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Identifying the Economic Impacts of Climate Change on Agriculture

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Abstract

The ultimate impact of climate change on human systems will depend on the natural resilience of ecosystems on which societies rely as well as adaptation measures taken by agents, individually and collectively. No sector of the economy is more reliant on climate than agriculture. Evidence from the American settlement process suggests that societies can successfully adapt to new climatic environments. Whether and how much agriculture will manage to adapt to a changing climate remains an open question in the empirical economics literature, however. This paper reviews the existing evidence on weather and/or climate impacts on agricultural outcomes from the economics literature, with a focus on methodological questions. Some key econometric issues associated with climate impact measurement are discussed. We also outline important questions that have not been adequately addressed and suggest directions for future research.

Key words: climate change; climate econometrics; agriculture; causal inference; panel data

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1 Introduction

The United Nations' Intergovernmental Panel on Climate Change (IPCC), in its fifth assessment report, states that the most serious impacts of climate change will affect industries like agriculture (Pachauri et al., 2014). Climate change will impact food supply, making it even more challenging to meet increasing food demand caused by population growth, income growth, and urbanization. It has become a priority for researchers to understand to what extent climate change affects agriculture as well as its distributional impacts, in order to inform policy on climate mitigation and adaptation.

A large body of work in the scientific literature has sought to model the effect of climate change on agriculture using laboratory or field experiments and agronomic models. Experiments, to the extent that they can carefully mimic future conditions, provide a reliable measure of how these conditions will affect narrowly defined systems, such as a single crop. Agronomic models link complex natural soil and weather processes to crop yields and are usually calibrated to experimental data. Despite their accuracy in reflecting crops' biological responses to changes in environmental factors, these methods generally fail to capture human behavior in the real-world agricultural system.

Over the past 25 years, economists have developed new approaches, with the help of rich observational data, to infer climate impacts on agriculture while accounting for the behavioral response of humans. Cross-sectional and panel regression analysis are the most commonly used methods in this literature. The former seeks to correlate cross-sectional differences in climate to agricultural outcomes and thus implicitly accounts for long-run adaptation to climate change, however it falls short on causal identification due to the likely presence of omitted variables. The latter features a cleaner identification strategy but may only partially capture long-run adaptive behavior. In addition to these two revealed-preference approaches, another important strand of literature has used multi-market equilibrium models in a partial or general equilibrium context. The first goal of this paper is to synthesize the evidence from these different approaches with an emphasis on the existing evidence on adaptation and adaptability of agriculture.

The panel approach is arguably the most promising method to provide causal evidence. We thus focus on important methodological issues that arise in the context of estimating climate change impacts from panel data. Given the importance of adaptation, we examine work that addresses the issue of long-run responses in the context of the panel approach. We also point to some overlooked econometric issues in the linear fixed effects model. First, we examine potential multicollinearity that can arise in the context of regressions with temperature and precipitation bins. We also discuss the econometric results on the bias of linear fixed effects estimation in the presence of response heterogeneity. Finally, given the high-frequency nature of the weather variables relative to yield and other agricultural outcomes, researchers are faced with a unique model selection problem in assessing climate change impacts. We present the cross-validation approach that has been used to address it and review some key results pertaining to its validity from the statistics literature.

The paper is organized as follows. Section 2 reviews the evidence on climate change impacts on agriculture based on the cross-sectional and panel approaches as well as market equilibrium models. Section 3 examines the literature on the panel approach and its ability to capture or estimate long-run adaptive responses. Section 4 presents econometric issues in the linear fixed effects model commonly overlooked in the climate change literature. Finally, Section 5 concludes and points to important directions for future research.

2 Estimates of Climate Impacts on Agriculture

2.1 Cross-Sectional Approach on Farmland Values and Revenues

Mendelsohn et al. (1994) are the first to implement a revealed-preference approach relying on observational data to estimate climate change impacts on agriculture. They exploit cross-sectional variation across US counties by regressing farmland values on climate variables and soil-quality and socioeconomic variables. This *Ricardian* approach posits that in a competitive land market, climate-contingent farm profitability should be reflected in land values. The method implicitly captures adaptive behavior, especially crop-switching. Though ground-breaking, this approach has been criticized for omitting the role of irrigation, among other things (Cline, 1996; Darwin, 1999; Schlenker et al., 2005).

Restricting the analysis to rain-fed regions, Schlenker et al. (2006) re-examine climate change impacts on US farmland values. Warming is estimated to decrease farmland values by 27-69% by the end of the century. Farmland values in irrigated areas have been found to be less sensitive to changes in local precipitation (Mendelsohn and Dinar, 2003), and the evidence shows that water availability is capitalized into farmland values (Schlenker et al., 2007; Buck et al., 2014).

In a recent review, Mendelsohn and Massetti (2017) report that the Ricardian approach has been applied in 46 countries in Africa, America, Asia, and Europe (e.g., Kurukulasuriya et al., 2008; Seo and Mendelsohn, 2008a,c; Wang et al., 2009; Van Passel et al., 2017).[1] They summarize that under global average temperature increases of 2°C and precipitation increases of 7%, Ricardian results predict a decline in net farm revenue of 8-12%. The Ricardian approach has also found that climate change impacts vary by region, with some winners and some losers. Warming benefits agriculture in cold regions but harms agriculture in warm regions, and overall more precipitation is beneficial everywhere except where it is already too wet for agriculture.

2.2 Panel Approach on Farm Profits and Crop Yields

The Ricardian approach is a practical tool for predicting climate change impacts on economic welfare, but the results cannot be disaggregated into impacts for specific

[1] Some of these studies are based on survey data on farm-level net revenues.

crops or types of livestock. Its overall reliability has also been challenged due to weak causal identification. In addition to the well-discussed omitted-variable concerns, the potential endogeneity of land-use decisions likely confounds the identification of marginal effects (Timmins, 2006).

As an alternative, Deschênes and Greenstone (2007) propose to exploit presumably random year-to-year fluctuations in weather to model the effect of climate change on agricultural profits and crop yields, in the context of a panel approach with fixed effects. This approach controls non-parametrically for unobservable time-invariant factors across regions, so it does a better job of addressing the omitted variable problem than the Ricardian approach does. However, climate effects are identified using weather fluctuations rather than climate differences, which has led many to argue that the estimated effects can only capture limited adaptation. Appealing to the envelope theorem, Hsiang (2016) and Blanc and Schlenker (2017) suggest that for relatively small changes in climate, the panel method is a good predictor of the response to climate, although it is based on weather variation rather than climate variation. This is only true if the outcome variable is being optimized, however. In some studies (e.g., Schlenker and Roberts, 2009), yield is the outcome variable, but farmers do not typically maximize yields.

Deschênes and Greenstone's latest results suggest that climate change will reduce current annual agricultural profits by about 30% by the end of the century (Deschênes and Greenstone, 2012). Note that the interpretation of farm profit effects as social welfare effects would require output prices to remain constant, which the authors achieve by including state-by-year fixed effects so that factors affecting farm profits uniformly across a state (like crop prices) are implicitly held constant in estimation.[2] However, as pointed out by Fisher et al. (2012), an annual profit regression likely violates the identification assumption in Deschênes and Greenstone (2007) because unobserved commodity inventory decisions are correlated with weather and affect annual profits. Perhaps due to this concern, studies following Deschênes and Greenstone (2007) have mostly employed panel-data methods to estimate crop yield responses to year-to-year weather fluctuations.

