The Perils of Problematic Proxies: Does Innovation Mitigate Agricultural Damage from Climate Change?

Jack Fitzgerald*

August 15, 2024

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. However, MS23's 'innovation exposure' variable does not measure innovation, instead proxying innovation using a measure of crops' national heat exposure. A re-examination of MS23's replication data – which permits a close but inexact reproduction of MS23's published findings – shows that this proxy moderates EHE impacts for reasons unrelated to innovation. The proxy is practically identical to local EHE, so MS23's models examining interaction effects between their proxy and local EHE effectively interact local EHE with itself. I document extensive evidence that MS23's findings on 'innovation exposure' are simply artefacts of nonlinear impacts in local EHE, 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 99.8% 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.

JEL Codes: O31, Q10, Q54.

Word count: 8218

*Email: j.f.fitzgerald@vu.nl. Address: De Boelelaan 1105, Amsterdam, 1081 HV, The Netherlands. I thank Jacob Moscona and Karthik Sastry for answering questions about their article and providing comments on this response, as well as the Institute for Replication for facilitating communication. I also thank Alan Barreca, Abel Brodeur, and Derek Mikola for helpful feedback. This project has approval from the Ethical Review Board at Vrije Universiteit Amsterdam's School of Business and Economics. The data and code necessary to reproduce these findings can be found at https://osf.io/d7wz9/.

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 in 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 1950-2020, 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 estimate that technological innovation offset roughly 20% of EHE-driven AL devaluation from 1960-2020, 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). MS23's replication repository, which fully provides both raw and analysis data, permits a close but inexact (i.e., partial) computational reproduction of their published findings. Using this data, I show 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 coefficient on the interaction term in MS23's HTE models does not reflect mitigatory impacts of innovation on EHE-induced AL devaluation, instead reflecting 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 this nonlinearity is the effect reflected by MS23's interaction effect estimates in two 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 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 decreases MS23's estimates of interest by at least 87.8%, and almost none of these new estimates are 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 HTE models, replacing MS23's proxy with these direct measures of innovation, and find no moderating effect estimate that is statistically significantly different from zero. The effect sizes of the moderating effect estimates in my replications are minuscule compared to MS23's estimates. The standardized coefficients of my moderating effect estimates are at least 99.8% less than the standardized coefficients of MS23's moderating effect estimates.

I supplement these reduced-form specifications with an instrumental variables strategy which instruments my direct innovation measures with MS23's exogenous heat-based proxy. Intuitively, if MS23's heat proxy 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, and despite clear exclusion restriction violations that are likely favorable to MS23's conclusions, these instrumental variables estimates are at least 50% smaller in standardized units than MS23's main estimates.

These estimates remain negligible across many specifications and in the face of many robustness checks. 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.

Section 2 of this paper overviews MS23's replication repository, the main published estimates of interest, and the reproducibility of such estimates. 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. Thereafter, Section 4 introduces the two direct innovation measures I construct from MS23's replication data. Section 5 displays the results after re-estimating MS23's HTE models using my direct innovation measures, and Section 6 concludes.

2 Data, Published Findings, and Reproducibility

My analyses rely on MS23's replication repository (Moscona & Sastry 2022). Appendix Table A-I details the completeness of the repository, with specific regard to its completeness for producing Table III, Figure VI, and Appendix Tables A18 and A20. Raw data appears to be fully available. However, there does not appear to be sufficient cleaning code to convert the raw data into cleaned datasets. Specifically, no instructions are provided in the repository's ReadMe file for reproducing dataset `county_level_data.dta`. This file – the analysis data for the replications of interest – is made available in the replication repository. However, the repository's analysis code contains minor errors.

Importantly, the repository lacks code for several results in the appendix. Two results of importance to this analysis for which code is missing include Table A18 and Table A20. These omissions lead to replication failures in the appendix; I defer discussion of this issue to Section 3.1.4. For all of the above reasons, I thus classify the results as partially computationally reproducible, and discuss these coding errors later in this section.

2.1 Table III

The main replication of interest to this report concerns the mitigatory effects of technological market innovations on EHE-induced AL devaluation. Let i index the county and t index the decade. By Equation 18, MS23's relevant estimates for the damage mitigation effects of technological innovation in Table III arise from a simple HTE model of the form

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

where $\log(\text{AgrLandPrice}_{i,t})$ 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 covariates. 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 $t \in \{1950, 1960, \cdots 2010\}$. To provide an example of indexing, $t = 1950$ implies that the observation covers all years between 1950-1959, inclusive of endpoints. 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 of interest for MS23's findings concerning the mitigatory impact of technological innovation on climate change damage. Table R–I shows $\hat{\beta}$ and $\hat{\phi}$ estimates, with standard errors (SEs) double-clustered at the county and state-decade levels.[1] Panel A directly copies the published results from MS23, and Panel B shows the results from my reproduction, confirming that the replication repository permits a nearly exact reproduction of Table III. 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.[2]

MS23 obtain significantly positive estimates for ϕ, and interpret this to mean that croplands which are 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, in line with Fitzgerald (2024). Panel C converts the estimates into partial correlation coefficients r, and subsequently into SE(r), using the following representation (van Aert & Goos 2023):

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

where 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).

[1] 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.

[2] When approached with this matter, the authors contended that the difference in observation counts is attributable to a package update to `reghdfe`, with my newer version handling collinear observations in a different manner than the older version used by MS23. However, this is not currently possible to verify, as MS23 do not report package versions in their documentation. I reproduce these results using `reghdfe` version 6.12.5, updated as of 27 December 2023.

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

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

where σ_D and σ_Y respectively represent the within-sample standard deviations of D and Y.[3] 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).

2.2 Figure VI

[Figure R-I about here]

MS23's Figure VI visualizes the marginal impact of county-level EHE on AL values across selected quantiles of InnovationExposure$_{i,t}$. I reproduce Figure VI, along with two corrections thereof, in Figure R-I. The left graph in Figure R-I reproduces Figure VI based on MS23's replication code, though like the middle and right graphs, the y-axis of this graph is given a custom scale to ensure uniform y-axis scaling across all three graphs.

I detect a very minor error in MS23's construction of Figure VI: the values used to construct the quantiles are incorrectly hard-coded into the do-file. Specifically, in `county_level.do`, MS23 extrapolate marginal effects using Stata's `lincom` command, multiplying coefficient estimates by hard-coded numbers intended to represent the 10th, 25th, 50th, 75th, and 90th percentiles of InnovationExposure$_{i,t}$. These hard-coded numbers are incorrect, though they are extremely close to the quantiles that I estimate.

The middle and right graphs in Figure R-I present extrapolated treatment effects after quantiles are corrected in one of two ways. The middle graph arises from corrected quantiles of InnovationExposure$_{i,t}$ based on the full data sample, whereas the right graph arises from corrected quantiles of InnovationExposure$_{i,t}$ based only on observations from 1950 and 2010

[3] 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.

(i.e., those in the LD specification). This correction is quite minor; the same conclusions arise for all three graphs, and the published version looks very similar to the right hand graph.

All three graphs in Figure R-I show that the marginal impact of EHE on logarithmic AL values is significantly negative for most of the distribution of $\text{InnovationExposure}_{i,t}$. However, this negative effect diminishes as $\text{InnovationExposure}_{i,t}$ increases. For sufficiently high quantiles of $\text{InnovationExposure}_{i,t}$, the impact of EHE on logarithmic AL values is not statistically significantly different from zero.

