THE ECONOMIC IMPACTS OF CLIMATE CHANGE: EVIDENCE FROM AGRICULTURAL OUTPUT AND RANDOM FLUCTUATIONS IN WEATHER: COMMENT

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Abstract

In a series of studies employing a variety of approaches we have found that the potential impact of climate change on US agriculture is likely negative. Deschenes and Greenstone (2007) report dramatically different results based on regressions of agricultural profits and yields on weather variables. The divergence is explained by (1) missing and incorrect weather and climate data in their study; (2) their use of older climate change projections rather than the more recent and less optimistic projections from the Fourth Assessment Report; and (3) difficulties in their profit measure due to the confounding effects of storage.
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Agriculture is the sector of the economy most directly linked to climate and thus likely to be affected by climate change. To date, however, there exists considerable disagreement about not only the magnitude of potential impacts but also the sign.

A recent paper by Deschênes & Greenstone (2007*b*), henceforth DG, criticizes the hedonic model, a cross-sectional approach that regresses farmland values on climate variables for the US first introduced by Mendelsohn, Nordhaus & Shaw (1994), and proposes to use random year-to-year weather fluctuations in a panel of US agricultural profits and yields.[1] DG find *no* statistically significant relationship between U.S. agricultural profits and weather variables in the same years. DG also find *no* statistically significant relationship between corn and soybean yields (output per acre) and weather. They argue that if short-run weather fluctuations have no influence on agricultural profits or output, then in the long-run, when adaptations are possible, climate change is likely to have no impact or even prove beneficial. They conclude that "the preferred estimates indicate that climate change will lead to a $1.3billion (2002$), or 4.0 percent, increase in annual agricultural sector profits. [...] The basic finding of an economically and statistically small effect is robust to a wide variety of specification checks [...]. Additionally, the analysis indicates that the predicted increases in temperature and precipitation will have virtually no effect on yields among the most important crops (i.e., corn for grain and soybeans) [...]."

In this comment we revisit their paper in an attempt to reconcile their findings with others in the literature, which suggest a less optimistic outcome.[2] We present evidence showing that the differences stem mainly from three sources: (i) data and coding errors in DG's weather data, agricultural data, and the construction of climate-change scenarios; (ii) the particular climate change scenario which is used for impact predictions; and (iii) standard errors that are biased due to spatial correlation. Correcting DG's data and coding errors makes predictions for climate-change impacts unambiguously negative in all but one specification. The exception is a profit regression with state-by-year fixed effects where the standard errors are very large because state-by-year fixed effects absorb almost all variation in weather.

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[1] The first part of DG's paper argues that the hedonic approach does not produce robust results. We replicate the same checks using a well-specified hedonic model in the online appendix and show them to be robust.

[2] For example, in Schlenker & Roberts (2009) we find a strong relationship between corn, soybean, and cotton yields and weather. The relationship is robust and very similar if derived from time-series variations in weather or cross-sectional variations in climate. Holding fixed the locations where crops are grown, we predict yields losses of 30-46% by the end of the century under the slowest warming scenario and 63-82% under the fastest warming scenario. These predictions also accord with our research that uses the hedonic approach, e.g., (Schlenker, Hanemann & Fisher 2006).

DG's measure of profits is reported sales in a given year minus reported production expenditures in that year, each variable being derived from the Census of Agriculture. This measure of profits creates a potential problem for DG's method of analysis. The measure does not account for implicit costs, for storage, or for inventory adjustments during the year being reported. It does not control for crops produced in the reporting year but not sold until a later year, or for crops sold in the reporting year that had been harvested in a previous year and stored.[3] The problem is that storage and other inventory decisions, like the holding of animal breeding stocks, are captured by the error term and are also correlated with weather, the key explanatory variable. The induced correlation between the error term and key explanatory variables violates the identification assumption and causes the estimated effect of weather to be biased toward zero.

1 Data Irregularities

To investigate the differences between DG's findings and others in the literature we downloaded DG's data and STATA code from the AER website. We found several irregularities in their weather and climate data. These data irregularities explain a large portion of differences in findings.

DG have two weather variables in their data set: the variable *dd89*, which measures growing degree days for each year and county, and *dd89_7000*, which measures the average number of degree days in each county between 1970 and 2000.[4] These two variables do not appear consistent with each other. The correlation of the county-level average of the four-year panel (*dd89*) and the 31-year average given in their data (*dd89_7000*) is only 0.39. Given the wide variation in temperatures in the cross-section, one would expect a stronger correlation between the 4-year and 31-year averages across counties. We reconstruct the same weather variables from raw data sources and find a correlation of 0.996. We also find the average of *dd89* is much lower and the standard deviation much higher than in our replication.

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[3] Since prices are low during years when production is high and vice versa, there is an incentive to smooth sales over time. Progressive income taxes and imperfect insurance also create an incentive to smooth sales and profits.

[4] Growing degree days integrate the product of temperature and time (measured in days) above a baseline temperature and below an upper threshold. For example, DG's baseline temperature is 8°C, so one day at a temperature of 13°C would equal 5 degree days. Time at temperatures above 32°C (A common upper threshold used in DG and in our replication) is treated as if it were 32°C, 24 degrees per day at or above this temperature.

Second, DG's baseline climate measure (*dd89_7000*) has a value of zero degree days for 163 counties. If correct, this measure implies temperatures do not exceed 8°C (46.4°F) in those counties during the growing season of April through September. Temperatures this low would seem implausible in any state, yet many of these counties are in warm southern states such as Texas.

Anomalies caused by missing or incorrect measurements, which as we shall show have an important influence on estimated impacts of climate change, are illustrated in Figures 1 and 2. We independently calculate the degree days variable *dd89_7000* used by DG and display it in the bottom panel of Figure 1.[5] Note the much smoother pattern as compared to the large discontinuous changes in the top panel. Average temperatures vary smoothly across space, where counties of the same latitude tend to have comparable average temperatures and latitudes further south are warmer. Natural exceptions are mountain chains like the Rockies in the West or the Appalachians in the East, where temperatures are cooler due to gains in altitude. The discontinuous pattern in DG's data induces excess weather variation, which causes significant attenuation bias on their parameter estimates, especially in regression models that use state-by-year fixed effects. Within-state-year temperature deviations in our replicated data set are approximately one seventh the size of DG's.

Third, DG's predicted changes in climate vary widely and abruptly over the contiguous United States. They range from a decrease of 880 growing degree days (equivalent to a uniform 4.8°F decrease during the growing season) to a 6572 growing degree days increase (equivalent to a uniform 35.9°F increase). This pattern is odd given that the underlying climate model does not predict cooling anywhere in the U.S. and the variance of the projected changes far exceeds that of any climate model. Predicted changes in DG's model and in our replication are shown in Figure 2. Again, compare the discontinuities in the top with the more coherent patterns in the bottom.

The large variability of DG's predicted climate changes stems from both inaccurate baseline values of zero for some observations and the way they combine observed weather and climate-change forecasts. General circulation models (GCMs) generate climate predictions on a coarser geographic scale than data available in historic records. DG use historic county-level data as a baseline combined with climate predictions that are uniform across each state. Thus, Los Angeles and San Francisco, the Salinas Valley and the San Joaquin Valley, Mount Whitney and Death Valley, are all assumed to have the same climate, after climate change

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[5] We derive degree days 8-32°C by first calculating average daily temperatures in each of the 2.5x2.5 mile grids of the data in Schlenker & Roberts (2009) and average over all cells with positive agricultural area.

occurs, since they are all in the same state. Much of the within-state variation, however, is maintained in the baseline values, which are county-level averages. Such a representation of climate change therefore displays regression towards the mean, with cooler counties becoming much warmer and some very warm counties becoming cooler.