Empirical studies on measuring yield responses to weather fluctuations may be categorized into two groups based on the data used. The first group applies panel estimation on publicly available agricultural data. The observations in these data are typically administrative units, like counties. Using county-level survey data, Schlenker

[2] To see why, consider a simple partial equilibrium model whereby a representative consumer has quasilinear indirect utility function $V(p, Y) = v(p) + Y$ and aggregate farm profits are described by the indirect profit function $\pi(p, \theta)$, where p is the price of the agricultural good, Y is income, and θ indexes climate. Applying Roy's identity and Hotelling's lemma, Marshallian surplus in the agricultural market is $MS = \int_{p^e}^{+\infty} -v'(p)dp + \int_0^{p^e} \pi_p(p, \theta)dp$, where p^e is the equilibrium price. The marginal change in Marshallian surplus caused by a change in climate $d\theta$ is therefore $dMS = \left[v'(p^e) + \pi_p(p^e, \theta)\right] dp^e + \pi_\theta(p^e, \theta)d\theta = \pi_\theta(p^e, \theta)d\theta$, where dp^e denotes the change in equilibrium price resulting from climate change and the last equality follows from the fact that $-v'(p^e) = \pi_p(p^e, \theta)$. Therefore, the marginal change in social welfare is given by the partial effect of climate on farm profits, holding output price constant.

and Roberts (2009) find nonlinear temperature effects on corn and soybean yields in the United States, and predict yields to decline by 30-46% before the end of the century even under the slowest warming scenario. Schlenker and Lobell (2010) find robust negative impacts of climate change on crop yields across African countries based on country-level statistics from FAO. Chen et al. (2016) examine climate change impacts on Chinese crops using county-level data and predict corn and soybean yield decreases of 3-12% and 7-19%, respectively, by the end of the century. In France, Gammans et al. (2017) predict end-of-the-century yield declines for winter wheat, winter barley, and spring barley of 21%, 17%, and 34%, respectively. In a study of Japanese rice, Kawasaki and Uchida (2016) find that crop quality decreases due to warming will outweigh potential increases in yield, leading to lower revenue.

The second group of studies uses farm- or field-level records to provide micro-level evidence of climate change impacts on crop yields. Welch et al. (2010) use panel estimation at the farm level for rice in tropical/subtropical Asia and find that yields will decrease in the coming decades because of warming. Tack et al. (2015) estimate yield responses of winter wheat based on a panel of field-trial data in Kansas and highlight the particular damaging effects of extreme low temperatures in the fall and excessive high temperatures in the spring.

2.3 Market Equilibrium Models

The Ricardian and panel methods do not explicitly account for the interaction between different markets and the feedback in prices when evaluating climate impacts. Market equilibrium models have a unique advantage in addressing these issues.

Adams et al. (1990) predict climate change impacts on agriculture by synthesizing information from climate models, crop-growth models, and a spatially explicit agricultural sector model. Their agricultural sector model is a mathematical programming model in which prices and quantities produced are determined endogenously in a partial-equilibrium framework. This method has been applied under various contexts (e.g., Adams et al., 1995; Alig et al., 1997; Adams et al., 1998; Yates and Strzepek, 1998; Chang, 2002). In the most recent version of the model, Reilly et al. (2003) show that climate change will increase global economic welfare (the sum of consumer and producer surpluses from agricultural commodities) by \$3.2-12.2 billion by 2090, but that these gains will be unevenly distributed. In particular, they find that US producers will lose due to lower prices brought about by increased agricultural productivity. These effects contrast with the recent econometric estimates discussed above, which suggest that US agricultural productivity is likely to decline under climate change. While it is difficult to identify the sources of these discrepancies, the consideration of CO_2 fertilization and potential changes in crop varieties, as well as the reliance on agronomic crop growth models in Reilly et al. (2003) could explain their more optimistic predictions regarding agricultural productivity and social welfare.

Beginning with Rosenzweig and Parry (1994), some studies have examined the global impacts of climate change under a general-equilibrium framework, by linking a set of national-level agricultural sector models with international trade (e.g., Parry et al., 1996, 1999; Fischer et al., 2002). Recent results document that climate change is likely to decrease world crop yields by 9-22% by 2080, with significant regional disparities and increased risk of hunger (Parry et al., 2004, 2005).

Another strand of literature relies on computational general-equilibrium (CGE) models, especially GTAP, to simulate the impacts on global food supply of various climate change scenarios (e.g., Iglesias and Rosenzweig, 2009; Hertel et al., 2010; Ciscar et al., 2011; Schenker, 2013; Calzadilla et al., 2013). Calzadilla et al. (2013) predict a 2.64% reduction in global agricultural production with welfare losses above $327 billion by 2050 under A1B warming scenario. Schenker (2013) highlight that the spillover effect associated with trade will increase costs in regions under low exposure to climate change.

Costinot et al. (2016) extend the general-equilibrium analysis by connecting micro-level responses to macro-level outcomes. The paper builds a general-equilibrium model of trade among 1.7 million fields covering 50 countries and 10 different crops. By implementing counterfactual simulations, they show that evolving comparative advantage under climate change will guide production substitution and greatly reduce the magnitudes of climate change impacts on crop production. Accounting for this effect, the negative impact on agriculture amounts to a 0.26% reduction in global GDP.

2.4 Adaptability

Understanding the adaptability of agriculture to climate change has become a priority in the economics literature since many studies indicate that climate change will negatively affect agriculture. Fishman (2012) finds that irrigation can effectively mitigate the sensitivity of rice yields to precipitation in India. Moore and Lobell (2014) jointly estimate short-run and long-run response functions using subnational yield and profit data in Europe, and find that adaptation potential would be high for maize, but low for wheat and barley. In the United States, Schlenker and Roberts (2009) and Burke and Emerick (2016) find no significant difference between short-run and long-run responses of corn and soybean yields to heat exposure, suggesting that limited historical adaptation has occurred. In contrast, Ortiz-Bobea and Just (2013) find that earlier planting by around two weeks could reduce corn yield losses by 44%. In the context of rice production in Japan, Kawasaki and Uchida (2016) provide evidence that delaying planting dates can more than offset the negative effects of climate change on crop yield, crop quality, and revenue.

Another strand of literature measures agricultural adaptation based on micro-level survey data. Hassan et al. (2008) survey adaptation strategies of African farms and estimate how individual characteristics influence adaptation decisions. Di Falco et al. (2011) and Huang et al. (2015) instrument self-reported adaptation decisions to estimate benefits associated with crop productivity and profits, respectively. Taraz (2017) shows that farmers in India have adjusted their irrigation investment and the water-intensiveness to medium-run variation in rainfall induced by different monsoon regimes. Economists have also examined whether climatic information significantly affects crop choices under a revealed-reference approach (e.g., Kurukulasuriya et al., 2007; Hassan et al., 2008; Seo and Mendelsohn, 2008b; Wang et al., 2010). The results indicate both temperature and precipitation affect farmers' crop-choice decisions, suggesting that farmers will partly adapt by switching crops.

