The empirically inscrutable climate-economy relationship

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Abstract

Empirical models of the macro-level climate-economy relationship project climate change impacts ranging from trivial to catastrophic. This wide range suggests identification challenges. Here, we show that such models indeed face identification challenges that are axiomatic and irreducible. The climate-economy relationship has qualitatively important variation across space and time, due to complex regional heterogeneity and affluence-driven adaptation, among other factors. Empirical models must assume away some of this variation to preserve degrees of freedom. Influential observations caused by economic disasters and miracles, along with low signal-to-noise ratios, create additional challenges. We illustrate these challenges theoretically and then empirically by replicating and sensitivity-testing prominent climate-econometric studies. These estimation challenges create major—not minor—sensitivities and fragilities. We recommend a cautious and uncertainty-emphasizing approach to applying climate econometrics to public and private decision making. Importantly, however, our analysis does not dismiss the existence of a negative carbon externality nor economically beneficial efforts to reduce greenhouse gas emissions.

JEL Codes: O44, O47, Q50, Q54, Q56

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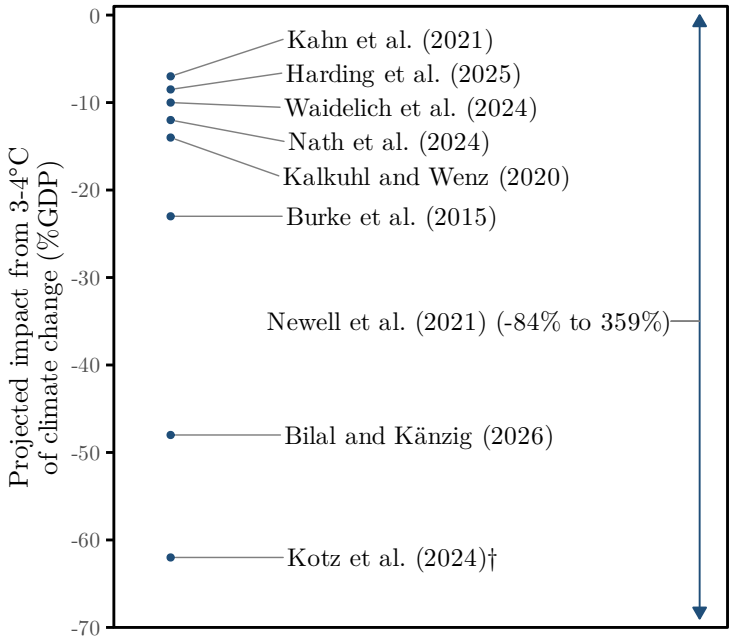
1 Introduction

Climate change can affect the economy through several channels (Stern, 2008; Hsiang and Kopp, 2018; Intergovernmental Panel on Climate Change (IPCC), 2022). Changing temperature and weather patterns can stress agriculture (Hultgren et al., 2025) and physical infrastructure (e.g., Burillo et al. (2019); Clark et al. (2019); Salimi and Al-Ghamdi (2020)). More frequent or intense extremes can create more frequent disaster events, affecting infrastructure, other economic activities, and financial markets (Monasterolo, 2020; Newman and Noy, 2023). Changes in heat and cold can affect mortality, morbidity, burdens of disease (Gasparrini et al., 2015; Mora et al., 2017; Carleton et al., 2022; Van de Vuurst and Escobar, 2023), and perhaps even violence, crime, and conflict (Burke et al., 2015b; Carleton and Hsiang, 2016). Sea-level rise threatens low-lying coastal infrastructure (Adshead et al., 2024). Changes in weather patterns, pollution related to greenhouse gas (GHG) emissions, or the frequency of events such as wildfires can affect air quality (Arshad et al., 2024). Regional impacts of climate change may alter human migration patterns with economic effects on both source and destination societies (Kaczan and Orgill-Meyer, 2020; Marotzke et al., 2020).

The potential policy significance of the climate-economy relationship cannot be overstated. The main sources of GHG emissions are bedrocks of the economy and society: energy, food systems, heavy industry, and cement manufacturing, to name a few (Lamb et al., 2021). At the same time, some estimates of economic damages from climate change imply negative externalities large enough to motivate emissions-mitigation efforts on scales equivalent or greater than major-war efforts (Bilal and Känzig (2026); see also Moore et al. (2024); Tol (2025b, 2024) for review). If these estimates are accurate, then society must figure out how to trade off its current economic foundations against climate risks large enough to jeopardize these foundations. In contrast, other estimates of the climate-economy relationship imply trivial or even positive economic effects of climate change (e.g., Newell et al. (2021); Greßer et al. (2021); Lakhtakia and McKitrick (2025)). If *these* estimates are accurate, then costly

climate policies based on inaccurately high damage estimates threaten to do serious and unnecessary economic harm (e.g., Lomborg (2020); Tol (2025a); Burgess et al. (2026)). Still other estimates of the climate-economy relationship span a wide range in between these two extremes (Figure 1; see Tol (2024, 2025b)).

Figure 1: Projected Global Economic Impacts from Climate Change



Notes: This figure depicts the various headline projected global economic impacts from climate change by the end of the 21st century from top-down studies. Impact is defined as the effect of climate change on global GDP or GDP per capita levels at years 2100 or 2099 at 3-4°C of warming. † denotes that the paper was retracted.

To illustrate the implications of this uncertainty more concretely, consider that the global economy produces roughly 3500 U.S. dollars (USD) of gross domestic product (GDP) for every tonne of CO_2 emissions (Our World in Data, 2025). Estimates of the social cost of carbon (SCC)—the marginal negative climate change externality—from major recent studies range from roughly 30 USD per tonne (Nordhaus, 2017) or less to roughly 1300 USD per tonne (Bilal and Känzig, 2026) (see Moore et al. (2024) for review). At the low end of this range, we should be willing to pay very little cost (1% of GDP or less) to mitigate our GHG emissions. At the high end of this range, we should be willing to mobilize nearly half of the

global economy—a greater proportional effort than the U.S.'s World War II effort, applied globally.

Driven by these potentially enormous stakes, key institutions of society have been increasingly making large (i.e., trillions of dollars or more) implicit bets on what the nature of the climate-economy relationship is. For instance, insurance markets are attempting to price in climate risk (Ingels et al., 2024). Consulting firms are selling climate-risk-evaluation products to other firms and governments (Christensen and Collington, 2024) (see also Weinkle (2020)). Central banks are developing and using tools for estimating climate risk (e.g., Network of Central Banks and Supervisors for Greening the Financial System (NGFS) (2025)). Government policies explicitly (e.g., via carbon pricing) or implicitly (via policies affecting GHG emissions) make assumptions about climate risk and its economic tradeoffs (e.g., Agnolucci et al. (2024); Stechemesser et al. (2024)).

In making these decisions, many institutions use a relatively narrow range of assumptions rather than integrating over the wide range of estimates that exist. For example, NGFS (central bank) damage scenarios (Network of Central Banks and Supervisors for Greening the Financial System (NGFS), 2025) are based on a now-retracted study (Kotz et al. (2024); see also Bearpark et al. (2025); Schötz (2025); Kotz et al. (2025)), which estimated climate damages on the extreme high end (two-thirds of GDP by 2100), based on an extreme climate change scenario (RCP8.5) that climate and energy scholars widely consider implausible (Ritchie and Dowlatabadi, 2017; Hausfather and Peters, 2020; Burgess et al., 2020; Pielke Jr and Ritchie, 2021; Hausfather, 2025). In contrast, the current U.S. government under President Trump has indicated its intent to rely on estimates implying no economic damage, or even a benefit, from climate change (Climate Working Group, 2025; U.S. Environmental Protection Agency (EPA), 2025). Previous U.S. administrations under Presidents Biden and Obama used specific SCC estimates in their cost-benefit analyses (e.g., 190 USD per tonne by the end of the Biden administration; see Carleton and Greenstone (2022); Janik (2023); EPA (2023)).

These institutions' actions raise the question: is it possible to precisely estimate the climate-economy relationship? In other words, is it possible to determine who is right, among the wide range of existing estimates (Figure 1)? Or, is the climate-economy relationship irreducibly uncertain or inscrutable, given empirical data available currently and in the foreseeable future (and might that explain the wide range of estimates)?

Here, we use a combination of theoretical and empirical analyses to show that the climate-economy relationship is, in fact, empirically inscrutable.

We start by exploring the estimation challenge in a first-principles growth model of the economy using simple and general assumptions. From this model, we show that the climate-economy relationship should be expected to have qualitatively important variation in both space and time, which empirical models must assume away to preserve degrees of freedom. Heterogeneity among countries (or regions) in their locally optimal environments and their sensitivities to climate contribute to spatial heterogeneity. Effects of affluence on economic resilience to climate and climate change contribute to temporal heterogeneity. We show that additional challenges arise from influential points caused by economic disasters and miracles; and high variability of weather compared to climate, affecting the signal-to-noise ratio.

We then conduct an empirical review of prominent climate-econometric studies, in which we illustrate these estimation challenges. We focus on explicitly replicating and analyzing three prominent studies, chosen for having diverse methodologies and conclusions, in addition to their prominence: Burke et al. (2015c), Kahn et al. (2021), and Bilal and Känzig (2026). Each of these studies provides an important contribution to the literature and to our understanding of the problem, but we find evidence in each of the axiomatic estimation challenges that our theoretical analysis reveals. For example, the headline findings of Burke et al. (2015c) and Kahn et al. (2021) are sensitive to a handful (<10 out of more than 6,000) of influential observations, which account for 20% or more of the estimated effect, as well as certain decades of variation. Simple controls for El Niño, long-run variation, and cyclicality attenuate the estimates of Bilal and Känzig (2026) by 50% or more, in some instances. We

then discuss the broader literature in relation to our findings.

Finally, we discuss the implications of our results for public and private decision-making. Specifically, we argue for policymaking approaches that account for deep uncertainty in the climate-economy relationship, instead of hyper-focusing on preferred specifications. We also call for greater focus in climate change policy on mitigation efforts with short-run economic co-benefits, and on the multiple benefits of affluence, good governance and economic development to climate-sensitive societal outcomes and their resilience to climate change (Burgess et al., 2026). (Climate-sensitive outcomes include crop yields, damage and death rates from climate-related hazards, and other indirectly affected outcomes such as conflict, violence, and health.) These benefits and their policy implications are largely insensitive to uncertainties in the climate-economy relationship.

We focus our analysis specifically on the challenge of estimating economic damages (or benefits) from climate change, using climate data and aggregate economic data such as output or GDP (e.g., as done by Dell et al. (2012); Burke et al. (2015c); Newell et al. (2021); Kahn et al. (2021); Bilal and Känzig (2026), and others; see Tol (2024)). The other possible approach involves estimating sector-specific effects of climate change, and then aggregating these across sectors to produce an estimate of the total effect (e.g., Nordhaus (1991)). This approach has two fundamental and well-understood challenges (e.g., see Keen (2021)). First, on top of the uncertainty in the estimated effect of climate change on each sector, the interactions between these effects are unknowable. Second, many sectors will invariably have insufficient data to estimate an effect, and thus an effect will need to be assumed when aggregating. Therefore, while sector-specific analyses are useful for understanding climate-change effects on specific sectors, they produce, at best, a partial set of partial effect estimates.

We also do not analyze uncertainties in applications of damage estimates, such as estimating the SCC (see Burke et al. (2015a)). Our argument that damage estimates are irreducibly uncertain implies that any applications of damage estimates are also irreducibly

uncertain. Moreover, the SCC is already known to be irreducibly uncertain due to its high (orders of magnitude) sensitivity to subjective social discounting parameters (Moore et al., 2024; Barrage and Nordhaus, 2024).

Our analysis builds on and generalizes conceptual points about the estimability of the climate-economy relationship raised by a few previous studies. Kolstad and Moore (2020), Dell et al. (2014), and Auffhammer (2018) discuss that panel estimators measure responses to short-run weather fluctuations which neglect adaptation and may not correspond to the effects of permanent climate change (see also Massetti and Mendelsohn (2018)). Newell et al. (2021) demonstrate large model uncertainty in the climate-economy relationship by systematically evaluating a relatively constrained class of models, finding estimates ranging from -84% of GDP damage to +359% benefit from 4–5°C of warming (empirical growth model uncertainty is also seminally explored in Sala-i Martin (1997)). Berg et al. (2024) show that relaxing homogeneity assumptions in temperature–growth regressions reveals substantial cross-country heterogeneity in both the sign and magnitude of responses. Lemoine et al. (2025) describe the unavoidable tradeoffs between precise identification, robustness to model assumptions, and distinguishing between persistent climate change effects and transient weather effects. They argue that estimation strategies can meet at most two of these objectives. Barker (2023) and Barker (2024) explore how statistical issues and influential points generate major fragilities of the findings of certain climate-econometric studies. Rosen (2019) argues that temperature-growth regressions omit core economic drivers and rely on implausible homogeneity assumptions. Keen (2021) criticizes sector-specific approaches and argues that estimates of the climate-economy relationship from historical data cannot account for the possibility of tipping points. Conversely, Kahn (2021) points out that the Lucas Jr (1976) critique could undermine the predictive accuracy of estimated climate-economy relationships, due to responsive adaptation.

2 Theory

Our analysis focuses on the question of whether and how historical time-series data can inform us about the future climate-economy relationship. We define this relationship as the causal effect of realized environmental conditions on an economy. Short-term variation of these conditions is referred to as weather, and long-run changes in the distribution of these conditions is referred to as climate change. To understand the effect of climate change on the global economy, the ideal study would conduct a controlled experiment—unfortunately, an impossible task. Thus, we must turn to estimating the climate-economy relationship from historical time-series data. To this end, we focus on the feasibility of internal and external validity: respectively, can an econometric study accurately measure the historical causal effect of climate change on the economy, and can this effect be confidently used in a future counterfactual exercise?

Each data point in a climate-economy time series has a time index (t) and a space index (i, often country, sometimes region). If the climate-economy relationship has qualitatively meaningful variation across both space and time—as we argue it does—there are not enough degrees of freedom to estimate it. Assuming away some of spatial and/or temporal variation to preserve degrees of freedom creates qualitatively meaningful bias. This is the core, irreducible estimation challenge. As we show, influential points, low signal-to-noise ratios, and difficulties in reducing the complexity of 'climate' to a small simple set of variables, pose additional challenges.

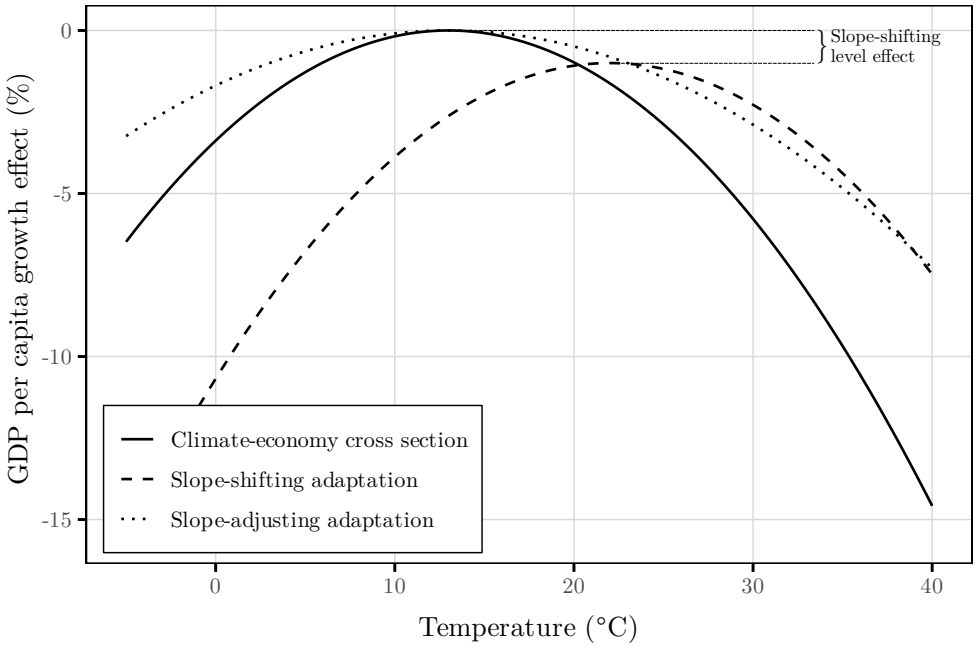
Countries and regions vary widely in both the climatic conditions they are adapted to—which we call their 'locally optimal environment'—and how their economies respond to weather shocks and climate changes. This creates meaningful spatial variation in the climate-economy relationship (see also Berg et al. (2024)). We demonstrate and discuss theoretical and empirical evidence for these points in sections 2.2 and 2.3.

