The Role of Nonfarm Influences in Ricardian Estimates of Climate Change Impacts on US Agriculture

Ariel Ortiz-Bobea

The Ricardian approach is a popular hedonic method for analyzing climate change impacts on agriculture. The approach typically relies on a cross-sectional regression of farmland asset prices on fixed climate variables, making it particularly vulnerable to omitted variables. I conduct a long-spanning Ricardian analysis of farmland prices in the eastern United States (1950–2012) and find a convergence of evidence indicating that large estimates of climate change damages for recent cross-sections (>1970s), also found in the literature, can be explained by the growing influence of omitted factors extraneous to the agricultural sector. I propose and evaluate a simple strategy to circumvent such nonfarm influences in the form of a Ricardian model based on cash rents (2009–2016), which better reflect agricultural profitability and do not capitalize expected land use changes. The new damage estimates on nonirrigated cropland and pasture rents are more optimistic and cannot be distinguished from zero. However, estimates remain imprecise under extreme climate change scenarios pointing to a cautionary long-term outlook for United States agriculture. The findings are robust to multiple checks and alternative explanations.

Key words: Climate change, agriculture, Ricardian approach, omitted-variable bias, nonfarm influences, farmland real estate, cash rents.

JEL codes: Q15, Q51, Q54.

Over two decades ago, Mendelsohn, Nordhaus, and Shaw (1994) introduced the so-called Ricardian approach, an innovative hedonic method and the first econometric attempt to estimate the economic impacts of climate change on US agriculture. The approach consists of a cross-sectional regression of county-level farmland prices on climate and other control variables. Because farmers maximize utility subject to a fixed local climate, the estimated climate parameters are interpreted as shadow values. These estimates are subsequently used to project long-run welfare impacts on the agricultural sector under climate change, accounting for the full range of available farmer adaptations under current prices and technology. The approach has generated considerable criticism (e.g., Cline 1996; Kaufmann 1998; Darwin 1999; Quiggin and Horowitz 1999; Deschênes and Greenstone 2007) but remains a popular approach for climate change impact assessment

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on agriculture (e.g., Liu et al. 2004; Schlenker, Hanemann, and Fisher 2005, 2006; Timmins 2005; Seo and Mendelsohn 2008a, 2008b; Lippert, Krimly, and Aurbacher 2009; Van Passel, Massetti, and Mendelsohn 2017).

Perhaps the preeminent shortcoming of the Ricardian approach is its vulnerability to omitted-variable bias. The approach fundamentally exploits the "between" variation of a panel, thus unobserved determinants of farmland prices that correlate with climate and do not vary—or vary slowly—over time cause bias in unknown direction. In this regard, the growing capitalization on US farmland prices of nonfarm influences related to land use change is potentially concerning, particularly over the last few decades (Hardie, Narayan, and Gardner 2001; Plantinga, Lubowski, and Stavins 2002; Livanis et al. 2006). In fact, recent hedonic evidence indicates that Americans are willing to pay more to live in locations with milder climates (Albouy et al. 2016), suggesting that nonfarm spillovers on farmland markets could be correlated with climate.

Previous Ricardian studies have attempted to account specifically for urban pressure by including control variables (e.g., population density or growth, income per capita, housing prices or the percentage of county urbanization; see Mendelsohn, Nordhaus, and Shaw 1994; Mendelsohn and Dinar 2003; Schlenker, Hanemann, and Fisher 2005; Massetti and Mendelsohn 2011) or by excluding urban counties from the sample (Schlenker, Hanemann, and Fisher 2006). However, alternative nonfarm land uses can take other forms including rural and exurban development, vacation and second homes, hobby or "gentlemen" farms, or special-use areas such as parks or wilderness areas (Nickerson et al. 2011). These can influence farmland prices outside urban and densely-populated areas (Heimlich and Anderson 2001; Bastian et al. 2002; Borchers, Ifft, and Kuethe 2014), making them particularly difficult to account for with standard socioeconomic controls.

The goal of this article is to detect and circumvent the potential presence of nonfarm omitted variables in the Ricardian approach. This contributes to a growing number of methodological improvements of the Ricardian approach, including accounting for irrigation (Schlenker, Hanemann, and Fisher 2005), introducing better-fitting climate variables and accounting for spatial error dependence (e.g., Schlenker, Hanemann, and Fisher 2006; Hendricks 2018), accounting for endogenous land use and adjustment costs (Timmins 2005), accounting for biases from spatial aggregation (Fezzi and Bateman 2015) and introducing forward-looking behavior to account for the capitalization of climate projections (Severen, Costello, and Deschênes 2018).

This article proposes a simple conceptual framework to interpret a wide body of evidence to indirectly verify the presence of nonfarm omitted variables and devise an empirical strategy to avoid associated biases in the Ricardian approach. In particular, the article proposes the use of Ricardian models based on farmland rental prices rather than asset prices to circumvent such biases. A convergence of empirical evidence supports this choice. This relates to the fundamental but testable hypothesis that land asset prices fully capitalize expectations of changing benefits from nonfarm land uses in a land market in disequilibrium, whereas farm rental prices generally do not.

The core of the article consists of two complementary Ricardian analyses; one based on a long panel of farmland prices from the US Census of Agriculture (1950–2012), the other based on a shorter and recent panel from annual surveys of cropland and pasture cash rents (2009–2016). To the best of my knowledge, this constitutes the longest-spanning Ricardian study to date.[1] In both cases I estimate separate Ricardian models for each cross-section, which introduces some inefficiency (see Massetti and Mendelsohn 2011) but allows analyzing the evolution of Ricardian estimates over time without imposing structure.[2] Results from both exercises are analyzed and contrasted in light of predictions derived from the theory.

In summary, a collection of evidence suggests that nonfarm influences introduce a

[1] For instance, Mendelsohn, Nordhaus, and Shaw (1994) considers two cross-sections (1978 and 1982), Schlenker, Hanemann, and Fisher (2005) considers five cross-sections (1982, 1987, 1992, 1997, and 2002), and Schlenker, Hanemann, and Fisher (2006) reports results for four cross-sections (1982, 1987, 1992, and 1997). Fisher et al. (2012) reports estimates based on six cross-sections (1969, 1974, 1978, 1982, 1997, and 2002) in its online appendix. More recently, Severen, Costello, and Deschenes (2018) consider one cross-section (2007). This study reports results based on fourteen and seven cross-sections of farmland prices and rental prices, respectively.

[2] I also consider a Ricardian model based on a "between" panel estimator harnessing all cross-sections and allowing hypothesis testing across time periods.

downward bias in Ricardian models based on farmland prices, particularly since the 1970s. Thus, the severe climate change damages stemming from the Ricardian approach for recent farmland prices appear overstated. In contrast, alternative models based on recent cash rents appear to circumvent nonfarm influences. These, however, point to climate change damages that are indistinguishable from zero although these remain imprecisely estimated, particularly for extreme climate scenarios. Because theoretical and empirical considerations support the rental model, the proposed strategy makes a clear practical difference. I consider but ultimately rule out alternative explanations of these findings, including measurement error in climate variables, climate-biased technological change, the capitalization of farm subsidies, and the profitability of peri-urban agriculture.

This article makes three main contributions. First, it provides evidence of nonfarm influences on Ricardian estimates based on recent US farmland prices. This confirms a long-standing concern in the literature. Second, the article proposes a simple strategy to implicitly account for nonfarm pressures in the Ricardian approach. The idea is to adopt dependent variables that mostly reflect expected near-term agricultural profits. To my knowledge, only Hendricks (2018) has recently implemented this strategy in the US context. In addition, Ricardian estimates for subcategories of farmland provide new insights regarding the climatic sensitivity of different types of agricultural production.

Finally, the article contributes to the overall debate of climate change impacts on US agriculture by reconciling estimates of climate change impacts from approaches that account for varying degrees of farmer adaptation. Basic production theory tells us that long-run effects of climate change (with full adaptation) should be more optimistic than short-run effects (with limited adaptation). Therefore the relatively small long-run effects of climate change found in this article with a Ricardian approach are internally consistent with negative and large short-run estimates of weather shocks found on panel studies of US crop yields (e.g., Schlenker and Roberts 2009), farm profits (Deschênes and Greenstone 2012; Fisher et al. 2012), or total factor productivity (Ortiz-Bobea, Knippenberg, and Chambers 2018). Because the Ricardian approach does not account for the cost of adaptation and the new damage estimates remain imprecise for extreme scenarios, the findings cannot rule out sizable damages on US agriculture under extreme climate change.

The rest of the article is structured as follows. I first provide the conceptual framework of the article. I then present the data, which I follow with a preliminary analysis. I subsequently provide empirical results for Ricardian models based on farmland asset and rental prices, which I later reconcile while considering limitations and alternative interpretations of the findings. I then conclude.

Conceptual Framework

The Model

There is a well-established literature exploring the various drivers of farmland prices (see Just and Miranowski 1993 for an overview), including the role of nonfarm factors (Capozza and Helsley 1989; Hardie, Narayan, and Gardner 2001; Plantinga and Miller 2001; Plantinga, Lubowski, and Stavins 2002; Livanis et al. 2006). I build on this previous work to clarify how rising nonfarm influences can affect Ricardian estimates of climate change impacts on the agricultural sector over time. Consider the agricultural producer's per-acre profit-maximizing problem:

$$(1) \quad \pi(\mathbf{p}, \mathbf{w}, \mathbf{z}, \mathbf{u}) = \max_{\mathbf{x}} \{ \mathbf{p} \cdot f(\mathbf{x}, \mathbf{z}, \mathbf{u}) - \mathbf{w} \cdot \mathbf{x} - R^F(\mathbf{p}, \mathbf{w}, \mathbf{z}, \mathbf{u}) \}$$

where $\mathbf{p}$ and $\mathbf{w}$ are output and input prices, $\mathbf{z}$ is local climate, $\mathbf{u}$ are fixed land characteristics (e.g., soil qualities), f is a production function, and R^F is the equilibrium rental price of land for farming purposes.

Individual producers are assumed to have no market power and thus cannot affect R^F. Free entry drives producer profit to zero as farmers bid up the land rental price. This equilibrium implies that $R^F(\mathbf{p}, \mathbf{w}, \mathbf{z}, \mathbf{u}) = \max_{\mathbf{x}} \{ \mathbf{p} \cdot f(\mathbf{x}, \mathbf{z}, \mathbf{u}) - \mathbf{w} \cdot \mathbf{x} \} = \pi^L$, where π^L is the agricultural profit without accounting for land rental costs. This reflects the "leftover" principle in urban economics whereby producer profit is captured by the landowner through higher rents. Let $B^F = R^F - C$ be the annual net benefit of renting the land for farming, where C is a per-acre cost of managing or maintaining the land. In general, farm benefits $B^F(s, \mathbf{z}, \mathbf{u})$ can be expressed as a function of time s, climate $\mathbf{z}$ and other nonclimatic drivers $\mathbf{u}$.

Allocating land to an alternative nonfarm use also generates annual benefits $B^N(s, \mathbf{z}, \mathbf{v})$ that also vary over time and are affected by climate and other non-climatic drivers $\mathbf{v}$.