This regression-toward-the-mean effect is accentuated by apparent errors in the baseline degree-day measure. Consider for example Fresno, Kings, and Tulare counties in the southern San Joaquin Valley of California. In DG's data, Fresno is predicted to have a *decrease* of 414 degree days (equivalent uniform temperature change of -2.3°F); Kings county has an increase of 403 degree days (+2.2°F) and Tulare an increase of 4685 degree days (+25.6°F). Tulare's large increase is the result of a zero (or apparently missing) baseline. But even for Kings and Fresno counties, for which there are no missing baselines, predicted climate changes are implausibly different for neighboring counties.

This treatment of climate change is unusual. We are not aware of any other application of the Hadley GCM model that predicts decreasing average temperatures by the end of the century in any U.S. location. The standard approach in the climate science literature is *not* to compare GCM projections with historic climate: it is to add GCM projections of regional climate *change* between, say, 1970-1999 and 2070-2099 to the sub-regional baselines, thereby preserving sub-regional variation and avoiding regression toward the mean.

2 Replication and Comparison

While there are other differences between DG's model of yields and our own model, much of the difference in the predicted impacts of climate change stems from the data issues described above. Comparisons of the original and replicated yield and profit regressions are summarized in Table 1.

In our replications we fix the sample so they exactly match those used by DG. This excludes some agriculturally important counties that are missing in DG's data. For example, 66 of Iowa's 99 counties are missing from their data set, yet Iowa is the largest producer of corn and soybeans, the nation's two largest crops. In an online appendix we present results where these counties are included but the results change little. The online appendix also replicates the analysis for the subsample of dryland counties east of the 100th meridian, the historic boundary between (primarily) irrigated and (primarily) rainfed agriculture in the United Sates.[6]

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[6] In irrigated areas west of the 100^{th} meridian, water comes mostly from aquifers or from snow or rain

The main weather variables used by DG are growing degree days, growing degree days squared, precipitation, and precipitation squared. All models include soil controls, county fixed effects and either year or state-by-year fixed effects, and regressions are area-weighted.[7] To avoid confounding our comparison with changes in specification, we use the same variables, the same weighting, and the same observations. There is one exception to this rule: 241 of DG's yield observations (3.5% of the observations for corn and 0.02% for soybeans) are zero even though the Agricultural Statistical Service shows that production was positive. We drop these observations in the yield regression.[8]

Columns (a) of Table 1 replicate results in DG using their original data set. Our replication of DG differs slightly from DG's original due to (i) a coding error in DG that we corrected (this is detailed in the appendix) and (ii) the fact that we drop observations with zero yields. Columns (b) replicate DG's regression model using our reconstruction of the weather data using their specification.[9]

The first row of Table 1 reports the variance explained by the weather variables.[10] For all models using year fixed effects (columns 1a-b for corn, 2a-b for soybeans, and 3a-3b for profits) our replicated weather variables explain roughly twice as much of the variance in the dependent variable. In the profit model using state-by-year fixed effects (columns 4a-b), our replication explains approximately 50 percent more. Recall that our replicate weather variable has a lower variance than DG's original data, yet explains a larger share of the variance of the dependent variable. This suggests that DG's weather data had significant measurement error, which likely results in attenuation bias towards zero.

The next six rows replicate the predicted climate change impacts. The first three rows give predicted impacts under the Hadley II scenario used by DG. The predicted impacts are insignificant if we use DG's data in columns (a), but are statistically significant in columns

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falling in distant locations, so local precipitation is a poor measure of water availability, and predicted climatic changes in local precipitation do not measure predicted changes in access to irrigation water.

[7] Yield regression use cropland-area weights, while profit regressions use farmland-area weights. Since the results are comparable whether we use year or state-by-year fixed effects in the yield regression, we only report them for year fixed effects here. The interested reader is referred to the online appendix for results using state-by-year fixed effects.

[8] However, we obtain similar point estimates if they are included.

[9] In a sensitivity check, DG include a variable to measure the potentially harmful effect of extreme heat on profits in Table 6 of their paper. In the online appendix we therefore also include one additional variable, the square root of degree days above 34°C in columns (c) of the regression to account for extreme temperatures. The results are comparable to columns (b) if predicted temperature increases are limited, but diverge further for larger, non-marginal changes under the Hadley III model projections.

[10] We calculate the variance explained by weather as one minus the ratio of the residual variance from the full specification over the residual variance from a model with all controls but no weather variables.

(1b), (2b), and (3b) if we use our replicated weather variables and year-fixed effects. We report two sets of standard errors. The first set uses the specification of DG and clusters the error by county. This allows for heteroscedasticity and auto-correlation of counties across year, but assumes that observations are identically distributed in space. In the second set of standard errors [square brackets] we therefore cluster by state after specifying the panel structure of our data, allowing for spatial correlation of counties within a state in a year.[11] This increases standard errors considerably, yet our predicted impacts are still significant at the five or even one percent level. Corn yields, soybean yields, and profits are predicted to decline by 11%, 16% and 37%, respectively. While the coefficients are statistically significant, the impacts are smaller in magnitude than earlier estimates in the literature that DG use as benchmarks.

We note, however, that DG used different climate change predictions than earlier studies to which they compare their results. We therefore replicate the predicted impacts using the Hadley III model in the next three rows, as an appropriate comparison should leave the climate forecasts fixed.[12] Predicted impacts remain significant and become larger in magnitude under the Hadley III model, comparable to earlier estimates in the literature. A discussion of the validity or the accuracy of either model is beyond the scope of this paper, but we stress that these differences in impacts are due not to differences in modeling assumptions but to differences in predicted climate change.

In columns (4a-b) of Table 1 we present the same set of results as in columns (3a-b) with one critical difference: we use state-by-year fixed effects in place of year fixed effects. While DG find insignificant impacts in their original paper using both year fixed effects and state-by-year fixed effects, the two diverge in our replication. We find significant damages in the former and insignificant impacts under the latter under all weather data sets and climate change scenarios, although the confidence intervals of the latter are very wide. As explained in footnotes 4 and 5 of DG, state-by-year fixed effects have the advantage of capturing regional price effects, which is especially useful if production of certain crops is concentrated geographically. For example, California produces 85 percent of the lettuce

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[11] An alternative would be to directly account for spatial correlation, e.g., using Conley (1999). We note that the results are comparable to the ones obtained using errors that are clustered by state. We report the latter as they simply require one minor modification in DG's code.

[12] The Hadley II scenarios were developed for use by the IPCC's Third Assessment Report; the Hadley III scenarios were developed for the IPCC's Fourth Assessment Report. Hadley III is an update and refinement of Hadley II. The key differences are that Hadley III projects larger temperature increases in North America, especially in summer, and a less optimistic forecast of changes in precipitation. In other research DG have used the Hadley III model as well (Deschênes & Greenstone 2007*a*).

grown in the United States. A country-wide yearly fixed effect would not capture the fact that crops specific to California might face unique price shocks. However, any crop-specific price response works as natural "insurance" for farmers that grow the crop. Prices move in the opposite direction from production shocks: If yields decline, prices increase, and vice versa.[13] Accounting for region-specific price responses should therefore make predicted impacts *more negative* as it cancels out the counterbalancing price response. It is counter-intuitive that predicted changes in profits are negative and significant in a regression using year fixed effects, yet turn positive when one includes state-by-year fixed effects to capture region-specific price responses.

What other effects apart from regional price effects might explain why the results become less damaging and insignificant with the use of state-by-year fixed effects? DG provide no clear answer. One hypothesis is that there is no statistical significance because there is too little statistical power. Thus, while we fail to reject no impact, we also fail to reject large negative impacts.