These points are also intuitive. A country's locally optimal environment is realized in its infrastructure and institutions; it reflects the environmental conditions by which an economy

is least impacted. For example, a cold weather snap that is majorly destabilizing in Texas is a trivial occurrence in Canada. The existence of a locally optimal environment can be demonstrated empirically: cities' most common daily temperatures are strongly correlated with their mortality-minimizing temperatures (Yin et al., 2019). Some economists argue (e.g., Deryugina and Hsiang (2017); Hsiang (2016)) that, under competitive equilibrium, observed economies are locally optimized with respect to their prevailing climate along existing margins of adjustment. We argue in Section 2.4 that assuming perfect optimization is too strong. However, modeling locally optimal environment captures the potential for *slope-shifting adaptation*. That is, adaptation which shifts the effects of climate-economy relationship (Figure 2).

How economies respond to weather shocks and climate changes is also a product of many historical, geographic, economic, and cultural factors, which widely vary. The above examples of cold snaps in Texas vs. Canada, and of cities' different mortality minima, illustrate this. For a more nuanced example, consider that the Arabian peninsula has a harsh, arid climate which prevents the deployment of large scale agriculture. Yet, nations like Saudi Arabia and Qatar are wealthy because of their extractive petroleum-based economies. Were a resource-poor nation, like Japan for instance, to have the same climate as Qatar, then we would expect different economic impacts.

Figure 2: Slope-shifting vs. slope-adjusting adaptation



Notes: This figure depicts the difference between slope-shifting adaptation, where a change in infrastructure yields a new locally optimal environment $\mathbf{C}^*_{it}$, and slope-adjusting adaptation, where affluence and technological progress moderate the effect across all climates, for a cross-section of the climate economy relationship at a given $\mathbf{C}^*_{it}$. Also depicted is a slope-shifting level effect, where shifting the locally optimal environment causes a level change in the effect across all climates. This level effect is important because it captures inevitable damages from climate change, even if an economy can choose the best locally optimal environment.

Economic development causes climate-economy relationships within countries and regions to change over time. This is also intuitive. Through technological means, capital mobilization, and efficient institutions, wealthier countries can withstand the negative impacts of environmental conditions better than poorer nations. For example, Kahn (2005) and Toya and Skidmore (2007) estimate roughly 0.5-percent decreases in death and damage rates from natural disasters for a one-percent increase in real GDP per capita. This assumption captures the potential for *slope-adjusting adaptation*. That is, adaptation which moderates the marginal effect of the climate-economy relationship across all potential climates (Figure 2). As we discuss and show in Section 2.3, affluence-driven adaptation both contributes to meaningful temporal heterogeneity in the climate-economy relationship and facilitates time-varying sources of estimation bias.

2.1 Model

To illustrate and formalize our argument, we modify and generalize the motivating model from Dell et al. (2012). Country i's economy in year t has output per capita:

$$y_{it} = R_i(\mathbf{C}_{it}, \mathbf{C}^*_{it}, y_{it}) A_{it} k_{it}^{\alpha_i}, \quad 0 \leq R_i(\cdot) \leq 1. \tag{1}$$

Here, $A_{it}k_{it}^{\alpha_i}$ is the standard intensive-form Cobb-Douglas growth model (A_{it} represents productivity; k_{it} represents capital per capita; α_i represents the elasticity of output with respect to capital). We model economic outcomes experienced by country i in year t as responding to the vector of realized annual environmental conditions, $\mathbf{C}_{it}$. These conditions are realizations drawn from an underlying stochastic climate process[1], F_{it}. The function $R_i(\mathbf{C}_{it}, \mathbf{C}^*_{it}, y_{it})$ captures the country-specific level effect on output from a realized environmental state and can range from 0 to 1. We denote it R because it represents resilience—higher R means *less* output damage from environmental conditions. $R_i(\cdot)$ is also moderated by increased output per capita y (formally, $\partial R_i / \partial y_{it} \equiv R_{i,y} \geq 0$), which allows for adaptation[2]. $\mathbf{C}^*_{it}$ is the locally optimal environment for country i in year t, as realized through a country's infrastructure and institutions[3].

If a country changes their locally optimal environment $\mathbf{C}^*_{it}$ (e.g. by investing in different infrastructure), we assume that this has an effect on output through depreciation of the existing stock of capital. Essentially, this is the cost of adaptation (Lemoine et al. (2025)

[1] In this sense, $\mathbf{C}_{it}$ is "weather" as we have defined it, since it fluctuates year-to-year, but it also "climate", since it embeds the intra-annual *distribution* of environmental conditions. This highlights a real tension: that there are econometric implications of the sampling frequency and aggregation of the stochastic climate process. By modeling $\mathbf{C}_{it}$ at the annual level, we do not allow for compounding effects *across* years. These issues are discussed further in Section 2.5.

[2] Because resilience depends on contemporaneous income, equation (1) implicitly defines as a fixed point. We assume that for all admissible states, a unique interior solution exists. A sufficient condition for local uniqueness and stability is $y_{it}\dfrac{R_{i,y}}{R_i} < 1$.

[3] $\mathbf{C}^*_{it}$ and $\mathbf{C}_{it}$ have the same dimensions.

have a more complex formulation of a similar idea). Change in capital is thus:

$$\dot{k}_{it} = I_{it} - \delta_i(\dot{\mathbf{C}}^*_{it})k_{it}, \tag{2}$$

where I_{it} is idiosyncratic investment and $\delta_i(\dot{\mathbf{C}}^*_{it})$ is the cost function of changing the locally optimal environment[4]. To allow for growth impacts (we discuss the definitions of growth vs. level effects in Appendix A; see Newell et al. (2021); Chang et al. (2023) for discussion of the importance of allowing for either level or growth effects), we define the growth rate of productivity as:

$$\frac{\dot{A}_{it}}{A_{it}} = g_{it} - d_i(\mathbf{C}_{it}, \mathbf{C}^*_{it}, y_{it}), \tag{3}$$

where $d_i(\mathbf{C}_{it}, \mathbf{C}^*_{it}, y_{it})$ is the country-specific negative growth effect (i.e., damage) from environmental conditions and g_{it} is the idiosyncratic growth rate. The function $d_i(\cdot)$ is similarly affected by locally optimal environment $\mathbf{C}^*_{it}$ and moderated by affluence ($d_y \leq 0$).

If we take the natural log and time-differentiate equation (1), and insert equation (3) into the result, we get (where R_i is shorthand for $R_i(\cdot)$):

$$\frac{\dot{y}_{it}}{y_{it}} = \frac{1}{1 - y_{it}\dfrac{R_{i,y}}{R_i}} \left[\alpha_i \left(\frac{I_{it}}{k_{it}} - \delta_i(\dot{\mathbf{C}}^*_{it}) \right) + g_{it} - d_i(\mathbf{C}_{it}, \mathbf{C}^*_{it}, y_{it}) + \frac{1}{R_i} \left(\dot{\mathbf{C}}_{it} \cdot \nabla_{\mathbf{C}_{it}} R_i + \dot{\mathbf{C}}^*_{it} \cdot \nabla_{\mathbf{C}^*_{it}} R_i \right) \right]. \tag{4}$$

With these assumptions, observations of economic growth, $\dot{y}_{it}/y_{it}$, will follow equation (4). The climate-economy relationship for country-i is thus defined by the surfaces of $R_i(\mathbf{C}_{it}, \mathbf{C}^*_{it}, y_{it})$ and $d_i(\mathbf{C}_{it}, \mathbf{C}^*_{it}, y_{it})$. For the purposes of counterfactual analyses, we are also interested in $\delta_i(\dot{\mathbf{C}}^*_{it})$ since this represents the cost of slope-shifting adaptation—effectively a climate damage (Deryugina and Hsiang, 2017). Thus, a comprehensive counterfactual of the effect of climate change will apply draws of the original climate stochastic process, F_{it}, and new process, F'_{it}, to these functions, and compare the trajectories of the economy.

Estimating these functions from empirical data is the principal objective of climate econometrics. As written in equation (4), the climate-economy relationship is not identifiable from country-level panel data without imposing strong *additional* structural restrictions on the

[4]To allow for baseline depreciation, we define $\delta_i(0) > 0$.

spatial and temporal heterogeneity of the relationship. The data indices are i and t; the assumed relationship cannot be allowed to vary finely along both dimensions (geography and time) and still be estimable. Yet, the true climate-economy relationship *does* vary finely on both dimensions.

As we show in Section 2.2, the climate-economy relationship has meaningful geographic heterogeneity (see also Berg et al. (2024)), and assuming it away has problematic effects—such as assuming that idiosyncratic growth miracles and disasters in one region have global implications for the climate-economy relationship. Affluence-driven adaptation creates important temporal heterogeneity in the climate-economy relationship, as we show in Section 2.3. Therefore, there is both important spatial heterogeneity and important temporal heterogeneity, which cannot be restricted without running out of degrees of freedom for estimation. Unobservable locally optimal environments, low signal-to-noise ratios, and complexity of the functionally relevant aspects of climate—which require empirical simplification—create additional challenges.

Our central claim is that these issues create major—not minor—issues for the accuracy and precision of the estimated climate-economy relationship. Because we cannot overcome these issues, the relationship is empirically inscrutable.

2.2 Heterogeneity and influential points

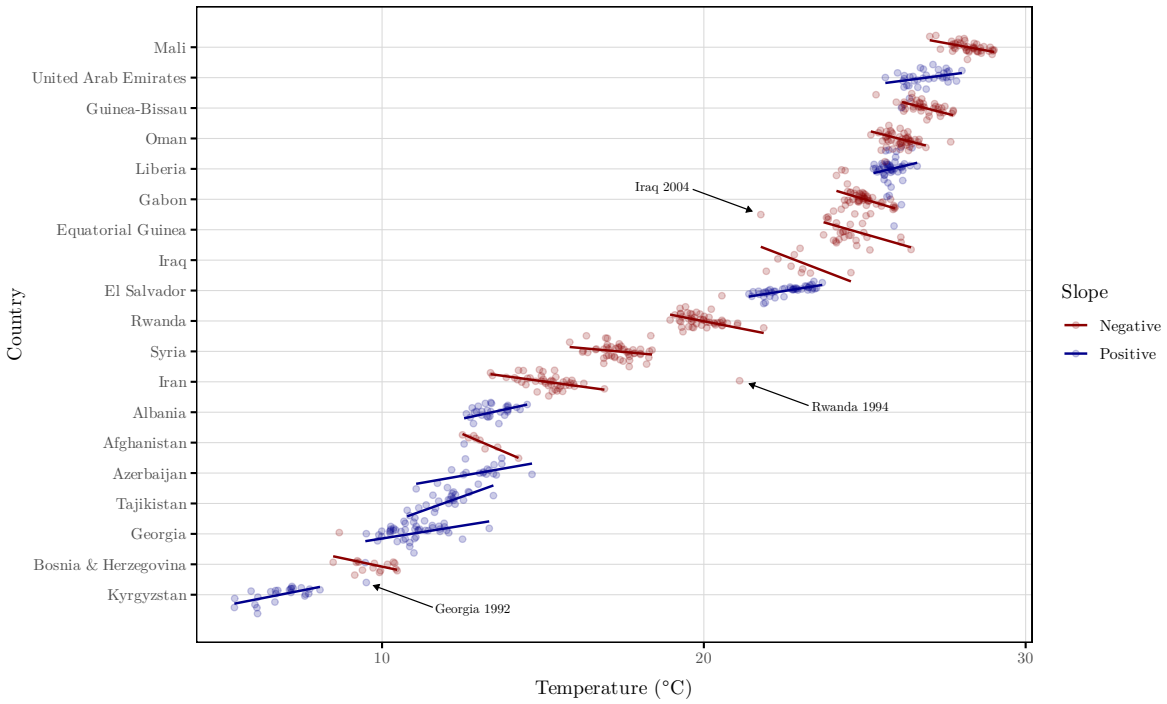
Many climate-econometric studies assume one global climate-economy relationship. They employ panel methods which treat every individual time-period observation as variation over this global function (e.g., Dell et al. (2012); Burke et al. (2015c); Kahn et al. (2021)).

There are several reasons climate-economy relationships should be heterogeneous across countries (and time). Affluence and locally optimal environment vary across countries and influence damages, and thus any point estimate of the marginal economic effect of climate should also vary across countries. We discuss affluence and locally optimal environment in the next two sections.

Even holding affluence and locally optimal environment equal, countries still most likely have heterogeneous climate-economy relationships, due to other idiosyncratic factors such as culture, geography, natural resources, political conditions, etc. There may be heterogeneous climate effects under full adaptation: different countries have varying abilities to adapt to different climates. This is empirically demonstrated in Berg et al. (2024), who show that relaxing the homogeneity assumption reveals a context-dependent and sign-heterogeneous climate-economy relationship.

As an illustrative example, we briefly ignore the fundamental identification challenges of the climate-economy relationship and estimate country-level GDP per capita-temperature relationships (see Appendix B for estimation details). Figure 3 shows these relationships for countries in which they are statistically significant.

Figure 3: Estimated Country-specific Temperature-GDP per Capita Effects



Notes: This figure depicts GDP per capita-temperature leverage plots for statistically significant country-specific temperature effects, using data from Burke et al. (2015c), with the sign of the effect denoted by color. The y-axis for each country is independent and plots GDP per capita growth residualized to all other factors, besides temperature. A few particularly influential points are labeled. Refer to Appendix B for methodology.

El Salvador and Iraq provide an instructive comparison. These countries have approximately the same average temperature and level of affluence, yet our model in Figure 3 estimates a negative marginal GDP per capita-temperature relationship for Iraq and a positive one for El Salvador. We do not claim that these are 'true' identified relationships for these countries, of course, but there are several reasons heterogeneity between them should not be surprising.

Despite experiencing similar average temperatures, El Salvador and Iraq have remarkably different climates (tropical vs. desert), which may affect the economy in different ways. This demonstrates the futility of reducing the complexity of climate to a single aggregate measure such as temperature (explored further in Section 2.5). Additionally, the two countries have different sectoral compositions. Iraqi oil could potentially be more susceptible to labor-heat productivity effects, and El Salvadoran agriculture could experience increased yields in hotter years, controlling for precipitation. As discussed in Section 3.1, Iraq had a growth miracle in a particularly cold year (economic rebounding after the 2003 invasion). El Salvador's economy is potentially buffered by remittances. Regardless of the reasons for this heterogeneity, panel methods treat all observations from El Salvador and Iraq as being generated from the same underlying process.

Assuming a global climate-economy relationship means that points from one country implicitly affect the estimate of the climate-economy relationship for all countries. Fixed effects models result in countries' relationships being implicitly weighted by their variations in temperature (and/or other climate variables used), with larger-variance countries given higher weight (Chernozhukov et al., 2013; Gibbons et al., 2019). Countries with more extreme temperatures are also implicitly given more weight when assuming a quadratic temperature effect. The result is that relatively small countries at temperature extremes can have a large influence on global estimates. We illustrate this in Section 3, using Burke et al. (2015c) as an example.

Economic growth disasters and miracles create an even larger problem of highly influential

points. Growth disasters and miracles can cause year-to-year GDP per capita changes of 25% to 65%—ten-to-thirty times larger than a typical year's growth rate, in absolute value. If these disasters and miracles happen to occur in years of extreme temperature for a country, then one or a small number of points from a few small countries can have *enormous* spurious effects on the estimated climate-economy relationship for the *entire world*.

A prominent and recently retracted climate-econometrics paper (Kotz et al., 2024, 2025) poignantly illustrates this problem. Bearpark et al. (2025) showed that inaccurate data from *a single country* (Uzbekistan)—which contained enlarged economic fluctuations spuriously correlated with temperature—*more than doubled* the estimated *global* projected damages from climate change in Kotz et al. (2024), compared to the same analysis with accurate data. Because these data were simply wrong, we know that the purported growth disasters and miracles in Uzbekistan could not have been caused by climate. Barker (2023) demonstrated that dropping 13 extreme observations from the main specification in Dell et al. (2012) removes statistical significance from the poor-country temperature interaction. We follow a similar approach and show in Section 3 that removing 6-10 influential observations associated with small-country growth miracles or disasters (out of more than 6,500 total observations) reduces global damage estimates in Burke et al. (2015c) and Kahn et al. (2021) by 20% or more. For nearly all of these points, it is very difficult to argue for a climate-related cause of the growth disaster or miracle (e.g., the collapse of the Soviet Union). Even if it were possible to make such an argument, however, it would not be possible to argue for a *global* implication of the observation for climate-economy relationship, given between-country heterogeneity.

How can climate-econometric studies address these issues? With longer and wider panels, more covariate data, and/or more sub-national data (see Greßer et al. (2021); Kotz et al. (2024)), studies can improve estimates of models that allow for heterogeneity (Desbordes and Eberhardt, 2024; Berg et al., 2024). Studies can and should conduct sensitivity analyses examining the role of growth miracles and disasters, and other influential points. However, the fundamental identification challenge in estimating equation (4) remains. Therefore, all

climate-econometric studies should be circumspect about the large model uncertainty—at best—that is unavoidable (Newell et al., 2021).

