THE PROBLEMS WITH POOR PROXIES: DOES INNOVATION MITIGATE AGRICULTURAL DAMAGE FROM CLIMATE CHANGE?

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Abstract

Moscona & Sastry (2023, *Quarterly Journal of Economics*) – henceforth MS23 – find that cropland values are significantly less damaged by extreme heat exposure (EHE) when crops are more exposed to technological innovation. Re-analyzing MS23's replication data, I document extensive evidence that this finding is not robust, and that the mitigatory effects of innovation on climate change damage are negligibly small. MS23's 'innovation exposure' variable does not measure innovation, instead proxying innovation using a measure of crops' national heat exposure. This proxy moderates EHE impacts for reasons unrelated to innovation. I show that the proxy is practically identical to local EHE, meaning that MS23's models examining interaction effects between their proxy and local EHE effectively interact local EHE with itself. I demonstrate that MS23's findings on 'innovation exposure' simply reflect nonlinear impacts of local EHE on agricultural land value, and uncover robustness issues for other key findings. I then construct direct measures of innovation exposure from MS23's crop variety and patenting data. Replacing MS23's proxy with these direct innovation measures decreases MS23's moderating effect estimates by at least 99.2% in standardized units; none of these new estimates are statistically significantly different from zero. Similar results arise from an instrumental variables strategy that instruments my direct innovation measures with MS23's heat proxy. These results cast doubt on the general capacity for market innovations to mitigate agricultural damage from climate change.
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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 such actors should dedicate towards mitigating climate change. Based on available mitigation pathways proposed by the Intergovernmental Panel on Climate Change, global climate change mitigation costs could range from 1-7% of global GDP each year (Fujimori et al. 2023). Considerable time and effort has been invested into providing governments and businesses with estimates on the optimal location of this investment spectrum on which to fall (see Auffhammer 2018). This will depend not just on the overall costs of climate change, but also on the mitigatory impacts of adaptations in practices and technology. A large literature has focused on the mitigatory impacts of such adaptations (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 use panel data estimates to offer evidence in support of two findings. First, 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 (HTE) models that interact 'innovation exposure' with extreme heat exposure (EHE). These HTE models yield negative coefficients for EHE, but positive coefficients for the interaction term between 'innovation exposure' and EHE, implying that agricultural land (AL) in counties that are more exposed to innovation is less severely devalued by EHE. In fact, MS23 find 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 EHE-driven AL devaluation since 1960, and will offset 13% of EHE-driven AL devaluation by 2100.

This paper critically re-examines MS23's findings due to a key flaw: MS23's 'innovation exposure' measure does not directly 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' proxy is thus a measure of heat, rather than innovation. I demonstrate this in a re-analysis of MS23's replication data (Moscona & Sastry 2022), which shows that MS23's proxy is nearly indistinguishable from local EHE. This is intuitive, as both local and 'leave-one-out' EHE reflect climate trends on the regional, national, and global levels.

This means that the positive coefficients on the interaction terms in MS23's HTE models 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 additional increases in local EHE diminish if counties are already exposed to higher levels of extreme heat. This is again intuitive. Though increases in EHE cause steep declines in agricultural productivity near thresholds for 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 in several ways. First, I confirm that 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 HTE models. Second, I show that one of MS23's critical robustness checks to rule out this possibility fails to replicate, and that 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 renders most of MS23's estimates of interest not statistically significantly different from zero.

Estimating the mitigatory impacts of innovation does not require using a heat proxy, as MS23 have data on 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 re-estimate MS23's models, replacing MS23's proxy with these direct innovation measures, and find no moderating effect estimate that is 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 are at least 99.2% less than the standardized coefficients of those estimates in MS23.

I additionally pursue an instrumental variables strategy which instruments my direct innovation measures with MS23's exogenous heat-based 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. However, I find that MS23's proxy is a weak instrument for innovation. Additionally, despite clear exclusion restriction violations that are likely favorable to MS23's conclusions, these instrumental variables estimates of innovation's mitigatory impact on EHE-driven AL devaluation are at least 43% smaller in standardized units than those estimates in MS23, with none being statistically significantly different from zero.

These mitigatory impact estimates remain non-robust 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, as these projections are entirely based on MS23's HTE models. 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.

Section 2 of this paper overviews the main published estimates of interest from MS23. Section 3 then details MS23's proxy, as well as its inappropriateness as a measure of innovation for the purposes of MS23's estimations of interest. Section 4 addresses MS23's and MS24's empirical justifications for the proxy. Thereafter, Section 5 introduces the two direct innovation measures I construct from MS23's replication data. Section 6 displays the results after re-estimating MS23's HTE models using my direct innovation measures. Section 7 addresses a wide range of alternative explanations for the differences between my mitigatory impact estimates and those in MS23. Section 8 concludes.

2 Data and Published Findings

My analyses rely on MS23's replication repository (Moscona & Sastry 2022). Unfortunately, the repository lacks code for Online Appendix Tables A18 and A20. These omissions lead to replication failures in the appendix; I defer discussion of this issue to Section 4.4.

The main replication of interest to this paper concerns the mitigatory effects of technological market innovations on EHE-induced AL devaluation. MS23's main estimates of this mitigatory impact 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 a simple HTE model of the form

$$\begin{aligned}\log(\text{AgrLandPrice}_{i,t}) = \delta_i &+ \alpha_{s(i),t} + \beta \text{ExtremeExposure}_{i,t} \\ &+ \gamma \text{InnovationExposure}_{i,t} \\ &+ \phi \left(\text{ExtremeExposure}_{i,t} \times \text{InnovationExposure}_{i,t}\right) \\ &+ \Gamma X'_{i,t} + \epsilon_{i,t}.\end{aligned} \tag{E1}$$

Here $\log(\text{AgrLandPrice}_{i,t})$ 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. I defer discussion of $\text{ExtremeExposure}_{i,t}$ and $\text{InnovationExposure}_{i,t}$ to Section 3.

The model in Equation E1 is estimated using a county-decade panel dataset with $t \in \{1950, 1960, \cdots 2010\}$.[1] Table III is estimated using two types of specifications. Models 1-5 are estimated using a 'long-difference' (LD) specification, which restricts time periods to $t \in \{1950, 2010\}$. Models 6-7 are estimated using a panel specification with no such temporal restrictions.

[Table R–I about here]

ϕ is the parameter that MS23 interpret as an estimand for innovation's mitigatory impact on climate change damage. Table R–I shows $\hat{\beta}$ and $\hat{\phi}$ estimates, with standard errors (SEs)

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

double-clustered at the county and state-decade levels.[2] Panel A juxtaposes the published results from MS23 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 more exposed to innovation experience less devaluation when exposed to extreme heat. To provide a sense of scale for these estimates, Panels C-D of Table R–I 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 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 my reproduction of Table III 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 the estimates into standardized coefficients σ. Let D be a given independent variable (either EHE or its interaction with innovation exposure) and Y be the dependent variable (logarithmic AL value). Because all D and Y are continuous, I compute σ and SE(σ) using estimate $\hat{\tau} \in \left\{\hat{\phi}, \hat{\beta}\right\}$ via the formulas

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

where σ_D and σ_Y respectively represent the within-sample standard deviations of D and Y.[4] σ estimates for $\hat{\phi}$ range from 0.274 to 1.028 in Table R–I, ranging from small to large in standardized effect size terms (see Cohen 1988). These standardized coefficients can be

[2] I 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.

