The Problems With Poor Proxies: Does Innovation Mitigate Agricultural Damage from Climate Change?

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Abstract

Moscona & Sastry (2023; MS23) find that cropland values are less damaged by extreme heat when crops are more exposed to innovation. However, MS23's 'innovation exposure' variable does not measure innovation, instead proxying innovation using a measure of crops' national heat exposure. This proxy moderates extreme heat impacts for reasons unrelated to innovation. I construct direct innovation exposure measures from MS23's crop variety and patenting data. Replacing the proxy with these direct measures decreases MS23's mitigatory impact estimates by over 99.2%, erasing their significance. These results imply that market innovation has little capacity to mitigate agricultural climate change damage.

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1 Introduction

As global actors determine the best course of action to address the ongoing climate crisis, there is still significant uncertainty about the level of investment that should be dedicated to mitigating climate change. Mitigation pathways proposed by the Intergovernmental Panel on Climate Change suggest that global climate change mitigation costs could range from 1-7% of global GDP each year (Fujimori et al. 2023). Considerable time has been invested into providing governments and businesses with estimates of the optimal location of this investment spectrum on which to fall (see Auffhammer 2018). Optimal investments into climate change mitigation depend not just on the costs of climate change, but also on the mitigatory impacts of adaptations in practices and technology. In other words, the question of how much investment should be targeted towards directly reducing climate change itself will depend on the extent to which market innovations can protect people and assets from climate change's worst consequences. Resultantly, a large literature has focused on the mitigatory impacts of market innovations on damage from climate change (e.g., see Hornbeck 2012; Carter et al. 2018; Cui 2020; Aragón, Oteiza, & Rud 2021; Lai et al. 2022; Wang et al. 2024).

Moscona & Sastry (2023) – henceforth MS23 – offer a contribution to this literature. Using data on crops and American croplands from the 1950s to the 2010s, MS23 offer evidence in support of two findings. First, they find that agricultural markets endogenously innovate to adapt to climate change: crops whose croplands are more exposed to extreme heat see increased development of crop varieties and increases in climate-related patenting. Second, MS23 find that this innovation mitigates agricultural damage induced by climate change. MS23 construct a measure of 'innovation exposure' and estimate simple heterogeneous treatment effect models that regress agricultural land (AL) value on 'innovation exposure', extreme heat exposure (EHE), and the interaction between the two. These models yield negative coefficients for EHE, but positive coefficients for the interaction term between 'innovation exposure' and EHE. MS23 interpret this positive interaction effect coefficient to mean that AL is less severely devalued by EHE in counties that are more exposed to innovation. In fact, MS23 conclude that for sufficiently high 'innovation exposure', the marginal impact of EHE on AL values is not statistically significantly different from zero. Based on these results, MS23 project that technological innovation offset roughly 20% of potential EHE-driven devaluation of American croplands since 1960, and will offset 13% of such devaluation by 2100.

This paper critically re-examines MS23's findings due to a key flaw: MS23's 'innovation exposure' variable does not measure innovation. Using their first set of findings as justification, MS23 instead proxy county i's 'innovation exposure' at time t using the average EHE experienced by other counties at time t. MS23's 'innovation exposure' variable is thus a measure of heat, rather than innovation. I demonstrate this in a re-analysis of MS23's replication data, which shows that MS23's proxy is practically indistinguishable from local EHE. This is intuitive, as both local and 'leave-one-out' EHEs reflect climate trends on the regional, national, and global levels.

The positive coefficients on the interaction terms in MS23's models thus do not reflect mitigatory impacts of innovation on EHE-induced AL devaluation. Because MS23's heat proxy is practically identical to local EHE, interacting local EHE with MS23's proxy effectively yields a squared term in extreme heat. The positive coefficient on this interaction term reflects the fact that the negative marginal AL devaluation impacts of EHE diminish if counties are already exposed to more extreme heat. This is again intuitive. Though increases in EHE cause steep declines in crop yields near optimal crop-growing temperatures, if a county's heat has become so extreme that no crops can grow anyways, then additional increases in EHE will have little to no impact on AL values.

I show that MS23's interaction effect estimates primarily capture nonlinear effects of extreme heat on agricultural productivity. Estimates from a specification that simply models AL values as a second-order polynomial of local EHE yield qualitative conclusions that are nearly identical to those yielded by MS23's models. Additionally, one of MS23's critical robustness checks to rule out this possibility fails to replicate, and the model used for this check is in any case incorrectly specified. After correcting the specification error, I show that controlling for nonlinear effects in EHE eliminates the statistical significance of most of MS23's estimates of interest.

MS23 do not need a heat proxy to estimate the mitigatory impacts of innovation, as MS23 have data on several direct measures of innovation. MS23 obtain their first set of findings using direct data on crop variety development and climate-related patenting. I thus use MS23's replication data to construct direct measures of innovation exposure, specifically 'variety exposure' and 'patent exposure.' I then show that MS23's heat proxy is not positively related with these direct innovation measures. In fact, in the county-decade panel data where MS23's main results are obtained, the heat proxy is negatively correlated with both variety exposure and patent exposure.

After replacing the heat proxy with my direct innovation measures in MS23's models, I find no moderating effect estimate that is statistically significantly different from zero. The effect sizes of the moderating effect estimates in my replications are minuscule compared to MS23's estimates. The standardized coefficients of my moderating effect estimates decrease by over 99.2% compared to the standardized coefficients of MS23's estimates. These new estimates can be bounded within tight regions around zero.

I additionally pursue an instrumental variables strategy which instruments my direct innovation measures with MS23's heat proxy. Intuitively, if MS23's heat proxy is such a strong exogenous predictor of innovation that it can serve as a direct measure of innovation, then it should also be a suitable instrument for innovation. Instrumental variables estimates of innovation's mitigatory impact on EHE-driven AL devaluation are at least 43% less in standardized units than those estimates in MS23, and none are statistically significantly different from zero. This is in spite of exclusion restriction violations that likely bias my instrumental variables estimates in the direction of MS23's original estimates.

These mitigatory impact estimates for direct innovation measures remain robustly negligible across a wide range of specifications. These findings further imply that MS23's projections of historical and future climate change damage mitigation from innovation are greatly overstated. My replication thus ultimately casts doubt on the capacity of market innovations to mitigate agricultural damage induced by climate change.

This paper reflects, and is improved by, public and private discourse with the authors about their paper's findings. In particular, Moscona & Sastry (2024) – henceforth MS24 – respond to an earlier version of this paper (Fitzgerald 2024b) in a public reply. In addition to my main results, this paper incorporates and addresses details from MS24's public reply. In Online Appendices A and B, I respectively address their empirical and theoretical rebuttals directly.

Section 2 of this paper overviews MS23's main published estimates of interest. Section 3 details MS23's proxy and its inappropriateness as a measure of innovation. Section 4 introduces the direct innovation measures that I construct from MS23's replication data. Section 5 addresses MS23's and MS24's justifications for the heat proxy. Section 6 displays results after re-estimating MS23's models using my direct innovation measures. Section 7 concludes.

2 Data and Published Findings

My analyses rely on MS23's replication repository (Moscona & Sastry 2022). The repository lacks code for MS23's Online Appendix Tables A18 and A20. This leads to replication failures in MS23's appendix; I discuss this further in Section 5.4.

The main estimates of interest to my paper concern the mitigatory effects of market innovations on EHE-induced AL devaluation. MS23's estimates of these mitigatory impacts are found in Table III. Let i index the county and t index the decade. By Equation 18, MS23's relevant estimates for the mitigatory impacts of innovation arise from simple interaction models of the form

$$\log(\text{ALValue}_{i,t}) = \delta_i + \alpha_{s(i),t} + \beta \text{EHE}_{i,t} + \gamma \text{IE}_{i,t} + \phi\left(\text{EHE}_{i,t} \times \text{IE}_{i,t}\right) + \Gamma X'_{i,t} + \epsilon_{i,t}. \tag{E1}$$

Here $\log(\text{ALValue}_{i,t})$ represents logarithmic AL prices per cultivated land acre, δ_i are county fixed effects, $\alpha_{s(i),t}$ are state-by-year fixed effects, and $X'_{i,t}$ is a matrix of control variables. $\text{EHE}_{i,t}$ is local extreme heat exposure and $\text{IE}_{i,t}$ is 'innovation exposure' – I defer discussing the details of these variables to Section 3.

The model in Equation E1 is estimated on county-decade panel data, where counties are observed for $t \in \{1950, 1960, \cdots 2010\}$.[1] Table III shows results from two types of specifications. Models 1-5 are estimated using 'long-difference' (LD) specifications, which restrict time periods to $t \in \{1950, 2010\}$. Models 6-7 are estimated using panel specifications with no temporal restrictions.

MS23 interpret ϕ as innovation's mitigatory impact on climate change damage. Table 1 replicates MS23's $\hat{\beta}$ and $\hat{\phi}$ estimates, with standard errors (SEs) double-clustered at the county and state-decade levels.[2] Panel A juxtaposes the published results from MS23's Table III against the results from my reproduction in Panel B, confirming that MS23's repository permits a nearly exact reproduction of Table III.[3]

MS23 obtain significantly positive estimates for ϕ, and interpret this to mean that croplands that are more exposed to innovation experience less devaluation when exposed to extreme heat. To provide a sense of scale, Panels C-D of Table 1 convert the $\hat{\phi}$ and $\hat{\beta}$ estimates from Table III into two standardized effect size measures (see also Fitzgerald 2024a). Panel C converts the estimates into

1. To provide an example of indexing, $t = 1950$ implies that the observation covers all years between 1950-1959, inclusive of endpoints.

2. I primarily focus on SEs double-clustered at the county and state-decade levels rather than SEs clustered solely at the state-decade level because double-clustering appears to produce smaller SEs for most of MS23's models, and is thus more lenient for replication purposes.

3. The only differences between my reproductions and the published estimates in Table III are the observation counts in Models 6 and 7; MS23 report 0.2% fewer observations than I obtain in my computational reproductions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Published estimates							
County-level EHE	-0.851	-1.519	-0.825	-0.862	-0.786	-0.232	-0.39
	(0.211)	(0.24)	(0.203)	(0.238)	(0.226)	(0.107)	(0.132)
County-level EHE ×	0.249	0.425	0.237	0.251	0.23	0.0912	0.128
innovation exposure	(0.0757)	(0.0745)	(0.0728)	(0.0791)	(0.0762)	(0.0315)	(0.0321)
Panel B: Reproductions							
County-level EHE	-0.851	-1.519	-0.825	-0.862	-0.786	-0.232	-0.39
	(0.211)	(0.24)	(0.203)	(0.238)	(0.226)	(0.107)	(0.132)
County-level EHE ×	0.249	0.425	0.237	0.251	0.23	0.0912	0.128
innovation exposure	(0.0757)	(0.0745)	(0.0728)	(0.0791)	(0.0762)	(0.0315)	(0.0321)
Panel C: Partial correlations							
County-level EHE	-0.454	-0.854	-0.459	-0.401	-0.382	-0.119	-0.163
	(0.081)	(0.028)	(0.081)	(0.086)	(0.088)	(0.054)	(0.053)
County-level EHE ×	0.32	0.505	0.317	0.31	0.295	0.156	0.213
innovation exposure	(0.092)	(0.076)	(0.092)	(0.093)	(0.094)	(0.053)	(0.052)
Panel D: Standardized coefficients							
County-level EHE	-0.597	-1.066	-0.579	-0.605	-0.551	-0.212	-0.355
	(0.148)	(0.168)	(0.142)	(0.167)	(0.158)	(0.098)	(0.121)
County-level EHE ×	0.603	1.028	0.572	0.607	0.554	0.274	0.387
innovation exposure	(0.183)	(0.18)	(0.176)	(0.191)	(0.184)	(0.095)	(0.097)
Estimation type	LD	LD	LD	LD	LD	Panel	Panel
County fixed effects	X	X	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X	X	X
Weighted by agricultural land area		X					X
Output prices and interactions			X		X		
Avg. temp. (° C) and interactions				X	X		
Observations (published)	6000	6000	5990	6000	5990	20931	20931
Observations (reproduction)	6000	6000	5990	6000	5990	20966	20966
R^2	0.989	0.991	0.989	0.989	0.989	0.979	0.984

Note: The dependent variable in all models is logarithmic AL values. Estimates of the model in Equation E1 are presented alongside standard errors double-clustered at the county and state-decade levels in parentheses. 'Innovation exposure' is defined as in Equation E5.

Table 1: Reproduction of Table III

partial correlation coefficients r, and subsequently into SE(r), using the following representation (Stanley & Doucouliagos 2012):

$$r = \frac{t}{\sqrt{t^2 + df}} \qquad \mathrm{SE}(r) = \frac{1 - r^2}{\sqrt{df}}. \tag{E2}$$

Here t is the usual t-statistic and df is the model's residual degrees of freedom. The partial correlation coefficients of $\hat{\phi}$ in Table 1 range from 0.156 to 0.505. Partial correlations of this magnitude range from small to large amongst published effect sizes in economics (Doucouliagos 2011).

Panel D converts estimates into standardized coefficients σ. Let D be an independent variable and let Y be the dependent variable. Because all D and Y are continuous, I compute σ and SE(σ) using estimate $\hat{\tau} \in \left\{\hat{\phi}, \hat{\beta}\right\}$ using the formulas

$$\sigma = \frac{\hat{\tau}\sigma_D}{\sigma_Y} \qquad \qquad \mathrm{SE}(\sigma) = \frac{\mathrm{SE}\left(\hat{\tau}\right)\sigma_D}{\sigma_Y}, \tag{E3}$$

where σ_D and σ_Y are respectively the within-sample standard deviations of D and Y.[4] σ estimates for $\hat{\phi}$ range from 0.274 to 1.028 in Table 1, ranging from small to large in standardized effect sizes (see Cohen 1988). These standardized coefficients can be interpreted on the scale of 'standard deviation effects.' I.e., σ is the number of standard deviations σ_Y by which Y increases when D increases by one standard deviation (i.e., $1\sigma_D$).

