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The Economic Impacts of Climate Change on Agriculture: Accounting for Time-invariant Unobservables in the Hedonic Approach

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By Ariel Ortiz-Bobea*

Abstract

I propose a strategy of measuring the long-run economic impact of climate change on farmland values that tackles the elusive problem of time-invariant spatially-dependent unobservables in the hedonic approach. The strategy exploits that a county's agricultural productivity is primarily influenced by its own climate, and the fact that climate assignment appears random conditional on average county-neighborhood characteristics. Results suggest that large impacts of climate change on US agriculture seem unlikely. Findings are robust to multiple checks and cannot be attributed to measurement error. Ignoring such confounders considerably overstates long-run climate change impacts on the sector. (JEL Q15, Q51, Q54, R21)

There is a growing consensus that climate change is the global environmental challenge of our era, and there is a pressing need for robust approaches of estimating its economic impacts. Agriculture has received unparalleled attention in this regard, due to its inherent dependence on climate and its central role in global development (Schelling, 1992). A fundamental debate in the literature revolves around the consistent estimation of causal climate effects on measures of agricultural welfare. On one hand, the so-called Ricardian or

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hedonic models estimating the cross-sectional effect of climate on farmland values appear vulnerable to time-*invariant* unobservables (see Schlenker et al., 2005, henceforth SHFa and Deschênes and Greenstone, 2007, henceforth DG). On the other hand, panel models estimating the effect of weather fluctuations on agricultural profits appear vulnerable to time-*varying* unobservables (see Fisher et al., 2012a and Deschênes and Greenstone, 2012). Despite a heated methodological debate over the past two decades, the literature on the impacts of climate change on US agriculture remains inconclusive.

This paper proposes a novel identification strategy to estimate the long-run effect of climate on farmland values. The approach posits, in line with the literature, that the spatial gradient of farmland value partly reflects the shadow value of climate. One can therefore rely on the estimated shadow values to derive long-run estimates of climate change on agriculture that implicitly account for the full range of farmer adaptations to climate. In addition, the proposed approach makes two identifying assumptions. First, it posits that the agricultural productivity in a non-irrigated county is primarily, if not only, affected by its *own* climate, and not by the climate of neighboring counties. Throughout the paper I refer to the *own*-climate effect as the *direct* effect of climate.

Second, the approach posits that climate assignment to a county is as good as random *conditional* on average observable characteristics of its neighboring counties. The idea is reminiscent of a distributed lag time-series model (with one lag) but applied to cross-sectional data indexed in a geographical space. Only the *direct* climate estimates (or *current* estimates in the time-series analogy) are relied upon for inference due to the aforementioned identifying restrictions. To simplify my exposition I will sometimes refer to climate variation conditional on neighborhood characteristics as *direct climate variation*, and to models relying on such variation as *direct models.*

I find convincing evidence that climate appears randomly assigned to counties *conditional* on observables of neighboring counties. In contrast, I find that climate does *not* appear randomly assigned to counties unconditionally or conditional on state or district fixed effects. If these observables are indicative of the distribution of unobserved determinants of farmland values (Altonji et al., 2000), then consistent estimation becomes possible based on direct climate variation. This identification strategy resolves a long-standing challenge in this literature. Relative to benchmark hedonic approaches (relying on unconditional or within-state or within-district climate variation), the proposed method can additionally account for spatially-dependent time-invariant unobservables that are regionally correlated with climate.[1]

The workings of the proposed approach are perhaps best illustrated with an example. Suppose people have a preference for certain climates (Albouy et al., 2016) and that residential sorting over time leads to growth and development pressure on farmland in areas with desirable climates to humans. Suppose further that climate-based sorting mainly operates into regions, not into counties within regions.[2] This phenomenon leads to a *regional* correlation of development pressure on farmland with climate which could bias benchmark estimates of climate effects on agricultural productivity rents.[3] However, climate remains uncorrelated with development pressure *conditional* on regional or "neighborhood" climate and other characteristics. The proposed identification strategy is based on this conditional climatic variation which isolates a consistent *direct* effect of climate.

I use the proposed identification strategy to evaluate the effect of climate change on US farmland values. I find no robust indication of large effects of climate change on agriculture. Climate change impact estimates on farmland values are relatively small and remain undistinguishable from zero for all climate scenarios. The preferred estimates range between -11.0 and $+12.9\%$ projected mean change on farmland values across all scenarios, time horizons and regression weights. This is equivalent to yearly changes of -5.7 to $+6.7$ billion, respectively. These estimates are surprisingly stable over sixty years of farmland value data and across regional subsets of the data, and are robust to the choice of dependent

[1] This conceptually coincides with the plausible structure of potential omitted variables that have been proposed in the literature, such as unobserved soil characteristics or development pressure (see Mendelsohn et al., 1994 and DG 2007).

[2] That is, people consider the broad climatic characteristics of a region when considering a move, but then sort further into counties based on non-climatic amenities or employment opportunities. I later show that increases in housing prices, a plausible indicator of desirable living locations, are not correlated with climate variables conditional on neighborhood-average changes in housing values. Note that I define a region as a set of spatially contiguous counties that need not correspond to administrative groupings such as districts or states.

[3] This would also be the case for state- or district-fixed-effect estimates if regions are loosely defined and do not follow state or district administrative boundaries.

and independent variables, to county neighborhood definitions and neighborhood weighting schemes, as well as climate change models and scenarios.

On the other hand, benchmark hedonic results with state-fixe-effect point to large climate change damages ranging from -18.1 and -74.5% projected mean change on farmland values across all scenarios, time horizons and regression weights. Those estimates are statistically significant and equivalent to yearly *losses* of 9.4 to 38.9 billion. These figures are in line with previous findings (e.g. Schlenker et al., 2006, henceforth SHFb, and Fisher et al., 2012b). However, these benchmark estimates are *not* stable over time or across regional subsamples.[4] As a result, the added robustness of the proposed strategy makes a clear practical difference.

The are three main threats to validity to the proposed strategy. First, the framework assumes climate affects agriculture through a direct local channel. This paper focuses on the mostly non-irrigated counties of the Eastern US so the water supply should overwhelmingly originate from local rainfall.[5] In addition, economic spillover effects from agriculture tend to be small (Hornbeck and Keskin, 2015). Thus, any indirect climate effect of neighboring counties should be minor, if any exists at all. Second, the incorporation of neighborhood-average controls may introduce collinearity. This inflates estimation uncertainty but does not cause bias. I find that estimation precision is indeed affected for least squares estimates but remains acceptable but for the most extreme scenarios. Finally, the proposed approach exacerbates vulnerability to classical measurement error of climate variables. To address this concern I conduct a series of placebo tests showing that identification of direct climate effects is proper. I also devise a strategy to indirectly rule out the presence of attenuation bias.[6]

[4]Elsewhere (Ortiz-Bobea, 2016), I find that exurban and rural development pressure on farmland in select US regions operates as a major omitted variable in the hedonic framework.

[5]This follows the sample restrictions in SHFa and SHFb to avoid the confounding effect of irrigation. The assumption is debatable in irrigated areas because runoff from precipitation in certain counties may affect the water supply for irrigation in neighboring counties. Moreover, only a relatively small fraction —in the range of 5 to 25%— of precipitation in the Eastern US ends up as surface runoff (personal communication from Meredith Reitz, United States Geological Survey, 2/10/2016). This runoff ends up in streams and rivers which are not heavily used for irrigation in the Eastern US. According to the 2012 US Agricultural Census, only 11.9% of agricultural land is irrigated in the Eastern US. In addition, irrigation in the East tends to be less intensive than in the West. For instance, the 2010 USGS water withdrawal estimates indicate that the median Eastern county uses 625 MGal/day/acre of water over irrigated lands, which is considerably lower than the 2,822 MGal/day/acre for the median Western county.

[6]The strategy consists in contrasting estimates of the proposed model with those of another model with comparable vulnerability to measurement error *but* with a different vulnerability to time-invariant spatially-

This research is naturally close to the seminal work of Mendelsohn et al. (1994, henceforth MNS) and subsequent improvements by SHFa, SHFb, Fezzi and Bateman (2015) and Severen et al. (2016). However, these studies rely on sample-wide or within-state variation to identify climate effects (benchmark models), which is well-known to be problematic. Indeed, I find climate is correlated with observables even within relatively small agricultural districts. This paper agrees with the diagnosis in DG (2007) regarding the vulnerability of the previous hedonic literature to omitted-variable-bias. DG (2007, 2012) instead propose a panel model with county fixed effects to control for time-invariant unobservables.[7] However, the approach yields short-run estimates which remain inconclusive in the long-run when negative short-run effects are found, as in DG (2012). My solution is considerably different and relies on a cross-sectional model that directly yields long-run effects of climate on farmland values. Because *direct* climate variation is arguably orthogonal to unobservables (tested on observable characteristics), the proposed approach resolves a long-standing challenge in this literature.

The proposed approach bears some resemblance to models and strategies adopted in other literatures. An example is Bajari et al. (2012), who recover hedonic prices of air pollution accounting for time-varying correlated unobservables in housing values. The study relies on prior sales prices to control for temporal autocorrelation of unobservable attributes of a house or neighborhood. The approach I propose is close to a spatial analog of their strategy and exploits the *spatial* autocorrelated structure of unobservables in recovering climate effects.[8] There is also some similarity with Muehlenbachs et al. (2015) who use a difference-in-differences-nearest-neighbor-matching estimator to explore the effects of shale gas development on property values. That paper assumes that time-invariant unobservables operate at a different (larger) spatial scale than the treatment (drilling), so that treatment appears random within each matched pair of properties.[9] In this paper, I also seek to exploit

dependent confounders.

[7]That paper also includes state-by-year fixed effects to account for regional price shocks.

[8]A notable difference is that I include (spatial) lags of *independent* variables as controls, not lags of the dependent variable.

[9]The identifying assumption is that nearby matching properties (within the same census tract) have common or similar neighborhood unobservables, which are therefore uncorrelated with treatment (drilling) status. The paper uses an array of empirical techniques including this matching estimator based on cross-sectional data. The matching estimator also allows to control for time-varying unobservables given that

the fact that the "treatment" (climate) is correlated with unobservables at certain spatial scales but not at others.

To a large extent, the model in this paper borrows its structure from models used to estimate spillover effects (e.g. Jaffe, 1986; Anselin et al., 1997; Bandiera and Rasul, 2006; LeSage and Pace, 2009; Conley and Udry 2010). These models are generally conceived to estimate the effect of independent variables of "neighboring" observations on the dependent variable of an observation of interest.[10] However, climate "spillovers" from neighboring counties are negligible by construction and regressors capturing neighboring effects operate as controls for spatially-dependent unobservables regionally correlated with climate.

Finally, this paper bears some surprising resemblance to the neighborhood sorting literature in labor, which exploits the fact that people sort themselves on unobservables into relatively coarse groupings or spatial scales, but not into finer ones. An example is Bayer et al. (2008), who compare neighbors residing on the same versus nearby city blocks to infer the role of local informal social interactions on labor outcomes. That study posits that people sort on unobservables into residential neighborhoods, but not into blocks within these neighborhoods, rendering "block assignment" random conditional on the neighborhood.[11] The identification strategy proposed here has a very similar spirit.

This paper makes three main contributions. First, it proposes a hedonic approach of estimating long-run climate change impacts on agriculture that demonstrably reduces omitted variable bias concerns that have plagued the literature. Its robustness can be tested *ex ante* against observable characteristics, enhancing its wide applicability to different contexts around the world. Second, the empirical results contribute to the overall debate on climate change impacts on US agriculture by inducing an internally consistent order of climate change impact estimates. As expected from theory, the long-run estimates in this study are more optimistic than the short-run estimates of the restricted profit panel approach in DG

matching is restricted to transactions within the same year.

[10] The concept of distance varies in these references and may refer to proximities in technological, social or geographical space between agents.

[11] The identifying assumptions are that unobserved attributes among block residents are uncorrelated *conditional* on characteristics of residents in nearby blocks (neighborhood) *and* that social interactions operate locally at the block-level. This strikingly resembles the present context in which unobservables are presumed orthogonal to climate *conditional* on the characteristics of neighboring counties *and* in which the effect of climate is presumed to be direct.

(2012). Overall, these new findings provide a neutral but cautionary long-run outlook for US agriculture. Third, the paper motivates an approach with applicability to other fields for estimating the direct causal effect of an exogenous variable in a cross-sectional setting plagued by time-invariant spatially-dependent omitted variables.

The remainder of the paper is structured as follows. In section 1, I present some background and relate my contribution to the treatment of unobservables in this literature. In section 2, I discuss and motivate the econometric strategy. I devote section 3 to data sources and summary statistics and present the empirical results based on US agriculture in section 4. I then discuss these results more broadly in section 5 and conclude in section 6.

1 Previous Literature

This paper builds upon the seminal work of MNS (1994), who introduced the so-called Ricardian approach, the first major attempt to econometrically estimate the potential impacts of climate change on agriculture. This hedonic method posits that climate is capitalized in farmland values because farmers allocate land to its most profitable use given their climatic constraints. The approach has consisted in estimating the effect of the cross-sectional county-level variation in climate on farmland values. The estimated shadow values of climate are subsequently used to make projections about the long-run climate change impacts on the sector under current technological and market conditions.

MNS initially found a small positive effect of climate change on US agriculture, which at the time contradicted earlier negative findings based on biophysical or production function approaches (Adams, 1989; Adams et al., 1990; Kaiser et al., 1993; Adams et al., 1995). However, SHFa (2005) finds that MNS confounds high temperatures with irrigation, which would understate the detrimental effect of a warmer climate. Because the pricing of irrigation water is highly distorted and its future supply is uncertain, SHFa restrict its conclusions to the eastern half of the US that is mostly non-irrigated. In contrast, the revised SHFa estimates point to major climate change damages on the non-irrigated eastern US. SHFb (2006) finds similar damages on non-irrigated US counties in a comprehensive hedonic study.[12]

There have been four main strategies to either reduce or detect the influence of omitted variables in the hedonic framework. The first approach, which is universal, is to simply add control variables to the model. As shown later (table 7), the fact that observable determinants of farmland value are correlated with climate suggests the same may apply to unobservables. This approach is largely unconvincing and cannot be relied solely upon. A second and related approach consists in examining the stability of climate change impacts estimates when omitting control variables. Stable climate change estimates would suggest that control variables are weakly correlated with climate variables. However, this check is uninformative regarding the strength of the correlation of climate variables with unobservables.

The third approach, which is also common practice, consists in indirectly assessing the presence of *time-varying* omitted variables by analyzing the stability of estimates for multiple cross-sections over time. Naturally, this check cannot detect omitted variables that *do not* vary over time or vary very slowly, which is possibly a greater concern. The fourth approach, first introduced in SHFb, simply consists in including state fixed effects to rely on within-state variation for the estimation of climate effects. This approach is effective when unobservables mainly operate as common farmland price shocks that follow state boundaries (e.g. a state-level policy change). However, some unobservables such as soil quality or development pressure are not exactly expected to follow coarse administrative lines. This leaves room for correlation between climate and unobservables within states or similar administrative units.

The hedonic approach is theoretically appealing but its empirical limitations have been widely conceded. Unobservable time-invariant or slowly-varying factors such as soil quality or the option value of farmland could be confounded with climate biasing estimates in unknown direction.

This shortcoming prompted DG (2007) to develop an alternative panel method that

[12] SHFb introduces several methodological improvements, including a new set of climate variables that improve model fit, and a more efficient GMM estimator that parametrically accounts for the spatial correlation of disturbances (Kelejian and Prucha, 1999).

estimates the effect of presumably random weather fluctuations on yearly agricultural profits, and controls for time-invariant unobservables that appear to plague cross-sectional hedonic studies. The approach estimates a short-run effect of climate on profits and, as a result, the small effect found in DG is interpreted as a possibly positive impact of climate change on long-run sector profits. This finding contradicts preceding negative estimates in SHFa and SHFb.

However, Fisher et al. (2012b) find that both errors in weather data and the countervailing smoothing of income by farmers to weather shocks, bias DG results toward zero. In a noteworthy response, DG (2012) acknowledge the data issues but propose a distributed lag panel model that addresses the latter criticism. While still negative, the revised DG (2012) damages are substantially smaller —less than half— than those based on the hedonic approach.

This leaves the literature in an unsettled and puzzling state. First, a negative short-run effect as in DG (2012) remains inconclusive regarding the sign of the long-run effect. Second, the ordering of short and long-run effects is counterintuitive because the long-run estimates of the hedonic approach should be more optimistic than the short-run estimates of the profit panel approach, not the opposite.[13]

In light of the importance of the problem and the state of the literature, this paper explores an approach that demonstrably reduces the vulnerability of long-run estimates of climate change impacts to an additional class of omitted variables. The proposed approach estimates the long-run climate effects on farmland value *conditional* on average characteristics in the vicinity of each observation. I now present the proposed strategy in greater detail.

[13] An interesting study by Burke and Emerick (2012) exploits spatial variation in climatic trends across the US to estimate the magnitude of adaptation in US crop agriculture. The study finds that little to no adaptation has occurred in response to recent changes in climate. However, recent climate trends in the region are relatively small so the study does not address the point of this paper, which is the long-run impact of major climate change if farmers are able to adapt given current technological possibilities and market prices.

2 New Econometric Strategy

The hedonic approach posits that agricultural producers chose to allocate their land to the most valuable uses given local climatic constraints. Because farmland capitalizes the discounted future stream of expected profits from the land, the spatial gradient of farmland value should partly reflect the shadow value of climate. This framework allows the implicit modeling of the role of climate in agriculture without explicitly considering individual production decisions.

One can approximate the outer envelope of the farmland value gradient with a model of the form:

$$\begin{aligned} y_{ct} &= \mathbf{X}_c^{'}\beta + \mathbf{Z}_{ct}^{'}\eta + \epsilon_{ct} \qquad (1) \\ \epsilon_{ct} &= u_c + e_{ct} \end{aligned}$$

where y_{ct} is farmland value in county c and year t, $\mathbf{X}_c$ is a vector of climate variables, $\mathbf{Z}_{ct}$ is a vector of observed controls that may vary over time, and ϵ_{ct} is a an error term that is unobservable to the econometrician. Furthermore, this disturbance can be decomposed into a permanent (u_c) and a transitory (e_{ct}) factor.

The major challenge of the hedonic approach is the consistent estimation of β. This would allow reliable climate change impact projections of the form $\Delta\hat{y}_c = \Delta\mathbf{X}_c^{'}\hat{\beta}$, where $\Delta\mathbf{X}_c$ is a vector of climate change projections for all climate variables included in the model. Consistent estimation of β requires $E[\mathbf{X}_c^{'}\epsilon_{ct}|\mathbf{Z}_{ct}] = 0$. This condition is violated if either permanent (u_c) or transitory (e_{ct}) omitted factors covary with climate variables $\mathbf{X}_c$. As noted in section 1, the stability of hedonic estimates over time is an implicit check of whether $E[\mathbf{X}_c^{'}e_{ct}|\mathbf{Z}_{ct}] = 0$ holds. The main unresolved problem is that $E[\mathbf{X}_c^{'}u_c|\mathbf{Z}_{ct}] = 0$ may not hold and cannot be easily verified. That is, climate might be correlated with time-invariant (or slowly-varying) unobservables that also explain farmland values.

The proposed identification strategy isolates the effect of climate by conditioning the regression with variables capturing average characteristics of neighboring counties. The identifying assumption is that u_c is orthogonal to climate *conditional* on the neighborhood-average controls and that the climate effect on agriculture is primarily direct. Essentially, the proposed model augments (1) in the following form:

$$y_{ct} = \mathbf{X}_c^{'}\beta^d + \mathbf{Z}_{ct}^{'}\eta^d + \mathbf{X}_{N(c)}^{'}\beta^i + \mathbf{Z}_{N(c)}^{'}\eta^i + \epsilon_{ct} \tag{2}$$

where $\mathbf{X}_{N(c)}$ and $\mathbf{Z}_{N(c)}$ are weighted averages of the climate and control variables of the nearby surrounding counties of c (excluding c), respectively. The set of neighbors to county c is noted $N(c)$, so that $\mathbf{X}_{N(c)} = \sum_{j \in N(c)} w_j \mathbf{X}_j$ where w_j are weights that sum to unity. I consider various weighting schemes and neighborhood definitions in the empirical part of the paper.

Notice the model in (2) has two β vectors which essentially partition the effect of climate on farmland values into two components. The vector β^d identifies the *direct* (local) effect of climate on farmland values, and β^i captures the *indirect* (neighborhood) effect of climate on farmland values. If the "true" climate effects on agricultural rents are primarily *direct* —as one should expect in a non-irrigated context— then these are solely embodied in β^d. On the other hand, β^i essentially captures the effect of unobservables that are regionally confounded with climate. Therefore, climate change impact estimates should be based on estimates of β^d. This marginal *direct* effect of climate on farmland values is essentially estimated off the neighborhood-conditioned cross-sectional variation of climate. This approach eliminates the effect of spatially-dependent omitted variables that are incidentally regionally correlated with climate.

Alternatively, the identification strategy in this paper assumes $E[\mathbf{X}_c^{'}\epsilon_{ct}|\mathbf{X}_{N(c)}, \mathbf{Z}_{ct}, \mathbf{Z}_{N(c)}] = 0$. Fortunately, this assumption can be tested against observables by checking whether observed determinants of farmland values are orthogonal to climate variables conditional on neighborhood-average values of these observables. In other words, we have to verify whether climate is just *regionally* correlated with observables, or if that correlation extends to a local *direct* level across the sample. To verify this, I later estimate a series of linear models regressing individual observables on individual climate variables conditional on the neighborhood-average value of the observable. If climate variables have any explanatory power in predicting observables in this setting, then the identifying assumption fails.

Throughout the paper I present least squares estimates with standard errors adjusted for spatial dependence of an unknown form following the semi-parametric approach developed by Conley (1999).[14] In the online appendix and in the summary discussion table (table 9), I also report estimates based on linear models weighted by the squared root of farmland area following modeling choices in MNS, SHFa and DG (2007). In addition, I report results based on the spatial error model GMM estimator developed by Kelejian and Prucha (1999) and used in SHFb. This estimator is more efficient than OLS but imposes a parametric structure on error dependence. Results based on these alternative estimators are comparable, although the associated measures of uncertainty differ.

3 Data Sources and Summary Statistics

A. Data Sources

This study relies on four major types of data: agricultural, climate, soil quality and general socio-economic data. I rely on standard data sources in this literature to isolate the contribution of the proposed identification strategy. Table 1 provides a summary of key variables in the study and their source. A portion of the agricultural data was obtained directly from *Quick Stats,* the US Department of Agriculture's (USDA) online database. This database provides data from the US Census of Agriculture as well as from various national surveys, such as the Cash Rent Survey, all conducted by USDA.

The Census provides county-level aggregates of data collected from all farms.[15] The dependent variable in the study, farmland value, is obtained from the Census by asking farmers their estimate of the current market value of their land and buildings. Unfortunately,

[14]Conley proposes a spatial Heteroskedasticity and Autocorrelation Consistent (HAC) covariance matrix of the OLS estimator. The spatial HAC covariance matrix is obtained by performing a weighted average of spatial autocovariances of observations falling within a certain cutoff distance. The weighting scheme in this paper is a Bartlett kernel that linearly declines from 1 to 0 with distance up to the cutoff distance. Throughout the paper the cutoff is set at 200 miles. Accounting for the spatial correlation of disturbances in this context is crucial to avoid overconfident estimates due to deceivingly small standard errors.

[15]USDA defines a farm as an operation having sold more than $1,000 of agricultural products during the census year.

Table 1: Variables and Data Sources

Variable(s)	Time Periods Used	Resolution	Source
Agriculture:			
Value of land and buildings, farmland area (Census)	1997, 2002, 2007, 2012	County	USDA *Quick Stats*
	1950, 1954, 1959, 1969, 1974, 1978, 1982, 1987, 1992	County	Haines (2004)
Non-irrigated cropland cash rent (Cash Rent Survey)	2009-2012	County	USDA *Quick Stats*
Climate:			
Daily minimum and maximum temperature	1950-2005	4 km	Schlenker and Roberts (2009)
Monthly average temperature and precipitation	1912-2005	4 km	PRISM
Cropland weights for grid-to-county aggregation	2008-2014	30 m	USDA CDL
Controls:			
Population	1970-2012	County	US Census
	1950, 1960	County	US Census via Haines (2004)
Personal income per capita	1969-2012	County	BEA
Family income	1949, 1959, 1969	County	US Census via Haines (2004)
Soil variables: average water capacity, clay content, minimum permeability, K-factor of topsoil, best soil class	N/A	Polygon, sub-county scale	USGS STATSGO
Altitude		100 m	USGS, National Atlas of the US
Satellite vegetation indices		250 m	USGS EROS Center, eMODIS Remote Sensing Phenology Data

Notes: Only farmland values for 1964 were missing from Haines (2004) at the time of data collection.