[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.

interpreted on the scale of 'standard deviation effects'. σ represents 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 EHE as $\text{ExtremeExposure}_{i,k,t}$, which is the number of growing degree days in county i above crop k's maximum optimal temperature in decade t. By Equation 16, MS23 measure EHE at the county level as a weighted average of $\text{ExtremeExposure}_{i,k,t}$ across all crops planted in county i, where the heat exposure for crop k is weighted by the proportion of land area in county i dedicated to planting crop k at baseline:

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

By Equation 17, MS23 measure 'innovation exposure' as an area-weighted average across counties in a given decade, in an analagous fashion to $\text{ExtremeExposure}_{i,t}$. However, rather than an area-weighted average of $\text{ExtremeExposure}_{i,k,t}$, MS23 specify $\text{InnovationExposure}_{i,t}$ as an area-weighted average of *other counties'* EHE:

$$\begin{aligned} \text{InnovationExposure}_{i,t} = \sum_{k} \Bigg[& \frac{\text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \\ & \times \sum_{j \neq i} \left[\frac{\text{Area}^{\text{Pre}}_{j,k}}{\sum_{j \neq i} \text{Area}^{\text{Pre}}_{j,k}} \times \text{ExtremeExposure}_{j,k,t} \right] \Bigg]. \end{aligned} \tag{E5}$$

Though MS23 are transparent about the calculation of $\text{InnovationExposure}_{i,t}$ in their article, this 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 primarily 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.

[Figure R-I about here]

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 R-I displays the relationship that $\hat{\phi}$ is most likely capturing in Table III. In particular, Figure R-I presents a binscatter regression plot between county-level EHE and logarithmic AL values. Each point and confidence band represents a quantile bin of the distribution of $\text{ExtremeExposure}_{i,t}$, so x-axis regions with more (fewer) points are more dense (sparse) in $\text{ExtremeExposure}_{i,t}$. The figure makes clearly visible that although local EHE decreases AL value across most of the distribution of $\text{ExtremeExposure}_{i,t}$, this relationship diminishes on average as local EHE increases, and functionally flatlines for sufficiently high values of $\text{ExtremeExposure}_{i,t}$.

Because national heat shocks naturally drive local heat shocks, LOO EHE very closely tracks county-level EHE (see Section 4.2), and thus the interaction term in Equation E1 is effectively interacting $\text{ExtremeExposure}_{i,t}$ with *itself*. As a result, Equation E1 functionally estimates logarithmic AL values as a function of a second-order polynomial in local EHE. $\hat{\phi}$'s sign in MS23's HTE specifications is thus effectively the sign of the average second derivative over the function displayed in Figure R-I. Therefore, MS23's positive $\hat{\phi}$ estimates largely reflect the deceleration of negative marginal EHE impacts as $\text{ExtremeExposure}_{i,t}$ increases towards the upper tail of its distribution. In other words, $\hat{\phi}$ is positive because a parabola fit to the points in Figure R-I 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 marginal damages of additional temperature increases to crop yields diminish or even flatline. In fact, the nonlinear relationship between county-level EHE and AL values displayed in Figure R-I is fairly similar to the nonlinear relationships between temperatures and crop yields that Schlenker & Roberts (2008; 2009) find for corn, soybeans, and cotton.[5]

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. Specifically, I estimate specifications akin to Equation E1, but replace the interaction specification between $\text{ExtremeExposure}_{i,t}$ and LOO EHE with a second-order polynomial in $\text{ExtremeExposure}_{i,t}$:

$$\begin{aligned}\log(\text{AgrLandPrice})_{i,t} = \delta_i + \alpha_{s(i),t} + \theta_1 \text{ExtremeExposure}_{i,t} \\ + \theta_2 \text{ExtremeExposure}^2_{i,t} + \Gamma X'_{i,t} + \epsilon_{i,t}.\end{aligned} \tag{E6}$$

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

[Table R–II about here]

Table R–II 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 R–II show that the effect sizes of these estimates are smaller than those from Table III (see Panels C-D in Table R–I), all seven models in Table R–II 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 Justifications for the Proxy

MS23 justify their innovation exposure proxy on four grounds. First, MS23 contend that EHE is a strong predictor of climate-adaptive innovation. Second, MS23 claim that computing

[5]MS24 object to this comparison, noting that Schlenker & Roberts (2008; 2009) are examining nonlinear relationships in raw temperature, whereas Figure R-I is displaying nonparametric relationships in EHE (which is locally indexed based on agronomically-verified killing temperatures of the crops growing in each county at baseline). However, these two findings are likely both capturing the same underlying nonlinear impacts in extreme heat. Both EHE and local temperatures exhibit strong positive correlations, which is intuitive given that they are both measures of heat. Online Appendix Table A-I 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.

their proxy in LOO fashion rids the proxy of correlations with local temperature. Third, MS23 provide checks showing 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 these arguments in turn throughout the remainder of this section. In Section 4.1, I note that an innovation proxy is unnecessary given that MS23 already possess direct measures of innovation, and establish several reasons to doubt that the relationship between EHE and innovation is either causal or strong enough to justify using heat as a direct measure of innovation. In Section 4.2, I show that computing MS23's proxy in LOO fashion does not purge the proxy of correlations with local EHE; their proxy maps both one-to-one linearly, and unit elastically, with local EHE. In Section 4.3, I show similar results for MS23's leave-state-out proxy. Finally, in Section 4.4, I show that MS23's robustness checks controlling for higher-order polynomials in local EHE fail to 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.

4.1 Heat as a Predictor of Climate-Adaptive Innovation

MS23 and MS24 note that their estimates in Section IV provide evidence that EHE strongly predicts innovation, showing that crops whose croplands are more exposed to extreme heat are also more exposed to increased innovation in crop varieties and climate-related patenting.[6] However, this means that a proxy is unnecessary, as LD and/or panel data is available on these variables. The estimation in Equation E1 can thus be conducted with direct measures of innovation exposure. I revisit this point in further detail in Sections 5 and 6.

Even setting aside the issue of whether a proxy is necessary, there is an important robustness issue concerning extreme heat's causal relationship with innovation: specifically,

[6]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."

innovation in MS23's data is concentrated in a handful of crops. As addressed in MS24, by the end of the 2010s, 37.8% of the crop varieties in MS23's data belonged to the top five crops with the most crop varieties. Further, 50% of the climate-related patents in MS23's data were related to the top five crops with the most climate-related patents. Many of these crops hold particularly large market shares in the American crop market.[7] If most of the variation in innovation arises from a handful of the most-innovated crops, then MS23's results in Section IV are effectively just identifying whether the counties growing the most-innovated large-market crops happen to be exposed to more extreme heat. This would pose a challenge to interpreting the results in Section IV as causal evidence that EHE induces endogenous climate-adaptive innovation. These crops may be more innovated upon not because they are more exposed to extreme heat, but simply because these crops have larger markets, and therefore marginal improvements in yield would promise larger financial returns.

MS23's finding that EHE induces growth in climate-related patents is entirely dependent on this handful of top crops. Online Appendix Table A-II replicates MS23's Table II, excluding the top five crops with the most climate-related patents by the end of the 2010s. Removing these top five crops decreases the estimated relationship between EHE and climate-related patenting by over 43%, and renders the relationship not statistically significantly different from zero.

Additionally, MS23's findings that EHE induces climate-adaptive crop variety development are considerably driven by this handful of top crops. Online Appendix Table A-III replicates MS23's Table I, removing the top five crops with the most crop varieties by the end of the 2010s. Though all of the estimates of EHE's impact on crop variety development remain statistically significantly different from zero, they attenuate considerably. These estimates are smaller than their respective estimates in Table I by 11.9-35.3%.

The questionable causal relationship between EHE and climate-adaptive innovation poses serious challenges to the appropriateness of LOO EHE as a proxy for 'innovation exposure'. MS23 rely on the strength and validity of this relationship to argue that LOO EHE can be used not just as an instrument for innovation exposure, but as a *direct measure* of innovation.

[7] 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.

The fact that the EHE-innovation relationship is neither particularly strong nor certainly causal creates serious doubts for the validity of such a proxy.