A concern with the use of state-by-year fixed effects is that they absorb a significant amount of weather variance. After removing county and state-by-year fixed effects, remaining weather variance pertains only to yearly within-state deviations from county means, as for example the amount by which northern Iowa is warmer than normal in a given year compared to how much southern Iowa is warmer than normal in the same year. Generally, whenever northern Iowa is warmer than normal, so is southern Iowa, because temperatures vary smoothly in space. DG report a significant amount of within-state weather variation in their Table 2. But it turns out this variation is largely an artifact of errors in their weather data, which exhibit large discontinuous shifts across neighboring counties as discussed above.

Statistics that summarize weather variation in DG's data set and our own are reported in Table 2. The table summarizes regressions of degree days against different sets of fixed effects: (1) an intercept; (2) county fixed effects; (3) county plus year fixed effects; and (4) county plus state-by-year fixed effects. The table reports the R-square, the standard deviation of the residual weather variation not absorbed by the fixed effects (in Fahrenheit equivalent),[14] and the fraction of residuals with an absolute value greater than 1 degree Fahrenheit. While the overall standard deviations of temperatures are similar in DG's measure and our replication (6.85F versus 6.10F), the two differ markedly once we include fixed effects. The residual standard deviation of DG's temperature measure is 2.70F with county fixed effects and

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[13] This is especially true for speciality crops like lettuce where world trade is limited in volume.

[14] We divide degree days by the number of days during the growing season.

2.39F with county plus state-by-year fixed effects. Our measure, on the other hand, has a residual standard deviation of 1.50F with county fixed effects and just 0.35F with county plus state-by-year fixed effects. These differences suggest a noise to signal ratio of DG's temperature measure of about 7 to 1 in their preferred fixed-effects model.[15]

3 Profit and the Role of Storage

The preceding section shows that predicted yield impacts from climate change are negative and significant if improved weather data are used in a model of corn and soybean yields. The results from the profit regressions are mixed and depend on whether one uses year or state-by-year fixed effects. We find significant negative impacts if year fixed effects are used and insignificant impacts (with large standard errors) for the case of state-by-year fixed effects.

As outlined at the end of the preceeding section, state-by-year fixed effects absorb almost all variation and the identification rests on very slim margins, so even small amounts of measurement error will be greatly amplified. The reason why impacts in the yield regression are insensitive to the inclusion of state-by-year fixed effects and not in the profit regression is related to DG's profit measure, the difference between agricultural sales reported for a given year and production expense reported for that same year. While production expense is essentially the cost associated with the crops grown in that year, the sales revenue is not necessarily the revenue from crops *grown* in that year - it is revenue from crops *sold* in that year. With major field crops in the U.S., such as corn, soybeans and wheat, farmers accumulate stocks in high-yielding years; in low-yielding years they deplete stocks accumulated in earlier years. Storage is thus one way for farmers to smooth weather-related shocks over time. It also creates a substantial disconnect between the weather-related shock and DG's metric for the impact of that shock, sales minus reported costs.

The amount farmers choose to place into or remove from storage is part of the error in the profit regressions. This error is directly related to the yield shock, and thus correlated with weather, DG's key explanatory variable. This creates an endogeneity bias toward zero, because storage is greater and sales lower in good years with positive weather shocks, and inventories are depleted in bad years with negative weather shocks.[16]

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[15] Recall that our replication of their degree days measure explains approximately twice the variance in year-to-year crop yields (Table 1). It is therefore unlikely that our replication smoothes temperatures too much in space as it is superior at explaining yield variation in space.

[16] Other factors besides storage could cause the short-run response to weather to understate the long-run response to climate. For example, after a bad yield shock, a livestock producer expects higher future feed prices, and therefore chooses to slaughter breeding stock in anticipation of higher future costs. Such a

The most plausible argument against these dynamic considerations is DG's use of year or state-by-year fixed effects.[17] These fixed effects account for the incentive to accumulate or deplete inventories, which are connected to prices (Williams & Wright 1991, Deaton & Laroque 1992). However, this would only work if prices of all commodities within a county move together and there is no sub-state price variation.[18]

To examine this issue empirically, we conducted the following exercise, the results of which are reported in Table 3. We regress *sales* against the *value of production*.[19] Columns (1) and (3) include year fixed effects, while columns (2) and (4) include state-by-year fixed effects. The first two columns also include county fixed effects, while the last two do not. Hence the first two columns use deviations from county means for identification: the regressions capture how much sales differ from average in relation to production value relative to its average. If storage variations are fully accounted for in the model, the coefficient should be one. That is, sales should increase one-for-one by the value of each extra unit that is produced. Columns (3) and (4), on the other hand, drop county fixed effects and the identification relies on the cross-section: these regressions show whether, on average, counties sell as much as they produce.

The first two columns reveal that storage is an important factor in sales. The coefficients are significantly different from one. State-by-year fixed effects do account for some of the tendency to store yield shocks as the coefficient in column (2) is closer to one than the coefficient in column (1). One alternative explanation for why the coefficients could be less

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reduction in cattle inventories could temporarily increase sales in a way that would not be feasible in the long run (Rosen, Murphy & Scheinkman 1994).

[17] Both the hedonic approach and the approach taken by DG assume constant prices, and thus assume no consumer-related impacts. In effect, the goal in both approaches is to obtain a first-order approximation of the economic impact by assessing the potential impacts on fundamental productivity. In the long run, we have little information about how prices will adjust (Cline 1996).

[18] Brennan, Williams & Wright (1997) present evidence showing so-called "convenience yields"–a motive to store commodities even when the futures price is below the spot price–may stem from local price variations for which futures markets do not exist. While such local price variation may be small relative to overall price fluctuations, true within-state weather variations in a given year are surely small too.

[19] To conduct this exercise we obtained access to individual farm-level data from the Agricultural Census Micro-files. These are the raw data used to construct the aggregate sales data used in DG. The survey asks farmers to report their sales of seven storable crops: corn, soybeans, wheat, cotton, oats, sorghum, and barley. It also asks for the yield of these crops and we derive the value of production by multiplying the total production of each farm by state-level prices (reported by USDA-NASS). We drop all farms that have livestock as sales could be less than the value of production when farmers feed these crops to animals. We then aggregate all non-livestock farms in a county and follow DG's specification as closely as possible: we construct sales per acre in a county (total sales divided by the total acreage) as the dependent variable and the ratio of total production divided by total acreage as the exogenous variable, plus county and state-by year fixed effects. The dependent variable in DG is profit per acre, sales minus costs. The regression results use area weights, as do DG, and cluster by state to get more conservative standard errors.

than one is measurement error and attenuation bias. However, note that if we drop county fixed effects in columns (3) and (4) and hence no longer rely on year-to-year deviations that give incentives for storage, the coefficient is no longer different from one. If measurement error was a pervasive problem, these coefficients should also be biased towards zero.[20]

Finally, one might wonder whether a coefficient of 0.8 is different from 1 in an economically meaningful way. Recall that we are looking at sales in Table 3 and profits are the difference between sales and expenses. For comparison, consider that production expenses in the 2002 census equaled 86% of agricultural sales, which is about the coefficient in the first two columns of Table 3.

4 Conclusions

Agriculture is the sector that has been most extensively studied in attempts to predict the economic impact of global climate change. This is not surprising, given climate variables such as temperature and precipitation directly enter agricultural production functions. Despite the existence of a large and growing literature, economists do not appear to have reached a consensus on the potential magnitude of the impact, or even on its sign.

A recent study by Deschênes & Greenstone (2007*b*) argues that predicted impacts are not economically significant: estimated relationships between weather variables, yields, and profits, are taken to imply that the impact of long run climate change on U.S. agriculture will be either insignificant or modestly beneficial. On the other hand, earlier research by us and others has found large potential impacts, estimated from both cross-sectional and time series variations in weather and climate.