In a static spatial equilibrium benefits do not change over time, leading to fixed farm and a nonfarm regions. In the farm region $B^F > B^N$, whereas $B^F < B^N$ in the nonfarm region. At the frontier between regions, B^F and B^N are equalized. In this context the value of land allocated to farm use, or farmland, is simply the future discounted stream of farm benefits $V_t(\mathbf{z}, \mathbf{u}) = \sum_{s=t}^{\infty} B^F(s, \mathbf{z}, \mathbf{u})/(1+r)^s$, where r is a discount rate.

However, land markets are not in a spatial equilibrium. The value of farmland is better described by a dynamic model where B^F and particularly B^N may be rising over time. Conversion to nonfarm use occurs at a time period t^* when $B^N > B^F$ (assuming no conversion costs). If land use change is irreversible, the value of farmland at time period t becomes:

$$(2) \quad V_t(t^*, \mathbf{z}, \mathbf{u}, \mathbf{v}) = E\left[\sum_{s=t}^{t^*} \frac{B^F(s, \mathbf{z}, \mathbf{u})}{(1+r)^s} + \sum_{s=t^*}^{\infty} \frac{B^N(s, \mathbf{z}, \mathbf{v})}{(1+r)^s}\right]$$

Thus, the value of land that is currently allocated to farm use reflects the future benefits of nonfarm use, even though farm benefits currently exceed nonfarm benefits. Equation (2) can be expressed more simply as a weighted average of farm and nonfarm components, where I omit the discount rate for clarity:

$$(3) \quad V_t(t^*, \mathbf{z}, \mathbf{u}, \mathbf{v}) = a(t^*) V_t^F(t^*, \mathbf{z}, \mathbf{u}) + [1 - a(t^*)] V_{t^*}^N(\infty, \mathbf{z}, \mathbf{v})$$

When land use change is distant ($t^* \to \infty$) we have $a(t^*) \to 1$ so that $V_t(t^*, \mathbf{z}, \mathbf{u}, \mathbf{v})$ converges to $E[\sum_{s=t}^{\infty} B^F(s, \mathbf{z}, \mathbf{u})/(1+r)^s]$. When land use change is imminent ($t^* \to t$) we have $a(t^*) \to 0$, so that $V_t(t^*, \mathbf{z}, \mathbf{u}, \mathbf{v})$ converges to $E[\sum_{s=t}^{\infty} B^N(s, \mathbf{z}, \mathbf{v})/(1+r)^s]$.

This phenomenon operates at the field level but is naturally reflected in aggregate county-level cross-sections at each point in time. Counties where B^N is growing more rapidly exhibit average farmland valuations with a nonfarm premium compared to counties where B^N is not growing as fast.

As a result, the shadow value of climate $dV_t/d\mathbf{z}$ converges to $dV_t^F/d\mathbf{z}$ when farmland conversions are distant, whereas it converges to $dV_t^N/d\mathbf{z}$ when farmland conversions are imminent. In reality, counties where land use conversions are either distant or imminent coexist in the same cross-section, meaning that Ricardian estimates may be influenced by how nonfarm benefits respond to climatic variations.

Key Predictions

This simple framework yields several useful predictions that help guide and interpret the empirical investigation. First, Ricardian estimates based on farmland asset prices and rental prices should be similar in the absence of nonfarm influences. When $t^* \to \infty$, $dC/d\mathbf{z} = 0$ and $dR^F/dt = 0$, one obtains $d\ln V_t/d\mathbf{z} = d\ln R_t^F/d\mathbf{z}$. Thus climate parameters of semi-log models of farmland prices and farmland rents have analogous economic interpretations. A corollary is that Ricardian estimates based on farmland prices and rents should diverge when land use conversion are imminent over substantial parts of the cross-section (provided $dB^F/d\mathbf{z} \neq dB^N/d\mathbf{z}$). Empirically, this can be tested by contrasting climate coefficients or climate change impacts from Ricardian models based on contemporaneous land asset and rental prices.

Second, as t^* decreases and nonfarm premium on farmland prices grow over some parts of the sample, one should observe $d\ln V/d\mathbf{u}$ attenuate under the presumption that $dB^N/d\mathbf{u} \approx 0$. That is, farmland prices should appear increasingly unrelated or inelastic to changes in non-climatic determinants of agricultural productivity embodied in $\mathbf{u}$ (e.g., soil quality). Similarly, in the presence of nonfarm influences one should expect $d\ln R^F/d\mathbf{v}$ to be small relative to $d\ln V/d\mathbf{v}$, because farm rents should not be affected, at least in this simple model, by characteristics that do not enter the producer's maximization problem. This can be tested by analyzing the evolution of regression coefficients over time for different farmland price cross-sections and by contrasting coefficients based on contemporaneous land asset and rental prices.

Third, Ricardian estimates based on farmland asset prices should change over time as nonfarm influences grow. Empirically, this can be assessed by observing whether $d\ln V/d\mathbf{z}$ or climate change impacts change as one considers different cross-sections of land asset prices over time. Note that changing Ricardian estimates over time is not, in itself, a proof of omitted variable-bias stemming from nonfarm pressures. Such pattern could be explained, for instance, by other factors such as technological change. Thus, a wider body of evidence is needed to reach such conclusions.

Fourth, technological change affects f, so it does not fundamentally alter the relationship between R^F and V.[3] Thus if $d\ln V/d\mathbf{z}$ evolves over time due to changes in the technology, then equivalent changes in $d\ln R^F/d\mathbf{z}$ should be reflected in the absence of nonfarm influences. Similarly, agricultural subsidies increase π^F and do not fundamentally alter the relationship between R^F and V in this model.

Fifth, the presence of nonfarm influences on farmland prices can be detected by inspecting the well-known price-to-rent ratio V/R^F. When $t^* \to \infty$, the ratio converges to $1/r$, whereas it exceeds $1/r$ when $t^* \to t$. Ratios that exceed $1/r$ in a sustained manner are typically interpreted as indicating the growing influence of nonfarm factors (Flanders, White, and Escalante 2004; Nickerson et al. 2012). Because farmland competes with other safe assets in the economy, the discount rate generally tracks the market interest rate (e.g., ten-year Treasury note) (Burns et al. 2018). Thus with recent interest rates in the $2-4\%$ range, ratios exceeding thirty appear suspect.

Finally, note that as r decreases in tandem with market interest rates, nonfarm influences play an increasingly larger role in farmland valuation for a given t^*. Interest rates have been declining since the early 1980s, which should exacerbate the influence of nonfarm pressures in recent decades.

Empirical Considerations

The Ricardian approach typically relies on the cross-sectional variation in farmland asset prices and climate across locations to estimate the total marginal effect of climate on farmland value $dV(\mathbf{z}, \mathbf{u}, \mathbf{v}, t^*)/d\mathbf{z}$, where r is omitted for clarity. While the econometrician seeks to estimate the direct farm effect or shadow value of climate associated with $\partial B^F/\partial \mathbf{z}$, only the reported asset price of farmland is typically relied upon and $dV/d\mathbf{z}$ is estimated instead. This effect is subsequently used to compute welfare changes on the sector from potential changes in climate assuming current prices and technology.

The empirical challenges are clear from equations (2) and (3). First, agricultural unobservables $\mathbf{u}$ may be correlated with climate $\mathbf{z}$ and cause bias when elements of $\mathbf{u}$ are omitted or mismeasured. This indirect farm effect is difficult to circumvent and motivates the inclusion of agricultural controls such as soil characteristics or state dummies in the Ricardian model.

Second, a direct nonfarm effect on farmland prices may also introduce bias. For instance, population may be sorting into certain climates which would accelerate the conversion of farmland to non-farm use and increase non-farm benefits. This drives up the price of surrounding farmland, conflating the direct climate effects on farm and nonfarm values.

Third, an indirect nonfarm effect can also introduce bias if non-climatic omitted determinants of nonfarm values $\mathbf{v}$ correlate with climate $\mathbf{z}$. For instance, mountainous areas and coastal towns tend to have distinctive climates and at the same time provide non-climatic amenities, such as the presence of wilderness areas or water bodies, which generate nonfarm value. The latter two points are also difficult to circumvent and have motivated the use of socioeconomic controls such as population density or income per capita.

This article considers the possibility that rising nonfarm pressures may be biasing Ricardian estimates of climate change impacts on agricultural value. Conceptually, there are at least three ways to reduce or circumvent biases from nonfarm effects.

First, one could simply include better controls for nonfarm influences. For instance, Plantinga, Lubowski, and Stavins (2002) develop measures of population change to represent the changing demand for developable on farmland markets. While adopting such measures would improve current practices in this literature, such demand-side measures do not capture the spatial heterogeneity of the elasticity of supply of land which is critical for

[3] Unless some dramatic technological change is expected in the medium term which would be reflected in V but not in R^F.

modeling land price changes (Saiz 2010). Thus I do not consider this strategy in this article and rely on controls commonly used in this literature.

The second strategy consists in relying on older farmland price cross-sections when nonfarm pressures were likely less pronounced (larger t^*). This reduces bias if omitted nonfarm influences are increasingly correlated with climate over time. The downside of this strategy is that relying on distant farmland prices may not necessarily reflect the current agricultural sensitivity to climate.

Finally, the third strategy consists in relying directly on farmland rents R^F or expected agricultural profit π^L. This strategy reduces vulnerability to nonfarm omitted variables, although it cannot circumvent the influence of indirect farm effects identified above. However, relying on rental price could circumvent a subset of indirect farm effects in the US context. The reason is that data on US farmland asset prices incorporate the value of both land and buildings (e.g., farmer's dwelling, greenhouses, barns, dairy facilities, etc.), whereas rental prices only incorporate the land component. If the presence or value of farm buildings is correlated with climate, which is plausible, this could further distort Ricardian estimates based on farmland asset prices. In terms of caveats, rental prices are likely more susceptible to input and output price swings than farmland asset prices, so cash rent estimates could be noisier.

Finally, note that in general R^F is not fully "shielded" from nonfarm influences. It is possible, for instance, that certain elements of $\mathbf{v}$ (e.g., proximity to city) may improve access to markets and increase farmer profit π^L, thus affecting R^F. This could be reflected through either $\mathbf{p}$ or $\mathbf{w}$ in the producer's problem. I do not model this explicitly, but I later explore this possibility. In addition, rental contracts are assumed to be short-lived, ensuring that land does not become vacant prior to the land use conversion. With longer and rigid contractual terms, agricultural producers would need to compensate landowners for the opportunity cost of renting them the land rather than fetching a higher nonfarm rental price. However, farmers are assumed to have identical abilities so they would not be willing to pay more than R^F, which is fundamentally dependent on expected profit π^L. That is, R^F reflects the agricultural opportunity cost of the land. If a landowner increases the rent above R^F, π^L would become negative and farmers would not rent the land, leaving it vacant.

Data Sources and Summary Statistics

This article relies on a long-spanning collection of agricultural, climate, soil quality, and general socioeconomic data. I rely, as much as possible, on standard data sources and variables in the literature. A detailed definition of variables used in this article is provided in the online supplementary appendix. Summary statistics are provided at the county level.