4.2 Leave-One-Out Computation

Though MS23 posit that computing national EHE 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 county-level extreme heat shocks. This is an intuitive consequence of the fact that both county-level and LOO EHE are driven by regional and national extreme heat increases induced by global climate change. EHE is thus exogenously 'assigned' at a much 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.

[Figure R-II about here]

There is strong evidence that MS23's LOO EHE measure closely proxies county-level EHE. The left-hand graph in Figure R-II plots the results of a binscatter regression between $\text{ExtremeExposure}_{i,t}$ and LOO EHE, showing that the two measures 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.

Columns 1 and 2 in Online Appendix Table A-IV provide additional evidence showing that the two EHE measures 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 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.

4.3 An Alternative Leave-State-Out Proxy

MS23 attempt to address concerns about EHE assignment spillovers by running an alternative specification that computes national EHE in leave-*state*-out fashion rather than LOO fashion.[8] Intuitively, for county i in state s, leave-state-out EHE is an area-weighted average of $\text{ExtremeExposure}_{i,k,t}$ for all $i \notin s$. The logic behind this check is that EHE is almost certainly exogenously assigned at a higher level than the county, but is less plausibly assigned at a higher level than the state. Therefore, if results arising from the leave-state-out proxy look similar to those arising from the LOO proxy, MS23 reason that this provides reassurance that their results are not driven by EHE spillovers from nearby geographic divisions.

Though MS23 contend that their results in 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 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 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 very little to rid the proxy of strong correlations with local EHE. The right-hand graph in Figure R-II shows a binscatter regression plot of the nonparametric relationship between leave-state out and local EHEs. The right-hand graph looks strikingly similar to the left-hand graph, which plots the same nonparametric relationship between LOO and local EHEs. Both relationships map closely to the perfect unit relationship of a 45-degree line for the vast majority of their distributions. Columns 3-4 of Online Appendix Table A-IV 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), which is close to a one-to-one unit relationship. The elasticity between local EHE and leave-state-out EHE is 0.886 (SE = 0.018), which is close to a unit elastic

[8]MS23 provide code for computing this measure in `make_county_shocks.py`, and store it by county and decade in dataset `county_shocks.csv` under variables `gdd_lso_1950`, `gdd_lso_1960`, and so forth. I write a simple R script that maps these leave-state-out EHE values to their respective county-decade observations in `county_data.dta`.

relationship. The relationship between local and leave-state-out EHEs is further away from linear one-to-one and unit-elastic than the relationship between local and LOO EHEs, which likely explains the attenuation of the mitigatory impact estimates in Online Appendix Table A20. However, the differences in the relationships with local EHE between the LOO and leave-state-out EHEs are minor (see Section 4.2). Like LOO EHE, leave-state-out EHE is functionally identical to local EHE.

4.4 Robustness of the Main Specifications

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

$$\begin{aligned}\log(\text{AgrLandPrice}_{i,t}) = \delta_i &+ \alpha_{s(i),t} + \beta_1 \text{ExtremeExposure}_{i,t} \\ &+ \beta_2 \text{ExtremeExposure}^2_{i,t} + \gamma \text{InnovationExposure}_{i,t} \\ &+ \phi \left(\text{ExtremeExposure}_{i,t} \times \text{InnovationExposure}_{i,t}\right) \\ &+ \Gamma X'_{i,t} + \epsilon_{i,t},\end{aligned} \tag{E7}$$

where ϕ remains the estimate of interest.

However, Online Appendix Table A18 fails to replicate, and this replication failure is important for MS23's claims. Online Appendix Table A-V juxtaposes the published version of Online Appendix Table A18 against my best attempt to replicate the table. The published version of Online Appendix Table A18 implies that after controlling for $\text{ExtremeExposure}^2_{i,t}$, MS23's 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 to this check 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 Online Appendix Table A18 reveals that controlling for $\text{ExtremeExposure}^2_{i,t}$ 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 robustness check is incorrectly specified. The HTE model in Equation E1 produces moderating effect estimates by interacting $\text{ExtremeExposure}_{i,t}$ with LOO EHE. Equation E7, which is the model used to produce the estimates in Online Appendix Table A18, augments Equation E1 by adding $\text{ExtremeExposure}^2_{i,t}$ as a control variable, but then fails to specify the additional interaction between $\text{ExtremeExposure}^2_{i,t}$ and LOO EHE that would be ordinarily expected in a model of this form. The HTE model in The model in Equation E7 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 E7 loses its intuitive econometric interpretation.

The average moderating effect of LOO EHE on EHE-induced AL devaluation can be appropriately obtained by additionally specifying the interaction between $\text{ExtremeExposure}^2_{i,t}$ and LOO EHE. For compactness, let $\text{ExtremeExposure}_{i,t}$ and $\text{InnovationExposure}_{i,t}$ be written as $\text{EHE}_{i,t}$ and $\text{LOO}_{i,t}$ respectively. Consider a model of the form

$$\begin{aligned}\log(\text{AgrLandPrice}_{i,t}) = \delta_i + \alpha_{s(i),t} + \beta_1 \text{EHE}_{i,t} + \beta_2 \text{EHE}^2_{i,t} + \gamma \text{LOO}_{i,t} \\ + \phi_1 \left(\text{EHE}_{i,t} \times \text{LOO}_{i,t}\right) + \phi_2 \left(\text{EHE}^2_{i,t} \times \text{LOO}_{i,t}\right) \\ + \Gamma X'_{i,t} + \epsilon_{i,t}.\end{aligned} \tag{E8}$$

Under a standard conditional mean independence assumption (which MS23 also implicitly impose), the average moderating effect of LOO EHE on EHE-driven AL devaluation can be isolated from Equation E8 as follows:

$$\mathbb{E}\left[\frac{\partial^2 \log(\text{AgrLandPrice}_{i,t})}{\partial \text{EHE}_{i,t} \partial \text{LOO}_{i,t}}\right] = \hat{\phi}_1 + 2\hat{\phi}_2 \mathbb{E}\left[\text{EHE}_{i,t}\right]. \tag{E9}$$

$\mathbb{E}\left[\text{EHE}_{i,t}\right]$ can be simply computed as a within-sample mean of $\text{EHE}_{i,t}$. MS23's moderating effect estimates in Equation E7 arise exclusively from ϕ, which is akin to ϕ_1 in Equation E8.

Therefore, given the average moderating effect identified in Equation E9, MS23's estimates in Online Appendix Table A18 do not fully identify the moderating effect of LOO EHE on EHE-driven AL devaluation.

[Table R–III about here]

Running the correctly-specified model in Equation E8 and obtaining average moderating effects via Equation E9 shows that MS23's estimates of the moderating effect of LOO EHE on EHE-driven AL devaluation are not robust to controlling for nonlinear functions of local EHE. Table R–III displays average moderating effect estimates from Equation E9, computed from specifications of the form in Equation E8. All five of the average moderating effect estimates for the LD models in Table R–III are smaller than their respective estimates in Table III, decreasing by 11.5-54.9%. Four of these five LD estimates 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 panel data estimates in Table III. These results ultimately 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.

5 Direct Measures of Innovation Exposure

As discussed in Section 4, it is not necessary to proxy innovation exposure with LOO EHE, as MS23 possess data on multiple direct measures of innovation. Section IV of MS23 shows correlations between county-level EHE and two forms of innovation. First, crops whose croplands are exposed to more extreme heat see increases in crop variety development. Repository dataset `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, crops whose croplands are exposed to more extreme heat see increases in associated patents that are related to climate change. `crop_level_data.dta` stores crop-level data on PatentsPre_k (the number of climate-related patents associated with crop k prior to

1960, stored as `tot_1960_cc_USA`) and PatentsPost_k (the number of climate-related patents associated with crop k between 1960-2020, stored as `tot_1960_2020_USA_cc`).