2.3 Adaptive effects of affluence

Adaptation to climate change has been previously acknowledged as a potential confounder to estimating climate-economy relationships from panel data (e.g., Lemoine et al. (2025)). If countries are increasing their resilience to environmental conditions and climate change over time, then the climate-economy relationship is changing. This can occur through countries changing what climatic conditions their infrastructure and economic practices are optimized to (slope-shifting adaptation), or through countries becoming simply better at withstanding all climatic conditions, primarily through economic development and affluence (slope-adjusting adaptation, see Figure 2). Both types of adaptation create estimation challenges. We discuss slope-shifting adaptation in the next sub-section on the locally optimal environment.

Slope-adjusting adaptation is strongly associated with affluence (Kahn, 2005, 2021; Toya and Skidmore, 2007; Burgess et al., 2026). In our model, affluence mediated adaptation implies that $R_{i,y} > 0$, and the condition for stability requires that $y_{it}\frac{R_{i,y}}{R_i} < 1$. Thus, the denominator of the leading term in Equation (4) must be greater than 1: $\frac{1}{1 - y_{it}\frac{R_{i,y}}{R_i}} > 1$. This term multiplies through all other terms in the model being estimated, magnifying the estimated damage terms if affluence-driven adaptation is not accounted for. We will refer to this as "adaptation bias."

It is not clear that adaptation bias can be adequately accounted for econometrically. For exposition, suppose that there is only a level effect in the climate-economy relationship (i.e. $R_i \leq 1, d_i = 0$), temperature T_{it} is the only relevant environmental variable (we abstract from locally optimal environment), and the resilience function is defined as $R_i(T_{it}, y_{it}) = e^{-h_i(T_{it})/y_{it}}$, where $h_i(T_{it})$ is an intermediate function of climate damages; it is convex in T_{it}

and positive. Thus, extreme T_{it} influences R_i toward 0, and greater affluence y_{it} influences R_i towards 1. Let σ_{R_iy} denote the elasticity of resilience to affluence ($\sigma_{R_iy} \equiv y_{it}\frac{R_{i,y}}{R_i}$). Therefore, $\sigma_{R_iy} = h_i(T_{it})/y_{it}$, and the adaptation bias term is $\frac{1}{1 - h_i(T_{it})/y_{it}}$.

There are several econometric challenges. First, the adaptation bias term is almost certainly non-constant. If resilience exhibits diminishing returns to income (a plausible assumption at the highest income levels at least) and income grows substantially faster than climate conditions change (e.g., real global GDP per capita has quadrupled since 1960), then this elasticity (σ_{R_iy}) will decline over time[5]. In order for σ_{R_iy} to be non-stationary in our expositional example, $\frac{\dot{h}}{h} \neq \frac{\dot{y}}{y}$ must be true, meaning the rate of income growth is not perfectly balanced with the rate of growth of the intermediate damage function. It would be a coincidence for these to perfectly move together.

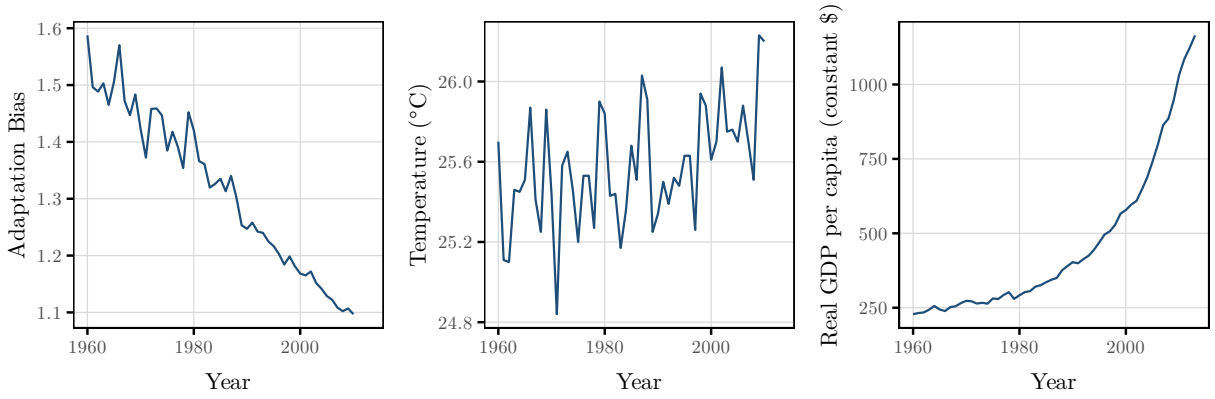
Figure 4 shows a hypothetical example, using India as a case study, where we calibrate a resilience function using coefficients estimated in Burke et al. (2015c). The adaptation bias term, depicted in the left panel, scales the marginal effect of the climate-economy relationship. Because India's GDP per capita has increased over time, outpacing the effect of a rising temperature, the adaptation bias term shrinks. Thus, in this hypothetical example, temperature sensitivities would be ≈1.5 times higher in 1960 than in 2010. If this were the true data generating process, we would be concerned that estimates of the climate-economy relationship would be sensitive to time period when enforcing temporal restrictions. Such estimates essentially capture an average of marginal effects over time. We find evidence of such sensitivities in Section 3.

Another challenge this expositional model demonstrates is that the adaptation bias term $\frac{1}{1 - h_i(T_{it})/y_{it}}$ is non-linear in both affluence y_{it} and intermediate damages $h_i(\cdot)$, so any proper estimation must account for non-linearity. Lastly, it should be noted that this bias is largest when intermediate damages are large ($h_i(\cdot) \gg 0$) and when affluence is small. We

[5]For countries with slow economic growth and comparatively fast growth in climate damages, we concede that the opposite could be true; climate effects may outpace income growth, and thus, elasticity, σ_{R_iy}, may be increasing.

should therefore expect the largest bias in high-damage, low-affluence countries, and the greatest temporal heterogeneity among countries that transitioned away from these states in the historical record.

Figure 4: Hypothetical Adaptation Bias for India



Notes: This figure depicts historical temperature and real GDP per capita for India from the Burke et al. (2015c) dataset, as well as a hypothetical adaptation bias term. This is purely to demonstrate *how* adaptation bias could change over time. We make no claims as to how it should be modeled or what the bias is. We calculate this term using the model from above where $R_i(T_{it}, y_{it}) = e^{-h_i(T_{it})/y_{it}}$. $h_i(T_{it}, T^*_{it})$ is a quadratic function of temperature calibrated using the coefficients from Burke et al. (2015c). Affluence is in units of thousands of constant 2010 dollars.

Second, even if we could estimate and account for *past* adaptation (either driven by affluence or by specific adaptation practices, as Hultgren et al. (2025) attempt for crop yields, for example), we could not possibly anticipate future technological changes affecting adaptation with any precision. History suggests that this is a major caveat to predicting the long-run severity of environmental problems. Inaccurate 1960s predictions of overpopulation-driven mass starvation (Ehrlich, 1968)—averted by the Green Revolution—and late-1700s Malthusian predictions of inescapable poverty (Malthus, 1798)—averted by the advent of the modern industrial economy—illustrate this profoundly.

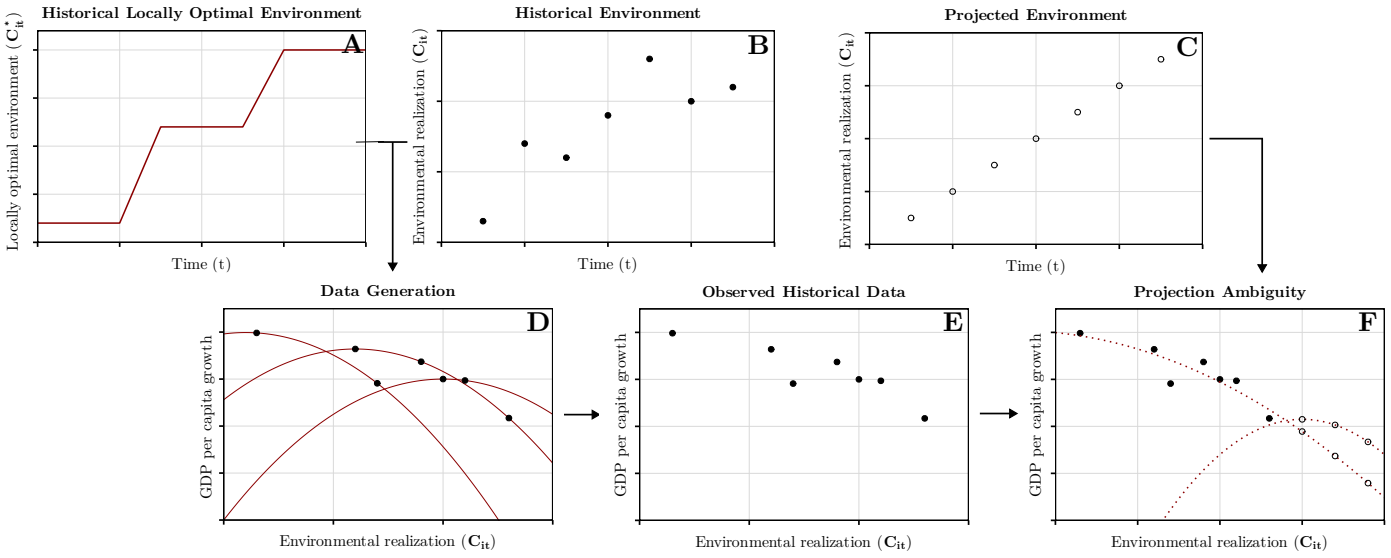
Some studies measure adaptation by examining changes in marginal effects of climate on climate-sensitive outcomes (including GDP per capita), estimated in different time periods using the same specification (e.g., Burke et al. (2024)). Such studies cannot address the challenges discussed in this section for two reasons. First, the specifications in each time

period suffer from the biases this section describes. Second, comparing marginal effects of climate in two different time periods does not capture the level effect of affluence in increasing outcomes (Burgess et al., 2026).

2.4 Unobservable locally optimal environment

Locally optimal environment, $\mathbf{C}^*_{it}$, is an input to the functions $R_i(\cdot)$ and $d_i(\cdot)$. $\mathbf{C}^*_{it}$ is determined by combinations of infrastructure, institutions, and sectors that an economy could deploy at any given level of affluence. While affluence and its adaptation effect capture the resilience of an economy across all possible climates, the effect of $\mathbf{C}^*_{it}$ constitutes the tradeoff of being resilient for one climate over all others. Thus, countries are able to adapt (or become more vulnerable) to a changing climate by shifting this parameter (and in effect, shifting the infrastructure of their economies).

Figure 5: Locally Optimal Environment Projection Ambiguity



Notes: This figure depicts the potential for projection ambiguity when locally optimal environment is not accounted for. A hypothetical time series of locally optimal environment (Panel A) and environmental conditions (Panel B) are realized across the surface of the climate-economy relationship, which produces environment-growth observations (Panel D). Importantly, it is only these data that can be observed (Panel E). Assuming no locally optimal environment, a researcher may naively estimate a relationship between environmental conditions and growth, collapsing a two-dimensional function into a one-dimensional one, as depicted in Panel F. However, if there is slope-shifting adaptation, then mapping a projection of climate (Panel C) to this naive relationship could lead to erroneous predictions. Panel F depicts two projection time series, one mapped to the naive estimation, and one mapped to the true climate-economy relationship at a certain locally optimal environment. The naive estimation exaggerates the impact of climate change on GDP per capita growth.

It should be noted that shifting $\mathbf{C}^*_{it}$ may not simply perform a 1:1 translation of the damage function across the variable $\mathbf{C}_{it}$; there may also be a shift in damage levels. For example, while it is feasible for an economy to minimize damages given an extreme climate, like Arctic Greenland, the minimized damages are still more severe than if that country were in a more moderate climate (this is the "slope-shifting level effect" depicted in Figure 2). For any economy, there may be some climate which provides the global minimum damage, but for any given climate, an economy has a local minimum damage—which is achievable through the appropriate $\mathbf{C}^*_{it}$.

The fact that $\mathbf{C}^*_{it}$ cannot be observed creates a key estimation challenge. When a country experiences climate damages, we cannot easily discern to what degree the damages could

have been prevented by a shift in $\mathbf{C}^*_{it}$ and to what degree they were inevitable. This is the fundamental issue behind the weather-vs-climate damages debate in the literature (Hsiang, 2016). The change in locally optimal environment, $\dot{\mathbf{C}}^*_{it}$, is also a term in the model. In the presence of climate change, countries may experience damages because they are not adapting quickly enough, or they may experience damages under full preparedness. Again, these effects are inseparable if locally optimal environment is itself unobservable.

However, these challenges can be sidestepped if strong assumptions are made about the relationship $\mathbf{C}^*_{it}$ has with $\mathbf{C}_{it}$. This is the primary subject of the working paper Deryugina and Hsiang (2017). The authors argue that, in the presence of a global damage function, like $R_i(\cdot)$, countries locally optimize over $\mathbf{C}^*_{it}$ to minimize expected damages and costs of adapting, given an expected distribution of environmental outcomes. If this distribution accurately predicts realized $\mathbf{C}_{it}$ and perturbations around $\mathbf{C}^*_{it}$ are small enough, then, by the envelope theorem, observations of environmental conditions and damages trace the frontier of the global expected damage function of the climate process, F_{it}. In the interest of climate econometrics, estimating this frontier is an acceptable substitute for the full global climate-economy relationship, since it represents both the extent to which countries can adapt and the empirical dynamics of adaptation. Importantly, it also incorporates the costs of adaptation into the damage function. Thus, Deryugina and Hsiang (2017) argue that the marginal effect of weather—i.e., short term variations in $\mathbf{C}_{it}$—estimates the marginal effect of climate change—i.e., long term change in the distribution F_{it}, from which $\mathbf{C}_{it}$ is drawn. According to these arguments, the weather-vs-climate issue is resolved.

This chain of reasoning, applied to our model, posits that the locally optimal environment for country-i at time-t, $\mathbf{C}^*_{it}$, is always a locally optimal response to the expected distribution of climate outcomes, F_{it}. In an econometric setting, where the distribution of F_{it} is captured by $\mathbf{C}_{it}$ in expectation, $\mathbf{C}^*_{it}$ can be approximated as $\mathbf{C}^*_{it} = C^*(\mathbf{C}_{it})$. Thus, $R_i(\mathbf{C}_{it}, C^*(\mathbf{C}_{it}), y_{it})$, $d_i(\mathbf{C}_{it}, C^*(\mathbf{C}_{it}), y_{it})$, and $\delta_i(\dot{\mathbf{C}}_{it} \cdot dC^*/d\mathbf{C})$ become the new functions of interest, representing the loci of the climate damage frontier. This assumption expresses all terms of the climate-

economy relationship and its derivatives as functions of $\mathbf{C}_{it}$ and y_{it}. If affluence y is assumed to have no effect (which we argue is a problematic assumption, in the previous section), then Equation (4) becomes easily estimable and is the motivating model behind some climate-econometric studies (Kalkuhl and Wenz, 2020).

However, these arguments rest on certain theoretical conditions (as Deryugina and Hsiang (2017) acknowledge), which are unlikely to hold in reality. Namely, 1) countries must accurately, naturally, and consistently optimize their economies around the climate process, 2) the damage functions and climate distributions must be ideal enough to be recoverable in an econometric setting.

There are a few reasons why we might expect this first assumption to be unstable. First, economies may have varying accuracy in predicting the future stochastic process of climate, F'_{it}, and the realized environmental conditions, $\mathbf{C}_{it}$. Temperature projections have occasionally been shocked by unusually hot years (Hansen et al., 2025). Second, an economy is unable to preemptively optimize because the costs of climate damages are unknown until they are realized (Pindyck, 2013); adaptation is *ex post*, not anticipatory. Third, the effectiveness of optimization could be undermined by public actors' information deficits regarding non-market benefits and costs (Mendelsohn, 2000). Additionally, since optimization occurs at the level of representative agents for their individual damage functions, then these may not sum together to produce a damage function of the macro-economy with the same optimum if there are emergent effects. These arguments are supported by evidence in Yin et al. (2019), which demonstrates that while there is a linear relationship between a country's annual mean temperature and the temperature with the lowest mortality, this relationship is noisy ($R^2 < 0.33$) (in contrast to the strong relationship between mortality and most-frequent temperature). For these reasons, the locally optimal environment $\mathbf{C}^*_{it}$ cannot be consistently expressed as an optimized response of climate, $\mathbf{C}_{it}$.