3 The Innovation Exposure Proxy

Let k index a given crop. Per Equation 8, MS23 measure crop-level extreme heat exposure as $\mathrm{EHE}_{i,k,t}$, which is the number of growing degree days in county i above crop k's maximum optimal growing temperature in decade t.[5] By Equation 16, MS23 measure EHE at the county level as a weighted average of $\mathrm{EHE}_{i,k,t}$ across all crops planted in county i, where the heat exposure for crop

4. Computationally, this is done by re-running the regressions of interest after dividing Y by σ_Y and each D of interest by its respective σ_D.

5. See pg. 654 and MS23's Online Appendix D.1 for details. Temperature data is from Wolfram Schlenker's PRISM dataset, which is no longer available at the link provided in MS23. Updated daily temperature data is now hosted by Schlenker (2024), but this data's description implies that it differs slightly from the original PRISM data used in MS23. Each crop's maximum optimal growing temperature comes from the United Nations Food and Agriculture Organization's EcoCrop database. MS24 call these thresholds "agronomically verified killing thresholds" (pg. 6).

k is weighted by the proportion of land area in county i dedicated to planting crop k at baseline:

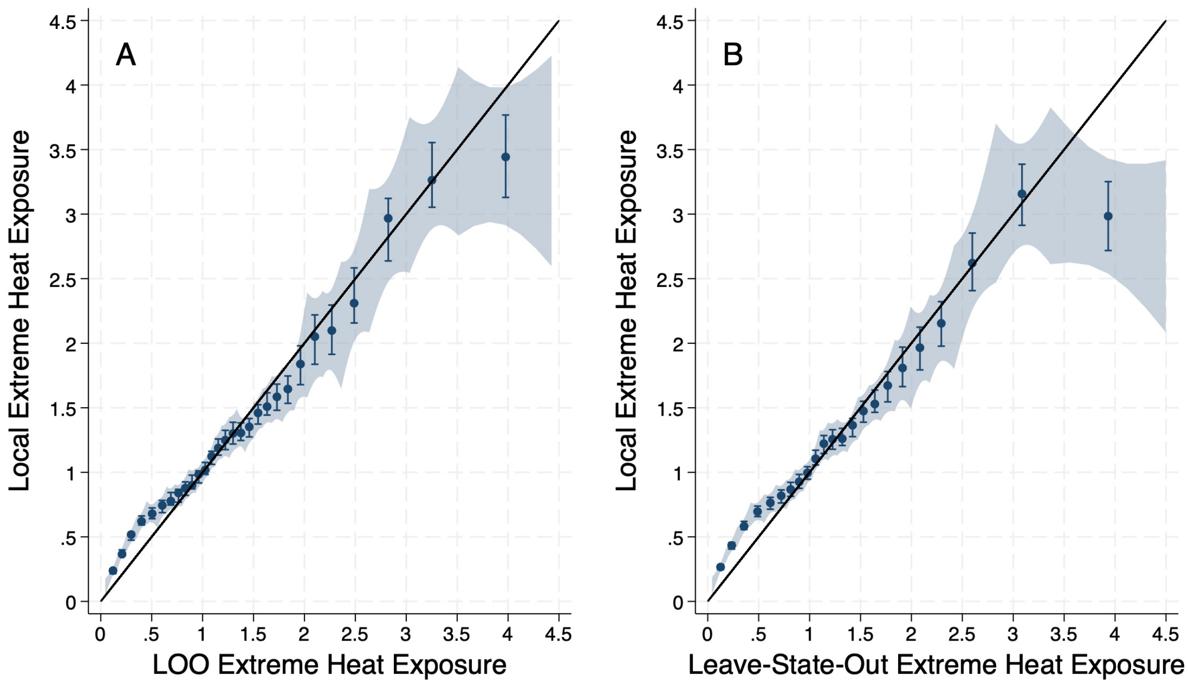
$$\text{EHE}_{i,t} = \sum_k \left[\frac{\text{EHE}_{i,k,t} \times \text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right]. \tag{E4}$$

By Equation 17, MS23 similarly measure 'innovation exposure' as an area-weighted average across counties in a given decade. However, instead of an area-weighted average of $\text{EHE}_{i,k,t}$, MS23 specify 'innovation exposure' variable $\text{IE}_{i,t}$ as an area-weighted average of *other counties' EHE*:

$$\text{IE}_{i,t} = \sum_k \left[\frac{\text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \sum_{j \neq i} \left[\frac{\text{EHE}_{j,k,t} \times \text{Area}^{\text{Pre}}_{j,k}}{\sum_{j \neq i} \text{Area}^{\text{Pre}}_{j,k}} \right] \right]. \tag{E5}$$

This 'innovation exposure' variable does not measure innovation; it measures heat. As MS23 write: "[We] calculate each county's innovation exposure as the average across all crops' national extreme-heat exposure... weighted by planted areas... We make only the small change of calculating this variable leaving out the county i to avoid any mechanical correlation" (pgs. 678-679). In fact, MS23 refer to this variable as 'leave-one-out' (LOO) EHE in their replication repository. I adopt this terminology to refer to MS23's innovation exposure proxy for the remainder of this paper.

LOO EHE is functionally identical to local EHE. The left-hand graph in Figure 1 (Panel A) plots the results of a binscatter regression between local EHE and LOO EHE, which shows that LOO EHE and local EHE positively move together in lockstep for the vast majority of their distributions. The slope of this relationship maps very closely onto a 45-degree line, which would indicate a perfect one-to-one unit relationship between county-level and LOO EHEs. This is an intuitive consequence of the fact that both county-level and LOO EHEs are driven by regional and national extreme heat shocks induced by global climate change. EHE is thus exogenously 'assigned' at a higher level than the county, and EHE in a given county's neighbors – even distant ones – are therefore often also reflective of EHE in that county.

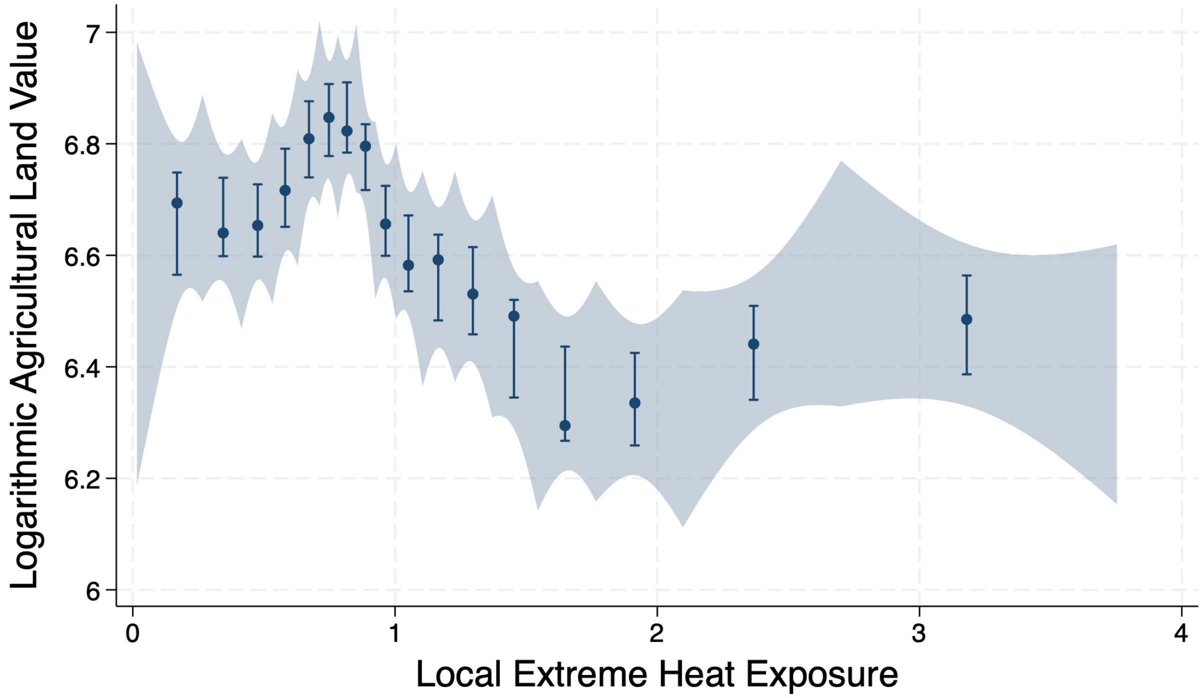


Note: The graph shows binscatter regression plots (see Cattaneo et al. 2024) showing the relationships between county-level EHE and both LOO EHE (left, Panel A) and leave-state-out EHE (right, Panel B). The ranges of both LOO and leave-state-out EHEs are restricted to $[0, 4.5]$ to improve interpretability; this range covers 99.7% and 99.5% of the distributions of LOO and leave-state-out EHEs (respectively). 95% confidence bands and intervals are constructed with standard errors clustered at the county level. A simple 45-degree line is appended to the graph for reference.

Figure 1: Nonparametric Relationships Between Extreme Heat Measures

This context completely changes the interpretation of ϕ in Equation E1, which is positive for reasons that have nothing to do with the damage mitigation impacts of directed innovation; specifically, $\hat{\phi}$ most likely reflects a nonlinear relationship between temperature and crop yields. Figure 2 displays the relationship that $\hat{\phi}$ is most likely capturing in Table III. In particular, Figure 2 presents a binscatter regression plot between county-level EHE and logarithmic AL values. The figure makes clearly visible that although local EHE decreases AL value across most of the distribution of $\text{EHE}_{i,t}$, this relationship diminishes on average as local EHE increases, and functionally flatlines for sufficiently high values of $\text{EHE}_{i,t}$.

Because LOO EHE very closely tracks county-level EHE, the interaction term in Equation E1 is effectively interacting $\text{EHE}_{i,t}$ with *itself*. As a result, Equation E1 effectively estimates logarithmic



Note: The graph shows a binscatter regression plot (see Cattaneo et al. 2024) showing the relationship between logarithmic AL values and county-level EHE for $\text{EHE}_{i,t} \in [0, 4]$; this restriction, instituted to improve interpretability, covers 95.3% of the distribution of $\text{EHE}_{i,t}$. 95% confidence bands and intervals are constructed with standard errors clustered at the county level.

Figure 2: Nonparametric Relationship Between AL Value and Local EHE

AL values as a function of a second-order polynomial in local EHE. $\hat{\phi}$'s sign in MS23's specifications is thus effectively the sign of the average second derivative over the function displayed in Figure 2. Therefore, MS23's positive $\hat{\phi}$ estimates largely reflect the deceleration of negative marginal EHE impacts as $\text{EHE}_{i,t}$ increases towards the upper tail of its distribution. In other words, $\hat{\phi}$ is most likely positive because a parabola fit to the points in Figure 2 faces upwards.

Such nonlinear dynamics between heat and AL value have been established in prior literature. For example, Schlenker & Roberts (2008; 2009) find that near maximum thresholds for optimal crop-growing temperatures, increases in temperature lead to steep declines in crop yields. However, when temperatures increase to sufficiently extreme highs, croplands can lose nearly all capacity for crop growth, so the negative impact of additional temperature increases on crop yields diminishes or even flatlines. In fact, the nonlinear relationship between county-level EHE and AL values displayed in Figure 2 is fairly similar to the nonlinear relationships between temperatures and crop yields that Schlenker & Roberts (2008; 2009) find for corn, soybeans, and cotton.[6]

I show that MS23's results are an artefact of *effectively* fitting a second-order polynomial in EHE by *explicitly* fitting a second-order polynomial in EHE. I.e., I estimate specifications akin to Equation E1, but replace the interaction between $\text{EHE}_{i,t}$ and LOO EHE with $\text{EHE}_{i,t}^2$:

$$\log(\text{ALValue})_{i,t} = \delta_i + \alpha_{s(i),t} + \theta_1 \text{EHE}_{i,t} + \theta_2 \text{EHE}_{i,t}^2 + \Gamma X_{i,t}' + \epsilon_{i,t}. \tag{E6}$$

In this specification, θ_1 and θ_2 are respectively akin to β and ϕ in Equation E1.

Table 2 displays the results from this second-order polynomial specification, which shows that fitting a second-order polynomial in county-level EHE yields results that are qualitatively nearly identical to MS23's results in Table III. Though Panels B-C in Table 2 show that the effect sizes of these estimates are smaller than those from Table III (see Panels C-D in Table 1), all seven models in Table 2 yield $\hat{\theta}_1$ and $\hat{\theta}_2$ estimates that respectively hold the same signs as the $\hat{\beta}$ and $\hat{\phi}$ estimates in Table III. These $\hat{\theta}_1$ and $\hat{\theta}_2$ estimates additionally yield the same statistical significance conclusions as the $\hat{\beta}$ and $\hat{\phi}$ estimates in Table III for six of seven models.

4 Direct Measures of Innovation Exposure

It is not necessary for MS23 to proxy innovation exposure with LOO EHE, as MS23 have data on multiple direct measures of innovation. Section IV of MS23 shows crop-level correlations between EHE and two forms of innovation. First, MS23 estimate the relationship between EHE on a given crop's croplands and the number of varieties developed for that crop. Repository dataset

6. MS24 object to this comparison, noting that Schlenker & Roberts (2008; 2009) are examining nonlinear relationships in raw temperature, whereas Figure 2 is displaying nonparametric relationships in EHE. However, these two heat measures are strongly positively correlated. Online Appendix Table C1 shows panel data regressions confirming that each 1000 additional crop-weighted growing degree days of EHE is associated with a 0.462-1.166 degree Celsius increase in average local temperatures. The t-statistics for these regression coefficients range from 28-58.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Raw coefficients							
County-level EHE	-0.898	-0.954	-0.866	-0.867	-0.761	-0.223	-0.169
	(0.16)	(0.243)	(0.162)	(0.19)	(0.179)	(0.09)	(0.116)
(County-level EHE)^2	0.058	0.061	0.063	0.068	0.064	0.02	0.01
	(0.018)	(0.028)	(0.016)	(0.016)	(0.016)	(0.008)	(0.01)
Panel B: Partial correlations							
County-level EHE	-0.701	-0.441	-0.656	-0.529	-0.486	-0.137	-0.08
	(0.052)	(0.083)	(0.058)	(0.074)	(0.078)	(0.054)	(0.054)
(County-level EHE)^2	0.316	0.219	0.37	0.396	0.372	0.131	0.053
	(0.092)	(0.098)	(0.089)	(0.087)	(0.088)	(0.054)	(0.054)
Panel C: Standardized coefficients							
County-level EHE	-0.63	-0.67	-0.607	-0.608	-0.533	-0.204	-0.154
	(0.113)	(0.17)	(0.114)	(0.133)	(0.125)	(0.082)	(0.105)
(County-level EHE)^2	0.271	0.288	0.298	0.32	0.302	0.116	0.06
	(0.083)	(0.132)	(0.077)	(0.076)	(0.077)	(0.048)	(0.061)
Estimation type	LD	LD	LD	LD	LD	Panel	Panel
County fixed effects	X	X	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X	X	X
Weighted by agricultural land area		X					X
Output prices and interactions			X		X		
Avg. temp. (° C) and interactions				X	X		
Observations	6000	6000	5990	6000	5990	20966	20966
R^2	0.988	0.99	0.989	0.989	0.989	0.979	0.984

Note: The dependent variable in all models is logarithmic AL values. Estimates of the model in Equation E6 are presented alongside standard errors double-clustered at the county and state-decade levels in parentheses.