Quick Stats only provides Census data since 1997 so older Census data going back to 1950 were obtained from Haines (2004). To the best of my knowledge, this is the first study to incorporate this historical Census data for the purpose of climate change impact analysis. The goal is to assess the stability of hedonic estimates over much longer time periods.

The primary climate data source is Schlenker and Roberts (2009), who provide a detailed daily gridded dataset for 1950-2005 based on the interpolation of daily weather station data and monthly gridded data from the PRISM Climate Group , which is USDA's official climatological data.[16] These and the underlying PRISM data have a spatial resolution of just 2.5 miles and cover the contiguous US.

Because the underlying climate data is gridded it needs to be aggregated to the county level to match the agricultural observations. I perform this aggregation by weighting each native PRISM grid by the amount of cropland it contains based on USDA's Cropland Data Layer (CDL).[17] Because I explore climate variables for time periods prior to 1950, I also rely on the monthly temperature data from PRISM, which is available since 1895.[18] I should emphasize that the level of spatial resolution of the underlying climate data is orders of magnitude higher than the county-level scale of the analysis.[19]

The hedonic model relies on the cross-sectional variation of farmland values which are affected by well-known time-invariant factors such as certain soil quality characteristics. Soil quality data was obtained from the US Geological Survey's (USGS) STATSGO database which aggregates similar soils into distinct polygons across the country. Similar to climate data, county-level soil quality data is obtained by weighting each soil polygon by the amount

[16] Following Schlenker and Roberts (2009), I rely on the monthly precipitation variables from PRISM, rather than on re-aggregated daily precipitation interpolations which appear to be noisy.

[17] The CDL provides 30 meter resolution land cover pixels corresponding to over 100 classes. The weights were based on cropland pixel counts falling within each PRISM data grid. The average of CDL cropland counts for years 2008-2014 were used. In the online appendix, I provide a map of the cropland weights as well as a table with all land cover classes that constitute cropland. Detailed crop cover data for older cross-sections (e.g. 1950) is not available. However, because farmland area has decreased by 27.4% from 1950 to 2012 and the most productive farmland has most likely remained in farms, cropland weights for recent periods can be thought as capturing the "core" agricultural area of each county for older time periods.

[18] Just recently, the PRISM group released daily data with 4 kilometer resolution free of charge. However, the earliest year is available is 1981.

[19] The average county area east of the 100th meridian west (referred to as the Eastern US in this paper) is 610.7 squared miles while the PRISM data grids are about 6.25 squared miles. Therefore, a county of average size "fits" about 98 distinct PRISM data grids. Ninety five (ninety nine) percent of counties in the Eastern US can "fit" at least 33 (5) distinct PRISM data grids.

of cropland based on the CDL.

The analysis also includes a set of economic control variables, namely county-level population density and income per capita. These controls have been introduced in an attempt to capture the influence of population pressures on farmland. County-level population data comes from the US Census and Intercensal Estimates. These data are only available online from the US Census for years 1970-2012. Prior census years were obtained from Haines (2004). Intercensal Estimates prior to 1970 were not readily available. I therefore interpolate population between decennial censuses for each county using a natural spline.[20]

Data on per capita personal income is obtained from the Bureau of Economic Analysis (BEA). Unfortunately, these only span the 1969-2012 period. I use family income from the US Census as a substitute for earlier time periods.[21] Similar to population, I interpolate family income between decennial censuses for each county using a natural spline.[22] All values in the paper are expressed in 2012 USD using the Consumer Price Index (CPI).[23]

Finally, the paper relies on auxiliary data for a series of placebo tests to determine whether direct climate effects can be reliably estimated. Data on altitude for each 100 by 100 meter grid cell for the lower 48 US States was obtained from USGS's National Atlas. This information was aggregated to the county level by averaging altitude values over grid cells falling within each county.

In the auxiliary analysis I also rely on satellite measurements of vegetation or "greenness" indices. These measurements are obtained for each 250 by 250 meter grid cell of the lower US States from the USGS EROS Center. Because my interest is to explore the spatial

[20]Just as with intercensal estimates, this approach is not meant to capture year-to-year fluctuations in population with precision, but provide an approximation of the population level between census years. Results based on the closest Census year are virtually identical.

[21]Note these variables are not directly comparable because family size varies across the country. I therefore compare personal capital income and family income for 1969 which is the earliest overlapping year. The correlation is 0.82 for all US counties and 0.87 for counties in the eastern sample. There are a few outliers. Counties with relatively low family income relative to personal income per capita include places like New York county (NY) or small coastal counties such as Kenedy county (TX). Counties with relatively low personal income per capita relative to family income per capita include highly remote counties such as Hinsdale county (CO) where family size is likely to be large. Outliers do not tend to be highly agricultural in nature, so this variable seems appropriate.

[22]Again, this is not intended to capture short run fluctuations in income within counties, but to preserve the variation in income across counties.

[23]Other studies have used the GDP implicit price deflator. I use the CPI because it is available over a longer time span and these two indexes are virtually indistinguishable within their overlapping time period with a correlation of 0.997 over 1947-2012.

association between climate and these vegetation indices, I rely on the time-averaged indices (2001-2014) rather than yearly satellite measurements that may be prone to weather fluctuations. The gridded data for the Eastern and Western halves of the country were merged, purged of grid cells corresponding to water bodies and aggregated to counties using the aforementioned procedure. I generate county-level vegetation indices for all nine vegetation indices available.[24]

B. Summary Statistics

Agricultural data. Summary statistics for farmland values are presented in table 2. Overall, farmland values have increased over the past several decades, with some areas experiencing disproportionally greater appreciations. Over this period, the farmland value cross-section has greatly changed. For instance, the correlation between (log) farmland values for individual years in 1950-2012 relative to the 1987-2012 average has fluctuated between 0.687 in 1950 and 0.972 in 1997.[25]

The spatially heterogenous pattern of farmland appreciation has been coupled with a steady but equally heterogenous fall in total farmland area across the eastern part of the country. In 1950, land in farms across the sample totaled 688 million acres. By 2012, this acreage had dropped to 500 million, a 27.4% decrease. The number of urban counties has more than doubled over 1950-2012 but the number of counties classified as non-urban remains large, exceeding 2,200 in all years. The analysis in this paper will be confined to these non-urban counties, following SHFb.

Climate data. I follow the literature and compute climate normals as the 30-year average of yearly weather. The main results in the paper follow the climate specification in SHFb,

[24]These vegetation indices include: Beginning of measurable photosynthesis in the vegetation canopy (SOST), Level of photosynthetic activity at the beginning of measurable photosynthesis (SOSN), End of measurable photosynthesis in the vegetation canopy (EOST), Level of photosynthetic activity at the end of measurable photosynthesis (EOSN), Time of maximum photosynthesis in the canopy (MAXT), Maximum level of photosynthetic activity in the canopy (MAXN), Length of photosynthetic activity or growing season (DUR), Maximum increase in canopy photosynthetic activity above the baseline (AMP) and Canopy photosynthetic activity across the entire growing season (TIN).

[25]The correlation of farmland value relative to 1987-2012 are 0.687 (for 1950), 0.715 (1954), 0.771 (1959), 0.810 (1969), 0.874 (1974), 0.840 (1978), 0.872 (1982), 0.948 (1987), 0.966 (1992) 0.972 (1997), 0.967 (2002), 0.966 (2007) and 0.934 (2012).

Table 2: Summary Statistics of Farmland Real Estate

Year(s)	Farmland Values (2012 USD)				Observations		
	μ	min	max	σ	*Non-urban*	*Urban*	*All*
1950	1,063	67	129,888	4,516	2,233	229	2,462
1954	1,283	68	297,803	7,071	2,233	229	2,462
1959	1,867	181	337,523	10,098	2,233	229	2,462
1969	2,324	244	772,776	16,351	2,227	225	2,452
1974	2,909	186	558,623	12,990	2,226	225	2,451
1978	3,632	440	224,056	6,412	2,227	226	2,453
1982	3,160	457	246,818	7,754	2,233	227	2,460
1987	2,439	362	321,491	7,817	2,223	226	2,449
1992	2,291	262	92,274	3,725	2,226	222	2,448
1997	2,795	280	346,299	9,419	2,234	227	2,461
2002	3,220	308	126,306	5,479	2,235	228	2,463
2007	3,989	497	147,550	6,254	2,236	227	2,463
2012	4,574	512	792,500	17,327	2,237	230	2,467
1959-1982	3,001	307	316,232	11,720	2,237	231	2,468
1987-2012	3,358	377	362,222	9,555	2,237	231	2,468

Notes: Data are for counties in the eastern sample multi-year averages ignore missing observations. Large changes in maximum values and standard deviations in consecutive sample years results from some highly populated counties being dropped from the Agricultural Census. This also drives changes in the standard deviation.

Table 3: Summary Statistics of Climate Variables

Variable(s)		Month(s)	μ	min	max	σ
Degree-days	8-32ºC	Apr-Sep	2429.9	1108.3	3686.8	520.1
	10-30ºC	Apr-Sep	2058.8	848.8	3171.2	473.7
	>34ºC	Apr-Sep	9.5	0.0	140.2	13.7
	>30ºC	Apr-Sep	64.3	0.2	411.8	56.5
Precipitation (mm)		Apr-Sep	602.1	321.5	1041.0	97.6
		Jan	74.2	9.4	169.3	39.3
		Apr	89.6	20.1	149.2	21.9
		Jul	105.6	38.4	217.6	26.3
		Oct	81.2	31.9	135.7	18.5
Mean temperature (ºC)		Jan	0.0	-16.4	19.1	6.5
		Apr	12.9	2.5	24.2	4.3
		Jul	24.8	16.8	30.7	2.6
		Oct	13.9	4.9	26.0	4.0

Notes: Climate variables are county-level averages over the 1976-2005 period. The data covers 2,470 counties lying east of the 100th meridian west. Mean temperature and precipitation are generated from the monthly gridded PRISM dataset while the degree-days variables were constructed from the daily gridded dataset in Schlenker and Roberts (2009).

Table 4: Correlation of Climate Normals Over Time

Variable(s)		Month(s)	Correlation relative to 1976-2005						
			1916 -1945	1926 -1955	1936 -1965	1946 -1975	1956 -1985	1966 -1995	1976 -2005
Degree-days	8-32ºC	Apr-Sep	-	-	-	-	0.999	1.000	1
	10-30ºC	Apr-Sep	-	-	-	-	0.999	1.000	1
	>34ºC	Apr-Sep	-	-	-	-	0.991	0.997	1
	>30ºC	Apr-Sep	-	-	-	-	0.995	0.999	1
Precipitation (mm)		Apr-Sep	0.944	0.940	0.935	0.950	0.967	0.983	1
		Jan	0.949	0.904	0.936	0.942	0.986	0.993	1
		Apr	0.882	0.865	0.848	0.889	0.931	0.966	1
		Jul	0.838	0.877	0.920	0.930	0.954	0.981	1
		Oct	0.820	0.776	0.781	0.813	0.881	0.942	1
Mean temperature (ºC)		Jan	0.996	0.994	0.996	0.996	0.999	0.999	1
		Apr	0.997	0.997	0.998	0.998	0.999	1.000	1
		Jul	0.988	0.991	0.994	0.996	0.995	0.999	1
		Oct	0.997	0.997	0.996	0.997	0.998	1.000	1

Notes: Climate variables are county-level averages over the corresponding 30-year period. The data covers 2,470 counties lying east of the 100th meridian west. Mean temperature and precipitation are generated from the monthly gridded PRISM dataset while the degree-days variables were constructed from the daily gridded dataset in Schlenker and Roberts (2009).

which includes linear and quadratic terms for degree-days between 8 and 32ºC and precipitation, the square root of degree-days exceeding 34ºC.[26] These variables are aggregated over the April-September period to encompass the typical growing season. I also report results for specifications based on monthly climate variables following MNS as well as linear degree-days variables.[27]

Summary statistics for all climate variables (1976-2005) considered in the paper are presented in table 3. There is considerable climatic variation across the sample with a cross-sectional range of approximately 35ºC and 14ºC for mean temperatures for January and July, respectively. Large variations are also observed for degree-days variables. Precipitation also varies considerably across all months, although the variation is naturally smaller when precipitation totals for a longer time period such as April-September is considered.

Because I estimate hedonic models over a long period of time, it seems natural to verify

[26]Degree-days are computed using the double-sine method with a horizontal cutoff. The computations account for the time-path of temperature throughout the day.

[27]The specification in MNS includes monthly climate normals for mean temperature and precipitation for the months of January, April, July and October. The linear degree-days specification includes linear terms for degree-days between 10 and 30ºC, and degree-days above 30ºC.

how climate cross-sections have evolved. Table 4 shows the correlation of climate normals over time. Because data from Schlenker and Roberts (2009) are only available since 1950, I cannot assess the correlations of degree-days variables for earlier time periods. However, the correlations in excess of 0.95 (for most variables, and especially for temperature) for the 30-year periods ranging from 1916-1945 through 1946-1975, indicate the climate cross-section has remained fairly stable.

It would be interesting to explore regional trends in climate and the resulting agricultural responses over such a long time period. However, the nature of the underlying PRISM data so far precludes from this type of analysis, making the detection of climate *trends* vulnerable to artifacts or even endogeneity.[28] I therefore rely on climate normals for 1976-2005 for all regressions.[29] It is worth noting that the farmland value cross-section appears to have evolved considerably more than the climate cross-section as indicated by the lower and steadily decreasing correlations over time in the farmland value variable.

Climate change impacts are reported for various warming scenarios as projected by the Hadley GEM2-ES General Circulation Model (GCM) or HadGEM2-ES (Jones et al., 2011).[30] Starting in its fifth Assessment Report (AR5) in 2014, the Intergovernmental Panel on Climate Change (IPCC) adopted warming scenarios that correspond to Representative Concentration Pathways (RCP). Instead of emissions, these scenarios represent trajectories of greenhouse gas concentration. The scenarios are named based on the radiative forcing values in year 2100 relative to pre-industrial levels. The four scenarios are RCP2.6, RCP4.5,

[28]The PRISM group discourages the use of their data for climate trend detection. More specifically, the PRISM documentation states that the long-term average datasets *"are not currently suitable for calculating multi-decadal climate trends. Although longer-term networks are used, grids still contain non-climatic variations due to station equipment and location changes, stations openings and closings, and varying observation times."* (See p.5 in http://www.prism.oregonstate.edu/documents/PRISM_datasets.pdf, accessed 7/12/2016). In other words, although highly detailed, we ignore if statistically significant differences in PRISM trends are driven by changes in climate or non-climatic factors. Moreover, the estimation of causal effects of recent climate change is made more difficult by the recent findings in Mueller et al. (2016) which indicate that the large-scale agricultural intensification of the US Midwest altered the local climate thus making recent climate *trends* (surprisingly) endogenous to agricultural productivity.

[29]SHFb find no differences in results when the climate normals are computed based on the preceding 30-year weather average for each census year. This is not surprising given that climate correlations across time are very high.

[30]Results based on other four GCM and a uniform warming scenario considered in MNS and SHFa are also available in the online appendix. These are not presented due to space constraints but results are similar across GCMs. The four additional CGMs are: the second generation Canadian Earth System Model (CanESM2), the Community Climate System Model (CCSM4), the Geophysical Fluid Dynamics Laboratory Earth System Model (GFDL-ESM2M), and the The Norwegian Earth System Model (NorESM1-M).

RCP6, and RCP8.5, corresponding to additional "trapped" atmospheric energy of 2.6, 4.5, 6.0, and 8.5 W/m^2, respectively. To put this in context, the RCP2.6 and RCP8.5 scenarios are likely to lead to *global* mean temperature increases of 1 and 2$^{\text{o}}$C by 2046-2065, respectively. Note that *regional* temperature changes can be much greater. I follow the approach outlined in Auffhammer et al. (2013) to generate county-level projections for mid-century (2036-2065) and end-of-century (2070-2099).[31]

Other data. Summary statistics for control variables are presented in table 5. The control variables in this study follow SHFb. Some controls overlap with those in MNS, but updated variables have greater explanatory power. As expected, income per capita and population density vary considerably across the sample, although the variation is substantially reduced when only non-urban counties are considered. For instance, the maximum income per capita drops from 119,000 to 83,600 USD. However, the mean income per capita remains fairly stable at around 37,000 USD. On the other hand, the mean population density drops significantly from over 250 to just under 80 inhabitants per square mile. In contrast, the distribution of soil quality controls does not seem to vary much when urban counties are excluded, indicating that the sample restriction is mainly removing the influence of highly populated and high income areas. The interested reader can find maps of all key climate and control variables in the online appendix.

County neighborhoods. This study relies on county "neighborhoods" to create controls that capture regional confounding effects with climate. Table 5 summarizes the distribution of both the number of neighbors and the geographical distance to them for alternative groupings. Neighborhood composition remains comparable when the sample is restricted to non-urban counties. The "first order" neighborhood is the most restrictive definition and only includes counties that are adjacent to the county of interest (first-order neighbors). This definition yields a median number of 6 neighbors in the sample. This is lower than the

[31] First, I compute changes in monthly climate normals for each variable for mid-century (2036-2065) and end-of-century (2070-2099) periods relative to a historical reference period (1976-2005). Second, I downscale the relatively coarse projections on the GCM grid to the PRISM grid based on inverse distance weights between the four nearest GCM grid centroids to each PRISM grid. Third, I add the downscaled projections to the fine-scale climatologies of PRISM or Schlenker and Roberts (2009). This preserves the smoothness of climate variation in the projections. Fourth, I aggregate these projections to the county-level using cropland weights based on the 2008-2014 CDL as previously mentioned.

Table 5: Summary Statistics of Control Variables

Variables	All counties ($n = 2,469$)				Non-urban Counties ($n = 2,236$)			
	μ	min	max	σ	μ	min	max	σ
Income per capita (thousand 2012US$)*	37.13	17.92	119.35	8.92	36.17	17.92	83.63	8.08
Population density (pop/mi^2)*	260.4	0.3	70,922.5	1,895.9	77.3	0.3	398.6	81.7
Average water capacity (fraction)	0.146	0.070	0.275	0.027	0.147	0.070	0.225	0.027
Clay content (%)	27.1	3.1	61.6	9.4	27.5	3.1	58.3	9.0
Minimum permeability (µm/sec)	7.78	0.14	98.10	8.07	7.38	0.14	98.10	7.30
K-factor of topsoil (index)	0.30	0.10	0.48	0.07	0.30	0.10	0.48	0.07
Best soil class (fraction)	0.65	0.00	1.00	0.23	0.65	0.00	1.00	0.22
Latitude (^{o})	37.8	25.5	48.8	4.7	37.7	25.5	48.8	4.6

Notes: Variable descriptions: Average water capacity, measured in cm of water per cm of soil, is the volume of water available to plants when excess water in the soil has been drained. Clay content is an aspect of soil texture, which identifies the percent of soil composed of particles that have a diameter of less than 0.002 mm. Soil permeability or saturated hydraulic conductivity describes soil permeability by water and is measured in µm/s. The K-factor or soil erodibility is an index ranging from 0.02-0.69 that predicts vulnerability to soil erosion as a function of soil texture, organic matter content, and various aspects of soil structure. The best soil class variable refers to the proportion of soils within a county classified in the top three (I-III) of an eight-class index based on capability to produce commonly cultivated crops and pasture plants. (Sources: National Soil Survey Handbook, part 618 and 622.02).
*For clarity of exposition, income per capita and population density correspond to 2012 levels.

median number of counties by agricultural district in the sample (9 to 10).[32] The "second-order" neighborhood expands the previous set with counties that are adjacent to first-order neighbors, that is, with second-order neighbors.

Table 6: Number of neighbors and Distance to Neighbors under Alternative County Neighborhood Definitions

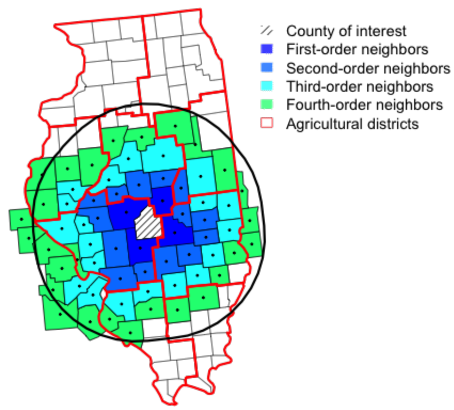
	Number of county neighbors						Distance from neighbors (miles)					
	min	Q_1	Q_2	μ	Q_3	max	min	Q_1	Q_2	μ	Q_3	max
All counties east of 100 meridian west:												
1rst-order	1	5	6	5.8	7	10	4.6	13.9	16.7	17.4	20.1	75.5
2nd-order	2	16	18	18	20	33	4.6	19.2	26.9	27.4	34.1	103.0
3rd-order	2	33	38	36.5	41	60	4.6	26.8	37.5	37.9	47.9	132.4
4th-order	2	55	65	61.2	70	91	4.6	34.1	48.4	48.5	61.9	165.4
Districts	1	7	10	10.3	12	33	-	-	-	-	-	-
States	2	27	67	66.4	92	171	-	-	-	-	-	-
Non-urban counties east of the 100th meridian west:												
1rst-order	1	5	6	5.6	6	10	4.6	14	16.8	17.5	20.3	93.9
2nd-order	1	15	17	16.7	19	32	4.6	19.2	27.0	27.6	34.2	117.6
3rd-order	1	29	35	33.4	39	56	4.6	26.7	37.4	38.0	47.9	151.8
4th-order	1	48	59	55.6	66	88	4.6	33.8	48.1	48.4	61.8	165.4
Districts	1	6	9	9.5	11	33	-	-	-	-	-	-
States	1	27	65	60.3	83	155	-	-	-	-	-	-

Notes: $Q1$, $Q2$ and $Q3$ correspond to first, second (median) and third quartiles. The "first-order county neighborhood" is comprised of all counties that are directly contiguous, i.e. share borders, with the county of interest (first-order neighbors). Only 2 counties have no contiguous neighbors and correspond to island counties (Richmond, NY, and Nantucket, MA). For these, I create a neighbor set that includes the 6 closest counties (which is the median number of contiguous neighbors). The "second-order (third-order) neighborhood" expands the first-order neighborhood with the set of counties contiguous to first-order (second-order) neighbors to the county of interest. The fourth neighborhood definition corresponds to the set of counties whose centroids fall within 75 miles of the centroid of the county of interest. In this table, "non-urban" counties have population densities below 400 inhabitants per square mile in 2012.

Higher-order neighborhoods are defined analogously. This naturally increases the range of counties included in the neighborhood and pushes up the average distance to a neighbor. The median number of neighbors for the second, third and fourth-order neighborhoods is around 17-18, 35-38 and 59-65 counties, respectively. These are larger groupings than districts, and the latter is comparable to the median number of counties by state in the sample (65-67).

[32] The USDA groups counties within states into Agricultural Statistics Districts (ASD) based on their similarities regarding geography, climate and cropping practices. I refer to these as agricultural districts in this paper and I use the latest definition of these districts as of 2016.

Note that county neighborhoods are defined relative to *each* county, so neighborhoods of adjacent counties actually overlap. This is *not* the case for administrative groupings such as districts or states which are non-overlapping. This plays a key role in controlling for "smooth" spatially-dependent omitted variables that do not follow administrative boundaries.



Notes: The map illustrates neighbor relationships which are typically overlapping. The first-order neighborhood includes only first-order neighbors. The second-order neighborhood includes first and second-order neighbors, and so on for higher order neighborhoods. The black circle has a radius of 100 miles around the county of interest to provide a scale. The county of interest is Christian county, IL.

Figure 1: Illustration of Various Neighborhood Sets For the State of Illinois.

Figure 1 illustrates how these neighborhoods compare to state and district size for the state of Illinois. The identifying assumption in this paper favors more restrictive neighbor definitions as these reduce the chances of incidental confounding of climate. However, these might increase vulnerability to measurement error. I explore this and other concerns in the following section.

4 Results

A. Evidence in Support of the Identifying Assumptions

The identification strategy in this paper relies on two assumptions. First, that a county's agricultural productivity is primarily affected by its *own* climate, and second, that climate assignment to a county is random *conditional* on neighborhood-average characteristics. The first assumption appears innocuous but the second assumption should be tested. I provide three types of evidence in support of the second assumption and to address concerns about identification.

First, I verify (against observables) whether climate appears randomly assigned under varying conditioning factors. To this end, I estimate a series of linear models of control variables on individual climate variables. If climate assignment is random then climate variables should provide no statistically significant explanatory power on the level of these observables. Observables include control variables typically used in this literature, including economic control variables such as income per capita and population density, as well as soil quality indicators.[33]

Table 7 shows the t-statistics for climate coefficients for these auxiliary regressions. Climate variables are unconditionally correlated with control variables at conventional levels ($p \leq 0.05$) in 18 out of the 21 associations examined (column 1). Columns 2 and 3 indicate the number of statistically significant associations conditional on state and district fixed effects is 13 and 12, respectively. This is important, because it confirms that dummy variables, even for very small groupings such as districts, cannot fully absorb the spatial covariance among climate and control variables.