The effects of potential adaptation, including adjustments in planting time and varieties, have also been discussed in simulations using market equilibrium models (see Hertel and Lobell (2014) for a detailed review). International trade has also been recognized as an important channel for mitigating climate impacts (Jones and Olken, 2010; Lybbert et al., 2014). A useful feature of CGE models is the ability to account for trade effects when evaluating global climate impacts. However, results in Costinot et al. (2016) show that, compared to induced within-country production substitution, international trade plays an almost negligible role in reducing the overall impact of climate change.

Technological improvements are crucial in enhancing agriculture's adaptability to climate change. As a proactive approach, Kaminski et al. (2012) develop a structural econometric model to identify specific targets of research investment in agricultural adaptation to climate change. Some studies have found evidence of improved adaptability through seed breeding. Incorporating field-trial data, Tack et al. (2016) find improved heat-resistance of wheat can be achieved by adopting certain varieties, but the heat-resistance may come at the price of a reduced average yield. Lusk et al. (2017) document that a 17% increase in US corn yields from 1980 to 2015 can be attributed to the adoption of genetically engineered (GE) corn, suggesting the potential critical role of genetic engineering in mitigating climate change impacts on crop yields.

Some institutional obstacles to climate change adaptation have also been discussed in the literature. For instance, crop insurance programs may have reduced farmers' incentives to carry out costly adaptation (Annan and Schlenker, 2015). Historical institutional arrangements on groundwater use (e.g., California groundwater institutions) can raise the cost of water relocation, disrupting a market method that could be used to confront challenges associated with climate change (Libecap, 2011).

3 Accounting for Adaptation in Econometric Models

From at least the mid-1990s until very recently, economists seem to have been convinced that agriculturalists adapt to climate in the long run in an economically meaningful way (Mendelsohn et al., 1994; Schlenker et al., 2005, 2006; Olmstead and Rhode, 2011). Such conviction may be grounded in the observable fact that agricultural production varies greatly across climates, both across and within countries. Indeed, agricultural specialization could well be the most blatant sign of agricultural adaptation. For a very long time, the empirical debate has thus been centered on whether certain econometric approaches such as cross-sectional and panel approaches were well or ill-suited to identifying impacts that *implicitly* allow for such climate adaptation, without actually attempting to *explicitly* measure the extent of it.

Proponents of the Ricardian approach pioneered by Mendelsohn et al. (1994) have argued that adaptation to climate matters and that estimates reflecting how outcomes respond to weather shocks, such as those obtainable from panel approaches, cannot capture the response of economic systems to climate change (Mendelsohn and Massetti, 2017). In the view of the Ricardian literature, the implicit accounting of long-run adaptive behavior in cross-sectional comparisons trumps potential concerns related to omitted variable bias. Even studies that rely on weather fluctuations and panel data to identify causal effects have acknowledged the lack of adaptation accounting as a major weakness of the panel approach with fixed effects (Deschênes and Greenstone, 2007; Schlenker and Roberts, 2009; Deschênes and Greenstone, 2011; Roberts and Schlenker, 2011). This flaw implies a fundamental trade-off between the "clean identification" afforded by the panel approach and the policy relevance of the identified effects (Hsiang, 2016).

Recent econometric work on US crop yields may have put the question of whether climate adaptation actually happens in agriculture at the center of the empirical debate. Studies by Schlenker and Roberts (2009), Schlenker et al. (2013), and Burke and Emerick (2016) all suggest that US crop yields respond similarly to changes in heat exposure in the short and long run, signaling little climate adaptation. If farmers do not respond to climate change in a way that is meaningfully different from how they respond to weather shocks, then using weather fluctuations to identify climate impacts could be perfectly justified. Of course, whether climate adaptation is present remains an empirical question, the answer to which may well be context-specific.

3.1 The nonlinear panel approach

A group of recent studies has emphasized that whenever weather variables enter nonlinearly in a panel model with fixed effects, e.g., in a quadratic fashion, cross-sectional variation in climate participates in coefficient identification (e.g., Auffhammer and Schlenker, 2014). Based on this observation, researchers have suggested that impact estimates derived from nonlinear panel models with fixed effects at least partially capture long-run adaptation to climate (McIntosh and Schlenker, 2006; Lobell et al., 2011; Burke et al., 2015; Schlenker, 2017; Blanc and Schlenker, 2017). Yet the extent to which such estimates should be thought of as inclusive of long-run adaptation remains unclear.

Whether and how much damage estimates obtained from nonlinear panel data reflect the underlying long-run adaptation potential is, of course, critical to their relevance for climate policy. One legitimate fear is that overly pessimistic short-run estimates in a context where significant adaptation potential exists might steer policy makers into making suboptimal policy choices or misdirecting public funding aimed at addressing the impacts of climate change.

Mérel and Gammans (2017) derive an analytical expression for the bias of the nonlinear panel estimator (relative to the underlying long-run response of interest) in the context of the quadratic specification in weather variables discussed in McIntosh and Schlenker (2006). The data-generating process (DGP) explicitly allows for both long-run adaptation to climate and short-run responses to weather, however the estimating equation only relates the economic outcome to weather, as in most existing panel studies. The authors first show that in addition to the actual extent of long-run adaptation undertaken by agents, skewness in the historical weather data conditional on location is an essential driver of the bias in the myopic estimates obtained from the panel model relative to the underlying long-run values. This skewness can actually cause bias in *either* direction.

Mérel and Gammans (2017) then show that in the absence of skewness, the myopic panel coefficient estimates can be written as a convex combination of the underlying short-run and long-run coefficients. The decomposition reveals that the estimates reflect long-run values whenever the cross-sectional variation in climate "dominates" the location-specific weather fluctuations. Said differently, panel estimates of the weather-outcome quadratic relationship can be thought of as a weighted average of short- and long-run responses, with the weight on the long-run parameters increasing with the share of the overall weather variation attributable to cross-sectional differences. In large countries like the United States where locational variation in climate dominates short-run weather fluctuations, existing panel estimates should thus be considered as already reflecting a significant share of the historical climate adaptation. Calculations for quadratic models indicate that panel coefficient estimates obtained from county-level weather data across the years 1950–2015 are heavily weighted towards long-run parameter values, namely 98% for average spring-summer temperature and 67% for precipitation.

3.2 The long-differences approach

While cross-sectional differences in climate represent, in the eyes of many, a useful source of climate variation in empirical studies, estimates of climate impacts that rely exclusively upon them are potentially subject to omitted variables bias. In this context, the presence of historical climate trends represents a promising–although relatively untapped–alternative source of climate variation.

Burke and Emerick (2016) exploit this variation by estimating the effect of heat on US crop yields in a long-differences linear regression framework. In order to better understand how the long-differences approach may capture long-run adaptation, let δ_S denote the short-run response of yield to weather shocks and δ_L denote the long-run response of yield to climate. Suppose that yield is given by the following DGP:

$$y_{it} = \delta_S w^*_{it} + \delta_L c^*_{it} + \alpha_i + u_{it} \tag{1}$$

where c^*_{it} and w^*_{it} denote climate and weather shocks, respectively, which might not be observed separately.[3] Instead, realized weather $x_{it} = c^*_{it} + w^*_{it}$ is observed. Burke and Emerick (2016) propose to compare the fixed effects (FE) estimator with the long-differences (LD) estimator of a regression of yield on realized weather in order to test for the presence of long-run adaptation, i.e. $\delta_S \neq \delta_L$.