I use these crop-level innovation variables to construct direct measures of innovation exposure in the county-decade panel data. First, I compute 'variety exposure':

$$\text{VarietyExposure}_{i,t} = \sum_k \left[\frac{\text{NCrops}_{k,t} \times \text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right]. \tag{E10}$$

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

$$\text{PatentExposure}_{i,t} = \begin{cases} \sum_k \left[\frac{\text{PatentsPre}_k \times \text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right] & \text{if } t = 1950 \\ \sum_k \left[\frac{\text{PatentsPost}_k \times \text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right] & \text{if } t = 2010 \end{cases}. \tag{E11}$$

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

6 Results

6.1 Reduced-Form Estimates

[Table R–IV about here]

Table R–IV shows the results of models estimating Equation E1, where LOO EHE is replaced with $\text{VarietyExposure}_{i,t}$. Replacing MS23's proxy with a direct measure of innovation exposure virtually eliminates the moderating effects found in Table R–I. Across all models in Table R–IV, no moderating effect estimate is statistically significantly different from zero.

The moderating effect estimates in Table R–IV are microscopic compared to those estimates in Table R–I. This is not due to a difference in units; though the partial correlation coefficients of the moderating effect estimates in Table R–I, Panel C range from $0.156r$ to

$0.505r$, those coefficients in Table R–IV, Panel B range from $-0.137r$ to $0.034r$. Partial correlation coefficients do not lend well to linear comparisons; $r = 0.015$ is not one tenth of $r = 0.15$ in the same way that $r = 1.5$ is not ten times $r = 0.15$. However, standardized coefficients do permit such linear comparisons. The standardized coefficient estimates of moderating effects in Table R–IV, Panel C are at least 99.2% less than those moderating effect estimates in Table R–I, Panel D.

[Table R–V about here]

The same reduction in moderating effect estimates occurs when LOO EHE is replaced with $\text{PatentExposure}_{i,t}$ rather than $\text{VarietyExposure}_{i,t}$. Table R–V shows LD specifications estimating Equation E1 with $\text{PatentExposure}_{i,t}$ replacing LOO EHE. Again, no moderating effect estimates from this specification are statistically significantly different from zero. Partial correlation coefficients on the moderating effect estimates in this table range from $-0.136r$ to $0.01r$. The standardized coefficient estimates of moderating effects in Table R–V, Panel C are at least 99.8% less than those moderating effect estimates in Table R–I, Panel D. Across both direct measures of innovation exposure, there is no statistically significant evidence that innovation moderates EHE impacts on AL values.

[Figure R-III about here]

Figure R-III plots heterogeneous marginal effects of county-level EHE on AL values for selected quantiles of different moderating variables.[9] The estimates in the left, center, and right panels of Figure R-III are produced using marginal effects post-estimation on the results from Column 1 in Tables R–I, R–IV, and R–V (respectively). The left graph in Figure R-III replicates MS23's key findings, showing that the marginal effect of EHE on AL value attenuates toward zero for higher quantiles of LOO EHE. However, the middle and right graphs in Figure R-III 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 from Column 1 in Tables R–IV and R–V.

[9] These quantiles match those in Figure VI of MS23, which plots HTEs of local EHE on AL value by quantile of LOO EHE based on estimates from Model 1 of Table III.

MS24 contend that their original results 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.

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 that instrument innovation exposure and its interaction with $\text{ExtremeExposure}_{i,t}$ using a second-order polynomial of LOO EHE. I repeat this specification twice, each time with either variety or patent exposure as the measure of innovation exposure. This IV strategy reflects the intuition that if LOO EHE is a good exogenous proxy for innovation exposure, then it should also be a good instrument for innovation exposure.

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 4.2, 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 results. Sections 4.2 and 4.3 establish that LOO EHE closely tracks local EHE. Table R–II also shows that when local EHE is interacted with heat measures, the resulting interaction effect on AL value is positive. Therefore, after instrumenting $\text{InnovationExposure}_{i,t} \times \text{ExtremeExposure}_{i,t}$ with LOO EHE, the resulting interaction effect coefficient is most likely biased in the direction of MS23's original mitigatory impact estimates (i.e., biased upward).

[Table R–VI about here]

Table R–VI replicates Tables R–IV and R–V 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 R–VI are generally larger than the respective estimates in Tables R–IV and R–V, the first-stage F-statistics show that these estimates are biased not only by the aforementioned exclusion restriction violations, but also by weak instruments. The second-order polynomial of LOO heat exposure is a relatively weak instrument for innovation exposure and its interaction with $\text{ExtremeExposure}_{i,t}$; Kleibergen & Paap (2006) first-stage F-statistics in Table R–VI range from 0.135 to 7.583. This provides further evidence that LOO EHE is an inappropriate proxy for innovation exposure, as LOO EHE is not a particularly strong predictor of innovation exposure.

Despite the increased size of the moderating effect estimates, none of the estimates in Table R–VI provide clear support for a mitigatory moderating impact of innovation exposure on climate-driven land devaluation. The moderating effect estimates in Table R–VI are very noisy. The SEs of the standardized moderating effect estimates in Panel C of Table R–VI exceed those of the respective estimates in Panel C of Tables R–IV and R–V by at least 38%. Further, the moderating effect estimates themselves are much smaller. Considering standardized coefficients, the moderating effect estimates in Table R–VI decrease by at least 43% compared to their respective estimates in Table R–I. Nine of the 12 IV estimates in Table R–VI exhibit the opposite sign of their respective estimate in Table R–I. None of the moderating effect estimates in Table R–VI are statistically significantly different from zero.

7 Addressing Alternative Explanations

MS24 argue that these 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 market innovations endogenously adapt to climate change. However, there are reasons to doubt this relationship too, which I detail in Section 4.1.

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 4.2 and 4.3. Replications of the model in Equation E1 that replace LOO EHE with VarietyExposure$_{i,t}$ or 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.

MS24 also 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 their estimand of interest is the mitigatory impact of *directed* technological changes. That is, MS24 are interested in the 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 more innovation 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 4).

The local average treatment effect discussed in MS24 can be best estimated using IV models that instrument innovation with LOO EHE. Indeed, if the estimand of interest is the local average treatment effect of innovation specifically on counties that innovated more because of extreme heat, then this IV model is ideal (notwithstanding the weak instruments and exclusion restriction violations detailed in Section 6.2; see also Imbens & Angrist 1994). However, I directly pursue this IV strategy in Section 6.2, and none of the IV estimates are precise nor statistically significantly different from zero.

Further, MS24 argue that my direct innovation measures may imprecisely capture *climate-adaptive* innovation. Not all crop varieties and crop-related patents are developed for the purpose of adapting 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 5 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 doubt on MS23's findings that EHE drives climate-adaptive innovation in Section IV.

This concern is also addressed by my IV strategy. 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. Such variation should arise from climate-adaptive innovation. However, these IV models in Section 6.2 yield qualitatively identical results to the reduced-form estimates from Section 6.1.

Additionally, MS24 argue that most variation in my direct innovation measures is driven by a handful of profitable crops. This issue is related to the high concentration of innovation in a handful of crops that I discuss in Section 4.1. 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.[10]

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

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. In Online Appendix Tables A-X and A-XI, I replicate Tables R–IV and R–V after replacing $\text{VarietyExposure}_{i,t}$ and $\text{PatentExposure}_{i,t}$ with these alternate measures. This check actually decreases my mitigatory impact estimates to the point that all mitigatory impact estimates in Online Appendix Table A-X, and four of the five mitigatory impact estimates in Online Appendix Table A-XI, 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.