On the other hand, *none* of the 21 associations examined are significant at conventional levels conditional on the first-order neighborhood average of the observable characteristic (column 4). This number slowly rises for less restrictive neighborhood definitions reaching 2, 4 and 5 significant associations when conditioning on second, third and fourth-order neighborhood-average characteristics (columns 5-7). Apparently, relying on less restrictive neighborhoods increases the chances of incidental correlations of climate with spatially-dependent observables. In other words, climate appears randomly assigned conditional on first-order neighborhood average characteristics, but not when considering less restrictive neighborhoods. This obviously favors the use of first-order neighborhoods for estimation.

However, this finding could potentially reflect an artifact. For instance, climate variables may appear uncorrelated with observables conditional on first-order neighborhood charac-

[33]These are control variables used in SHFb. I exclude latitude of a county's centroid for reasons that will soon become apparent.

Table 7: t-STATISTICS FROM PAIRWISE REGRESSIONS OF CONTROL VARIABLES ON CLIMATE VARIABLES

		Fixed Effects		Neighborhood Order			
	Pooled	State	District	1rst	2nd	3rd	4th
Degree-days 8-32ºC	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Income per capita (2012)	-13.59***	5.34***	7.27***	-0.56	0.95	1.85	2.77**
Population density (2012)	-1.12	6.36***	9.50***	0.66	0.24	0.25	0.31
Average water capacity	-3.16**	3.00**	3.31***	1.61	2.12*	2.34*	2.30*
Clay content	16.86***	-1.89	5.84***	-0.74	-0.95	-0.99	-1.06
Minimum permeability	-3.61***	1.90	-2.12*	-0.91	-0.87	-0.77	-0.44
K-factor of topsoil	-1.92	2.59**	3.65***	1.43	1.58	1.47	1.36
Best soil class	-2.27*	12.89***	9.51***	0.72	1.48	2.02*	2.59**
Degree-days >34ºC	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Income per capita (2012)	-3.22**	-0.96	-2.38*	-1.01	-1.66	-1.80	-1.56
Population density (2012)	-3.54***	-1.03	1.10	-0.17	-1.35	-1.57	-1.65
Average water capacity	1.02	-3.31***	1.57	0.64	0.38	-0.09	-0.61
Clay content	14.19***	-6.29***	0.46	-0.83	-1.78	-2.26*	-2.52*
Minimum permeability	-7.08***	2.16*	-1.65	-0.39	0.05	0.53	0.87
K-factor of topsoil	2.15*	-4.85***	1.44	1.03	0.61	-0.20	-0.95
Best soil class	2.73**	5.18***	5.38***	0.45	0.80	0.93	1.05
Precipitation	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Income per capita (2012)	-14.56***	-1.59	-0.75	-0.71	0.95	1.39	1.45
Population density (2012)	2.34*	1.97*	-0.16	0.11	1.03	1.21	1.27
Average water capacity	-8.74***	1.69	-1.57	0.14	0.58	1.12	1.44
Clay content	-5.19***	-3.38***	-6.82***	-1.81	-2.51***	-3.19***	-3.67***
Minimum permeability	7.88***	0.33	0.92	-0.08	0.08	0.04	0.37
K-factor of topsoil	-6.50***	1.09	-3.18**	-1.08	-1.35	-1.20	-0.80
Best soil class	-11.61***	-3.90***	-6.32***	-0.92	-1.48	-1.54	-1.41

Notes: The table presents the t-statistics for pairwise regression coefficients of climate variables (shown by panel) on observable determinants of farmland values (shown by rows). Symbols *,** and *** indicate statistical significance at the 5, 1 and .1 percent level, respectively. Standard errors are corrected for spatial correlation following Conley (1999). Results based on farmland-weighted regressions are comparable. The sample ($n = 2,457$) includes all counties east of the 100th meridian west. Column (1) indicates the coefficient for a pooled unconditional regression based on sample-wide variation. Columns (2) and (3) correspond to state- and district-fixed-effect specifications, respectively. Columns (4) to (7) indicate coefficients conditional on neighborhood-average values of the observable based on different neighborhood definitions (see section 3 for more details).

Table 8: *t*-STATISTICS FROM PAIRWISE REGRESSIONS OF SELECT INDICATORS ON NORMAL DEGREE-DAYS

		Fixed Effects		Neighborhood Order			
	Pooled	State	District	1rst	2nd	3rd	4th
Degree-days 8-32ºC	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Geographical indicators:							
Latitude	-151.71***	-50.91***	-25.91***	-3.22**	-5.94***	-8.95***	-11.89***
Longitude	-11.67***	7.00***	-1.02	0.84	1.19	1.58	1.76
Altitude	-31.78***	-52.44***	-50.03***	0.79	0.44	-0.04	-0.17
Satellite indicators (2001-2014):							
AMP	-59.55***	-12.95***	-10.22***	2.42*	3.91***	3.79***	3.47***
DUR	-17.27***	-5.70***	0.10	2.46*	3.35***	3.47***	3.39***
EOSN	34.25***	3.24**	-1.86	-3.47***	-5.01***	-5.54***	-5.93***
EOST	-25.46***	1.36	-3.68***	1.93	1.37	1.37	1.93
MAXN	-23.55***	-11.50***	-14.45***	-0.37	0.15	0.57	0.90
MAXT	-6.13***	18.11***	3.99***	2.04*	2.27*	3.02**	4.04***
SOSN	33.63***	4.03***	-1.71	-3.36***	-4.87***	-5.41***	-5.81***
SOST	0.15	10.95***	-1.59	0.37	-0.04	0.51	1.23
TIN	-98.82***	-28.58***	-17.03***	1.99*	2.56*	2.23*	1.73
Agricultural indicators (1976-2005):							
Corn yield	-26.79***	8.18***	3.54***	1.81	5.04***	6.28***	5.82***
Soybean yield	-29.16***	5.26***	5.30***	2.85**	4.36***	4.36***	3.72***

Notes: The table presents results presented in table 7 but for different observables and for a single climate variable. Results for the other 2 climate variables are presented in the online appendix. Symbols *,** and *** indicate statistical significance at the 5, 1 and .1 percent level, respectively. Standard errors are corrected for spatial correlation following Conley (1999). More details and definitions of regarding the satellite indicators are provided in section 3.

teristics because there is insufficient variation for identification of climate effects.[34] To verify this possibility, I conduct a similar analysis but on a different set of observables for which one *should expect* a at least some degree of correlation with climate. Good candidates for such analysis are variables that are known to *affect* climate or to be *affected* by climate. Examples I explore in table 8 include geographic, vegetation indices captured remotely from satellites and agricultural indicators which I regress on normal degree-days (8-32ºC).[35]

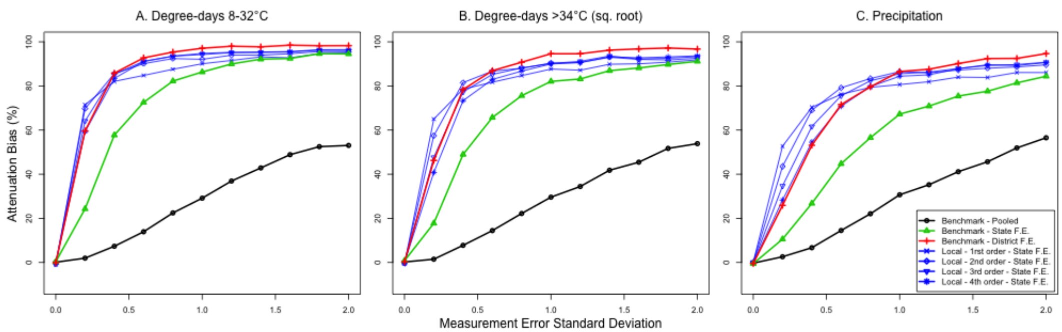
In contrast to previous results, climate variables are correlated with *most* of these indicators conditional on first-order neighborhood average characteristics. Out of the 14 associations, 8 are significant at conventional levels when the first-order neighborhood is considered (table 8, column 4). This number reaches 9 for second and third-order neighborhoods, and finally 8 for fourth-order neighborhoods. Similarly, 13, 13 and 9 of these associations are significant at conventional levels for unconditional associations and associations conditional on state and district fixed-effects, respectively. This result indicates that —conditional on first-order neighborhood characteristics— climate variables are *uncorrelated* with seemingly unrelated observables, but remain *correlated* with related ones.

These findings seem encouraging but cannot fully rule out the possibility that the relationships in table 8 (column 4) may be nonetheless attenuated due to errors-in-variables issues. It is difficult to directly disprove this. I therefore devise a strategy to indirectly detect attenuation bias in the proposed model estimates. The idea consists in contrasting estimates of the proposed model with those of another model with comparable vulnerability to measurement error *but* with a different vulnerability to time-invariant spatially-dependent confounders.

To this end, I developed a simulation to assess the degree of attenuation bias for com-

[34]In other words, the neighborhood-average regressor may be "wiping out" too much variation, leaving little residual variation for climate variables to explain. An important concern in this literature is the amount of climate variation used in the estimation of climate effects. For instance, a point of contention in FHRS (2012) and DG (2012) revolves around the extent to which *state-by-year* fixed effects "wipe out" meaningful weather variation in the panel model for the consistent estimation of weather effects.

[35]Geographical indicators (latitude, longitude and altitude) are expected to correlate with climate because geographical coordinates and orography are major drivers of climate. In addition, vegetation type and intensity should relate to climate. I therefore rely on 9 vegetation indices that capture the timing and/or intensity of "greenness" of ground vegetation as measured from satellites (see section 3). The same reasoning applies to agricultural production as measured by crop yields. Due to space constraints, I show the results for the other two climate variables, extreme degree-days (>34ºC) and precipitation, in the online appendix.



Notes: Each panel provides simulation results based on the three main climate variables used in the paper. Within each panel, the curves represent the level of attenuation bias (0% is "no attenuation", 100% is "full attenuation") to varying levels of measurement error variance for competing models. The data generating process (DGP) of the simulation is $y_c = \beta x_c + \epsilon_c$ with $\beta = 1$, x_c is a real climate variable (degree-days 8-32C, square root of degree-days >34C or precipitation) and ϵ_c is an i.i.d normally distributed error term with variance 1. Climate variables are normalized so that $\sigma_x^2 = 1$. The econometrician does not observe the "true" climate variable x_c, but x_c^* which is measured as $x_c^* = x_c + m_c$ where m_c is a i.i.d normally-distributed measurement error. The exercise explores how mean values of $\hat{\beta}$ resulting from 1,000 draws of ϵ_c change with the variance of the measurement error (σ_m^2 varies from 0 to 4). The neighborhood-average climate regressor is computed using equal weights of its neighbors (see section 3 for details about neighborhood definitions).

Figure 2: Attenuation Bias of Competing Models Classical Measurement Error of Climate Variables

peting models when facing varying levels of measurement error in climate variables. Results depicted in figure 2 show, unsurprisingly, that benchmark pooled models are less vulnerable to attenuation bias than state- or district-fixed-effect models.[36] This is true for all three climate variables analyzed.[37]

Also, and as expected, the preferred model is more vulnerable to attenuation than the pooled and state fixed-effect benchmark models. However, the degree of attenuation remains *comparable* to that of the district fixed-effect model, especially for temperature variables (degree-days) which play a key role in driving climate impacts in this literature.[38] In the online appendix I develop a complementary simulation where I assess the performance of

[36]This is the classic result from Griliches and Hausman (1986) that classical measurement error is "amplified" in a *within* model.

[37]Interestingly, attenuation bias rises more rapidly with measurement error for degree-days variables than for precipitation. This is due to the higher degree of spatial autocorrelation for temperature variables (degree-days) relative to precipitation.

[38]Also, the degree of attenuation remains very similar across first, second, third and fourth order neighborhood direct models. This is particularly true for temperature variables (degree-days) with a higher degree of spatial autocorrelation.

competing models in the presence of varying forms of omitted variables.[39] The key finding is that the proposed direct model is *superior* to the district fixed-effect model in the presence of a smooth spatially-dependent regional confounder.

In other words, the proposed model and the benchmark district-fixed-effect model behave *similarly* in the presence of classical measurement error, but perform *differently* when facing a spatially-dependent omitted variable that is regionally confounded with climate. We thus have a clear way to distinguish attenuation from omitted-variable bias in the empirical results.

B. Benchmark Hedonic Estimates of the Impact of Climate Change

Replication of Benchmark Hedonic Estimates— Here I present climate change impact projections on farmland values based on benchmark hedonic models relying on pooled, within-state and within-district climate variation. I follow the specification proposed in SHFb and Fisher et al. (2012, *appendix*), which is a semi-log model of farmland values with degree-days climate variables and various controls.[40] I follow this specification because SHFb is arguably the most comprehensive hedonic study on US agriculture. Following SHFb, I restrict the sample to counties lying east of the 100th meridian west to avoid confounding a warmer climate with irrigation. Climate change impacts on farmland values are depicted for all cross-sections in figure 3 for the HadGEM2-ES climate model.[41]

Three major points stand out. First, damage estimates for *recent* cross-sections (1987-2012) are large and fairly stable across the *three* benchmark specifications.[42] I find impacts of -76.5, -74.5 and -68.1% toward the end of the century for the most severe warming scenario (RCP 8.5) under the pooled, state and district-fixed-effect models, respectively. Previous

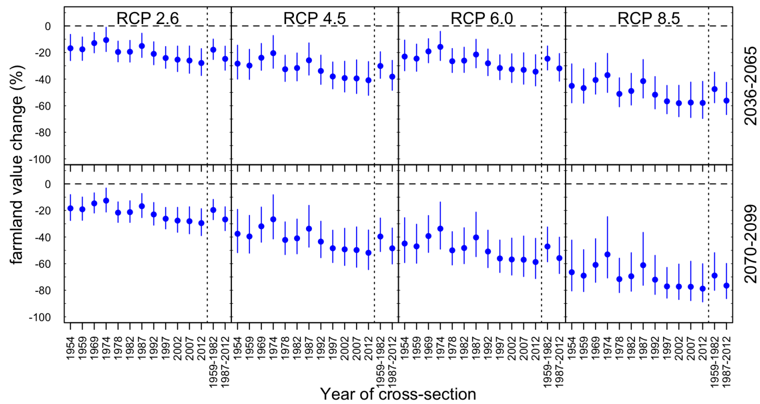
[39] Including linear, state-level and spatially-dependent regional climate confounders.

[40] The specification includes: degree-days 8-32ºC (linear and quadratic terms), squared root of degree-days >34ºC, precipitation (linear and quadratic terms), income per capita, population density (linear and quadratic terms), average water capacity, clay content, minimum permeability, K-factor of topsoil and best soil class. All climate variables correspond to normal April-September climate for 1976-2005 (see section 3). I exclude latitude as a control because it is a well known predictor of climate. Results remain stable with this omission.

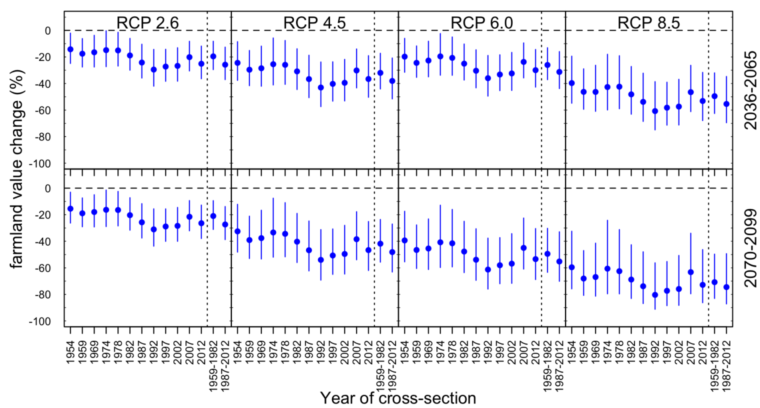
[41] Following suggestions in Burke et al. (2014), I report results based on four other climate models and a uniform climate change scenario in the online appendix. However, results remain qualitatively very similar to those report here.

[42] In addition, climate change impact estimates remain fairly stable when control variables are omitted (see online appendix) which at first glance seems reassuring.

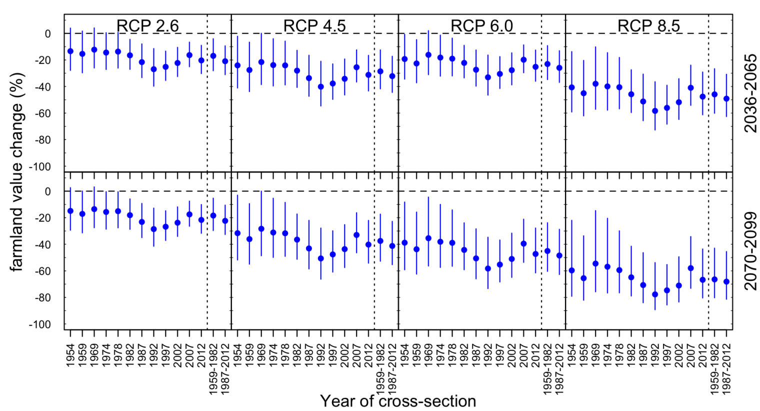
A. Pooled



B. State Fixed Effects



C. District Fixed Effects



Notes: Projected farmland value percent changes correspond to the farmland-weighted sample average projection. The top (bottom) row of each panel corresponds to the predicted farmland value change for 2036-2065 (2070-2099) relative to the 1976-2005 reference period. RCP scenarios increase in severity from left to right as described in section 3. The 95 percent confidence intervals for the predicted mean change are corrected for spatial correlation (Conley, 1999).

Figure 3: Climate Change Impacts Based on Benchmark Hedonic Models

studies have not reported within-district estimates. This similarity is important because it indicates these estimates are *not* attenuated.[43] Recall that measurement error in climate variables leads to a rapid attenuation for within models, especially for the district-fixed-effect specification (see figure 2). However, such a pattern is not apparent in these empirical estimates.

Second, damage estimates for *recent* cross-sections (1987-2012) are very similar to those in the literature. For instance, for the state fixed-effects model, SHFb finds impacts of -27.4, -31.6, -61.6 and -68.5% for the B1, B2, A2 and A1F1 climate change scenarios for the end of the century. In this replication I find impacts of -27.3, -48.1, -55.2% and -74.5% for RCP 2.6, 4.5, 6.0 and 8.5 climate change scenarios (figure 3 panel B). Although these scenarios are not equivalent, they show a high agreement between low and high warming scenarios.[44] To provide some context, the most severe estimate of -74.5% change in farmland value is equivalent to a 38.9 billion annual loss in profits.[45]

Finally, and perhaps more intriguingly, climate change impact estimates appear unstable *over time* and tend to drift toward smaller estimates for older cross-sections. In fact, some of the impact estimates for older cross-sections for the district-fixed-effect model (figure 3 panel C) are no longer significant at conventional levels for the RCP2.6 warming scenario. Because earlier implementations of benchmark hedonic models have not incorporated older Agricultural Census data, this pattern had gone relatively unnoticed.[46]

To verify whether this pattern is found across subsets of the data, I estimate the model for

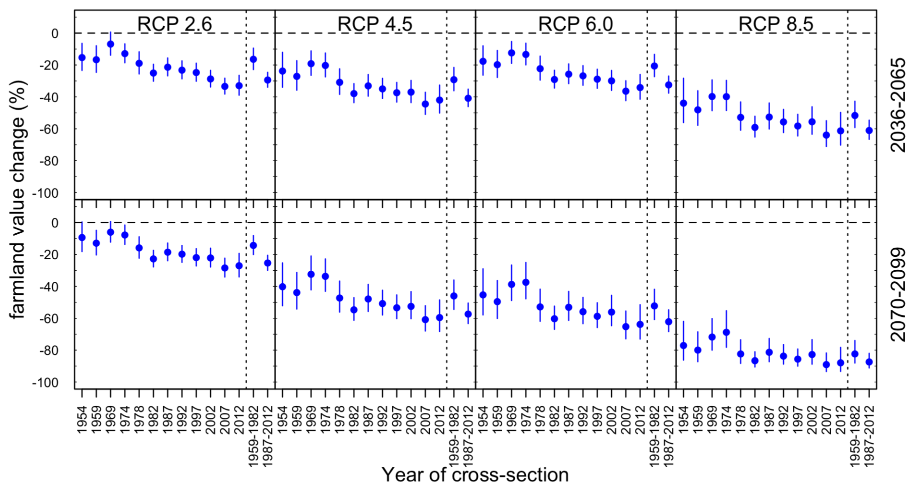
[43]The district-fixed-effect estimates are only about 10% lower than the pooled estimates.

[44]When I include latitude as a control variable, as in SHFb, results appear slightly more detrimental for the state fixed effect specification: -34.5, -55.6, -62.0 and -79.4% for RCP 2.6, 4.5, 6.0 and 8.5 climate change scenarios, respectively. The results are perhaps even closer to SHFb when, in addition to including latitude, the estimation is based on the GMM estimator used in SHFb: -24.6, -43.0 and -49.3 and -68.2 for RCP 2.6, 4.5, 6.0 and 8.5 climate change scenarios, respectively. Results based on the GMM model without latitude as a control variable are shown in the online appendix.

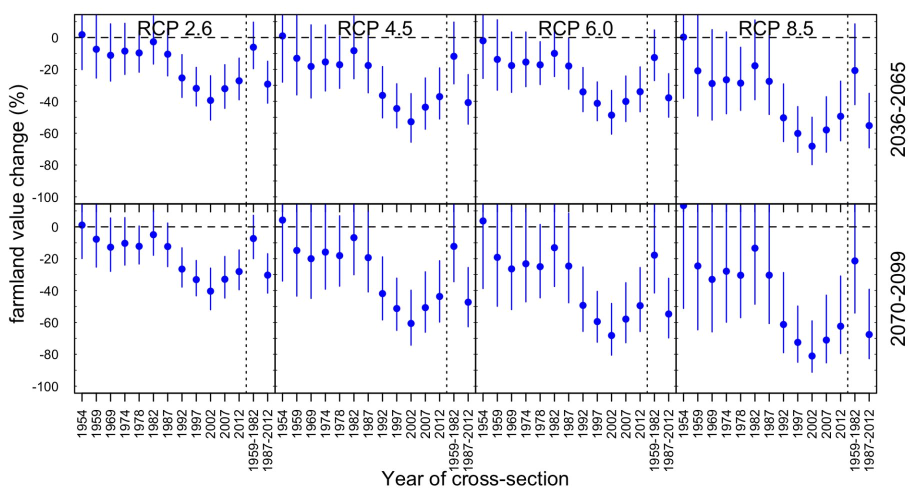
[45]This estimate has as a 95% confidence interval of -87.2 to -49.3%, which is fairly wide but remains negative. The total value of farmland for the sample is approximately $1 trillion USD (2012$). This is calculated by multiplying the average farmland value for 1987-2012 by the average number of farmland acres in the same period. Assuming a 5% capitalization rate this is equivalent to a yearly profit of $52.258 billion (G$).

[46]MNS considers 2 cross-sections (1978 and 1982), SHFa considers 5 cross-sections (1982, 1987, 1992, 1997, and 2002) and SHFb reports results for 4 cross-sections (1982, 1987, 1992 and 1997). However, Fisher et al. (2012b) reports (in the online appendix) estimates based on 6 cross-sections (1969, 1974, 1978, 1982, 1997 and 2002). The same attenuation pattern reported here is also noticeable in that study. For instance, the impact estimates for the state fixed effects model with farmland weights are roughly *half* for the 1969 relative to the 2002 cross-section. Note this study reports results based on 13 cross-sections (spanning 1950 through 2012).

A. North



B. South



Notes: All counties are non-urban and fall east of the 100th meridian west. The "North" corresponds to counties falling in states roughly north of the 37th parallel north (ND, SD, NE, KS, MN, IA,MO,WI, IL,MI, IN, KY, OH, WV, VA, PA, NY, VT, ME, NH, MA, RI, CT, NJ DE, MD). The "South" comprises counties within states falling roughly south of the 37th parallel north (TX, OK, AR, LA, TN, MS, AL,NC, SC, GA, FL). For the 1987-2012 cross-section, the northern (southern) sample has 1,332 (942) observations.

Figure 4: Climate Change Impacts Based on Pooled Benchmark Hedonic Models For Two Subsamples

northern and southern subsamples. Figure 4 shows the corresponding climate change impact estimates for a pooled model.[47] A similar attenuation pattern prevails in the northern subsample but an even stronger attenuation operates in the southern subsample. In fact, climate change impact estimates are mostly insignificant for the southern subsample for the 1954-1987 period. The regional and temporal instability alone of these impact estimates raise concerns regarding the robustness of benchmark hedonic models.