Table 2: Fitting AL Values on a Second-Order Polynomial in Extreme Heat Exposure

`crop_level_data.dta` stores crop-decade panel data on $\text{NCrops}_{k,t}$, the number of crop varieties listed on the U.S. Department of Agriculture's *Variety Name List* for crop k in decade t. Second, MS23 estimate the relationship between EHE on a given crop's croplands and the number of patents associated with that crop that are related to climate change. `crop_level_data.dta` stores crop-level data on PatentsPre_k and PatentsPost_k, the counts of climate-related patents associated with crop k from before and after 1960 (respectively).[7]

I use these innovation variables to construct direct measures of innovation exposure in the

7. In `crop_level_data.dta`, PatentsPre_k and PatentsPost_k are respectively stored under variables `tot_1960_cc_USA` and `tot_1960_2020_USA_cc`.

county-decade panel data. First, I compute 'variety exposure':

$$\text{VarietyExposure}_{i,t} = \sum_k \left[\frac{\text{NCrops}_{k,t} \times \text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right]. \tag{E7}$$

This measure is constructed similarly to $\text{EHE}_{i,t}$, as it is an area-weighted average of crop variety (rather than EHE). The same is true of my second direct innovation measure, 'patent exposure':

$$\text{PatentExposure}_{i,t} = \begin{cases} \sum_k \left[\frac{\text{PatentsPre}_k \times \text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right] & \text{if } t = 1950 \\ \sum_k \left[\frac{\text{PatentsPost}_k \times \text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right] & \text{if } t = 2010 \end{cases}. \tag{E8}$$

Patent exposure is only defined for $t \in \{1950, 2010\}$, as MS23 only have patent data in these two time periods. It is thus only possible to replicate LD versions of Equation E1 using patent exposure.

5 Justifications for the Heat Proxy

Given that MS23 possess direct innovation measures that can be naturally extended to their panel data, why would they proxy innovation with a measure of heat? MS23 justify the LOO EHE proxy on four grounds. First, MS23 contend that EHE is a strong positive predictor of climate-adaptive innovation. Second, MS23 claim that computing the proxy in LOO fashion rids the proxy of correlations with local temperature. Third, MS23 show the robustness of their key estimates when proxying innovation exposure with a 'leave-*state*-out' EHE measure. Fourth and finally, MS23 claim that their results are robust to controlling for higher-order polynomials in EHE.

I address each of MS23's justifications for the LOO EHE proxy throughout the remainder of this section. In Section 5.1, I show that the positive relationships MS23 find between EHE and direct innovation measures are artefacts of over-aggregating the county-decade panel data to a nationwide crop-level cross-section. These relationships either flip signs or lose their statistical significance in the county-level panel data. In Section 5.2, I show that computing MS23's proxy in LOO fashion does not purge the proxy of correlations with local EHE. LOO EHE maps both one-to-one linearly, and unit elastically, with local EHE. In Section 5.3, I show similar results for MS23's leave-state-out proxy. Finally, in Section 5.4, I show that MS23's checks controlling for higher-order polynomials in local EHE do not replicate, and that the published check misleads readers about the impact of this control strategy on MS23's mitigatory impact estimates. I also find that their robustness check is misspecified. After correcting the misspecification, controlling for nonlinear functions of EHE erases the statistical significance of the majority of MS23's mitigatory impact estimates.

5.1 Heat as a Predictor of Climate-Adaptive Innovation

MS23 argue that their estimates in Section IV provide evidence that EHE positively predicts innovation.[8] To construct the measures used to obtain their results in Section IV, MS23 aggregate county-level EHE in the county-decade panel data up to a nationwide crop-decade EHE measure. They then generate 'long-difference' versions of this aggregated variable, subtracting nationwide crop-level EHEs in the 1950s from nationwide crop-level EHEs in the 2010s. MS23 construct similar long-difference, crop-level counts of crop varieties and associated climate-related patents. The results in Tables I and II are respectively produced by regressing crop-level, long-difference crop variety counts and climate-related patent counts on long-difference, crop-level EHE. MS23 obtain statistically significant positive estimates from these specifications.

These models disregard around 99% of the available variation in MS23's data. As discussed in Section 4, MS23 possess crop-decade panel data on $\text{NCrops}_{k,t}$ for 69 crops over seven decades, and have two decades of data on the number of climate-related patents associated with each crop. MS23's data on EHE and planting areas also varies at the county level, where there are 3004 counties with

8. From MS23, pg. 679: "This measure will allow us to investigate the role of endogenous technological progress because, as documented in the first part of the article, it is a strong predictor of innovation and hence the existence of new, climate-induced technology that can be used for production in county i."

non-missing data on $\text{VarietyExposure}_{i,t}$ and $\text{PatentExposure}_{i,t}$. However, MS23's models in Tables I and II do not utilize any of this temporal or county-level variation. By computing innovation and EHE at the crop level in 'long-difference' form, MS23 collapse their data into a cross-section, running their estimations with just 69 crop-level observations. This is down from around 6000 observations in the long-difference panel data used in Table 1, and down from over 20,000 observations in Table 1's full county-decade panel specifications.

Aggregating away nearly all variability in the data generates substantial risks of errors in conclusions. The low power of models fit to only 69 observations creates a serious risk of 'Type S' sign errors (Gelman & Carlin 2014). MS23 may also be committing an ecological fallacy by inferring conclusions about relationships in the disaggregated county-level panel data based on relationships in the aggregated crop-level cross-section (see Piantadosi, Byar, & Green 1988).

In the disaggregated county-decade panel data, the significant positive relationship between EHE and innovation either reverses or disappears. Table 3 shows linear estimates of the form

$$\text{DI}_{i,t} = \delta_i + \alpha_{s(i),t} + \lambda \text{LOO}_{i,t} + \Gamma X'_{i,t} + \epsilon_{i,t}, \tag{E9}$$

where $\text{DI}_{i,t}$ is either $\text{VarietyExposure}_{i,t}$ or $\text{PatentExposure}_{i,t}$, and $\text{LOO}_{i,t}$ is the LOO EHE proxy from Equation E5. This specification is an extension of the models used to produce MS23's Tables I and II to the panel data setting, with the caveat that in this specification, LOO EHE is the exposure variable of interest instead of local EHE. All 12 coefficients on LOO EHE in Table 3 are negative. If taken at face value, these estimates would imply that extreme heat is associated with *less* exposure to crop varieties and patents. However, these estimates are not particularly robust, varying both in size and statistical significance depending on the control specification. Notably, these estimates are much smaller, and are never statistically significantly different from zero, in models that control

	(1) Variety Exposure	(2) Variety Exposure	(3) Variety Exposure	(4) Variety Exposure	(5) Variety Exposure	(6) Variety Exposure	(7) Variety Exposure	(8) Patent Exposure	(9) Patent Exposure	(10) Patent Exposure	(11) Patent Exposure	(12) Patent Exposure
Panel A: Raw coefficients												
LOO EHE	-2650.489	-2394.693	-2529.699	-16.583	-533.706	-798.147	-327.686	-7221.465	-897.926	-5488.85	-2410.581	-2498.583
	(331.575)	(435.557)	(294.21)	(578.438)	(608.151)	(114.795)	(102.125)	(1114.11)	(1875.508)	(1143.973)	(1411.446)	(1555.538)
Panel B: Partial correlations												
LOO EHE	-0.147	-0.101	-0.159	-0.001	-0.016	-0.128	-0.059	-0.119	-0.009	-0.088	-0.031	-0.029
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Panel C: Standardized coefficients												
LOO EHE	-1.694	-1.531	-1.614	-0.011	-0.341	-0.649	-0.266	-1.559	-0.194	-1.183	-0.52	-0.539
	(0.212)	(0.278)	(0.188)	(0.37)	(0.388)	(0.093)	(0.083)	(0.241)	(0.405)	(0.247)	(0.305)	(0.335)
Estimation type	LD	LD	LD	LD	LD	Panel	Panel	LD	LD	LD	LD	LD
County fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
Weighted by agricultural land area		X					X		X			
Output prices and interactions			X		X					X		X
Avg. temp. (° C) and interactions				X	X						X	X
Observations	6004	6004	5994	6004	5994	21014	21014	6004	6004	5994	6004	5994
R^2	0.962	0.976	0.965	0.965	0.966	0.966	0.977	0.968	0.979	0.972	0.968	0.973

Note: The table shows estimates from a model of the form in Equation E9. The dependent variable of each model is either variety exposure or patent exposure, depending on the column. Standard errors clustered at the county level are presented in parentheses.

Table 3: Relationships Between LOO EHE and Direct Innovation Measures in the County-Decade Panel Data

for local average temperatures and their interactions with national temperatures.

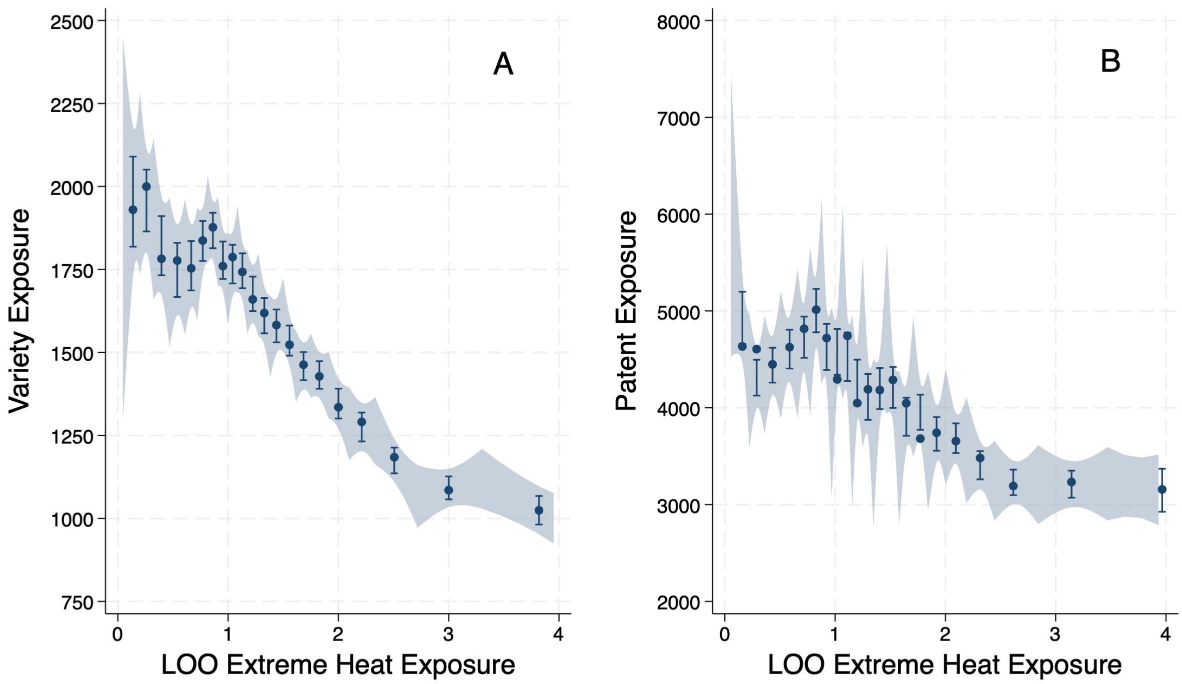
In Online Appendix Table C2, I replicate these specifications using models that are somewhat closer to those used to produce MS23's Tables I and II. Specifically, I use local EHE instead of LOO EHE as the exposure variable of interest, and I respect the fact that the original specifications for MS23's Tables I and II are Poisson models by using a pseudo-Poisson maximum likelihood estimator.[9] The majority of local EHE coefficients in Online Appendix Table C2 are not statistically significantly different from zero.

Figure 3 shows binscatter regression plots visualizing the nonparametric relationships between LOO EHE and both variety exposure and patent exposure in the county-decade panel data, which are clearly negative throughout the distribution of LOO EHE. At the very least, these results provide strong evidence that the relationship between extreme heat and innovation is not robustly positive in MS23's county-decade panel data. These results also provide some weak evidence that the relationship between EHE and innovation may be negative, implying that EHE may deter firms from making long-term investments into affected crops by decreasing those crops' potential yields.

5.2 Leave-One-Out Computation

Though MS23 posit that computing the heat proxy in LOO fashion "[purges] the measure of national crop-level damage driven by the county in question" (pg. 679), LOO computation does virtually nothing to rid MS23's proxy of correlations with local extreme heat shocks. Columns 1 and 2 in Online Appendix Table C3 show that LOO EHE and local EHE are effectively identical on average. A simple random effects panel data regression of county-level EHE on LOO EHE yields a coefficient of 0.994 (SE = 0.018), implying that on average, the two measures linearly map onto one another in nearly one-to-one fashion. Marginal effect post-estimation yields a constant elasticity estimate

9. I do not replicate Columns 2, 7, or 9 from Table 3 using the pseudo-Poisson maximum likelihood estimator, as `ppmlhdfe` in Stata cannot weight observations using analytic weights (see Correia, Guimarães, & Zylkin 2020).



Note: The graph displays binscatter regression plots (see Cattaneo et al. 2024) showing the relationships between LOO EHE and both variety exposure (in the left graph, Panel A) and patent exposure (in the right graph, Panel B) for $\text{LOO}_{i,t} \in [0, 4]$. This domain restriction covers 98.8% of the distribution of $\text{LOO}_{i,t}$. 95% confidence bands and intervals are constructed with standard errors clustered at the county level.