The linear FE model is given by the following:

$$y_{it} = \beta_{FE} x_{it} + \alpha_i + u_{it}. \tag{2}$$

[3]This DGP is also used by Burke and Emerick (2016) to justify their estimation framework.

Assume that $E[u_{it}|x_{it}, \alpha_i] = 0$ and let $E[c^*_{it}|x_{it}, \alpha_i] = \rho_c x_{it}$ and $E[w^*_{it}|x_{it}, \alpha_i] = \rho_w x_{it}$. Then,

$$\begin{aligned} E[y_{it}|x_{it}, \alpha_i] =& \delta_S E[w^*_{it}|x_{it}, \alpha_i] + \delta_L E[c^*_{it}|x_{it}, \alpha_i] + \alpha_i \\ =& (\delta_S \rho_w + \delta_L \rho_c) x_{it} + \alpha_i. \end{aligned} \tag{3}$$

It follows that the marginal effect $\partial E[y_{it}|x_{it}, \alpha_i]/\partial x_{it} = \delta_S \rho_w + \delta_L \rho_c = \beta_{FE}$, the probability limit of the FE estimator. If we assume $\rho_w = 1 - \rho_c$,[4] then β_{FE} is a weighted average of δ_S and δ_L.[5]

The LD estimator, on the other hand, is the OLS estimator of the following model:

$$\bar{y}_{i\tau_2} - \bar{y}_{i\tau_1} = \beta_{LD}(\bar{x}_{i\tau_2} - \bar{x}_{i\tau_1}) + \bar{u}_{i\tau_2} - \bar{u}_{i\tau_1} \tag{4}$$

where for a variable z_{it}, $\bar{z}_{i\tau_1} = \sum_{t=\tau_1}^{\tau_1+s} z_{it}/s$ and $\bar{z}_{i\tau_2} = \sum_{t=\tau_2}^{\tau_2+s} z_{it}/s$, for $\tau_2 > \tau_1 + s$. If $E[\bar{x}_{i\tau_2} - \bar{x}_{i\tau_1}] = E[\bar{c}_{i\tau_2} - \bar{c}_{i\tau_1}]$, i.e., $E[\bar{w}_{i\tau_2}] = E[\bar{w}_{i\tau_1}]$, then β_{LD} only uses variation in climate and not transitory weather shocks, i.e., $\beta_{LD} = \delta_L$. However, the identification of β_{LD} relies on long-run within-variation in climate.[6] In the absence of such variation, δ_L is not identified.[7]

Under the above assumptions, the difference between β_{FE} and β_{LD} is indeed related to the difference between δ_S and δ_L. If $\delta_S = \delta_L$, i.e., there is no adaptation, then $\beta_{FE} = \beta_{LD}$. However, suppose that δ_S and δ_L are both strictly positive and $\delta_L > \delta_S$, as illustrated in the following diagram,

$$0 \quad \delta_S \qquad\qquad \underbrace{(1-\rho_c)\delta_S + \rho_c \delta_L}_{\beta_{FE}} \quad \underbrace{\delta_L}_{\beta_{LD}}$$

then the difference between β_{FE} and β_{LD} will depend on ρ_c, which is the proportion of climate variability out of total weather variability. The larger ρ_c, the closer β_{FE} and β_{LD} will be, even if δ_S and δ_L are different from each other. Hence, the power of the test based on the difference between the FE and LD estimators may be compromised in finite samples, and a non-rejection of such a test has to be interpreted with caution. Moreover, even in large samples, the difference between β_{FE} and β_{LD} will not reflect the true adaptation potential. Intuitively, if climate trends are present, then these trends will participate in the identifying weather variation used in the FE model, which implies that the FE estimator will be "contaminated" by adaptation and will not purely reflect the short-run response. Although the LD estimator will still provide an unbiased estimate of the long-run effect, inference regarding the actual extent of adaptation and its economic significance is compromised.

[4]This holds if c^*_{it} and w^*_{it} are jointly normal and independent.

[5]This decomposition is conceptually distinct from that derived in Section 3.1, which uses a quadratic specification.

[6]In practice, models include at least state-level time trends, so the variation used for estimation is residual variation. This type of variation may be small in practice.

[7]In this case, if $E[\bar{w}_{i\tau_2}] \neq E[\bar{w}_{i\tau_1}]$, then the long-differences transformation is simply a different transformation that removes α_i and allows us to estimate δ_S consistently.

4 Challenges with the Panel Approach

As mentioned above, the FE model is the most widely used method to estimate climate change impacts using panel data. Though practitioners tend to choose models that are linear in the parameters, a flexible specification of the regressors is typically used. Let i denote a county, t denote a year, and s a state or province, the relationship between an outcome y_{it} and a function of the weather variables, which are observed at a higher frequency (e.g., daily), is estimated by the following, for $i = 1, \ldots, n$ and $t = 1, \ldots, T$

$$y_{it} = \mathcal{X}_{it}'\beta + z_{it}'\gamma + \alpha_i + \lambda_t + \gamma_s f(t) + \epsilon_{it} \tag{5}$$

where $\mathcal{X}_{it}$ is a $k \times 1$ vector of functions of the underlying weather variables, $\mathcal{X}_{it} = g(\{W_{ith}\}_{h=1}^{H})$. For instance, if we observe temperature, T_{ith}, and precipitation, P_{ith}, for each day h in year t, then $W_{ith} = (T_{ith}, P_{ith})'$. Suppose we include average temperature, $\bar{T}_{it}$, and precipitation, $\bar{P}_{it}$, as well as their squares, then $\mathcal{X}_{it} = (\bar{T}_{it}, \bar{T}_{it}^2, \bar{P}_{it}, \bar{P}_{it}^2)'$.[8] Other popular choices are temperature and precipitation bins as well as degree days and total precipitation. The above model is flexible in its accommodation of nonlinearities in weather variables, however it is linear in time-invariant and time-varying unobservables, typically referred to as "fixed effects." It assumes away response heterogeneity across cross-sectional units, i.e., β is not allowed to vary across states and counties. In this section, we discuss econometric issues that may arise in this empirical setting.

4.1 Multicollinearity in Binned Regressions

The recent statistical yield literature has emphasized the role of extreme temperature on crop yields (Schlenker and Roberts, 2009; Tack et al., 2015). When estimating the historical relationship between weather and yield, it is therefore essential that the included right-hand-side weather variables capture, in some way or another, exposure to extreme temperatures. Using average daily temperature would likely mask such exposure as, for instance, exposure to extreme heat during hot summer days may be concealed through averaging with exposure to cooler night temperatures. Given that crops may respond dramatically to exposure to hot temperatures (e.g., in excess of 30°C), it is evident that models based on average daily temperatures will often fail to account for the negative effects of extreme heat on crop yields. The standard solution, in the absence of hourly temperature data, is to infer from daily minimum and maximum temperature data the within-day distribution of temperature, using a sine or linear interpolation (Schlenker and Roberts, 2009; Burke and Emerick, 2016; Gammans et al., 2017).