Elsewhere (Ortiz-Bobea, 2016), I find that the likely driver of this instability is exurban and rural development pressure on farmland which operates as a growing and slowly-varying omitted variable affecting benchmark models. More specifically, I find that benchmark estimates attenuate and become insignificant when counties under high development pressure are dropped. Moreover, I find that benchmark results are not robust to an alternative dependent variable that, in principle, does not embody the option value of farmland (cropland rent) and is therefore arguably free from the underlying influences of development pressure.

To some extent, the finding is not that surprising. Roback (1982) and Albouy et al. (2016) have shown that climate plays an important role in the quality of life of US households. The resulting regional sorting of the US population into desirable climates can therefore lead to development pressures that are regionally correlated with climate.[48]

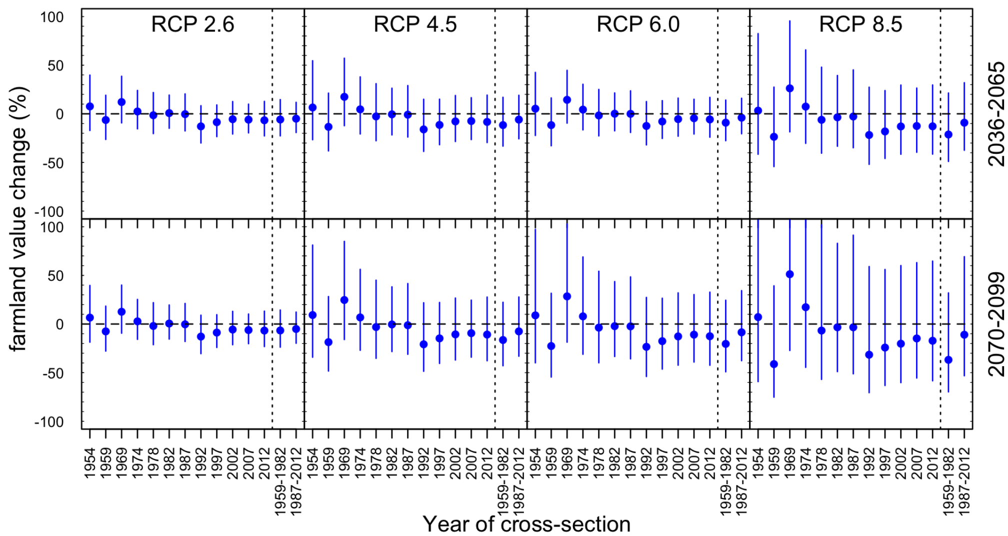
C. New Hedonic Estimates of the Impact of Climate Change

New Hedonic Estimates— I now turn to the preferred approach that estimates the effect of climate conditional on neighborhood-average characteristics. Recall the proposed model simply augments the benchmark hedonic model with neighborhood-average values of each regressor.[49] This additional set of regressors is designed to control for spatially-dependent climate confounders. Results are summarized in figure 5 for all cross-sections for a state-

[47] Results are comparable for other benchmark specifications.

[48] I conduct a similar analysis to that in table 7 to verify whether increases in housing prices (1950-1980 1990-2010), which arguably reflect more desirable living locations, can be explained by climate *conditional* on neighborhood-average housing value change. I find *no* evidence this is the case. This seems to indicate that the US population could be sorting *regionally* into desirable climates. In other words, regional differences in climate may help explain why people move across states, but the within-regions climate variation does not appear to explain in which counties people prefer to locate.

[49] The main results presented in the paper are based on a first-order neighborhood with equal weights. I explore varying neighborhood definitions and weighting schemes later on.



Notes: The model is based on a first-order neighborhood with equal weights. Climate change projections correspond to the HadGEM2-ES climate model.

Figure 5: Climate Change Impacts Based on Direct Climate Variation With State Fixed Effects

fixed-effect specification.

Notice that *none* of the climate change impact estimates on farmland values is distinguishable from zero. This is true for *all* cross-sections, *all* scenarios and time horizons. The result is surprisingly stable over time. There is no "drifting" of estimates over cross-sections as for benchmark estimates. In the online appendix I also show this is true for all other climate models considered.

Notice, however, that confidence intervals are relatively wide for the most extreme scenario, especially for end-of-century estimates. This is the likely consequence of collinearity between climate and the neighborhood-average regressors. This would not rule out potentially sizable effects of climate change on the sector under extreme scenarios. However, in the online appendix I show these new estimates based on a more efficient spatial GMM estimator (Kelejian and Prucha, 1999) and I obtain much tighter confidence intervals. The associated point estimates appear slightly negative but remain overwhelmingly insignificant.[50]

[50] Only 2 out of 12 estimates are significant at conventional levels for the most extreme scenario (RCP 8.5). The remaining 36 estimates for other climate scenarios (RCP 2.6, 4.5 and 6.0) cannot be distinguished from zero.

The main threat to validity for these new estimates is that climate effects may be attenuated due to an amplification of measurement error resulting from the identification strategy. Previously, I showed that the preferred model is similarly vulnerable to measurement error than the benchmark model with district fixed effects. As a result, the presence of measurement error in climate should have generated attenuated estimates for *both* models, not *just* the proposed one. As it is clear from figures 3 (panel C) and 5, these estimates differ. The preferred estimates are close to zero and insignificant while the benchmark district-fixed effects estimates are large, negative and significant, in line with other benchmark specifications.[51]

These new results provide a stark departure from benchmark estimates. The findings here point to *no* robust evidence for or against large benefits or damages from climate change on non-irrigated US agriculture. This result is robust across all cross-sections and across state-fixed-effect and pooled specifications (see online appendix). I now consider multiple checks to verify the robustness of these new findings.

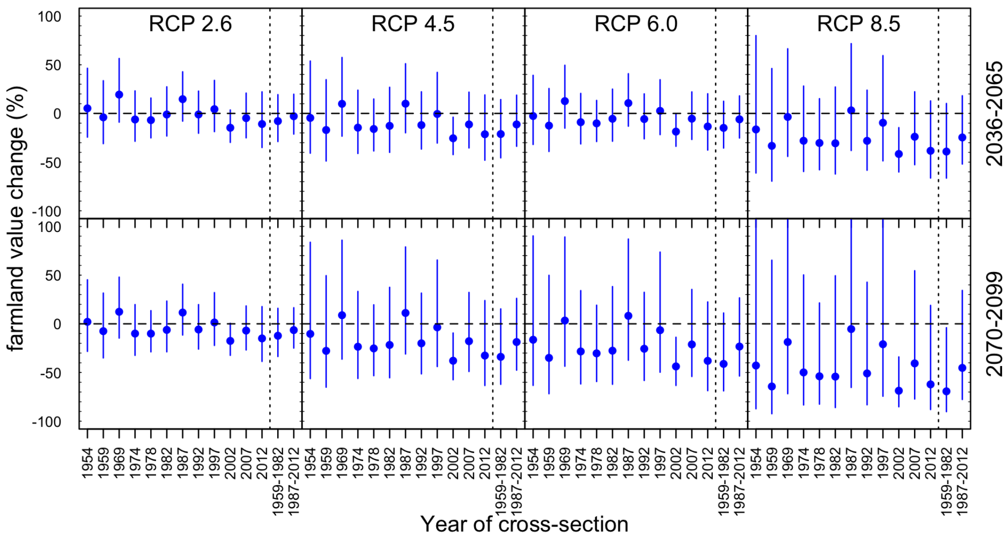
Are Empirical Results Robust to Subsamples?— To verify whether the stability of these results is maintained in subsamples of the data, I estimate the proposed model for northern and southern subsamples and report the corresponding climate change impact estimates in figure 6. These correspond to the state-fixed-effects specification but results based on the pooled results are very similar. In contrast to benchmark models, these results are consistent with the full sample result. Because the sample is smaller confidence intervals become wider, especially for the most severe scenario (RCP 8.5) and for the end-of-century horizon.

Are Results Sensitive to the Choice of the Dependent Variable?— Here I consider cash rent for non-irrigated cropland instead of the price of farmland.[52] Impact estimates based on the proposed model and a state-fixed-effect specification are presented in figure 7. Estimates are also indistinguishable from zero and consistent with the main results of the paper based

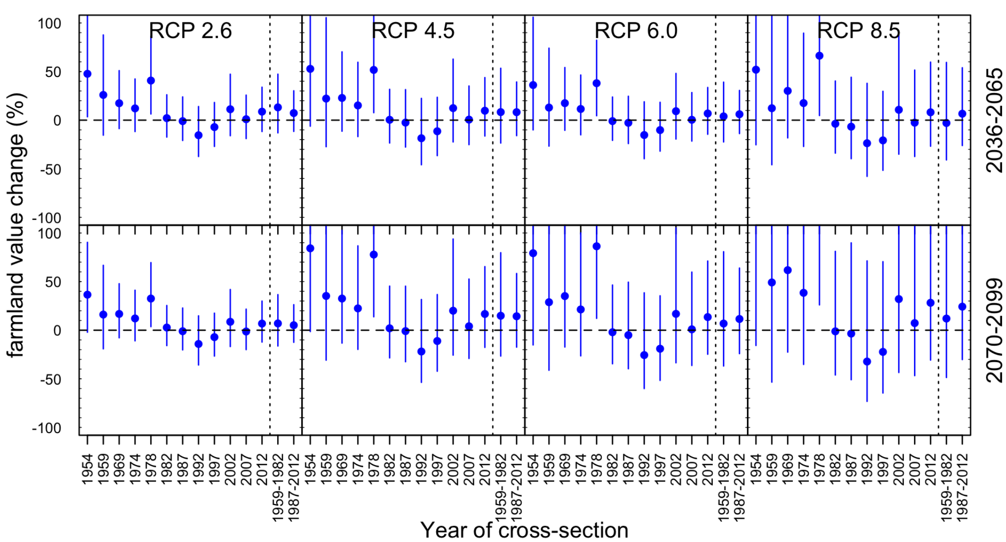
[51] The confidence intervals across benchmark and proposed models do no not overlap when based on the more efficient GMM estimator shown in the online appendix or on weighted regressions.

[52] In principle, using this variable rather than farmland values circumvents biases from the option value of farmland. The reason is that cropland renters are not residual claimants of the land and should therefore not be willing to pay a premium to rent cropland that is potentially developable in the distant future. In Ortiz-Bobea (2016), I find that the negative estimates of benchmark models are not robust to cash rents as an alternative dependent variable.

A. North

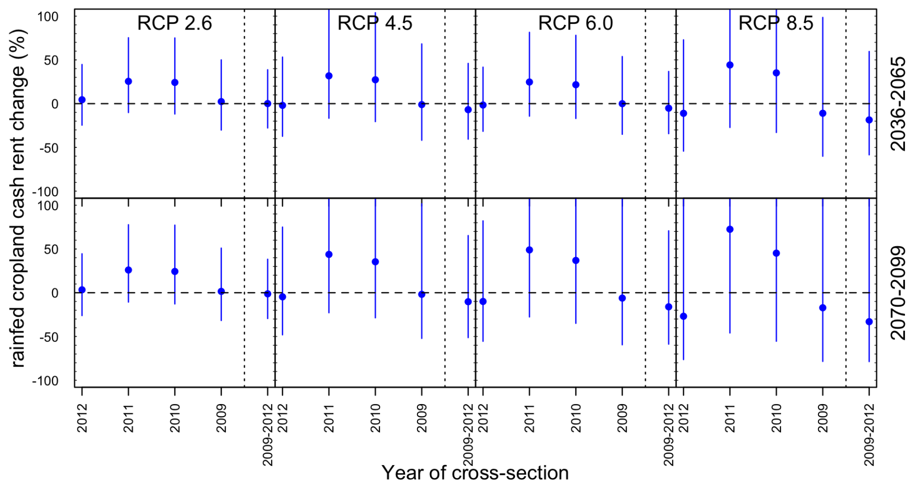


B. South



Notes: Subsample definitions are described in figure 4.

Figure 6: Climate Change Impacts Based on State-Fixed-Effect Preferred Models For Two Subsamples



Notes: Climate scenarios are based on the HadGEM2-ES model. Models adopt the same predictors than the main models based on farmland values. The sample size slightly varies for each cross-section and ranges from 1,790 (2009) to 2,010 (2012) counties. This is roughly 80 to 90% of the farmland value data for 2012 which is comprised of 2,232 observations.

Figure 7: Climate Change Impacts for State-Fixed-Effect Direct Models Based on Cash Rents as Dependent Variable

on farmland values.[53]

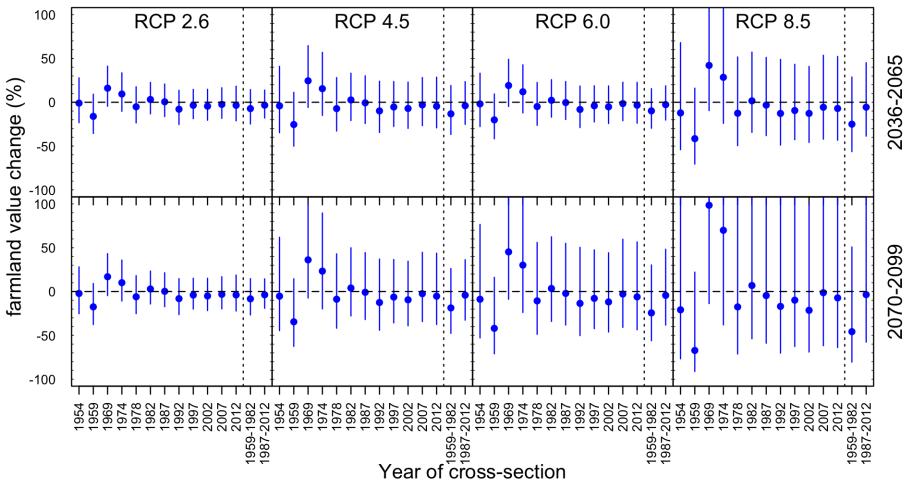
Are Results Sensitive to the Choice of Climate Variables?— Previous results are based on the degree-day variables and specification adopted in SHFb. To assess how results are affected by the choice of alternative climate variables I estimate two models, one based on a linear degree-day variable specification, and another based on monthly climate normals following MNS. Results for both specification are presented in figure 8. Again, the results are qualitatively similar for alternative climate variables.

Are Results Sensitive to County Neighborhood Definitions?— Until now I have relied on a first-order county neighborhood and equal weights to construct the neighborhood-level control variables. This choice was supported by evidence in tables 7 and 8 and figure 2, showing that neighborhoods of order 2 and higher, allow for incidental correlations between climate variables and unrelated observables. One should therefore expect bias in the direction of benchmark estimates when relying on less restrictive neighborhoods.[54]

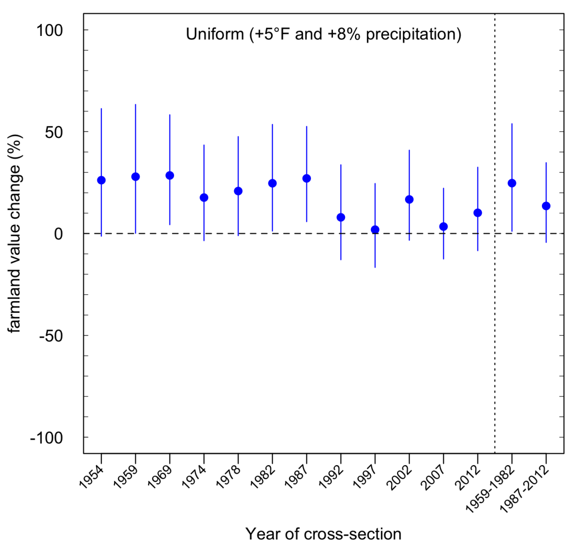
[53]Results based on a cross-sectional specification are very similar.

[54]Indeed, in simulation results in the online appendix, I find that the adoption of increasingly large neighborhoods leads to bias in the direction of the benchmark model biases in the presence of a spatial

A. Linear Degree-Day Variables



B. Monthly Average Climate Variables



Notes: All proposed models adopt a first-order neighborhood definition and equal weights. The linear degree-days specification includes separate linear terms for degree-days 10-30°C and >30°C as well as linear and quadratic precipitation variables. The monthly climate variable specification includes linear and quadratic terms for monthly mean temperature and precipitation normals for the months of January, April, July and October. The climate scenarios for the linear degree-days specification is based on the HadGEM2-ES climate model. The scenario for the monthly average variable specification is a uniform scenario. This was chosen because climate change projections become highly unstable for relatively large changes in climate reflected in the GCM scenarios.

Figure 8: Climate Change Impacts Based on Direct Climate Variation with State-Fixed-Effects Based on Different Climate Variables

To save space, I report climate change impact estimates based on the proposed models but with increasingly expansive neighborhoods (second to fourth-order neighborhoods) in the online appendix. The second-order neighborhood estimates are very similar to the first-order neighborhood estimates (figure 5). However, the third and especially the fourth-order proposed estimates appear increasingly negative and significant for *recent* cross-sections, resembling the time profile of the impact estimates of benchmark models. This pattern matches simulation predictions in the online appendix and confirms the presence of a spatial confounder which appears to gain influence toward more recent cross-sections. Note that this pattern *cannot* be explained as a "reduction" in attenuation bias. The reason is that the various neighborhoods considered lead to similar attenuations when climate variables are measured with error (see figure 2).

Are Results Sensitive to Neighborhood Weighting Schemes?— So far I have relied on equal neighboring-county weights when constructing neighborhood-average regressors. I present these results based on alternative weighting schemes in the online appendix.[55] I find that climate change impact estimates are fairly insensitive to the choice of neighborhood weights.

5 Discussion

To put the new findings in perspective, I summarize climate change impact estimates on farmland values for competing models in table 9. All models include state fixed effects. These estimates are based on the same data and correspond to a model based on averaged variables for the 1987-2012 cross-sections. In line with previous findings in the literature, benchmark models systematically point to large negative effects of climate change on farmland values. This is the case for both unweighted and weighted regressions (columns 1 and 2).

Climate change impact estimates for the benchmark state fixed effects model ranges

confounder. This occurs when the chosen neighborhood exceeds the true scale of the spatial confounder.

[55]These weights include: equal, inverse of the squared root of the distance, inverse of the distance and linear or Bartlett weights. The latter simply represents a weighting scheme that linearly decreases until it reaches 0 for a cutoff distance. Here the cutoff distance is the maximum distance (in miles) to the farthest neighbor plus 1 mile.

Table 9: Climate Change Impacts for Competing Models under Alternative Scenarios

	(1) Benchmark (Conley)	(2) Benchmark (WLS)	(3) Direct (Conley)	(4) Direct (WLS)
Scenario	*Impacts for 2036-2065 (%)*			
RCP 2.6	-25.8 [$-37.1; -12.6$]	-18.1 [$-21.8; -14.2$]	-5.0 [$-19.2; 11.8$]	2.3 [$-10.8; 17.2$]
RCP 4.5	-38.1 [$-51.6; -20.8$]	-28.5 [$-33.2; -23.6$]	-5.9 [$-25.7; 19.1$]	4.4 [$-13.3; 25.7$]
RCP 6.0	-31.3 [$-43.8; -15.9$]	-22.8 [$-27.0; -18.3$]	-4.0 [$-20.5; 16$]	4.7 [$-10.1; 21.9$]
RCP 8.5	-55.4 [$-69.5; -34.7$]	-44.8 [$-50.0; -39.0$]	-9.1 [$-37.4; 32.0$]	6.2 [$-19.0; 39.3$]
Scenario	*Impacts for 2080-2099 (%)*			
RCP 2.6	-27.3 [$-38.7; -13.8$]	-19.5 [$-23.2; -15.6$]	-5.1 [$-19.8; 12.3$]	2.7 [$-10.6; 17.9$]
RCP 4.5	-48.1 [$-63.1; -27$]	-37 [$-42.7; -30.7$]	-7.5 [$-33.1; 27.8$]	5.9 [$-18; 36.7$]
RCP 6.0	-55.2 [$-70.1; -32.9$]	-43.7 [$-49.6; -37.1$]	-8.5 [$-37.7; 34.3$]	6.8 [$-20.5; 43.3$]
RCP 8.5	-74.5 [$-87.2; -49.3$]	-63.4 [$-69.7; -55.7$]	-11 [$-53.2; 69.2$]	12.9 [$-30; 82.1$]

Notes: Results are based on the 1987-2012 averaged cross-section and the HadGEM2-ES climate model. All models include state fixed-effects. Ninety five percent confidence intervals are presented in brackets below the point estimate. Percent impacts are computed as $100(\exp(\Delta\mathbf{X}\beta)-1)$, where $\Delta\mathbf{X}\beta$ are log farmland changes driven by changes in climatic variables only. As a result, percent confidence intervals are asymmetrical. $\Delta\mathbf{X}$ is computed as the farmland-weighted change in climate variables under each scenario. Columns 1 and 3 correspond to linear regressions corrected for spatial correlation using the Conley (1999) approach described earlier. Columns 2 and 4 corresponds to Weighted Least Squares (WLS) with observations weighted by the square root of farmland. All climate change scenarios correspond to Hadley GEM2-ES climate model. Results for other climate models are presented in the online appendix.

from -18.1% to (RCP 2.6, mid-century, weighted) to -74.5% (RCP 8.5, end-of-century, unweighted). These represent yearly profit changes of -9.4 to -38.9 billion, respectively. However, as previously shown, benchmark models are unstable over long time periods (figure 3) and across subsamples of the data (figure 4).

On the other hand, the corresponding impacts for the preferred model with state fixed effects ranges from -11.0% to -4.0% to (-5.7 to -2.1 G\$/year) for unweighted regressions, and from $+2.3\%$ to $+12.9\%$ ($+1.2$ to $+6.7$ G\$/year) for weighted regressions. However, *none* of these estimates are statistically different from zero. This finding is stable over time (figure 5), across subsamples of the data (figure 6), and is robust to an alternative dependent variable (figure 7), climate variables (figure 8) and neighborhood weighting schemes (online appendix).

In addition, simulation results confirm that the preferred estimates are robust to a wider range of confounders than benchmark models (see online appendix). However, this flexibility comes at a price of higher vulnerability to measurement error relative to a pooled or state-fixed-effect benchmark model. Nevertheless, the small and insignificant preferred estimates *cannot* be attributed to classical measurement error. The reason is that mismeasurement of local climate would *also* lead to attenuation of benchmark fixed effects estimates, which is clearly not the case given their similarity to pooled benchmark estimates (figure 3).

The proposed approach is can *additionally* account for the presence of time-invariant spatially-dependent regional climate confounders relative to benchmark models. As a result, these results are more robust. Evidence discussed in Ortiz-Bobea (2016) suggests that development pressure and the resulting rising option value of farmland is a likely confounder in benchmark hedonic models. The proposed approach can account for such omitted variables.

Overall, these new findings provide a neutral but cautionary long-run outlook for US agriculture. The new long-run estimates are considerably more optimistic than benchmark estimates and suggests there is no robust evidence of either large benefits or damages from climate change on eastern US agriculture. However, because confidence intervals remain relatively wide for extreme and end-of-century scenarios, these findings cannot definitively rule out sizable effects of climate change on the sector. In the online appendix I compute preferred estimates based on a more efficient spatial GMM estimator and I obtain much tighter confidence intervals. I reach a similar and stronger conclusion that there is no robust evidence for or against large effects of climate change on eastern US agriculture.[56]

Finally, note that these new estimates are *long-run* estimates. As a result, their relatively small magnitude does not contradict negative *short-run* estimates found in the literature on profits (DG 2012) or crop yields (e.g. Schlenker and Roberts, 2009). Divergence between short-run and long-run estimates should be expected, but long-run estimates should be systematically more optimistic than their short-run counterparts. The neutral findings provided in this paper therefore provide an internally consistent order of estimates in the literature.

6 Conclusion

Over the past two decades there has been a lively debate regarding the potential impacts of climate change on US agriculture. The more recent studies based on the hedonic approach find large detrimental effects of climate change on the sector. I find very similar results in my replication, with impacts ranging from -18.1% (-9.4 G\$/year) to -74.5% (-38.9 G\$/year) for the state-fixed-effect benchmark model across all scenarios and time horizons. However, I find these results are not robust and are likely affected by time-invariant spatially-dependent omitted variables.

This paper proposes a novel identification strategy to estimate the long-run effect of climate on farmland values that relies on climatic variation conditional on county neighborhood-average regressors. This approach *additionally* addresses the type of confounder that appears to affect the benchmark models. The approach makes two identifying assumptions that appear to hold for eastern US agriculture. First, it posits that the effect of climate on a county's agricultural productivity operates through its *own* climate. Second, the approach posits that unobservables are uncorrelated with climate conditional on neighborhood

[56] Confidence intervals across models based on this alternative estimator do *not* overlap, indicating that the associated climate change impact estimates are clearly statistically different.

characteristics of each county observation.

New climate change impact estimates based on the proposed approach are dramatically different and range from −11.0% (−5.7 G$/year) to +12.9% (+6.7 G$/year) for the specification including state fixed effects. These estimates, however, are statistically insignificant. While the confidence intervals remain relatively wide for least squares estimates, more precise estimates are obtained with alternative spatial models. I find *no* robust evidence of large beneficial or detrimental impacts of climate change on eastern US agriculture.