Even if economies were optimizing, econometric identification of the climate frontier requires certain regularity conditions. Since the envelope theorem only provides a first-order

approximation, the marginal effect of the local damage function equals the gradient of the global climate damage function at a point only if higher-order terms are negligible. If the local damage function is highly curved, if the distribution of environmental realizations is non-symmetric, or if climate change alters higher moments of the distribution rather than shifting it marginally, the estimated marginal effect need not approximate the derivative at the relevant point. Curvature and non-linearity can also enter if agents optimize according to concave utility preferences (loss aversion) or if their choice set is non-continuous. As we discuss in Section 2.5, econometric settings often reduce the full complexity of environmental realizations to one or two variables, most often temperature and precipitation. Not only is the estimated marginal effect at risk of omitted variable bias, but if climate change shifts the joint-distribution of environmental variables, then temperature may not be a sufficient statistic for climate change. Lastly, the tangency argument presumes ergodicity: that repeated realizations do not alter state variables in ways that generate path dependence. If environmental realizations compound through capital destruction, drought, or favorable conditions, the dynamic response to persistent climate change may not be recoverable from transitory weather variation.

Stepping aside from Deryugina and Hsiang (2017)'s envelope theorem argument, one could argue that the locally optimal environment $\mathbf{C}^*_{it}$ follows from some 1:1 mapping function of $\mathbf{C}_{it}$ other than an optimal response to climatic conditions. For example, $\mathbf{C}^*_{it}$ could be determined by a moving average of historically realized climatic conditions, $\mathbf{C}_{it}$, capturing *ex post* adaptation rather than anticipatory. Thus, our motivating model would be expressed in terms of $\mathbf{C}_{it}$, its lags, and y: all observable variables. Kahn et al. (2021) adopt this methodology. However, this methodology requires the relationship between $\mathbf{C}^*_{it}$ and $\mathbf{C}_{it}$ is static over time. We argue above that the relationship is unstable.

In sum, locally optimal environment, $\mathbf{C}^*_{it}$, is unobservable, an important part of the model, and not easily reducible to realized climatic conditions, $\mathbf{C}_{it}$. This presents a major obstacle to estimating the climate-economy relationship from historical data.

2.5 Quantification of climate and omitted-variable bias

So far, we have described environmental realizations, $\mathbf{C}_{it}$, as a vector of all environmental variables that country-i experiences in a window of time t: temperature, precipitation, humidity, cloudiness, etc. In the real world, we would expect these variables to have complex interactive effects on the economy. For example, the interaction of temperature and humidity increases the heat index and has been shown to reduce worker productivity (Kjellstrom et al., 2009).

We might also expect complex intertemporal effects within the unit of time, t. For example, crop productivity does not depend on the average temperature within a year; it is more accurately affected by the time series of temperature, from spring to fall. Callahan and Mankin (2022) find effects from the hottest five *consecutive* days; Kotz et al. (2021) examine day-to-day variability within a year. Yang et al. (2023) discuss how equally-weighted temporal aggregation can lead to biased estimates. They replicate the estimation from Dell et al. (2012) with an alternatively weighted model and find that the results operate through level, not growth effects. Additionally, our motivating model, as well as most other climate-econometric studies, assumes ergodicity and does not allow for intratemporal effects—e.g. interactions across consecutively hot years.

Similarly, spatial effects across the individual i may have complex effects, e.g. economic outcomes may depend on how environmental conditions are geographically distributed across a country, with agricultural areas being more sensitive than urban ones. If the climate-economy relationship is to be estimated from historical data, we face an issue of complexity: can the multitude of interactive effects be simplified to a reduced-form model with spatially and temporally aggregated variables?

Since the complex vector of environmental conditions is often reduced solely to annually aggregated temperature and precipitation in climate-econometric studies (Dell et al., 2012; Burke et al., 2015c; Kahn, 2021), this question is of paramount importance. Akyapı et al. (2025) and Waidelich et al. (2024) demonstrate that choosing from weather variables outside

average aggregates improves explanatory power. Depending on one or two climate variables increases the risk of omitted variable bias and confounding. Temperature may be correlated with other factors of climate, such as extreme rainfall, drought, severe wind, which also have economic effects. If this is true, omitting these factors generates bias and does not causally identify the effect of temperature. However, this may not pose a threat to climate damage projections if this correlation is expected to hold in the future. For instance, if annual temperature is correlated with a model's unobserved climate factors, like hurricane intensity, and this correlation remains constant, then damage projections of climate change (projected solely as an increase in temperature) from this model would be valid. The biased estimate captures both channels of damages: temperature and hurricane intensity.

However, these correlations may not hold. For example, if a study is estimating the economic effect of temperature fluctuations, it may inadvertently measure the effect of El Niño (drought, changing precipitation patterns), which is correlated with temperature fluctuations. Thus, for the estimate to be externally valid, a projected temperature rise from climate change would need to have the same effects as El Niño. This is not expected to be the case (Gulev et al., 2021; Seneviratne et al., 2021). We discuss this issue in the context of Bilal and Känzig (2026) in Section 3.3.

An ideal study would be able to 1) observe the entire vector of environmental variables 2) with enough observations to isolate the interactive effects within the climate-economy relationship, and 3) apply the entire vector of environmental realizations drawn from a forecast of the future climate distribution, F'_{it}, to this relationship. In reality, it is infeasible to observe the entire climate vector. Concessions must be made towards aggregations and variable choice. It is then difficult to identify and isolate the effects of specific variables because of low signal-to-noise ratios and confounding, as we discuss in the next section, 2.6. Lastly, projections of climate change do not represent a distribution of the entire vector of environmental conditions; rather, they are often just a path of temperature.

For these reasons, we argue that quantifying climate is a daunting practical challenge to

estimating the climate-economy relationship. Weak identification, compounded with superficial monovariate climate projections, cannot lead to accurate expectations of damages from climate change.

2.6 Low signal-to-noise ratio

In the presence of slope-shifting adaptation, climate econometrics must contend with a practical challenge. To estimate the surface of the climate-economy relationship from panel data, variation in economic growth attributable to the changing climate distribution, $F_{it} \to F'_{it}$, must be large relative to all other sources of variation. We argue that this condition fails.

The econometrician never observes the climate distribution, F_{it}, directly. Rather, environmental realizations, $\mathbf{C}_{it}$, are drawn from F_{it}, which can decomposed as:

$$\mathbf{C}_{it} = \boldsymbol{\mu}_{it} + \boldsymbol{\varepsilon}_{it}, \tag{5}$$

where $\boldsymbol{\mu}_{it} = \mathbb{E}[F_{it}]$ is the evolving climate mean and $\boldsymbol{\varepsilon}_{it}$ is mean-zero weather noise with covariance $\boldsymbol{\Sigma}^{\boldsymbol{\varepsilon}}_{it}$. Locally optimal environment, $\mathbf{C}^*_{it}$, is not observable, but if we assume it approximately tracks the climate mean, it can be decomposed as

$$\mathbf{C}^*_{it} = \boldsymbol{\mu}_{it} + \boldsymbol{\nu}_{it}, \tag{6}$$

where $\boldsymbol{\nu}_{it}$ is the idiosyncratic adaptation mismatch (not necessarily mean-zero), with covariance $\boldsymbol{\Sigma}^{\boldsymbol{\nu}}_{it}$.

The econometrician is interested in estimating the climate-economy relationship. While Equation (4) from our motivating model is composed of $\mathbf{C}_{it}$, $\mathbf{C}^*_{it}$, their derivatives with respect to time, y_{it}, and other variables which track investment dynamics, for simplicity, let us write the climate-economy relationship as $f(\mathbf{C}_{it}, \mathbf{C}^*_{it})$. Thus, the econometrician faces the

reduced-form data generating process of economic growth

$$\dot{y}_{it}/y_{it} = f(\mathbf{C}_{it}, \mathbf{C}^{*}_{it}) + u_{it} = f(\boldsymbol{\mu}_{it} + \boldsymbol{\varepsilon}_{it}, \boldsymbol{\mu}_{it} + \boldsymbol{\nu}_{it}) + u_{it}. \tag{7}$$

We show in Appendix C that this results in the following variance decomposition of economic growth:

$$\begin{aligned}\mathrm{Var}(\dot{y}_{it}/y_{it}) = &\underbrace{(\widehat{\mathbf{f}}_{C} + \widehat{\mathbf{f}}_{C^*})^{\top}\boldsymbol{\Sigma}^{\boldsymbol{\mu}}_{it}(\widehat{\mathbf{f}}_{C} + \widehat{\mathbf{f}}_{C^*})}_{\text{Climate change}} + \underbrace{\widehat{\mathbf{f}}_{C}^{\top}\boldsymbol{\Sigma}^{\boldsymbol{\varepsilon}}_{it}\widehat{\mathbf{f}}_{C}}_{\text{Weather}} \\ &+ \underbrace{\mathrm{Var}(R_2)}_{\text{Curvature}} + \underbrace{\widehat{\mathbf{f}}_{C^*}^{\top}\boldsymbol{\Sigma}^{\boldsymbol{\nu}}_{it}\widehat{\mathbf{f}}_{C^*}}_{\text{Adaptation mismatch}} + \underbrace{\sigma^2_u}_{\text{Economic noise}}.\end{aligned} \tag{8}$$

In other words, variance in economic growth comes from slowly-evolving climate change, highly-variable weather, any unmodeled non-linear effects, mismatch in locally optimal environment, and generic economic noise. Weather variation helps identify marginal economic responses to environmental conditions, but the adaptation component of the damage function depends on slow changes in the climate mean. Because the variance of climate change is small relative to weather variability, the relevant signal for estimating damages from climate change is weak. Furthermore, these are both dominated by economic noise, such as the growth miracles and disasters which exert influential leverage on econometric studies (which we explore further in Section 3). Adaptation noise is also unobservable; it can never be known to what degree damages were incurred because of imperfect adaptation (discussed in Section 2.4). Taken together, these features imply that the signal-to-noise ratio relevant for estimating the climate–economy relationship may be very low.

These problems compound when the vector of environmental realizations $\mathbf{C}_{it}$ is compressed onto a low-dimension index $\mathbf{c}_{it}$ (temperature, precipitation, etc.), as is common practice—an issue discussed above in Section 2.5. In Appendix C, we show that this reduction exacerbates the low signal-to-noise problem. The variance decomposition of economic

growth is now:

$$\begin{aligned}\mathrm{Var}(\dot{y}_{it}/y_{it}) = &\underbrace{(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\mu},\mathrm{Obs}} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})}_{\text{Observable climate change}} + \underbrace{(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\mu},\boldsymbol{r}} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})}_{\text{Unobservable climate change}} \\ &+ \underbrace{(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\mu},\boldsymbol{r},\mathrm{Obs}} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})}_{\text{Confounding climate change}} \\ &+ \underbrace{\widehat{\mathbf{f}}_C^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\varepsilon},\mathrm{Obs}} \widehat{\mathbf{f}}_C}_{\text{Observable weather}} + \underbrace{\widehat{\mathbf{f}}_C^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\varepsilon},\boldsymbol{r}} \widehat{\mathbf{f}}_C}_{\text{Unobservable weather}} + \underbrace{\widehat{\mathbf{f}}_C^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\varepsilon},\boldsymbol{r},\mathrm{Obs}} \widehat{\mathbf{f}}_C}_{\text{Confounding weather}} \\ &+ \underbrace{\widehat{\mathbf{f}}_{C^*}^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\nu}} \widehat{\mathbf{f}}_{C^*}}_{\text{Adaptation mismatch}} + \underbrace{\mathrm{Var}(R_2)}_{\text{Curvature}} + \underbrace{\sigma_u}_{\text{Economic noise}} .\end{aligned} \tag{9}$$

The econometrician now faces three challenges: 1) the signal is smaller, since only the components of weather and climate change that are observable via the low-dimension index $\mathbf{c}_{it}$ can be used; 2) unobservable components increase the level of noise; and 3) if unobservable components are correlated with observable components, then there is confounding and risk of misidentification (discussed in Section 2.5).

Projections of climate damages depend on the response of the economy to slow changes in climate means. However, the relevant variation for estimating the climate–economy relationship is precisely the component that is weakest in the historical data. For these reasons, we argue that low signal-to-noise ratios present a fundamental empirical challenge to estimating the climate–economy relationship from historical panel data.

2.7 Review

Taken together, the challenges articulated in this section—spatially heterogeneous damage functions, affluence-mediated adaptation introducing spatially and temporally varying biases, locally optimal environment, high-dimensional climate inputs, and low signal-to-noise ratios—imply that the climate–economy relationship is neither stable nor directly observable in historical data. Even under optimistic conditions, empirical estimates derived from panel variation conflate realized damages with unobserved adaptation choices, time-varying

resilience, and country-specific economic structures. As a result, the objects required for credible counterfactual projections—namely the structural damage functions and the costs of adaptation—are not well-identified without strong, largely untestable, and almost-certainly unrealistic assumptions.

This does not imply that historical data are uninformative, but rather that their informational content is fundamentally limited: reduced-form estimates may describe past responses of the economy to climate under particular institutional, technological, and climatic regimes, yet they cannot be straightforwardly extrapolated to novel future climates. Any attempt to do so necessarily embeds assumptions about optimization, adaptation, and homogeneity that lie outside the data themselves.

The remainder of this paper builds on this insight, demonstrating how these challenges affect previous climate-econometric studies.

3 Empirical Review

To concretely demonstrate the estimation challenges presented in Section 2, we replicate and explore three prominent climate-econometric studies: Burke et al. (2015c), Kahn et al. (2021), and Bilal and Känzig (2026). We select these papers for their influence, methodological diversity, and varied conclusions—*not* because they are uniquely flawed. We are arguing that each paper attempts an impossible feat; we are not criticizing them for not achieving it. Each of these papers—at the time it was written—employed a novel and innovative estimation strategy to overcome some perceived shortcoming of the existing climate-econometric literature. Later, we discuss the broader literature, putting the estimation challenges found in these three papers into context.

3.1 Burke et al. (2015)

Burke et al. (2015c), which we will refer to as BHM, is perhaps the most influential paper in the macro-level climate-econometric literature. The authors build off of the country-year fixed effects strategy of Dell et al. (2012), which attempts to sidestep omitted-variable biases of cross-sectional studies. The innovative contribution of BHM is to allow for non-linear (quadratic) temperature and precipitation effects on GDP per capita growth, which are often assumed by IAMs (e.g., Barrage and Nordhaus (2024)). BHM's identification of local marginal effects comes from within-country interannual variation, which is plausibly exogenous, while the curvature of the global response function is inferred by stitching together these local derivatives across countries at different mean temperatures. BHM's main specification takes on the form:

$$\Delta \ln y_{it} = \alpha + \beta_0 T_{it} + \beta_1 T_{it}^2 + \beta_2 P_{it} + \beta_3 P_{it}^2 + \mu_i + \nu_t + \theta_{i1} t + \theta_{i2} t^2 + \varepsilon_{it}, \tag{10}$$

where trends in GDP per capita growth are accounted for with a country-specific quadratic time term.

BHM find statistically significant temperature coefficients that produce an inverted-U quadratic function with an optimal temperature of 13°C. They apply this function to country-specific projections of climate change and economic growth and estimate that climate change will reduce global GDP per capita 23% by 2100 under the extreme-emissions scenario RCP8.5. We reproduce the results of their main regression in Appendix E.

BHM marked an influential turning point in climate econometrics, yet we find that their methods cannot overcome the inscrutability of the climate-economy relationship. While BHM is the subject of prior critiques (Newell et al., 2021; Barker, 2024; Tol, 2019; Rosen, 2019), we focus on how their estimation restricts spatial and temporal heterogeneity. We find that their results are quite sensitive to influential observations and time periods.

3.1.1 Spatial Heterogeneity

BHM's main specification assumes one global damage function for all countries, a choice previously criticized by Rosen (2019) and Barker (2024). BHM perform heterogeneity checks on rich and poor countries, and they do not find meaningful differences between the two groups, but this still assumes away most potential heterogeneity. As we describe in Section 2.2, pooled estimations are sensitive to highly influential countries and observations, and thus risk being externally invalid.

To explore this risk, we perform a DFBETA sensitivity exercise (Cook, 1977) on the main regression of BHM, which calculates the difference in a given coefficient estimate when each observation is dropped[6], identifying influential observations. We find that there is an asymmetry in the strength and direction of influential observations. For BHM's quadratic temperature coefficient, eight of the ten most influential observations exert a *negative* influence, intensifying the magnitude of estimated climate damage on the economy. Furthermore, when these specific observations are examined, they appear to be related to extreme growth miracles and disasters (e.g., the Rwandan genocide, collapse of the Soviet Union) that coincided with years of extreme weather, rather than events primarily *caused* by climate or weather.

While there is a developed literature on the relationship between conflict/political instability and climate (see Burke et al. (2015b) for review), we argue that these events are plausibly exogenous and unrelated to climate (with the exception of Niger 1973, which was suffering from severe drought; see Baier (1976)). For instance, the majority of the Rwandan genocide occurred in April, before the hottest part of the year (Center for Holocaust and Genocide Studies, nd); any reasonable attribution to climate should be placed in the year(s) leading up to the event. Even if these observations were due to climate, we might still wonder if other idiosyncratic factors, such as poor institutions, were at play, and thus, these influential observations might still be distorting the global estimate of the climate effect.

[6]Differences are normalized by the coefficient standard error of the leave-one-out model.