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:

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

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:

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

Though MS23 are transparent about the calculation of $\text{InnovationExposure}_{i,t}$ in the paper, this variable does not measure innovation; it measures heat. As MS23 write (pgs. 678-

679): "[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." 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-II about here]

This context completely changes the interpretation of ϕ in Equation E1, which will be 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-II displays the relationship that $\hat{\phi}$ is most likely capturing in Table III. In particular, Figure R-II presents a binscatter regression plot between county-level EHE and logarithmic AL values. Each bar represents a quantile bin of the distribution of $\text{ExtremeExposure}_{i,t}$, so x-axis regions with more (fewer) bars 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 3.1.2), 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-II. 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.

Such nonlinear dynamics between heat and AL value have been established in prior literature, including literature cited by MS23. 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-II is fairly similar to the nonlinear relationships between temperatures and crop yields that Schlenker & Roberts (2008; 2009) find for corn, soybeans, and cotton.[4]

This context also explains the findings in Figure VI. As EHE increases, the marginal damages induced by additional local extreme heat shocks diminish, and for sufficiently high quantiles of EHE, the marginal harms of additional extreme heat shocks disappear. This is intuitive: to provide an extreme example, if a county's temperatures are so hot that the county's croplands are completely unsuitable for agriculture, then additional local increases in temperature will have virtually zero impact on such croplands' AL value. However, this intuitive moderating relationship has nothing to do with innovation.

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 produce specifications akin to Equation E1, but replacing 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,

[4] One potential objection to this comparison is that Schlenker & Roberts (2008; 2009) are examining nonlinear relationships in raw temperature, whereas Figure R-II 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 capturing the same underlying nonlinearity, as both EHE and local temperatures exhibit strong positive correlations, which is naturally intuitive given that they are both measures of heat. Appendix Table A-II shows panel data regressions confirming that each 1000 additional crop-weighted growing degree days of EHE is associated with a 1.075-1.166 degree Celsius increase in average local temperatures. The t-statistics for these regression coefficients range from 28-58.

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.

3.1 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 their 'innovation exposure' 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 3.1.1, 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 3.1.2, 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 3.1.3, I show similar results for MS23's leave-state-out proxy. Finally, in Section 3.1.4, 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 on the robustness of MS23's mitigatory impact estimates to controlling for nonlinear functions of local EHE. 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.

3.1.1 Heat as a Predictor of Climate-Adaptive Innovation

MS23 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.[5] 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 4 and 5.

Even setting aside the issue of whether a proxy is necessary, and even beyond the overarching issues outlined earlier in Section 3, there are two additional reasons to question whether MS23's results in Section IV justify using LOO EHE as a direct measure of 'innovation exposure'. First, not all crop innovation is related to climate adaptation. For the purposes of this replication, this is not a particularly serious issue for patents, as MS23 already distinguish between climate-related and climate-unrelated patents. However, crop varieties are not similarly separated in MS23's data, and not all crop varieties are developed for the purpose of adapting to climate change. More varieties may be developed for crops grown in the regions impacted most by climate change for reasons unrelated to climate adaptation. This prospect would imply that the correlations between EHE and variety/patent development found in MS23's Section IV do not provide evidence of markets developing new crop varieties to adapt to climate change, instead implying that this innovation emerged for unrelated reasons.

It is actually quite plausible that more varieties are developed for crops grown in counties exposed to more extreme heat for reasons unrelated to climate adaptation, namely because crops grown in these locations are more financially lucrative. MS23's Appendix Figure A5(a) shows local EHE by county from the 1950s through the 2010s. Most of the counties that are most heavily impacted by EHE are located on the Pacific and Atlantic coastlines, with additional EHE concentration in several western inland states such as Idaho and Colorado. Appendix Figure A-I shows each county's average value of sold crops per acre of harvested cropland, based on data from the U.S. Department of Agriculture's 2017 Census of Agricul-

[5] 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."

ture (the most recent census fielded before the end of MS23's time horizon). Cross-referencing MS23's Appendix Figure A5(a) with my Appendix Figure A-I shows that the counties where crops are most lucrative align relatively closely with the counties that have experienced the largest EHE shocks.

This is an economically intuitive consequence of baseline geographical features. In the U.S., coastline states are well-known for exhibiting higher economic productivity, economic activity, and population density than inland states, and thus the opportunity cost of casting land for crop growth is higher on the coasts than it is further inland. Because of this, a crop must be particularly lucrative in order for land in a coastal county to be set aside for growing that crop. It is thus sensible that the crops grown in these coastal counties receive more investment into variety development because these crops are more valuable per unit of land, and thus marginal improvements in yield provide more promising financial results per acre of land for these crops than for crops grown in inland states. It is plausible that this is a primary factor explaining why crops grown in coastal counties see more (successful) investment into innovation, and that the higher EHE in the counties that grow these crops is a sheer coincidence. This casts doubt on the appropriateness of LOO EHE as a proxy for *climate-adaptive* innovation exposure.

Ruling out the prospect that MS23's key estimates of interest are driven by innovations unrelated to climate adaptation requires an instrumental variables strategy whereby direct innovation measures are instrumented by exogenous (LOO) EHE shocks, and I explicitly pursue this strategy in Section 5.2. However, the results of this instrumental variables analysis are generally unfavorable for MS23's conclusions. First-stage F-statistics show that LOO EHE is a weak instrument for the direct innovation measures that I construct in Section 4. Additionally, the instrumental variables estimates themselves yield mitigatory impact estimates that decrease in size by more than half compared to those same estimates in MS23's Table III (see Section 5.2 for more details).

The second further issue with interpreting the results from Section IV in MS23 as evidence that EHE induces climate-adaptive innovation is that innovation in MS23's data is concentrated in a handful of crops. 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, and 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.[6] 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, as 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. Appendix Table A-III 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 53%, 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. Appendix Table A-IV 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. The fact that the EHE-innovation relationship is neither particularly strong nor certainly causal creates serious doubts for the validity of such a proxy.

[6] 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.

3.1.2 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-III about here]

There is strong evidence that MS23's LOO EHE measure closely proxies county-level EHE. The left-hand graph in Figure R-III 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.

Models 1 and 2 in Appendix Table A-V 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.

3.1.3 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.[7] 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 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 Appendix Table A20 are 20.2-22.9% smaller than these same estimates in Table III. In the panel data models, the moderating effect estimates of interest in Table A20 are 5.5-9% 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-III 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 Appendix Table A-V 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 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 Appendix Table A20. However, the

[7] 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`.

differences in the relationships with local EHE between the LOO and leave-state-out EHEs are minor (see Section 3.1.2). Like LOO EHE, leave-state-out EHE is still strongly positively correlated with local EHE.

Leave-state-out computation of the proxy fails to purge the proxy of strong correlations with local EHE for the same reason that LOO computation fails to purge these correlations: EHE is assigned at a higher level than either the county or the state. Appendix Figure A-II shows average mean temperature deviations from historical climatic averages by U.S. county in June 2018. June sits in the middle of the April-October growing season targeted by MS23, and 2018 sits near the end of the time horizon in MS23's analysis. The figure shows that large contiguous swaths of the U.S. – including much of the Midwest and Southwest – experienced mean temperature anomalies within the same 3 degree Fahrenheit (≈ 1.667 degree Celsius) band of one another. Most of the remainder of the U.S. experienced temperature anomalies within a ± 3 degree Fahrenheit band of zero. Removing one state from the proxy's computation does not overcome EHE spillovers arising from these broader climatic patterns, which I show empirically in Figure R-III and Appendix Table A-V.

