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No. 159

DISCUSSION PAPER SERIES

Response to Fitzgerald (2024), “Does Innovation Mitigate Agricultural Damage from Climate Change?”

Jacob Moscona

Karthik Sastry

This paper responds to:

Fitzgerald, Jack. 2024. The Problems with Poor Pixies: Does Innovation Mitigate Agricultural Damage from Climate Change?. *I4R Discussion Paper Series* No. 158. Institute for Replication.

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Response to Fitzgerald (2024), “Does Innovation Mitigate Agricultural Damage from Climate Change?”

Jacob Moscona* and Karthik Sastry†

September 24, 2024

In this note, we address questions raised by [Fitzgerald \(2024\)](#) about Sections 5 and 6 of our article, [Moscona and Sastry \(2023\)](#). Before addressing specific points, we outline our main responses here:

- The first part of [Fitzgerald \(2024\)](#) investigates the replicability of our paper. All main results in our paper are correct and as intended; [Fitzgerald \(2024\)](#) replicates them exactly.
- The second part of [Fitzgerald \(2024\)](#) comments on the county-level proxy for exposure to climate-induced agricultural innovation used in the final part of our paper. However, the issues raised are not new: we describe them at length in our original manuscript and report a range of sensitivity analyses to address them. These tests, along with our theoretical justification and empirical rationale for the measure that we use, are ignored by the comment.
- The third part of [Fitzgerald \(2024\)](#) reports results from an alternative strategy for measuring county-level exposure to agricultural innovation. There are major shortcomings to the proposed strategy. The new estimates, as reported, are not easily interpretable for precisely the reasons that led us to use the approach developed in [Moscona and Sastry \(2023\)](#). Moreover, when variables are correctly transformed to logarithmic units, [Fitzgerald \(2024\)](#)’s own approach generates results entirely consistent with our original estimates for recent decades. These periods might be most relevant for understanding contemporaneous and future climate adaptation.

Despite all of this, the main conclusion that [Fitzgerald \(2024\)](#) draws from their analysis is exactly one of the conclusions of our original article. This leaves us wondering if the comment’s focus and extreme language are misplaced. [Fitzgerald \(2024\)](#) concludes from their analysis that we may want to question the notion that “market innovations [will] mitigate agricultural damage from climate change.” We wrote in the Conclusion (Section 7) of our original article that:

Even in the US, a country that has a comparatively large and wealthy agricultural sector and is a global leader in agricultural R&D, 80% of climate damage as we measure it has been unchecked by technology development ([Moscona and Sastry, 2023](#), p. 695).

An even larger share, close to 90%, is unabated in our out-of-sample estimates. Understanding how innovation may be harnessed to ease the burden of climate change is a very important question. We are excited by additional work in this area and do not see our paper as the end of the story.

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In the remainder of the note, we first discuss the replicability of the main results in [Moscona and Sastry \(2023\)](#) (Section 1), next describe our strategy of measuring innovation exposure and the extensive robustness checks included in the published article (Section 2), and finally discuss the comment's additional proposed analysis (Section 3).

1 Replicability of Main Results

The first part of [Fitzgerald \(2024\)](#) concerns the replicability of our results. We want to emphasize that that all results reported in the original publication are correct and as intended. The data and code provided with the replication materials are fully compliant with the requirements of the journal and there is zero evidence of error in data use or data cleaning.

Nevertheless, [Fitzgerald \(2024\)](#) refers to the replicability of the results as “partial.” We disagree with this characterization and believe it is highly misleading, so wanted to clarify. First, [Fitzgerald \(2024\)](#) claims that we use incorrectly coded quantile values in our construction of Figure 6 in [Moscona and Sastry \(2023\)](#). This point is incorrect. We compute quantile values based on the distribution of the relevant variables during the decades included in the regression sample. Moreover, when [Fitzgerald \(2024\)](#) uses alternative samples to construct the quantiles, the quantile values often differ by less than 0.001 and the results with the alternative quantile values are virtually identical to those in our original publication (see Figure R-I of [Fitzgerald, 2024](#)). Second, [Fitzgerald \(2024\)](#) notes that in two columns of one table (columns 6-7 of Table 3), the listed number of observations is 0.2% lower in the manuscript compared to the replication. This is true, driven by how the regression command we were using dealt with collinear observations when reporting the number of observations. It does not affect the estimates or their interpretation.

Thus, the main results from the original paper are all correct.¹ The claim that the paper is “partially replicable” on the basis of these comments, to us, highly misleading.