At the same time, the literature has uncovered nonlinear, and often non-monotonic relationships between crop yields and temperature (Schlenker and Roberts, 2009), confirming agronomic knowledge about plant growth processes (Hertel and Lobell, 2014) and suggesting the need for flexible models capable of capturing such nonlinear effects. One popular specification consists of "binning" cumulative temperature

[8] $\bar{T}_{it} = \sum_{h=1}^{H} T_{ith}/H$ and $\bar{P}_{it} = \sum_{h=1}^{H} P_{ith}/H$.

exposure across the growing season within relatively narrow temperature intervals and allowing for flexible effects of exposure to each temperature bin on crop yield (Schlenker and Roberts, 2009; Chen et al., 2016; Gammans et al., 2017; Schauberger et al., 2017). In the least restrictive specification, exposure to each temperature bin is allowed to have an effect on yield independent of that of adjacent bins.

An issue that has been overlooked in the literature is the effect of interpolating within-day exposure from daily data on regressor multicollinearity in the flexible binned regression framework. Because the within-day interpolation fits a continuous curve between the minimum and maximum temperatures of consecutive days, exposures to adjacent temperatures (i.e., times spent at adjacent temperatures across the season) are systematically correlated in the interpolated data. This partially "constructed" multicollinearity typically results in very unstable relationships between temperature exposure and yield whenever temperature bins specified in the regression are narrow, so one solution may be to consider wider bins. At the same time, if temperature intervals are too large, critical non-linearities or non-monotonicities may go undetected by the model, and thus warming impact estimates may suffer from severe misspecification bias.

[Figure 1 about here.]

Figure 1 illustrates this point using temperature exposure data from 88 French departments during the months March–July, the warm season for cereal crops like wheat and barley. The weather data covers the period 1950-2016.[9] The figure depicts the assumed underlying relationship between temperature exposure and yield as a piecewise linear function. Log yield increases with exposure to moderate to warm temperatures but decreases with exposure to temperatures above 28°C. A data set of department-level crop yields is generated by adding an i.i.d disturbance term to the central yield implied by this non-monotonic relationship.[10]

A set of binned regression models is then estimated using ordinary least squares, with the size of the bins increasing from 1°C to 8°C. Results are represented in panels 1(a)–1(d), where the boxes indicate the 95% confidence interval around bin estimates. It is clear that for the smaller bins, adjacent estimates fluctuate quite a bit from the underlying parameter values, with very large confidence intervals. Although the underlying temperature-yield relationship only exhibits non-monotonicity

[9] The department-level weather data are constructed from the gridded dataset E-OBS version 14.0, made available by the EU-FP6 project ENSEMBLES (http://ensembles-eu.metoffice.com) and the data providers in the ECA&D project (Haylock et al., 2008).

[10] Specifically, log yield in department i and year t is constructed as $\ln y_{it} = \sum_{h=0}^{36} g(h+0.5)\phi_{it}(h) + \epsilon_{it}$, where $g(h) = \begin{cases} 0 & \text{if } h \leq 8°C \\ 0.0005h - 0.004 & \text{if } 8 \leq h \leq 28°C \\ 0.43 - 0.015h & \text{otherwise} \end{cases}$, $\phi_{it}(h)$ is the time in days spent between temperatures h and $h+1$, and $\epsilon_{it} \sim \mathcal{N}(0, 0.4)$. Although the literature has insisted on accounting for spatial correlation in real data (Auffhammer et al., 2013), our point here is about multicollinearity, therefore we assume independence across space and time in the data generating process and the regression framework. Figure 1 plots the relationship $g(h)$, which is interpreted as the percentage change in yield caused by replacing one day of exposure below 0°C by one day at a given temperature.

at $h = 28°C$, the estimated relationship with 1°C bins is very unstable and shows many non-monotonicities, particularly at higher temperature bins where exposure is scarce. For instance, there is a large upward jump in the marginal exposure effect between the bins 35–36°C and the bin above 36°C, that does not vanish when increasing the bin size to 2°C. The problem for the econometrician is that it is unclear whether such non-linearities reflect true underlying effects or are simply caused by multicollinearity in the exposure data.

In order to confirm that multicollinearity is indeed driving the apparent instability of the estimates, we re-shuffle the temperature exposure data, namely, we randomly permute adjacent bin exposures within 10°C intervals, where the permutation rule varies across departments and years. We then re-construct the yield data based on the underlying data generating process (DGP) and re-estimate the model using OLS.[11] The results appear in panel 1(e) for the 1°C bins and clearly show that the estimated relationship is both smoother and more precisely estimated.

[Figure 2 about here.]

These simulations suggest a trade-off between the size of the bins and the precision and reliability of the estimates. Large bins may fail to capture underlying changes in the weather-yield relationship, which may cause (or aggravate) misspecification bias. Estimates obtained from regressions with narrower bins will typically suffer from multicollinearity, and therefore nonlinearities arising from such models should be interpreted with caution and in light of available agronomic knowledge. If the underlying temperature-yield relationship is "too nonlinear," it will be difficult to distinguish between legitimate non-linearities and mere artifacts from the weather data construction in a binned regression framework.

In addition, estimates obtained from regressions with narrow bins will be more imprecise, and so will the climate change impacts calculated from them. Figure 2 depicts the distribution of climate change impacts implied by the above regressions when considering a uniform warming by +4°C. The warming is assumed to be uniform in space and time (within the growing season), so that exposure to a given bin h under the new climate can be deduced from exposure to the bin $h - 4$ under the reference climate. Impacts are calculated for 1,000 draws of the disturbances. Figure 2 shows that with the simple underlying relationship assumed here, and with a uniform warming, binning with larger intervals does not accentuate misspecification bias and results in more precise estimates.

4.2 Response Heterogeneity

In climate change impact studies, heterogeneous responses at the regional, state or county levels can be of particular interest. In this section, we present the potential bias of the linear FE estimator in the presence of response heterogeneity. To facilitate

[11] The disturbances are taken from the same distribution as in the initial simulation.

presentation, we consider a simple model with a scalar regressor, say annual mean temperature, that exhibits response heterogeneity,

$$y_{it} = \beta_i x_{it} + \alpha_i + u_{it}. \tag{6}$$

Note that by replacing y and x with their residuals after projecting out z_{it}, λ_t and $\gamma_s f(t)$, the results discussed below can be extended to the more general model in Eqn (5).