Table 1: Burke et al. (2015c) — Most Influential Observations—Temperature^2

Country	Year	Growth	Temperature Anomaly	DFBETA	Sign	Historical Notes
Rwanda	1994	−64.1%	1.90	−0.340	(−)	Rwandan genocide
Armenia	1992	−52.3%	−2.20	−0.289	(−)	Fall of the Soviet Union
Georgia	1992	−60.4%	−1.68	−0.220	(−)	Fall of the Soviet Union
Oman	1968	56.9%	−1.79	−0.201	(−)	Oil discoveries
Niger	1973	−21.5%	1.11	−0.164	(−)	Severe drought
Iraq	2004	40.6%	−0.26	−0.152	(−)	Second Gulf War rebound
Guinea-Bissau	1998	−35.2%	1.86	−0.149	(−)	Guinea-Bissau Civil War
Greenland	1983	2.2%	−2.35	0.141	(+)	
Greenland	1989	5.5%	−1.50	0.129	(+)	
Greenland	1984	−6.5%	−2.10	−0.129	(−)	

Notes: This table examines the ten most influential observations (defined as the largest absolute DFBETA measures) of the temperature^2 coefficient in Burke et al. (2015c). Real GDP-per-capita growth, country-specific temperature anomalies (in standard deviations), and historical notes provide context.

Therefore, we remove the six most influential observations, except Niger 1973, producing a more symmetrically influential field of observations, and re-estimate the main specification of BHM. We find that the relevant temperature coefficients remain statistically significant but attenuate in magnitude by ≈25%. Interestingly, the precipitation coefficients strengthen slightly and gain confidence.

To highlight the quantitative importance of six observations driving 25% of the results, consider that a country at 25°C (India) would experience a −1.15% yearly impact to growth from 1°C of warming in the original BHM estimation, and a −0.9% impact in the estimation without the influential observations[7]. If we assume a 2% annual baseline GDP per capita growth, these two outcomes result in 28% different GDP per capita levels after 100 years. Thus, estimating a pooled, global function allows a handful (6) of observations from small, volatile countries to suggest whether India, the world's fifth largest economy, would have a 28% higher or lower GDP per capita level after a century (with similar effects on many other countries).

[7]This exposition is consistent with BHM committing to a permanent-growth interpretation of their model for their counterfactual projections.

Table 2: Burke et al. (2015c) without Plausibly Exogenous Influential Observations

	Burke et al. (2015c)	w/o Influential Observations
Temperature	0.013^{***}	0.009^{***}
	(0.004)	(0.003)
Temperature^2	-0.00049^{***}	-0.00036^{***}
	(0.0001)	(0.0001)
Precipitation	0.014	0.019^{*}
	(0.010)	(0.010)
Precipitation^2	-0.005^{*}	-0.005^{**}
	(0.003)	(0.003)
Constant	1.457^{**}	1.710^{***}
	(0.645)	(0.625)
Observations	6,584	6,578
R^2	0.286	0.291

Notes: This table compares the original results of Burke et al. (2015c) to an estimation without six of the most influential observations that are deemed distortionary from plausibly exogenous historical events. Standard errors are shown in parentheses and are clustered at the country level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

How can these observations and countries exert such high leverage on the estimation in the BHM model? While the fixed-effects specification formally identifies the temperature–growth relationship from within-country variation, Barker (2024) argues that BHM's estimated global quadratic is weakly identified by these interannual temperature fluctuations and is therefore heavily shaped by how countries with different mean temperatures load onto different regions of the quadratic. Countries with both large temperature variance and extreme average temperature act as endpoints that disproportionately determine the estimated curvature. Because within-country temperature responses are small and noisy, a few extreme points can drastically alter within-country slopes. If these occur for countries with extreme temperature averages, they exert high leverage on the curvature of the estimated quadratic.

The model also contains no explicit representation of a country's locally optimal envi-

ronment, $\mathbf{C}_{it}^{*}$, nor any variable governing adaptation, another component of spatial heterogeneity. As a result, slope-shifting adaptation—whereby economies reorganize infrastructure, institutions, or sectoral composition—is ruled out by construction. Observed climate–growth responses are therefore interpreted as damages without distinction between unavoidable impacts and those arising from incomplete or delayed adaptation. It could be argued that these effects are trivial or averaged out, and the methodology captures a fuzzy but mean-centered estimate. However, if there is heterogeneity in adaptation, such that countries with extreme climates are more likely to have mismatched locally optimal environments and experience steeper-than-necessary local marginal effects, then the estimated *global* quadratic could be particularly influenced by those countries (as discussed above).

The estimated coefficients from BHM, in combination with their counterfactual methodology, imply that a country at 25°C (India) would have had 6% higher yearly economic growth in perpetuity—doubling every 11 years *on top of* the rapid growth it already had in recent decades—if it were only in a more moderate climate of 13°C, *ceteris paribus*. This magnitude implies that climate alone dominates many well-established drivers of long-run growth. An alternative explanation seems more likely: the estimated quadratic function is exaggerated by the within-country variation of poorly adapted countries in extreme climates, combined with bias caused by influential points[8]. Thus, this global function (if such a relationship exists, which is unlikely) is unstable and not well identified.

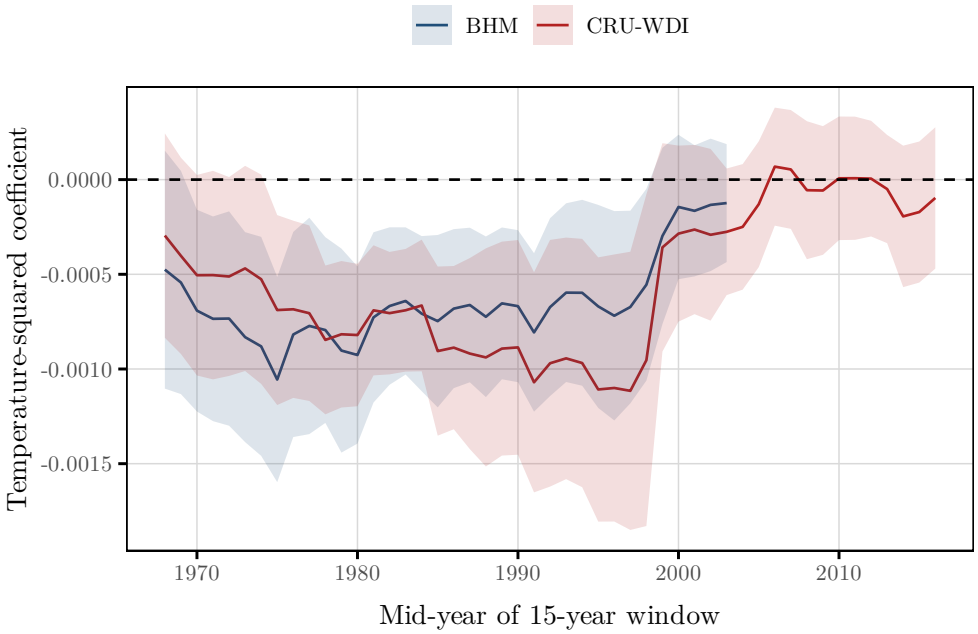
3.1.2 Temporal Heterogeneity

Next, we explore the effect of temporal heterogeneity on BHM. In Section 2.3, we argue that affluence moderates the effects of climate, creating temporal heterogeneity. BHM perform

[8]An additional alternative explanation is that BHM are identifying level effects as if they were pure growth effects. This would drastically alter counterfactual projections. The authors acknowledge in their supplementary appendix that they cannot decisively distinguish permanent growth effects from persistent level effects, which is consistent with an analysis by Newell et al. (2021). Yet, their headline climate change counterfactual is derived from assuming permanent growth effects. Regardless of the right way to interpret the growth dynamics, our argument holds that the pooled estimation is sensitive to a small number of observations.

a heterogeneity check across two time periods (1960-1989 and 1990-2010), and they find no meaningful differences. However, if we perform the same check with the years 1995-2010, we find that the temperature coefficients are significantly reduced and no longer statistically significant. This is not being driven by low sample size. We conduct the sample replication over all other 15-year subsamples, often with fewer data points, and we find meaningful and statistically significant relationships. Thus, the years 1990-1995 are driving most of the results of the 1990-2010 robustness check.

Figure 6: Burke et al. (2015c) — 15-year subsample replication



Notes: This figure depicts the estimated temperature^2 coefficient over different 15-year subsamples for both the original Burke et al. (2015c) data and CRU-WDI data. 95% confidence intervals are constructed from standard errors clustered at the country level.

We might wonder: did the climate-economy relationship meaningfully change at the turn of the millennium? To test this hypothesis, we replicate BHM with 13 additional years of data. BHM use 1960-2010 GDP per capita data from the World Bank's 2012 World Development Indicators and temperature data from Matsuura and Willmott (2012). We develop a new temperature dataset (which we call CRU-WDI) from 1960 to 2023, using the same population weights as BHM, with updated data from the University of East Anglia's

Climate Research Unit (v4.09 of Harris et al. (2020)). We show, for robustness (Appendix D), that our constructed data, over the same period, are nearly identical to the 1960-2013 BHM data. We also update the GDP per capita data from the 2024 World Development Indicators database (World Bank, 2024). This allows us to include more countries than BHM and more years for certain countries. Harding et al. (2025) also perform a replication of BHM with updated data to 2017, but they do not restrict the years of the regression. Burke et al. (2024) perform an updated replication for 2000-2020 and find statistically insignificant average marginal effects when weighted by GDP and a negative and statistically significant average marginal effect when weighted by population.

Table 3: Burke et al. (2015c) vs. Updated Data

	Burke et al. (2015c)	Updated Data	Updated Data (≥2000)
Temperature	0.013***	0.016***	0.003
	(0.004)	(0.004)	(0.004)
Temperature^2	−0.0005***	−0.001***	−0.0002
	(0.0001)	(0.0001)	(0.0002)
Precipitation	0.014	0.050	−0.049
	(0.010)	(0.087)	(0.123)
Precipitation^2	−0.005*	−0.131	0.167
	(0.003)	(0.228)	(0.347)
Constant	1.457**	0.680***	2.847***
	(0.645)	(0.054)	(0.054)
Observations	6,584	10,676	4,771
R^2	0.286	0.240	0.375

Notes: This table compares the original results of Burke et al. (2015c) to a replication with updated data (CRU-WDI). The first column represents the baseline results from Burke et al. (2015c), replicated from their original data. The second column pairs updated data with the original model specification, and thus, represents the years 1960-2023. The final column applies updated data to the original specification but only for years 2000-2023. Standard errors are shown in parentheses and are clustered at the country level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Replicating the main regression of BHM with the CRU-WDI data, we find no statistically

significant relationship in the 21st century, and the relevant coefficients are attenuated (see Table 3). This is evidence of a temporally unstable relationship, potentially one trending towards moderation, that is suggested from our motivating model. This is a sensitivity that complicates the interpretation of BHM's results and undermines confidence in projecting their results forward.

Our results seem to contrast with those of Burke et al. (2024), who also replicated and updated Burke et al. (2015c) using more recent data. Burke et al. (2024) reported statistically significant effects for the 2000s and 2010s. It is not clear exactly why this contrast exists between our results and those of Burke et al. (2024) (Burke et al. (2024) is a working paper, whose replication package is not yet public, as is common for working papers). The most likely explanation would be slight differences in countries included or data sources used. If that is the case, though, it would suggest a large sensitivity of the results to minor differences in data, which only underscores our larger point about the inscrutability of the climate-economy relationship.

It also should be noted that the inclusion of country-specific trend terms and year fixed effects absorbs smooth, low-frequency components of income and temperature, effectively discarding long-run effects of climate change most useful for the question at hand (a point mentioned in a Tol (2019) footnote). Thus, the time series of marginal effects (as seen in Burke et al. (2024)) does not contain information about the time series of level effects brought about by climate change.

3.1.3 Review

The BHM identification strategy—combining a non-linear temperature response with fixed effects—was an important contribution to climate econometrics, and we do not refute their core insight—that macroeconomic outcomes respond nonlinearly to temperature. However, the spatial and temporal restrictions imposed on their model create severe fragilities, which undermine confidence in their estimates of the effect of climate change on the economy.

3.2 Kahn et al. (2021)

The novel innovation of Kahn et al. (2021), hereafter KMN, is to focus on the effect of climate deviations from historical norms. Unlike BHM, this implicitly allows for an adaptation mechanism, under the assumption that countries adapt according to a moving average of climate realizations. In our motivating model, this assumption corresponds to locally optimal environment, $\mathbf{C}^*_{it}$, being reducible to a 1:1 function of past realizations of climate $\mathbf{C_{it}}$. This imposes a structural restriction: adaptation operates through backward-looking norm updating, thereby ruling out unobserved time-varying slope-shifting in the climate–growth relationship.

One of the primary specifications of KMN—the one used in their climate change damage projections—is an autoregressive distributed lag (ARDL) model of the form:

$$\Delta y_{it} = \alpha_i + \sum_{\ell=1}^{p} \phi_\ell \, \Delta y_{i,t-\ell} + \sum_{\ell=0}^{p} \beta_\ell^T \, \Delta|\tilde{T}_{it}(m)| + \sum_{\ell=0}^{p} \beta_\ell^P \, \Delta|\tilde{P}_{it}(m)| + \varepsilon_{it}, \tag{11}$$

where temperature and precipitation deviations are defined as normalized differences from their moving averages, over m years:

$$\tilde{T}_{it}(m) \equiv \frac{2}{m+1}\left(T_{it} - \frac{1}{m}\sum_{\ell=1}^{m} T_{i,t-\ell}\right), \qquad \tilde{P}_{it}(m) \equiv \frac{2}{m+1}\left(P_{it} - \frac{1}{m}\sum_{\ell=1}^{m} P_{i,t-\ell}\right). \tag{12}$$

In an ARDL model, the dependent variable—in this case GDP per capita growth—is regressed on lags of itself and lags of other variables—in this case the absolute value of temperature and precipitation deviations. This avoids the problem of spurious correlation between persistent growth and persistent climate variables. The long-run effects of a climate variable on GDP per capita growth are given by the cumulative dynamics of climate effects, amplified by the persistence of GDP per capita growth: $\theta = (\sum_{\ell=0}^{p} \beta)/(1 - \sum_{\ell=1}^{p} \phi_\ell)$.

Using the results from this regression, for a moving average of 30 years, KMN perform a counterfactual exercise and anticipate 7% damages to real world GDP per capita by 2100

under the extreme-emissions RCP8.5 scenario. We reproduce the results of this regression[9] in Appendix F.

3.2.1 Transmission Channels

The main specification of KMN omits: 1) climate levels (T, P), and 2) levels of deviations, $|\tilde{T}_{it}(m)|$, only allowing for the *first difference* in deviations to have an effect (e.g. $\Delta|\tilde{T}_{it}(m)|$). There are structural implications of this choice which manifest in KMN's counterfactual exercise. Regarding 1), this disallows any shift to a new climate level to have a permanent growth effect, as in BHM. Regarding 2), it implies that smooth, linear climate change, once incorporated into the moving average, does not generate sustained growth effects on economic growth[10]. Climate change only has a permanent level effect on GDP per capita; there can only be a sustained growth effect if climate change is perpetually faster than an economy's adaptation window—i.e., accelerating climate change.

Thus, we can think of the first difference in climate deviations ($\Delta\tilde{T}_{it}(m)$) as approximating the *acceleration* of climate change. This is because deviations from a moving average measure the gap between the current temperature and its trailing historical norm. When temperature follows a smooth linear trend, this gap converges to a constant, and thus, the first difference converges to zero. If the trend smoothly accelerates, the gap increase over time, and the first difference becomes positive. Thus, under sustained, linear climate change, the main identification device of KMN, $\Delta\tilde{T}_{it}(m)$, converges toward zero.

By construction, the KMN specification rules out any persistent growth effects from being at a constant new climate level and transient growth effects from smoothly, linearly

[9]For all of our replications, we use the standard fixed-effects estimator, rather than the half-panel jackknife estimator, used in KMN.

[10]Under this possible channel, then if climate change were to cease, there would not be a persistent growth effect. That would only arise from a climate level effect.

moving to that level[11]. Damages can only be transmitted to economic growth through the acceleratory path of climate change. These restrictions reveal themselves in KMN's counterfactual exercise. Rather than compare a world with climate change to one without (as done in BHM), they compare a world where climate change approaches a predicted 2100 temperature level along a quadratic (accelerating) path to a world where historical climate change continues linearly. We depict his projection methodology in Figure 7. Once the moving average has fully incorporated the trend[12], the effect of climate change is zero in the linear projection—no matter how high of a temperature this linear projection reaches—and negative in the forced quadratic projection.