2 The Interpretation of Innovation Exposure

2.1 Our Approach

The last part of our original study measures whether access to new agricultural technology mitigates the economic damage of extreme heat to US farmers. As we observe in the paper, this is a difficult question to answer for two main reasons. First, the extent agricultural innovation is endogenous to various forces that affect potential market size, profitability, and technological possibilities. The endogeneity of innovation to these conditions is the central prediction of theories of directed technological change, including the theory that we propose in our paper. Second, not all agricultural innovations are created equal. Different classes of agricultural technology vary significantly in their potential for mitigating the consequences of adverse ecological conditions (e.g., [Hayami and Ruttan,](#)

¹We regret the small error uncovered in the revised comment in Appendix Table 20. We thank [Fitzgerald \(2024\)](#) for also pointing out that this does not affect the interpretation of any results.

1970; Ruttan and Hayami, 1984). We may thus expect different types of technology to respond differently to changing incentives induced by climate change (Section 2, Appendix E), and in the first part of our study we find strong empirical evidence for this (Section 4).

Our strategy to identify the downstream consequences of new technology, therefore, is to exploit a *shifter of climate-induced agricultural innovation* in the US, motivated both by our theoretical framework (Section 2) and crop and technology-level analysis of innovation (Section 4). This shifter is constructed using crop-specific national trends in heat stress, which are (i) plausibly independent from other changes in the agricultural market and from local agricultural outcomes and (ii) predictive of agricultural innovation, especially in areas conducive to ecological adaptation.

Specifically, to implement this strategy, we construct two variables at the level of US counties, indexed by i , and rounds of the agricultural census, indexed by t . The first variable measures *local damage* (i.e., local exposure to extreme heat): County-Level Extreme Exposure $_{i,t}$. Specifically, the variable is a weighted average of a primitive measure of location-by-crop-by-time heat stress, where the weights are pre-period shares of area planted for those crops. The second variable is our shifter of innovation induced by national trends in extreme heat: Innovation Exposure $_{i,t}$. We calculate this variable as a weighted average of crop-specific heat stress in *other parts of the United States*, where the weights are once more the pre-period shares of area planted for those crops. We document extensively in the main results of the paper that crop-specific innovation responds dramatically to this measure of national extreme heat stress, and that the technological response is concentrated in the development of new tools and techniques that might facilitate adaptation to climate change.

The hypothesis that we test is whether counties with more innovation exposure (i.e., counties whose crops have experienced more aggregate climate damage and hence climate-induced innovation) see a lower marginal sensitivity of agricultural outcomes (in our main analysis, agricultural land values per acre) to county-level extreme exposure. Specifically, we run the regression:

$$\begin{aligned} \log \text{AgrLandPrice}_{i,t} = & \delta_i + \alpha_{s(i),t} + \beta \cdot \text{Extreme Exposure}_{i,t} + \gamma \cdot \text{InnovationExposure}_{i,t} \\ & + \phi \cdot \left(\text{Extreme Exposure}_{i,t} \times \text{InnovationExposure}_{i,t} \right) + \epsilon_{i,t} \end{aligned} \quad (1)$$

where δ_i is a county fixed effect, $\alpha_{s(i),t}$ is a state-by-time fixed effect, and the key econometric test is whether $\phi > 0$. As we discuss in the paper, this is an appealing strategy in light of the identification and measurement challenges outlined above.

We also formalize this strategy via a theoretical model, which generates our estimating equations for induced innovation and for agricultural land values (Equation 1). The key result is described in Proposition 3 of Moscona and Sastry (2023).

Our model interpretation of the coefficient ϕ combines three margins: the pathway by which climatic trends affect technology, which in turn affects agricultural productivity, which in turn affects land values. If we wanted to identify the parameters governing *each* margin separately, it would be problematic that our approach does not “directly measure innovation” (Fitzgerald, 2024, p. 2). But, for our question at hand, we view this as a *strength*: ϕ is a sufficient statistic for a multiple-stage, general-equilibrium process of adaptation.

Finally, we explain in the paper that national climate trends could impact local agricultural outcomes through other conceivable channels. We use the model to precisely discuss some of these possible violations of the exclusion restriction, such as the effect via final goods prices. For others, we conduct empirical robustness checks. These extensive tests are described in Section 5 of our paper.

2.2 Addressing Comments in Fitzgerald (2024)

The first criticism of this approach raised by Fitzgerald (2024) is that local extreme heat exposure and our measure of innovation exposure are highly correlated “as both local and [aggregate extreme heat exposure] reflect, and are driven by, national and global climate trends” (p. 3). The observation that the two are correlated is not new to us. We designed several sensitivity checks described in Moscona and Sastry (2023) in order to address exactly this potential concern and invite anyone interested to read more about these in our article.