There are several results in the econometrics literature that establish the potential inconsistency of linear FE estimators in the presence of response heterogeneity (Chernozhukov et al. (2013, Theorem 1), Gibbons et al. (2017, Proposition 1)). In order to understand the key insight behind these results, it is important to first define the average marginal effect of x on y, which is given by

$$E[\beta_i] = \lim_{n\to\infty} \frac{1}{n}\sum_{i=1}^{n} \beta_i, \tag{7}$$

where we assume that we have a random sample of cross-sectional units from the population, and hence would give each unit the same weight.[12] A consistent estimator of $E[\beta_i]$ can be obtained by the plug-in method, specifically

$$\hat{\beta}_{SA} = \frac{1}{n}\sum_{i=1}^{n} \hat{\beta}_i \tag{8}$$

where $\hat{\beta}_i = \sum_{t=1}^{T}(x_{it} - \bar{x}_i)(y_{it} - \bar{y}_i)/\sum_{t=1}^{T}(x_{it} - \bar{x}_i)^2$ is the coefficient from an OLS regression for each unit i.[13]

The linear FE estimator, on the other hand, is an OLS estimator on the within-group transformation of the linear FE model given by the following,

$$y_{it} - \bar{y}_i = \beta_{FE}(x_{it} - \bar{x}_i) + u_{it} - \bar{u}_i. \tag{9}$$

A simple manipulation of the linear FE estimator quickly illustrates that it is a variance weighted average of the estimators of the unit-specific slope coefficients, $\hat{\beta}_i$,

$$\hat{\beta}_{FE} = \frac{\sum_{i=1}^{n}\sum_{t=1}^{T}(x_{it} - \bar{x}_i)(y_{it} - \bar{y}_i)}{\sum_{i=1}^{n}\sum_{t=1}^{T}(x_{it} - \bar{x}_i)^2} = \sum_{i=1}^{n} \underbrace{\frac{\sum_{t=1}^{T}(x_{it} - \bar{x}_i)^2}{\sum_{i=1}^{n}\sum_{t=1}^{T}(x_{it} - \bar{x}_i)^2}}_{\text{variance weight}} \underbrace{\frac{\sum_{t=1}^{T}(x_{it} - \bar{x}_i)(y_{it} - \bar{y}_i)}{\sum_{t=1}^{T}(x_{it} - \bar{x}_i)^2}}_{\hat{\beta}_i}.$$

[12] When we have county-level data, other weights, such as acreage shares or population proportions, may be more appropriate. The weighted average would yield a definition similar to β_{ATE} in Gibbons et al. (2017). We also assume that we can identify β_i for all units i in the population, this implicitly assumes that $var(x_{it} - \bar{x}_i) > 0$ for all i. If this assumption does not hold, one can look at the average marginal effect of the identified units, i.e. $E[\beta_i | var(x_{it} - \bar{x}_i) > 0]$. Chernozhukov et al. (2013) refer to this object of interest as the "identified effect."

[13] This equation is only estimable if we have within-unit variability in the weather, $\sum_{t=1}^{T}(x_{it} - \bar{x}_i)^2 > 0$.

Suppose our cross-sectional units were counties, the above shows that the coefficient estimates for counties that experience proportionately greater inter-annual variability in temperature are given higher weight in the FE estimator.[14] This can lead to a downward bias in the estimates of the average effect of temperature on the outcome in practice, if we expect counties that experience higher variability to take measures to adapt and hence be less sensitive to temperature changes. In general, however, the sign and magnitude of the bias will depend on the empirical setting.

We illustrate this problem with a simulation design, where the outcome follows a random coefficient model and x_{it} follows a first-order auto-regressive process, given by the following, for $i = 1, \dots, n$, $t = 1, \dots, T$,

$$\begin{aligned}
& y_{it} = \beta_i x_{it} + \alpha_i + u_{it}, \text{ where } \beta_i = 0.5\alpha_i^3, \\
& x_{it} = 0.5x_{i,t-1} + \epsilon_{it}, x_{i0} \overset{i.i.d.}{\sim} N(a_1, 0.5) + N(a_2, 0.5) \\
& \alpha_i \overset{i.i.d.}{\sim} N(\bar{X}_i, std(X_i)), \text{where } std(X_i) = \sqrt{\sum_{t=1}^{T}(X_{it} - \bar{X}_i)^2/T} \\
& \epsilon_{it} \overset{i.i.d.}{\sim} N(0, 1), u_{it} \overset{i.i.d.}{\sim} N(0, 1). \qquad (10)
\end{aligned}$$

Figure 3 presents the simulation distribution of the FE estimator, $\hat{\beta}_{FE}$, and the sample average estimator, $\hat{\beta}_{SA}$. The simulation design provides numerical evidence that the bias of the linear FE estimator can be upward, downward or small. It also gives an example where the sampling variability of $\hat{\beta}_{FE}$ is larger than that of $\hat{\beta}_{SA}$.

[Figure 3 about here.]

The simulation design illustrates that in the presence of response heterogeneity, practitioners should use a consistent estimator of the average marginal effect in the spirit of $\hat{\beta}_{SA}$ or the weighted FE estimator proposed in Gibbons et al. (2017).

4.3 Model Selection

As pointed out in the discussion of Eqn (5), one of the key choices empirical researchers make in the climate change literature is the summary statistics of daily weather variables that constitute the regressors $\mathcal{X}_{it}$ in the linear FE model. This is a clear model selection choice that empirical researchers make, yet few discuss this choice explicitly. Schlenker and Roberts (2009) use Monte Carlo cross-validation (MCCV) to illustrate that a particular model minimizes mean squared out-of-sample prediction error relative to other models. Other papers such as Gammans et al. (2017) follow a similar procedure. There are several variants of cross-validation. For a recent review of the statistical literature, see Arlot and Celisse (2010). The MCCV procedure used in the aforementioned papers selects among several models, $\mathcal{M}_1, \mathcal{M}_2, \dots, \mathcal{M}_\ell$, as follows,

[14]It is worth noting that if the effect of temperature on the outcome is homogeneous across counties, the variance weighting is "efficient" since higher variability imply greater precision. However, when response is heterogeneous across counties, this weighting can lead to inconsistent estimation.

1. Repeat steps (a)-(c) for $b = 1, 2, \ldots, B$:

   (a) Randomly re-sample years into a training and testing sample of size T_c and T_v, respectively, where $T_c/T \simeq 0.85$,

   (b) Using the training data set, estimate the parameters of each model $\hat{\mathcal{M}}_1^b, \ldots, \hat{\mathcal{M}}_\ell^b$,

   (c) Estimate the out-of-sample prediction error, $\hat{\epsilon}_{it}^{j,b}$, for each i, t observation in the b^{th} testing sample for each model j, using the estimates $\hat{\mathcal{M}}_j^b$, and construct

$$\hat{\Gamma}_{b,nT}^{j} = \frac{1}{nT_v} \sum_{i=1}^{n} \sum_{t=1}^{T_v} (\hat{\epsilon}_{it}^{j,b})^2 \tag{11}$$

2. Choose the model that minimizes the average mean-squared error across all Monte Carlo testing samples,

$$\bar{\Gamma}_{nT}^{j} = \frac{1}{B} \sum_{b=1}^{B} \hat{\Gamma}_{b,nT}^{j} \tag{12}$$

   among models $\mathcal{M}_j$ for $j = 1, \ldots, \ell$.