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

$$\begin{aligned}\log(\text{AgrLandPrice}_{i,t}) = {} & \delta_i + \alpha_{s(i),t} + \theta_1 \text{ExtremeExposure}_{i,t} \\ & + \theta_2 \text{InnovationExposure}_{i,t} + \theta_3 t \\ & + \theta_4 \left(\text{ExtremeExposure}_{i,t} \times \text{InnovationExposure}_{i,t}\right) \\ & + \theta_5 t \times \text{ExtremeExposure}_{i,t} \\ & + \theta_6 t \times \text{InnovationExposure}_{i,t} \\ & + \theta_7 \left(t \times \text{ExtremeExposure}_{i,t} \times \text{InnovationExposure}_{i,t}\right) \\ & + \Gamma X'_{i,t} + \epsilon_{i,t},\end{aligned} \tag{E12}$$

where $t \in \{0, 1, \cdots 6\}$ is a linear decadal time trend and $\text{InnovationExposure}_{i,t}$ is either $\text{VarietyExposure}_{i,t}$ or $\text{PatentExposure}_{i,t}$. In a similar fashion to Equation E8, I then compute the average moderating effect of innovation exposure on EHE-driven AL devaluation as

$$\mathbb{E}\left[\frac{\partial^2 \log(\text{AgrLandPrice}_{i,t})}{\partial \text{ExtremeExposure}_{i,t} \partial \text{InnovationExposure}_{i,t}}\right] = \hat{\theta}_4 + \hat{\theta}_7 \mathbb{E}\left[t\right], \tag{E13}$$

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

Online Appendix Tables A-XII and A-XIII 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 Tables R–IV and R–V. 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.

8 Conclusion

This paper shows that MS23's estimates of the mitigatory moderating impact of innovative agricultural adaptations 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, the mitigatory effect estimates I obtain are at least 99.2% less than those obtained by MS23, and none are statistically significantly different from 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. MS23 use their significant mitigatory impact estimates to project that innovation mitigated one fifth of all climate-driven AL devaluation since 1950, and that innovation will abate 13% of all AL 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 (Supplementary Data)

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 (2024b), replacing my variety exposure measure with their logarithmic variety exposure measure. Most of the models in MS24 produce statistically significantly positive estimates for the mitigatory impact of innovation on EHE-driven AL devaluation.

These methodological augmentations have several clear 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 exclusively estimate their models for decades after 1980. This departs from their analytical choices in MS23's Table III, eliminating over half of their temporal domain and over 57% of their 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 no 'logs with zeros' issue to correct. MS24's new models thus incur the robustness issues associated with log-like transformations unnecessarily (see Chen & Roth 2024).

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

$$\text{LP1VarietyExposure}_{i,t} = \sum_k \left[\log\left(1 + \text{NCrop}_{k,t}\right) \times \frac{\text{Area}^{\text{Pre}}_{i,k}}{\sum_{k'} \text{Area}^{\text{Pre}}_{i,k'}} \right], \tag{E14}$$

[1]This can be seen by comparing the observation counts in Online Appendix Table A-VI to those in Columns 6-7 in Table R–I.

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:

$$\text{LogVarietyExposure}_{i,t} = \sum_k \left[\log\left(\text{NCrop}_{k,t}\right) \times \frac{\text{Area}_{i,k}^{\text{Pre}}}{\sum_{k'} \text{Area}_{i,k'}^{\text{Pre}}} \right]. \tag{E15}$$

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 in the model 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 A-VI displays results from a replication attempt of Table 1 in MS24. Specifically, I run models of the form

$$\begin{aligned} \log(\text{AgrLandPrice}_{i,t}) = \delta_i &+ \alpha_{s(i),t} + \beta \text{ExtremeExposure}_{i,t} \\ &+ \gamma \text{LP1VarietyExposure}_{i,t} \\ &+ \phi(\text{ExtremeExposure}_{i,t} \times \text{LP1VarietyExposure}_{i,t}) \\ &+ \rho \text{LP1VarietyExposure}_{i,t_0-10} \\ &+ \sum_{t'=t_0}^{2010} \left[\zeta_{t'} \mathbb{1}\left[t = t'\right] + \eta_{t'} \mathbb{1}\left[t = t'\right] \times \text{LP1VarietyExposure}_{i,t_0-10} \right] + \epsilon_{i,t}. \end{aligned} \tag{E16}$$

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 E16 are respectively the coefficients on decadal fixed effects and their interactions with $\text{LP1VarietyExposure}_{i,t_0-10}$. Online Appendix Table A-VI 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 A-VII shows estimates from models of the form in Equation E16 that replace $\text{LP1VarietyExposure}_{i,t}$ with $\text{LogVarietyExposure}_{i,t}$. Estimates from this model are quite similar to those in Online Appendix Table A-VI, and the majority of interaction effects between local EHE and logarithmically-transformed 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 A-VIII and A-IX report estimates from alternative models of Columns 2-3, 5-6, and 8-9 of Online Appendix Tables A-VI and A-VII (respectively) after altering the 'pre-period log variety exposure' measure. Specifically, in models of the form in Equation E16, I replace $\text{LP1VarietyExposure}_{i,t_0-10}$ with $\text{LP1VarietyExposure}_{i,t-10}$. Thus in Online Appendix Table A-VIII (A-IX), instead of 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 $\text{LP1VarietyExposure}_{i,t}$ ($\text{LogVarietyExposure}_{i,t}$). Again, the first-stage F-statistics in the IV models of Online Appendix Tables A-VIII and A-IX are much weaker than those in Table 1 of MS24, and the majority of estimates for innovation's mitigatory impact on EHE-driven AL devaluation are not statistically significantly different from zero.

These reproduction attempts show that logarithmic variety exposure measures do not have robust positive mitigatory impacts on EHE-driven AL devaluation even if I cannot reproduce MS24's analysis exactly. Online Appendix Tables A-VI-A-IX 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 A-VI-A-IX 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 Online Appendix Tables

	(1)	(2)	(3)	(4)
County-level extreme exposure	1.145	1.166	0.462	0.486
	(0.019)	(0.02)	(0.016)	(0.017)
County fixed effects		X		X
State × 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 A-I: Relationship Between Extreme Heat and Local Temperatures

	(1)	(2)
Δ ExtremeExposure	0.002	0.007
	(0.005)	(0.005)
All baseline controls	X	X
Climate-related patents		X
Observations	64	64

Note: Replication of Table II in MS23 with alfalfa (and varieties thereof), barley, corn, soybeans, and tobacco removed. Standard errors are given in parentheses.

Table A-II: Table II Replication, Top Five Crops Removed

	(1)	(2)	(3)	(4)	(5)	(6)
Δ ExtremeExposure	0.011	0.012	0.011	0.015	0.02	0.029
	(0.004)	(0.004)	(0.004)	(0.008)	(0.009)	(0.008)
1950-2016 sample period	X	X	X	X	X	
1980-2016 sample period						X
Log area harvested	X	X	X	X	X	X
Preperiod climate controls		X	X	X	X	X
Preperiod varieties			X	X	X	X
Cut-off temp. and cut-off temp sq.				X	X	X
Average temperature change					X	
Observations	65	65	65	65	65	65

Note: Replication of MS23's Table I with corn, lettuce and romaine, soybeans, and wheat removed. Though tomatoes are also a 'top five' crop in varieties, they cannot be removed because they were never part of the sample for Table I; MS23 lack LD data on EHE for tomatoes. Standard errors are displayed in parentheses.