3.1.4 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 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 Appendix Table A18, but the table appears identical to Table III with the exception that all models in Appendix Table A18 control for $\text{ExtremeExposure}^2_{i,t}$. The models in Appendix Table A18 are thus akin to Equation E1, taking the form

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

whre ϕ remains the estimate of interest.

However, Appendix Table A18 fails to replicate, and this replication failure is important for MS23's claims. Appendix Table A-VI juxtaposes the published version of Appendix Table A18 against my best attempt to replicate the table. The published version of 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 Appendix Table A18 reveals that controlling for $\text{ExtremeExposure}^2_{i,t}$ actually *decreases* MS23's mitigatory impact estimates by 4.7-24.7%. Controlling for higher-order polynomials in EHE thus attenuates the estimates of interest; MS23's published estimates in Appendix Table A18 are in this sense misleading.

Additionally, the model used to demonstrate the robustness of MS23's mitigatory impact estimates 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 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 Equation E7 is thus incorrectly saturated, in a manner akin to running a triple-differences model without its triple 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:

$$\begin{aligned}\frac{\partial^2 \log(\text{AgrLandPrice}_{i,t})}{\partial \text{EHE}_{i,t} \partial \text{LOO}_{i,t}} &= \phi_1 + 2\phi_2 \text{EHE}_{i,t} \\ \mathbb{E}\left[\frac{\partial^2 \log(\text{AgrLandPrice}_{i,t})}{\partial \text{EHE}_{i,t} \partial \text{LOO}_{i,t}}\right] &= \hat{\phi}_1 + 2\hat{\phi}_2 \mathbb{E}\left[\text{EHE}_{i,t}\right].\end{aligned} \tag{E9}$$

$\mathbb{E}\left[\text{EHE}_{i,t}\right]$ can be simply computed as a within-sample mean of $\text{EHE}_{i,t}$. Note that MS23's moderating effect estimates in Equation E7 arise exclusively from ϕ, which is akin to ϕ_1 in Equation E8. Thus given the average moderating effect identified in Equation E9, MS23's estimates in Appendix Table A18 do not fully identify the moderating effect 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 11.5-55.2%. 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 64.5-72.7%. 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.

4 Direct Measures of Innovation Exposure

As discussed in Section 3.1, 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, Table I and Figure IV show that 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, Table II and Figure V show that 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`).[8]

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':

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

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,

[8]Though I contacted the authors to request crop-decade panel data on patent development, the authors informed me that crop-decade panel data was never stored, and patent data was only scraped for use in LD specifications.

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 data is only available on patenting in these two time periods. It is thus only possible to replicate LD estimates of Equation E1 using patent exposure.

5 Results

5.1 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 $0.091r$ to $0.425r$, 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 99.8% less, and at least 93% smaller, 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, and at least 98.5% smaller, 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-IV about here]

Figure R-IV plots HTEs of county-level EHE on AL values for selected quantiles of different moderating variables. This figure is computed differently than Figure VI; while Figure VI is calculated using `lincom`, I directly estimate the marginal effects of EHE over each quantile of the moderating variables using the `margins, dydx() at()` post-estimation suite in Stata.[9] The left graph in Figure R-IV shows that this change in procedure makes no functional difference to the HTE estimates for LOO extreme heat exposure, as the attenuation of HTEs toward zero for higher values of LOO extreme heat exposure practically identically matches that of the corrected right-hand graph in Figure R-I. However, the middle and right graphs in Figure R-IV, respectively constructed from the Model 1 estimates in Tables R–IV and R–V, 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 Model I in Tables R–IV and R–V.

5.2 Instrumental Variables Estimates

To show that the results in Section 5.1 are not driven by the endogenous determination of my direct innovation measures, I estimate IV models of Equation E1 that instrument innovation

[9] One reason I make this change is that HTE estimates from `lincom` exhibit unintuitive behavior. In particular, the marginal effect of EHE on AL value appears to slightly increase (i.e., slightly diminish) for higher values of variety exposure and patent exposure. This is despite the fact that the interaction effect estimates from the relevant models (particularly those from Model I in Tables R–IV and R–V) are (very slightly) negative, implying that higher values of EHE should induce (slight) decreases in the estimated marginal effect of EHE on AL values. The evolution of EHE HTEs over the distributions of variety exposure and patent exposure visualized in Figure R-IV correctly reflect this property.

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 3.1.2, 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).

Critically, these exclusion restriction violations should be favorable for MS23's results. As shown in Table R–II, when local EHE is interacted with itself, the resulting relationship with AL value is positive. Therefore, given the empirical evidence in Sections 3.1.2 and 3.1.3 that LOO EHE closely tracks local EHE, after instrumenting $\text{InnovationExposure}_{i,t} \times \text{ExtremeExposure}_{i,t}$ with LOO EHE, the resulting coefficient is most likely upward-biased.

Appendix Tables A-VII and A-VIII respectively replicate 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 Tables A-VII and A-VIII are generally larger than those in Tables R–IV and R–V (respectively), the first-stage F-statistics show that these different effect sizes 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 Tables A-VII and A-VIII 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

Tables A-VII or A-VIII provide clear support for a mitigatory moderating impact of innovation exposure on climate-driven land devaluation. The moderating effect estimates in Tables A-VII and A-VIII are very noisy. The SEs of the standardized moderating effect estimates in Panel C of Table A-VII (Table A-VIII) exceed those of the respective estimates in Panel C of Table R–IV (Table R–V) by at least 277% (328%). Further, the moderating effect estimates themselves are much smaller. Considering standardized coefficients, the moderating effect estimates in Table A-VII (Table A-VIII) decrease by at least 62% (45%) compared to their respective estimates in Table R–I. None of the moderating effect estimates in Tables A-VII or A-VIII are statistically significantly different from zero.

5.3 Addressing Alternative Explanations

One could 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 3.1.1.

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. The reason why is intuitive. 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 3.1.2 and 3.1.3. Replications of the model in Equation E1 that replace LOO EHE with $\text{VarietyExposure}_{i,t}$ or $\text{PatentExposure}_{i,t}$ thus provide less biased estimates of the mitigatory impacts of innovation on climate-driven AL devaluation. Section 5.2 also reports instrumental variables (IV) specifications that exploit whatever exogenous variation LOO EHE induces in innovation, and the results from these IV models do not qualitatively differ from the results arising from models that omit the IV strategy.

Another potential concern with estimating the mitigatory impact of innovation using direct measures of innovation is that these direct measures may imprecisely capture *climate-adaptive* innovation, whose mitigatory impact on EHE-driven AL devaluation is the key causal effect of interest. As aforementioned in Section 3.1.1, 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/patent exposure on EHE-driven AL devaluation.

This concern is partially allayed by MS23's data. As aforementioned in Section 3.1.1, MS23 already split patent counts into climate-related and climate-unrelated patents. My patent exposure measure from Section 4 only makes use of 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 (nearly) entirely from climate-adaptive innovation. However, these IV models in Section 5.2 yield qualitatively identical results to the reduced-form estimates from Section 5.1.

Additionally, one potential source of confounding that could be impacting my replication results is the fact that both AL value and innovation both naturally grow considerably over time. 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 5.1 and 5.2 are effectively reflecting an increasing time trend that is common to both AL value and innovation.

This concern is addressed by MS23's fixed effects strategy, which I retain in all specifications from Sections 5.1 and 5.2. Like MS23's Table III, my specifications incorporate state-by-year fixed effects, which effectively control nonparametrically for temporal effects.