One check that we do is to control directly for the square of local extreme heat exposure in order to rule out the possibility that our estimate of ϕ is capturing higher order effects of local extreme heat exposure. Our results remain very similar after including these controls. This was not mentioned in the original version of Fitzgerald’s comment and only added after we pointed out the omission. Thus, the results do not simply capture the omitted effect of the square of extreme heat exposure, and this was addressed in our original paper.

Related to this idea, Fitzgerald (2024) presents additional non-parametric estimates of the relationship between our measure of local extreme heat exposure and local outcomes (Figure R-II) and notes that their results are “similar to the nonlinear relationships between temperatures and crop yields that Schlenker and Roberts (2009) find.” This comment reflects a misunderstanding of our measurement strategy. Our definition of extreme heat exposure *already measures incidence of temperatures above agronomically verified killing thresholds* in a way that is exactly consistent with Schlenker and Roberts (2009)’s measurement strategy and results. The visual similarity between Figure R-II and Figure 1 in Schlenker and Roberts (2009) is coincidental—they are measuring completely different things.

Another check reported in Moscona and Sastry (2023) is to re-calculate innovation exposure leaving out the entire state rather than just the individual county. By excluding a broad set of nearby counties from the calculation, this further reduces the likelihood that estimates of ϕ are driven by Fitzgerald (2024)’s concern. Reassuringly, the results are very similar after making this restriction.

Finally, Fitzgerald (2024)’s comment that “national and global climate trends” are mechanically related to local patterns of warming (see above) is incorrect. Patterns of warming have differed dramatically across US counties in recent decades, and agricultural counties have experienced very different levels of heat stress (see e.g., Burke and Emerick, 2016). A large body of work has exploited this variation for causal identification of local climate shocks. Some regions have even *cooled* in recent decades. The map reported in the Appendix to our paper (Figure A5a) reports this heterogeneity. Moreover, Figure A5b maps the variation in innovation exposure across counties, showing that it also has substantial regional heterogeneity that differs from heterogeneity in local extreme heat stress.

3 Additional Proposed Analysis

3.1 Problems with Interpreting [Fitzgerald's](#) Variety Exposure

[Fitzgerald \(2024\)](#) proposes alternative strategies to measure US counties' access to innovation. In particular, they propose measuring "Variety Exposure" as a sum, weighted by relative crop area, of the total released plant varieties and crop-specific agricultural patents, which we also measure in our study. They argue that having access to this "direct" measure of innovation is critical for ruling out other threats to interpretation.

However, there are many reasons to believe that the *opposite* is true: the Variety Exposure and Patent Exposure strategy are highly prone to alternative interpretations, rendering it very difficult to interpret [Fitzgerald \(2024\)](#)'s empirical results.

First, as argued above and substantiated in our empirical analysis, not all varieties or patents concern environmental adaptation; this makes these variables very imprecise measures of the conceptually relevant object. For some crops, the vast majority of innovative activity may have nothing to do with adaptation.

Second, the most important determinant of relative rates of innovation is market size. For example, 40% of our measured crop varieties (and 50% of our patents) correspond to the top five crops by each measure. This makes the cross-sectional variation exploited by [Fitzgerald \(2024\)](#) tantamount to simply identifying the largest crops. The analysis, therefore, in large part just identifies whether extreme heat exposure causes larger losses in counties that plant crops with large vs. small markets. That said, excluding these economically important crops from the analysis would be a mistake since these crops are also a very large share of crop area and agricultural output in the US. This set of issues is highly problematic for this approach.

Third, there is (unsurprisingly) large growth in numbers of varieties over time. There are 5.8 times more registered varieties in our data as of 2010 compared to 1960. This means that "Variety Exposure" is mechanically 5.8 times bigger in 2010 than in 1960. Thus, testing for heterogeneous effects as a function of "Variety Exposure" is quite similar to testing to an increasing or decreasing time trend in the effect of extreme heat on agricultural land values. As we describe both in the Introduction and Section 5 of our paper, it is *not* an appropriate strategy in the studied context. Moreover, it is fraught by low power and high potential for spurious correlation given the few time periods.

3.2 Consistent Results with Correctly Scaled Variety Exposure

We think the intellectually most valuable activity is to debate the merits of each modeling approach, and *not* to compare results for all conceivable regression models. The first version of this document, shared with the comment's author in response to the first draft of their comment, included no additional analysis of this sort.