Table A-III: Table I Replication, Top Five Crops Removed

	Linear Coefficient	Elasticity	Linear Coefficient	Elasticity
LOO extreme heat exposure	0.994	1.002		
	(0.018)	(0.023)		
Leave-state-out extreme heat exposure			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 A-IV: Relationships Between Extreme Heat Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Published Estimates							
County-level extreme exposure	-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 extreme exposure ×	0.259	0.445	0.247	0.261	0.24	0.0923	0.13
innovation exposure	(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 extreme exposure	-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 extreme exposure ×	0.181	0.389	0.154	0.173	0.148	0.0771	0.145
innovation exposure	(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 A-V: MS23's Table A18 and a Replication Attempt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Published estimates									
County-level extreme exposure	-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 extreme exposure ×	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 extreme exposure	-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 extreme exposure ×	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 extreme exposure	-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 extreme exposure ×	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 extreme exposure	-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 extreme exposure ×	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 E14. 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 A-VI: Reproduction of MS24, $\log(1+x)$-Transformed Variety Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Published estimates									
County-level extreme exposure	-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 extreme exposure ×	0.13	0.128	0.135	0.0555	0.06	0.0941	0.0834	0.118	0.274
logarithmically-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 extreme exposure	-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 extreme exposure ×	0.01017	0.00612	0.01674	0.00462	0.00364	-0.01227	0.00231	8e-05	0.10334
logarithmically-transformed 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 extreme exposure	-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 extreme exposure ×	0.2659	0.16172	0.096	0.17414	0.12238	-0.07299	0.06013	0.0018	0.16343
logarithmically-transformed 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 extreme exposure	-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 extreme exposure ×	0.50606	0.30475	0.83297	0.2298	0.18108	-0.61044	0.13726	0.00469	6.142
logarithmically-transformed 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. Logarithmically-transformed variety exposure is computed using the formula in Equation E15. Additional controls include the logarithmically-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 A-VII: Reproduction of MS24, Logarithmically-Transformed Variety Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Published estimates						
County-level extreme exposure	-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 extreme exposure ×	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 extreme exposure	-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 extreme exposure ×	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 extreme exposure	-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 extreme exposure ×	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 extreme exposure	-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 extreme exposure ×	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 × 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 E14. 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 A-VIII: Alternate Reproduction of MS24, $\log(1+x)$-Transformed Variety Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Published estimates						
County-level extreme exposure	-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 extreme exposure ×	0.128	0.135	0.06	0.0941	0.118	0.274
logarithmically-transformed variety exposure	(0.0361)	(0.0677)	(0.0251)	(0.0749)	(0.0411)	(0.133)
Panel B: Raw coefficients						
County-level extreme exposure	-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 extreme exposure ×	0.00739	0.01598	0.00404	-3e-05	-0.00022	0.34641
logarithmically-transformed variety exposure	(0.00326)	(0.01448)	(0.0024)	(0.02165)	(0.00447)	(2.13809)
Panel C: Partial correlations						
County-level extreme exposure	-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 extreme exposure ×	0.18639	0.09192	0.13911	-0.00011	-0.00495	0.01662
logarithmically-transformed variety exposure	(0.08072)	(0.08292)	(0.08201)	(0.08362)	(0.1026)	(0.10257)
Panel D: Standardized coefficients						
County-level extreme exposure	-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 extreme exposure ×	0.36801	0.79557	0.20083	-0.0014	-0.01282	20.59002
logarithmically-transformed 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. Logarithmically-transformed variety exposure is computed using the formula in Equation E15. Additional controls include the first lag of logarithmically-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 A-IX: Alternate Reproduction of MS24, Logarithmically-Transformed Variety Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Raw coefficients							
County-level extreme exposure	-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 extreme exposure ×	-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 extreme exposure	-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 extreme exposure ×	-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 extreme exposure	-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 extreme exposure ×	-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 specification in Equation E1, where InnovationExposure$_{i,t}$ is replaced with a variant of InnovationExposure$_{i,t}$ that omits corn, lettuce and romaine, soybeans, tomatoes, and wheat are presented alongside SEs double-clustered at the county and state-year levels in parentheses.

Table A-X: Mitigatory Impacts of Variety Exposure, No 'Top Five' Crops

	(1)	(2)	(3)	(4)	(5)
Panel A: Raw coefficients					
County-level extreme exposure	-0.37902	-0.4298304	-0.3783317	-0.2111482	-0.2616331
	(0.1141425)	(0.142894)	(0.1212683)	(0.1189039)	(0.1259963)
County-level extreme exposure ×	-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 extreme exposure	-0.3623624	-0.324456	-0.3378589	-0.1852934	-0.2180518
	(0.0891261)	(0.0917972)	(0.0908864)	(0.0990753)	(0.0977197)
County-level extreme exposure ×	-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 extreme exposure	-0.2659207	-0.3015693	-0.2652131	-0.1481417	-0.1834065
	(0.0800825)	(0.1002546)	(0.0850099)	(0.083423)	(0.0883243)
County-level extreme exposure ×	-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 specification in Equation E1, where $\text{InnovationExposure}_{i,t}$ is replaced with a variant of $\text{PatentExposure}_{i,t}$ that omits alfalfa (and varieties thereof), barley, corn, soybeans, and tobacco are presented alongside SEs double-clustered at the county and state-year levels in parentheses.

Table A-XI: 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 $\times$ 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. ($^\circ$ C) and interactions				X	X		

Note: Average moderating effects are calculated by passing Equation E12 through the `lincom` suite in Stata after estimating a model of the form in Equation E13, where variety exposure is the innovation exposure measure of interest. SEs double-clustered at the county and state-year levels are presented in parentheses.

Table A-XII: 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 $\times$ decade fixed effects	X	X	X	X	X
Weighted by agricultural land area		X			
Output prices and interactions			X		X
Avg. temp. ($^{\circ}$ C) and interactions				X	X

Note: Average moderating effects are calculated by passing Equation E12 through the `lincom` suite in Stata after estimating a model of the form in Equation E13, where patent exposure is the innovation exposure measure of interest. SEs double-clustered at the county and state-year levels are presented in parentheses.

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

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Tables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Published estimates							
County-level extreme exposure	-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 extreme exposure ×	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 extreme exposure	-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 extreme exposure ×	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 extreme exposure	-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 extreme exposure ×	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 extreme exposure	-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 extreme exposure ×	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 specification in Equation E1 are presented alongside SEs double-clustered at the county and state-year levels in parentheses.

Table R–I: Reproduction of Table III

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Raw coefficients							
County-level extreme exposure	-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 extreme exposure)^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 extreme exposure	-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 extreme exposure)^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 extreme exposure	-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 extreme exposure)^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 specification in Equation E6 are presented alongside SEs double-clustered at the county and state-decade levels in parentheses.

Table R–II: Fitting a Second-Order Polynomial in Extreme Heat Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Average moderating effect	0.139	0.376	0.114	0.118	0.103	0.15	0.221
of LOO EHE	(0.118)	(0.138)	(0.114)	(0.123)	(0.123)	(0.044)	(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 $\times$ 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. ($^\circ$ C) and interactions				X	X		

Note: Average moderating effects are calculated based on results from a model of the form in Equation E8, running the formula in Equation E9 through Stata's `lincom` command. All models incorporate 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 R–III: Average Moderating Effect of LOO EHE on EHE-Induced AL Devaluation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Raw coefficients							
County-level extreme exposure	-0.3967733	-0.5056133	-0.4126515	-0.30271	-0.3169444	-0.0228589	-0.0885235
	(0.1107134)	(0.1332734)	(0.1068332)	(0.1212079)	(0.1223538)	(0.0663453)	(0.0962329)
County-level extreme exposure ×	-3.5e-06	5.4e-06	-1.86e-05	-1e-06	-2.67e-05	-6.2e-06	9e-07
variety exposure	(9.6e-06)	(1.61e-05)	(2.38e-05)	(9.2e-06)	(2.03e-05)	(7.2e-06)	(1.1e-05)
Panel B: Partial correlations							
County-level extreme exposure	-0.3953861	-0.4225602	-0.431632	-0.2650822	-0.2756833	-0.0188278	-0.0503225
	(0.0865587)	(0.0842783)	(0.0834832)	(0.0953884)	(0.0948003)	(0.0546165)	(0.0544975)
County-level extreme exposure ×	-0.0372501	0.034496	-0.0803705	-0.0117239	-0.1365211	-0.0465052	0.0042307
variety exposure	(0.1024555)	(0.1024757)	(0.1019351)	(0.1025837)	(0.1006856)	(0.0545177)	(0.0546349)
Panel C: Standardized coefficients							
County-level extreme exposure	-0.2783764	-0.3547386	-0.2892716	-0.2123816	-0.2221802	-0.02086	-0.0807825
	(0.0776766)	(0.0935047)	(0.0748909)	(0.0850395)	(0.0857709)	(0.0605437)	(0.0878177)
County-level extreme exposure ×	-0.005034	0.0078194	-0.0268635	-0.0015185	-0.0386643	-0.0094378	0.001311
variety exposure	(0.0138748)	(0.0232426)	(0.0344035)	(0.01329)	(0.0293264)	(0.0110998)	(0.0169298)
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 specification in Equation E1, where LOO EHE is replaced with $\text{VarietyExposure}_{i,t}$, are presented alongside SEs double-clustered at the county and state-year levels in parentheses.