Any linear or polynomial time trend term is perfectly multicollinear with these fixed effects. Therefore, if there are differential time trends that could confound estimates of innovation's mitigatory impacts, then they are partialled out by these fixed effects.

Finally, one may worry that the bulk of the variation in my direct innovation measures is driven by a handful of crops for whom innovation is particularly profitable. This issue is related to the high concentration of innovation in a handful of crops that I discuss in Section 3.1.1. If my results from Sections 5.1 and 5.2 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]

I address this concern empirically by repeating my analyses in Sections 5.1 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. I replicate Tables R–IV and R–V, replacing $\text{VarietyExposure}_{i,t}$ and $\text{PatentExposure}_{i,t}$ with these alternate measures, in Appendix Tables A-IX and A-X. This check actually decreases my mitigatory impact estimates to the point that all mitigatory impact estimates in Appendix Table A-IX, and four of the five mitigatory impact estimates in Appendix Table A-X, are *negative*. If taken at face value, these estimates would imply that counties exposed to more innovation are *more severely impacted* by EHE than counties exposed to less innovation. However, none of these mitigatory impact estimates are statistically significantly different from zero.

In addition, this concern is also addressed by my IV strategy. The variation that produces the IV estimates in Section 5.2 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 5.2 are not qualitatively different from the reduced-form results in Section 5.1.

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

6 Conclusion

This paper shows that MS23's estimates of the mitigatory moderating impact of technological market adaptations on EHE-induced AL devaluation are largely an artefact of an inappropriate proxy for innovation exposure. Though full raw and analysis data are available, MS23's replication repository permits a partial computational reproduction of their published findings. However, when I re-estimate MS23's models using newly-constructed direct measures of innovation, the mitigatory effect estimates I obtain are at least 99.8% 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. Contrary to MS23's key conclusions, my results suggest that technological market adaptations have little capacity to mitigate the agricultural damage induced by climate change. My conclusions align with those of several prior studies that estimate similar mitigatory effects (e.g., see Hornbeck 2012; Aragón, Oteiza, & Rud 2021).

The findings in this paper cast doubt on more than just MS23's main estimates of technological innovation's moderating influence on EHE-induced AL devaluation. Principally, MS23's projections of aggregate historical and future climate change damage mitigation from technological innovation depend on their estimates in Table III (see pgs. 685 and 690-691). The fact that the moderating effect estimates that I obtain using direct measures of innovation are at least 99.8% less than the estimates obtained using MS23's proxy imply that the projected mitigatory benefits of technological innovations likely decrease to similar degrees. However, more importantly, my findings show that one should not put too much faith in the capacity of technological market innovations to substantially mitigate the agricultural harms of climate change. This last point is critically important for those seeking to compute – or decide – optimal investment levels for abating climate change.

Author affiliation: Vrije Universiteit Amsterdam; School of Business and Economics; Department of Ethics, Governance, and Society.

Appendix

Replication Package Item	Fully	Partial	No
Raw data provided	X		
Analysis data provided	X		
Cleaning code provided		X	
Analysis code provided		X	
Reproducible from raw data		X	
Reproducible from analysis data		X	

Note: This table summarizes the reproducibility of MS23's published findings based on the replication repository in Moscona & Sastry (2022).

Table A-I: Reproducibility of Published Findings from Replication Repository

	(1)	(2)	(3)	(4)
County-level extreme exposure	1.145	1.166	0.462	0.486
	(0.019)	(0.02)	(0.016)	(0.017)
	[0]	[0]	[0]	[0]
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 is the dependent variable. Standard errors are presented in parentheses, and p-values (rounded to three decimal places) are displayed in brackets.

Table A-II: Relationship Between Extreme Heat and Local Temperatures

	(1)	(2)
Δ ExtremeExposure	0.002	0.007
	(0.005)	(0.005)
	[0.752]	[0.16]
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, while p-values are displayed in brackets.

Table A-III: 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)
	[0.008]	[0.009]	[0.013]	[0.049]	[0.027]	[0]
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 can not 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, and p-values are given in brackets.

Table A-IV: Table III 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 estimate is obtained via the `margins, eyex()` post-estimation command in Stata.

Table A-V: 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 × innovation exposure	0.259	0.445	0.247	0.261	0.24	0.0923	0.13
	(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)
	[0]	[0]	[0]	[0]	[0]	[0.015]	[0.006]
County-level extreme exposure × variety exposure	0.181	0.389	0.154	0.173	0.148	0.0771	0.145
	(0.0765)	(0.0793)	(0.0668)	(0.0743)	(0.0684)	(0.0356)	(0.0371)
	[0.02]	[0]	[0.024]	[0.022]	[0.033]	[0.031]	[0]
Observations	6000	6000	5990	6000	5990	20966	20966
A^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, whereas p-values (rounded to three decimal places) are presented in brackets.

Table A-VI: MS23's Table A18 and a Replication Attempt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Raw coefficients							
County-level extreme exposure	0.04751	-0.1973	-0.07448	-0.3257	0.12248	0.13951	-0.05239
	(0.25192)	(0.24639)	(0.18544)	(0.42494)	(0.38848)	(0.52994)	(0.71966)
	[0.8508]	[0.42526]	[0.68887]	[0.4453]	[0.75325]	[0.79252]	[0.94201]
County-level extreme exposure ×	2e-05	3e-05	2e-05	0.00012	0.00014	-1e-05	-3e-05
variety exposure	(4e-05)	(6e-05)	(6e-05)	(8e-05)	(0.00013)	(0.00016)	(0.00035)
	[0.56738]	[0.59335]	[0.68934]	[0.13748]	[0.25311]	[0.93673]	[0.92555]
Panel B: Partial correlations							
County-level extreme exposure	0.01935	-0.08244	-0.04124	-0.07888	0.03233	0.01438	-0.00398
	(0.10256)	(0.1019)	(0.10242)	(0.10196)	(0.10249)	(0.05462)	(0.05463)
	[0.85077]	[0.42054]	[0.68812]	[0.44106]	[0.75312]	[0.79249]	[0.94201]
County-level extreme exposure ×	0.05878	0.05489	0.0411	0.15189	0.11716	-0.00434	-0.00511
variety exposure	(0.10224)	(0.10229)	(0.10242)	(0.10023)	(0.10119)	(0.05463)	(0.05463)
	[0.56672]	[0.5928]	[0.68909]	[0.13298]	[0.24985]	[0.93673]	[0.92555]
Panel C: Standardized coefficients							
County-level extreme exposure	0.03334	-0.13843	-0.05221	-0.22851	0.08586	0.12731	-0.04781
	(0.17674)	(0.17287)	(0.12999)	(0.29814)	(0.27233)	(0.4836)	(0.65673)
	[0.8508]	[0.42526]	[0.68887]	[0.4453]	[0.75325]	[0.79252]	[0.94201]
County-level extreme exposure ×	0.0354	0.0493	0.03534	0.17487	0.2082	-0.02007	-0.05085
variety exposure	(0.06168)	(0.09202)	(0.08814)	(0.11675)	(0.18107)	(0.25265)	(0.54382)
	[0.56738]	[0.59335]	[0.68934]	[0.13748]	[0.25311]	[0.93673]	[0.92556]
First-stage F	3.097	2.545	7.583	2.425	3.361	0.307	0.143
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

Note: The dependent variable in all models is logarithmic AL values. Estimates of the specification in Equation E1, where LOO EHE is replaced with $\text{VarietyExposure}_{i,t}$, and $\text{VarietyExposure}_{i,t}$ and $(\text{VarietyExposure}_{i,t} \times \text{ExtremeExposure}_{i,t})$ are instrumented by a second-order polynomial of LOO EHE, are presented alongside SEs double-clustered at the county and state-year levels in parentheses, as well as p-values in brackets. F-statistics are computed in accordance with Kleibergen & Paap (2006).