This contribution can help rationalize the relative magnitude of projected climate change impacts stemming from alternative approaches that allow varying degrees of farmer adaptations. Methods that only allow short-run and within-year adjustments should naturally point to more detrimental effects than methods that allow for long-run adaptations, such as the hedonic approach. Therefore, the findings in this paper do not contradict the large negative effects of weather shocks on crops yields because such estimates allow for a narrower range of farmer responses and adjustments.

It is important to emphasize that the proposed approach is not a panacea for controlling any type of time-invariant omitted variables in a cross-sectional setting. The proposed approach only eliminates bias from spatially-dependent regional confounders. In addition, the model increases vulnerability to measurement error, although this is not a concern in this paper. Studies relying on the proposed approach should allocate significant efforts to carefully constructing climate regressors based on detailed data and conduct placebo tests to rule the presence of measurement error and local confounders from results.

The hedonic approach, in general, has several important caveats. The approach relies historical variation in the data to infer future responses. However, there are changes that are not perceptible in historical observations that are expected to occur with climate change. These include the rise of atmospheric carbon concentrations, the depletion of aquifers or large-scale ecological changes that affect pest populations and thus agricultural production. These remain important unknowns and add to the uncertainty of these results. On the other hand, the reduced-form nature of this approach does not allow unpacking the mechanisms through which farmers adapt.

Finally, more research is needed to identify potentially fruitful pathways to enhance farmer adaptations to a changing climate (Ortiz-Bobea and Just, 2013) and assess policies that may hinder such adaptations (Annan and Schlenker, 2015). Also, more research effort should focus in areas, such as sub-saharan Africa, where data is scarce and the potential effects of climate change on agriculture are likely to be most disruptive (Schlenker and Lobell, 2010). Perhaps one of the major challenges of a changing climate for the agricultural and food sectors is the rise of climate variability. Particular attention should be devoted to the understanding of the linkage between climate variability and food price volatility as well as its resulting distributional consequences (Fafchamps, 1992; Bellemare, 2015).

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— For Online Publication —

The Economic Impacts of Climate Change on Agriculture: Accounting for Time-invariant Unobservables in the Hedonic Approach

Online Appendix

By Ariel Ortiz-Bobea

Contents

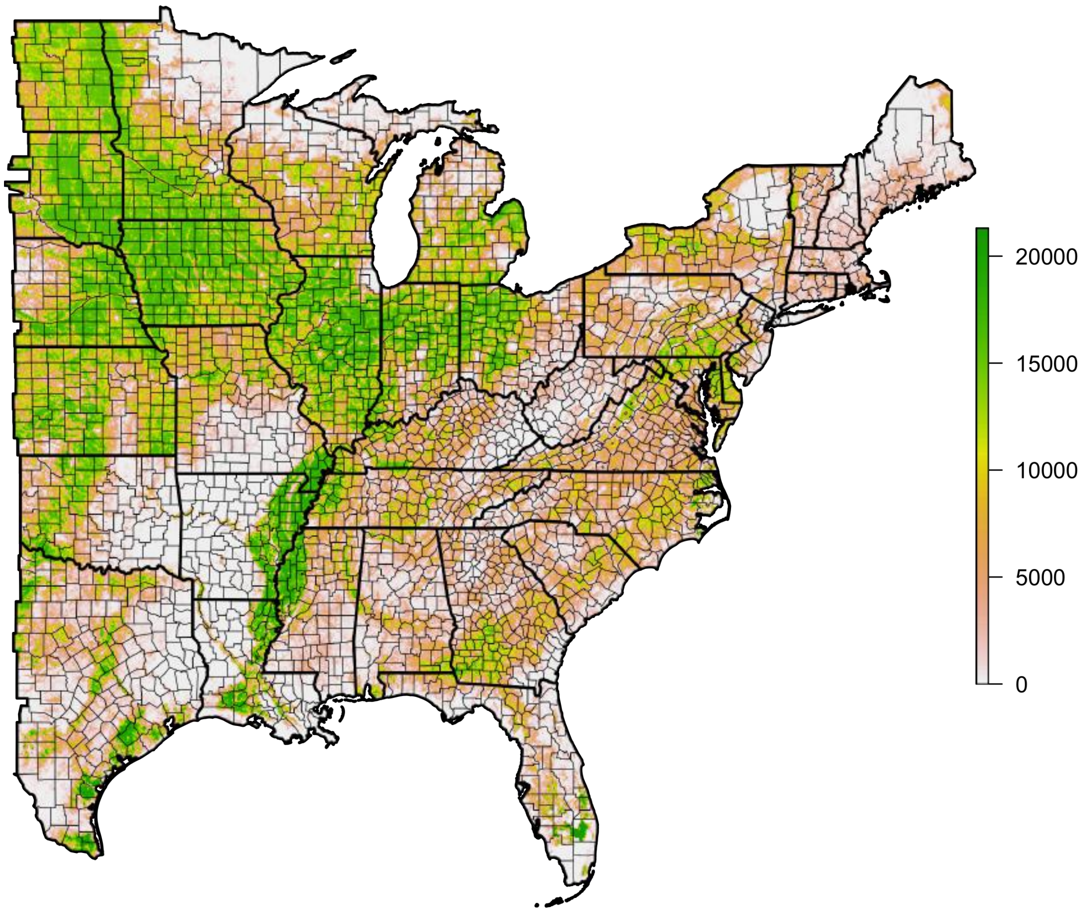


Figure 1: 2008-2014 CDL cropland counts per PRISM data grid

1 Data

This section describes how climate variables were constructed. I also provide maps of these climate and controls variables to allow for a clear visual inspection.

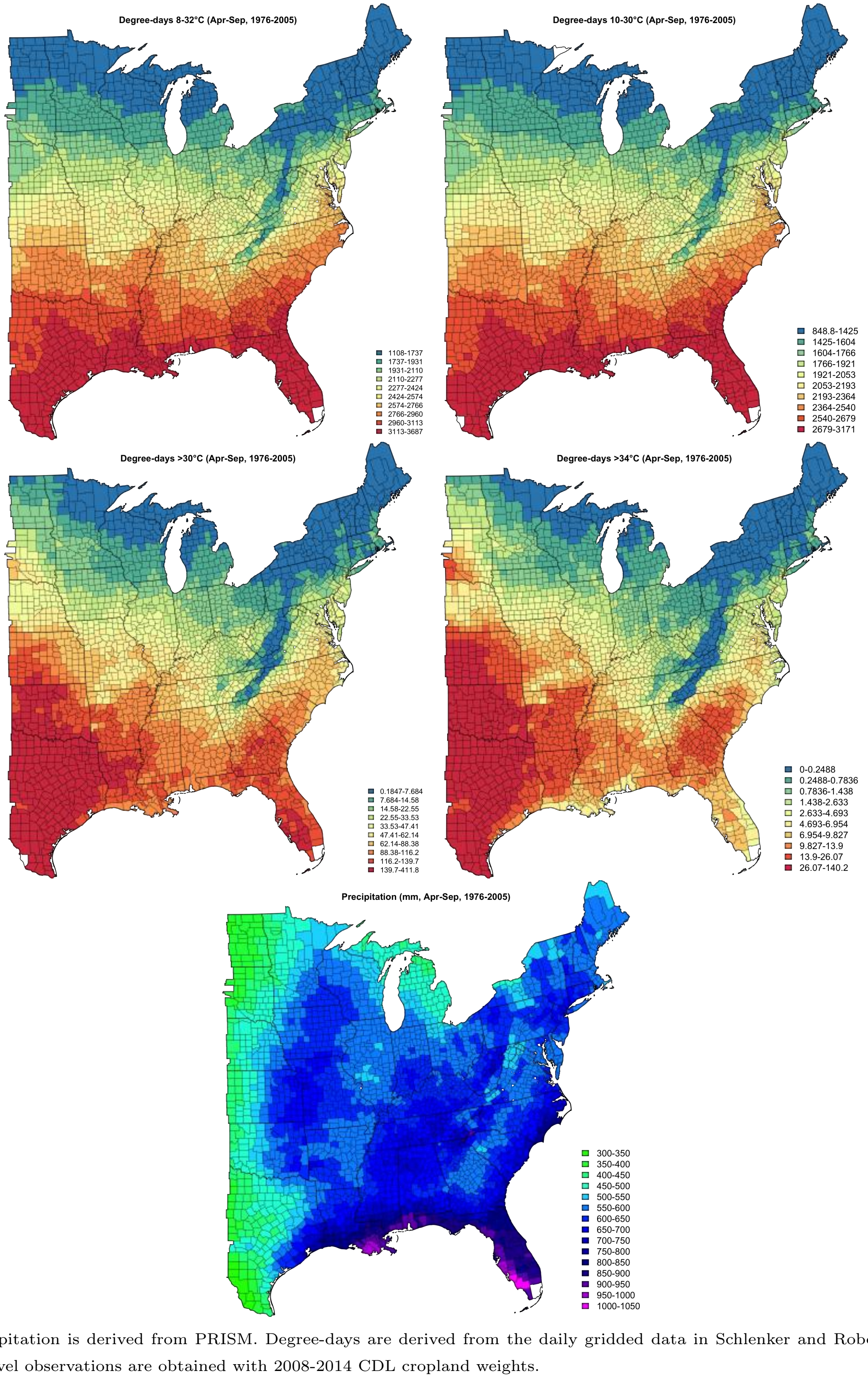
1.1 Cropland Weights for Climate Data Aggregation

County-level climate variables were obtained by aggregating PRISM data based on cropland weights. These weights were obtained by averaging the the cropland CDL counts for years 2008-2014 falling within each PRISM data grid. A map of these weights and the land cover classes used to classify CDL pixels to cropland are provide in the figure and table below.

Table 1: Cropland Classes in USDA Cropland Data Layer

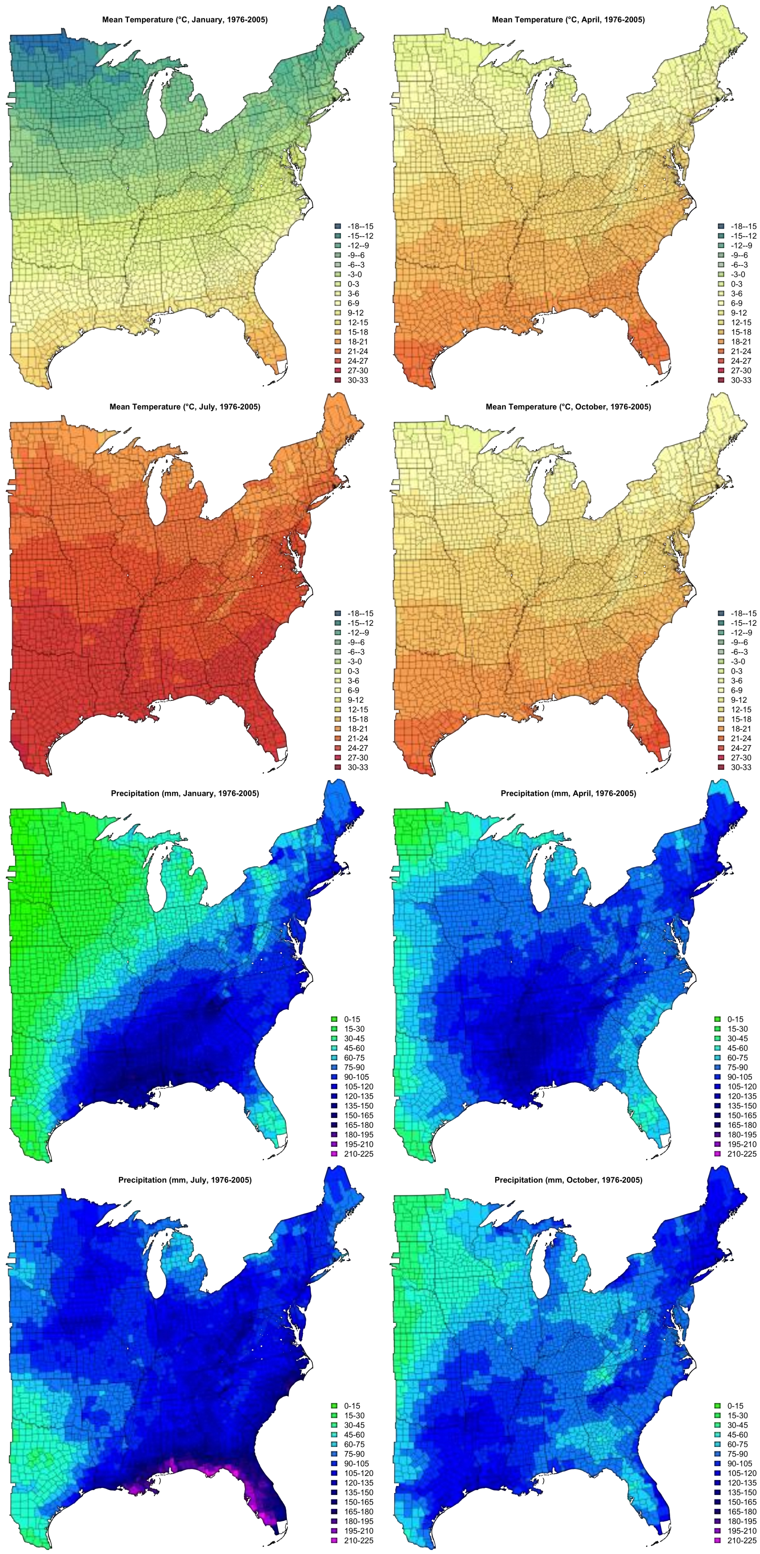
Value	Category	Value	Category	Value	Category	Value	Category
1	Corn	37	Other Hay/Non Alfalfa	70	Christmas Trees	225	Dbl Crop WinWht/Corn
2	Cotton	38	Camelina	71	Other Tree Crops	226	Dbl Crop Oats/Corn
3	Rice	39	Buckwheat	72	Citrus	227	Lettuce
4	Sorghum	41	Sugarbeets	74	Pecans	229	Pumpkins
5	Soybeans	42	Dry Beans	75	Almonds	230	Dbl Crop Lettuce/Durum Wht
6	Sunflower	43	Potatoes	76	Walnuts	231	Dbl Crop Lettuce/Cantaloupe
10	Peanuts	44	Other Crops	77	Pears	232	Dbl Crop Lettuce/Cotton
11	Tobacco	45	Sugarcane	204	Pistachios	233	Dbl Crop Lettuce/Barley
12	Sweet Corn	46	Sweet Potatoes	205	Triticale	234	Dbl Crop Durum Wht/Sorghum
13	Pop or Orn Corn	47	Misc Vegs & Fruits	206	Carrots	235	Dbl Crop Barley/Sorghum
14	Mint	48	Watermelons	207	Asparagus	236	Dbl Crop WinWht/Sorghum
21	Barley	49	Onions	208	Garlic	237	Dbl Crop Barley/Corn
22	Durum Wheat	50	Cucumbers	209	Cantaloupes	238	Dbl Crop WinWht/Cotton
23	Spring Wheat	51	Chick Peas	210	Prunes	239	Dbl Crop Soybeans/Cotton
24	Winter Wheat	52	Lentils	211	Olives	240	Dbl Crop Soybeans/Oats
25	Other Small Grains	53	Peas	212	Oranges	241	Dbl Crop Corn/Soybeans
26	Dbl Crop WinWht/Soybeans	54	Tomatoes	213	Honeydew	242	Blueberries
27	Rye	55	Caneberries	214	Broccoli	243	Cabbage
28	Oats	56	Hops	216	Peppers	244	Cauliflower
29	Millet	57	Herbs	217	Pomegranates	245	Celery
30	Speltz	58	Clover/Wildflowers	218	Nectarines	246	Radishes
31	Canola	59	Sod/Grass Seed	219	Greens	247	Turnips
32	Flaxseed	60	Switchgrass	220	Plums	248	Eggplants
33	Safflower	66	Cherries	221	Strawberries	249	Gourds
34	Rape Seed	67	Peaches	222	Squash	250	Cranberries
35	Mustard	68	Apples	223	Apricots	254	Dbl Crop Barley/Soybeans
36	Alfalfa	69	Grapes	224	Vetch		

1.2 Maps of Key Variables



Notes: Precipitation is derived from PRISM. Degree-days are derived from the daily gridded data in Schlenker and Roberts (2009). All county-level observations are obtained with 2008-2014 CDL cropland weights.

Figure 2: Seasonal Climate Variables in the Eastern United States (1976-2005)



Notes: Variables are derived from PRISM based on 2008-2014 CDL cropland weights.

Figure 3: Monthly Climate Variables in the Eastern United States (1976-2005)

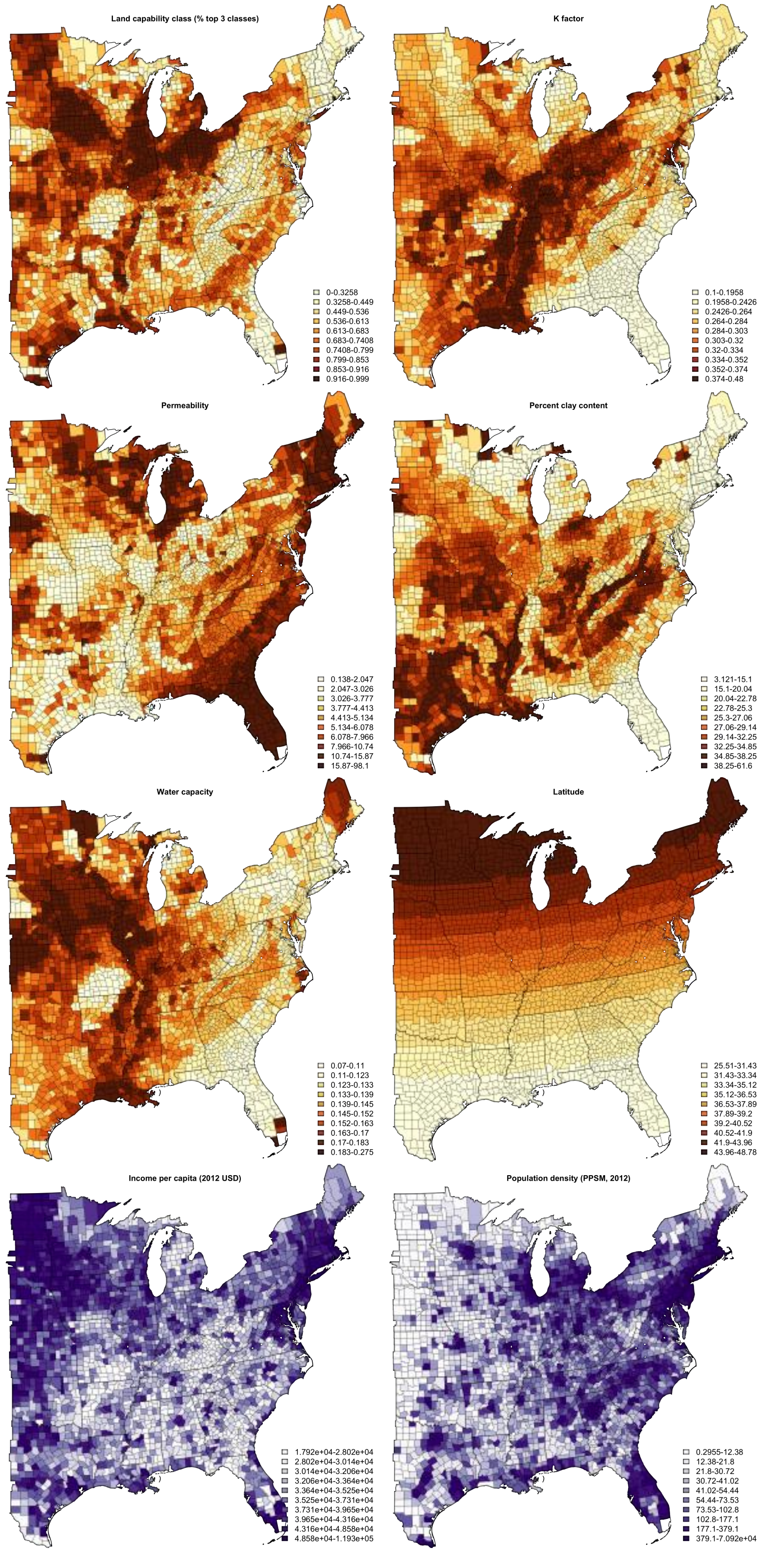


Figure 4: Soil and Economic Control Variables

2 Additional Evidence in Favor of the Identifying Assumptions

2.1 Correlation Analysis Between Select Observables and Other Climate Variables

I present an extension of the correlation analysis performed in table 8 of the paper for the other two climate variables: extreme degree-days and precipitation. The analysis explores where these climate variables can explain variation in these select observables *conditional* on neighborhood-average values of the observables. As stated in the paper, we should expect some degree of association with these select observables because they either explain climate, or are affected by climate. Results are presented in tables 2 and 3.

For extreme degree days (2), 5 out of the 14 associations are statistically significant at conventional levels for the first-order neighborhood scale. The number is 7, 5 and 6 for second, third and fourth-order neighborhood scales. For pooled, and within state and district variation this number is 13, 12 and 12, respectively. For precipitation (table 3), three out of the 14 associations are statistically significant at conventional levels for the first-order neighborhood scale. The number is 5, 8 and 11 for second, third and fourth-order neighborhood scales. For pooled, and within state and district variation this number is 14, 11 and 10, respectively.

Again, in contrast to table 7 in the paper, these results show that climate variables are correlated with some observables (those that have some known relationship with climate) but not with others (those are are seemingly unrelated).

2.2 Simulation Exploring Performance of Competing Models Under Alternative Sources of Bias

To determine whether one can distinguish attenuation bias from other sources of bias, I conduct a series of simulations to characterize how competing models perform under varying forms of unobservables and measurement error. The purpose is not to lay out a comprehensive analysis of the properties of these models but to derive general insights to help differentiate, if possible, among alternative sources of bias. If one can establish different "footprints" for different sources of bias, than these insights can inform our interpretation of the previous empirical results.

For this exercise, I assume a general data generating process (DGP) of the form $y_c = \alpha + \beta x_c + \epsilon_c$, where the unknown parameter of interest ($\beta = 1$) represents the direct effect of climate x_c on farmland values y_c. The spatial structure of the simulated data mimics the true sample data for the eastern US and x_c is a real climate variable.[1] The analysis focuses on the empirical distribution of $\hat{\beta}$ rather than impact predictions (*i.e.* $\Delta\hat{y}$) to simplify exposition. Table 4 provides a description of the DGP for the various cases explored as well as greater detail about simulation parameters.

Results for the simulations are presented in table 5 where each panel (A-E) corresponds to cases described in table 4. The results in this section remain qualitatively similar under various simulation assumptions regarding the sign and magnitude of the climate confounding, neighbor weighting schemes, alternative neighboring definitions, magnitudes of spatial dependence in the disturbance and errors-in-variables magnitude.

[1] I adopt a DGP with a single climate explanatory variable, extreme degree-days (>34ºC), for simplicity of exposition.

Table 2: COEFFICIENTS FROM PAIRWISE REGRESSIONS OF SELECT INDICATORS ON EXTREME DEGREE-DAYS

	Pooled		Fixed Effects State		Fixed Effects District		Neighborhood Order 1rst		2nd		3rd		4th	
Degree-days >34ºC	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
Geographical indicators:														
Latitude	-32.46	***	-9.08	***	-2.31	*	2.15	*	2.65	**	3.18	**	3.28	**
Longitude	-29.66	***	-15.13	***	-10.58	***	-3.21	**	-4.58	***	-5.74	***	-6.53	***
Altitude	-5.1	***	-2.05	*	-5.4	***	-0.35		0.13		0.66		1.09	
Satellite indicators (2001-2014):														
AMP	-22.66	***	-2.72	**	-1.31		0.74		1.21		0.99		0.87	
DUR	-1.78		2.31	*	1.5		0.76		1.36		1.83		2.17	*
EOSN	3.66	***	-12.07	***	-6.93	***	-1.31		-2.48	*	-3.38	***	-4.35	***
EOST	-42.48	***	-12.77	***	-8.09	***	2.81	**	1.78		0.49		-0.43	
MAXN	-53.92	***	-24.55	***	-11.22	***	2.21	*	2.02	*	0.99		0.04	
MAXT	-22.32	***	-0.18		-4.37	***	2.56	*	2.96	**	3.27	**	3.43	***
SOSN	2.43	*	-12.95	***	-7.86	***	-1.35		-2.58	**	-3.57	***	-4.63	***
SOST	-25.67	***	-7.91	***	-7.19	***	1.71		1.09		0.68		0.52	
TIN	-31.64	***	-10.89	***	-3.71	***	0.36		0.51		0.17		-0.1	
Agricultural indicators (1976-2005):														
Corn yield	-18.41	***	4.16	***	2.45	*	1		2.75	**	1.42		1.56	
Soybean yield	-21.98	***	-0.02		-1.96	*	1.1		0.69		0.94		-0.26	

Notes: The table presents the t-statistics for pairwise regression coefficients of climate variables (shown by panel) on observable determinants of farmland values (shown by rows). Symbols *,** and *** indicate statistical significance at the 5, 1 and .1 percent level, respectively. Standard errors are corrected for spatial correlation following Conley (1999). The sample ($n = 2,457$) includes all counties east of the 100th meridian west. Column (1) indicates the coefficient for a pooled unconditional regression based on sample-wide variation. Columns (2) and (3) correspond to state- and district-fixed-effect specifications, respectively. Columns (4) to (7) indicate coefficients conditional on neighborhood-average values of the observable based on different neighborhood definitions (see Data section in the paper for more details).