Likely explanations for the divergence in findings include: (1) missing and almost certainly incorrect weather and climate data in DG's study, amplified by the use of state-by-year fixed effects that absorb most year-to-year weather variation; (2) DG's unusual and in our judgment incorrect treatment of climate-change predictions that assume a uniform future climate within each state; (3) DG's implicit assumption that errors are not spatially correlated; and (4) conceptual difficulties in their profit-based approach due to the confounding effects of storage and possibly also capital and inventory adjustments or local price movements that are associated with weather fluctuations.

A careful account of these factors shows that the balance of evidence weighs heavily on the

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[20] DG argue in their reply that including lagged weather variables will solve the storage problem. We believe this is incorrect as it does not break the contemporaneous correlation between weather and the error. We explain this in more detail and provide empirical evidence in the online appendix.

side of severe adverse potential impacts to U.S. agriculture by the end of the century stemming from anticipated global warming. This conclusion is subject to the possible mitigating influences of new heat-resistant crop varieties and carbon fertilization, though evidence from recent experiments that more realistically simulate fertilization suggests that its impact on crop yields will be much more limited than previously believed (Long et al. 2006)). Further, other experiments suggest that at least some of the projected increase in yields may be offset by a decline in nutritional value (Jablonski, Wang & Curtis 2002).

Conceptually, DG are correct in noting that omitted variables can in principle cause bias in a hedonic regression and that fixed effects can control for time-invariant idiosyncratic features of the unit of observation, in this case the county. However, it is also possible that fixed effects can increase the bias due to omitted variables if *time-varying* omitted variables (or data errors) are more strongly correlated with the treatment than *time-invariant* omitted variables that have been removed via the fixed effects. These fixed effects increase bias stemming from both endogeneity and measurement error. We have identified some important data errors and time-varying omitted variables, like storage, that are strongly correlated with both weather (the treatment variable) and DG's dependent variable, reported sales minus reported expenditures. These data errors and omitted variables bias toward zero results obtained by regressions that use sales as a proxy for production value.

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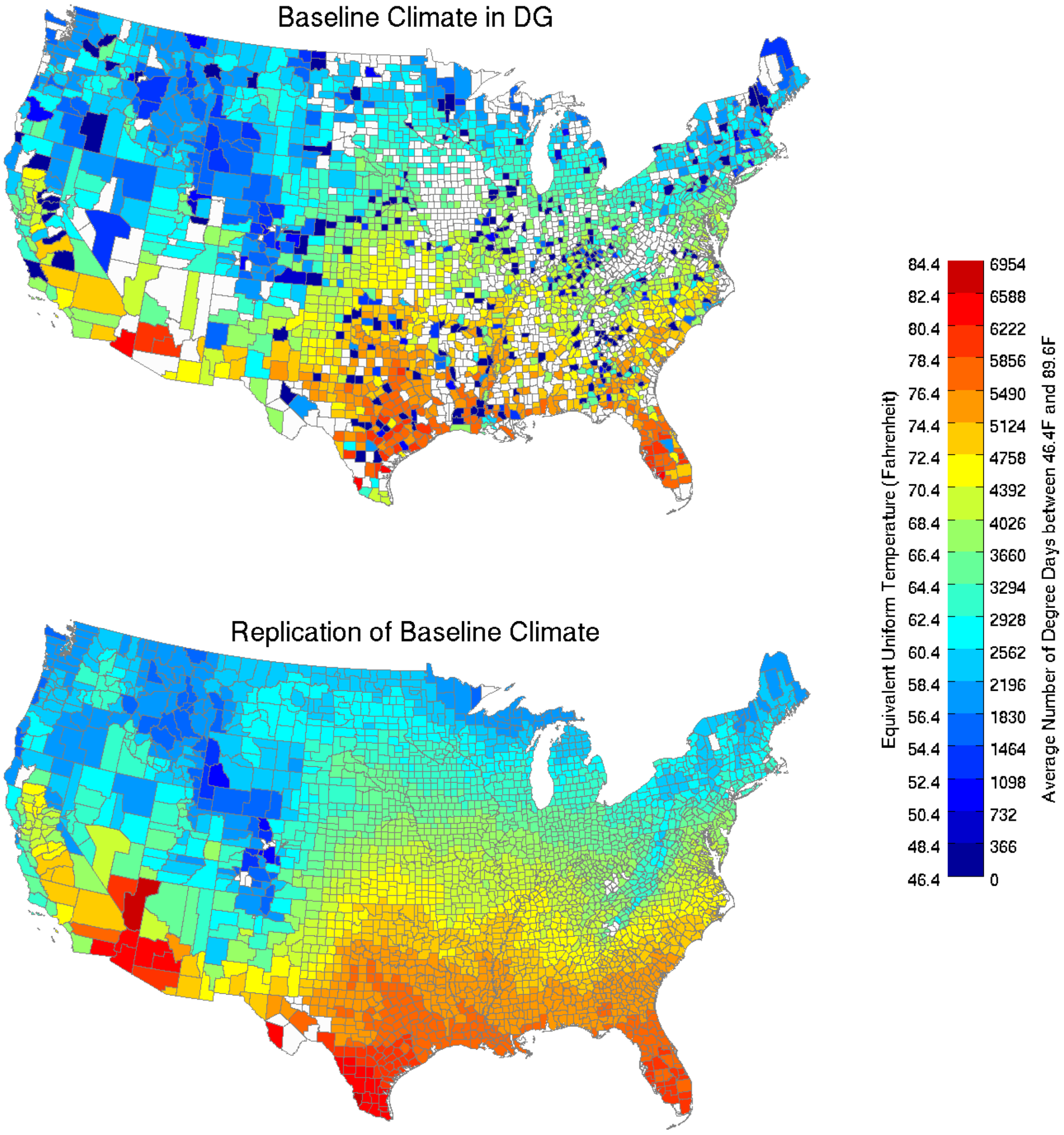
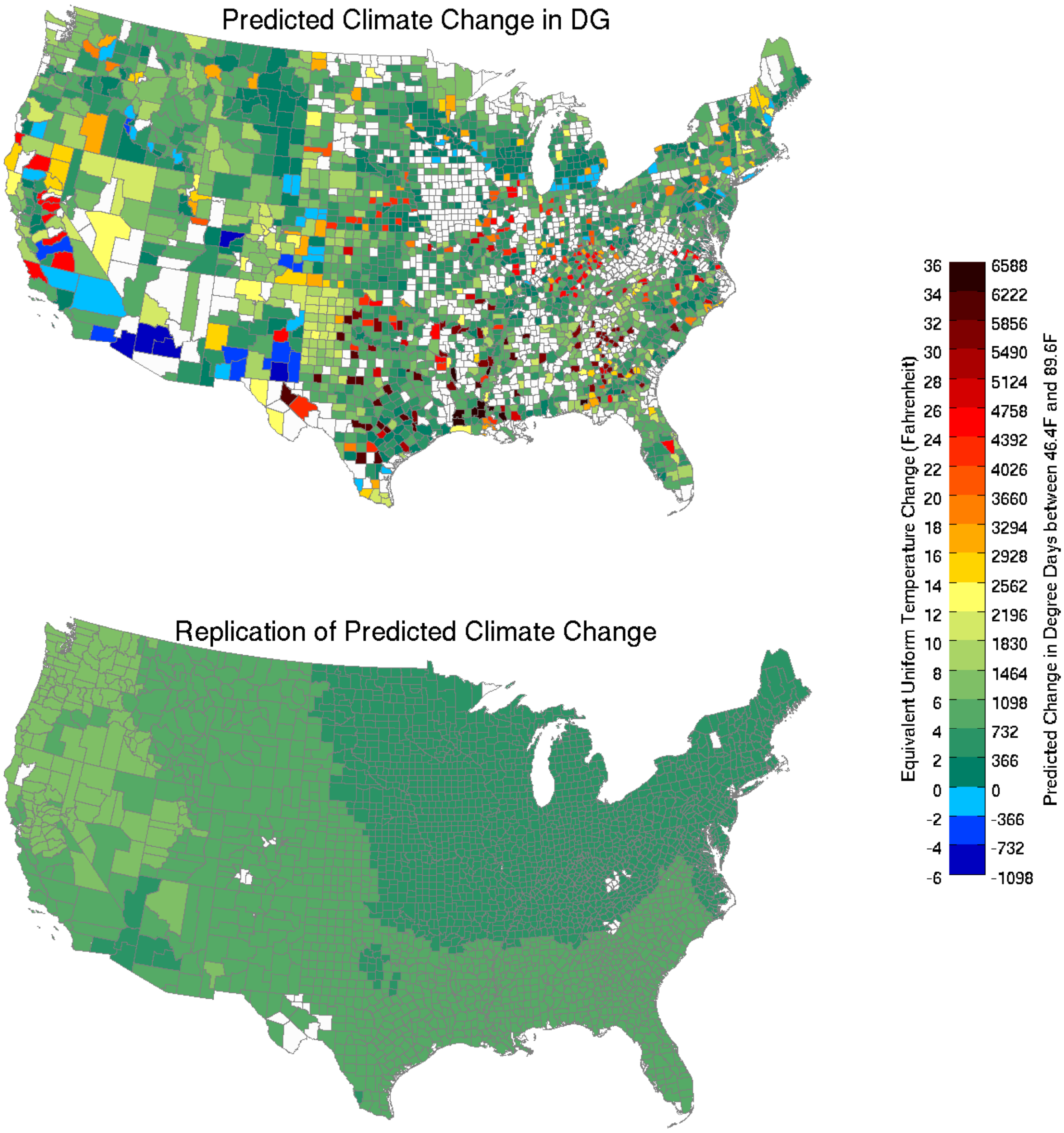
Figure 1: Baseline Climate in Deschenes and Greenstone