Table R–IV: Mitigatory Impacts of Variety Exposure

	(1)	(2)	(3)	(4)	(5)
Panel A: Raw coefficients					
County-level extreme exposure	-0.4220814	-0.5217386	-0.4485648	-0.3207568	-0.3215077
	(0.1113025)	(0.1388546)	(0.1133408)	(0.119662)	(0.1292944)
County-level extreme exposure × patent exposure	-2e-06	4e-07	-7.9e-06	-8e-07	-9.8e-06
	(2.7e-06)	(4.6e-06)	(8.5e-06)	(2.5e-06)	(7.5e-06)
Panel B: Partial correlations					
County-level extreme exposure	-0.4223497	-0.4177993	-0.4443255	-0.286046	-0.2638545
	(0.0842965)	(0.0846887)	(0.0823424)	(0.094203)	(0.0954551)
County-level extreme exposure × patent exposure	-0.0742286	0.0099249	-0.0958975	-0.0342088	-0.1361847
	(0.1020325)	(0.1025877)	(0.1016543)	(0.1024778)	(0.100695)
Panel C: Standardized coefficients					
County-level extreme exposure	-0.2961326	-0.3660521	-0.314447	-0.2250432	-0.2253791
	(0.0780899)	(0.0974205)	(0.0794527)	(0.0839549)	(0.0906363)
County-level extreme exposure × patent exposure	-0.0086029	0.0019263	-0.0343705	-0.0036543	-0.0425796
	(0.0119236)	(0.0199124)	(0.0369407)	(0.0109664)	(0.0323744)
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	0.988	0.99	0.989	0.988	0.989

Note: The dependent variable in all models is logarithmic AL values. Estimates of the specification in Equation E1, where LOO EHE is replaced with PatentExposure$_{i,t}$ and $t \in \{1950, 2010\}$, are presented alongside SEs double-clustered at the county and state-year levels in parentheses.

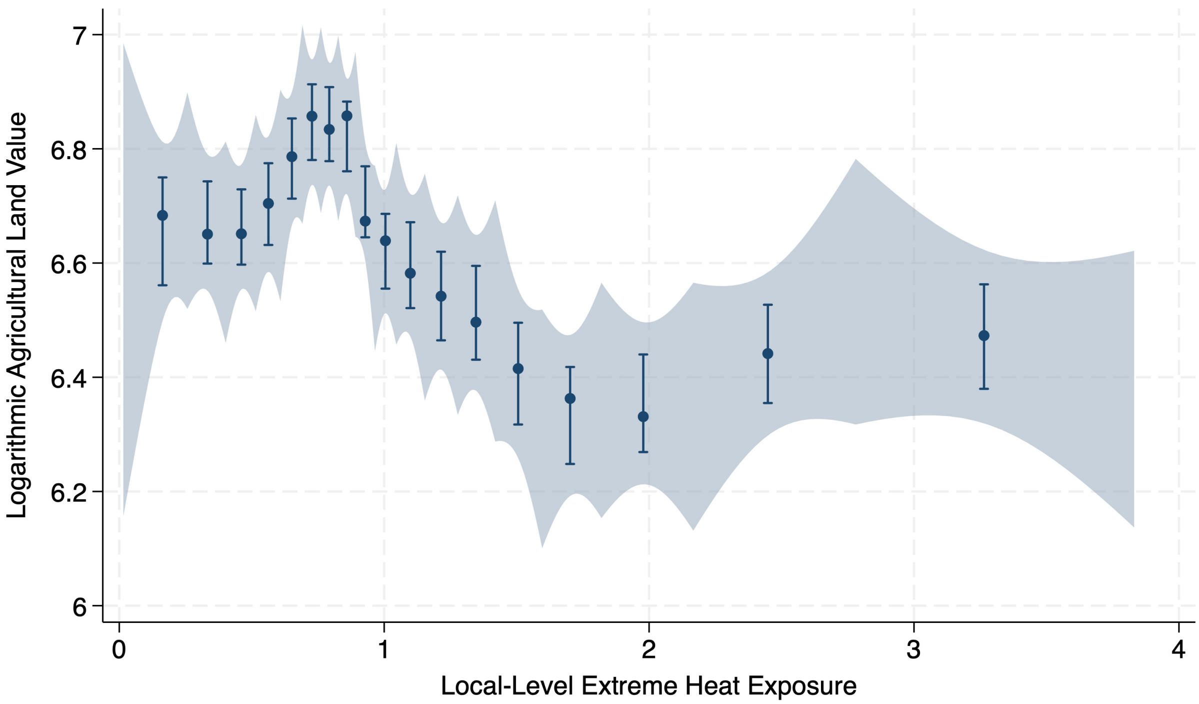
Table R–V: Mitigatory Impacts of Patent Exposure

	(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												
County-level extreme exposure	0.04751	-0.1973	-0.07448	-0.3257	0.12248	0.13951	-0.05239	-0.08358	-0.11345	-0.12403	-0.18451	-0.09377
	(0.25192)	(0.24639)	(0.18544)	(0.42494)	(0.38848)	(0.52994)	(0.71966)	(0.27675)	(1.07558)	(0.22589)	(0.28401)	(0.33166)
County-level extreme exposure ×	2e-05	3e-05	2e-05	0.00012	0.00014	-1e-05	-3e-05	2e-05	0.00013	1e-05	2e-05	2e-05
innovation exposure	(4e-05)	(6e-05)	(6e-05)	(8e-05)	(0.00013)	(0.00016)	(0.00035)	(2e-05)	(0.00033)	(3e-05)	(1e-05)	(4e-05)
Panel B: Partial correlations												
County-level extreme exposure	0.01935	-0.08244	-0.04124	-0.07888	0.03233	0.01438	-0.00398	-0.031	-0.01082	-0.05642	-0.0668	-0.02902
	(0.10256)	(0.1019)	(0.10242)	(0.10196)	(0.10249)	(0.05462)	(0.05463)	(0.1025)	(0.10259)	(0.10227)	(0.10214)	(0.10251)
County-level extreme exposure ×	0.05878	0.05489	0.0411	0.15189	0.11716	-0.00434	-0.00511	0.08463	0.04144	0.03394	0.18175	0.06308
innovation exposure	(0.10224)	(0.10229)	(0.10242)	(0.10023)	(0.10119)	(0.05463)	(0.05463)	(0.10186)	(0.10242)	(0.10248)	(0.09921)	(0.10219)
Panel C: Standardized coefficients												
County-level extreme exposure	0.03334	-0.13843	-0.05221	-0.22851	0.08586	0.12731	-0.04781	-0.05864	-0.07959	-0.08695	-0.12945	-0.06573
	(0.17674)	(0.17287)	(0.12999)	(0.29814)	(0.27233)	(0.4836)	(0.65673)	(0.19417)	(0.75462)	(0.15835)	(0.19926)	(0.2325)
County-level extreme exposure ×	0.0354	0.0493	0.03534	0.17487	0.2082	-0.02007	-0.05085	0.06993	0.57873	0.04428	0.10482	0.10644
innovation exposure	(0.06168)	(0.09202)	(0.08814)	(0.11675)	(0.18107)	(0.25265)	(0.54382)	(0.08447)	(1.43144)	(0.13379)	(0.05819)	(0.17278)
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 VarietyExposure$_{i,t}$ or PatentExposure$_{i,t}$, depending on the column). Both innovation exposure and its interaction with ExtremeExposure$_{i,t}$) are instrumented by a second-order polynomial of LOO EHE. SEs double-clustered at the county and state-year levels are in parentheses. F-statistics are computed in accordance with Kleibergen & Paap (2006).