Figure 3: Nonparametric Relationships Between LOO EHE and Direct Innovation Measures

of 1.002 (SE = 0.023), implying that on average, the two measures are virtually unit elastic. LOO computation does not purge MS23's proxy of correlations with county-level EHE. LOO EHE and county-level EHE are practically identical.

5.3 An Alternative Leave-State-Out Proxy

MS23 attempt to address concerns about EHE assignment spillovers with an alternative specification that computes national EHE in leave-*state*-out fashion rather than LOO fashion. Intuitively, for county i in state s, leave-state-out EHE is an area-weighted average of $\text{EHE}_{i,k,t}$ for all $i \notin s$. MS23 store this variable by county and decade in dataset `county_shocks.csv` under variables `gdd_lso_1950`, `gdd_lso_1960`, and so forth.

Though MS23 contend that the results in their Online Appendix Table A20 show that their estimates of the mitigatory impact of 'innovation exposure' remain similar to those in Table III when using the leave-state-out proxy (pgs. 683-684), this robustness check attenuates all estimates, and attenuates some to a considerable degree. The moderating effect estimates of interest in the LD models of MS23's Online Appendix Table A20 are 20.2-25.1% smaller than these same estimates in Table III. In the panel data models, the moderating effect estimates of interest in MS23's Online Appendix Table A20 are 5.8-9% smaller than those estimates in Table III.

However, the biggest problem with the leave-state-out proxy is that leave-state-out computation still does not rid MS23's heat proxy of strong correlations with local EHE. The right-hand graph in Figure 1 (Panel B) shows a binscatter regression plot of the nonparametric relationship between local and leave-state out EHEs. The right-hand graph looks strikingly similar to the left-hand graph (Panel A), which plots the same relationship between local and LOO EHEs. Both relationships map closely onto the perfect unit relationship of a 45-degree line for the vast majority of their distributions. Columns 3-4 of Online Appendix Table C3 provide confirmatory results from panel data regression models. The coefficient from a simple random effects regression of local EHE on leave-state-out EHE is 0.92 (SE = 0.019), close to a one-to-one unit relationship. The post-estimated constant elasticity between local EHE and leave-state-out EHE is 0.886 (SE = 0.018), close to a unit elastic relationship. The relationship between local and leave-state-out EHEs is somewhat further away from linear one-to-one and unit-elastic than the relationship between local and LOO EHEs. This likely explains the attenuation of the mitigatory impact estimates in MS23's Online Appendix Table A20. However, between LOO and leave-state-out EHEs, the differences in these variables' relationships with local EHE are minor (see Section 5.2). Like LOO EHE, leave-state-out EHE is functionally identical to local EHE.

5.4 Robustness of the Main Specifications

MS23's main empirical argument that their proxy "is not capturing higher-order terms of county-level extreme-temperature exposure" is supported by MS23's Online Appendix Table A18, where they claim to achieve "very similar" mitigatory impact estimates after controlling for nonlinear functions of local EHE (pg. 683). MS23 do not provide replication code for their Online Appendix Table A18, but the table appears identical to Table III with the exception that all models in MS23's Online Appendix Table A18 control for $\text{EHE}_{i,t}^2$. The models in MS23's Online Appendix Table A18 are thus akin to Equation E1, taking the form

$$\begin{aligned}\log(\text{ALValue}_{i,t}) = \delta_i + \alpha_{s(i),t} + \beta_1 \text{EHE}_{i,t} + \beta_2 \text{EHE}_{i,t}^2 \\ + \gamma \text{IE}_{i,t} + \phi\left(\text{EHE}_{i,t} \times \text{IE}_{i,t}\right) + \Gamma X'_{i,t} + \epsilon_{i,t},\end{aligned} \tag{E10}$$

where ϕ remains the estimate of interest.

However, MS23's Online Appendix Table A18 fails to replicate, and this replication failure is important for MS23's claims. Online Appendix Table C4 juxtaposes the published version of MS23's Online Appendix Table A18 against my best attempt to replicate the table. The published version of MS23's Online Appendix Table A18 implies that after controlling for $\text{EHE}_{i,t}^2$, estimates of the mitigatory impact of 'innovation exposure' increase by 1.2-4.7% compared to Table III. Given that the initial estimates of interest are positive, this published robustness check would suggest that if anything, controlling for higher-order polynomials in EHE strengthens the evidence for the mitigatory impact of 'innovation exposure.' In contrast, my reproduction of MS23's Online Appendix Table A18 reveals that controlling for $\text{EHE}_{i,t}^2$ actually decreases six of MS23's seven mitigatory impact estimates by 15.4-35.5%. This control strategy thus considerably attenuates most of MS23's mitigatory impact estimates of interest.

Additionally, the model used for this check is incorrectly specified. The model in Equation

E1 produces moderating effect estimates by interacting $\text{EHE}_{i,t}$ with LOO EHE. Equation E10 augments Equation E1 by adding $\text{EHE}_{i,t}^2$ as a control variable, but then fails to specify the additional interaction between $\text{EHE}_{i,t}^2$ and LOO EHE that would be ordinarily expected in a model of this form. The model in Equation E10 is thus incorrectly saturated, in a manner akin to running a triple-differences model without its three-way interaction (see Olden & Møen 2022). Because of this misspecification, the coefficient on the interaction term in Equation E10 loses its intuitive econometric interpretation.

The average moderating effect of LOO EHE on EHE-induced AL devaluation can be appropriately obtained by adding an interaction term between $\text{EHE}_{i,t}^2$ and LOO EHE. Consider model

$$\begin{aligned}\log(\text{ALValue}_{i,t}) = \delta_i + \alpha_{s(i),t} + \beta_1\text{EHE}_{i,t} + \beta_2\text{EHE}_{i,t}^2 + \gamma\text{IE}_{i,t} \\ + \phi_1\left(\text{EHE}_{i,t} \times \text{IE}_{i,t}\right) + \phi_2\left(\text{EHE}_{i,t}^2 \times \text{IE}_{i,t}\right) + \Gamma X_{i,t}' + \epsilon_{i,t}.\end{aligned} \tag{E11}$$

Under a standard conditional unconfoundedness assumption (which MS23 also implicitly impose), the average moderating effect of LOO EHE on EHE-driven AL devaluation can be isolated from Equation E11 as follows:

$$\mathbb{E}\left[\frac{\partial^2\log(\text{ALValue}_{i,t})}{\partial\text{EHE}_{i,t}\partial\text{LOO}_{i,t}}\right] = \hat{\phi}_1 + 2\hat{\phi}_2\mathbb{E}\left[\text{EHE}_{i,t}\right]. \tag{E12}$$

$\mathbb{E}\left[\text{EHE}_{i,t}\right]$ can be simply computed as a within-sample mean of $\text{EHE}_{i,t}$.

These corrections show that MS23's estimates of LOO EHE's moderating effect on EHE-driven AL devaluation are not robust to controlling for squared local EHE. Table 4 displays average moderating effect estimates from Equation E12, computed from specifications of the form in Equation E11. All five of the average moderating effect estimates for the LD models in Table 4 are smaller than their respective estimates in Table III, decreasing by 11.5-54.9%. Four of these five LD esti-

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Average moderating effect of LOO EHE	0.139 (0.118)	0.376 (0.138)	0.114 (0.114)	0.118 (0.123)	0.103 (0.123)	0.15 (0.044)	0.221 (0.056)
Observations	6000	6000	5990	6000	5990	20966	20966
R^2	0.989	0.991	0.989	0.989	0.989	0.979	0.984
Estimation type	LD	LD	LD	LD	LD	Panel	Panel
County fixed effects	X	X	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X	X	X
Weighted by agricultural land area		X					X
Output prices and interactions			X		X		
Avg. temp. (° C) and interactions				X	X		

Note: Average moderating effects are calculated based on results from a model of the form in Equation E11 after running the formula in Equation E12 through Stata's `lincom` command. All models control for a second-order polynomial in local EHE and its interaction with LOO EHE. Standard errors double-clustered at the county and state-decade level are shown in parentheses.

Table 4: Average Moderating Effect of LOO EHE on EHE-Induced AL Devaluation, Controlling for Squared Local EHE

mates are not statistically significantly different from zero. The only estimates that 'benefit' from this control scheme are the panel data estimates, which increase by 64.1-72.4% compared to the respective estimates in Table III. These results show that MS23's estimates of the mitigatory impact of 'innovation exposure' are not robust to controlling for nonlinear functions of EHE. This is evidence that MS23's mitigatory impact estimates are heavily driven by a nonlinear relationship between EHE and AL value.

6 Results from Direct Innovation Measures

6.1 Reduced-Form Estimates

Replacing MS23's heat-based innovation proxy with direct measures of innovation exposure virtually eliminates the positive moderating effects found in Table 1. Table 5 shows the results of models estimating Equation E1, where LOO EHE is replaced with either $\text{VarietyExposure}_{i,t}$ or $\text{PatentExposure}_{i,t}$. Nine of the 12 moderating effect estimates in Table 5 are negative, and none

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Raw coefficients												
County-level EHE	-0.396773	-0.505613	-0.412652	-0.30271	-0.316944	-0.022859	-0.088524	-0.422081	-0.521739	-0.448565	-0.320757	-0.321508
	(0.110713)	(0.133273)	(0.106833)	(0.121208)	(0.122354)	(0.066345)	(0.096233)	(0.111302)	(0.138855)	(0.113341)	(0.119662)	(0.129294)
County-level EHE ×	-3e-06	5e-06	-1.9e-05	-1e-06	-2.7e-05	-6e-06	1e-06					
variety exposure	(1e-05)	(1.6e-05)	(2.4e-05)	(9e-06)	(2e-05)	(7e-06)	(1.1e-05)					
County-level EHE ×								-2e-06	0	-8e-06	-1e-06	-1e-05
patent exposure								(3e-06)	(5e-06)	(9e-06)	(3e-06)	(7e-06)
Panel B: Partial correlations												
County-level EHE	-0.395386	-0.42256	-0.431632	-0.265082	-0.275683	-0.018828	-0.050322	-0.42235	-0.417799	-0.444325	-0.286046	-0.263855
	(0.086559)	(0.084278)	(0.083483)	(0.095388)	(0.0948)	(0.054616)	(0.054497)	(0.084297)	(0.084689)	(0.082342)	(0.094203)	(0.095455)
County-level EHE ×	-0.03725	0.034496	-0.080371	-0.011724	-0.136521	-0.046505	0.004231					
variety exposure	(0.102455)	(0.102476)	(0.101935)	(0.102584)	(0.100686)	(0.054518)	(0.054635)					
County-level EHE ×								-0.074229	0.009925	-0.095897	-0.034209	-0.136185
patent exposure								(0.102033)	(0.102588)	(0.101654)	(0.102478)	(0.100695)
Panel C: Standardized coefficients												
County-level EHE	-0.278376	-0.354739	-0.289272	-0.212382	-0.22218	-0.02086	-0.080783	-0.296133	-0.366052	-0.314447	-0.225043	-0.225379
	(0.077677)	(0.093505)	(0.074891)	(0.08504)	(0.085771)	(0.060544)	(0.087818)	(0.07809)	(0.097421)	(0.079453)	(0.083955)	(0.090636)
County-level EHE ×	-0.005034	0.007819	-0.026864	-0.001519	-0.038664	-0.009438	0.001311					
variety exposure	(0.013875)	(0.023243)	(0.034404)	(0.01329)	(0.029326)	(0.0111)	(0.01693)					
County-level EHE ×								-0.008603	0.001926	-0.03437	-0.003654	-0.04258
patent exposure								(0.011924)	(0.019912)	(0.036941)	(0.010966)	(0.032374)
Estimation type	LD	LD	LD	LD	LD	Panel	Panel	LD	LD	LD	LD	LD
County fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
Weighted by agricultural land area		X					X		X			
Output prices and interactions			X		X					X		X
Avg. temp. (° C) and interactions				X	X						X	X
Observations	6000	6000	5990	6000	5990	20966	20966	6000	6000	5990	6000	5990
R^2	0.988	0.99	0.989	0.988	0.989	0.979	0.984	0.988	0.99	0.989	0.988	0.989

Note: The dependent variable in all models is logarithmic AL values. Estimates arise from the specification in Equation E1, where LOO EHE is replaced with a measure of innovation exposure (either VarietyExposure$_{i,t}$ or PatentExposure$_{i,t}$, depending on the column). Standard errors double-clustered at the county and state-decade levels are presented in parentheses.

Table 5: Reduced-Form Estimates of Innovation Exposure's Mitigatory Impacts

are statistically significantly different from zero.