Table A-VII: Mitigatory Impacts of Variety Exposure, IV Estimates

	(1)	(2)	(3)	(4)	(5)
Panel A: Raw coefficients					
County-level extreme exposure	-0.083581	-0.113446	-0.124031	-0.184512	-0.093768
	(0.276747)	(1.075576)	(0.22589)	(0.284015)	(0.33166)
	[0.763303]	[0.916222]	[0.584243]	[0.517481]	[0.778005]
County-level extreme exposure ×	1.6e-05	0.000133	1e-05	2.4e-05	2.4e-05
patent exposure	(1.9e-05)	(0.00033)	(3.1e-05)	(1.3e-05)	(4e-05)
	[0.409823]	[0.6869]	[0.741386]	[0.074807]	[0.539357]
Panel B: Partial correlations					
County-level extreme exposure	-0.031001	-0.010822	-0.056423	-0.066802	-0.029019
	(0.102499)	(0.102586)	(0.102271)	(0.10214)	(0.102511)
	[0.762972]	[0.916207]	[0.582447]	[0.514676]	[0.777732]
County-level extreme exposure ×	0.084632	0.041445	0.033939	0.181745	0.063076
patent exposure	(0.101863)	(0.102422)	(0.10248)	(0.099209)	(0.10219)
	[0.408146]	[0.686646]	[0.741242]	[0.070093]	[0.53855]
Panel C: Standardized coefficients					
County-level extreme exposure	-0.05864	-0.079593	-0.086946	-0.129454	-0.065732
	(0.194166)	(0.754625)	(0.158351)	(0.199265)	(0.232496)
	[0.763304]	[0.916222]	[0.584243]	[0.517481]	[0.778006]
County-level extreme exposure ×	0.069929	0.578733	0.044281	0.10482	0.106438
patent exposure	(0.084469)	(1.431441)	(0.133785)	(0.058187)	(0.172784)
	[0.409823]	[0.6869]	[0.741386]	[0.074807]	[0.539357]
First-stage F	6.465	0.135	5.304	5.597	2.311
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

Note: The dependent variable in all models is logarithmic AL values. Estimates of the specification in Equation E1, where LOO EHE is replaced with $\text{PatentExposure}_{i,t}$, $t \in \{1950, 2010\}$, and $\text{PatentExposure}_{i,t}$ and $\left(\text{PatentExposure}_{i,t} \times \text{ExtremeExposure}_{i,t}\right)$ are instrumented by a second-order polynomial of LOO EHE, are presented alongside SEs double-clustered at the county and state-year levels in parentheses, as well as p-values in brackets. F-statistics are computed in accordance with Kleibergen & Paap (2006).

Table A-VIII: Mitigatory Impacts of Patent Exposure, IV Estimates

	(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)
	[0.0008634]	[0.0010653]	[9.36e-05]	[0.0217366]	[0.0112753]	[0.854838]	[0.4574868]
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)
	[0.6014858]	[0.9554616]	[0.0882405]	[0.9300682]	[0.0570108]	[0.1605285]	[0.6134907]
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)
	[4.33e-05]	[6.79e-05]	[1e-07]	[0.0120815]	[0.0046396]	[0.8548164]	[0.4563705]
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)
	[0.5999039]	[0.9554594]	[0.0737244]	[0.9300597]	[0.0432449]	[0.1568254]	[0.6130843]
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)
	[0.0008634]	[0.0010653]	[9.36e-05]	[0.0217366]	[0.0112753]	[0.854838]	[0.4574867]
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)
	[0.6014857]	[0.9554615]	[0.0882405]	[0.930068]	[0.0570109]	[0.1605287]	[0.6134909]
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

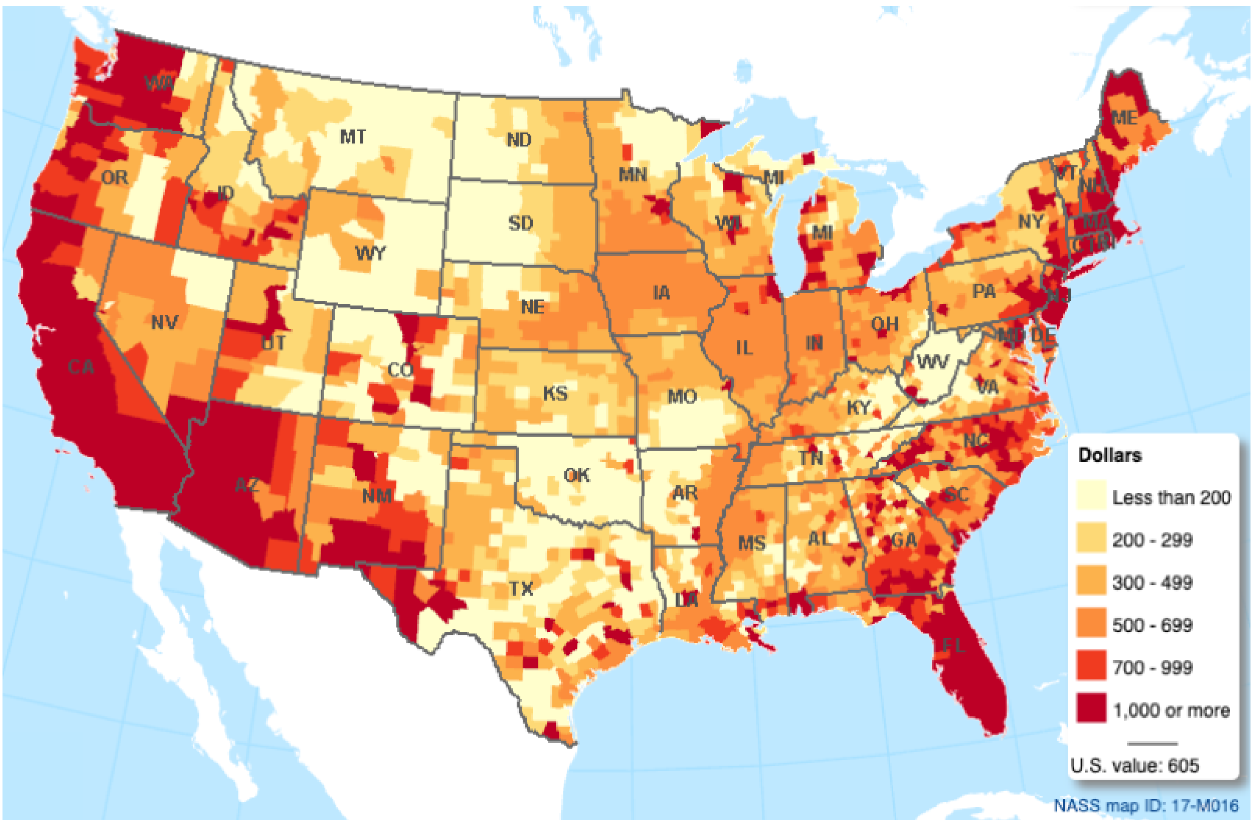
Note: The dependent variable in all models is logarithmic AL values. Estimates of the specification in Equation E1, where $\text{InnovationExposure}_{i,t}$ is replaced with a variant of $\text{InnovationExposure}_{i,t}$ that omits corn, lettuce and romaine, soybeans, tomatoes, and wheat are presented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