Notes: Graphs displays the baseline climate. The top panel are the data used in Deschenes and Greenstone, the bottom panel show our replications of the same variable degree days 8-32°C. The right index of the legend shows the number of degree days. Since degree days are difficult to interpret, we added another index at the left of the legend that shows the equivalent uniform temperature in degrees Fahrenheit, i.e., the equivalent constant temperature that would give the same number of degree days.

Figure 2: Climate Change Predictions in Deschenes and Greenstone



Notes: The top panel are the data used in Deschenes and Greenstone, the bottom panel shows our replications of the predicted changes in the same variable degree days 8-32°C. The right index of the legend shows the predicted change in the number of degree days. Since degree days are difficult to interpret, we added another index at the left of the legend that shows the equivalent uniform temperature change in degrees Fahrenheit.

Table 1: Comparison of Various Data Sources in Yield and Profit Regressions

	Corn		**Soybeans**		**Profit (Sales - Expenditures)**			
	(1a)	**(1b)**	**(2a)**	**(2b)**	**(3a)**	**(3b)**	**(4a)**	**(4b)**
Regression diagnostics								
Variance explained by weather	11.6%	19.6%	14.4%	30.6%	0.4%	1.5%	0.4%	0.6%
Climate change impact (Percent)								
Hadley II-IS92a scenario	-0.80	-10.61	-2.73	-15.63	-6.63	-36.50	3.75	1.21
(s.e.)	(1.24)	(1.45)	(1.38)	(1.60)	(3.03)	(5.41)	(2.82)	(12.88)
[s.e. clustered by state]	[2.08]	[4.18]	[2.08]	[4.93]	[4.98]	[10.34]	[3.98]	[15.18]
Hadley III-B2 scenario		-42.01		-51.59		-55.99		-3.28
(s.e.)		(3.23)		(3.65)		(8.93)		(20.61)
[s.e. clustered by state]		[11.14]		[11.80]		[16.58]		[25.12]
Observations	6623	6623	5140	5140	9024	9024	9024	9024
Soil controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No
State-by-Year FE	No	No	No	No	No	No	Yes	Yes

Notes: This table summarizes and compares alternative regression models. Columns (a) replicate the results in DG using their code and data (a quadratic in degree days 8-32°C and precipitation); columns (b) are the same models as (a) estimated with our reconstructed data. The variance explained by weather is 1 minus the ratio of the residual variance of the full specification over the residual variance of the model excluding weather. Standard errors in round brackets cluster by fips code following DG, while standard errors in brackets cluster by state.

Table 2: Temperature Variation under Various Sets of Fixed Effects

	Variable dd89 in DG			**Replication of dd89**		
	$\mathbf{R}^2$	$\boldsymbol{\sigma_e}$	$\lvert\mathbf{e}\rvert > \mathbf{1F}$	$\mathbf{R}^2$	$\boldsymbol{\sigma_e}$	$\lvert\mathbf{e}\rvert > \mathbf{1F}$
	(1a)	**(1b)**	**(1c)**	**(2a)**	**(2b)**	**(2c)**
No Fixed Effects (F.E.)		6.85F	91.2%		6.10F	89.9%
County F.E.	0.845	2.70F	56.8%	0.940	1.50F	65.0%
County + Year F.E.	0.867	2.50F	55.0%	0.979	0.88F	24.4%
County + State-by-Year F.E.	0.879	2.39F	50.8%	0.997	0.35F	1.3%

Notes: This table summarizes regressions of degree days, a temperature measure, on various sets of fixed effects and how much of the variation they absorb. The first three columns use the variable dd89 from DG and the last three columns use our recalculation of the same variable when data errors are corrected. Columns (a) report the R-square of the regression; columns (b) report the standard deviation of the residuals (remaining temperature variation) in degrees Fahrenheit during the growing season; and columns (c) report what fraction of the observations have a residual that is larger than 1 degree Fahrenheit over the growing season.

Table 3: Regressing Sales on Value of Production

	(1)	**(2)**	**(3)**	**(4)**
Coefficient	0.822***	0.870***	1.015	0.978
(s.e.)	(0.029)	(0.028)	(0.039)	(0.034)
p-val. for coeff. = 1	<0.0001	<0.0001	0.70	0.52
Observations	10891	10891	10891	10891
County FE	Yes	Yes	No	No
Year FE	Yes	No	Yes	No
State-by-Year FE	No	Yes	No	Yes

Notes: This table summarizes regressions of county-level sales on value-of-production for corn, soybeans, wheat, cotton, oats, sorghum, and barley. Sales and production data for all farms that do *not* have any livestock are added for each county. Following DG both the dependent and independent variables are on a per-acre basis, and the regression is weighted by acres. Significance levels whether coefficients are different from one are indicated by: *** (1%); ** (5%); and * (10%), and the p-value of this test is given in the third row. Errors are clustered by state.

The Economic Impacts of Climate Change: Evidence from Agricultural Output and Random Fluctuations in Weather: Comment

Online Appendix

Anthony C. Fisher, W. Michael Hanemann, Michael J. Roberts, and Wolfram Schlenker

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In this appendix we document our replication of DG in more detail and report additional analysis not presented in the main article.