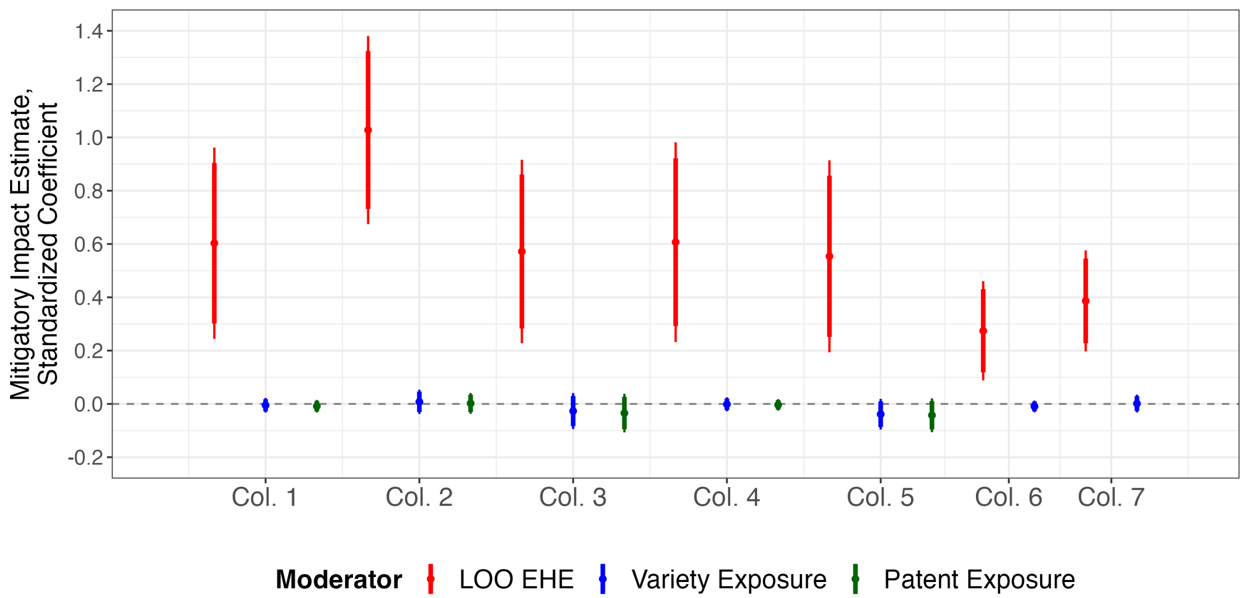
The moderating effect estimates in Table 5 are microscopic compared to those estimates in Table 1. This is not due to a difference in units. Though the partial correlation coefficients of the moderating effect estimates in Table 1, Panel C range from $0.156r$ to $0.505r$, those coefficients in Table 5, Panel B range from $-0.137r$ to $0.034r$. The standardized coefficient estimates of moderating effects in Table 5, Panel C are at least 99.2% less than the respective moderating effect estimates in Table 1, Panel D.[10]

Figure 4 visualizes the differences between the mitigatory impact estimates on LOO EHE and those on my direct innovation measures, showing that in contrast to the mitigatory impact estimates on LOO EHE, those estimates on variety exposure and patent exposure can be bounded within tight regions around zero. The confidence intervals shown in Figure 4 imply that at a 5% significance level, there is statistically significant evidence that the positive mitigatory impacts of both variety exposure and patent exposure are bounded beneath 0.047σ, and that all mitigatory impacts of variety exposure and patent exposure (whether positive or negative) are bounded beneath a size of 0.097σ (see Fitzgerald 2024a).

Figure 5 plots heterogeneous marginal effects of county-level EHE on AL values for selected quantiles of different moderating variables.[11] Figure 5's left graph (Panel A) replicates MS23's findings, showing that EHE's marginal effect on AL value attenuates toward zero for higher quantiles of LOO EHE. However, the middle and right graphs in Figure 5 (Panels B and C, respectively) show that the impact of EHE on AL values is virtually flat in variety exposure and patent exposure. If anything, the marginal AL devaluation impacts of EHE appear to grow more negative for higher quantiles of variety exposure and patent exposure, reflecting the negative interaction effect estimates

10. I focus on percentage comparisons of standardized coefficients rather than partial correlation coefficients because unlike standardized coefficients, partial correlation coefficients are not linearly comparable. E.g., $r = 0.015$ is not one tenth of $r = 0.15$ in the same way that $r = 1.5$ is not ten times $r = 0.15$.

11. These quantiles match those in MS23's Figure VI, which plots heterogeneous effects of local EHE on AL value by quantile of LOO EHE based on estimates from Model 1 of MS23's Table III.



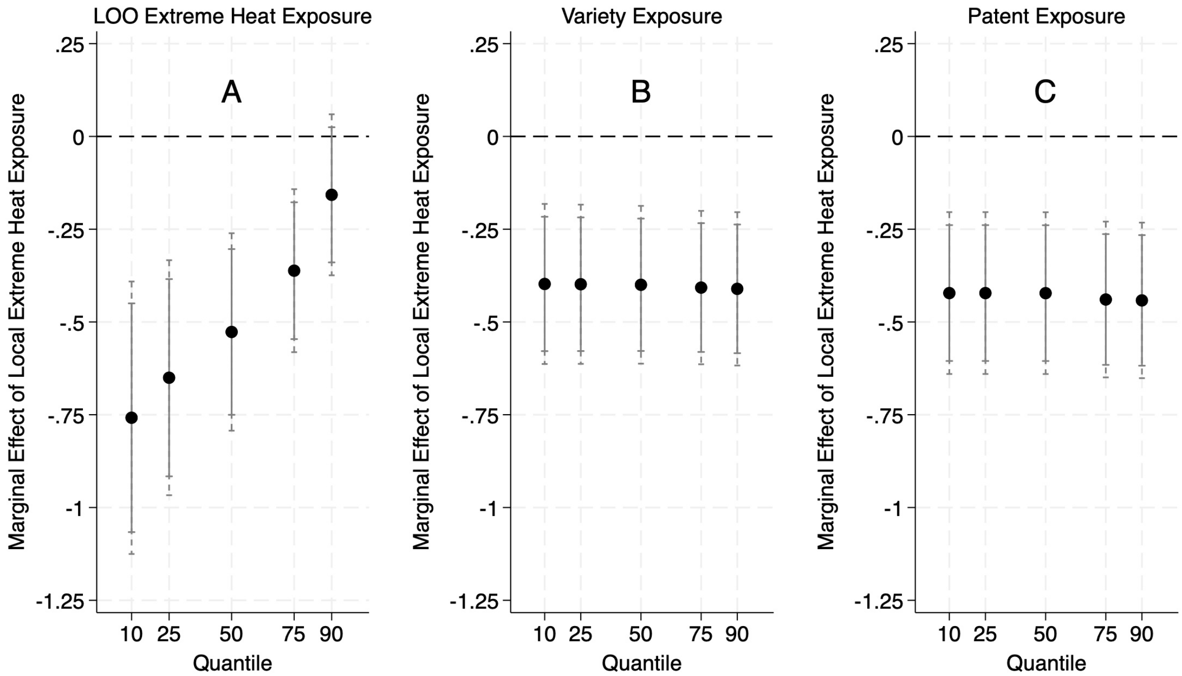
Note: Standardized coefficient estimates of the mitigatory impact of different moderating variables on EHE-driven AL devaluation are displayed alongside 90% and 95% confidence intervals (in thicker and thinner bands, respectively). Estimates for LOO EHE and variety exposure are from Columns 1-7 of Tables 1 and 5 (respectively), while estimates for patent exposure are from Columns 8-12 of Table 5.

Figure 4: Mitigatory Impact Estimates for Different Moderators

from Columns 1 and 8 in Table 5.

MS24 contend that their original conclusions in MS23 are supported by different models that use an augmented version of my direct variety exposure measure. In Online Appendix A, I show that MS24's specifications do not replicate. I also show that a wide variety of versions of the specifications they report running produce non-robust estimates of innovation's mitigatory impact on EHE-driven AL devaluation. These results imply that there is no robust support for the claim that innovation significantly mitigates EHE's negative impact on AL value.

MS24 also offer rebuttals to argue that the results arising from their LOO EHE proxy are more valid than those arising from my direct innovation measures. I address these arguments in Online Appendix B. Estimates of innovation's mitigatory impact on EHE-driven AL devaluation remain robustly negligible after conducting a wide range of checks that address MS24's rebuttals.



Note: The graphs display extrapolated marginal effects of county-level EHE on AL values for selected quantiles of different moderators (indicated in the titles of each graph), alongside 90% and 95% confidence intervals. The left, middle, and right graphs (Panels A, B, and C, respectively) are constructed by processing the raw coefficients from Column 1 in Panel B of Table 1, Column 1 in Panel A of Table 5, and Column 8 in Panel A of Table 5 (respectively) through the `margins` command in Stata.

Figure 5: Heterogeneous Treatment Effects of Extreme Heat Exposure

6.2 Instrumental Variables Estimates

To show that the results in Section 6.1 are not driven by the endogenous determination of my direct innovation measures, I estimate instrumental variables (IV) models of Equation E1. These IV models instrument my direct innovation exposure measures and their interactions with $\text{EHE}_{i,t}$ using a second-order polynomial of LOO EHE. This IV strategy reflects the intuition that if LOO EHE is a good exogenous predictor of innovation exposure, then it should also be a good instrument for innovation exposure. Because Table 3 shows that the linear relationships between LOO EHE and both variety exposure and patent exposure are negative in the county-decade panel data, this IV strategy relies on LOO EHE to produce exogenous *negative* shocks on innovation. These heat shocks could be causally interpreted as deterrents to future investment in crop innovation, as they reduce the profitability of long-term investments into crops that are becoming increasingly difficult to grow in progressively warming climates.

Considering this IV framework provides additional intuition for why LOO EHE is an inappropriate proxy for innovation exposure, as a clear exclusion restriction violation arises. LOO EHE is naturally expected to impact AL values through mechanisms other than innovation exposure. Specifically, as established in Section 5.2, LOO EHE will impact AL values through county-level EHE because LOO EHE reflects national and global climate trends that directly impact county-level EHE. Prior literature also establishes that using heat as an instrument is known to induce many potential exclusion restriction violations beyond this relatively simple case (see Mellon 2024).

These exclusion restriction violations are likely favorable for retaining MS23's original conclusions, as they likely bias the IV estimates upward. Section 5.2 shows that LOO EHE has a strong positive correlation with local EHE. Table 2 also shows that when local EHE is interacted with heat measures, the resulting interaction effect on AL value is positive. Therefore, after instrumenting the interaction between local EHE and variety/patent exposure with LOO EHE, the resulting interaction effect coefficient is likely biased upward, partially capturing the positive interaction effect between local and LOO EHEs on AL value.

Table 6 replicates Table 5 after instrumenting innovation exposure and its interaction with county-level EHE using a second-order polynomial in LOO EHE. Though the moderating effect estimates in Table 6 are generally larger than the respective estimates in Table 5, the first-stage F-statistics show that these estimates are not only biased by the aforementioned exclusion restriction violations, but also amplified in magnitude by the weakness of the instruments.[12] Kleibergen & Paap (2006) first-stage F-statistics in Table 6 range from 0.135 to 7.583.

None of the estimates in Table 6 provide clear support for a positive mitigatory impact of

12. As Lal et al. (2024) note, linear IV estimators are effectively ratio estimators, reflecting the ratio between reduced-form and first-stage estimates. When the first stage is weak, the denominator of the estimator can approach zero, causing the estimator's magnitude to explode. A similar intuition applies for multiple-instrument settings.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Raw coefficients												
County-level EHE	0.047514	-0.197302	-0.074475	-0.325701	0.122476	0.139509	-0.052389	-0.083581	-0.113446	-0.124031	-0.184512	-0.093768
	(0.251915)	(0.246389)	(0.18544)	(0.424943)	(0.38848)	(0.52994)	(0.719661)	(0.276747)	(1.075576)	(0.22589)	(0.284015)	(0.33166)
County-level EHE ×	2.4e-05	3.4e-05	2.4e-05	0.000121	0.000144	-1.3e-05	-3.3e-05					
variety exposure	(4.3e-05)	(6.4e-05)	(6.1e-05)	(8.1e-05)	(0.000125)	(0.000165)	(0.000354)					
County-level EHE ×								1.6e-05	0.000133	1e-05	2.4e-05	2.4e-05
patent exposure								(1.9e-05)	(0.00033)	(3.1e-05)	(1.3e-05)	(4e-05)
Panel B: Partial correlations												
County-level EHE	0.019348	-0.082436	-0.04124	-0.078881	0.032329	0.014382	-0.003977	-0.031001	-0.010822	-0.056423	-0.066802	-0.029019
	(0.102559)	(0.101901)	(0.102423)	(0.101959)	(0.102491)	(0.054625)	(0.054635)	(0.102499)	(0.102586)	(0.102271)	(0.10214)	(0.102511)
County-level EHE ×	0.05878	0.054889	0.041104	0.151895	0.117156	-0.00434	-0.005109					
variety exposure	(0.102243)	(0.102289)	(0.102424)	(0.100231)	(0.10119)	(0.054635)	(0.054634)					
County-level EHE ×								0.084632	0.041445	0.033939	0.181745	0.063076
patent exposure								(0.101863)	(0.102422)	(0.10248)	(0.099209)	(0.10219)
Panel C: Standardized coefficients												
County-level EHE	0.033336	-0.138427	-0.052207	-0.228512	0.085857	0.12731	-0.047808	-0.05864	-0.079593	-0.086946	-0.129454	-0.065732
	(0.176744)	(0.172867)	(0.129995)	(0.29814)	(0.272327)	(0.483599)	(0.656729)	(0.194166)	(0.754625)	(0.158351)	(0.199265)	(0.232496)
County-level EHE ×	0.035399	0.049305	0.03534	0.17487	0.2082	-0.02007	-0.050852					
variety exposure	(0.061681)	(0.092021)	(0.088136)	(0.116746)	(0.181073)	(0.25265)	(0.543816)					
County-level EHE ×								0.069929	0.578733	0.044281	0.10482	0.106438
patent exposure								(0.084469)	(1.431441)	(0.133785)	(0.058187)	(0.172784)
First-stage F	3.097	2.545	7.583	2.425	3.361	0.307	0.143	6.465	0.135	5.304	5.597	2.311
Estimation type	LD	LD	LD	LD	LD	Panel	Panel	LD	LD	LD	LD	LD
County fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
Weighted by agricultural land area		X					X		X			
Output prices and interactions			X		X					X		X
Avg. temp. (° C) and interactions				X	X						X	X
Observations	6000	6000	5990	6000	5990	20966	20966	6000	6000	5990	6000	5990

Note: The dependent variable in all models is logarithmic AL values. Estimates arise from the specification in Equation E1, where LOO EHE is replaced with a measure of innovation exposure (either $\text{VarietyExposure}_{i,t}$ or $\text{PatentExposure}_{i,t}$, depending on the column). Both variety/patent exposure and its interaction with $\text{EHE}_{i,t}$ are instrumented by a second-order polynomial of LOO EHE. Standard errors double-clustered at the county and state-decade levels are in parentheses. First-stage F-statistics are computed in accordance with Kleibergen & Paap (2006).

Table 6: Instrumental Variables Estimates of Innovation Exposure's Mitigatory Impacts

innovation exposure on climate-driven land devaluation. The moderating effect estimates in Table 6 are very noisy. The standard errors of the standardized moderating effect estimates in Panel C of Table 6 exceed those of the respective estimates in Panel C of Table 5 by at least 56%. Further, though the IV moderating effect estimates are somewhat greater than those reduced-form estimates in Table 5, the moderating effect estimates in Table 6 are still much smaller than those original estimates in MS23. Considering standardized coefficients, the moderating effect estimates in Table 6 decrease by at least 43% compared to their respective estimates in Table 1. As in Table 5, nine of the 12 IV estimates in Table 6 exhibit the opposite sign of their respective estimate in Table 1. None of the moderating effect estimates in Table 6 are statistically significantly different from zero.