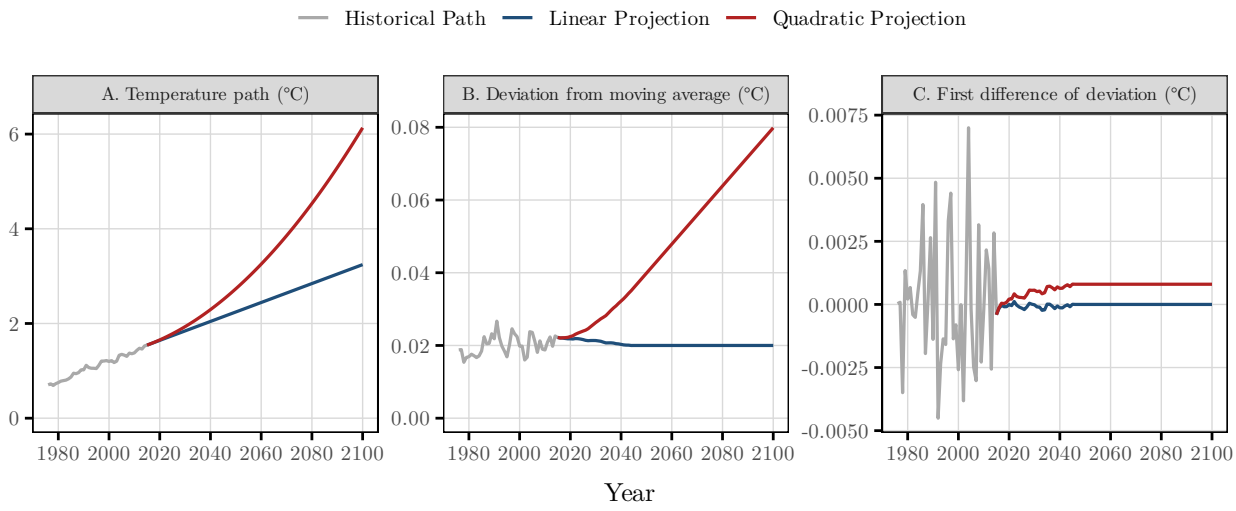
Thus, projected damages in KMN's counterfactual exercise only depend on the dynamic path of climate change, potentially missing effects from permanently shifting to a new climate. In attempting to control for a major shortcoming of previous papers—the lack of adaptation—KMN have disallowed possibly other important channels of climate damages. For these reasons, KMN seem to project economic *benefits* from climate change under the lower-emissions scenario RCP2.6 for many countries in Central America and sub-Saharan Africa that are commonly characterized as vulnerable to climate change (Intergovernmental Panel on Climate Change (IPCC), 2022)[13] (see Figure 7 in KMN).

[11]In their appendix, the authors test for the inclusion of deviation levels, which would imply a growth effect from climate change, but do not find statistically significant coefficients. Given the data, this may be a reasonable decision; through the haze of multicollinearity, there is potentially not enough statistical variation to confidently identify several lags of levels *and* several lags of differences. However, this choice has implications for their counterfactual projections.

[12]Because the projections feed historical data in the moving average (for the first 30 years of the projections), there are fluctuations which potentially make the total effect of the linear projection not exactly zero.

[13]We demonstrate in Appendix Figure F1 how this projection methodology could lead to such positive estimates.

Figure 7: Kahn et al. (2021) — Counterfactual Methodology



Notes: This figure depicts a conceptual example of the Kahn et al. (2021) counterfactual methodology. Two climate projections are generated, a quadratic projection which approaches some fixed temperature level at 2100, given by a climate scenario, and a linear projection which follows the historical trend in climate. Temperature deviations from the 30-year moving average are calculated as well as the first difference in those deviations. By construction, the quadratic projection first differences will necessarily be positive, and those of the linear projection will reach zero. These first differences are used to project damages from climate change in each scenario, the difference of which is the "counterfactual".

3.2.2 Spatial Heterogeneity

KMN's estimation also suffers from the same fragilities generated by enforced spatial homogeneity, as we show above with BHM. To explore these, we conduct a similar DFBETA sensitivity exercise as before on the primary regressions of KMN. We find similarly influential observations as BHM—many of the same—as seen in Table 4. Again, these observations reflect extreme macroeconomic events whose timing appears unrelated to climate deviations, as constructed by KMN, and yet, seven out of ten observations exert a negative influence on the estimation. When nine of these observations are removed (all except Equatorial Guinea 2010, which did not seem to experience an extreme event), the results attenuate by 35%, as seen in Table 5.

Table 4: Kahn et al. (2021) — Most Influential Observations—$\hat{\theta}_{\Delta|\tilde{T}|}$

Country	Year	Growth	DFBETA	Sign	Historical Notes
Rwanda	1994	-47.7%	-0.359	(−)	Rwandan genocide
Iraq	2004	50.1%	-0.345	(−)	Second Gulf War rebound
Equatorial Guinea	2001	58.0%	-0.316	(−)	Oil boom
Libya	2011	-62.2%	-0.269	(−)	Libyan civil war
Iraq	2003	-34.9%	0.267	(+)	Second Gulf War
Equatorial Guinea	2010	-11.6%	-0.219	(−)	
Iraq	1991	-65.0%	-0.211	(−)	First Gulf War
Rwanda	1995	37.1%	0.210	(+)	Rwandan genocide rebound
Liberia	1997	91.7%	0.194	(+)	Liberian civil war rebound
Georgia	1992	-45.3%	-0.164	(−)	Fall of the Soviet Union

Notes: This table examines the ten most influential observations (defined as the largest absolute DFBETA measures) of the $\hat{\theta}_{\Delta|\tilde{T}|}$ coefficient in Kahn et al. (2021) with a 30-year moving average. Real GDP growth and historical notes provide context.

Table 5: Kahn et al. (2021) without Plausibly Exogenous Influential Observations

	Kahn et al. (2021)	w/o Influential Observations
ΔTemperature deviation ($\hat{\theta}_{\Delta|\tilde{T}|}$)	-0.582^{***}	-0.381^{**}
	(0.198)	(0.194)
ΔPrecipitation deviation ($\hat{\theta}_{\Delta|\tilde{P}|}$)	0.032	−0.199
	(0.472)	(0.408)
Observations	6,714	6705
R^2	0.078	0.104

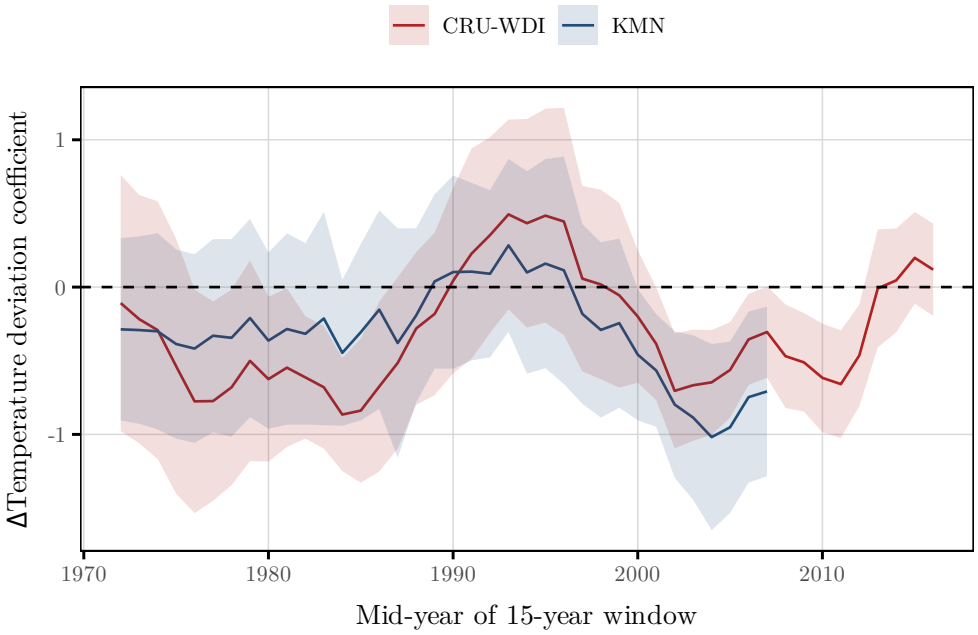
Notes: This table compares the original results of Kahn et al. (2021) to an estimation without nine of the most influential observations that are deemed distortionary from plausibly exogenous historical events. Standard errors are shown in parentheses and are derived using the Delta method. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

3.2.3 Temporal Heterogeneity

KMN do not perform a temporal heterogeneity check on their results. To do so, we conduct a replication over all possible 15-year subsamples, and we find the results unstable. The earliest subsamples produce attenuated coefficients with no statistical significance, whereas the 21st-century subsamples have a much more statistically significant and economically larger effect. This is perhaps not surprising, considering that climate change may have

been accelerating in recent decades (Rahmstorf and Foster, 2026), and KMN's regression is roughly estimating the economic impacts of accelerated warming.

Figure 8: Kahn et al. (2021) — 15-year subsample replication



Notes: The estimated $\hat{\theta}_{\Delta|\bar{T}|}$ coefficient over different 15-year subsamples for both the original Kahn et al. (2021) data and CRU-WDI data. 95% confidence intervals are constructed from standard errors derived from the Delta method.

As a further check, we replicate their regression with the same updated dataset compiled in the previous section, CRU-WDI. The rolling 15-year replication with the CRU-WDI data displays the same instability as the original dataset, albeit with more significance for earlier decades (many countries experienced a cooling trend in the 1950s and 1960s, the reverse of which may produce the acceleratory component of temperature necessary for identification in these earlier subsamples). For the entire sample, the KMN regression with CRU-WDI data produces coefficients with a smaller magnitude and much less statistical power; the temperature coefficient is no longer significant at the 5% level. However, the 21st-century subsample does not produce remarkably different coefficients. While this is not evidence of a non-stationary relationship trending towards moderation, it is evidence of a temporally unstable relationship, as suggested from our motivating model, and sensitivities which

Table 6: Kahn et al. (2021) vs. Updated Data

	Kahn et al. (2021)	Updated Data	Updated Data (≥2000)
ΔTemperature deviation ($\hat{\theta}_{\Delta\|\tilde{T}\|}$)	-0.582^{***}	-0.326^{*}	-0.311^{**}
	(0.198)	(0.175)	(0.153)
ΔPrecipitation deviation ($\hat{\theta}_{\Delta\|\tilde{P}\|}$)	0.032	0.000	0.001^{**}
	(0.472)	(0.001)	(0.001)
Observations	6,714	9,868	4,735
R^2	0.078	0.045	0.010

Notes: This table compares some of the original results of Kahn et al. (2021) to a replication with updated data (CRU-WDI). The first column represents the 30-year moving average absolute deviation regression from Kahn et al. (2021), replicated from their original data. The second column pairs updated data with the original model specification, and thus, represents the years 1960-2023. The final column applies updated data to the original specification but only for years 2000-2023. Standard errors are shown in parentheses and are derived using the Delta method. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

undermine the confidence of their results.

3.2.4 Review

KMN provide a disciplined framework that highlights the effect of climate deviations relative to adaptive norms. However, this very discipline imposes a structural restriction that long-run damages arise only when warming outpaces the adaptation window, effectively tying projected damages to nonlinear or accelerating climate paths, rather than overall climate changes. Furthermore, the fragility of their results to the time period and data source highlight that the additional econometric sophistication of KMN cannot overcome the fundamental inscrutability of the climate-economy relationship.

3.3 Bilal and Känzig (2026)

Bilal and Känzig (2026), hereafter BK, has been a notable contribution to the literature for its innovative methods and striking conclusions, which have roused debate in the climate economics community. BK move away from country-level panel data and assess the effect

of *global* temperature on *global* GDP per capita. This has the advantage of: 1) potentially capturing complex spillover effects between countries that are missed in panel studies, and 2) measuring global effects that aggregate over, rather than assuming away, cross-country heterogeneity. To avoid the problem of spurious correlation between temperature and GDP per capita, which have both increased over time, BK use a quasi-instrumental variable approach where temperature *shocks* are the identification device for the effect of temperature *levels*. They have two parallel datasets: a 1960-2019 temperature-GDP per capita dataset (known as PWT, from the Penn World Tables) and another 1860-2019 dataset (known as BU, from Barro and Ursúa (2008)).

BK first generate a measure of temperature shocks by extracting the residual from an autoregressive model of global temperature (a method proposed by Hamilton (2017)):

$$\widehat{T_t^{\text{shock}}} = T_t - \left(\hat{\alpha} + \hat{\beta}_1 T_{t-h} + \cdots + \hat{\beta}_{p+1} T_{t-h-p}\right). \tag{13}$$

They select the horizon $h = 2$, essentially isolating temperature shocks which persist for two years, and select the number of lags $p = 2$ for PWT and $p = 6$ for BU.

Following Jordà (2005), BK construct an 10-year impulse response of the change in log GDP per capita (i.e. GDP per capita growth) on these temperature shocks, accounting for a variety of controls x_t. By estimating the following equation over different horizon lengths h, the dynamic effect of a temperature shock on GDP per capita growth is drawn out (i.e. how does a temperature shock affect growth over time?):

$$y_{t+h} - y_{t-1} = \alpha_h + \theta_h \, T_t^{\text{shock}} + x_t' \beta_h + \varepsilon_{t+h}. \tag{14}$$

The area of this impulse response can be considered the cumulative impact of a temperature shock on output: essentially dy/dT^{shock}. Using the same method, they also construct an impulse response of temperature levels on these temperature shocks, which can be regarded as a measure of the impact of a temperature shock on temperature levels: dT/dT^{shock}. Thus, if

GDP per capita is a function of temperature levels, $y = f(T)$, and temperature levels respond to exogenous temperature shocks, by the chain rule, we have $dy/dT^{\text{shock}} = dy/dT \cdot dT/dT^{\text{shock}}$. The aforementioned impulse responses provide two components of this expression, allowing the authors to find the effect of temperature levels on economic growth:

$$\frac{dy}{dT} = \frac{dy/dT^{\text{shock}}}{dT/dT^{\text{shock}}} \approx \frac{\sum_{h=0}^{H} \widehat{\theta}_h}{\sum_{h=0}^{H} \widehat{\phi}_h}, \tag{15}$$

where $\widehat{\theta}_h$ and $\widehat{\phi}_h$ represent the estimated coefficients from the impulse response of output and temperature levels respectively. This is known as an impulse response function (IRF) ratio and is essentially the Wald estimator. From this method, BK find that a 1°C increase in temperature levels leads to a 20% reduction in global GDP per capita in the PWT analysis and a 29% reduction in the BU analysis. Using the paper's replication package, we follow their methods and find approximately[14] the same estimated effect on GDP per capita from a 1°C increase in temperature (see Figure G1 and Table 7).

3.3.1 Exclusion Restriction

While this approach cleverly solves the spurious correlation problem, its validity depends on a questionable assumption: temperature shocks must *only* affect GDP through the economic consequences of temperature levels. This is the exclusion restriction necessary for a valid instrumental variable. If GDP per capita is itself directly influenced by temperature shocks, such that $y = f(T, T^{\text{shock}})$, then $dy/dT^{\text{shock}} = \partial y/\partial T \cdot dT/dT^{\text{shock}} + \partial y/\partial T^{\text{shock}}$. Thus, the calculated dy/dT effect is not identified without accounting for $\partial y/\partial T^{\text{shock}}$. From the exclusion restriction, the authors assume that this term is zero, but there are reasons to believe that temperature shocks *do* influence GDP per capita through channels other than

[14]There are minor discrepancies, which we attribute to inconsistent filtering practices in BK's replication code. For example, in their BU baseline model, they create certain lags after filtering out years prior to 1860. This effectively pushes their regression to 1865. We stick with the spirit of the paper and create our lags before filtering to allow for more observations. Additionally, rather than using their provided temperature shocks, we reproduce them according to their methodology. There are extremely minor discrepancies ($R^2 = 0.9991$), but the shock creation process is not included in their replication package.

temperature levels, and thus, any measured effect is not valid in the context of climate change.

As BK discuss, temperature shocks are composed of temperature variation which is categorically unique, driven by volcanic activity, solar fluctuations, and climate cycles, like El Niño-Southern Oscillation (ENSO). For the instrumental-variable interpretation to hold, temperature shocks must affect GDP per capita exclusively through global mean temperature levels. Formally, global temperature levels must be a sufficient statistic for the economic consequences of these temperature shocks. Under the exclusion restriction, any two climate states that generate the same global mean temperature must also generate the same economic effects, regardless of whether the underlying shock arises from ENSO, volcanic forcing, or anthropogenic warming. Thus, the exclusion restriction requires the effect of a 0.1°C increase in temperature levels from an El Niño-driven temperature shock to be equivalent to 0.1°C of climate change.

BK also demonstrate that their temperature shocks correlate with increases in extreme weather (extreme heat, precipitation, wind, and drought). However, they discuss these channels as if they are *mediators* of the temperature effect, rather than *confounders*. If these channels are in fact mediators, then any change in temperature levels must generate the same mediated effects—warming from El Niño and climate change must produce equivalent changes in extreme weather.