A1 Coding Issues

DG model yields y using a quadratic function of degree days d, i.e., $y = \beta_0 + \beta_1 d^2 + \beta_2 d$, so the effect of a change in degree days from d_0 to $d_1 = d_0 + \Delta$ affects yields by

$$\begin{aligned} y_1 - y_0 &= \beta_0 + \beta_1 d_1^2 + \beta_2 d_1 - \beta_0 - \beta_1 d_0^2 - \beta_2 d_0 \\ &= \beta_1 (d_0 + \Delta)^2 + \beta_2 (d_0 + \Delta) - \beta_1 d_0^2 - \beta_2 d_0 \\ &= \beta_1 (2\Delta d_0 + \Delta^2) + \beta_2 \Delta \end{aligned}$$

In their STATA code, DG use the variable dd89_7000 to derive the change Δ but use the variable dd89 to derive the average number of baseline degree days d_0. However, because those two variables are inconsistent (dd89 appears to contain many errors, as described in the main paper), this calculation introduces additional noise in their predictions.

DG include 240 observations where corn yield equals zero and one observation where soybean yield equals zero. This is inconsistent with data from USDA's National Agricultural Statistics Service that show positive production for these observations. We speculate these errors stem from an incomplete merge. In our replication we drop observations where yields equal zero in DG's data.

DG weight observations by acreage, which varies over time and across counties. STATA's xtreg command does not allow weights to vary within groups in a panel so we use the average area of a county across all four years in the panel. Because the planting area is endogenous to year-to-year price fluctuations, weighting by the long-term average seems preferable.

In our replication of DG using their data we obtain slightly different damage estimates. DG derive the area-weighted sum of the changes in the weather variables. Summary statistics are provided using STATA's command `summ` and the mean value is then multiplied with the coefficient estimates of the corresponding weather variable. The log-files posted on the AER website reveal that sometimes the authors multiply the coefficients by numbers that differ from what was obtained in the `summ` command.

A2 Sensitivity Checks

Tables A1 , A2 , and A3 extend the results from Table 1 in the main paper for corn, soybeans, and profits, respectively. Columns labeled (1) in each table use year fixed effects, while columns labeled (2) use state-by-year fixed effects. Similar to Table 1 in the main article, columns (a) replicate the results in DG using their data and specification, while columns (b) replicate their specification using our reconstructed data set. Here we add several additional columns to show results from further sensitivity checks.

Columns (1c) and (2c) of each table include one additional variable, the square root of degree days above 34°C, to account for extreme temperatures. DG include such a variable to measure the potentially harmful effect of extreme heat on profits in Table 6 of their paper, but not in their yield regression. Columns (1c) and (2c) also use a slightly different calculation for degree days that accounts for within-day temperature variation.[1] Adding this additional variable increases the fraction of the variance explained through weather variables. Predicted climate change impacts are comparable under the Hadley II model, but more negative under the Haldey III model, which predicts larger temperature increases. It seems intuitive that nonlinear temperature effects are especially important for larger changes. We include the additional weather variable in subsequent regressions (columns (d) and (e)).

Columns (1d) and (2d) include counties for which data is available but missing in DG's analysis. We follow DG's definition and call a county irrigated if more than 10 percent of the farmland is irrigated. The results are reasonably robust to this change.

In columns (1e) and (2e) we limit the comparison to counties east of the 100 degree meridian, which are predominantly non-irrigated. Non-irrigated agriculture is quite unlike agriculture west of the 100^{th} meridian that predominantly uses heavily subsidized irrigation water. DG account for this difference by including separate weather coefficients for irrigated and non-irrigated counties. The problem with DG's approach is that water rights can (and do in California's agricultural regions) vary considerably on a sub-county level. More fundamentally, in irrigated areas the water input comes from groundwater or from precipitation falling elsewhere, so local precipitation is not a valid measure of water supply, and predicted climatic changes in precipitation do not measure predicted changes in access to irrigation water (see Schlenker, Hanemann & Fisher (2007)).When we limit the sample to predomi-

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[1] Construction of our weather data from raw sources is detailed in Schlenker & Roberts (2009). These data use the distribution of temperatures within a day between minimum and maximum instead of just the average for the day. This variance can make a difference due to the way growing degree days are defined, especially since maximum temperatures often exceed 34°C while average temperatures do not.

nantly non-irrigated areas, predicted impacts increase only slightly in the yield regressions. This is not surprising as little corn and soybeans are grown in the western United States and the regression uses area weights following DG. Predicted impacts vary more in the profit regression, but standard errors are also much larger.

A3 Storage and Lagged Weather Variables

DG argue in their reply that including lagged weather variables solves the storage problem described in the main article. We disagree. Consider, for example, the theoretical model of competitive storage by Scheinkman & Schechtman (1983). The model predicts a strong contemporaneous correlation between storage and weather shocks. Lagged weather variables cannot mitigate the problem of storage because lagged weather variables do not break the contemporaneous correlation between weather in period t and storage in period t, which enters the error term. Indeed, if weather is random as argued by DG, lagged weather should bear *no* relation to current weather, so past weather cannot control for endogeneity created by current weather. Besides, the theory indicates current weather has a much stronger influence on storage decisions than does past weather.

Using the notation from DG's original paper on page 367:

$$y_{ct} = \alpha_c + \gamma_{st} + X_{ct}\beta + \sum_i \theta_i f_i(W_{ict}) + u_{ct}$$

where y_{ct} is the economic profit in county c in year t, α_c is a county fixed effect, γ_{st} is a year fixed effect or state-by-year fixed effect, X_{ct} are time-varying county-specific control variables like soil quality, W_{ict} is weather variable i in county c in year t. Finally, u_{ct} is the error term.

The economic profit in period t is the value of production minus expenditures, i.e., $y_{ct} = p_{ct}q^p_{ct} - e_{ct}$, where q^p_{ct} is the amount produced and e_{ct} are expenditures. However, the Census of Agriculture only reports sales s_{ct} in a given period, which does not account for the amount stored q^n_{ct}, i.e., $s_{ct} = p_{ct}(q^p_{ct} - q^n_{ct})$ or $p_{ct}q^p_{ct} = s_{ct} + p_{ct}q^n_{ct}$. Substituting this expression into the previous equation above we obtain what DG estimate:

$$s_{ct} - e_{ct} = \alpha_c + \gamma_{st} + X_{ct}\beta + \sum_i \theta_i f_i(W_{ict}) + \underbrace{u_{ct} - p_{ct}q^n_{ct}}_{v_{ct}}$$

The value of storage $p_{ct}q^n_{ct}$ enters the combined error term v_{ct}. As long as there is a contemporaneous correlation between the weather shock W_{ict} and storage q^n_{ct}, this violates the identifying assumption of DG that $\mathbb{E}[f_i(W_{ict})v_{ct}|\alpha_c + \gamma_{st} + X_{ct}\beta] = 0$.

Including lagged weather variable does not break this contemporaneous correlation of W_{ict} and v_{ct}. Independent of the past, bad weather shocks increase price and good weather shocks decrease price, and this contemporaneous price movement induces storage behavior. One might wonder whether temporal fixed effects (year or state-by-year fixed effects) account for such storage behavior. Table 3 in our manuscript shows that this is not the case.

Here we present direct evidence that shows contemporaneous weather shocks influence decisions to store corn and soybeans, the nation's two largest crops, even after controlling for prices using year fixed effects and lagged weather shocks. Because no county-level inventory data exist, we use state-level data from the National Agricultural Statistics Service on stock levels of corn and soybeans by state. We regress state-level log inventory levels on damaging extreme heat (degree days above 29°C for corn and degree days above 30°C for soybeans) for the years 1950-2005.[2] In all regressions we include year fixed effects which will capture variation in price of a commodity. Results are reported in Table A4 .