Agricultural Data

This study explores Ricardian estimates based on two competing types of dependent variables: farmland asset prices and the rental price of nonirrigated cropland and pasture. County-level *farmland value* is obtained from the US Census of Agriculture, which collects data from all farms.[4] This variable is obtained by asking farmers their estimate of the current market value of their land and buildings. As a result, the variable reflects the capitalized value of potential nonfarm land uses if local land use regulations allow (or are expected to allow) such land use conversions.

The US Department of Agriculture's (USDA) online database, *Quick Stats*, only provides Census data since 1997, so older Census data going back to 1950 are obtained from Haines (2004). Following Schlenker, Hanemann, and Fisher (2006), I restrict the analysis to non-urban counties lying east of the 100th meridian west. Urban counties are defined as those with population densities exceeding 400 inhabitant per square mile in 2012. This sample restriction aims to reduce the influence of urban development pressure and to avoid confounding warmer climates in the Western United States with irrigation. The number of nonurban eastern counties with nonmissing farmland values exceeds 2,200 in all years, which is roughly 90% of all eastern counties.

Unfortunately, the Census does not report farmland rental price per acre paid by farmers. I therefore rely on USDA's recent Cash Rent Survey, which provides mean rental

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

rates for irrigated cropland, nonirrigated cropland, and permanent pasture, for all US counties (excluding Alaska) with 20,000 acres of cropland plus pasture since 2008. The data are available annually for 2008–2014 and bi-annually starting in 2016. The target population is all farms that rent land and pay with cash in the qualifying counties. The sample farms (about 275,000 in 2016) are drawn with a stratified design to derive county-level estimates.[5] As a result, not all counties with farmland value data have rental price data.

To conduct an analysis that is comparable across models based on farmland values and cash rents, I restrict the sample to nonurban counties with complete rental data for nonirrigated cropland in 2012. Nonurban counties with complete rental price data represent roughly 80% of the set of nonurban counties with complete farmland value data. The share rises to 87.5% when accounting for the farmland area of nonurban counties in the Eastern United States. This suggests that the overlap in data availability is considerable (online supplementary figure S1). Counties with only farmland value data are either relatively small or have little crop agriculture.

Summary statistics for *farmland values* (1950–2012) are provided in table 1 for counties with complete and incomplete cropland rent data. The mean, standard deviation and minimum county-level *farmland values* remain comparable across these two samples. However, the sample restriction appears to exclude certain counties with extremely high farmland values close to urban centers such as New York City.[6]

Summary statistics for *non-irrigated cropland* and *pasture rental prices* (2009–2016) are provided in table 2.[7] A few aspects regarding these data bear mentioning. First, although county-level data prior to 2008 are unavailable, an analysis of state-level cropland rents indicates the rental price cross-sections for cropland and pasture have remained fairly stable since the 1960s in the Eastern United States (online supplementary figure S2). Second, cropland and pastures constitute close to 90% of US farmland, so an analysis based on these subsets should not drastically distort the analysis as it pertains to all farmland. In addition, crop production, and especially nonirrigated crop production, is arguably the most climate-sensitive agricultural activity. Therefore, Ricardian estimates based on nonirrigated cropland should be particularly conspicuous.

Third, the target population of the Cash Rent Survey are farmers (and land) that participate in the rental market. While the survey is stratified by county, resulting in the sampling of a wide range of climates, sampling within counties may be non-random. This may cause selection bias if the sampling incidentally truncates an unobservable determinant of cash rent in a way that is correlated with climate. I return to this point later, but it is important to highlight that the Cash Rent Survey was developed to *"determine market-based rates in administering USDA programs, such as the Conservation Reserve Program (CRP)"* as well as to be used *"by individual producers in planning for their agricultural operation or by Agricultural Extension Services or university staff in developing operating budgets for agricultural operations in their locale."* Therefore, even if the cash rent data are not representative of all cropland or pasture, these data are the US government's best comprehensive county-level estimates of the opportunity cost of the land for agricultural use.

Finally, some auxiliary analysis relies on state-level data obtained from USDA/ERS. This includes annual measures of total factor productivity (TFP) for the agricultural sector (1960–2004) and cash rents from the Agricultural Land Values Survey (1960–1994) and the more recent June Area Survey (1994–2016).

Climate Data

Climate data are obtained from two sources. The primary source is Schlenker and Roberts (2009), which provides a detailed daily gridded data set for 1950–2005 based on the interpolation of daily weather station data and monthly gridded data from the PRISM

[5] More details are provided here: https://www.nass.usda.gov/Publications/Methodology_and_Data_Quality/Cash_Rents/08_2016/rentqm16.pdf (Accessed 3/20/2019)

[6] Excluded counties are somewhat suspect. The average farmland value per acre (1987–2002) for counties with only farmland value data is $2,478 versus $2,457 for counties with complete data for both variables. These values are comparable. However, counties with only farmland value data have average net revenues per acre (1997–2012) that are 2.63 times lower than the subset of counties with complete data for both variables. This strongly indicates that there are high nonfarm (expected) land returns in these counties to rationalize these elevated farmland values.

[7] I ignore cash rents for the first year of data collection for the Cash Rent Survey, 2008, because the sample is considerably smaller than in subsequent years.

Table 1. Summary Statistics of Farmland Real Estate in Eastern Nonurban Counties

Year(s)	Farmland Value (2012 USD) in Eastern Nonurban Counties									
	With Complete Data					With Incomplete Data				
	μ	Min	Max	σ	n	μ	Min	Max	σ	n
1950	792	114	3,212	489	1,789	745	63	3,212	475	2,235
1954	909	130	4,616	584	1,787	858	66	11,179	611	2,233
1959	1,144	85	6,027	703	1,787	1,120	85	81,118	1,830	2,233
1964	1,331	237	5,940	723	1,789	1,282	204	40,428	1,093	2,236
1969	1,627	382	7,043	797	1,790	1,564	244	8,057	801	2,227
1974	2,223	438	9,604	1,018	1,790	2,161	228	9,604	1,012	2,227
1978	3,137	549	10,134	1,634	1,789	3,040	440	58,557	1,979	2,227
1982	2,548	457	8,590	1,118	1,790	2,492	457	31,317	1,285	2,233
1987	1,788	362	9,692	858	1,789	1,809	362	9,692	923	2,223
1992	1,747	299	15,428	982	1,789	1,774	262	15,490	1,065	2,226
1997	2,020	316	10,506	1,044	1,789	2,032	280	12,981	1,116	2,234
2002	2,414	313	15,309	1,294	1,790	2,479	308	64,872	1,969	2,235
2007	3,166	565	22,337	1,616	1,790	3,208	497	29,219	1,823	2,236
2012	3,609	739	16,401	1,749	1,790	3,524	512	26,659	1,848	2,237
1950–1974	1,338	285	5,204	692	1,790	1,293	185	33,314	957	2,237
1987–2012	2,457	495	14,945	1,173	1,790	2,478	377	28,938	1,400	2,237

Note: Values are provided on a per-acre basis. Counties with "complete" data are counties with both non-missing *farmland values* (varies by year) and *cropland rents* (for 2012). Counties with "incomplete" data correspond to counties with non-missing *farmland values* only. Multi-year averages ignore missing observations. *n* corresponds to the number of counties.

Table 2. Summary Statistics of Cash Rents in Eastern Non-Urban Counties

Year(s)	Cash Rent (2012 USD)									
	Nonirrigated Cropland					Permanent Pasture				
	μ	Min	Max	σ	n	μ	Min	Max	σ	n
2009	73	8	254	52	1,804	26	2	87	14	1,594
2010	73	8	255	52	1,801	24	3	116	13	1,555
2011	75	9	265	56	1,801	23	3	84	12	1,393
2012	83	8	324	66	1,790	23	4	94	13	1,335
2013	89	7	379	72	1,784	24	4	76	13	1,336
2014	89	5	312	72	1,821	24	3	82	13	1,458
2016	86	7	288	67	1,829	25	3	78	13	1,464
2009-2016	77	7	287	61	2,046	25	4	108	13	1,882

Note: Cash rent is provided on a per-acre basis. Summary statistics correspond to all nonurban Eastern counties with cash rent data. Multi-year averages ignore missing observations. *n* corresponds to the number of counties.

Climate Group.[8] Because I rely on relatively old Census data, I also rely on the monthly temperature data from PRISM which are available since 1895.[9] I aggregate the gridded climate data sets to the county level by weighting each native 2.5-mile PRISM grid by the amount of cropland it contains based on USDA's Cropland Data Layer (CDL).[10] Following the literature, climate variables are computed as the thirty-year average of yearly weather variables.

[8] PRISM is USDA's official climatological data. 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.

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

[10] The CDL provides 30 meter resolution land cover pixels. 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 based on land cover categories listed in online supplementary table S1.

The climate variables used and the definition of the growing season (April–September) follow to a large extent Schlenker, Hanemann, and Fisher (2006) and Schlenker and Roberts (2009) (online supplementary figure S3). Climate variables include season-long *normal degree-days* (10–30°C), *extreme degree-days* (>30°C), and *total precipitation*. To verify whether the findings in this article are robust to alternative climate variables, I also present results based on alternative temperature thresholds (8–32°C and >34°C) following Schlenker, Hanemann, and Fisher (2006). Results based on monthly average temperature and precipitation, following Mendelsohn, Nordhaus, and Shaw (1994), are qualitatively similar and are also reported.

Degree-day variables are constructed by assuming a temperature time path following a double sine curve passing through the minimum and maximum temperature of consecutive days within each PRISM grid cell. Table 3 provides summary statistics for all variables corresponding to the 1976–2005 climatology. The sample-wide statistics remain comparable across eastern non-urban counties with complete and incomplete rental price data for nonirrigated cropland.

Stability of Climate over Time

It is important to highlight that although climate has been changing over time, the *cross-sectional* variation in climate has remained fairly stable over the past century. This is important because it is the cross-sectional or "between" variation in climate that is relied upon for identification in the Ricardian framework. Table 4 shows that past climate cross-sections are highly correlated to the recent climate (1976–2005). Unfortunately, detailed daily weather data are not available prior to 1950, so computing this correlation for degree-day variables is not feasible. However, the high correlation for monthly climate variables constructed from the PRISM data show that this stability extends to the early twentieth century. This is particularly true for temperature variables which play a critical role in this literature. This stability explains why Ricardian estimates remain virtually unchanged when climate cross-sections corresponding to different time periods are used in the analysis (see Schlenker, Hanemann, and Fisher 2006). As a result, I rely on the 1976–2005 climate cross-section for all regressions presented in the article.