Table A-IX: 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)
	[0.0012755]	[0.0033655]	[0.0023966]	[0.0789697]	[0.0405466]
County-level extreme exposure × patent exposure	-7e-07	7.7e-06	-2e-06	-7e-07	-6.2e-06
	(3.9e-06)	(8.5e-06)	(4.3e-06)	(3.8e-06)	(4.6e-06)
	[0.857124]	[0.3669293]	[0.637424]	[0.8561642]	[0.1794076]
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)
	[9.87e-05]	[0.0006332]	[0.0003402]	[0.0645311]	[0.0280062]
County-level extreme exposure × patent exposure	-0.0185248	0.0926121	-0.0485678	-0.0186507	-0.1401252
	(0.1025626)	(0.1017179)	(0.1023558)	(0.1025621)	(0.1005833)
	[0.8570513]	[0.3648736]	[0.6362326]	[0.8560899]	[0.166833]
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)
	[0.0012755]	[0.0033655]	[0.0023966]	[0.0789697]	[0.0405468]
County-level extreme exposure × patent exposure	-0.0018477	0.0200921	-0.0052439	-0.0017755	-0.0160377
	(0.0102351)	(0.0221628)	(0.0110907)	(0.0097686)	(0.0118573)
	[0.8571238]	[0.3669295]	[0.6374245]	[0.856164]	[0.1794078]
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

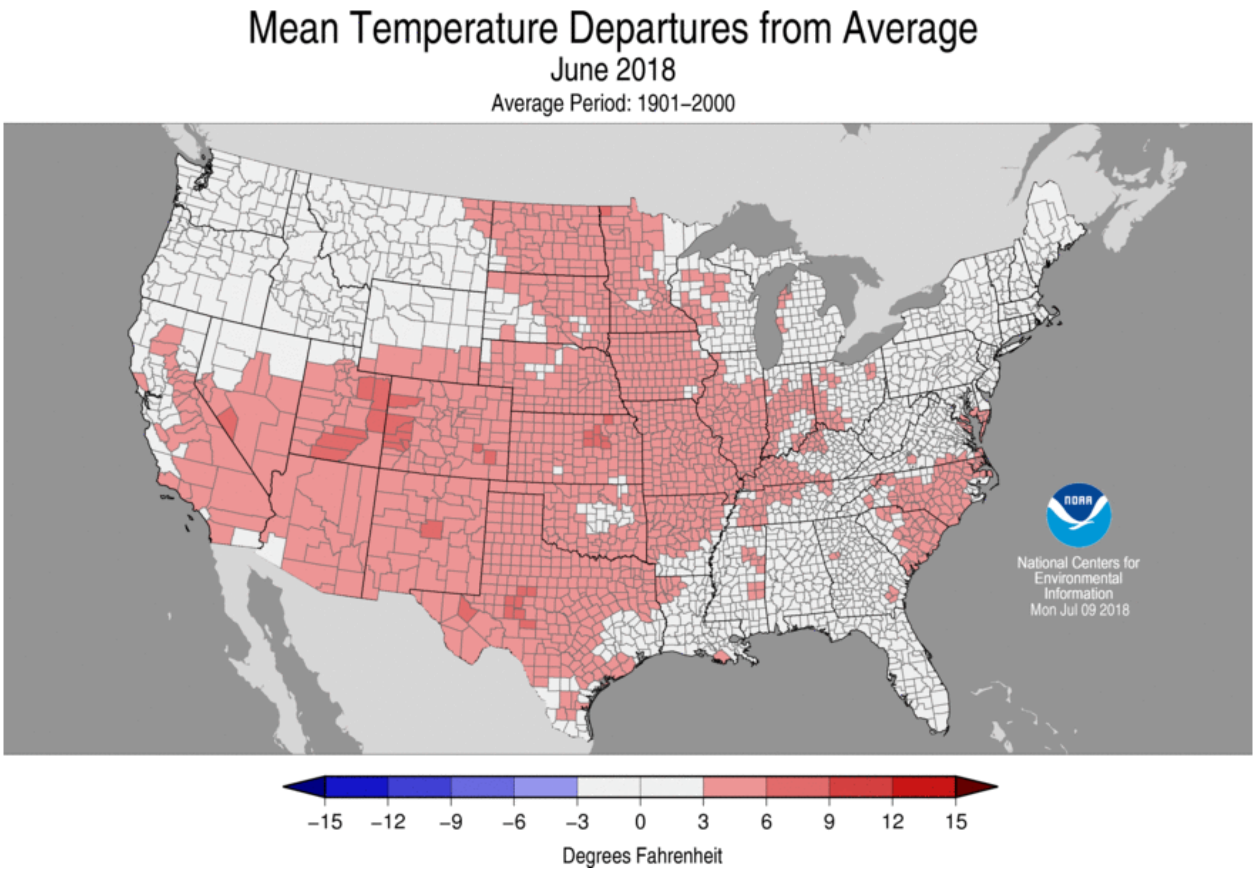
Note: The dependent variable in all models is logarithmic AL values. Estimates of the specification in Equation E1, where $\text{InnovationExposure}_{i,t}$ is replaced with a variant of $\text{PatentExposure}_{i,t}$ that omits alfalfa (and varieties thereof), barley, corn, soybeans, and tobacco are presented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

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



Note: Average value of crops sold per acre of harvested cropland is displayed for each county in the continental U.S. Data is based on the U.S. Department of Agriculture's 2017 Census of Agriculture. Figure retrieved from https://www.nass.usda.gov/Publications/AgCensus/2017/Online_Resources/Ag_Census_Web_Maps/index.php on 12 August 2024. The water regions of the map are slightly modified to remove interactive elements of the web figure; the land elements remain unchanged.

Figure A-I: Per-Acre Crop Value by County, 2017



Note: Each county's June 2018 deviation in average mean temperature from climatic averages from 1901-2000. Retrieved from https://www.ncei.noaa.gov/access/monitoring/monthly-report/national/201806/supplemental/page-1 on 9 August 2024. Data is based on the National Oceanic and Atmospheric Administration's Monthly National Climate Report for June 2018.

Figure A-II: Average Mean Temperature Deviations from Climatic Averages by U.S. County

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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)
	[0]	[0]	[0]	[0]	[0.001]	[0.031]	[0.003]
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)
	[0.001]	[0]	[0.002]	[0.002]	[0.003]	[0.004]	[0]
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)
	[0]	[0]	[0]	[0]	[0]	[0.028]	[0.002]
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)
	[0.001]	[0]	[0.001]	[0.001]	[0.002]	[0.004]	[0]
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)
	[0]	[0]	[0]	[0]	[0.001]	[0.031]	[0.003]
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)
	[0.001]	[0]	[0.002]	[0.002]	[0.003]	[0.004]	[0]
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, as well as p-values in brackets. These values are not reported in Panel A, as they are not reported in the published estimates in Table III.

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)
	[0]	[0]	[0]	[0]	[0]	[0.013]	[0.146]
(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)
	[0.002]	[0.031]	[0]	[0]	[0]	[0.016]	[0.33]
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)
	[0]	[0]	[0]	[0]	[0]	[0.011]	[0.142]
(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)
	[0.001]	[0.027]	[0]	[0]	[0]	[0.015]	[0.329]
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)
	[0]	[0]	[0]	[0]	[0]	[0.013]	[0.146]
(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)
	[0.002]	[0.031]	[0]	[0]	[0]	[0.016]	[0.33]
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, as well as p-values in brackets.

