions of parallel and convergent adaptation – or at least to make it clear how these terms are defined. The main definitions here seem to be that parallel adaptation is due to any change **in the** same gene and convergent adaptation is due to changes in different genes. Some other authors will likely have a different definition (as in Storz NRG, 2016), in which parallel adaptation (at the genetic level) is due to the same specific polymorphism, while convergent adaptation (at the genetic level) is due to different changes that occurred in either the same or different genes. At the very least, the definition used should be framed in the literature and it may be necessary to clarify that the definitions used here may differ from in other articles in the issue.  *Authors' response: Thank you for your favorable and helpful comments. We have taken them seriously and because of your feedback, we have clarified our definition of convergence and parallel in the manuscript, included a figure to help explain these definitions, and have reorganized the entire paper and included a figure demonstrating post-domestication adaptation and parallelism in the hopes the manuscript will be more clear.*  Specific comments:  Page 1 Lines 41-42: I would have thought that by definition ‘crops’ did not exist >10kya. I think this sentence should be rephrased.  *Authors' response: Edited to say "Certain species of edible plants have been continually selected by humans over the last 10,000 years for traits including nutrition, yield, and other attractive features, a process that has also dramatically changed the physiology of these plants into the crops we are familiar with today."*  Page 1 Line 56: comma after ‘orthologs’ should be removed.  *Authors' response: Done.*  This entire paragraph is a bit confusing. On the first reading I took from it that the authors were defining parallel evolution as functional changes occurring not only in the same gene but even in the same pathway. On further reading I am pretty sure they are defining it as same gene now**,** but the mention of orthologous pathways in the last sentence of this paragraph should probably be removed.  *Authors' response: We have clarified our definition of parallel evolution to include pathways too, included a figure, and reorganized the paper to put orthologs in a specific subcategory.*  Page 3 Lines 17-18: I would remove ‘and, to some extent…traits’ or clarify how this makes these species conducive to meaningful studies of parallel and convergent adaptation.  *Authors' response: We have removed that part.*   Page 3 Lines 42-43: suggesting an example of parallel selection  <- suggesting the possibility for parallel selection ??  *Authors' response: Edited to “suggesting the possibility for parallel selection.”*  Page 4 Lines 55-56: Sentence structure is problematic –  Move ‘we propose that’ to the beginning of the sentence. I.e., We propose that both demographic bottlenecks and selective…  *Authors' response: Done.*  Table 2: This table is difficult to read/interpret because the first column contains a mix of species and they are present in multiple disconnected rows. This table would be easier to follow if it were reorganized so that the first column was ‘Trait Type’ and the second was ‘Phenotype’.  Then, it could be cleaned up so that the table is broken into sub-sections based on Trait type and phenotype.  *Authors' response: Table edited as suggested, and "grasses" was replaced with "cereals" to reflect the new emphasis.*  Page 5, Lines 4-5: I am not familiar with this particular example, but it is not clear from the text whether the variation in agronomic traits associated with variation at Ghd7 is functionally the same as the loss of function variation described in Ghd7. Are both adaptive changes and domestication associated with loss of function? **MATT**  Page 5, Line 14: does Table 2 ‘match’ adaptive and domestication traits? … or collate them?  *Authors' response: "collate" is better. Edited as such.*  Referee: 2  Comments to Author(s) Categorizing traits in reviews on the domestication syndrome, particularly when addressing genetic parallelism versus convergence, brings up an important theoretical distinction: are the traits binned based on function or based on development? Thorns, spines, prickles, and awns arise from developmentally different pathways and tissues, and yet they are all involved in defense. Based on what we know about genetic cooption in evo-devo and neofunctionalization, the genetic underpinnings should represent a combination of function absent of common development and development absent of common function (for example of the latter, imagine constrain because of tissue type, or being above-ground versus below-ground, or only being able to arise from an axial bud.) The thoroughness of this review to cover so many genes is impressive, of value to the field of plant evolution, and deserves to go one step further in building sensible categories that don't mix function and anatomy/development as if they can't be teased apart, because now, clearly, they can. All these genes have been functionally characterized and so they can be placed into smarter categories that will actually do more to test the authors' questions.  *Authors' response: Thank you for your helpful comments and suggestions. We have taken your comments seriously, and toward that end we have removed Table 1 and introduced a new section and figure regarding the likelihood of parallelism based on specific characteristics*.  Line 24: "increased spikelet number" is a trait that could be found outside of the grasses if you consider what the spikelet is. It is just a reproductive branch that is more specific than inflorescence.  *Authors' response: In response to this comment and comments from another reviewer, we are removing Table 1 and its references from the paper.*  I cannot get past that Saccharum was not included in this review. It is a close relative of Sorghum and of course, a major grass. Many domestication genes are understood in Saccharum as well. It would change the results to include it, but omitting this genus biases results, particularly within the Poaceae, and that is what the entire review hangs on.  *Authors' response: This is true and valid, and it helps us to realize that we were strictly focusing on cereal grasses. We clarify this in the abstract and introduction (i.e. “We will focus mainly on* ***cereal*** *grass crops, since the major domesticates--maize, rice, sorghum, wheat, barley, and millet--include a range of divergence times conducive to adaptation and evolution of domestication syndrome traits through both parallelism and convergence.*  ***They also share a specific domestication phenotype whereby the end product is a millable grain.”),*** *and this will be reflected in the rest of the review*  Table 1. In grass crops, "Sexual to vegetative reproduction" should be Yes. The Saccharum domesticates are examples of this.  *Authors' response: Table 1 removed.*  Table 1. In grass crops, "Reduced defensive structures (spines, thorns)" should also be Yes for grasses. This should include the awn and thick prickles/trichomes on some rice that was reduced or eliminated during domestication. This occurred in both Asian and African rice and are well-known domestication genes like LABA1 (see Hua et al., 2015).  *Authors' response: Table 1 removed.*  Less critical but major points: Rice is oversimplified: there are two or three domesticates of rice, and only Asian rice is mentioned. African rice even has a genome and several studies have explored domestication genes within that species (Oryza glaberrima). The authors should make it more clear why only some domesticates were chosen.  *Authors' response: True. However, we focused on Asian rice since there is a lot more literature on Asian rice and studies of functional and candidate genes.*   Line 54 and 55: the order presented is not the sequence of domestication. What sequence are the authors presenting?  *Authors' response: : the authors are unclear by what is being asked, since page number hasn’t been clarified. If Reviewer 2 means Page 2, we have edited it to read* ***“****The grass clade is thought to have arisen around 75 MYA [10, 56],* ***eventually leading to the rice, wheat, barley, millet, maize, and sorghum lineages*** *(Figure 1)****.”***   The authors are not specific enough about gene choice. For example, Line 42, "the coloration gene BADH2 is found in both rice and soybean". Of course, this gene could be found in other species, too, but it has just not been characterized in all others. The authors could do better to test presence and then whether the trait changes in the other crops, or at least they should be more specific in their wording.  *Authors' response: It is true that we were focusing on genes that have been characterized. We have clarified it by emphasizing that these are characterized genes. The whole section, in addition, has been completely reorganized, and that paragraph is rewritten to focus first on putatively adaptive genes. The referenced section has been removed.*  The paragraph in lines 44-57 on page 3 is confusing. Do the authors agree with the four points by Lenser and Theissen? I don't think these are truths (I would actually disagree with the fourth point). I hope the authors can clarify their position and complete the thought that responds to the first sentence question of the paragraph. My dissatisfaction with this paragraph is also because the question asks about traits, and then the points are about genes, but they need to be reconnected to traits, as in, traits controlled by simple metabolic pathways.  *Authors' response: In response to Reviewer 2’s concerns, we have removed this paragraph and replaced it with a new section better describing the predictiveness of parallel traits***.**  Minor:   Line 19 of conclusions: shouldn't 'affects' be 'reduces'? Cant the authors make an assertion from their evidence amassed?  *Authors' response: Edited.*   extra comma in abstract. Line 31. genome size),  *Authors' response: Edited.*  Referee: 3  Comments to Author(s) Woodhouse and Hufford provide an examination of parallel and convergent evolution in crops, particularly grasses. They provide a nice table of putative events of parallel versus convergent evolution (Table 2), that is worth the price of admission. Overall, the manuscript is a little challenging to follow and in a few places, issues should be clarified. For example, the differences in convergent and parallel evolution could be drawn out more clearly in the Introduction. That said, there are no major issues with the text. Unfortunately, there are lots of minor issues that need to be addressed.  - Drawing a clearer distinction between parallel and convergent evolution in the Introduction would be helpful.  *Authors' response: Thank you for your comments. They were very helpful and we took them seriously enough to revise and clarify our definitions of convergence and parallel and included a figure to help illustrate these concepts.*  - The opening of the Introduction seems to restate the obvious. Of course, humans are largely dependent on crops. Without more specifics, this isn't particularly elucidating.  *Authors' response: We have removed that sentence.*  - When page 1 of the manuscript can't be incorporated into the review PDF, it is time to consider using something other than LaTeX for typesetting!  *Authors' response: We will ensure the pdf is correctly compiled.*  - Page 2, line 53 - The wording starting around here is a little odd. The lineages that gave rise to rice, wheat, barley, etc. may have arisen shortly after 75 MYA, but the species are almost certainly much younger.  *Authors' response: Rephrased to "The grass clade is thought to have arisen around 75 MYA [10, 55], eventually leading to the rice, wheat, barley, millet, maize, and sorghum lineages."*  - Page 3, line 25 - With regard to defensive structures, in barley and wheat, wild versions of both species often have long, barbed awns. The awns may primarily contribute to seed dispersal, but they also reduce predation. Cultivated barley and wheat both have reduced awns relative to some of the wild relatives and forage-types may have been selected to have no awns. Also, kernel row number does not seem fundamentally different from greater fruit or seed density found in other crops such as grape or pomegranate.  *Authors' response: In response to this comment and comments from another reviewer, we have removed Table 1 and its references from the paper.* |