Climate Change Data

I report climate change impacts based on various warming scenarios as projected by the Hadley GEM2-ES General Circulation Model (GCM) or HadGEM2-ES (Jones et al. 2011). The four scenarios are RCP2.6, RCP4.5, RCP6, and RCP8.5, which corresponding to additional trapped atmospheric energy of 2.6, 4.5, 6.0, and 8.5 W/m^2, respectively. 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) periods. Although not shown, the findings in the article are qualitatively similar across climate projections from four other GCMs and the uniform warming scenario considered in Mendelsohn, Nordhaus, and Shaw (1994).[11]

Soil Quality Data

Soil quality data are obtained from the USDA's Natural Resource Conservation Service (NRCS) State Soil Geographic (STATSGO) database. According to the NRCS the level of detail in this database is designed for *"broad planning and management uses covering state, regional, and multistate areas."* The database is based on the interpretation of hundreds of soil surveys conducted throughout the nation since the 1950s. Similar to climate data, county-level soil quality data are constructed by weighting each soil polygon by the amount of cropland it contains based on CDL cropland weights. The choice of soil quality variables is also based on Schlenker, Hanemann, and Fisher (2006) (online supplementary figure S4). Summary statistics and definitions of individual soil quality indicators are provided in table 5 for eastern nonurban counties with both complete and incomplete rental price data.

Socioeconomic Data

Following the literature, the analysis also includes control variables, namely, *population density* and *income per capita* (online

[11] 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).

Table 3. Summary Statistics of Climate Variables in Eastern Nonurban Counties

			Eastern Nonurban Counties							
			With Complete Data ($n = 1{,}790$)				With Incomplete Data ($n = 2{,}237$)			
Variable(s)		Month(s)	μ	Min	Max	σ	μ	min	max	σ
Degree-days	8–32°C	Apr–Sep	2,458.8	1,324.6	3,669.8	500.3	2,471.5	1,135.6	3,739.8	525.0
	10–30°C	Apr–Sep	2,082.5	1,047.3	3,158.1	455.0	2,094.2	871.9	3,216.5	477.8
	>30°C	Apr–Sep	66.2	1.2	374.6	56.1	67.2	0.2	419.3	57.7
	>34°C	Apr–Sep	10.3	0.0	121	13.9	10.3	0.0	143.3	14.3
		Apr–Sep	591.4	328.3	941.3	88.8	600.3	321.5	1,036	96.8
Precipitation		Jan	69.5	9.4	169.3	39.8	73.3	9.4	169.3	40.0
(mm)		Apr	88.7	20.1	149.2	22.2	89.5	20.1	149.2	22.4
		Jul	103.2	38.4	211.3	23.8	105.3	38.4	217.6	26.0
		Oct	78.9	31.9	135.7	18.7	80.4	31.9	135.7	18.6
		Jan	−0.6	−16.4	15.8	6.4	−0.1	−16.4	19.1	6.6
Mean		Apr	12.8	3.3	24.0	4.1	12.9	2.5	24.2	4.3
temperature		Jul	24.8	18.3	30.3	2.5	24.8	16.8	30.7	2.7
(°C)		Oct	13.7	4.9	24.1	3.8	13.9	4.9	26	4.0

Note: Climate variables are county-level yearly averages over the 1976–2005 period. *Mean temperature* and *precipitation* are generated from the monthly gridded PRISM data set, while the *degree-days* variables were constructed from the daily gridded data set in Schlenker and Roberts (2009). *n* corresponds to the number of counties.

supplementary figure S4). These controls are introduced in a standard attempt to capture the influence of urban pressures on farmland. County-level population data comes from the US Census and Intercensal Estimates. These data are only available online from the Census for years 1970–2012. Prior census years were obtained from Haines (2004). Similarly, Intercensal Estimates prior to 1970 were not readily available. I therefore interpolate population between decennial censuses for each county using a natural spline.[12]

Data on per capita personal income were obtained from the Bureau of Economic Analysis. Unfortunately, these are only available for 1969–2012. I therefore use data on family income from the US Census to estimate per capita personal income for earlier time periods.[13] Similar to population, I interpolate family income between decennial censuses for each county using a natural spline prior to estimating per capita income.[14] All values in the article are expressed in 2012 USD based on the Consumer Price Index. Summary statistics for socio-economic variables are also provided in table 5.

Preliminary Evidence

Nonfarm influences could cause bias in the Ricardian approach if they affect farmland prices, are correlated with climate and remain omitted. Because such influences have likely evolved over time, the evolution of key farmland market indicators in the sample can provide some insights.[15]

Figure 1 (panel A) shows the within-state correlation over time of *farmland values* and *cash rents* with a fixed soil quality indicator in the sample. The correlation with *farmland*

[12] This approach is not aimed at capturing year-to-year fluctuations in population but at providing an approximation of the population level between census years. Results are insensitive to using the closest Census year.

[13] These variables are not directly comparable because family size varies across the country. The correlation between personal capital income and family income for 1969 (earliest overlapping year) is 0.82 for all US counties and 0.87 for counties in the eastern sample. Counties with relatively low (high) family income relative to personal income per capita do not tend to be agricultural in nature, so the proposed strategy does not appear particularly problematic.

[14] Again, this is intended at preserving the variation in income across counties.

[15] Throughout the article I follow Schlenker, Hanemann, and Fisher (2005, 2006) and restrict the sample to counties located east of the 100th meridian west to avoid the confounding effect of irrigation. Following that study, I also exclude counties with population densities exceeding 400 inhabitants per square mile (urban counties) in 2012 from the analysis.

Table 4. Correlation of Climate Normals over Time

Variable(s)		Month(s)	1916–1945	1926–1955	1936–1965	1946–1975	1956–1985	1966–1995	1976–2005
			Correlation relative to 1976–2005						
Degree-days	10–30° C	Apr–Sep	...	...	...	...	0.999	1.000	1
	8–32°C	Apr–Sep	...	...	...	...	0.999	1.000	1
	>30°C	Apr–Sep	...	...	...	...	0.995	0.999	1
	>34°C	Apr–Sep	...	...	...	...	0.991	0.997	1
		Apr–Sep	0.944	0.94	0.935	0.950	0.967	0.983	1
Precipitation		Jan	0.949	0.904	0.936	0.942	0.986	0.993	1
(mm)		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
		Jan	0.996	0.994	0.996	0.996	0.999	0.999	1
Mean		Apr	0.997	0.997	0.998	0.998	0.999	1.000	1
temperature		Jul	0.988	0.991	0.994	0.996	0.995	0.999	1
(°C)		Oct	0.997	0.997	0.996	0.997	0.998	1.000	1

Note: Climate variables are county-level averages of yearly weather over the corresponding thirty-year period. The data cover 2,510 counties lying east of the 100th meridian west. *Mean temperature* and *precipitation* are generated from the monthly gridded PRISM data set. *Degree-days* variables were constructed from the daily gridded data set in Schlenker and Roberts (2009).

Table 5. Summary Statistics of Control Variables

Variables	Eastern Nonurban Counties							
	With Complete Data ($n = 1{,}790$)				With Incomplete Data ($n = 2{,}236$)			
	M	Min	Mmax	σ	μ	min	max	σ
Soil quality controls:								
Average water capacity (fraction)	0.150	0.070	0.225	0.026	0.147	0.070	0.225	0.027
Clay content (%)	28.2	4.2	58.3	8.5	27.5	3.1	58.3	9.0
Minimum permeability (μm/sec)	6.67	0.14	53.02	5.92	7.38	0.14	98.10	7.27
K-factor of topsoil (index)	0.301	0.101	0.478	0.068	0.295	0.101	0.478	0.072
Best soil class (fraction)	0.690	0.021	0.999	0.201	0.649	0.000	0.999	0.222
Socioeconomic and other controls:								
Income per capita (thousand 2012US$)[a]	36.7	19.1	78.7	8.1	36.2	17.9	83.6	8.1
Population density (pop/mi^2)[a]	77.6	1.0	396.2	81.0	77.3	0.30	398.6	81.7
Latitude (°)	38.0	26.5	48.8	4.5	37.7	25.5	48.8	4.6

Note: 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. *n* corresponds to the number of counties.
[a]For clarity of exposition, *income per capita* and *population density* correspond to 2012 levels.

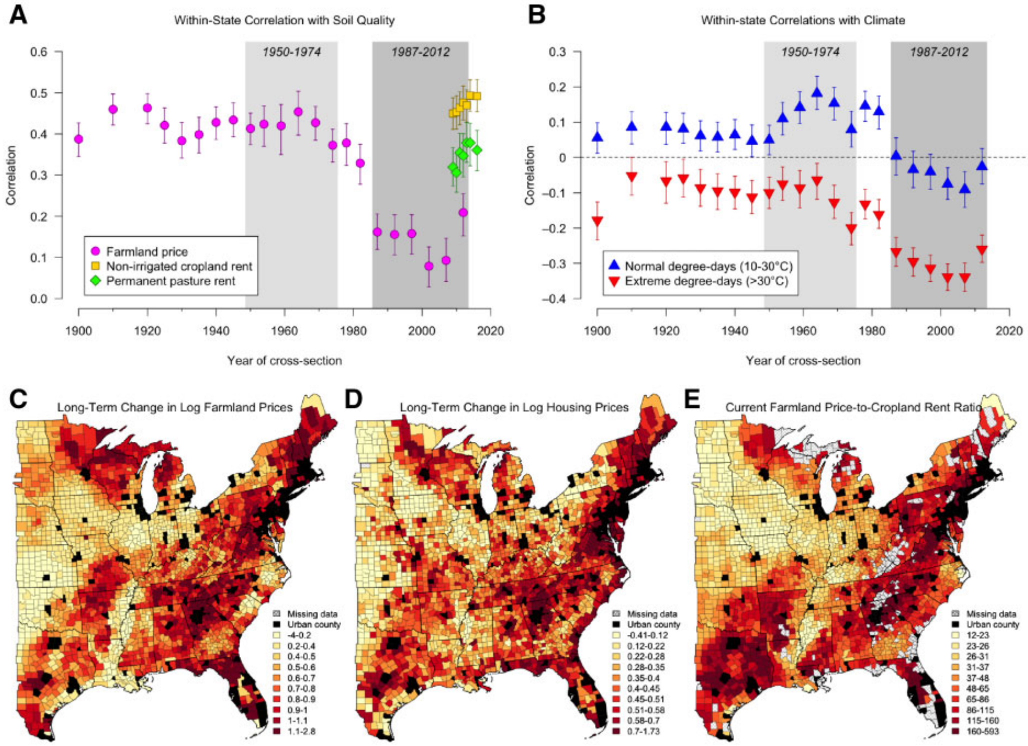

Figure 1. The evolution of key economic indicators in the Eastern United States

Note: All data sources and variables are described in detail in the data section and online supplementary appendix. The color scales in panels C–E correspond to deciles. (A) The time-invariant soil quality variable is Best soil class, which represents the proportion of soils within a county classified in the top three of an eight-class index based on capability to produce commonly cultivated crops and pasture plants (table 5). The within-state Pearson correlation is computed from county-level deviations from the state mean of each variable. The 95% confidence interval is obtained from 1,000 bootstraps. Farmland-weighted correlations yield almost identical results. Only nonurban counties lying east of the 100th median are included. The pattern is even more pronounced in sample-wide correlations (online supplementary figure S5). (B) County-level within-states correlations of degree-days climate variables (1976–2005) and farmland values over time. Similarly, the 95% confidence intervals are bootstrapped. (C) Log change in farmland values between 1987–2012 and 1950–1974. (D) Log change in median housing prices between 1990–2010 and 1950–1980. (E) Farmland price-to-cropland rent ratio based on the average of 2007 and 2012 farmland prices and 2009–2016 cash rents.

value had been relatively high and stable for almost a century prior to drastically declining in the 1980s. In contrast, the correlation with recent *cash rents* for cropland and pasture is relatively high. I obtain virtually identical results against a variety of other indicators of soil quality and agricultural productivity.[16]

Figure 1 (panel B) shows that changes in farmland prices over the past few decades are correlated with climate, as indicated by the shifting within-state correlation between key degree-days variables and *farmland values*. *Normal degree-days* (10–30°C) had been positively correlated with farmland values (as expected) for almost a century prior to rapidly declining and becoming negatively correlated in the 1980s. Similarly, *extreme degree-days* (>30°C) had exhibited a stable negative correlation (as expected) until the 1980s, after which the the association became even more negative.