Table 3: Coefficients from Pairwise Regressions of Select Indicators on Precipitation

			Fixed Effects				Neighborhood Order							
	Pooled		State		District		1rst		2nd		3rd		4th	
Precipitation	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
Geographical indicators:														
Latitude	-26.16	***	-12.71	***	-10.75	***	-5.06	***	-7.3	***	-9.41	***	-10.92	***
Longitude	18.94	***	15.71	***	7.00	***	3.81	***	5.82	***	7.72	***	9.59	***
Altitude	-20.73	***	-7.34	***	5.35	***	7.43	***	8.02	***	7.64	***	7.51	***
Satellite indicators (2001-2014):														
AMP	-23.79	***	-5.37	***	-2.2	*	1.46		1.61		1.56		1.37	
DUR	-8.54	***	-0.21		-0.36		0.62		1.31		1.59		1.64	
EOSN	36.39	***	16.90	***	10.06	***	-0.68		-0.15		0.82		1.94	
EOST	11.87	***	15.47	***	8.05	***	-0.31		0.66		2.11	*	3.53	***
MAXN	16.99	***	18.09	***	10.29	***	-0.22		0.78		2.11	*	3.29	**
MAXT	12.34	***	8.16	***	7.33	***	0.51		0.68		1.19		2.07	*
SOSN	37.85	***	18.22	***	11.08	***	-0.52		0.12		1.22		2.48	*
SOST	25.91	***	13.40	***	8.93	***	0.11		0.63		1.76		2.83	**
TIN	-19.63	***	0.62		0.07		1.65		2.19	*	3.00	**	3.70	***
Agricultural indicators (1976-2005):														
Corn yield	-8.07	***	-2.06	*	-0.57		-0.62		0.74		3.84	***	3.87	***
Soybean yield	-7.62	***	1.89		0.58		0.69		2.01	*	2.29	*	2.86	**

Notes: See notes of table 2.

Table 4: Performance of Competing Models Under Alternative Forms of Unobservables and Measurement Error

Case	Description	Indep. var.	Error term
A	No omitted variable	x_c	$\epsilon_c = u_c$
B	Local or linear confounder	x_c	$\epsilon_c = u_c + \gamma x_c$
C	State-level confounder	x_c	$\epsilon_c = u_c + \gamma \bar{x}_{state_c}$
D	Spatial or regional confounder	x_c	$\epsilon_c = u_c + \gamma x_{N(c)}$
E	Classical measurement error	$x_c + m_c$	$\epsilon_c = u_c$

Notes: The data generating process (D.G.P) for all cases takes the general form $y_c = \alpha + \beta x_c^* + \epsilon_c$, where $\alpha = \beta = 1$ and where $x_c^* = x_c$ for cases A through D and $x_c^* = x_c + m_c$ for case E with measurement error ($\sigma_m^2 = 1$). The climate variable x_c is extreme degree-days (>34$^{\text{o}}$C) for April-September for county c which is a real variable used in the empirical analysis. The disturbance always comprises u_c, a spatially-dependent error that is uncorrelated with the regressor, with $u_c = \rho \sum_{i \in N(c)} w_i u_i + e_c = \rho u_{N(c)} + e_c$, $\sigma_e^2 = 1$, $\rho = 0.95$ and w_is are weights that sum to unity. In this analysis the weights decay with the inverse of the square root of distance and the set of county neighbors is of second-order contiguity. Different weights are explored later in the appendix. In addition, the disturbance may incorporate an omitted variable taking varying forms (cases B through D) with $\gamma = 1$. The variance of x_c and x_c^* is scaled to 1 prior to data manipulation to facilitate comparisons.

Table 5: Performance of Competing Models Under Alternative Forms of Unobservables and Measurement Error

	Benchmark Models			Direct Models — Pooled				Direct Models — State FE			
	Pooled	State FE	District FE	1st	2nd	3rd	4th	1st	2nd	3rd	4th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					A. No omitted variable						
Mean Est.	1.008	0.999	0.996	1.002	1.000	0.999	0.998	1.004	1.002	1.001	1.001
Bias	0.008	-0.001	-0.004	0.002	0.000	-0.001	-0.002	0.004	0.002	0.001	0.001
Std. Error	0.167	0.194	0.122	0.163	0.167	0.167	0.177	0.143	0.14	0.131	0.132
Var. Expl. (%)	50.39	20.21	11.33	1.37	2.21	3.18	4.08	2.15	3.38	4.79	6.01
					B. Positive local confounder						
Mean Est.	1.896	1.899	1.899	1.899	1.899	1.900	1.900	1.898	1.898	1.899	1.899
Bias	0.896	0.899	0.899	0.899	0.899	0.900	0.900	0.898	0.898	0.899	0.899
Std. Error	0.015	0.086	0.071	0.125	0.125	0.121	0.121	0.102	0.102	0.099	0.098
Var. Expl. (%)	94.8	81.56	69.30	19.51	28.36	36.58	42.68	27.59	37.82	46.71	52.70
					C. Positive state-level confounder						
Mean Est.	1.812	1.000	0.999	0.582	0.614	0.702	0.761	1.000	1.000	1.001	1.001
Bias	0.812	0.000	-0.001	-0.418	-0.386	-0.298	-0.239	0.000	0.000	0.001	0.001
Std. Error	0.024	0.088	0.059	0.065	0.069	0.073	0.081	0.067	0.068	0.065	0.065
Var. Expl. (%)	90.61	52.17	35.86	1.40	2.61	4.98	7.52	8.68	13.16	17.92	21.72
					D. Positive regional confounder						
Mean Est.	1.880	1.721	1.486	0.833	1.000	1.176	1.289	0.861	0.999	1.162	1.267
Bias	0.880	0.721	0.486	-0.167	0.000	0.176	0.289	-0.139	-0.001	0.162	0.267
Std. Error	0.015	0.081	0.057	0.071	0.079	0.085	0.092	0.065	0.067	0.067	0.070
Var. Expl. (%)	94.00	75.89	54.59	4.20	9.44	17.25	24.30	6.91	13.90	23.74	31.74
					E. Classical measurement error						
Mean Est.	0.707	0.184	0.062	0.221	0.118	0.09	0.085	0.132	0.100	0.087	0.085
Bias	-0.293	-0.816	-0.938	-0.779	-0.882	-0.910	-0.915	-0.868	-0.900	-0.913	-0.915
Std. Error	0.123	0.040	0.018	0.037	0.019	0.016	0.017	0.029	0.021	0.019	0.019
Var. Expl. (%)	25.04	2.61	0.56	2.53	0.72	0.42	0.38	1.39	0.82	0.63	0.61

Notes: The simulation is based on 1,000 repetitions. The Mean Estimate (Mean Est.) and Standard Error (Std. Error) are simply the mean and standard deviation of the empirical distribution of $\hat{\beta}$. Bias is the absolute difference between the Mean Estimate and the true value of the parameter ($\beta = 1$) . The "Variance explained" (Var. Expl.) refers to the variance explained by the climate variable used to estimate $\hat{\beta}$. It is computed as 1 minus residual variance with the relevant climate variable divided by residual variance without the relevant climate variable multiplied by 100.

Panel A illustrates the baseline case without omitted variables or measurement error. As expected, all models are unbiased. Notice how climate effects conditional on neighborhood climate are estimated off a very small share of the climate variation (around 1-6%) relative to the benchmark models (11-50%). And yet, the proposed models yield standard errors of the same order of magnitude. In the absence of omitted variables and measurement error, all models converge to the same estimate and just differ in efficiency. This obviously does not correspond to the empirical results in the paper given the large divergence in climate change impacts across benchmark and proposed models.

Panel B deals with the case of a positive "local" or linear confounder. This is a textbook case of an omitted variable that is linearly dependent with climate. None of the models can control for this type of confounder, which explains why the bias is virtually identical across all models. For practical purposes, results with a local confounder (panel B) and without any confounder (panel A) cannot be distinguished unless the correlation of the omitted variable with climate fluctuates over time. It is difficult to imagine what could, in practice, constitute such type of confounder other than omitted climate variables intricately related to the observed climate.[2] Again, this case cannot explain the divergence in the empirical estimates between benchmark and the preferred models.

The case with a state-level omitted variable correlated with climate is presented in panel C. Naturally, state and district fixed effects models closely match the DGP and are therefore unbiased (2-3). Interestingly, pooled models conditional on neighborhood average climate are biased in the opposite direction of the omitted variable (4-7). However, the proposed models *with* state fixed effects are unbiased (8-11). The pooled benchmark model is the most vulnerable to this type of confounder followed by pooled direct models.[3]

For panel C, the overall pattern of estimates across models holds irrespective of the magnitude and sign of the state-level confounder, which leads to a predictable order of estimates in which both benchmark and proposed models with state fixed effects (unbiased) fall between pooled proposed models (moderately biased in opposite direction of confounder) and benchmark pooled models (severely biased). This constitutes a clear "footprint" for a state-level confounder which might be more or less pronounced depending on the intensity of the bias. Note this case does not match the order of the empirical estimates in the paper, in which both pooled, state- and district-fixed-effect benchmark models point to large damages, while the pooled and state fixed effects models based on the proposed approach point to small insignificant effects.

Panel D presents the case with a spatially-dependent omitted variable correlated with climate. As highlighted in the paper, previous hedonic studies have largely ignored this form of omitted variable although it seems to plausibly characterize potential confounders such as unobserved soil quality or development pressure. The simulated confounder was generated assuming a second-order neighborhood structure with equal weights. This matches the neighborhood structure and weights of the second proposed model used, which explains why these are unbiased (columns 5 and 9). Note that the benchmark models (1-3) are considerably biased although results are less so for the benchmark fixed-effect models. It seems that the introduction of state or district dummies slightly reduces, but does not eliminate, the spatial covariance between the climate variable and the confounder. Also, the proposed models are slightly biased when the

[2] In fact, an analysis on climate variable covariance similar to that illustrated in tables 7 and 8 in the paper for non-climate observables indicates that *all* observed climate variables are locally correlated amongst each other. This does not pose, at first glance, a major threat given that observed climate variables may serve as proxies for unobserved climate. A problem may arise if the covariance of observed and unobserved climate is expected to change substantially under climate change. In such cases, observed climate variables are unreliable proxies for unobserved climate and climate change impact projections may be biased.

[3] Pooled proposed models are substantially less vulnerable than the pooled benchmark model. The likely reason is that the proposed models remove a great deal of the state-level unobservable through the neighborhood average control. However, the state-level unobservable is not fully removed for counties close to state boundaries which explains why the proposed models are slightly biased in this case.

assumed neighborhood does not match that of the underlying spatial confounder. The proposed model based on a first-order neighborhood —which assumes a smaller scale than the underlying scale of the confounder— is slightly biased downward (columns 4 and 8). Similarly, the proposed models based on third and fourth-order neighborhoods —which assume a larger scale than the underlying scale of the confounder— are increasingly biased in the direction of the benchmark model bias (upward) as more expansive neighborhoods are employed. However, the magnitude of the biases are small relative to the benchmark models.

This indicates that the proposed identification strategy is robust to spatially dependent confounders when neighborhood definitions approximately match the true scale of the regional confounder. On the other hand, benchmark pooled and fixed effects models are *increasingly* biased as larger dimensions of data variation are considered. Even the district-fixed-effect benchmark model is considerably biased, suggesting that the incorporation of regional dummies cannot fully absorb unobservables that are smoothly distributed in space and are correlated with climate. These results indicate that a regional spatial confounder also leads to a predictable order of estimates, which, in order of bias is: pooled, state fixed effects, district fixed effects and finally the proposed models.[4] This pattern strikingly matches the order of empirical estimates suggesting that the benchmark hedonic models are plausibly affected by spatially dependent confounders.

Finally, panel E presents the case with classical measurement error in the climate variable. This results in local climate variation that is noisy but does not affect regional differences in climate. As expected, all models suffer from attenuation bias, but to varying degrees. The pooled model is the least affected simply because it does not solely rely on local climatic variation. As expected (Griliches and Hausman, 1986), the measurement error is amplified in the within dimension as state and district fixed effects are introduced, exacerbating the attenuation. This pattern may also possibly explain why the empirical estimates based on state and then on district fixed effects are less severe than pooled ones.[5]

However, the crucial point is that the proposed models are *just as or less attenuated* than the benchmark state and district fixed effects models. In other words, if measurement error severely affects local models, then it should *also* affect the benchmark fixed effect models in a comparable fashion. These findings indicate that measurement error leads a distinct "footprint" than that of a spatial confounder. The pattern highlighted here clearly does not match empirical results in which state and district fixed effect estimates are large and significantly negative, while the proposed models point to small and statistically insignificant effects of climate change on farmland values. In conclusion, measurement error in climate cannot explain the small effects of climate change on farmland values found in the paper.

In summary, each form of omitted variable analyzed here leads to predictable order of estimates across models. These patterns are clearly distinct from that resulting from classical measurement error. This suggests that one can unequivocally distinguish attenuation bias from other forms of biases by contrasting estimates of different models. The analysis of the empirical results shows that state and district fixed effects estimates differ substantially from estimates based on the preferred models. This *would not* be the case in the presence of measurement error but *would* be the case in the presence of a *negative* spatial confounder.

[4]Regarding the proposed models, less restrictive neighborhoods could lead to bias in the direction of the benchmark models.

[5]Under certain conditions this ordering may be isomorphic to the presence of a regional confounder. Measurement error leads to climate effect estimates that approach zero as more restrictive fixed effects are introduced. Similarly, the presence of a positive (negative) spatial confounder when true climate impacts are positive (negative) would also lead to climate effect estimates that approach zero as more restrictive fixed effects are introduced. Note that while estimates that approach zero in the case of attenuation bias are clearly undesirable, estimates that approach zero for the latter case is a reflection of bias reduction.

3 Variations of Climate Change Impacts for Benchmark Models

3.1 Stability from Control Variable Omission

As discussed in the paper, one way to assess the robustness of the hedonic model is examining the stability of climate change impacts estimates when omitting control variables. Figure 5 shows the main benchmark climate change impacts reported in the paper but I also include impacts based on models omitting control variables. Estimates without control variables are slightly more negative but remain fairly similar to the main results presented in the paper. Again, while stable climate change estimates suggest that control variables are just weakly correlated with climate variables, this strategy remains uninformative regarding the strength of the correlation of unobservables with climate variables.

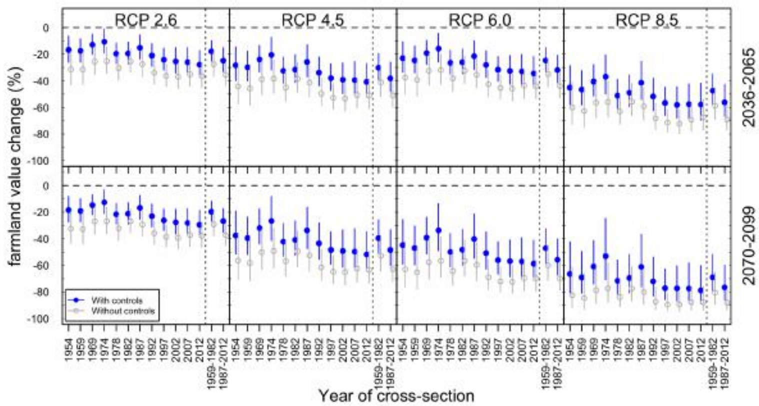
3.2 Alternative GMM Estimator

In this section I present climate change impact estimates on farmland values based on the spatial error model GMM estimator used in SHFb and developed by Kelejian and Prucha (1999). Results are summarized in figure 6. This estimator is more efficient than least squares but requires parametric assumptions regarding the structure of error dependence. The error weight matrix that captures the spatial dependence structure assumes a first-order neighborhood relationship with equal weights. Results are very similar for other weighting schemes, including inverse distance, inverse of the squared root of the distance and Bartlett or linear weights.

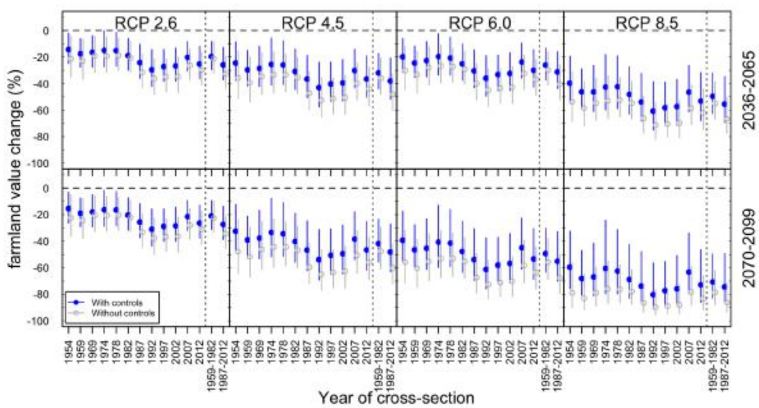
3.3 Alternative General Circulation Models (GCM)

Figures 7 through 11 present climate change impacts for the benchmark models (pooled, within-state and within-district) based on 4 different General Circulation Models (GCMs) as well as a uniform warming scenario of 5ºF and an increase of 8% in precipitation. All regressions are weighted by the squared root of farmland area. The GCMs correspond to: the second generation Canadian Earth System Model (CanESM2), the Community Climate System Model (CCSM4), the Geophysical Fluid Dynamics Laboratory Earth System Model (GFDL-ESM2M), and the The Norwegian Earth System Model (NorESM1-M).

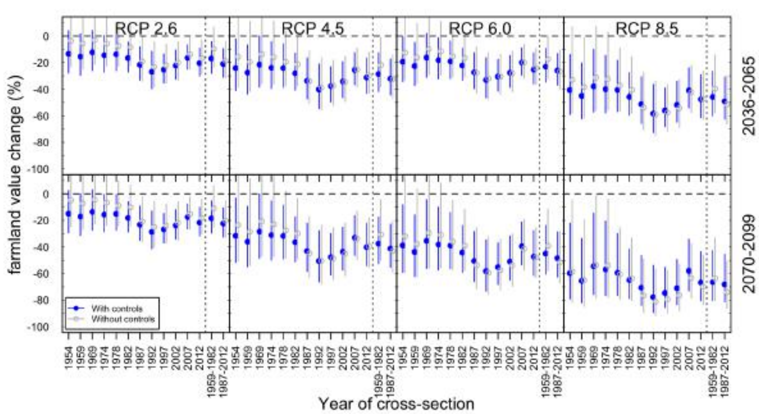
A. Pooled



B. State Fixed Effects



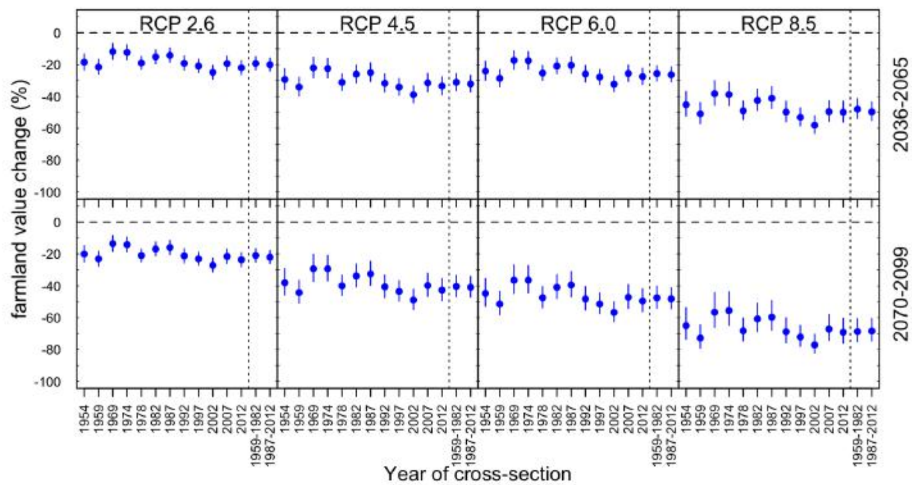
C. District Fixed Effects



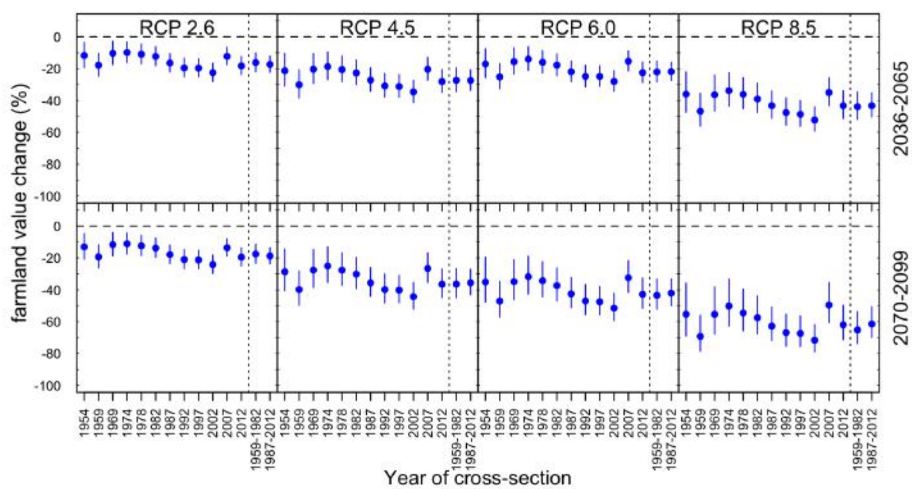
Notes: Results correspond to the climate model used in the paper (HadGEM2-ES).

Figure 5: Climate Change Impacts Based on Benchmark Hedonic Models With and Without Control Variables

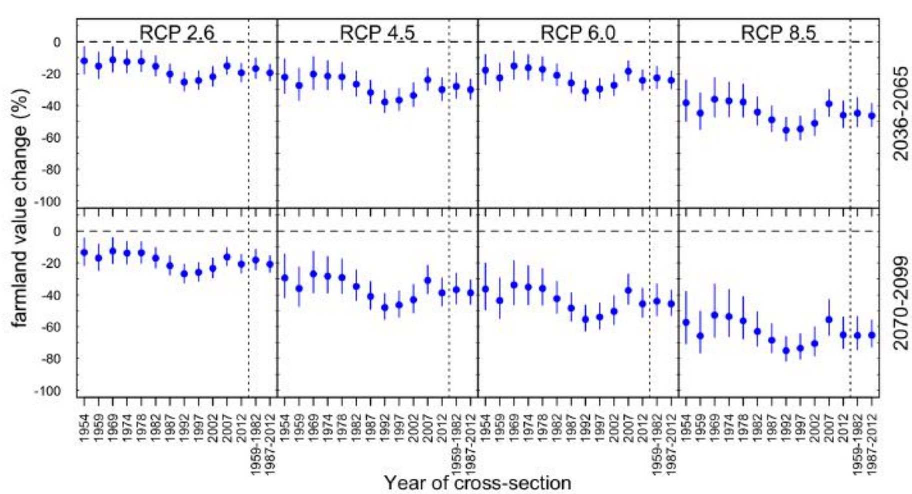
A. Pooled



B. State Fixed Effects



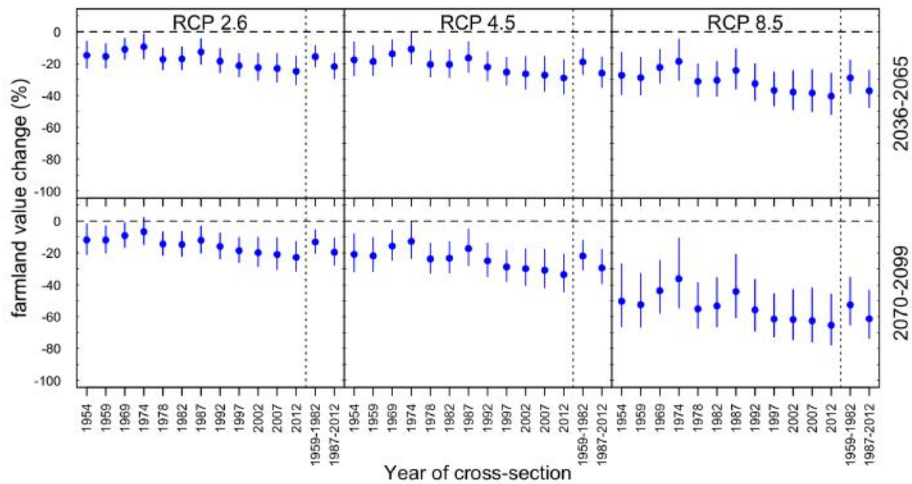
C. District Fixed Effects



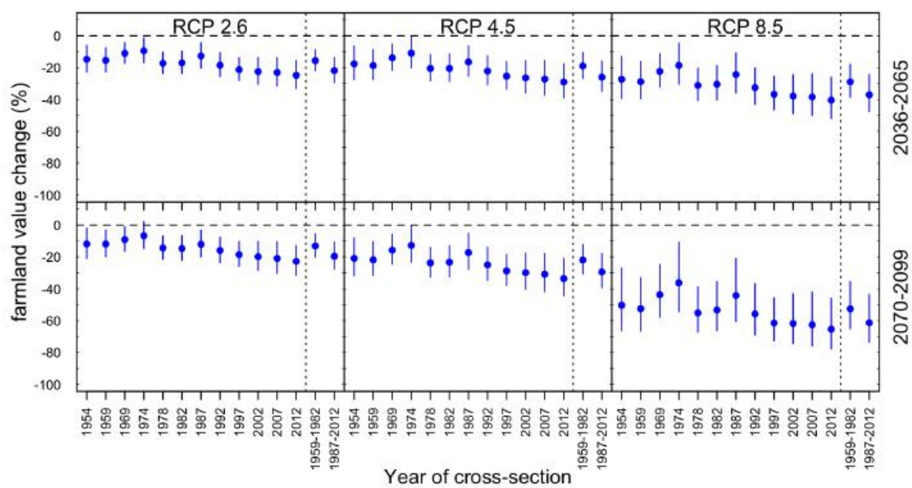
Notes: Results correspond to the climate model used in the paper (HadGEM2-ES).