In the main paper, we cite evidence that local price variation exists due to transportation costs or convenience yields. Local price variation will not be captured by yearly fixed effects, but correlated with weather. We observe evidence of this in our regression results: state-level variation in harmful heat is a significant predictor of state-level changes in inventories both for corn (column 1a) and for soybeans (column 2a). More extreme heat reduces yields and decreases inventories as farmers supplement reductions in output by depleting inventories, even after accounting for national prices using year fixed effects. Moreover, if we include two lags of the weather variable (reported in columns b of Table A4), the contemporaneous correlations between extreme heat and storage in the first row remain significant, and even increases for the case of corn.

A4 Robustness of Hedonic Model

In the first half of their paper, DG argue that results from the cross-sectional hedonic approach are not robust. They contend that cross-sectional studies rely on climate variations that are too closely associated with unobserved factors relating to location, and thus likely to be biased. DG's standard of robustness is the consistency of parameter estimates over

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[2] These weather variables have been shown to be the single best predictor of corn and soybean yields in the US (Schlenker & Roberts 2009). In this analysis weather variables are cropland-area weighted averages of our county-level weather measures.

different subsets of the data. However, when DG assess the robustness of the hedonic model, the weather variables they use, following Mendelsohn, Nordhaus & Shaw (1994), are average monthly temperature and precipitation for January, April, July and October; when they demonstrate the robustness of their fixed-effects model, the weather variables they use are summed precipitation and degree days over the growing season, which have been shown to be superior on both economic and econometric grounds in a hedonic analysis (Schlenker, Hanemann & Fisher 2006).[3]

The difference in the representation of weather confounds DG's comparison of robustness. For these reasons, we believe it is appropriate to repeat DG's tests of robustness, shown in their Figure 3, using the degree days representation of temperature and applying the tests to a hedonic model. This analysis is summarized in our Figure A1 . The figure shows predicted impacts from climate change using the hedonic model specified in Schlenker, Hanemann & Fisher (2006), estimated using various sets of control variables, in various census years. We replicate this analysis for farms east of the 100th meridian, an approximate boundary between rainfed and irrigated agriculture in the U.S., comprising 80% of county observations. Farms west of the 100th meridian rely on heavily subsidized irrigation water that is capitalized into farmland values. Some areas east of the 100th meridian also rely on irrigation. For example, 79 percent of the corn acreage in Arkansas was irrigated in 2007. The main difference is that access to water, where it exists, is much less heavily subsidized. Cline (1996) emphasizes that a hedonic approach in which observations are pooled assumes that farmers can obtain irrigation at existing marginal cost, which seems unrealistic. The bias induced by pooling observations from areas with subsidized irrigation with areas not primarily dependent on (subsidized) irrigation is demonstrated in Schlenker, Hanemann & Fisher (2005).[4]

Panel A in Figure A1 includes only climate variables with no other controls. If the climate variables are correlated with other variables, such as soil quality, the coefficient on the climate variables will be biased. Accordingly, panel B includes controls for both soil and socio-economic variables to avoid potential omitted variable problems. Finally, panel C additionally includes state fixed effects. Columns marked with a [0] are unweighted regressions, while columns marked with a [1] are regressions weighted by the square root of acres of farmland.

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[3] DG write that they are replicating Schlenker, Hanemann & Fisher (2005), but this is incorrect. They critique Schlenker, Hanemann & Fisher's (2005) *replication* of Mendelsohn, Nordhaus & Shaw (1994).

[4] DG assert that Schlenker, Hanemann & Fisher (2005) force the coefficient on irrigated and nonirrigated areas to be the same. The assertion is incorrect - the main point of that paper is that a *different* model is needed to deal with irrigated agriculture since the water supply there does not depend on local precipitation.

All estimates indicate strong negative impacts from climate change, even when all independent variables except for the climatic variables are dropped (panel A). Based on the tests suggested by DG, a hedonic model using degree days and precipitation summed over the growing season and applied to counties not primarily dependent on irrigation is robust. The model also passes a wide array of additional robustness tests discussed in Schlenker, Hanemann & Fisher (2006).

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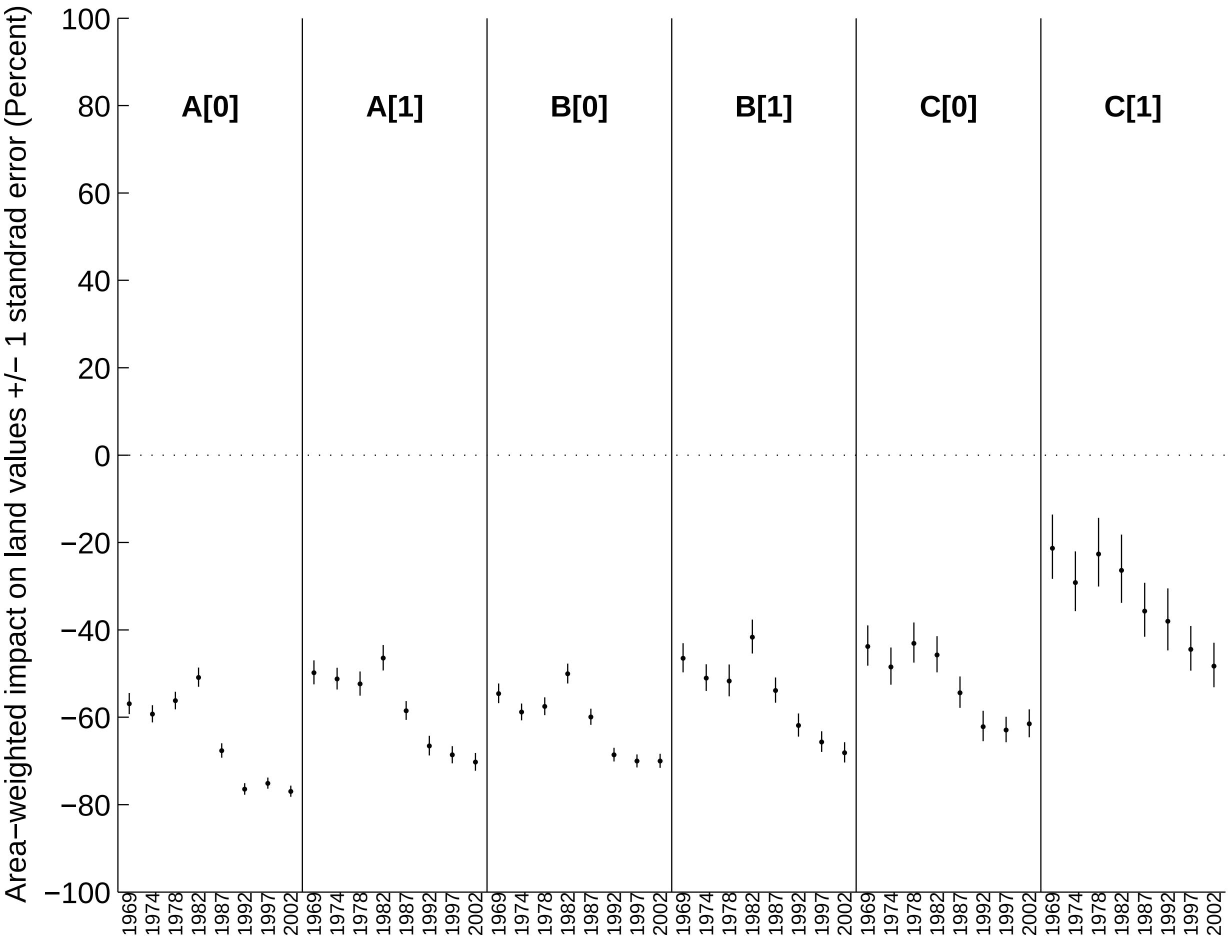
Figure A1 : Robustness of Hedonic Model using Degree Days



Notes: This is a replication of Figure 3 in Deschenes and Greenstone using the degree days from Schlenker, Hanemann & Fisher (2006) instead of average monthly temperatures. Panel A only includes climate variables with no other controls. Panel B includes controls for both soil and socio-economic variables to avoid potential omitted variable problems. Panel C additionally includes state fixed effects. Columns marked with a [0] are unweighted regressions, while columns marked with a [1] are regressions weighted by the square root of acres of farmland. The x-axis lists the year in which the cross-section is estimated.