Important changes in the farmland market must have occurred between 1950–1974 and 1987–2012, which are periods highlighted in figure 1 (panels A and B). Over this time, farmland values per acre indeed increased in real terms by an average of about 75% in the sample. These appreciations are potentially problematic in the Ricardian approach if the

[16] I find very similar patterns when correlations are computed against other indicators, including recent average net revenue per acre for 1997–2012 (within-state correlation drops from ≈0.4 to ≈0.2 in the late 1980s) and average non-irrigated cropland rental price for 2009–2016 (within-state correlation drops from ≈0.6 to ≈0.25 in the late 1970s). I also explored correlations for 56 alternative county-level measures of soil quality from a new online warehouse described in Woodard (2016) as well as from the soil quality indicators used in Mendelsohn, Nordhaus, and Shaw (1994). For the indicators with the highest correlation with farmland values, I find a virtually identical deterioration of the correlation over time.

underlying drivers are omitted and unrelated to agriculture.

A closer spatial look reveals important clues. Panel C of figure 1 shows that the farmland appreciations over this period were concentrated in very specific areas outside the core agricultural areas of the Midwest, the Central Plains, the Cotton Belt, and the Delta.[17] Interestingly, panel D shows that these farmland appreciations mirror very closely the appreciations in median housing values over a similar period. In turn, panel E shows the farmland price-to-cropland rent ratio in recent years. The conceptual framework suggested that high ratios (>30 or 40) are plausibly signaling the presence nonfarm influences that "inflate" farmland prices beyond their agricultural value. These high ratio areas also coincide with areas of both high farmland and housing appreciations.

This preliminary correlational evidence suggests that population-related changes may have spilled over onto farmland markets over time in a way that is correlated with climate and that has deteriorated the relationship of farmland asset prices with critical indicators of agricultural productivity. This forewarns that changes in Ricardian estimates based on farmland values over this period may be suspect.

Estimates Based on Farmland Prices

Regression Results

In this section I explore results from a benchmark Ricardian based on farmland values. As indicated in the conceptual framework, this type of model is potentially vulnerable to omitted nonfarm influences, particularly in more recent decades.

To clarify the contribution of this article, I follow to a large extent a standard model in the literature (Schlenker, Hanemann, and Fisher 2006). More specifically, I estimate a semi-log model based on the GMM estimator developed by Kelejian and Prucha (1999) to account for spatial correlation of disturbances.[18] This spatial error model (SEM) estimator is more efficient than its least squares counterpart. I later show that findings are robust to alternative estimators (OLS and WLS), climate variable definitions, and multiple other variations.

I follow the literature and restrict the sample to eastern non-urban counties. In addition, I further restrict the sample to counties with complete cropland rent data to ensure the comparability of results across models based on alternative dependent variables (online supplementary figure S1). As in most recent Ricardian studies, I favor a specification with state fixed effects to account for unobserved factors affecting farmland values such as agricultural policies. I later discuss results without fixed effects and why they might not be preferred.

Regression results for select coefficients are presented in table 6.[19] Complete results are shown in the online supplementary appendix (table S2). It is somewhat difficult to interpret coefficients directly, but two major points stand out. First, the coefficient for *normal degree-days* is positive and significant but its magnitude decreases over time. Similarly, the coefficient for *extreme degree-days* (>30°C) is negative and mostly significant, and its magnitude increases slightly over time. This pattern suggests a reduction of benefits from *normal degree-days* and an increase in damages of *extreme degree-days*.

Second, the coefficient for *Best soil class* is positive and significant, as expected, but its magnitude decreases by almost an order of magnitude over the time, signaling a marked diminishing role of agricultural "fundamentals" in farmland valuation.[20] In addition, the *income* coefficient is positive and significant indicating that nonagricultural

[17] Areas with the highest farmland appreciations include northern parts of the Lake States (northern Minnesota, Wisconsin, and Michigan), the Ozark Mountains (areas in Missouri and Arkansas), the outskirts of major cities in Texas, the Piedmont of the Southern Appalachian Mountains (northern Georgia, western North Carolina, eastern Tennessee), areas surrounding the Northeast corridor (northern and western Virginia, eastern Pennsylvania, most of Vermont, New Hampshire, and Maine) as well as most of Florida.

[18] This spatial GMM estimator requires the specification of a spatial weight matrix which characterizes the structure of the spatial error correlation. The weight matrix presented here is based on first-order "queen" neighborhood definition and equal weights among neighbors. Results are robust to multiple alternative definitions of this matrix including inverse-distance and binary weights, as well as more expansive neighborhood definitions.

[19] The climate variables in the model include linear terms for normal degree-days (10–30°C) and extreme degree-days (>30°C) and linear and squared terms for precipitation. In terms of control variables, the model includes average water capacity, clay content, minimum permeability, K-factor of topsoil, best soil class, latitude, income per capita and linear and squared terms for population density.

[20] The coefficient change for *Best soil class*, suggests that, all else equal, the value of farmland in a county with only high-quality land (best soil class= 1) in 1950 was 59.2 log points higher than in a county without any high-quality land (best soil class= 0). This difference fell to 7.9 log points in 2007.

Table 6. Selected Regression Results Based on Farmland Values

Independent variables	Older Cross-sections				Recent Cross-sections			
	1950	1954	1959	1964	1997	2002	2007	2012
Degree-days (10–30°C)	0.0424	0.051	0.069	0.0969	0.0241	0.00982	0.00774	0.028
	[3.14]	[3.69]	[5.09]	[7.99]	[2.28]	[0.9]	[0.89]	[2.7]
Degree-days (>30°C)	−0.142	−0.226	−0.353	−0.374	−0.489	−0.463	−0.276	−0.358
	[−1.68]	[−2.61]	[−4.11]	[−4.92]	[−7.37]	[−6.77]	[−5.14]	[−5.61]
Best soil class	59.2	62.0	63.8	62.8	22.5	16.3	7.9	26.1
	[10.85]	[10.93]	[11.31]	[12.81]	[5.27]	[3.73]	[2.32]	[6.5]
Income	0.00422	0.00389	0.00291	0.00319	0.00209	0.00148	0.00098	0.000792
	[6.26]	[6.12]	[5.64]	[8.16]	[14.07]	[11.02]	[11.34]	[10.22]
Pop. dens.	0.511	0.452	0.418	0.297	0.285	0.278	0.174	0.208
	[10.08]	[8.8]	[8.86]	[8.01]	[11.8]	[11.36]	[9.97]	[11.08]
Pop. dens. sq.	−0.0013	−0.00106	−0.000911	−0.000581	−0.000537	−0.000516	−0.000295	−0.000351
	[−7.97]	[−6.68]	[−6.54]	[−5.41]	[−7.38]	[−7.21]	[−5.96]	[−6.67]
Observations	1,787	1,785	1,787	1,788	1,789	1,788	1,790	1,790

Note: All coefficients are multiplied by 100 for clarity. *t*-statistics are shown in brackets and account for spatial correlation of disturbances. All models include state fixed effects. Full results are shown in online supplementary table S2.

factors do influence farmland valuation across all cross-sections. Similarly, the net contribution of *population density* is positive and highly significant with an inverted U-shaped response curve peaking toward the right tail of the range of observations in all cross-sections.

The models here are estimated separately for each cross-section in order to adopt the SEM. This precludes formally testing whether coefficients remain stable over time. I thus consider an alternative panel model based on all cross-sections but estimate separate coefficients for early (1950–1974) and late (1982–2012) periods. In order to rely on the cross-sectional variation of farmland values and climate within states, this model includes year and state fixed effects and I cluster errors at the same levels. Regression results are reported in online supplementary table S3. I conduct a Wald test and reject the hypothesis that coefficients are stable across these 2 time periods ($p = 9.68 \times 10^{-14}$). I also estimate a panel model with separate coefficients for each year (see online supplementary table S4) and obtain similar estimates to those reported in table 6 and online supplementary table S2.

In summary, regression results indicate that climatic parameters are changing over time in a direction that reduces the beneficial effects of *normal degree-days* while increasing the detrimental effects of *extreme degree-days.* Results also reveal that farmland valuation is less and less driven by agricultural fundamentals.

Climate Change Impacts

Climate change impact projections on farmland values for all cross-sections based on the HadGEM2-ES climate model are presented in figure 2. From left to right, columns indicate more severe climate scenarios, while rows correspond to time horizons. Two major points stand out. First, damage estimates for recent cross-sections (1987–2012) are large, negative, significant and consistent with the previous literature. For instance, I find end-of-century climate change impacts of –19.6, –37.0, –45.9 and –68.9% for the average 1987–2012 cross-section for the RCP 2.6, 4.5, 6.0, and 8.5 climate change scenarios, respectively.[21]

[21] These results are comparable to those for the full non-urban eastern counties and to those reported in Schlenker, Hanemann, and Fisher (2006). That study finds end-of-century impacts of –27.4, –31.6, –61.6 and –68.5% for the B1, B2, A2, and A1F1 climate change scenarios based on the Hadley

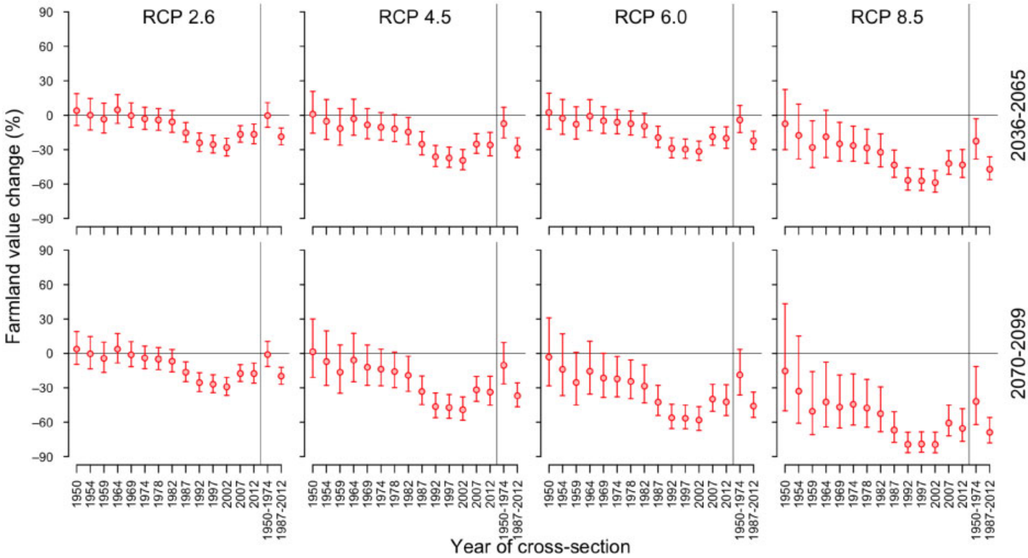

Figure 2. Estimates of climate change impacts based on farmland values

Note: 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. The 95% confidence intervals for the predicted mean change are represented and account for the spatial correlation of disturbances. Models include state fixed effects. The sample is restricted to eastern nonurban counties with complete cropland rent data for 2012.