Nonetheless, we think it is worth pointing out that [Fitzgerald's](#) own strategy yields results very similar to our original estimates for recent decades (since the 1980s) when both warming and innovation have picked up. We re-compute the "Variety Exposure" measure, except we use the log of

Table 1: Regression Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Outcome Variable is log Land Value per Acre								
	OLS	OLS	IV	OLS	OLS	IV	OLS	OLS	IV
Extreme Heat Exposure	-1.188	-1.183	-1.250	-0.413	-0.453	-0.671	-0.514	-0.791	-1.953
	(0.331)	(0.326)	(0.604)	(0.213)	(0.218)	(0.604)	(0.302)	(0.355)	(1.014)
Extreme Heat Exposure x log Variety Exposure	0.130	0.128	0.135	0.0555	0.0600	0.0941	0.0834	0.118	0.274
	(0.0365)	(0.0361)	(0.0677)	(0.0244)	(0.0251)	(0.0749)	(0.0350)	(0.0411)	(0.133)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	-	-	-	-	-	-
State x Year Fixed Effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	8,990	8,990	8,990	8,990	8,990	8,990	5,996	5,996	5,996
R-squared	0.961	0.961	0.005	0.978	0.978	-0.043	0.980	0.981	-0.073

Notes: The unit of observation is a county-year. Standard errors, double-clustered by county and state-year pair, are reported in parentheses. Columns 1-6 focus on the decades since 1980 and columns 7-9 focus on decades since 1990. The first stage F-statistics (Kleibergen-Paap) in columns 3, 6, and 9 are 26.52, 10.03, and 6.022 respectively. Additional controls include pre-period log variety exposure interacted with decade fixed effects.

varieties for each crop-decade (instead of the level) as an input in the weighted exposure measure. The use of logs is consistent with the analysis in the rest of the paper and with our theory (and common sense, when handling highly skewed data).² The estimates are reported in Table 1.

Columns 1-2 of Table 1 report estimates with county and decade fixed effects. In column 2, we add initial log variety exposure interacted with year fixed effects to control for trends in baseline variety exposure, which may be correlated with things like crop market size (see above). In both cases, the effect of extreme heat is negative while the effect of innovation exposure interacted with extreme heat is positive and significant. In column 3, we use an instrumental variables (IV) approach where we use our own innovation exposure measure and its interaction with extreme heat exposure as instruments – if anything, the coefficient estimate is larger.³ Columns 4-6 repeat the same estimates, except after including state-by-decade fixed effects, which exploits more localized variation in extreme heat (but also has some shortcomings; see Fisher et al., 2012). The story is similar. Finally, columns 7-9 focus on the even more recent period since 1990 and the results are even more pronounced.

3.3 Additional Comments

Fitzgerald (2024) makes a handful of smaller points that we do not respond to in detail, but are also worth mentioning. For example, they revisit our analysis of crop variety development and argue that the coefficients are attenuated after dropping a set of large crops that account for 60-70% of crop output value. While our baseline results remain positive and both statistically and economically significant (and our original analysis includes a range of sensitivity and graphical analysis to show that the findings are not driven by outliers or extreme observations; see e.g., Figures 3 and 5), we do not think it makes sense to consider arbitrary subsets of the analysis that exclude important crops and

²We handle the (very rare) zeros by using the $\log(1+x)$ transformation, where x is the relevant count of varieties.

³Fitzgerald (2024) also reports some IV specifications but these all suffer from a very low first stage F-statistic, which is puzzling given the strong relationship between aggregate crop-level extreme heat exposure and innovation documented extensively in Section 4 of the paper.

regions. In fact, most seminal work in this area focuses *only* on a small set of economically important crops (e.g., [Schlenker and Roberts, 2009](#); [Burke and Emerick, 2016](#)). We urge any interested readers to check our original paper for the many ways we replicated the results across samples, measures of innovation, measures of climate damage, specifications, etc.

3.4 Ideas for the Future

It is ultimately very challenging to empirically study adaptation to climate change. We believe our study takes many steps toward isolating a specific channel—the development of agricultural innovations—using detailed longitudinal data and direct measurement of induced innovation. But we also think that there is much more work to do. To give one speculative example: in our setting, we cannot collect farm-level data on adoption of specific agricultural technologies designed for climatic tolerance (e.g., heat-resistant seeds). One could use such information to calculate a very precise measure of technology use, in the spirit of the new analysis proposed by [Fitzgerald \(2024\)](#). One could then use these data to even better understand the farm-level effects of new technology availability and the mechanisms that underlie adaptation-via-technological-progress (or lack thereof).

We are excited about further work on the topic of climate adaptation, in agriculture and in other sectors, that furthers our understanding of one of the most pressing economic issues of our times.

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