7 Conclusion

This paper revisits and adjusts estimates of innovation's capacity to mitigate agricultural damage from climate change. I show that MS23's significant, positive estimates of agricultural innovation's mitigatory impact on EHE-induced AL devaluation are largely an artefact of an inappropriate proxy for innovation exposure. When I re-estimate MS23's models using newly-constructed direct measures of innovation, I obtain mitigatory effect estimates that are at least 99.2% less than those obtained by MS23, all of which are practically equal to zero. These estimates remain negligible in the face of a wide range of specifications and robustness checks. These results align with those of several prior studies that estimate similar mitigatory effects (e.g., see Hornbeck 2012; Aragón, Oteiza, & Rud 2021).

These findings change MS23's key conclusions about innovation's mitigatory impact on climate change damage. Using their significant mitigatory impact estimates, MS23 project that innovation mitigated one fifth of all potential climate-driven AL devaluation in American croplands since 1960, and that innovation will abate 13% of such devaluation by the end of the century. MS24 argue that this is a relatively small proportion of overall (potential) climate damage, but given that MS23 project that this 13% decline in climate change damage would yield \$1.05 trillion in savings by 2100, this is clearly an economically meaningful effect size. My main replications reduce the mitigatory impact estimates underlying these projections by over 99.2%, and erase these estimates' statistical significance.

My findings cast doubt on the capacity for agricultural innovations to effectively abate climate change damage. This is both due to the ineffectiveness in damage abatement that my estimates imply and due to the high cost of adapting agriculture to climate change. Offsetting predicted climate-driven losses in crop yield by 2050 through innovative adaptations would require \$187 billion to \$1.384 trillion in global public research spending (in 2005 \$PPP; see Baldos, Fuglie, & Hertel 2020). Public funds spent on such agricultural adaptations divert from resources that may be used to diminish climate change itself, including investments into clean energy and more energy-efficient technology. Thus in all probability, trusting technological innovation to mitigate the agricultural harms of climate change will incur astronomical explicit and implicit costs. My findings are therefore critically important for those seeking to compute – or decide – optimal investments for abating climate change.

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Online Appendix

A Robustness Checks With Logarithmic Measures

MS24 construct a 'logarithmic variety exposure' measure that uses area-weighted sums of *logarithmic* crop variety counts, rather than area-weighted sums of *linear* crop variety counts. They then fit altered versions of the models in Fitzgerald (2024), replacing my variety exposure measure with this logarithmic measure. Most of MS24's reported estimates for innovation's mitigatory impact on EHE-driven AL devaluation are statistically significant, and all are positive.

These methodological augmentations have several disadvantages. First, MS24 address the fact that some values of $\text{NCrop}_{k,t}$ are zeros by using a 'log-like' $\log(1+x)$ transformation. However, the magnitudes of coefficients from regressions that use variables transformed with these functions can be arbitrarily sensitive to linear rescalings of input x, and thus *per se* non-robust to specification choices (Chen & Roth 2024). Further, MS24 exclude all decades prior to 1990. This departs from the analytical choices in MS23's Table III, eliminating over half of the temporal domain and over 57% of observations.[1] Additionally, because MS24 make this temporal restriction, a log-like transformation is unnecessary. All $\text{NCrop}_{k,t}$ values from 1990 onwards are strictly positive, meaning that there is effectively no 'logs with zeros' issue to correct. MS24's new models thus unnecessarily incur robustness issues associated with log-like transformations.

MS24 provide no replication code for their new models, and many ambiguities in modelling choices and variable definitions make reproducibility challenging. Principally, MS24 write that they "handle the (very rare) zeros by using the $\log(1+x)$ transformation, where x is the relevant count of varieties" (Footnote 2). This could either imply that they transform *all* linear counts with the

1. This can be seen by comparing the observation counts in Online Appendix Tables C5-C8 to those in Columns 6-7 in Table 1.

$\log(1+x)$ transformation, creating variable

$$\text{LP1VarietyExposure}_{i,t} = \sum_k \left[\log\left(1 + \text{NCrop}_{k,t}\right) \times \frac{\text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right], \tag{A1}$$

or that they transform all nonzero values of $\text{NCrop}_{k,t}$ using the natural logarithm and *only* apply the $\log(1+x)$ transformation when $\text{NCrop}_{k,t} = 0$. Interestingly, because no $\text{NCrop}_{k,t} = 0$ for the temporal period analyzed in MS24, this latter methodological choice would actually create a genuine logarithmic variety exposure measure:

$$\text{LogVarietyExposure}_{i,t} = \sum_k \left[\log\left(\text{NCrop}_{k,t}\right) \times \frac{\text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right]. \tag{A2}$$

Further, MS24 additionally control for "pre-period log variety exposure interacted with decade fixed effects" (see Table 1 in MS24). There are four 'pre-period' decades prior to the start of MS24's augmented temporal window. It is not clear which (combination[s] of) decadal 'log variety exposure(s)' are interacted with the decade fixed effects. It is also unclear whether the main effect(s) of 'pre-period log variety exposure' are estimated alongside the aforementioned interactions.

I cannot replicate Table 1 in MS24, and in my best attempts to reproduce it, the majority of the mitigatory impact estimates of interest are not statistically significantly different from zero. Online Appendix Table C5 displays results from a replication attempt of Table 1 in MS24. Specifically, I run models of the form

$$\begin{aligned}\log(\text{ALValue}_{i,t}) = {} & \delta_i + \alpha_{s(i),t} + \beta \text{EHE}_{i,t} + \gamma \text{LP1VarietyExposure}_{i,t} \\ & + \phi(\text{EHE}_{i,t} \times \text{LP1VarietyExposure}_{i,t}) + \rho \text{LP1VarietyExposure}_{i,t_0-10} \\ & + \sum_{t'=t_0}^{2010} \left[\zeta_{t'} \mathbb{1}\left[t = t'\right] + \eta_{t'} \mathbb{1}\left[t = t'\right] \times \text{LP1VarietyExposure}_{i,t_0-10} \right] + \epsilon_{i,t}.\end{aligned} \tag{A3}$$

Here t_0 is the first decade of the temporal period (either 1990 or 2000, depending on the model) and

$t_0 - 10$ represents the last decade prior to t_0. The ζ and η terms in Equation A3 are respectively the coefficients on decadal fixed effects and their interactions with $\text{LP1VarietyExposure}_{i,t_0-10}$. Online Appendix Table C5 shows that estimates from this model are substantially weaker than those in Table 1 of MS24. First stage F-statistics in my IV models are much smaller than those reported in MS24's Table 1, and the majority of interaction effects between local EHE and $\log(1 + x)$-transformed variety exposure are not statistically significantly different from zero.

This replication failure is not driven by a misunderstanding of MS24's alternative variety exposure measure. Online Appendix Table C6 shows estimates from models of the form in Equation A3 that replace $\text{LP1VarietyExposure}_{i,t}$ with $\text{LogVarietyExposure}_{i,t}$. Estimates from this model are quite similar to those in Online Appendix Table C5, and the majority of interaction effects between local EHE and logarithmic variety exposure are not statistically significantly different from zero.

MS24's mitigatory impact estimates remain non-robust for reasonable alternative definitions of their control variables. Online Appendix Tables C7 and C8 report estimates from alternative models of Columns 2-3, 5-6, and 8-9 of Online Appendix Tables C5 and C6 (respectively) after altering the 'pre-period log variety exposure' measure. Specifically, in models of the form in Equation A3, I replace $\text{LP1VarietyExposure}_{i,t_0-10}$ with $\text{LP1VarietyExposure}_{i,t-10}$. Thus in the models used to produce the results displayed in Online Appendix Table C7 (C8), rather than controlling for county i's last pre-period value of $\text{LP1VarietyExposure}_{i,t}$ ($\text{LogVarietyExposure}_{i,t}$), I control for county i's first lag of $\text{LP1VarietyExposure}_{i,t}$ ($\text{LogVarietyExposure}_{i,t}$). The first-stage F-statistics in the IV models of Online Appendix Tables C7 and C8 remain much weaker than those in Table 1 of MS24, and the majority of estimates for innovation's mitigatory impact on EHE-driven AL devaluation remain statistically insignificant.

These reproduction attempts show that logarithmic variety exposure measures do not have robust mitigatory impacts on EHE-driven AL devaluation even if I cannot reproduce MS24's analysis exactly. Online Appendix Tables C5-C8 represent my best attempts to reproduce MS24's findings, using reasonable interpretations of their variable descriptions and modelling choices. The fact that my mitigatory impact estimates in Online Appendix Tables C5-C8 are not robustly significant implies at the very least that reasonable robustness checks on MS24's specifications do not support MS23's original conclusions concerning the positive mitigatory impacts of innovation exposure.

B Addressing MS24's Rebuttals

First, MS24 argue that my direct measures of innovation exposure are inappropriate for use in models estimating Equation E1 because these direct measures are endogenously determined. Indeed, MS23 find evidence that markets endogenously innovate to adapt to climate change. However, there are reasons to doubt this relationship too, as I detail in Section 5.1.

That said, for the purposes of estimating the mitigatory impacts of innovation in a model of the form in Equation E1, $\text{VarietyExposure}_{i,t}$ and $\text{PatentExposure}_{i,t}$ are *per se* less endogenous measures of innovation exposure than LOO EHE. To whatever extent LOO EHE reflects innovation exposure, the latent innovation exposure captured by LOO EHE is subject to the same endogenous data-generating process as the latent innovation exposure captured by direct measures of innovation. However, LOO heat exposure reflects *additional* endogeneity arising from its strong relationships with both local and national climate trends, as demonstrated in Sections 5.2 and 5.3. Replications of the model in Equation E1 that replace LOO EHE with $\text{VarietyExposure}_{i,t}$ or $\text{PatentExposure}_{i,t}$ thus provide less biased estimates of the mitigatory impacts of innovation on climate-driven AL devaluation. Section 6.2 also reports IV specifications that exploit whatever exogenous variation LOO EHE induces in innovation. The results from these IV models do not qualitatively differ from the results arising from models that omit the IV strategy.

Second, MS24 argue that their LOO EHE proxy is a better variable for capturing their causal estimand of interest: "the pathway by which climatic trends affect technology, which in turn affects agricultural productivity, which in turn affects land values" (pg. 5). MS24 contend that the estimand of interest is the mitigatory impact of *directed* technological changes. I.e., MS24 are interested in mitigatory impacts of crop innovation that arises *specifically* because of extreme heat, and are not interested in the impacts of innovation that arises for other reasons.

LOO EHE is useful for targeting this causal estimand, but not in the models that MS23 estimate. The mitigatory impact that MS24 argue they are targeting is a local average treatment effect – the mitigatory impact of innovation specifically for counties that saw changes in innovation exposure because of extreme heat. The ordinary least squares models in MS23's Table III do not estimate this local effect. Instead, these models estimate the global interaction effect between LOO and local EHEs on AL value. This effect only partially channels through innovation. MS23's estimates additionally reflect other channels through which LOO EHE moderates EHE-driven AL devaluation, such as nonlinearities in the relationship between local EHE and AL value (see Sections 3 and 5).

The local average treatment effect discussed in MS24 is best estimated using IV models that instrument innovation with LOO EHE. Notwithstanding the weak instruments and exclusion restriction violations detailed in Section 6.2, these IV models are ideal for capturing the local average treatment effect of innovation specifically on counties that innovated more because of extreme heat (see Imbens & Angrist 1994). I directly pursue this IV strategy in Section 6.2. None of the IV estimates are precise nor statistically significantly different from zero.

Third, MS24 argue that my direct innovation measures may imprecisely capture *climate-adaptive* innovation. Not all crop varieties and crop-related patents are developed to adapt to climate change. If a substantial proportion of the crop varieties and patents in MS23's data are unrelated to climate adaptation, then it would be unsurprising for my replications to reveal negligible mitigatory impacts of variety exposure and patent exposure on EHE-driven AL devaluation.

This concern is partially allayed by MS23's data. MS23 already split patent counts into climate-related and climate-unrelated patents. My patent exposure measure from Section 4 only uses data on climate-related patents, which should in principle precisely capture climate-adaptive innovation. If this is not the case, then this poses a problem for the quality of MS23's data, and casts further doubt on MS23's findings that EHE drives climate-adaptive innovation.

My IV strategy also addresses this concern. Even if $\text{VarietyExposure}_{i,t}$ and/or $\text{PatentExposure}_{i,t}$ imprecisely capture climate-adaptive innovation, IV models that instrument these direct innovation measures with LOO EHE only exploit variation in variety/patent exposure that can be directly traced back to exogenous climate shocks. These IV models in Section 6.2 and the reduced-form estimates in Section 6.1 yield qualitatively identical results.

Fourth, MS24 argue that most variation in my direct innovation measures is driven by a handful of profitable crops. By the end of the 2010s, 37.8% of the crop varieties in MS23's data belong to the top five crops with the most crop varieties. Further, 50% of the climate-related patents in MS23's data are related to the top five crops with the most climate-related patents. If my results from Sections 6.1 and 6.2 are effectively just identifying the most-innovated crops, then this may pose a 'selection on returns' issue – these crops may be more innovated upon precisely because innovations on these crops are known to be better at preserving the value of these crops.[2]

I address this concern by repeating my analyses in Section 6.1 using versions of my direct innovation measures that exclude innovation from the most heavily-innovated crops. Specifically, I construct alternative versions of $\text{VarietyExposure}_{i,t}$ and $\text{PatentExposure}_{i,t}$ after omitting the top five crops by total number of crop varieties or climate-related patents (respectively) in the 2010s.[3] In Online Appendix Tables C9 and C10, I replicate Table 5 after replacing $\text{VarietyExposure}_{i,t}$ and

2. Note that such selection would imply that my mitigatory impact estimates are biased *upward*, which would be favorable for maintaining MS23's original conclusions.

3. Corn and soybeans are on both 'top five' lists. The remaining crops that are 'top five' in varieties include lettuce/romaine, tomatoes, and wheat, whereas the remaining crops that are 'top five' in patents include alfalfa (and varieties thereof), barley, and tobacco.