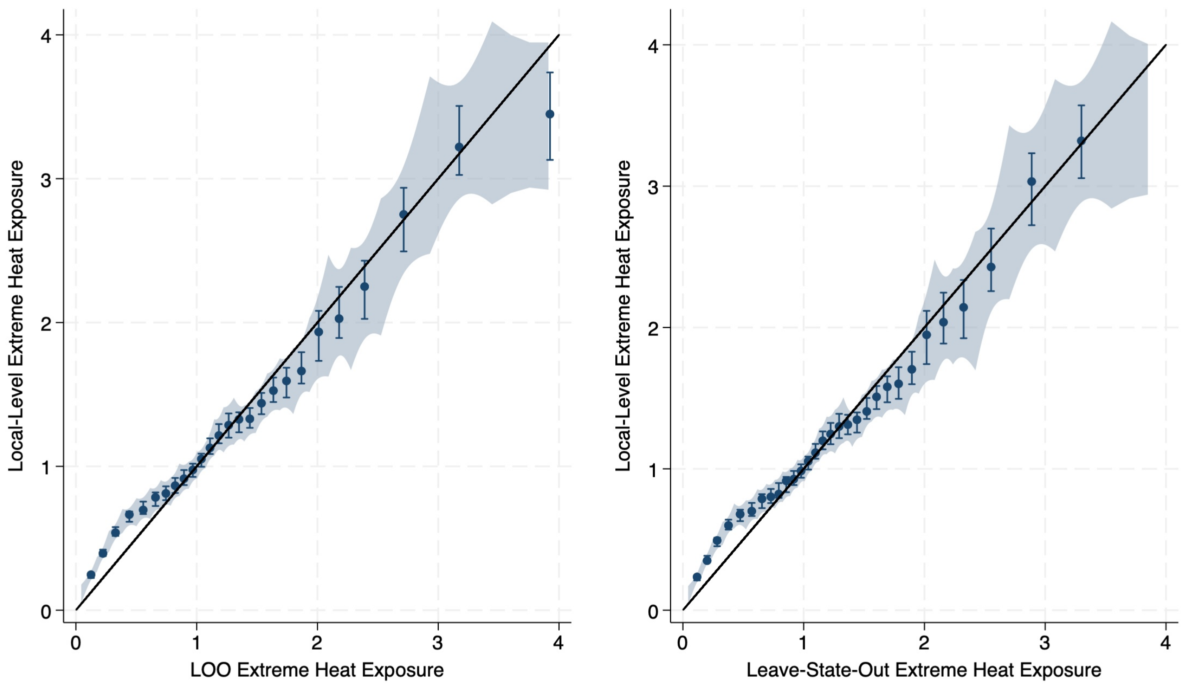
Table R–VI: Instrumental Variables Estimates of Innovation Exposure's Mitigatory Impacts

Figures



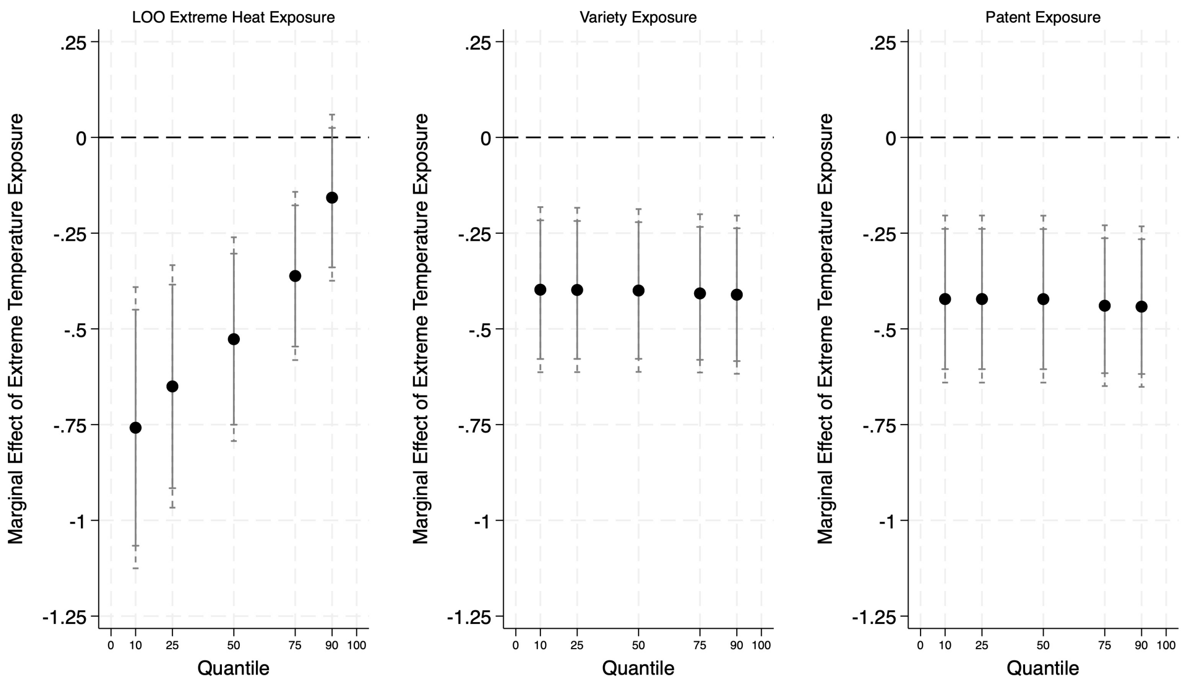
Note: The graph shows a binscatter regression plot (see Cattaneo et al. 2024) showing the relationship between logarithmic AL values and county-level EHE for ExtremeExposure$_{i,t} \in [0, 4]$; this restriction, instituted to improve interpretability, covers 95.3% of the distribution of ExtremeExposure$_{i,t}$.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

Figure R-I: Dynamics of Agricultural Land Value and Local Extreme Heat Exposure



Note: The graph shows binscatter regression plots (see Cattaneo et al. 2024) showing the relationships between county-level EHE and both LOO EHE (left) and leave-state-out EHE (right). The ranges of both LOO and leave-state-out EHEs are restricted to $[0, 4]$ to improve interpretability; this range covers 98.8% and 98.5% of the distributions of LOO and leave-state-out EHEs (respectively). 95% confidence bands and intervals are constructed with SEs clustered at the county level. A simple 45-degree line is appended to the graph for reference.

Figure R-II: Relationship between County-Level and LOO Extreme Heat Exposure



Note: The graphs display extrapolated marginal impacts of county-level EHE on AL values for selected quantiles of different moderators (indicated in the titles of each graph). The left, middle, and right graphs are respectively constructed based on Column 1 estimates from Panel B in Table R–I, Panel A in Table R–IV, and Panel A in Table R–V.

Figure R-III: HTEs of Extreme Heat Exposure