Figure 6: Climate Change Impacts Based on Benchmark Hedonic Models Estimated via Spatial GMM

A. Pooled



B. State Fixed Effects



C. District Fixed Effects

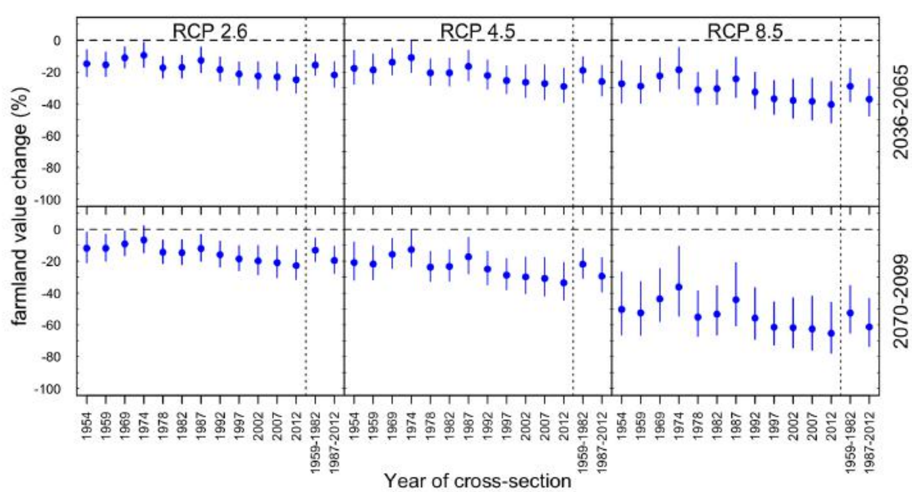
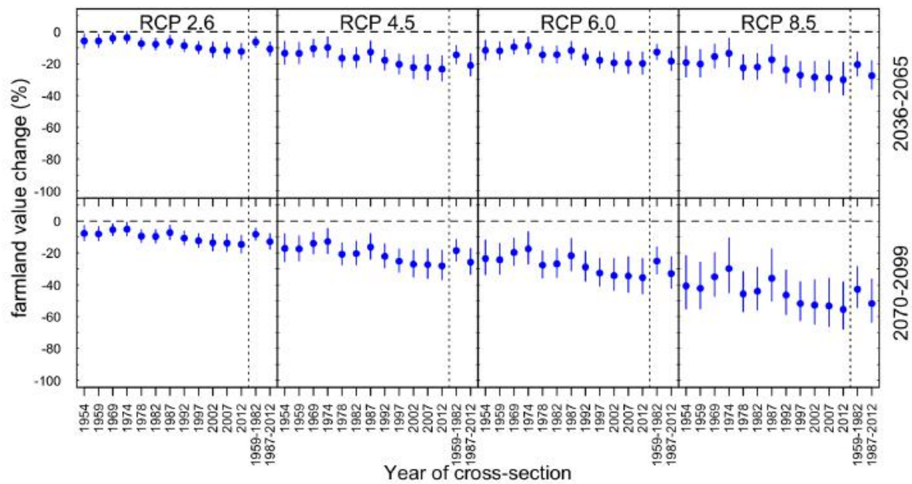
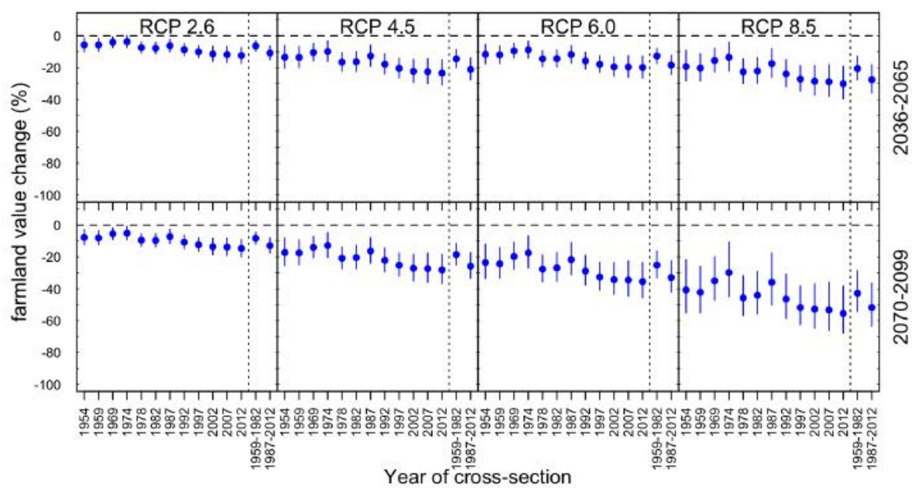


Figure 7: Climate Change Impacts For Benchmark Model Based on the CanESM2

A. Pooled



B. State Fixed Effects



C. District Fixed Effects

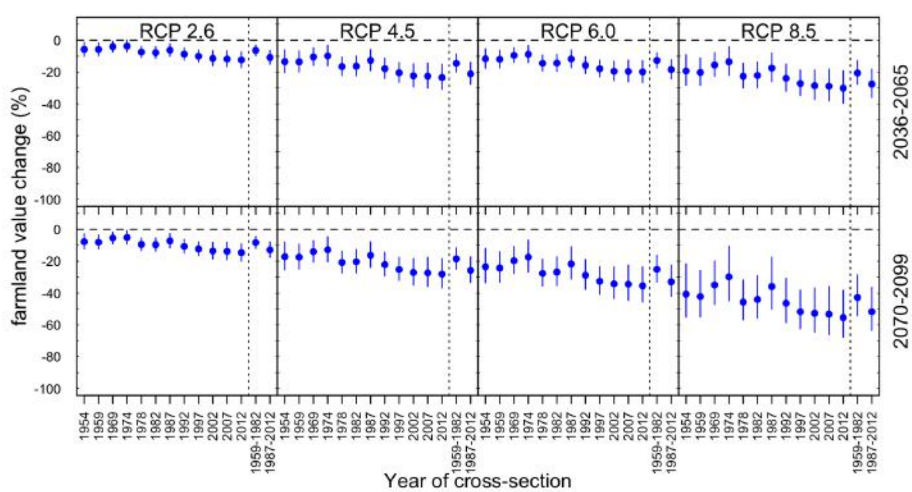
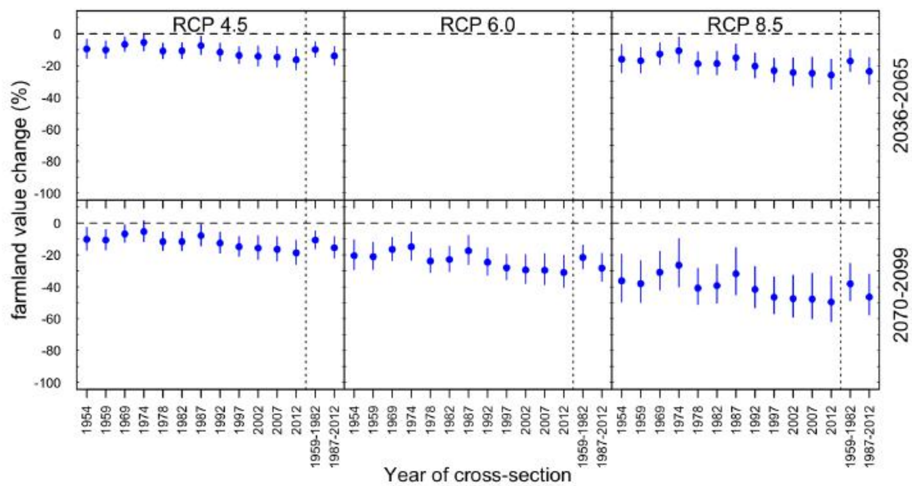
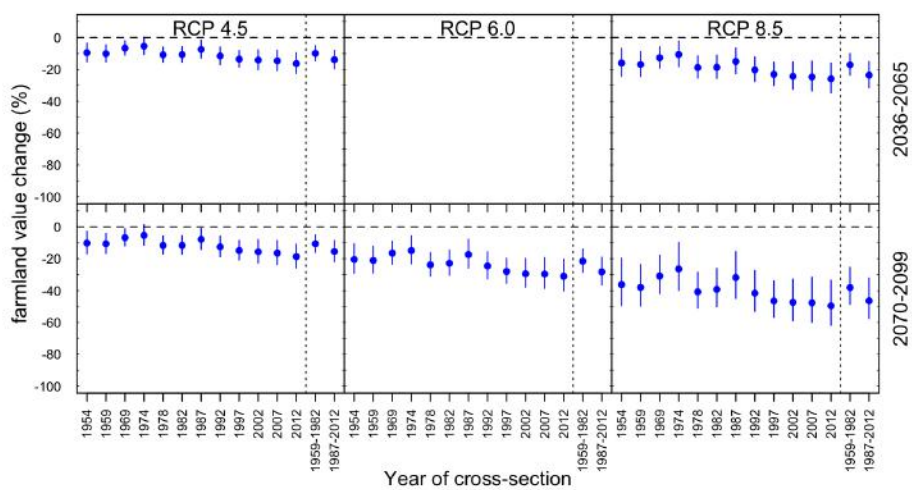


Figure 8: Climate Change Impacts For Benchmark Model Based on the CCSM4

A. Pooled



B. State Fixed Effects



C. District Fixed Effects

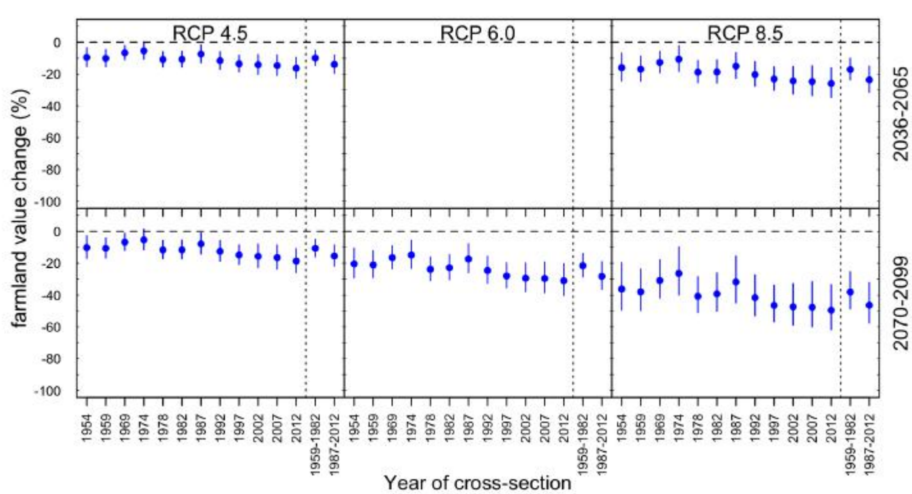
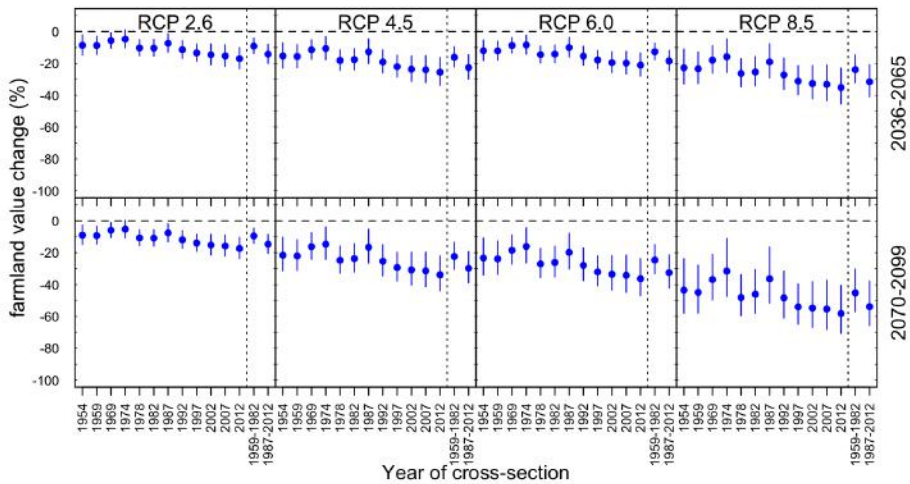
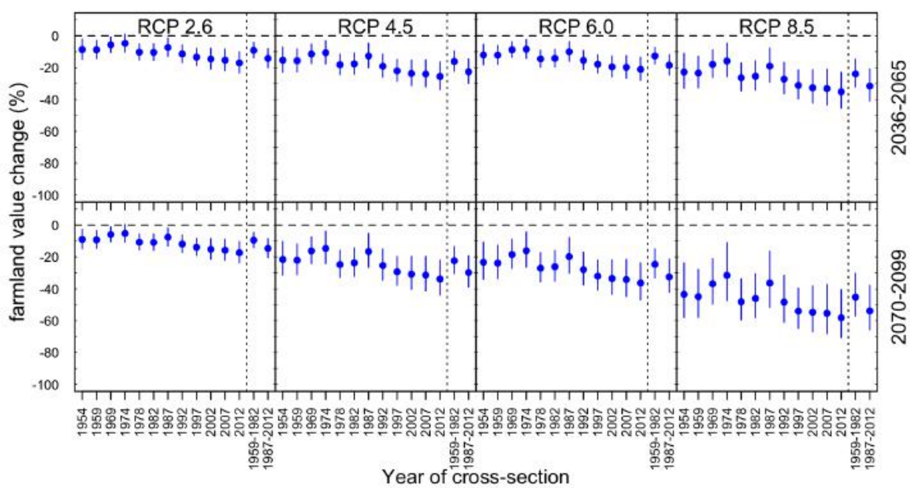


Figure 9: Climate Change Impacts For Benchmark Model Based on the GFDL-ESM2M

A. Pooled



B. State Fixed Effects



C. District Fixed Effects

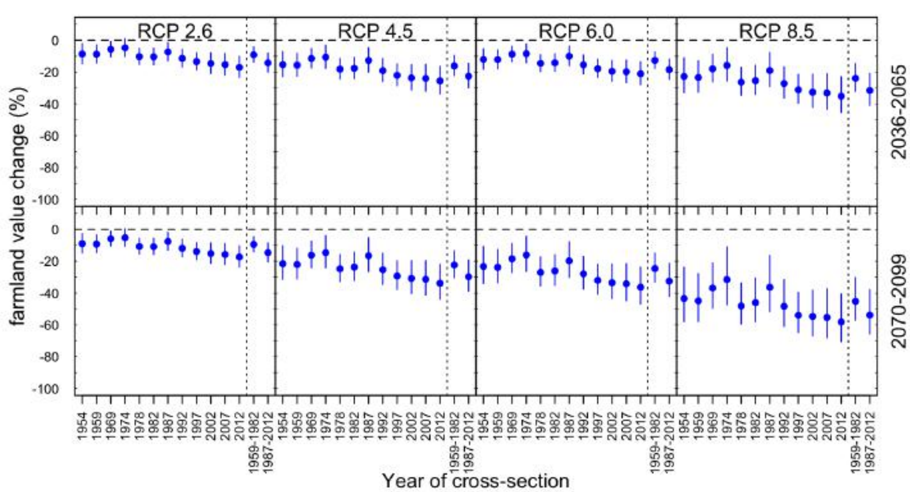
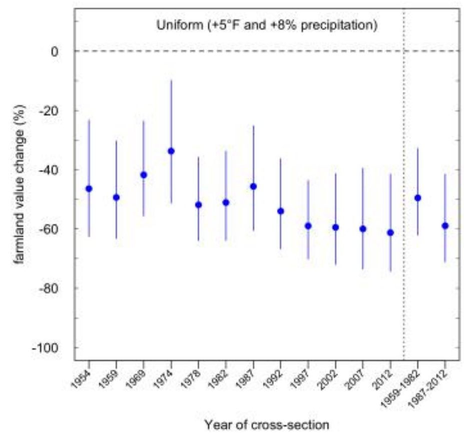
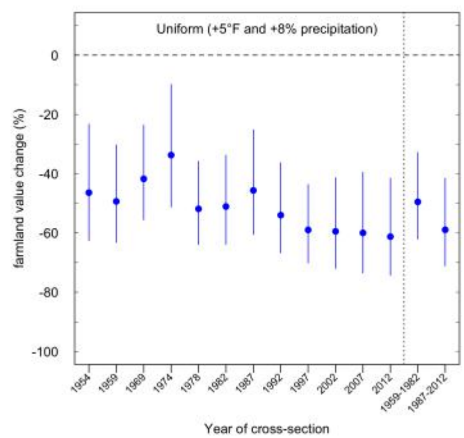


Figure 10: Climate Change Impacts For Benchmark Model Based on the NorESM1-M

A. Pooled



B. State Fixed Effects



C. District Fixed Effects

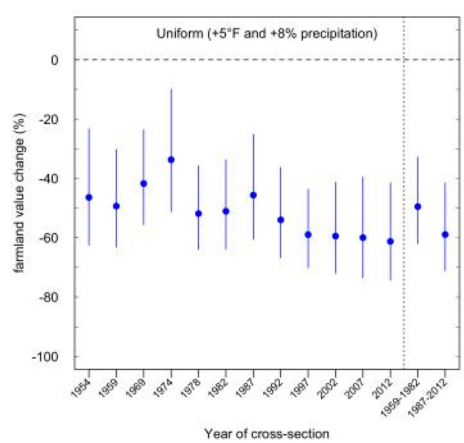
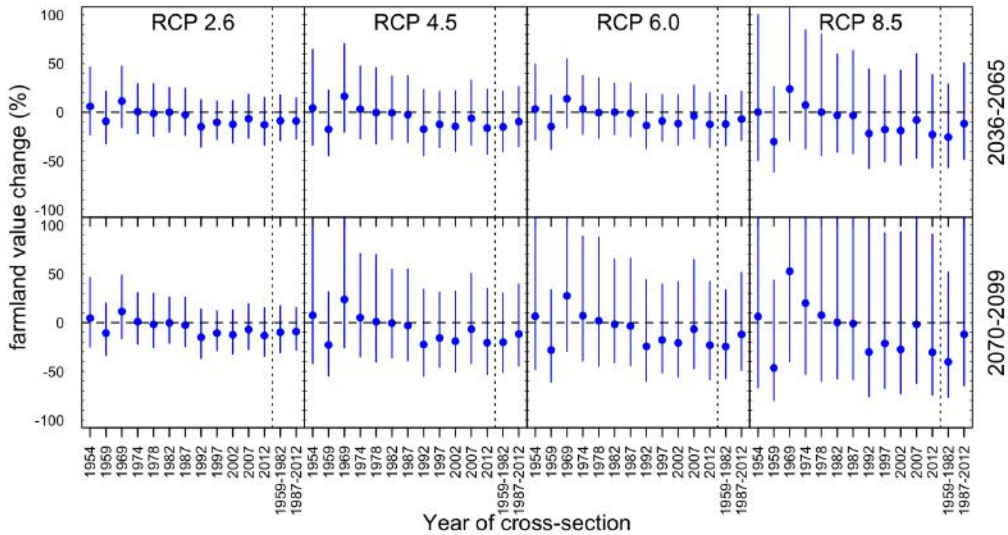


Figure 11: Climate Change Impacts For Benchmark Model Based on A Uniform Scenario



Notes: The model is based on a first-order neighborhood with equal weights. Climate change projections correspond to the HadGEM2-ES climate model.

Figure 12: Climate Change Impacts Based on Direct Pooled Climate Variation

4 Variations of Climate Change Impacts for Preferred Model

4.1 Pooled Specification

Climate change impact results for the proposed model presented in the paper correspond to a direct model with state fixed effects. I present the results based on a pooled specification in figure 12. Results are very similar to those shown in the paper.

4.2 Stability from Control Variable Omission

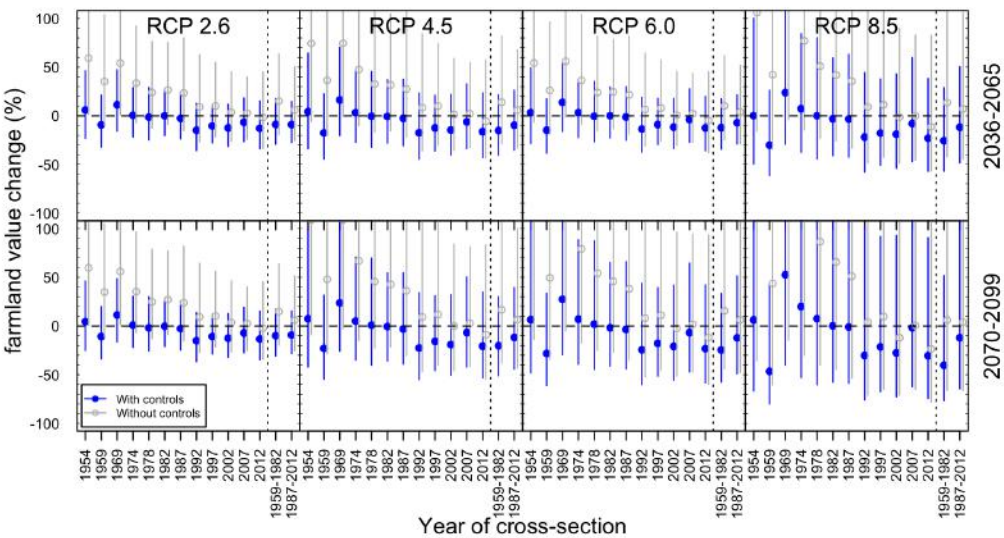
Similarly to the benchmark model, I examine the stability of climate change impacts estimates based on the preferred local model when omitting control variables. Figure 13 shows the preferred climate change impacts reported in the paper together with climate change impacts based on local models that omit control variables. Estimates without control variables tend to be more positive but remain statistically similar to the main results presented in the paper.

4.3 Alternative Neighborhood Definitions

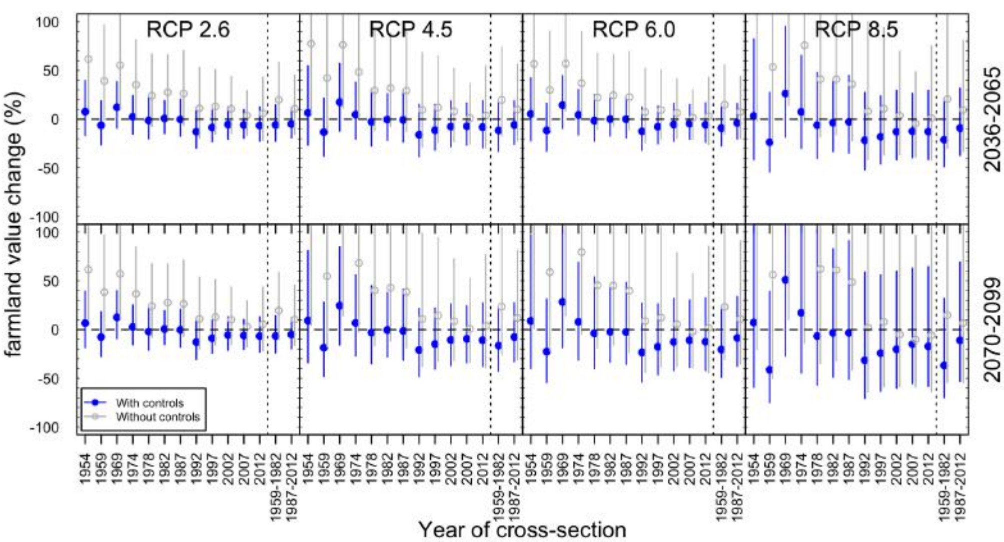
In the paper I rely almost exclusive on first-order neighborhood definitions. This choice was guided by evidence suggesting that non-climate-related (climate-related) observables were uncorrelated (correlated) with climate variables *conditional* on first-order neighborhood characteristics. Unfortunately, the evidence also suggested that incidental correlations were present when higher order neighborhoods were considered.