$\text{PatentExposure}_{i,t}$ (respectively) with these alternate measures. This check decreases my mitigatory impact estimates to the point that all mitigatory impact estimates in Online Appendix Table C9, and four of the five mitigatory impact estimates in Online Appendix Table C10, are negative. None of these mitigatory impact estimates are statistically significantly different from zero.

This concern is also addressed by my IV strategy. The variation that produces the IV estimates in Section 6.2 can be entirely tied back to the average innovation response to EHE *across the entire crop market*, including crops for whom innovation may be less successful at retaining crop value. This greatly mitigates the concern that the IV estimates are driven by a handful of crops or by selection on returns. As aforementioned, the IV results in Section 6.2 are not qualitatively different from the reduced-form results in Section 6.1.

Fifth and finally, MS24 posit that mitigatory impact estimates for my direct innovation measures are confounded by a joint time trend between AL value and innovation. From the 1950s to the 2010s, the median county-level AL value in MS23's data jumped from 4.7 to 8.123 log points. Likewise, over the same timeframe, median county-level crop variety exposure increased by over 673%, and median county-level patent exposure increased by several orders of magnitude. This raises the possibility that my estimates from Sections 6.1 and 6.2 are effectively reflecting an increasing time trend that is common to both AL value and innovation. That said, the existence of such a joint time trend would imply that my mitigatory impact estimates are *upward*-biased, which would in principle be beneficial for retaining MS23's original conclusions.

I address this concern by fitting versions of model E1 that control for differential time trends. Specifically, I run 'triple-difference' models of the form

$$\begin{aligned}\log(\text{ALValue}_{i,t}) = {}& \delta_i + \alpha_{s(i),t} + \theta_1 \text{EHE}_{i,t} + \theta_2 \text{DI}_{i,t} + \theta_3 t + \theta_4 \left(\text{EHE}_{i,t} \times \text{DI}_{i,t}\right) \\ & + \theta_5 \left(t \times \text{EHE}_{i,t}\right) + \theta_6 \left(t \times \text{DI}_{i,t}\right) + \theta_7 \left(t \times \text{EHE}_{i,t} \times \text{DI}_{i,t}\right) + \Gamma X'_{i,t} + \epsilon_{i,t},\end{aligned} \tag{B1}$$

where $t \in \{0, 1, \cdots 6\}$ is a linear time trend and $\text{DI}_{i,t}$ is either $\text{VarietyExposure}_{i,t}$ or $\text{PatentExposure}_{i,t}$. Similarly to Equation E11, I then compute the average moderating effect of innovation exposure on EHE-driven AL devaluation as

$$\mathbb{E}\left[\frac{\partial^2 \log(\text{ALValue}_{i,t})}{\partial \text{EHE}_{i,t} \partial \text{DI}_{i,t}}\right] = \hat{\theta}_4 + \hat{\theta}_7 \mathbb{E}\left[t\right], \tag{B2}$$

where $\mathbb{E}\left[t\right]$ is the within-sample mean of t.

Online Appendix Tables C11 and C12 respectively present estimates of variety exposure and patent exposure's average moderating effects on EHE-driven AL devaluation after controlling for differential time trends. All of these estimates are negative, and all decrease compared to the respective mitigatory impact estimates in Table 5. This is intuitive; as aforementioned, a positive joint time trend between innovation and extreme heat exposure would *upward*-bias my mitigatory impact estimates. This implies that my key finding – that the mitigatory impact of innovation on EHE-driven AL devaluation is *less* than that estimated by MS23 – is not driven by such a joint time trend.

C Online Appendix Tables

	(1)	(2)	(3)	(4)
County-level EHE	1.145	1.166	0.462	0.486
	(0.019)	(0.02)	(0.016)	(0.017)
County fixed effects		X		X
State $\times$ decade fixed effects			X	X
Weighted by agricultural land area		X		
Observations	21027	21027	21014	21014
R^2	0.054	0.054	0.877	0.863

Note: Results arise from panel data regressions where county-level EHE is the independent variable and county-level average mean temperature (in degrees Celsius) is the dependent variable. Standard errors clustered at the county level are presented in parentheses.

Table C1: Relationship Between Extreme Heat and Local Temperatures

	(1) Variety Exposure	(2) Variety Exposure	(3) Variety Exposure	(4) Variety Exposure	(5) Variety Exposure	(6) Patent Exposure	(7) Patent Exposure	(8) Patent Exposure	(9) Patent Exposure
Panel A: Raw coefficients									
County-level EHE	0.09	0.5	-0.247	0.666	0.005	-1.01	-0.234	-2.541	-0.458
	(0.303)	(0.22)	(0.328)	(0.377)	(0.076)	(0.298)	(0.793)	(0.623)	(0.685)
Panel B: Standardized coefficients									
County-level EHE	0.085	0.472	-0.234	0.629	0.005	-0.956	-0.221	-2.404	-0.433
	(0.287)	(0.208)	(0.31)	(0.356)	(0.069)	(0.282)	(0.749)	(0.589)	(0.647)
Estimation type	LD	LD	LD	LD	LD	Panel	LD	LD	LD
County fixed effects	X	X	X	X	X	X	X	X	X
State $\times$ decade fixed effects	X	X	X	X	X	X	X	X	X
Output prices and interactions		X		X			X		X
Avg. temp. ($^\circ$ C) and interactions			X	X				X	X
Observations	6004	5994	6004	5994	21014	6004	5994	6004	5994

Note: Pseudo-Poisson maximum likelihood estimates from the `ppmlhdfe` command in Stata are presented along with standard errors double-clustered at the county and state-decade levels. The dependent variable is either variety exposure or patent exposure (depending on the column).

Table C2: Poisson Models of Relationships Between Local EHE and Direct Innovation Exposure in the County-Decade Panel Data

	Linear Coefficient	Elasticity	Linear Coefficient	Elasticity
LOO EHE	0.994	1.002		
	(0.018)	(0.023)		
Leave-state-out EHE			0.92	0.886
			(0.019)	(0.018)
Observations	21027	21027	21027	21027

Note: Results are based on simple random effects panel data regressions where county-level EHE is the dependent variable. The elasticity estimates are obtained via the `margins, eyex()` post-estimation command in Stata.

Table C3: Relationships Between Extreme Heat Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Published Estimates							
County-level EHE	-0.861	-1.55	-0.838	-0.872	-0.798	-0.232	-0.391
	(0.211)	(0.238)	(0.203)	(0.238)	(0.226)	(0.107)	(0.132)
County-level EHE ×	0.259	0.445	0.247	0.261	0.24	0.0923	0.13
LOO EHE	(0.0755)	(0.0718)	(0.0725)	(0.0786)	(0.0757)	(0.0315)	(0.032)
Observations	6000	6000	5990	6000	5990	20931	20931
R^2	0.989	0.991	0.989	0.989	0.989	0.979	0.984
Panel B: Replication Attempt							
County-level EHE	-1.066	-1.574	-1.011	-1.103	-0.986	-0.264	-0.359
	(0.208)	(0.262)	(0.203)	(0.239)	(0.225)	(0.109)	(0.129)
County-level EHE ×	0.181	0.389	0.154	0.173	0.148	0.0771	0.145
LOO EHE	(0.0765)	(0.0793)	(0.0668)	(0.0743)	(0.0684)	(0.0356)	(0.0371)
Observations	6000	6000	5990	6000	5990	20966	20966
R^2	0.989	0.991	0.989	0.989	0.989	0.979	0.984
Estimation type	LD	LD	LD	LD	LD	Panel	Panel
Local EHE squared	X	X	X	X	X	X	X
County fixed effects	X	X	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X	X	X
Weighted by agricultural land area		X					X
Output prices and interactions			X		X		
Avg. temp. (° C) and interactions				X	X		

Note: Panel A copies the results directly from Table A18 in MS23. Panel B is my best attempt to replicate these published results. Standard errors double-clustered at the county and state-by-decade levels are presented in parentheses.

Table C4: MS23's Table A18 and a Replication Attempt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Published estimates									
County-level EHE	-1.188	-1.183	-1.25	-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)
County-level EHE ×	0.13	0.128	0.135	0.0555	0.06	0.0941	0.0834	0.118	0.274
$\log(1+x)$-transformed variety exposure	(0.0365)	(0.0361)	(0.0677)	(0.0244)	(0.0251)	(0.0749)	(0.035)	(0.0411)	(0.133)
Panel B: Raw coefficients									
County-level EHE	-0.4937	-0.44577	-0.73572	-0.15792	-0.16394	-0.14298	0.07409	0.07685	-4.28067
	(0.18034)	(0.17759)	(0.56896)	(0.103)	(0.10755)	(1.04803)	(0.15624)	(0.16336)	(2.91802)
County-level EHE ×	0.01038	0.00701	0.01464	0.00457	0.00415	0.00585	-0.00019	-3e-04	0.1237
$\log(1+x)$-transformed variety exposure	(0.00326)	(0.00324)	(0.01465)	(0.00224)	(0.00247)	(0.0282)	(0.00385)	(0.00427)	(0.08024)
Panel C: Partial correlations									
County-level EHE	-0.23518	-0.2147	-0.10877	-0.12928	-0.12852	-0.01141	0.04859	0.04821	-0.15224
	(0.079)	(0.07977)	(0.08263)	(0.08223)	(0.08224)	(0.08361)	(0.10236)	(0.10236)	(0.10022)
County-level EHE ×	0.25771	0.17816	0.08325	0.16797	0.13933	0.01735	-0.00496	-0.00709	0.15622
$\log(1+x)$-transformed variety exposure	(0.07807)	(0.08097)	(0.08304)	(0.08126)	(0.082)	(0.0836)	(0.1026)	(0.10259)	(0.10009)
Panel D: Standardized coefficients									
County-level EHE	-0.71016	-0.64122	-1.05828	-0.22715	-0.23582	-0.20566	0.12473	0.12938	-7.2067
	(0.2594)	(0.25545)	(0.81841)	(0.14816)	(0.1547)	(1.50752)	(0.26304)	(0.27502)	(4.91263)
County-level EHE ×	0.54175	0.36582	0.76374	0.23823	0.2167	0.30531	-0.01158	-0.01837	7.69698
$\log(1+x)$-transformed variety exposure	(0.16986)	(0.16896)	(0.76447)	(0.11692)	(0.12879)	(1.4713)	(0.23933)	(0.26585)	(4.99297)
First-stage F (published)			26.52			10.03			6.022
First-stage F (reproduction)			21.345			1.691			2.312
Estimation type	OLS	OLS	IV	OLS	OLS	IV	OLS	OLS	IV
Start of temporal period	1990	1990	1990	1990	1990	1990	2000	2000	2000
County fixed effects	X	X	X	X	X	X	X	X	X
Decade fixed effects	X	X	X						
State × decade fixed effects				X	X	X	X	X	X
Additional controls		X	X		X	X		X	X
Observations	8990	8990	8990	8990	8990	8990	5996	5996	5996
R^2 (published)	0.961	0.961	0.005	0.978	0.978	-0.043	0.98	0.981	-0.073
R^2 (reproduction)	0.961	0.962	0.027	0.978	0.978	-0.037	0.98	0.98	-0.688

Note: Panel A copies the results directly from Table 1 in MS24. Panel B shows the results of my replication attempt. Panels C and D respectively present the estimates in Panel B converted into partial correlation coefficients and standardized coefficients. $\log(1+x)$-transformed variety exposure is computed using the formula in Equation A1. Additional controls include the $\log(1+x)$-transformed variety exposure from the decade prior to the start of the temporal period, both on its own and interacted with decade fixed effects. Standard errors double-clustered at the county and state-by-decade levels are presented in parentheses. Kleibergen & Paap (2006) first-stage F-statistics are presented for IV models.

Table C5: Reproduction of MS24, $\log(1+x)$-Transformed Variety Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Published estimates									
County-level EHE	-1.188	-1.183	-1.25	-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)
County-level EHE ×	0.13	0.128	0.135	0.0555	0.06	0.0941	0.0834	0.118	0.274
logarithmic variety exposure	(0.0365)	(0.0361)	(0.0677)	(0.0244)	(0.0251)	(0.0749)	(0.035)	(0.0411)	(0.133)
Panel B: Raw coefficients									
County-level EHE	-0.47314	-0.418	-0.80304	-0.15265	-0.14399	0.49366	-0.00799	0.04736	-3.39436
	(0.17072)	(0.16943)	(0.54111)	(0.09826)	(0.1038)	(0.52732)	(0.15334)	(0.16519)	(2.21404)
County-level EHE ×	0.01017	0.00612	0.01674	0.00462	0.00364	-0.01227	0.00231	8e-05	0.10334
logarithmic variety exposure	(0.00308)	(0.00312)	(0.01451)	(0.00218)	(0.00247)	(0.01409)	(0.00393)	(0.00451)	(0.064)
Panel C: Partial correlations									
County-level EHE	-0.23825	-0.21084	-0.12507	-0.13103	-0.11679	0.07805	-0.00534	0.0294	-0.15928
	(0.07888)	(0.07991)	(0.08232)	(0.08219)	(0.08248)	(0.08311)	(0.10259)	(0.10251)	(0.1)
County-level EHE ×	0.2659	0.16172	0.096	0.17414	0.12238	-0.07299	0.06013	0.0018	0.16343
logarithmic variety exposure	(0.07771)	(0.08144)	(0.08285)	(0.08109)	(0.08237)	(0.08318)	(0.10223)	(0.1026)	(0.09986)
Panel D: Standardized coefficients									
County-level EHE	-0.68058	-0.60127	-1.15512	-0.21958	-0.20712	0.7101	-0.01345	0.07973	-5.71458
	(0.24557)	(0.24372)	(0.77835)	(0.14133)	(0.1493)	(0.75852)	(0.25816)	(0.2781)	(3.72745)
County-level EHE ×	0.50606	0.30475	0.83297	0.2298	0.18108	-0.61044	0.13726	0.00469	6.142
logarithmic variety exposure	(0.15342)	(0.15551)	(0.7222)	(0.10867)	(0.1228)	(0.70128)	(0.23379)	(0.26816)	(3.80408)
First-stage F (published)			26.52			10.03			6.022
First-stage F (reproduction)			22.984			6.518			3.333
Estimation type	OLS	OLS	IV	OLS	OLS	IV	OLS	OLS	IV
Start of temporal period	1990	1990	1990	1990	1990	1990	2000	2000	2000
County fixed effects	X	X	X	X	X	X	X	X	X
Decade fixed effects	X	X	X						
State × decade fixed effects				X	X	X	X	X	X
Additional controls		X	X		X	X		X	X
Observations	8990	8990	8990	8990	8990	8990	5996	5996	5996
R^2 (published)	0.961	0.961	0.005	0.978	0.978	-0.043	0.98	0.981	-0.073
R^2 (reproduction)	0.961	0.962	0.028	0.978	0.978	-0.027	0.98	0.98	-0.607

Note: Panel A copies the results directly from Table 1 in MS24. Panel B shows the results of my replication attempt. Panels C and D respectively present the estimates in Panel B converted into partial correlation coefficients and standardized coefficients. Logarithmic variety exposure is computed using the formula in Equation A2. Additional controls include the logarithmic variety exposure from the decade prior to the start of the temporal period, both on its own and interacted with decade fixed effects. Standard errors double-clustered at the county and state-by-decade levels are presented in parentheses. Kleibergen & Paap (2006) first-stage F-statistics are presented for IV models.