Second, climate change damage estimates on farmland values are small and often indistinguishable from zero for early cross-sections (1950–1974). Importantly, note that the transition from largely insignificant to large and negative impact estimates occurs around the 1970s which precisely coincides with the transformations in the farmland market highlighted in figure 1. This result has eluded previous research because earlier implementations of the Ricardian approach for the United States only incorporate recent farmland value data. Finally, note that the results based on the alternative panel estimator with coefficients differentiated by year yield similar findings (online supplementary figure S6).

Causes of Growing Damage Estimates

Benchmark Ricardian estimates are unstable over time, ranging from statistical zeros for older cross-sections to large damages for more recent time periods. Because the climate cross-section remains stable over time, the instability of Ricardian estimates is reflecting changes in the farmland price cross-section that remain omitted. Understanding these farmland price changes thus provide the key to identifying the underlying cause of these growing damage estimates.

Various indicators associate farmland appreciations with nonfarm influences. Figure 1 (panels C–E) shows that farmland appreciations are linked to housing appreciations and high price-to-rent ratios. In addition, a state-level correlation analysis shows that farmland appreciations are much more closely associated with population growth than with growth in agricultural productivity (online supplementary figure S7).

This suggests that omitted nonfarm influences are growing over time in a way that is correlated with climate. One way to verify this is to estimate an analogous Ricardian model but for housing prices. Results are shown in online supplementary figure S8 and exhibit the same "amplification" of damages over time, with insignificant impacts for early cross-sections (1950–1960) and increasingly large damages for more recent cross-sections (1970–2010).

Surprisingly, including log of housing prices as a control variable does not affect much the Ricardian estimates based on farmland

III climate model. Although the climate change scenarios and model in the article are more recent, the results show a fair degree of agreement between low and high warming scenarios. This highlights that the sample restriction to counties with complete cropland rent data does not affect the bottom line of the result.

prices. One possibility is that the short-term dynamic interactions between housing prices (available every ten years) and farmland prices (available every 5 years) as well as the spatial spillovers may not be well captured in this framework.[22] However, excluding counties that are presumably under strong nonfarm influences, using farmland appreciation as a threshold variable, points to climate change impacts that are mostly insignificant, even for recent cross-sections (online supplementary figure S9).

These findings support the hypothesis that the growing projected damages on US farmland found in the benchmark model stems from the influence of counties having experienced the greatest appreciations in farmland values over the past several decades. These farmland appreciations appear more closely related to the differential growth in population-related influences than in differential increases in agricultural productivity.

Robustness Checks

The baseline findings in this section and the instability of Ricardian estimates shown in figure 2 are robust to different variations of the model. For instance, I find a very similar patterns of projected impacts for a model based on the specification and degree-day variable thresholds proposed in Schlenker, Hanemann, and Fisher (2006) (online supplementary figure S10) and for a model based on monthly climate variables and a uniform climate projection proposed in Mendelsohn, Nordhaus, and Shaw (1994) (online supplementary figure S11).

I also find that these results are fairly robust to the specification of the weight matrix in the GMM estimator, which I rely upon for its improved efficiency (online supplementary figure S12). To verify the choice of this estimator does not drive these results, I explore impact estimates based on alternative estimators including OLS and a farmland-weighted linear model. Impact projections remain largely unchanged (online supplementary figure S13).

The preferred specification in this article includes state fixed effects to account for regional unobservables. I nonetheless explore results based on a model without fixed effects. Interestingly, omitting state dummies points to significant negative impacts for almost all cross-sections (online supplementary figure S14). Impact estimates appear more negative relative to the fixed-effects specification but maintain a similar temporal pattern.

In principle, including state fixed effects should not introduce bias unless regional dummies amplify attenuation bias stemming from classical measurement error in climate variables (Griliches and Hausman 1986). Such attenuation would lead to coefficients estimates that are closer to zero when transitioning to a fixed effects specification. However, I find that coefficients for both normal and extreme degree-days are not closer to zero but simply more negative in the specification without fixed effects (online supplementary figure S15). This is not consistent with attenuation bias and explains why impacts based on the model without fixed effects appear more detrimental relative to the fixed effects specification. This finding favors the fixed effects specification in line with the literature (Schlenker, Hanemann, and Fisher 2006; Severen, Costello, and Deschenes 2018; Hendricks 2018).[23]

Estimates Based on Cash Rents

Regression Results

I now turn to an alternative Ricardian model based on cash rent per acre for nonirrigated cropland and pasture as the dependent variable. In principle, relying on these variables should circumvent the capitalization of nonfarm influences stemming from expected land use changes.

Note that the following results rely on the exact same estimator, independent variables, and county sample restrictions used for the benchmark Ricardian model based on farmland values. As indicated in the conceptual framework, coefficients from semi-log models based on farmland asset prices and annual rental prices have analogous economic interpretation. However, because rental prices are likely more susceptible to input and output

[22] For instance, housing appreciations appear less spatially dependent or "diffuse" than farmland appreciations as shown in figure 1 (panels C and D).

[23] A potential caveat of introducing state fixed effects is it may amplify biases from nonfarm influences if their correlation with climate is stronger within states than across the entire sample. I thank an anonymous reviewer for pointing this out.

Table 7. Regression Results Based on Cash Rents

Independent variables	2009	2010	2011	2012	2013	2014	2016
A. Nonirrigated Cropland Cash Rent							
Degree-days	0.0878	0.115	0.101	0.103	0.0833	0.101	0.123
(10–30°C)	[5.84]	[7.89]	[7.01]	[6.45]	[4.94]	[6.03]	[7.27]
Degree-days	−0.197	−0.354	−0.229	−0.323	−0.197	−0.198	−0.27
(>30°C)	[−2.07]	[−3.85]	[−2.52]	[−3.26]	[−1.87]	[−1.92]	[−2.64]
Best soil class	68.6	62.9	70.0	80.4	85.9	88.6	84.5
	[11.26]	[10.5]	[11.7]	[12.76]	[13.04]	[13.02]	[12.7]
Income	4.54E−05	0.000203	8.34E−05	0.000159	0.000101	−4.52E−05	7.58E−05
	[0.31]	[1.46]	[0.71]	[1.29]	[0.9]	[−0.41]	[0.6]
Pop. dens.	−0.0101	0.00255	0.00524	0.0228	0.0189	0.0199	0.0149
	[−0.34]	[0.09]	[0.19]	[0.76]	[0.6]	[0.62]	[0.48]
Pop. dens. sq.	1.43E−05	−5.28E−06	−2.88E−05	−0.000121	−0.000115	−0.000103	−4.69E−05
	[0.17]	[−0.06]	[−0.37]	[−1.44]	[−1.31]	[−1.15]	[−0.56]
Observations	1,636	1,648	1,662	1,790	1,662	1,648	1,643
B. Pasture Rent							
Degree-days	0.0329	0.0479	0.0535	0.0671	0.0741	0.0554	0.0619
(10–30°C)	[2.31]	[3.38]	[3.72]	[4.38]	[4.91]	[3.63]	[4.04]
Degree-days	−0.0649	−0.258	−0.22	−0.315	−0.212	−0.123	−0.187
(>30°C)	[−0.76]	[−3.03]	[−2.53]	[−3.44]	[−2.29]	[−1.38]	[−2.09]
Best soil class	41.6	37.6	49.4	49.4	44.4	57.3	48.0
	[6.91]	[6.26]	[8.18]	[7.58]	[6.91]	[8.95]	[7.4]
Income	0.00014	−7.52E−05	0.000123	7.53E−05	4.16E−05	6.72E−05	−1.14E−05
	[0.87]	[−0.48]	[0.89]	[0.57]	[0.37]	[0.64]	[−0.09]
Pop. dens.	0.0579	0.0305	0.0713	0.0519	0.0497	0.073	0.0559
	[1.75]	[0.94]	[2.06]	[1.53]	[1.5]	[2.2]	[1.71]
Pop. dens. sq.	−0.000133	−5.02E−05	−0.000217	−0.000134	−0.000135	−0.000207	−0.000126
	[−1.39]	[−0.53]	[−2.19]	[−1.39]	[−1.42]	[−2.17]	[−1.41]
Observations	1,393	1,362	1,211	1,172	1,162	1,287	1,278

Note: t-statistics are shown in brackets and account for spatial correlation. All models include state fixed effects.

price swings than farmland asset prices, the cash rent estimates could be noisier.

Regression results for select coefficients are presented in table 7 for state fixed effects models based on both cropland and pasture cash rents. Full results are shown in the online supplementary appendix (tables S5 and S6).

Two major points stand out. First, the coefficient for *normal degree-days* is positive and significant and remains stable over time for both cropland and pasture. Interestingly, its magnitude is comparable to that in models based on older farmland values (1950–1969), whereas it is an order of magnitude larger than that of models based on recent farmland values (2007–2012) (table 6). On the other hand, the coefficient for *extreme degree-days* is negative and significant in most cross-sections. It is also of similar magnitude than in models based on farmland values (see table 6).

Second, the coefficient for *Best soil class* is positive in cash rent models, and of similar magnitude (if not larger) than for models based on older *farmland values* (1950–1969, table 6). On the other hand, the magnitude of this coefficient is considerably larger for cash rent models than for models based on recent farmland values (2007–2012). This indicates that soil quality plays a sizable role in explaining recent cash rents.[24] This is somewhat comparable to models based on older farmland values, but unlike those based on recent ones (table 6). In addition, the coefficient for *income* is insignificant for all cash rent models. Coefficients for *population density* are also largely insignificant (online supplementary tables S5 and S6). This is unlike

[24] The coefficient change for *Best soil class*, suggests that, all else equal, the value of cropland in a county with only high-quality land (best soil class= 1) in 2012 was approximately 80.4 log points higher than in a county without any high-quality land (best soil class= 0). The difference amounts to 49.4 log points for pastures.

benchmark models, which strongly reflect such nonfarm influences (table 6).

Again, these models were estimated separately for each cross-section, so one cannot test whether the coefficients remain stable over this relatively short time period (2009–2016). I thus consider a panel model based on all cross-sections but estimate separate coefficients for early (2009–2012) and late (2013–2016) periods. Similarly to the panel model based on farmland values, these models for cash rents include year and state fixed effects, and errors are clustered at the same levels. Results are reported in online supplementary table S7 for cropland and pasture rental models. I conduct a couple of Wald tests and reject the hypotheses that coefficients are stable across these 2 time periods for both cropland and pasture rent models ($p = 0.2$ and 1, respectively).