Figure 14 shows climate change impact estimates based on the proposed models but with increasingly expansive neighborhoods (second to fourth-order neighborhoods). The second-order estimates are very similar to the first-order estimates presented in figure 12 as they cannot be distinguished from zero. However, the third and especially the fourth-order proposed estimates are increasingly negative and significant for *recent* cross-sections, resembling the time profile of the impact estimates of benchmark models. This pattern

A. Pooled



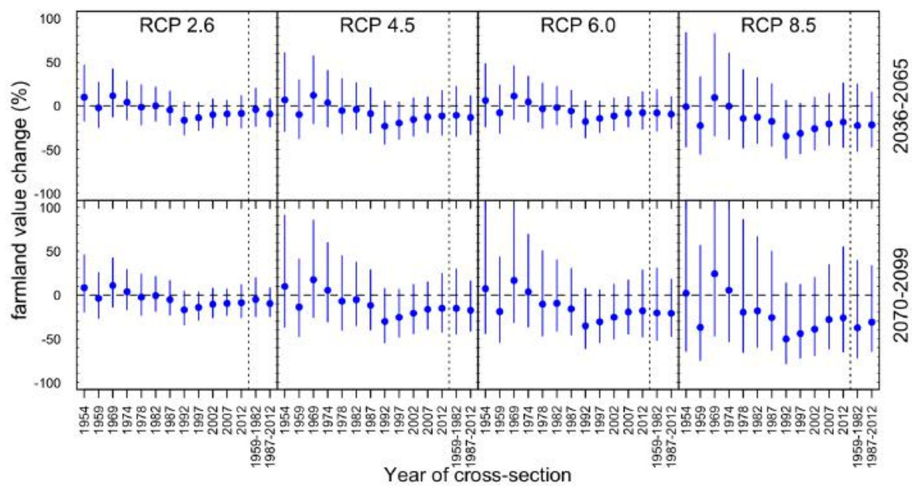
B. State Fixed Effects



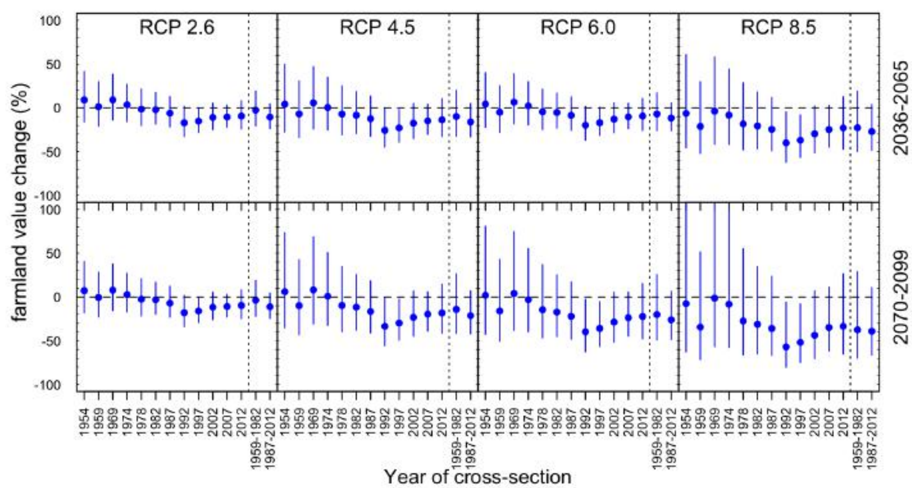
Notes: Results correspond to the climate model used in the paper (HadGEM2-ES).

Figure 13: Climate Change Impacts For Preferred Local Models With and Without Control Variables

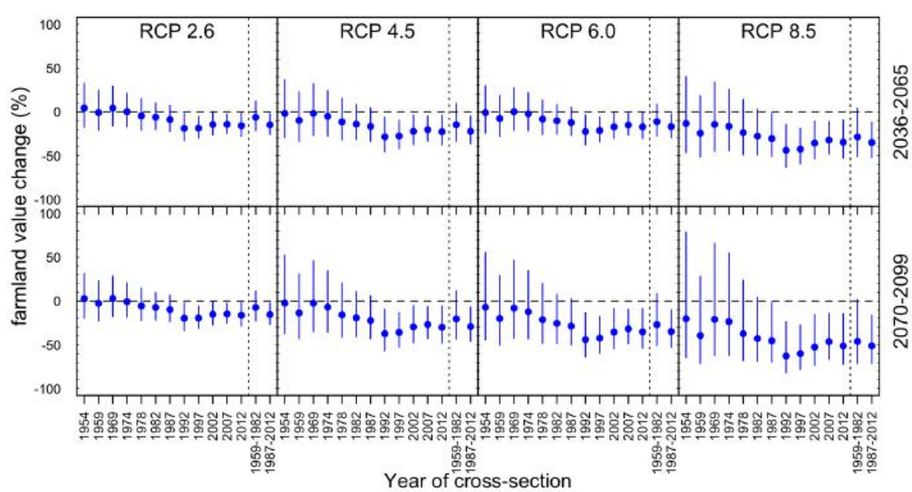
A. Second-Order Neighborhood



B. Third-Order Neighborhood

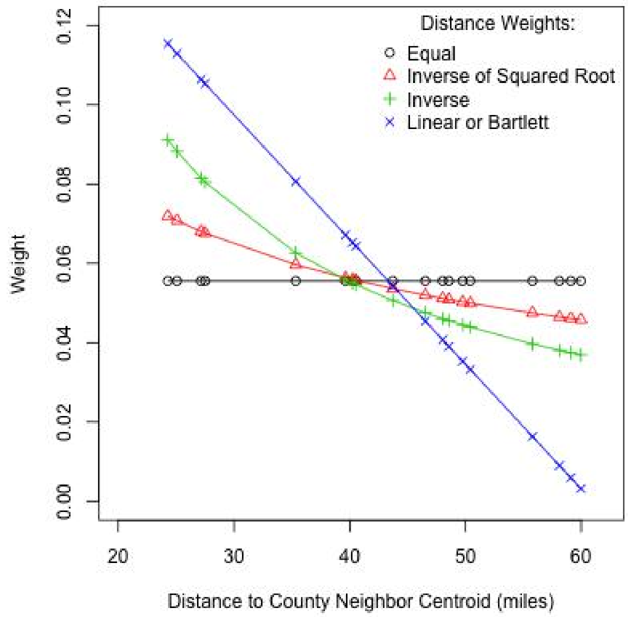


C. Fourth-Order Neighborhood



Notes: All proposed models assume equal weights.

Figure 14: Climate Change Impacts Based on Neighborhood Conditional Climate Variation and Varying Neighborhood Definitions with State Fixed Effects



Notes: The weights correspond to the second-order neighborhood of Christian county, IL. Each point corresponds to one of its 18 second-order county neighbors. For relatively small counties with close-by neighbors, the inverse distance scheme typically gives relatively greater weights to close neighbors than the Bartlett weights.

Figure 15: Climate Change Impacts Based on Direct Climate Variation and Varying Neighborhood Definitions

matches simulation predictions in section 2 of this appendix and confirms the presence of a spatial confounder which appears to gain influence toward more recent cross-sections.

4.4 Alternative Neighboring Weights

In the paper I relied on equal weights for each neighboring county. Here I show results for alternative weighting schemes illustrated in figure 15 for the second-order neighborhood of a select county in the sample. These weights include, equal, inverse of the squared root of the distance, inverse of the distance and linear or Bartlett weights.[6] I compute climate change impacts on US farmland values for the proposed direct models based on these weighting schemes. Results are presented in figure 16 for the pooled model and show that climate change impact estimates are fairly insensitive to the choice of neighborhood weights.[7]

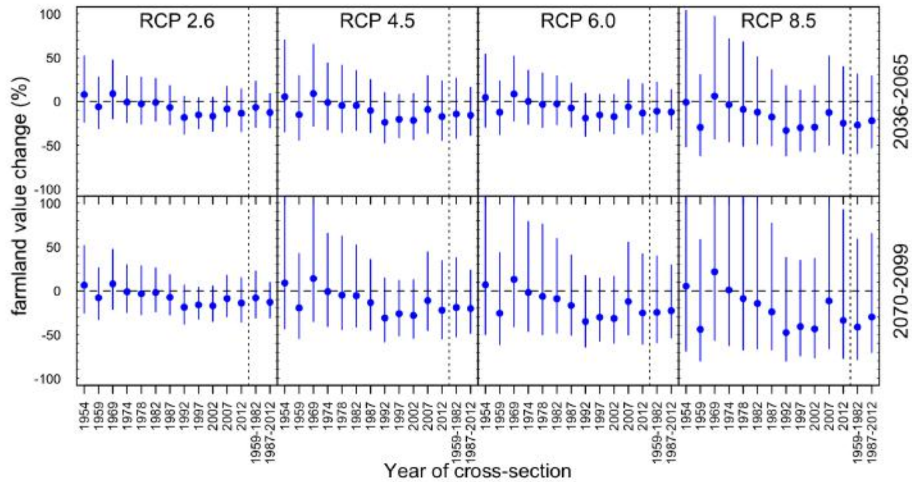
4.5 Alternative GMM Estimator

In this section I present climate change impact estimates on farmland values based on the preferred "local" model estimated via the spatial error model GMM estimator developed by Kelejian and Prucha (1999). Results are presented in figure 17. Again, this estimator is more efficient than least squares estimators but assumes a certain structure of error dependence. The error weight matrix that capture the spatial dependence structure assumes a first-order neighborhood relationship with equal weights (the same structure

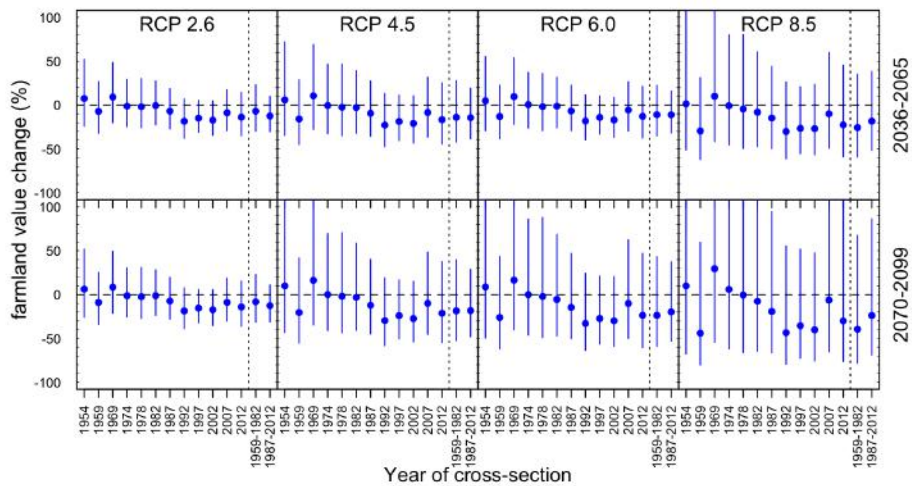
[6] The latter simply represents a weighting scheme that linearly decreases until it reaches 0 for a cutoff distance. Here the cutoff distance is the maximum distance (in miles) to the farthest neighbor plus 1 mile.

[7] It appears that the choice of the size of the neighborhood is a more vital modeling choice than neighborhood weighting schemes.

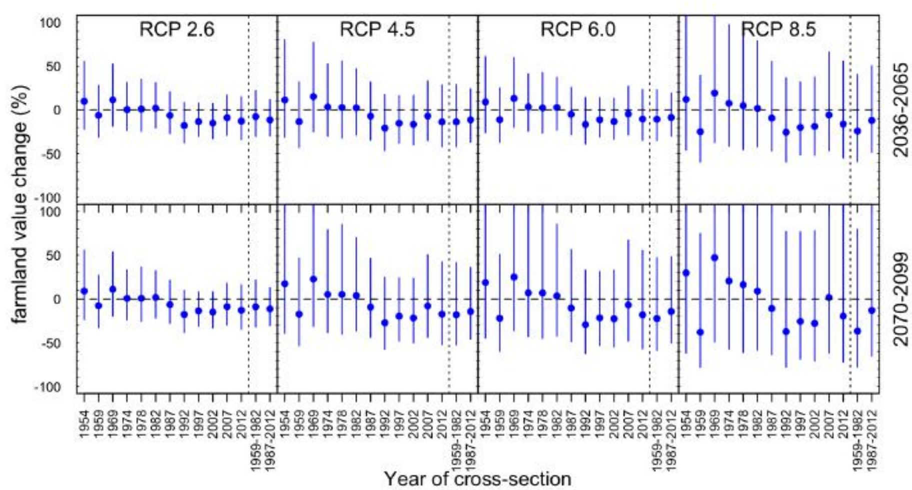
A. Inverse of the Squared Root of the Distance Weights



B. Inverse Distance Weights



C. Linear or Bartlett Weights



Notes: All proposed models adopt a second-order neighborhood definition.

Figure 16: Climate Change Impacts Based on Pooled Direct Climate Variation and Varying Neighborhood Definitions

used to construct the neighborhood-average controls). Results are very similar for other weighting schemes, including inverse distance, inverse of the squared root of the distance and Bartlett or linear weights. The point estimates are negative under this estimator for relatively recent cross-sections. However, these are largely statistically insignificant.

4.6 Alternative General Circulation Models (GCM)

Figures 18 through 22 present climate change impacts for the preferred model in the paper (first-order neighborhood with equal weights) based on the 4 aforementioned General Circulation Models (GCMs) as well as a uniform warming scenario of 5^{o}F and an increase of 8% in precipitation.

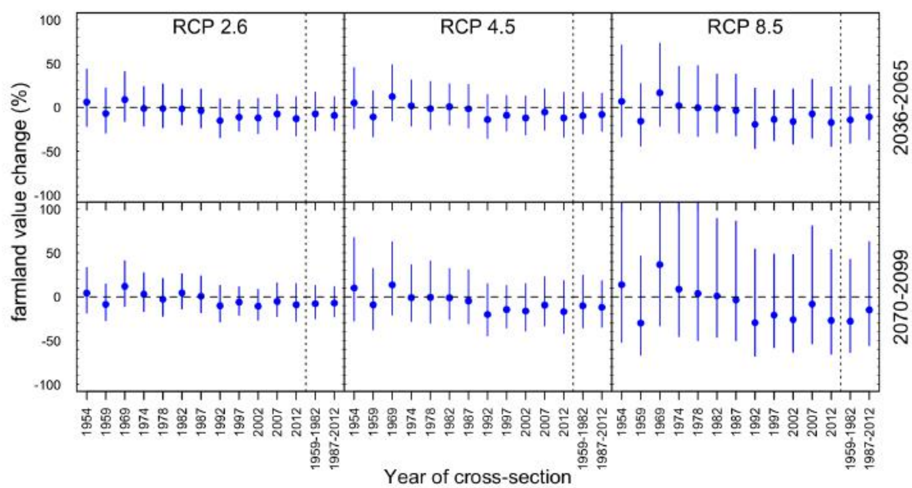


Figure 18: Climate Change Impacts For Preferred Model Based on the CanESM2

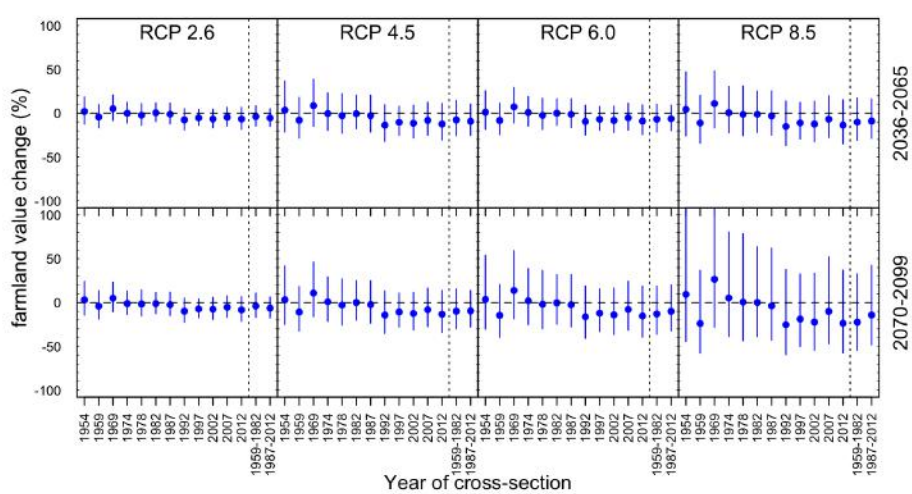
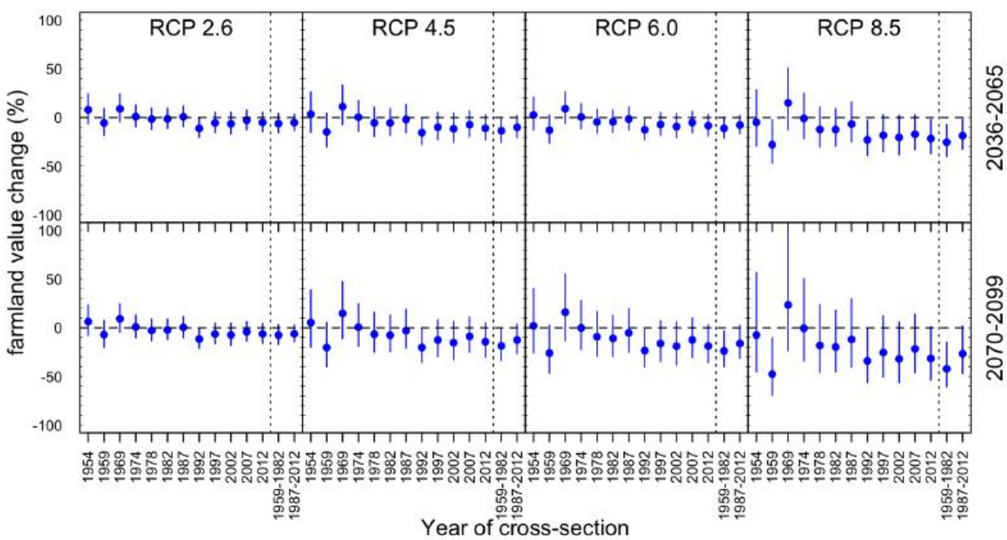
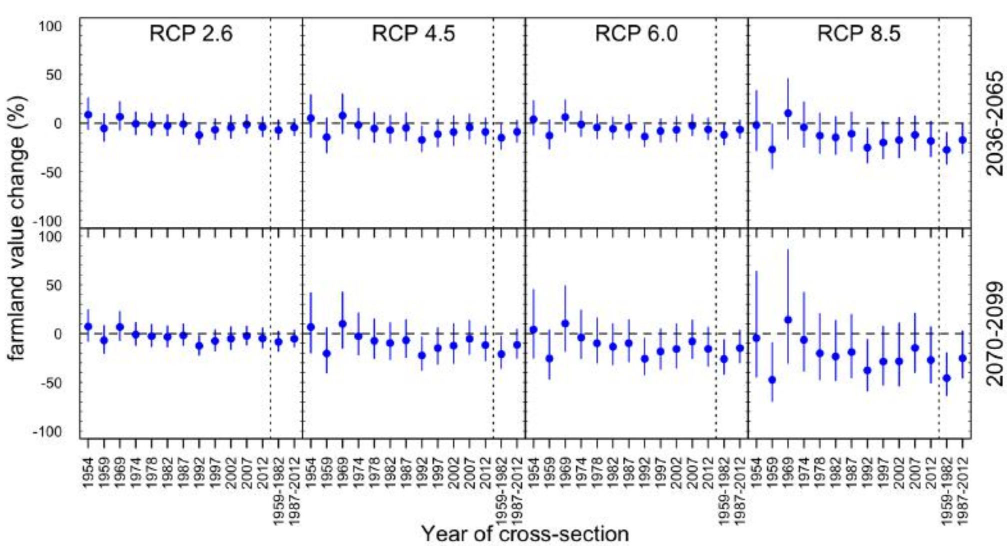


Figure 19: Climate Change Impacts For Preferred Model Based on the CCSM4

A. Pooled



B. State Fixed Effects



Notes: Results correspond to the climate model used in the paper (HadGEM2-ES).

Figure 17: Climate Change Impacts For Preferred Model Based on a Spatial GMM Estimator

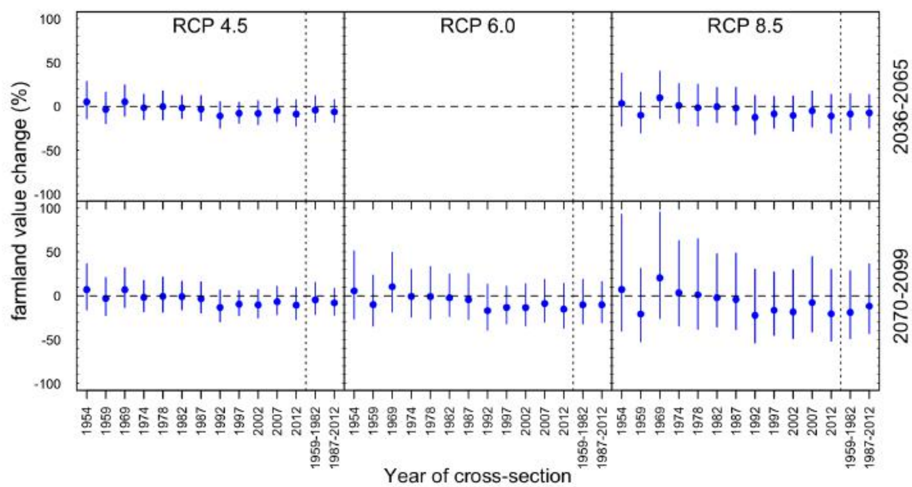


Figure 20: Climate Change Impacts For Preferred Model Based on the GFDL-ESM2M

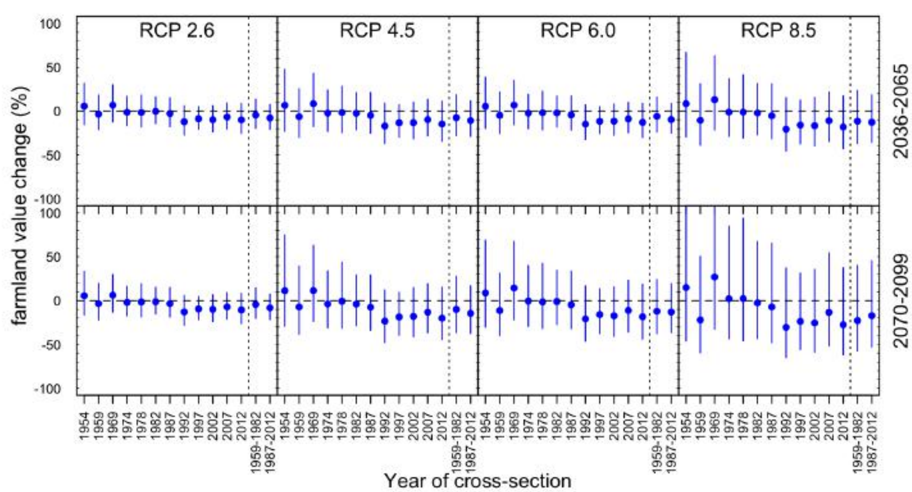


Figure 21: Climate Change Impacts For Preferred Model Based on the NorESM1-M

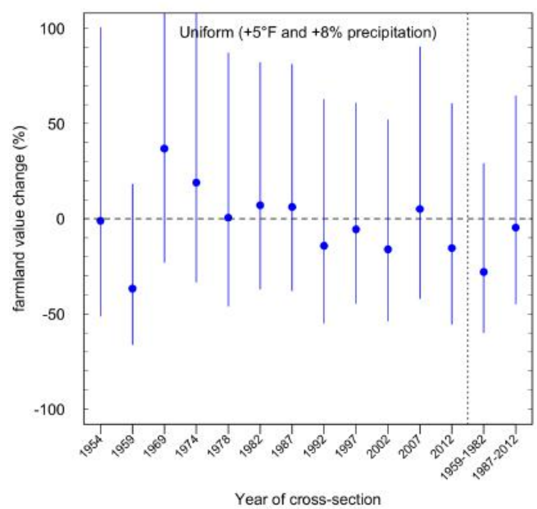


Figure 22: Climate Change Impacts For Preferred Model Based on A Uniform Scenario

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