Table C6: Reproduction of MS24, Logarithmic Variety Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Published estimates						
County-level EHE	-1.183	-1.25	-0.453	-0.671	-0.791	-1.953
	(0.326)	(0.604)	(0.218)	(0.604)	(0.355)	(1.014)
County-level EHE $\times$	0.128	0.135	0.06	0.0941	0.118	0.274
$\log(1+x)$-transformed variety exposure	(0.0361)	(0.0677)	(0.0251)	(0.0749)	(0.0411)	(0.133)
Panel B: Raw coefficients						
County-level EHE	-0.41993	-0.74316	-0.16135	-0.14298	0.03832	7.56295
	(0.18213)	(0.56528)	(0.10743)	(1.04803)	(0.15774)	(36.24108)
County-level EHE $\times$	0.00703	0.01531	0.00403	0.00585	0.00049	-0.20139
$\log(1+x)$-transformed variety exposure	(0.00339)	(0.01456)	(0.00243)	(0.0282)	(0.0042)	(0.98161)
Panel C: Partial correlations						
County-level EHE	-0.1965	-0.11061	-0.12659	-0.01141	0.02492	0.02141
	(0.0804)	(0.0826)	(0.08228)	(0.08361)	(0.10253)	(0.10255)
County-level EHE $\times$	0.17102	0.08757	0.1374	0.01735	0.01203	-0.02105
$\log(1+x)$-transformed variety exposure	(0.08118)	(0.08298)	(0.08205)	(0.0836)	(0.10258)	(0.10255)
Panel D: Standardized coefficients						
County-level EHE	-0.60404	-1.06898	-0.23209	-0.20566	0.06451	12.73272
	(0.26198)	(0.81311)	(0.15453)	(1.50752)	(0.26556)	(61.0148)
County-level EHE $\times$	0.36669	0.79867	0.2101	0.30531	0.03064	-12.53114
$\log(1+x)$-transformed variety exposure	(0.17666)	(0.75979)	(0.12666)	(1.4713)	(0.26135)	(61.07985)
First-stage F (published)		26.52		10.03		6.022
First-stage F (reproduction)		20.278		1.691		0.065
Estimation type	OLS	IV	OLS	IV	OLS	IV
Start of temporal period	1990	1990	1990	1990	2000	2000
County fixed effects	X	X	X	X	X	X
Decade fixed effects	X	X				
State $\times$ decade fixed effects			X	X	X	X
Additional controls	X	X	X	X	X	X
Observations	8990	8990	8990	8990	5996	5996
R^2 (published)	0.961	0.005	0.978	-0.043	0.981	-0.073
R^2 (reproduction)	0.962	0.027	0.978	-0.037	0.98	-34.317

Note: Panel A copies the results directly from Table 1 in MS24. Panel B shows the results of my replication attempt. Panels C and D respectively present the estimates in Panel B converted into partial correlation coefficients and standardized coefficients. $\log(1+x)$-transformed variety exposure is computed using the formula in Equation A1. Additional controls include the first lag of $\log(1+x)$-transformed variety exposure, both on its own and interacted with decade fixed effects. Standard errors double-clustered at the county and state-by-decade levels are presented in parentheses. Kleibergen & Paap (2006) first-stage F-statistics are presented for IV models.

Table C7: Alternate Reproduction of MS24, $\log(1+x)$-Transformed Variety Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Published estimates						
County-level EHE	-1.183	-1.25	-0.453	-0.671	-0.791	-1.953
	(0.326)	(0.604)	(0.218)	(0.604)	(0.355)	(1.014)
County-level EHE ×	0.128	0.135	0.06	0.0941	0.118	0.274
logarithmic variety exposure	(0.0361)	(0.0677)	(0.0251)	(0.0749)	(0.0411)	(0.133)
Panel B: Raw coefficients						
County-level EHE	-0.41352	-0.7472	-0.15916	0.06894	0.06499	-11.28961
	(0.17384)	(0.54453)	(0.10312)	(0.77877)	(0.16472)	(69.37839)
County-level EHE ×	0.00739	0.01598	0.00404	-3e-05	-0.00022	0.34641
logarithmic variety exposure	(0.00326)	(0.01448)	(0.0024)	(0.02165)	(0.00447)	(2.13809)
Panel C: Partial correlations						
County-level EHE	-0.20297	-0.11551	-0.13016	0.0074	0.04045	-0.0167
	(0.08018)	(0.08251)	(0.08221)	(0.08362)	(0.10243)	(0.10257)
County-level EHE ×	0.18639	0.09192	0.13911	-0.00011	-0.00495	0.01662
logarithmic variety exposure	(0.08072)	(0.08292)	(0.08201)	(0.08362)	(0.1026)	(0.10257)
Panel D: Standardized coefficients						
County-level EHE	-0.59482	-1.07479	-0.22894	0.09916	0.10942	-19.00704
	(0.25006)	(0.78328)	(0.14832)	(1.12021)	(0.27731)	(116.80574)
County-level EHE ×	0.36801	0.79557	0.20083	-0.0014	-0.01282	20.59002
logarithmic variety exposure	(0.16221)	(0.72073)	(0.11955)	(1.07757)	(0.26593)	(127.0851)
First-stage F (published)		26.52		10.03		6.022
First-stage F (reproduction)		22.749		2.453		0.008
Estimation type	OLS	IV	OLS	IV	OLS	IV
Start of temporal period	1990	1990	1990	1990	2000	2000
County fixed effects	X	X	X	X	X	X
Decade fixed effects	X	X				
State × decade fixed effects			X	X	X	X
Additional controls	X	X	X	X	X	X
Observations	8990	8990	8990	8990	5996	5996
R^2 (published)	0.961	0.005	0.978	-0.043	0.981	-0.073
R^2 (reproduction)	0.962	0.022	0.978	-0.04	0.98	-303.369

Note: Panel A copies the results directly from Table 1 in MS24. Panel B shows the results of my replication attempt. Panels C and D respectively present the estimates in Panel B converted into partial correlation coefficients and standardized coefficients. logarithmic variety exposure is computed using the formula in Equation A2. Additional controls include the first lag of logarithmic variety exposure, both on its own and interacted with decade fixed effects. Standard errors double-clustered at the county and state-by-decade levels are presented in parentheses. Kleibergen & Paap (2006) first-stage F-statistics are presented for IV models.

Table C8: Alternate Reproduction of MS24, Logarithmic Variety Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Raw coefficients							
County-level EHE	-0.4012426	-0.5274014	-0.4386258	-0.3064452	-0.3102808	-0.0129256	-0.0776217
	(0.1166121)	(0.1562014)	(0.1074939)	(0.1313304)	(0.1200561)	(0.0705969)	(0.1043509)
County-level EHE ×	-1.1e-05	-1.8e-06	-8.8e-05	-1.6e-06	-8.96e-05	-2.12e-05	-1.06e-05
variety exposure	(2.1e-05)	(3.26e-05)	(5.11e-05)	(1.84e-05)	(4.65e-05)	(1.51e-05)	(2.1e-05)
Panel B: Partial correlations							
County-level EHE	-0.3773154	-0.3692783	-0.4609898	-0.246571	-0.2750046	-0.0100038	-0.0406746
	(0.0879913)	(0.0886069)	(0.0807946)	(0.0963602)	(0.0948386)	(0.0546304)	(0.0545454)
County-level EHE ×	-0.0538411	-0.0057453	-0.1795464	-0.0090282	-0.2016499	-0.0770684	-0.0276328
variety exposure	(0.1023004)	(0.1025944)	(0.0992904)	(0.1025895)	(0.0984259)	(0.0543113)	(0.0545941)
Panel C: Standardized coefficients							
County-level EHE	-0.2815121	-0.3700252	-0.3074797	-0.2150022	-0.2175089	-0.0117953	-0.0708341
	(0.0818151)	(0.109591)	(0.075354)	(0.0921415)	(0.0841602)	(0.0644235)	(0.0952259)
County-level EHE ×	-0.0080186	-0.0013315	-0.0641776	-0.0011818	-0.0653999	-0.0181015	-0.0090688
variety exposure	(0.015302)	(0.023778)	(0.0372593)	(0.0134306)	(0.0339447)	(0.0128707)	(0.0179378)
Estimation type	LD	LD	LD	LD	LD	Panel	Panel
County fixed effects	X	X	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X	X	X
Weighted by agricultural land area		X					X
Output prices and interactions			X		X		
Avg. temp. (° C) and interactions				X	X		
Observations	6000	6000	5990	6000	5990	20966	20966
R^2	0.988	0.99	0.989	0.988	0.989	0.979	0.984

Note: The dependent variable in all models is logarithmic AL values. Estimates of the model in Equation E1, where $\text{IE}_{i,t}$ is replaced with a variant of $\text{IE}_{i,t}$ that omits corn, lettuce and romaine, soybeans, tomatoes, and wheat are presented alongside standard errors double-clustered at the county and state-decade levels in parentheses.

Table C9: Mitigatory Impacts of Variety Exposure, No 'Top Five' Crops

	(1)	(2)	(3)	(4)	(5)
Panel A: Raw coefficients					
County-level EHE	-0.37902	-0.4298304	-0.3783317	-0.2111482	-0.2616331
	(0.1141425)	(0.142894)	(0.1212683)	(0.1189039)	(0.1259963)
County-level EHE ×	-7e-07	7.7e-06	-2e-06	-7e-07	-6.2e-06
patent exposure	(3.9e-06)	(8.5e-06)	(4.3e-06)	(3.8e-06)	(4.6e-06)
Panel B: Partial correlations					
County-level EHE	-0.3623624	-0.324456	-0.3378589	-0.1852934	-0.2180518
	(0.0891261)	(0.0917972)	(0.0908864)	(0.0990753)	(0.0977197)
County-level EHE ×	-0.0185248	0.0926121	-0.0485678	-0.0186507	-0.1401252
patent exposure	(0.1025626)	(0.1017179)	(0.1023558)	(0.1025621)	(0.1005833)
Panel C: Standardized coefficients					
County-level EHE	-0.2659207	-0.3015693	-0.2652131	-0.1481417	-0.1834065
	(0.0800825)	(0.1002546)	(0.0850099)	(0.083423)	(0.0883243)
County-level EHE ×	-0.0018477	0.0200921	-0.0052439	-0.0017755	-0.0160377
patent exposure	(0.0102351)	(0.0221628)	(0.0110907)	(0.0097686)	(0.0118573)
Estimation type	LD	LD	LD	LD	LD
County fixed effects	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X
Weighted by agricultural land area		X			
Output prices and interactions			X		X
Avg. temp. (° C) and interactions				X	X
Observations	6000	6000	5990	6000	5990
R^2	0.989	0.991	0.989	0.989	0.989

Note: The dependent variable in all models is logarithmic AL values. Estimates of the model in Equation E1, where $\text{IE}_{i,t}$ is replaced with a variant of $\text{PatentExposure}_{i,t}$ that omits alfalfa (and varieties thereof), barley, corn, soybeans, and tobacco, are presented alongside standard errors double-clustered at the county and state-decade levels in parentheses.

Table C10: Mitigatory Impacts of Patent Exposure, No 'Top Five' Crops

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Average moderating effect	-0.00043	-0.00053	-0.00035	-4e-04	-0.00032	-9e-05	-1e-04
of variety exposure	(9e-05)	(9e-05)	(1e-04)	(9e-05)	(9e-05)	(2e-05)	(3e-05)
Observations	6000	6000	5990	6000	5990	20966	20966
R^2	0.989	0.991	0.989	0.989	0.989	0.979	0.984
Estimation type	LD	LD	LD	LD	LD	Panel	Panel
County fixed effects	X	X	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X	X	X
Weighted by agricultural land area		X					X
Output prices and interactions			X		X		
Avg. temp. (° C) and interactions				X	X		

Note: Average moderating effects are calculated by passing Equation B1 through the `lincom` suite in Stata after estimating a model of the form in Equation B2, where variety exposure is the innovation exposure measure of interest. Standard errors double-clustered at the county and state-decade levels are presented in parentheses.

Table C11: Average Moderating Impact of Variety Exposure on EHE-Driven AL Devaluation

	(1)	(2)	(3)	(4)	(5)
Average moderating effect	-0.01592	-0.03424	-0.01263	-0.01394	-0.01215
of patent exposure	(0.00618)	(0.00867)	(0.00998)	(0.00603)	(0.01011)
Observations	6000	6000	5990	6000	5990
R^2	0.988	0.991	0.989	0.989	0.989
Estimation type	LD	LD	LD	LD	LD
County fixed effects	X	X	X	X	X
State × decade fixed effects	X	X	X	X	X
Weighted by agricultural land area		X			
Output prices and interactions			X		X
Avg. temp. (° C) and interactions				X	X

Note: Average moderating effects are calculated by passing Equation B1 through the `lincom` suite in Stata after estimating a model of the form in Equation B2, where patent exposure is the innovation exposure measure of interest. Standard errors double-clustered at the county and state-decade levels are presented in parentheses.

Table C12: Average Moderating Impact of Patent Exposure on EHE-Driven AL Devaluation

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