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
	(0.118)	(0.138)	(0.114)	(0.123)	(0.123)	(0.044)	(0.056)
	[0.241]	[0.007]	[0.318]	[0.34]	[0.404]	[0.001]	[0]
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. Standard errors double clustered at the county and state-decade level are shown in parentheses, whereas p-values are presented in brackets.

Table R–III: Average Moderating Effects 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)
	[0.0005366]	[0.0002609]	[0.0002049]	[0.0142292]	[0.0110957]	[0.7306531]	[0.3582927]
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)
	[0.7175476]	[0.7372921]	[0.4368398]	[0.9092712]	[0.1905351]	[0.3957842]	[0.9383229]
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)
	[1.48e-05]	[2.5e-06]	[1.3e-06]	[0.0065729]	[0.0045277]	[0.7305154]	[0.356468]
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)
	[0.7169847]	[0.7371415]	[0.4323979]	[0.9092525]	[0.1783406]	[0.3942519]	[0.9383224]
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)
	[0.0005366]	[0.0002609]	[0.0002049]	[0.0142292]	[0.0110958]	[0.730653]	[0.3582926]
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)
	[0.7175474]	[0.7372923]	[0.4368401]	[0.9092709]	[0.1905353]	[0.3957844]	[0.9383227]
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 VarietyExposure$_{i,t}$, are presented alongside SEs double-clustered at the county and state-year levels in parentheses, as well as p-values in brackets.

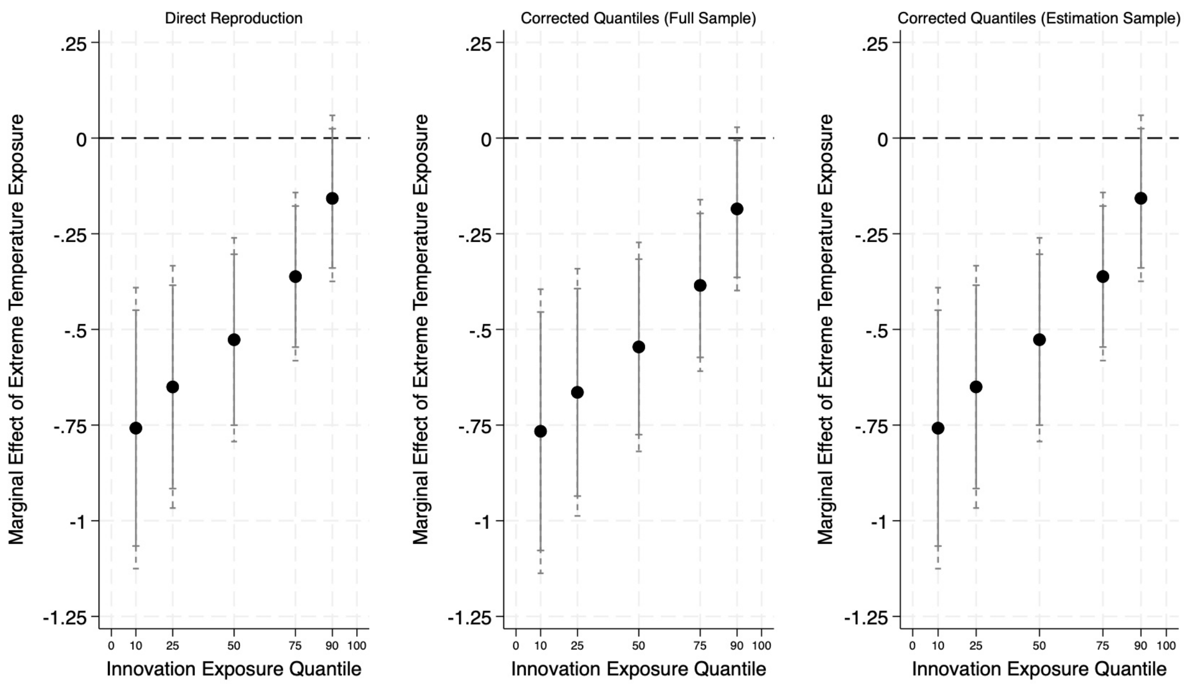
Table R–IV: Mitigatory Impacts of Variety Exposure

	(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)
	[0.0002624]	[0.0002962]	[0.000146]	[0.0086664]	[0.014641]
County-level extreme exposure ×	-2e-06	4e-07	-7.9e-06	-8e-07	-9.8e-06
patent exposure	(2.7e-06)	(4.6e-06)	(8.5e-06)	(2.5e-06)	(7.5e-06)
	[0.4723702]	[0.9231358]	[0.3545097]	[0.7396941]	[0.1916005]
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)
	[2.5e-06]	[3.4e-06]	[5e-07]	[0.0030893]	[0.0068548]
County-level extreme exposure ×	-0.0742286	0.0099249	-0.0958975	-0.0342088	-0.1361847
patent exposure	(0.1020325)	(0.1025877)	(0.1016543)	(0.1024778)	(0.100695)
	[0.4687091]	[0.9231321]	[0.3478854]	[0.7392535]	[0.179443]
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)
	[0.0002624]	[0.0002962]	[0.000146]	[0.0086664]	[0.0146411]
County-level extreme exposure ×	-0.0086029	0.0019263	-0.0343705	-0.0036543	-0.0425796
patent exposure	(0.0119236)	(0.0199124)	(0.0369407)	(0.0109664)	(0.0323744)
	[0.47237]	[0.9231359]	[0.3545099]	[0.7396939]	[0.1916007]
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, as well as p-values in brackets.

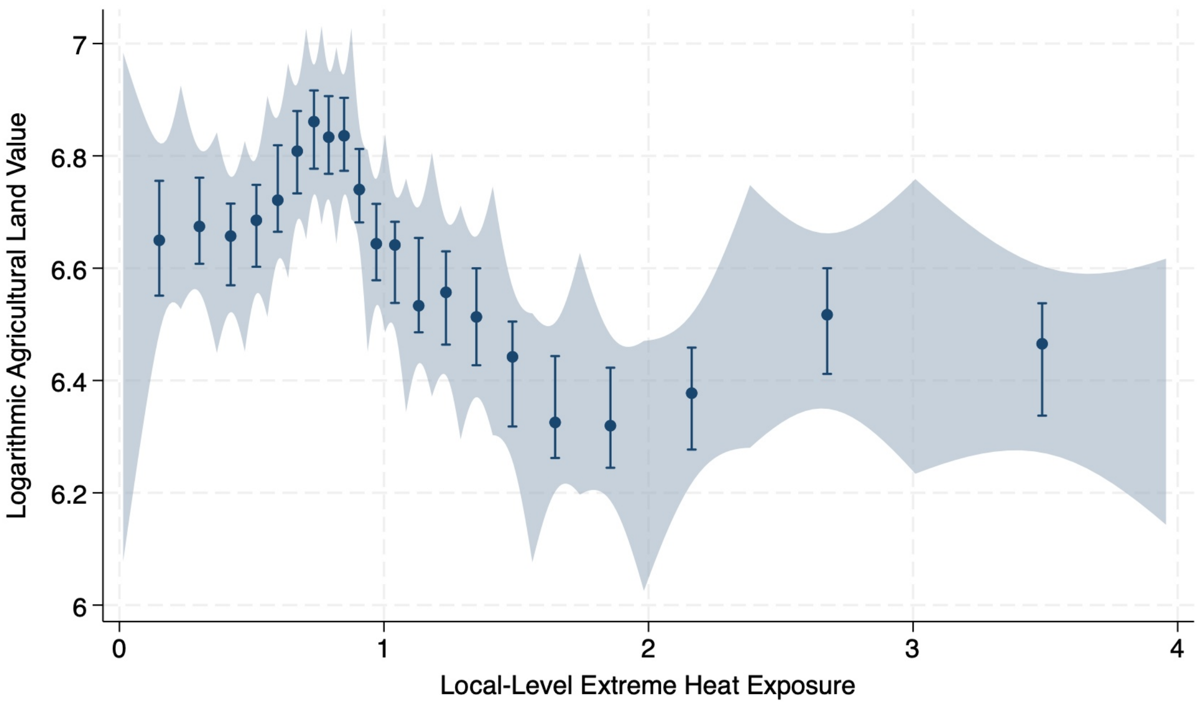
Table R–V: Mitigatory Impacts of Patent Exposure