This assumption is very unrealistic. Internal modes of climate variability, such as ENSO, redistribute heat within the climate system and generate substantial changes in atmospheric circulation, precipitation, and drought patterns. This occurs independently of shifts in global mean temperature but correlates with temperature shocks (Gulev et al., 2021; Seneviratne et al., 2021). Since these climatic states materially affect economic outcomes (Callahan and Mankin, 2023; Proctor et al., 2018; Callahan and Mankin, 2022; Waidelich et al., 2024), temperature shocks are associated with damages that are not fully mediated by temperature levels. For example, BK show that extreme winds are associated with global temperature

shocks, but extreme winds are becoming *less* common for the majority of inhabited latitudes as global mean temperatures rise[15] (Seneviratne et al., 2021). This directly undermines BK's exclusion restriction. As we argue in Section 2.5, reducing the complexity of the climate-economy relationship to effects of a single climate variable risks misidentification and confounding through omitted variables. Thus, BK identify the effect of a particular mixture of global climate variability—dominated by circulation dynamics—and scale this effect as if it were permanent anthropogenic warming.

This is corroborated by an alternative analysis in BK, where they compare the effect of country-level temperature shocks to global shocks on country-level GDP per capita. Country-level shocks do not produce a statistically significant effect on GDP per capita—barely above 0% in magnitude—whereas global shocks produce a strong effect. However, since country-level shocks do not necessarily correspond to climate cycles, they may better isolate the direct effect of temperature than global shocks. The fact that BK do not find a direct effect of temperature from their country-level analysis suggests that the estimated effect depends on the type of temperature variation exploited, raising concerns about the exclusion restriction and transportability to climate change.

It should also be noted that BK discuss the IRF ratio in terms of the impact on GDP per capita from a *permanent* 1°C increase in warming, yet this is extrapolated from *temporary* temperature shocks, the largest of which is ≈0.3°C in either dataset. Thus, they extrapolate their estimated temperature effect three-times beyond what is observed, as if it were permanent, anticipated climate change (neglecting the potential for slope-shifting adaptation, discussed in a section below). For the PWT analysis, the largest observed shock would produce a 6% negative impact on GDP per capita, and a one-standard deviation positive shock would produce a 3% impact. These numbers are more consistent with what is known about global economic impacts from ENSO (Callahan and Mankin, 2023).

[15] A point raised by Trembath and Brown (2024).

3.3.2 ENSO Confounding

To explore this further, we test BK's estimates for confounding from ENSO and other dynamics (results depicted in Table 7). As a robustness check in their paper, BK control their country-level specifications for ENSO with the Oceanic Niño Index (ONI) and show that the GDP per capita impulse responses do not meaningfully change. However, they do not check their primary global GDP per capita specifications for sensitivity—the ones used in their headline findings[16]. Conversely, we *do* find some evidence that their global results are potentially confounded by ENSO.

Table 7: Bilal and Känzig (2026) — Baseline Model IRF Ratios with Controls

	PWT	BU
Baseline	−19.6%	−27.6%
ENSO Control	−19.0%	−17.6%
Quadratic Trend	−11.5%	−27.1%
Quadratic Trend + ENSO	−9.9%	−16.8%
1960's Dummy	−13.9%	−25.5%
1960's Dummy + ENSO	−10.8%	−13.8%
Bandstop Filter	−4.9%	−28.0%
ENSO Residualization	−20.2%	−17.4%

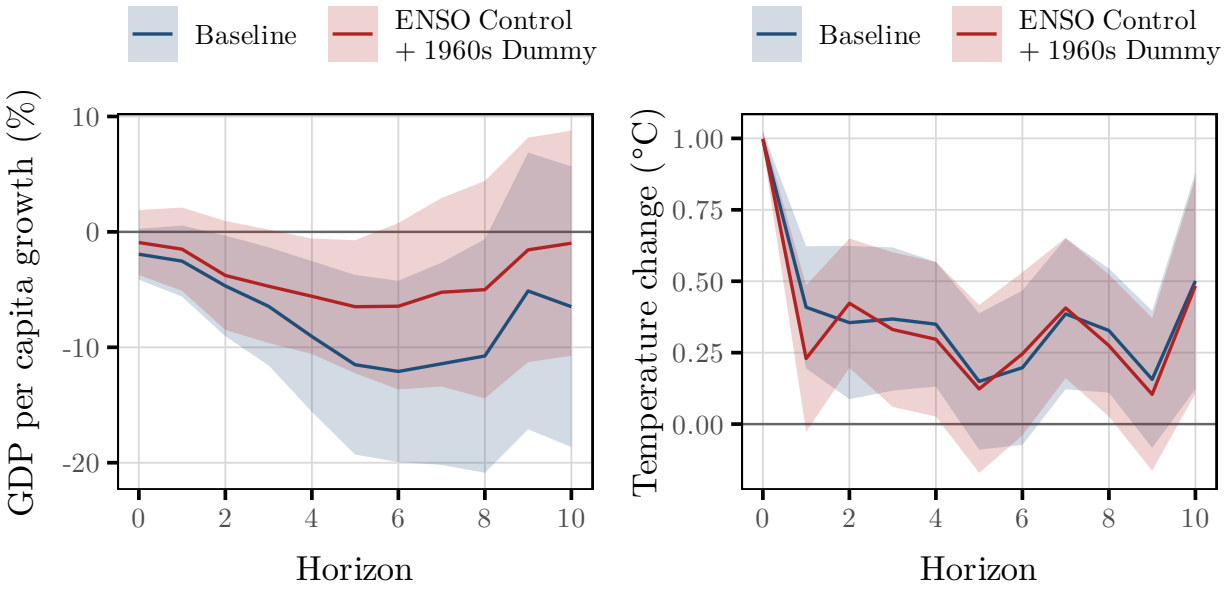
Notes: This table compares the IRF ratios of the baseline Bilal and Känzig (2026) models with a variety of controls.

First, we include the contemporaneous ONI control in the global specifications. For the BU sample, adding the control attenuates the IRF ratio from −28% to −18%. The PWT sample is less affected, moving from −20% to −19%. However, as we discover in the next section, the PWT sample seems to be especially sensitive to medium-to-long-run temporal variation. Here, we find that these sensitivities compound with the ENSO control. Including a quadratic trend or 1960's dummy variable (we explain these choices in the next subsection) reduces the effect to −10% and −11% respectively (effects that are statistically indistinguishable from zero for most years). As a further check, we residualize the temperature shocks

[16]They also do not show how the temperature response or the IRF ratio change when adding an ENSO control.

via a distributed lag model of ENSO[17], dynamically orthogonalizing the temperature shocks to ENSO variation in both models. This orthogonalization model reveals a statistically significant relationship between the BK temperature shocks and ENSO variation (Table G1). Using this new temperature shock, we find that the estimated effect attenuates from −28% to −18% for the BU dataset, and again, the PWT dataset is marginally affected (a slight increase). It is possible that the BU analysis is more susceptible than PWT to confounding from ENSO because temperature shocks are constructed using a richer set of lags which may isolate ENSO more cleanly, or perhaps the global economy was more vulnerable to ENSO in the deeper historic variation of the BU dataset.

Figure 9: Bilal and Känzig (2026) — Impulse Responses of Baseline vs. ENSO Control + 1960s Dummy (PWT Dataset)



Notes: This figure compares the GDP per capita and temperature impulse responses of the baseline Bilal and Känzig (2026) PWT model to one with a control for ENSO and a 1960s dummy variable. Shaded areas depict 95% confidence intervals.

While none of these controls entirely collapse the headline effects in BK, they do suggest that the exclusion restriction is fragile. ONI is only one possible way to measure and con-

[17]For the PWT data, we use lags 1 and 6, and for the BU data, we use lags 0, 1, 2, 5, 7. We selected these by minimizing the Bayesian information criterion (BIC) of all model permutations, with a max parameter number according to BK's specified p.

trol for ENSO, and we concede there may be a variety of better controls, as well as other confounders to test for (solar cycles, volcanic eruptions, etc). It could be argued that if ENSO is a mediator of the effect of temperature levels, then adding ENSO controls removes a valid channel. However, ENSO *cannot* be a mediator—that assumption would require that climate change deterministically induces ENSO and ENSO-like economic damages. Thus, we caution against interpreting BK's estimated damages as valid in the context of future climate projections.

3.3.3 Dynamic Confounding

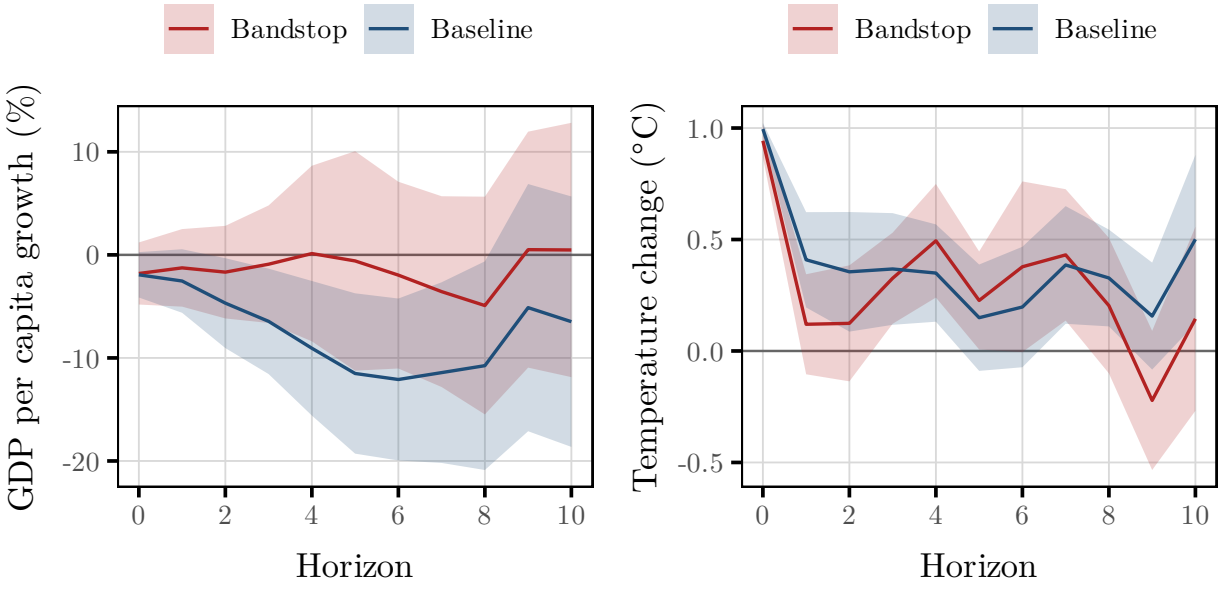
While the PWT dataset is not especially sensitive to ENSO controls, we have discovered a complementary concern that it may be dynamically confounded. First, the PWT analysis begins at an unusual time: the year 1960. As observed in the data, the 1960s experienced both a period of global cooling, which translates to negative temperature shocks in BK, and also the highest average annual global GDP per capita growth. Each of these features are reversed in the 1970s, with the global oil crisis bringing about slower growth, and a return to global warming leading to positive temperature shocks. To alleviate the concern that their results are driven by long-run idiosyncratic correlation, we include a 1960s dummy variable in the PWT specification. This attenuates the IRF ratio from -20% to -14%. A similar control for long-run confounding, allowing for a more flexible quadratic time trend, produces an IRF ratio of -12%. These are major sensitivties which suggest that long-run variation is driving a substantial portion of the effect. Even if this pattern is not the result of a spurious correlation, we should be concerned about relying on explanatory variation from the 1960s to project damages from climate change for the 21st-century.

Furthermore, we see signs that the temperature shock may not be dynamically isolated. BK do not discuss the choice of horizon length for their impulse response functions—i.e. how far in the future the temperature shock is allowed to impact GDP per capita—yet this is an important choice. They only estimate effects out to 10 years in the future, but this seems to

be arbitrarily chosen. If we increase the horizon length, we should expect the IRF ratio to stabilize and converge. Otherwise, we might be concerned that these temperature shocks are being spuriously estimated to have permanent growth effects. By systematically calculating the IRF ratio for longer and longer horizons (up to 25 years), we do see slower signs of convergence for the BU dataset, but the ratio does not stabilize at all for the PWT dataset (Figure G2). The effect continues to grow in magnitude, eventually reaching -36%, nearly twice the original estimate. This is because the GDP per capita impulse response continues to fall for longer horizons (with occasional significance) while the temperature response stays approximately constant (Figure G4).

One explanation for this pattern could be that temperature shocks are not isolated events but are cyclical, and thus, the impulse response is measuring the effects of future shocks as if they were caused by current shocks (either spuriously coinciding with GDP per capita cycles or double counting a true effect). In the long-horizon test, we do visually detect oscillations in the PWT GDP per capita impulse response, at approximately 8-12 year intervals. This is confirmed with a spectral coherence test of temperature shocks and GDP per capita growth, which detects shared frequency components of the two timeseries. This test reveals a strong shared component around the 8-12 year periodicity (Figure G3). Using a bandpass filter, we isolate the 8-12 year periodic component in the temperature shocks and remove it from the timeseries—effectively a bandstop filter. Without this component, the estimated IRF ratio attenuates to -5% (with no statistical significance over the 10-year horizon; Figure 10) and the GDP per capita growth impulse response no longer oscillates. This suggests that the PWT temperature shocks contain cyclical components that undermine the interpretation of the estimated impulse responses as causal effects.

Figure 10: Bilal and Känzig (2026) — Impulse Responses of Baseline vs. Bandstop-Filter Shocks (PWT Dataset)



Notes: This figure compares the GDP per capita and temperature impulse responses of the baseline Bilal and Känzig (2026) PWT temperature shock and the shock after it has been filtered using an 8-12 year bandstop filter. Shaded areas depict 95% confidence intervals.

We cannot determine what is driving these 8-12 cycles, but they could be possibly driven by solar cycles (which have an approximate 9 year cycle), ENSO (and the ONI timeseries is not a sufficient control), and/or spurious correlation with business cycles. For robustness, we apply the same filter to the BU data and do not find any significant change. This could be because the BU temperature shocks are constructed using a more rich series of lags which captures this medium-range oscillation. Overall, these results suggests that the effect in the PWT analysis—which is used to calibrate the paper's climate change damage function—is not well identified.

3.3.4 Adaptation

There is also a misidentification issue if the global economy responds to climate change by changing its locally optimal environment through slope-shifting adaptation. The estimates of BK identify the marginal effect of unexpected temperature deviations around the current

climate, not the effect of anticipated, permanent long-run climate change. To see this, we follow our motivating model; the impact to GDP per capita depends on temperature, T (which is composed of a trend $\overline{T}$ and a shock T^{shock}) and locally optimal temperature T^*, which allows for adaptation: thus, $y = f(T, T^*)$. This locally optimal temperature may respond to the trend in temperature such that $T^* = T^*(\overline{T})$. Thus, the impact to GDP per capita from climate change, which is an increase in $\overline{T}$, is $dY/d\overline{T} = \partial Y/\partial T \cdot dT/d\overline{T} + \partial Y/\partial T^* \cdot dT^*/d\overline{T}$. BK claim to identify $\partial Y/\partial T$, but importantly, this is *not* the effect of climate change. It is the marginal effect of fluctuations in T at a current locally optimal temperature. The effect of permanent, anticipated climate change crucially depends on the $\partial Y/\partial T^*$ component.

3.3.5 Review

We commend BK for moving beyond country-level panel data to estimate and discuss the spillover effects of climate shocks. However, the issues outline above undermine confidence in their projections. Their identification strategy is valid if the exclusion restriction holds, their effect is causally isolated, and there is no adaptation to climate change. Given that these assumptions are fragile, and likely violated, BK's estimated effect is actually a composition of temperature level and temperature shock effects. Projecting these effects forward as though they reflect economic damages from temperature level changes is equivalent to assuming that climate change is akin to an El-Niño-like shock getting stronger and stronger over time (one might call this 'El Hombre'), compared to the baseline. For these reasons, we believe the headline finding of BK should be viewed with skepticism. Despite the clever approach of the paper, it cannot overcome the empirical inscrutability of the climate-economy relationship.

3.4 Broader Literature

We have shown that these three papers (BHM, KMN, BK) use structural restrictions, which abstract from temporal and spatial heterogeneity, adaptation, and the multidimensional na-

ture of climate. These restrictions generate substantial sensitivity in their results. However, our broader thesis is that no estimation strategy can avoid all restrictions of these types and still have sufficient degrees of freedom. Therefore, other papers using similar restrictions as BHM, KMN and BK are likely to have similar empirical vulnerabilities.

We therefore identify the corresponding vulnerabilities that structural assumptions may introduce (Table 8), based on our theory and analysis, and classify additional prominent climate-econometric papers according to these assumptions (Table 9). Exploring these studies in-depth is beyond the scope of this paper; future research should more rigorously assess their results for the potential sensitivities we hypothesize.