In summary, cropland and pasture cash rents strongly reflect agricultural "fundamentals", whereas they do not exhibit significant influences from nonfarm drivers such as income or population density.[25] This is a stark difference with benchmark models based on farmland values, which exhibit strong nonfarm influences and an attenuating role of agricultural fundaments over time (table 6). More importantly for this analysis, *normal degree-days* play a beneficial role whereas *extreme degree-days* play a detrimental role in explaining cash rents. These effects are sizable and comparable to those found in older farmland value cross-sections. This finding foreshadows a similarity between climate change impact estimates based on cash rents and those based on older farmland values.

Climate Change Impacts

Climate change impact projections based on the HadGEM2-ES climate model for cash rents for all cross-sections are presented in figure 3. Two major points stand out. First, virtually none of the climate change impact estimates are significant at conventional levels for either cropland or pasture rents.

Second, the uncertainty surrounding projected impacts is large for the most rapid warming scenario (RCP 8.5), especially towards the end of the century. Because I rely on the same sample restriction and explanatory variables than in the farmland value model, the greater uncertainty likely originates from a higher spatial dependence of rental prices. This greater uncertainty indicates that one cannot rule out large effects of climate change on cash rents for the most severe scenarios at the end of this century.

Note that the results based on recent cash rents (2009-2016) are mostly insignificant and, to a great extent, consistent with estimates based on older farmland values (e.g., 1950–1974; see figure 2). However, they exhibit an important departure from estimates based on recent farmland values (e.g., 1987–2012). This discrepancy is consistent with the underlying hypothesis in this article regarding the growing influence of omitted nonfarm pressures in Ricardian models based on farmland values. Nonfarm pressures appear absent from cash rents and reduced in older farmland values. However, these pressures seem to have a pronounced effect on recent farmland values and appears to be strongly affecting associated Ricardian estimates.

Robustness Checks

The results presented here are fairly stable. In the online supplementary appendix, I show these are robust to alternative climate variables (online supplementary figure S16) and to alternative estimators, including OLS and area-weighted regressions (online supplementary figures S17 and S18).[26]

As previously mentioned, a potential caveat of the current implementation is that the survey data targets cropland and pastures participating in the rental market, and not all cropland and pastures. While the data necessary to carefully analyze this selection problem are unavailable, one can speculate about microlevel selection patterns from aggregate data. Specifically, the fact that counties with better soils tend to exhibit a higher share of rented farmland (online supplementary figures S19 and S4) suggests that parcels with better soil quality are more likely to be rented out. This suggests that high-quality land may be overrepresented in each county. As a result, if

[25] Conceptually, one could expect some effect of these variables on cash rent because they can influence farm returns (e.g. lower transportation costs). However, the results suggest this effect is tenuous at best, possibly because most agricultural production is not sold directly in nearby population centers.

[26] The results shown in the online supplementary appendix for OLS and WLS point to positive impacts for most scenarios. However, the result is not robust to an alternative growing degree-days specification.

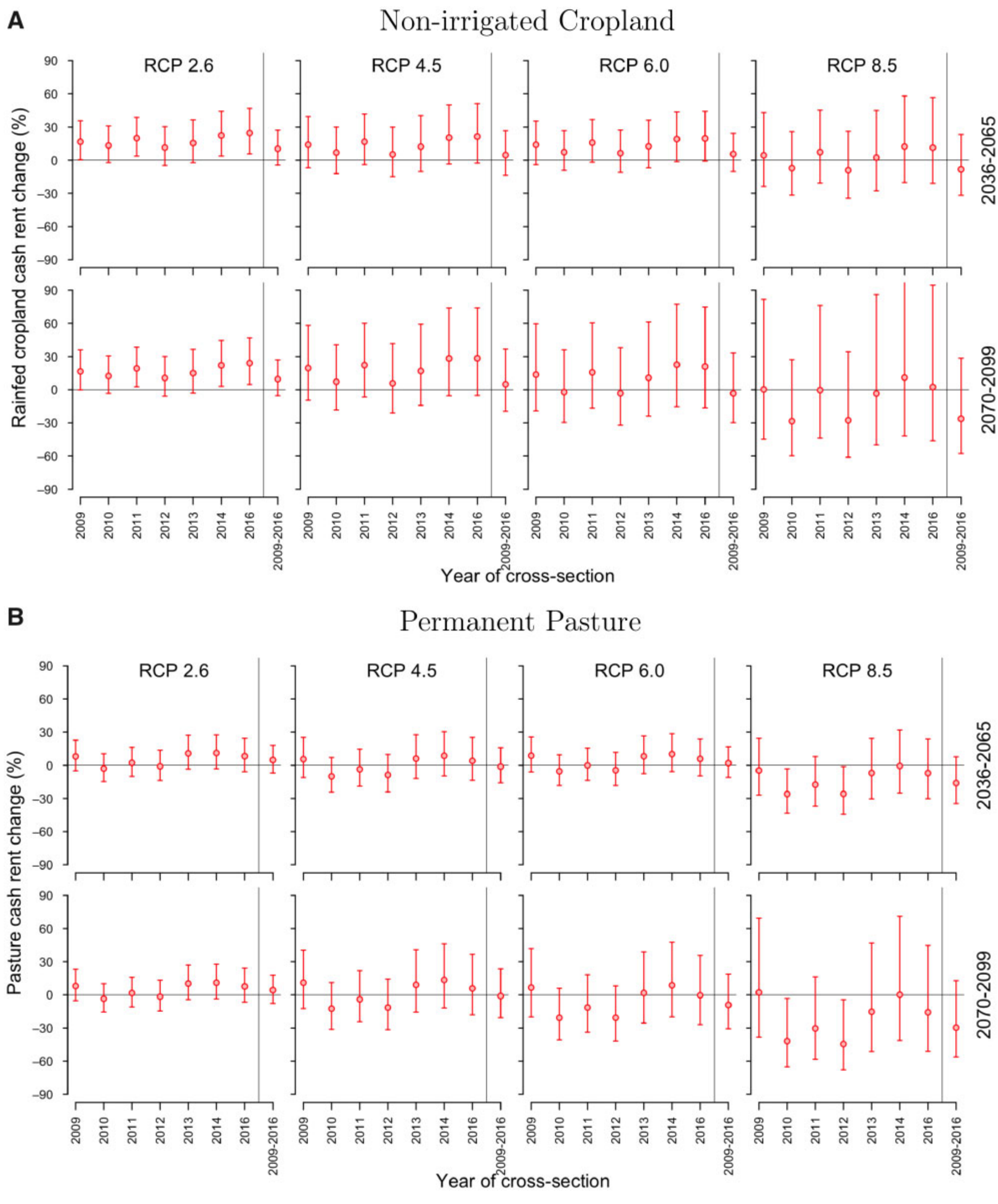

Figure 3. Estimates of climate change impacts based on cash rents

Note: Projected farmland value percent changes correspond to the area-weighted sample average projection. The top (bottom) row of each panel corresponds to the predicted rental value change for 2036–2065 (2070–2099) relative to the 1976–2005 reference period. RCP scenarios increase in severity from left to right. The 95% confidence intervals for the predicted mean change account for spatial correlation of disturbances. Models without controls omit all but climate variables. All models include state fixed effects.

land of poor quality is more vulnerable to climate change, then this truncation of the sample would bias Ricardian estimates upward.

To verify whether land of lower quality responds differently to climate variation, I estimate the same model based on two subsamples with low and high rates of rental incidence. I find that impact results remain stable across subsamples, which may temper concerns about external validity regarding land of poorer quality (online supplementary figures S20 and S21).

For the sake of completeness, I also explore a specification without state fixed effects which is arguably more vulnerable to omitted variables (online supplementary figure S22). Similarly, omitting state dummies results in negative impact estimates on nonirrigated cropland rents, but these are only consistently distinguishable from zero at conventional levels for the most extreme scenario (RCP 8.5). Negative effects are slightly larger and more stable for a model of pasture rents without state fixed effects. However, estimates without state fixed effects are arguably less reliable and are not favored in the literature.

Discussion

Reconciling Differences between Benchmark and Alternative Models

Ricardian estimates based on recent cash rents and older farmland values tend to point to impacts of climate change that cannot be distinguished from zero. To clarify the drivers that underpin this result, I decompose climate change impacts to show the contribution of each variable to overall projection for each model type for different time horizons and climate scenarios. Results are summarized in table 8 for representative cross-sections for each type of model (i.e., cropland rents, pasture rents, old farmland values, and recent farmland values).

Three points stand out. First, changes in degree-day variables play a much greater role than precipitation changes in explaining overall impacts. This is in line with the literature. While changes in *normal degree-days* contribute positively, changes in *extreme degree-days* contribute negatively to overall impacts. This is the case for all models, time horizons and climate scenarios.

Second, the detrimental effect of *extreme degree-days* is large and stable across all models. Therefore, discrepancies in overall impacts across models do not originate from the *extreme degree-days* variable.

Third, the beneficial effect of *normal degree-days* is large for models based on cropland and pasture rents and based on older farmland values. However, the benefits accrued from *normal degree-days* drop considerably with more recent farmland values. In fact, the single major difference across models based on recent and old farmland values is the contribution of *normal degree-days* to total impacts.

This decomposition shows that the negative climate change estimates based on recent farmland values stem from discrepancies in the *normal degree-days* coefficient. This is robust to alternative specifications of degree-day variables.[27] As previously discussed, this coefficient is unusually small for models based on recent farmland values relative to the magnitudes exhibited for older farmland values (table 6), nonirrigated cropland rents and pasture rents (table 7).

The beneficial role of normal degree-days has an intuitive biophysical interpretation. Normal degree-days are indicative of the length of the growing season. Agronomists, agricultural scientists, and farmers rely on this measure to choose crop varieties that take advantage of the full length of the nonfreezing period in their region. Longer growing seasons allow farmers to grow crops that stay longer on the ground, are able to absorb more sunlight and nutrients to produce more biomass and higher yields. This explains why farmers tend to plant early when weather conditions permit. In addition, longer nonfreezing periods also allow more flexibility for timing the growing season so that vulnerable crop stages do not coincide with detrimental weather conditions in the summer (Ortiz-Bobea and Just 2013).

The findings here suggest that rising temperatures can have both sizable positive and negative effects. When Ricardian models are estimated based on cash rents, the beneficial effects of temperature rise are sizable and offset its negative effects. This is also the case for Ricardian models based on older farmland values. However, the beneficial effect of temperature rise decreases considerably with more recent farmland data, whereas its negative effect remains stable. This explains negative estimates based on recent farmland value cross-sections.

[27] Although not shown here, the impact decomposition for models based on a different specification of degree-days specification (linear and quadratic degree-days, 8–32°C, and squared root of degree-days, >34°C) is very similar. The effect of extreme degree-days (>34°C) remains stable across all time periods and models. However, the effect of normal degree-days (8–32°C) is positive and much larger for benchmark models based on older farmland values and for models based on recent cash rents.