Figures



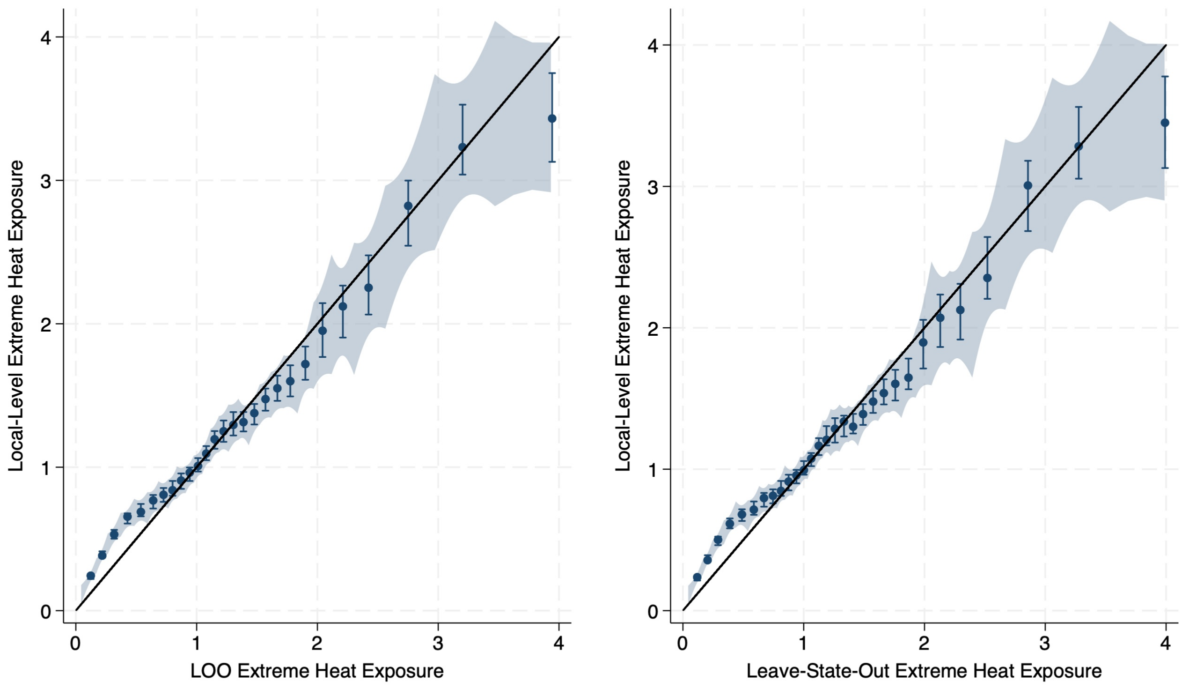
Note: The left graph reproduces Figure VI from MS23, while the center and right graphs correct the hard-coded quantiles used to construct the original graphs. Bars represent 90% and 95% confidence intervals. Estimates are based on Table R–I, Panel B, Model 1.

Figure R-I: Reproduction and Corrections of MS23's Figure VI



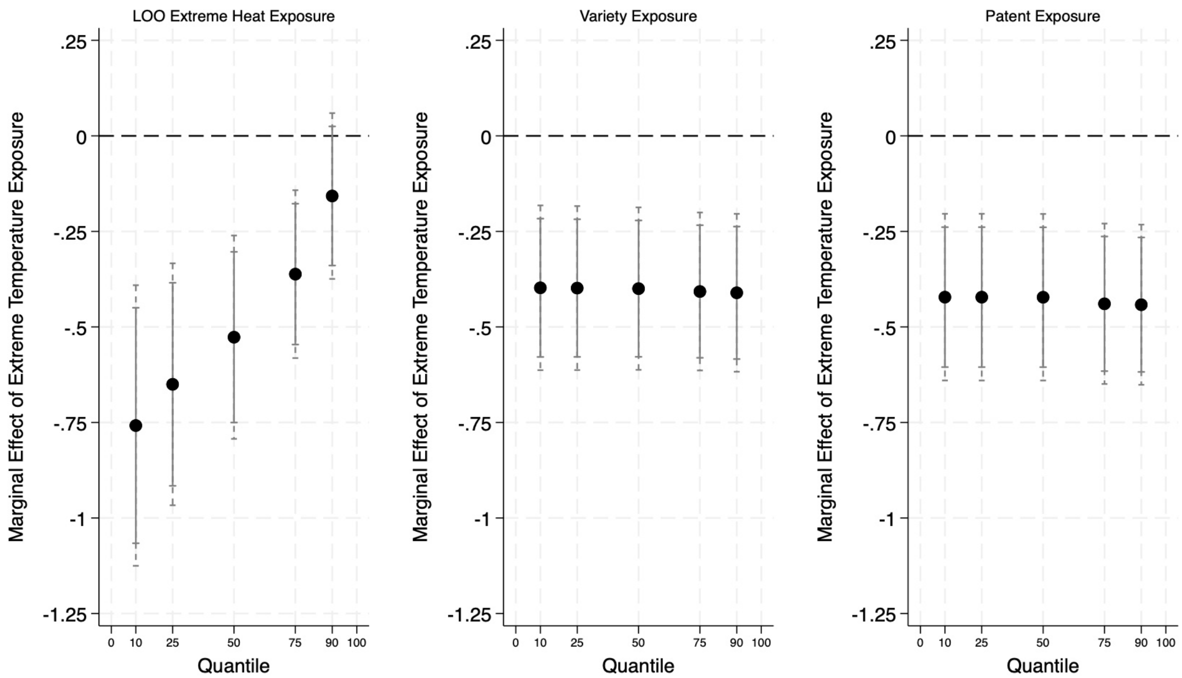
Note: The graph shows a binscatter regression plot (see Cattaneo et al. 2024) showing the relationship between logarithmic AL values and county-level EHE for $\text{ExtremeExposure}_{i,t} \in [0, 4]$; this restriction, instituted to improve interpretability, covers 95.3% of the distribution of $\text{ExtremeExposure}_{i,t}$.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

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



Note: The left graph reproduces the right graph from Figure R-I, while the center and right graphs display extrapolated marginal impacts of county-level EHE on AL values for selected quantiles of different moderators. The middle and right graphs are respectively constructed based on Model 1 estimates from Tables R–IV and R–V, Panel B.

Figure R-IV: HTEs of Extreme Heat Exposure