The desirable properties of a model selection procedure, such as MCCV, are asymptotic optimality and/or consistent model selection. Asymptotic optimality or loss efficiency ensures that as sample size grows, the procedure in question chooses the model with the smallest mean squared error. Consistent model selection ensures that if a number of correct models are under consideration, the most parsimonious among them is chosen with probability approaching 1 as sample size grows. This is important for out-of-sample performance, since it is well-known that in-sample fit is inversely related to out-of-sample fit. For linear regression models with independent and identically distributed (i.i.d.) data, Shao (1993) explains why leave-one-out cross-validation (LOOCV) has a tendency to over-fit. Furthermore, for MCCV to deliver consistent model selection, the condition $T_v/T \to 1$ has to hold as $T \to \infty$, i.e. with larger samples, proportionately larger test samples should be chosen. For instance, this condition is satisfied for the training sample size $T_c \simeq T^{3/4}$. If $T = 50$, this condition would yield a training sample that is only 38% of the total sample, whereas the testing sample would equal 62% of the total sample size. These results only hold under the assumption that the correct model is finite-dimensional. Shao (1997) points out that MCCV with $T_v/T \in (0, 1)$, such as the version of MCCV used in the aforementioned papers, is a compromise between LOOCV and MCCV with $T_v/T \to 1$, and hence will perform worse than MCCV with $T_v/T \to 1$ if the correct model is finite-dimensional.[15]

Another concern with the MCCV implementation in the climate change literature is that formal results on the validity of MCCV are based on re-sampling i.i.d. observations. This assumption ensures that the estimators obtained from the training

[15] Note that LOOCV is consistent if the correct model is infinite-dimensional (Shao, 1997).

sample are independent of the observations in the testing sample. This is important to formally justify cross-validation. In the case of dependent data, its properties are not formally established (Arlot and Celisse, 2010), even though cross-validation tends to perform well in simulations despite the presence of dependence. Re-sampling years in the panel data context deviates substantially from the i.i.d. assumption since the "fixed effects" of the cross-sectional units introduce very strong correlation between the data in the training and testing samples. Finally, once a model selection choice is made, classical inference methods are no longer valid. Future work is required to obtain formally justified procedures for model selection that are robust to data dependence and deliver valid post-selection inference.

5 Conclusion and Directions for Future Research

History has taught us that even with a fairly stable climate, the globe has experienced devastating food crises and famines due to adverse weather events. Climate scientists now predict that in most food producing regions of the world, the future climate will be different from the past. Could increased climate variability result in more periodic food crises and, if so, what are the implications for feeding a growing population and what are the likely policy responses? This paper reviews the findings of climate impacts on agriculture, and discusses some methodological issues in empirically measuring the effects of climate change, including climate adaptation potential.

A comprehensive assessment of the state of current scientific knowledge regarding the impact of climate change on world agriculture would require reviewing literatures beyond economics. But what has the economic literature taught us on the topic to date? First, although studies are still lacking for many developing countries, most econometric estimates of crop yield impacts are negative. Given that developing countries tend to be located in hot regions that are predicted to get hotter, one can speculate that global crop productivity will decline under warming. Second, when put into the context of economywide GDP, these losses are predicted to be small, -0.34% of world GDP according to the study by Costinot et al. (2016), although this small average effect could mask large negative effects for some countries. Third, climate adaptation could mean that these effects will even be smaller in the long run, if agents and/or institutions can effectively respond to changes in climate. This is perhaps the area of research that is in most need of development, including from a methodological standpoint. From an identification perspective, using times-series variation in climate, in the spirit of Burke and Emerick (2016), might represent the most convincing framework for relating climate to outcomes (such as profits) or behavior (such as crop choice) that include long-run adaptation.

Let us also point out some other directions for future research. The use of degree-days variables is a critical development in the literature as they better characterize the differential effects of heat accumulated over the growing season. This development has also motivated a series of innovations on flexible specifications to model potential nonlinear effects in the response relationship. Despite some work in this area (e.g., Welch et al., 2010; Ortiz-Bobea and Just, 2013; Tack et al., 2015), the effects that are specific to different stages have not received much attention. Given the seasonal heterogeneity in climate change, future work should further address the stage-specificity in measuring climate impacts on agriculture.

Blanc and Schlenker (2017) make an important observation that pollution is correlated with weather shocks. Recent studies find that air pollution, specifically surface ozone, negatively affects crop yields (Boone et al., 2013; McGrath et al., 2015; Carter et al., 2017). This raises a potential omitted variable problem in identifying climate impacts on crop yields when only including weather and climatic variables as regressors. Future research should take this concern into account and carefully evaluate the potential bias caused by omitted variables, such as pollution.

Many discussions in this review, as well as in the literature, have focused on farmland values, revenues, and crop yields. But it is also important to document climate impacts on other aspects of agriculture. Kawasaki and Uchida (2016) point out the impacts of climate change on crop quality can be more negative than on crop yields. Lee and Sumner (2015) uncover moderate acreage responses to changes in local climate in Yolo County, California. Examining corn and soybeans in Brazil, Cohn et al. (2016) suggest that climate change impacts on crop acreage and cropping frequencies can be potentially larger than on crop yields. The consequences of climate policy on land use, including the induced shift between cropland and grassland, and implications for the livestock sector, have also been discussed (Golub et al., 2013; Fezzi et al., 2015). Given livestock's importance in global agriculture, it is surprising how little attention economists have devoted to understanding climate change impacts on this sector.

More frequent extreme weather events clearly pose a challenge to food security. In fact, such events have greater potential to disrupt the global food equation than does a slow warming trend. It is important to recognize that past food crises have generated policy responses that made the situation worse (Timmer, 2010). For instance, hoarding and export embargoes occurred during the 2007-08 food crisis (Carter et al., 2011). More research is hence needed to understand the impacts of extreme weather events and their policy implications.

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List of Figures

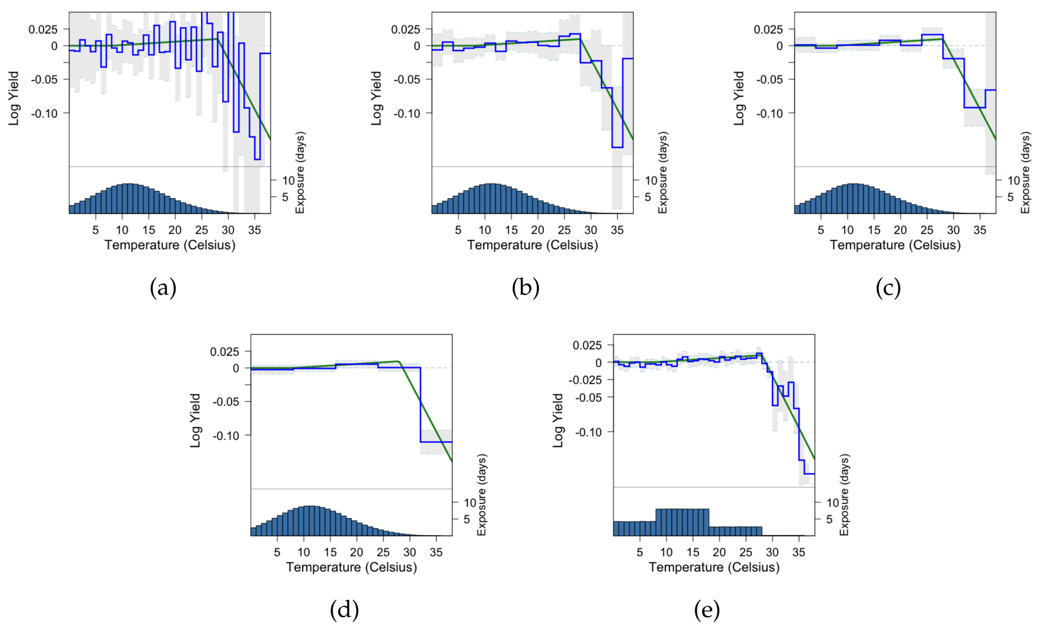


Figure 1 Effect of bin size on estimates of temperature effects

Notes: Green piecewise linear curve represents the underlying relationship. Estimates of temperature exposure effects obtained from specifications with: (a) 1°C bins, (b) 2°C bins, (c) 4°C bins, (d) 8°C bins, (e) 1°C bins from re-shuffled exposure data. Histograms at the bottom of each panel represent the mean exposure in the underlying weather data. Gray bands represent 95% confidence intervals around point estimates.