Table 8. Climate Change Impact Decomposition for Alternative Ricardian Models

	Midcentury (2036–2065) Impact (%)				End-of-Century (2070–2099) Impact (%)			
	Rent (2009–2016)		Farmland Value		Rent (2009–2016)		Farmland Value	
	Cropland	Pasture	(1950–1974)	(1987–2012)	Cropland	Pasture	(1950-1974)	(1987-2012)
Hadley GEM2-ES—RCP 2.6								
Degree-days (10–30°C)	39.0	26.7	25.7	8.9	39.7	27.2	26.2	9.0
Degree-days (>30°C)	–21.1	–17.9	–21.1	–25.9	–22.0	–18.7	–22.0	–26.9
Precipitation	0.5	0.8	0.4	0.6	0.5	0.9	0.5	0.6
Total impact	10.3	4.8	–0.4	–18.8	9.6	4.3	–1.1	–19.9
S.E.	(7.6)	(6.3)	(5.6)	(4.7)	(7.8)	(6.4)	(5.8)	(4.8)
Hadley GEM2-ES—RCP 4.5								
Degree-days (10–30°C)	48.3	32.8	31.5	10.7	67.9	45.1	43.3	14.4
Degree-days (>30°C)	–29.3	–25.1	–29.3	–35.5	–38.1	–33.1	–38.2	–45.5
Precipitation	–0.3	–0.6	–0.4	–0.3	1.0	1.8	1.1	1.2
Total impact	4.6	–1.2	–7.4	–28.8	4.9	–1.1	–10.4	–37.0
S.E.	(10.2)	(8.5)	(7.6)	(6.3)	(14.4)	(12.0)	(10.7)	(8.8)
Hadley GEM2-ES—RCP 6.0								
Degree-days (10–30°C)	37.3	25.6	24.6	8.6	76.5	50.4	48.4	15.8
Degree-days (>30°C)	–24.0	–20.5	–24.0	–29.3	–45.2	–39.5	–45.2	–53.2
Precipitation	1.2	2.1	1.3	1.4	–0.1	–0.2	–0.2	–0.1
Total Impact	5.6	2.0	–4.1	–22.2	–3.3	–9.2	–18.8	–45.9
S.E.	(8.7)	(7.2)	(6.5)	(5.4)	(17.8)	(14.7)	(13.2)	(10.8)
Hadley GEM2-ES—RCP 8.5								
Degree-days (10–30°C)	69.2	45.9	44.1	14.6	117.3	74.7	71.4	22.3
Degree-days (>30°C)	–43.8	–38.2	–43.8	–51.8	–65.9	–59.3	–66.0	–74.4
Precipitation	–3.6	–6.9	–4.4	–4.2	–0.5	–0.9	–0.6	–0.6
Total Impact	–8.4	–16.1	–22.6	–47.1	–26.3	–29.6	–42.0	–68.9
S.E.	(16.3)	(13.6)	(12.1)	(10.0)	(32.8)	(27.3)	(24.1)	(19.6)

Note: Total Impacts in percent are computed as $100(\exp(\Delta X\beta) - 1)$, where $\Delta X\beta$ are changes in log *farmland value* or*cropland rental price* driven by changes in climatic variables only. $\Delta \mathbf{X}$ is computed as the farmland-weighted change in climate variables under each scenario. The impact contributions of all variables sum to the total impact.

Because cash rents for both cropland and pastures are strongly linked to agricultural fundamentals such as soil quality but remain unaffected by nonfarm variables such as population density or income, the associated Ricardian estimates appear better suited for capturing the effect of climate on agricultural returns than models based on farmland values. Moreover, because the role of agricultural fundamentals in farmland valuation weakens over time, Ricardian estimates based on more recent cross-sections appear less reliable.

Alternative Explanations

Measurement error of the distant climate.— At first glance, the amplification pattern of larger climate change impact estimates toward more recent farmland value cross-sections (figure 2) may seem consistent with attenuation bias. Measurement error of the distant climate may lead to attenuation of estimated coefficients and subsequently lead to small climate effects for older cross-sections. However, as the regression results and the decomposition of climate change impacts show, this pattern does not stem from attenuated coefficients in older cross-sections (tables 6, 8, and online supplementary table S2). Moreover, an analysis based on a century of climate data from PRISM indicates the climate cross-section has remained very stable (table 4), suggesting measurement error of older climate cross-sections is an unlikely driver.

Climate-biased technological change.— Alternatively, the aforementioned pattern of climate change impacts over time based on farmland values may be consistent with a rise in the climate sensitivity of US agriculture (Lobell et al. 2014; Ortiz-Bobea, Knippenberg, and Chambers 2018). This may be driven by technological change that is "biased" against certain climates giving rise to a differential growth of farmland values correlated with climate over the past fifty years. However, the conceptual model implies that in the absence of nonfarm influences, technological change would be reflected in both farmland prices and rents. Yet the large negative impacts for climate change are solely found on the farmland price cross-sections. In addition, there is a clear coincidence between areas with farmland and housing appreciation. In fact, changes in farmland values are weakly linked to increases in agricultural TFP (online supplementary figure S7) whereas are strongly linked to population change. Thus, while climate-biased technological change may be occurring, it does not appear to be the main driver of farmland value change over the past several decades.

Profitability of peri-urban agriculture.—The rise in farmland values in areas with a growing population could be reflecting a rise in the profitability of peri-urban agriculture and local food markets. However, higher profitability of these peri-urban activities would entail a rising demand for farmland with commensurate increases in rental price in these locations. That is, the upward pressure on farmland values from greater peri-urban agricultural profitability would also be putting upward pressure on rental prices. This is not the pattern emerging from the data, as socioeconomic indicators (e.g., income per capita, population density) have little bearing on rental prices.

Capitalization of farm subsidies.—It is well known that farmland values capitalize farm subsidies (Barnard et al. 1997; Goodwin, Mishra, and Ortalo-Magné 2003). If these payments are correlated with climate, then these could potentially affect Ricardian estimates based on farmland values. However, changes in government subsidies would have been reflected in disproportionate farmland appreciations in the highly agricultural areas that capture most of these government payments (e.g., Corn Belt). However, farmland appreciations coincide with appreciating home values. In addition, the conceptual framework and empirical evidence suggests that rental prices should *also* capitalize farm subsidies (Roberts, Kirwan, and Hopkins 2003; Kirwan 2009), meaning such influences would similarly affect the alternative Ricardian estimates based on cash rents, which is not the case.

Other omitted variables.— The pattern exposed in this article is consistent with a *slowly varying* omitted variable. Nevertheless, these results could still be biased due to *time-invariant* farm-related omitted variables which the proposed approach does not account for implicitly. These could include factors such as unobserved soil characteristics which cannot be ruled out.

Reconciling Results in the Literature

The findings presented here for nonirrigated cropland cash rents appear somewhat different from those in Hendricks (2018). That study focuses on the nation's Heartland and excludes large parts of the South and the Northeast, whereas this study excludes counties in the Great Plains lying west of the 100th meridian (online supplementary figure S23). To explore this sample difference, I compute climate change impact projections for this alternative sample based on a similar model weighted by farmland area and including state fixed effects (online supplementary figure S24, top). The mid-century impact on these cash rents is around –12% for the RCP 4.5 scenario, falling in the lower range of the reported impacts in that study based on a wider range of climate models. Additional differences can stem from the choice of control variables which appear critical for this region; omitting them from the model would point to considerable larger damages (around –37%, see online supplementary figure S24, bottom). Thus, the overall discrepancies between both studies appear to stem from differences in the sample and possibly control variables. This raises an interesting question regarding the regional stability of Ricardian estimates.

More generally, the results presented in this article may help reconcile other results in this literature. Basic production theory suggests that one should expect an approach that allows for greater farmer adaptation—like the Ricardian approach—to yield more optimistic welfare impacts than approaches that do not account for important adaptation mechanisms by restricting input adjustments. As a result, the Ricardian estimates presented here are in agreement with relatively large short-run damages of climate change impacts on US agriculture based on weather shocks on net revenues (Deschênes and Greenstone 2012; Fisher et al. 2012), crop yields (Schlenker and Roberts 2009; Ortiz-Bobea and Tack 2018), or TFP (Ortiz-Bobea, Knippenberg, and Chambers 2018).

Note that the findings in this article should be interpreted with caution because confidence intervals remain large for the most extreme scenarios. In addition, Ricardian estimates do not account for the adjustment costs of adaptation, which could be the critical component of climate change impacts. Finally, there is some debate regarding whether US farmers are adapting to recent trends in climate (Burke and Emerick 2016).

Implications for Ricardian Models

The approach in this article seeks to estimate the effect of climate on the farm returns to the land by circumventing biases from nonfarm returns. This presumes that nonfarm returns should not be considered in projections of climate change impacts on agriculture. This seems reasonable given that the amenity value of climate could be better captured through housing prices and wages (e.g., Albouy et al. 2016). In essence, the reliance on cropland and pasture cash rents in the Ricardian analysis conceptually restricts adaptation to specific *agricultural* land uses. Therefore, the resulting impact projections provide an upper bound on agricultural damages relative to a model allowing for adjustments to an even broader category of land uses.

Conclusion

This article conducts a long-spanning Ricardian study to assess the potential impacts of climate change on US farmland. The results suggest that nonfarm pressures on farmland operate as an influential omitted variable in the Ricardian approach. I find that benchmark Ricardian estimates of climate change impacts become increasingly large and negative over time. This pattern does not seem to be explained by measurement error of climate or by technological change in agriculture. There are indications, however, that the result relates to spillovers from the differential growth in nonfarm influences across climates within states.

The article finds concording evidence from an alternative Ricardian model that largely circumvents influences associated with expected land use changes. The empirical implementation in this article is based on nonirrigated cropland rents and pastures for the eastern United States, but other variables such as measures of expected net revenues are also appropriate. The alternative model suggests that climate plays a significant role in explaining rents for nonirrigated US crop agriculture and pastures, but that projected changes in climate would lead to an inconclusive long run effect on rental prices due to the countervailing effects of extreme and normal temperatures.

This research contributes to the overall debate of climate change impacts on US agriculture and hopefully helps reconcile somewhat counterintuitive climate change impact estimates in the literature. Long-run effects of climate change that allow for the full range of farmer adaptations should be more optimistic than short-run estimates that only account for limited within-year farmer adaptations to weather fluctuations. As a result, the small effects found in this article are congruent with negative effects of weather shocks found on agricultural revenues or crop yields. It is important, however, to highlight that projected impacts of climate change are imprecisely estimated for the most extreme scenario so substantial impacts cannot be ruled out.

Several caveats apply to this study. The study focuses on identifying the presence and reducing the influence of nonfarm influences in the Ricardian approach. However, other omitted variables cannot be ruled out, particularly variables that affect farm returns directly. Also, the use of rental price could potentially introduce sample selection issues. Finally, the Ricardian approach does not account, in general, for a number of important aspects, including CO_2 fertilization, adjustment costs of adaptation, or large-scale ecological changes affecting pest and weed populations. Future research should focus on ways to enhance adaptation, measuring the costs of adjustment to a new climate and on providing robust estimates of climate change impacts in understudied regions of the world such as sub-Saharan Africa.

Supplementary material

Supplementary materials are available at *American Journal of Agricultural Economics* online. Computer code and data necessary to reproduce this study can be downloaded at https://doi.org/10.6077/2dhd-f934.

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