Table A1 : Comparison of Various Data Sources in Corn Regressions

	(1a)	**(1b)**	**(1c)**	**(1d)**	**(1e)**	**(2a)**	**(2b)**	**(2c)**	**(2d)**	**(2e)**
Regression diagnostics										
Variance explained by weather	11.6%	19.6%	24.1%	24.8%	24.8%	7.0%	12.9%	16.9%	17.9%	20.4%
Climate change impact (Percent)										
Hadley II-IS92a scenario	-0.80	-10.61	-12.74	-14.66	-16.90	0.23	-20.36	-18.58	-16.71	-13.74
(s.e.)	(1.24)	(1.45)	(1.43)	(1.18)	(1.29)	(1.11)	(3.04)	(2.84)	(2.33)	(2.34)
[s.e. clustered by state]	[2.08]	[4.18]	[3.56]	[4.98]	[5.99]	[2.04]	[5.40]	[3.70]	[3.58]	[3.35]
Hadley III-B2 scenario		-42.01	-61.26	-67.36	-75.97		-60.58	-72.30	-71.95	-74.48
(s.e.)		(3.23)	(3.51)	(3.13)	(3.60)		(6.55)	(6.51)	(5.57)	(5.74)
[s.e. clustered by state]		[11.14]	[9.72]	[12.86]	[14.91]		[12.16]	[12.16]	[11.68]	[9.71]
Observations	6623	6623	6623	8562	7538	6623	6623	6623	8562	7538
Soil controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
State-by-Year FE	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes

Notes: This table summarizes and compares alternative regression models. Columns (a) replicate the results in DG using their code and data (a quadratic in degree days 8-32°C and precipitation); columns (b) are the same models as (a) estimated with our reconstructed data; columns (c) account for within-day temperature variation and extremely warm temperatures (square root of degree days above 34°C as an additional variable); columns (d) include observations that are missing in DG's data; columns (e) use only counties east of the 100 degree meridian that are treated as dryland. The variance explained by weather is 1 minus the ratio of the residual variance of the full specification over the residual variance of the model excluding weather. Standard errors in round brackets cluster by fips code following DG, while standard errors in brackets cluster by state.

Table A2 : Comparison of Various Data Sources in Soybeans Regressions

	(1a)	**(1b)**	**(1c)**	**(1d)**	**(1e)**	**(2a)**	**(2b)**	**(2c)**	**(2d)**	**(2e)**
Regression diagnostics										
Variance explained by weather	14.4%	30.6%	37.0%	35.9%	35.4%	12.0%	19.4%	22.1%	20.8%	20.9%
Climate change impact (Percent)										
Hadley II-IS92a scenario	-2.73	-15.63	-16.74	-14.77	-14.85	1.46	-18.36	-14.42	-13.25	-13.77
(s.e.)	(1.38)	(1.60)	(1.55)	(1.20)	(1.21)	(0.94)	(2.58)	(2.50)	(2.01)	(2.01)
[s.e. clustered by state]	[2.08]	[4.93]	[4.45]	[4.10]	[4.16]	[1.15]	[3.85]	[3.21]	[3.40]	[3.36]
Hadley III-B2 scenario		-51.59	-70.72	-64.25	-65.01		-55.63	-60.30	-56.14	-58.28
(s.e.)		(3.65)	(3.65)	(2.91)	(2.93)		(5.72)	(5.65)	(4.63)	(4.67)
[s.e. clustered by state]		[11.80]	[9.88]	[8.99]	[9.35]		[9.32]	[8.71]	[8.44]	[8.25]
Observations	5140	5140	5140	6742	6504	5140	5140	5140	6742	6504
Soil controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
State-by-Year FE	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes

Notes: This table summarizes and compares alternative regression models. Columns (a) replicate the results in DG using their code and data (a quadratic in degree days 8-32°C and precipitation); columns (b) are the same models as (a) estimated with our reconstructed data; columns (c) account for within-day temperature variation and extremely warm temperatures (square root of degree days above 34°C as an additional variable); columns (d) include observations that are missing in DG's data; columns (e) use only counties east of the 100 degree meridian that are treated as dryland. The variance explained by weather is 1 minus the ratio of the residual variance of the full specification over the residual variance of the model excluding weather. Standard errors in round brackets cluster by fips code following DG, while standard errors in brackets cluster by state.

Table A3 : Comparison of Various Data Sources in Profit Regressions

	(1a)	**(1b)**	**(1c)**	**(1d)**	**(1e)**	**(2a)**	**(2b)**	**(2c)**	**(2d)**	**(2e)**
Regression diagnostics										
Variance explained by weather	0.4%	1.5%	1.5%	1.1%	1.2%	0.4%	0.6%	0.8%	0.4%	0.3%
Climate change impact (Percent)										
Hadley II-IS92a scenario	-6.63	-36.50	-31.38	-25.44	-28.94	3.75	1.21	4.45	5.60	1.05
(s.e.)	(3.03)	(5.41)	(5.28)	(4.11)	(4.90)	(2.82)	(12.88)	(12.93)	(10.96)	(6.59)
[s.e. clustered by state]	[4.98]	[10.34]	[10.82]	[9.76]	[14.88]	[3.98]	[15.18]	[17.06]	[14.67]	[13.55]
Hadley III-B2 scenario		-55.99	-44.29	-33.89	-69.40		-3.28	6.40	6.75	-7.63
(s.e.)		(8.93)	(9.52)	(7.55)	(9.01)		(20.61)	(20.99)	(17.82)	(12.25)
[s.e. clustered by state]		[16.58]	[19.80]	[18.60]	[26.08]		[25.12]	[28.80]	[25.37]	[26.47]
Observations	9024	9024	9024	11949	9653	9024	9024	9024	11949	9653
Soil controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
State-by-Year FE	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes

Notes: This table summarizes and compares alternative regression models. Columns (a) replicate the results in DG using their code and data (a quadratic in degree days 8-32°C and precipitation); columns (b) are the same models as (a) estimated with our reconstructed data; columns (c) account for within-day temperature variation and extremely warm temperatures (square root of degree days above 34°C as an additional variable); columns (d) include observations that are missing in DG's data; columns (e) use only counties east of the 100 degree meridian that are treated as dryland. The variance explained by weather is 1 minus the ratio of the residual variance of the full specification over the residual variance of the model excluding weather. Standard errors in round brackets cluster by fips code following DG, while standard errors in brackets cluster by state.

Table A4 : Regressing Storage on Contemporaneous and Lagged Weather

	Corn		**Soybeans**	
	(1a)	**(1b)**	**(2a)**	**(2b)**
Extreme Heat	-1.57e-3**	-2.57e-3***	-4.60e-3***	-4.18e-3***
(s.e.)	(7.65e-4)	(7.73e-4)	(9.41e-4)	(9.05e-4)
Extreme Heat (Lag 1)		4.02e-4		-2.64e-3***
(s.e.)		(7.85e-4)		(9.18e-4)
Extreme Heat (Lag 2)		7.36e-4		-2.71e-3***
(s.e.)		(7.50e-4)		(9.39e-4)
Observations	1535	1453	1006	971
Year FE	Yes	Yes	Yes	Yes

Notes: This table regresses state-level log inventory levels on extreme heat (degree days 29°C for corn and degree days 30°C for soybeans). Stars indicate significance levels: *** 1%; ** 5%; and * 10%.