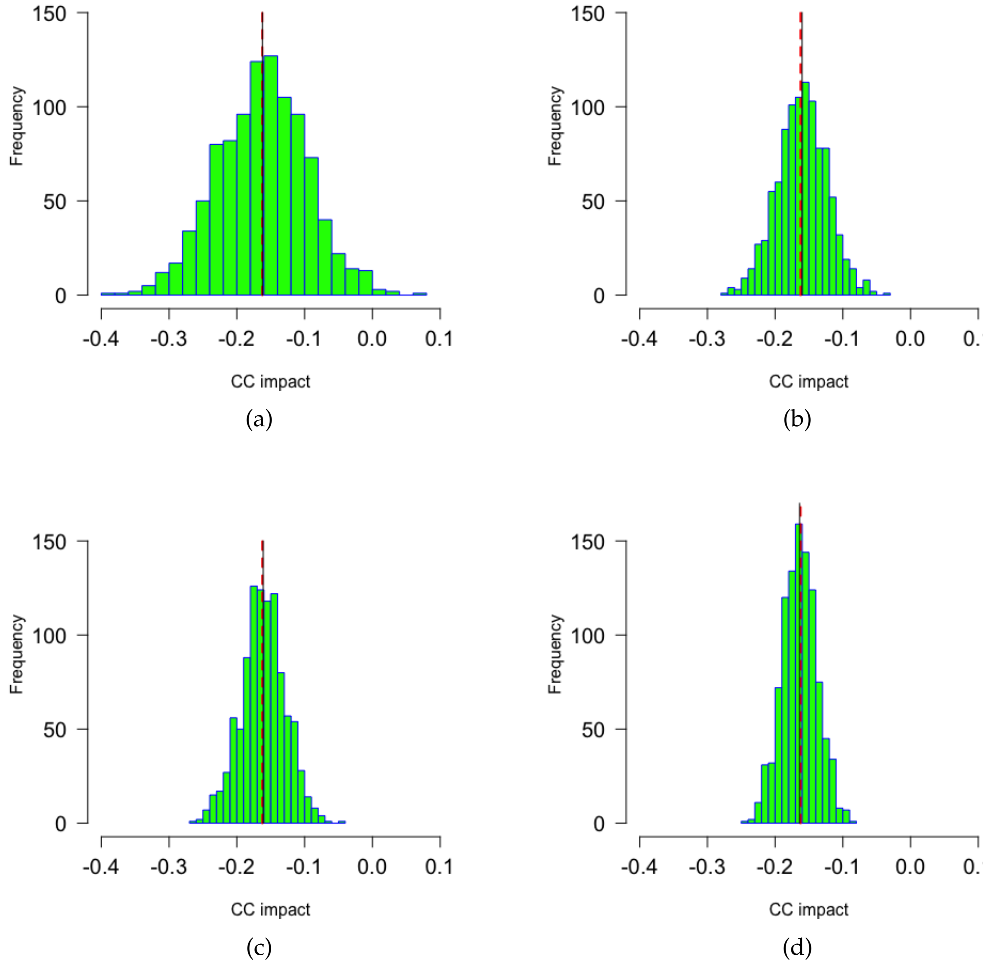
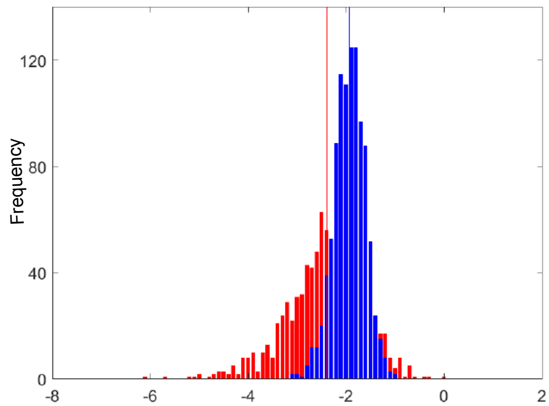
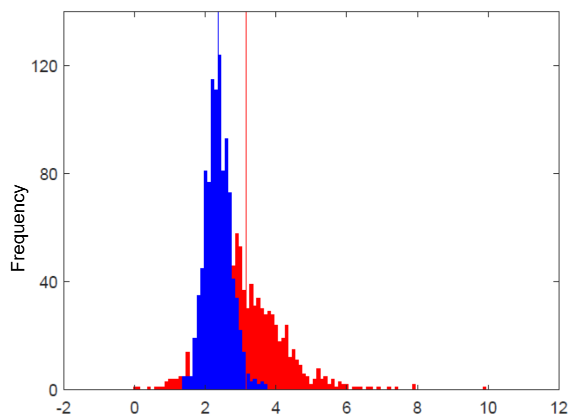


Figure 2 Effect of bin size on calculated warming impacts

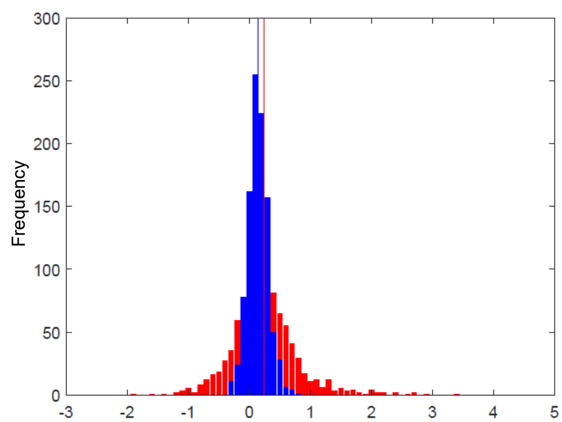
Notes: True impact represented by the dashed red line. Mean calculated impact represented by the solid black line. Histogram shows the distribution of calculated impacts across 1,000 draws of the disturbances. Impacts are obtained from specifications with: (a) 1°C bins, (b) 2°C bins, (c) 4°C bins, (d) 8°C bins.



(a)



(b)



(c)

Figure 3 Simulation distribution of $\hat{\beta}_{FE}$ vs. $\hat{\beta}_{SA}$

Notes: The simulation distribution of $\hat{\beta}_{FE}$ ($\hat{\beta}_{SA}$) is in red (blue) for $n = 500$, $T = 5$ and $1,000$ simulation replications. The vertical lines denote the mean of the distribution in question. The simulation design is given in Equation (10). In (a), X_{i0} is generated using $a_1 = -6$, $a_2 = 2$, in (b) $a_1 = -2$, $a_2 = 6$, and in (c) $a_1 = -4$, $a_2 = 4$.