Table 8: Structural Restrictions and Corresponding Vulnerabilities

Structural restriction	**Corresponding vulnerability**
Identification: within-country/year variation	Identifies only from within-unit fluctuations; cross-sectional variation and global climate change are differenced out.
Identification: innovations & local projections	Relies on a questionable exclusion restriction; shocks must affect output only through the modeled climate channel.
Identification: functional-form choice (levels/differences/interactions)	Long-run damages are sensitive to specification of persistence and growth effects per Newell et al. (2021).
Low-dimensional climate representation	Risk of omitted-variable bias and misidentification.
Spatial homogeneity	Assumes shared, global relationship; pooled coefficients overweigh extreme or highly variable units.
Temporal homogeneity	Assumes structural stability; estimates are sensitive to sample period.
No locally optimal climate	Confounds weather responses with structural climate change; rules out slope-shifting adaptation.

Table 9: Assumptions of Macro-Level Climate Econometrics Literature

Study	**Identification**	**Climate representation**	**Spatial homogeneity**	**Temporal homogeneity**	**Locally optimal climate**
Burke et al. (2015c)	Within-country/year levels	T, P	Strong	Strong	None
Kahn et al. (2021)	First-differenced within-country/year deviations	T, P	Strong	Strong	Moving average
Bilal and Känzig (2026)	Global innovations (shocks) & LPs	T	NA	Strong	None
Kalkuhl and Wenz (2020)	Within-region/year levels, differences, interactions	T, P	Strong	Strong	None
Kotz et al. (2024)†	Within-region/year differences	T, P, variability, extremes	Weak	Strong	None
Nath et al. (2024)	Country innovations (shocks) & LPs	T	Weak	Strong	None
Waidelich et al. (2024)	Within-region/year levels, differences, interactions	T, P, variability, extremes	Weak	Strong	None
Harding et al. (2025)	Within-country/year levels, differences; global shocks	T, P	Strong	Strong	None

Note: T represents annually aggregated average temperature. P represents annually aggregated total precipitation. "Variability" and "extremes" denote additional aggregated measures of temperature and precipitation which capture those respective qualities. "Strong" spatial homogeneity indicates a fully pooled response with common coefficients across units. "Weak" spatial homogeneity denotes whether marginal effects are allowed to differ by country-level climate means, but a global function is still enforced. "Strong" temporal homogeneity denotes a stationary climate–growth relationship applied consistently over time and in projections. † denotes that the paper was retracted.

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4 Discussion

Empirical estimates of the climate-economy relationship from time-series data imply a wide range of damages from climate change. Some studies imply little to no damage, or even small benefits; others imply catastrophic damages from plausible emissions and warming scenarios (Figure 1). A logical explanation for this variation is that the climate-economy relationship simply cannot be estimated reliably. We provide theoretical and empirical evidence suggesting that is indeed the case—the climate-economy relationship is empirically inscrutable from time-series data (and it may not be scrutable by other methods either; see, e.g., Keen (2021)).

The primary estimation challenge is that there is qualitatively meaningful variation in the climate-economy relationship across both space and time. Some of this variation must be structurally assumed away to preserve sufficient degrees of freedom for estimation. We show theoretically why the spatial and temporal variation should be meaningful enough to create major—not minor—sensitivities when assumed away. We show how influential points, unobservable locally optimal climates, and low signal-to-noise ratios create additional challenges. We demonstrate these challenges empirically, using three prominent papers with diverse methodologies (BHM, KMN, BK) as illustrative examples, showing major sensitivities in each. We posit that similar sensitivities should exist in all other time-series econometric studies of the climate-economy relationship (Tables 9 and 8)—because it is inscrutable.

Our findings should provoke a fundamental reevaluation of how climate-econometric studies are used and referenced in research and policy making. Estimates of economic damages from climate change—and other related quantities like the SCC—affect trillions of dollars in public and private decisions. Often, decision-makers choose one or a small range of preferred damage estimates, which suit their purposes or political preferences (e.g., Democrat vs. Republican administrations' choices of the SCC), and ignore others. We provide specific examples of such practices in the Section 1. Our analysis suggests that these practices are misguided, and they risk producing misleading or unwise decisions. This aligns with

broader calls in the literature to interpret climate damage estimates with caution (Kolstad and Moore, 2020; Newell et al., 2021; Tol, 2024, 2025a; Lemoine et al., 2025).

Relatedly, our results suggest that climate-econometric studies should be much more clear and circumspect about the model uncertainties in their analyses. For example, it is common for studies to visualize error bars and confidence bounds from a single structural model, whereas the true uncertainties could be one or more orders of magnitude larger, as Newell et al. (2021) demonstrate in an analysis that considers only a small fraction of the possible model uncertainties (as the authors acknowledge). Our results also suggest that model uncertainties are fundamentally unquantifiable because all estimable empirical models are likely to have qualitatively important structural restrictions whose assumptions are violated (see Tables 8 and 9).

Importantly, our analysis does not cast doubt on the physical reality of human-caused climate change—which is extremely well established (Intergovernmental Panel on Climate Change (IPCC), 2021)—nor does our analysis refute the notion that human-caused climate change causes negative externalities. Despite the large uncertainties that our analysis reveals, the weight of evidence suggests that climate change most likely has negative impacts on the global economy, all else equal, and these impacts may be especially large in tropical developing countries (Intergovernmental Panel on Climate Change (IPCC), 2022). Even if human-caused climate change caused no economic externalities, GHG-emitting activities cause many other negative externalities, such as pollution and biodiversity loss, to name two (*Ibid*).

However, our analysis does suggest that the magnitude of negative economic externalities of human GHG emissions, via climate change, is highly and irreducibly uncertain. This calls for climate-economics research and policymaking frameworks that explicitly incorporate and/or are robust to deep uncertainty—the subject of a relatively small but rich climate change economics literature (e.g., Costello et al. (2010); Weitzman (2011); Millner (2013); Brock and Hansen (2018)). Heal and Millner (2014) provide an instructive review of theo-

ries of decision-making under deep uncertainty applied to climate change in a broad sense. Conte and Kelly (2021) provide an instructive review of theories related to decision-making specifically under 'fat-tailed' uncertainty—e.g., when extreme outcomes have non-negligible probabilities. Theories in this area tend to favor policies that are: robust to multiple uncertainties and modeling frameworks, adaptable to new information, and designed to minimize catastrophic risks.

GHG emissions mitigation strategies (i.e., policies, private-sector or civil society practices, etc.) with immediate socioeconomic co-benefits are examples of uncertainty-robust, 'no-regrets' strategies. Improving energy efficiency of transportation, industry and buildings; deploying renewable electricity in places where it is already cheaper than fossil-fuel alternatives; and deploying cheap abatements of point-source methane emissions are examples of such win-win mitigation strategies (IPCC, 2023). Such strategies will have multiple benefits for society regardless of how sensitive to climate change economies turn out to be. Their desirability does not depend on cost-benefit analyses weighing immediate costs against highly uncertain future benefits.

Strategies advancing adaptation and preparedness to climate-related hazards and climate change are also robust to uncertainties in the climate-economy relationship. Indeed, the range of climatic conditions that societies will need to adapt to for the rest of the 21st century are substantially less uncertain than the potential impacts of these climatic conditions on the economy (Burgess et al., 2026). Strategies for improving economic development and governance—one of the most important avenues to adapting to climate and climate change, and improving climate sensitive outcomes (crop yields, productivity, security, damage and death rates from climate-related hazards, etc.) (*Ibid*)—are also robust to an uncertain climate-economy relationship.

5 Conclusion

Our analysis asked a deceptively simple but consequential question: *how much do we know—and how much can we know—about the climate–economy relationship?* Drawing on first-principles theory and a critical review of the most influential climate-econometric studies, we argue that the relationship policymakers most care about—the future macroeconomic response to unprecedented climatic change—cannot be credibly identified from historical time-series data. The climate–economy relationship therefore remains deeply uncertain. Recognizing this uncertainty is not a failure of economics, but a necessary step toward more honest analysis and more robust policy and practice. Future research should focus less on extracting ever more precise estimates from insufficient data, and more on understanding the mechanisms of adaptation, resilience, and institutional change that will shape economic outcomes in a warming world.

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A Growth vs. level effects

It is important to be precise about what is meant by growth and level effects. Our model allows climate to impact output through two channels: 1) a primitive level effect, $R_i(\cdot)$, which scales the output of productivity, A_{it}, and capital, k_{it}, and 2) a primitive growth effect, $d_i(\cdot)$, which effects the growth rate of productivity. We use the term "primitive" to describe these effects in how they enter the model and to distinguish them from the dynamic "output" effects that are observed in y_{it}.

Output effects can consist of 1) permanent growth effects, when economic growth rates are permanently lower, 2) permanent level effects, when economic levels are permanently lower (which can arise from temporary growth effects), and 3) temporary level effects, when economic levels are temporarily lower but a rebound occurs, bringing levels back to where they otherwise would have been. Depending on the assumptions of climate-economy relationship, adaptation, and investment dynamics, any of these effects can be produced from our motivating model.

Our model assumes that economies can adapt by changing their locally optimal climate, $\mathbf{C}^*_{it}$ (what we call slope-shifting adaptation), but we purposely do not specify the behavior of this adaptation. However, the dynamics of this behavior matter immensely to growth paths, defining whether there is an economic impact specifically from the *change* in climate, not the level. We will call this a $\Delta\mathbf{C}$-effect. If countries are always perfectly anticipating climate change or cannot adapt, then there can only be economic impacts from the new climate itself: a level-$\mathbf{C}$ effect. Importantly, both effects could exist. An economy may experience damages from climate change where subsequent adaptation reduces the effect, yet adaptation does not entirely return the economy back to the pre-damage effect; there is still a level effect from the new climate (this is depicted in Figure 2).

Thus, we have two possible channels, primitive level effects through $R_i(\cdot)$ and primitive growth effects through $d_i(\cdot)$, and two possible effects from climate change, $\Delta\mathbf{C}$ and level-$\mathbf{C}$ effects. These combinatorially generate four possible effects, any number of which could be

present, each affecting the growth dynamic.

To complicate matters further, varying growth effects can be achieved through different investment assumptions. As with the behavior of locally optimal climate, our motivating model does not make assumptions about investment dynamics. The Solow model assumes a static savings rate, meaning that a one time effect to output persists through capital dynamics. However, if this savings rate is endogenous to output or climate, then output could rebound to its original path, at the cost of consumption, producing a temporary level effect. A static versus endogenous savings rate, in combination with any number of the four possible effects from above, allows for a multitude of different growth dynamics from a one time increase in climate.

To conclude, these effects are not exhaustive; any number of other assumptions about adaptation and investment can be made to produce different growth paths from climate change in our model.

B Country-specific climate effects

To derive country-specific climate effects depicted in Figure 3, the following regression specification was estimated with data from Burke et al. (2015c):

$$y_{it} = \alpha + T_{it} \times \delta_i + P_{it} \times \delta_i + \delta_i + \delta_t + t \times \delta_i + t^2 \times \delta_i. \tag{16}$$

Like Burke et al. (2015c), this specification controls for country- and time-specific intercepts and a country-specific quadratic trend. However, now the temperature and precipitation terms are interacted on a country-specific dummy which allows for heterogeneous country effects. Importantly, because of the country fixed effect, country-specific temperature coefficient must be interpreted as measuring the effect of temperature deviations away from the mean.

Country effects with a p-value of less than 0.05 were selected for Figure 3. To visualize the country-specific temperature effects, a leverage plot is constructed, where GDP per capita growth is partialled-out by removing from all components of the fitted model except the temperature term.

C Signal-to-noise ratio

Here, we derive the economic growth variance decompositions used in Section 2.6. Environmental realizations $\mathbf{C}_{it}$ are drawn from F_{it}, which can decomposed as

$$\mathbf{C}_{it} = \boldsymbol{\mu}_{it} + \boldsymbol{\varepsilon}_{it}, \tag{17}$$

where $\boldsymbol{\mu}_{it} = \mathbb{E}[F_{it}]$ is the evolving climate mean and $\boldsymbol{\varepsilon}_{it}$ is mean-zero weather noise with covariance $\boldsymbol{\Sigma^{\varepsilon}}_{it}$. Locally optimal environment $\mathbf{C}^*_{it}$ is not observable, but if we assume it approximately tracks the climate mean, it can be decomposed as

$$\mathbf{C}^*_{it} = \boldsymbol{\mu}_{it} + \boldsymbol{\nu}_{it}, \tag{18}$$

where $\boldsymbol{\nu}_{it}$ is the idiosyncratic adaptation mismatch (not necessarily mean-zero), with covariance $\boldsymbol{\Sigma^{\nu}}_{it}$

For simplicity, let us write the climate-economy relationship as $f(\mathbf{C}_{it}, \mathbf{C}^*_{it})$. Thus, the econometrician faces the reduced-form data generating process

$$\dot{y}_{it}/y_{it} = f(\mathbf{C}_{it}, \mathbf{C}^*_{it}) + u_{it} = f(\boldsymbol{\mu}_{it} + \boldsymbol{\varepsilon}_{it}, \boldsymbol{\mu}_{it} + \boldsymbol{\nu}_{it}) + u_{it}, \tag{19}$$

which can be expanded via a first-order Taylor series around $\boldsymbol{\mu}_{it}$ to yield

$$\dot{y}_{it}/y_{it} = f(\boldsymbol{\mu}_{it}, \boldsymbol{\mu}_{it}) + \nabla_{\mathbf{C}} f(\boldsymbol{\mu}_{it}, \boldsymbol{\mu}_{it})^{\top} \boldsymbol{\varepsilon}_{it} + \nabla_{\mathbf{C}^*} f(\boldsymbol{\mu}_{it}, \boldsymbol{\mu}_{it})^{\top} \boldsymbol{\nu}_{it} + R_2 + u_{it}, \tag{20}$$

where R_2 is the second-order remainder term. If the partial gradients of $f(\cdot)$ change slowly with respect to $\boldsymbol{\mu}_{it}$, we can assume that they are constant (e.g. $\nabla_{\mathbf{C}} f(\boldsymbol{\mu}_{it}, \boldsymbol{\mu}_{it}) = \widehat{\mathbf{f}}_C$):

$$\frac{\dot{y}}{y} = f(\boldsymbol{\mu}_{it}, \boldsymbol{\mu}_{it}) + \widehat{\mathbf{f}}_C^{\top} \boldsymbol{\varepsilon}_{it} + \widehat{\mathbf{f}}_{C^*}^{\top} \boldsymbol{\nu}_{it} + R_2 + u_{it}. \tag{21}$$

The climate level effect component can be expanded via a Taylor series to yield

$$f(\boldsymbol{\mu}_{it}, \boldsymbol{\mu}_{it}) = f(\bar{\boldsymbol{\mu}}_{it}, \bar{\boldsymbol{\mu}}_{it}) + (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top (\boldsymbol{\mu}_{it} - \bar{\boldsymbol{\mu}}). \tag{22}$$

Assuming $\boldsymbol{\mu}_{it}$, $\boldsymbol{\nu}_{it}$, $\boldsymbol{\varepsilon}_{it}$, and u are mutually uncorrelated, variance of economic growth can be decomposed as

$$\begin{aligned} \mathrm{Var}(\dot{y}_{it}/y_{it}) = &\underbrace{(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}^{\boldsymbol{\mu}}_{it} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})}_{\text{Climate change}} + \underbrace{\widehat{\mathbf{f}}_C^\top \boldsymbol{\Sigma}^{\boldsymbol{\varepsilon}}_{it} \widehat{\mathbf{f}}_C}_{\text{Weather}} \\ &+ \underbrace{\mathrm{Var}(R_2)}_{\text{Curvature}} + \underbrace{\widehat{\mathbf{f}}_{C^*}^\top \boldsymbol{\Sigma}^{\boldsymbol{\nu}}_{it} \widehat{\mathbf{f}}_{C^*}}_{\text{Adaptation mismatch}} + \underbrace{\sigma_u^2}_{\text{Economic noise}}. \end{aligned} \tag{23}$$

Thus, if $\boldsymbol{\Sigma}^{\boldsymbol{\mu}}_{it}$ is much smaller in magnitude across all dimensions relative to $\boldsymbol{\Sigma}^{\boldsymbol{\varepsilon}}_{it}$ (the variance in climate change is small relative to weather), then variance from the climate level component is dominated by the weather component:

$$(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}^{\boldsymbol{\mu}}_{it} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*}) \ll \widehat{\mathbf{f}}_C^\top \boldsymbol{\Sigma}^{\boldsymbol{\varepsilon}}_{it} \widehat{\mathbf{f}}_C. \tag{24}$$

The signal-to-noise ratio of the climate level effect, which is important for estimating the climate-economy relationship, is

$$\mathrm{SNR}_\mu = \frac{\overbrace{(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}^{\boldsymbol{\mu}}_{it} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})}^{\text{Climate change}}}{\underbrace{\widehat{\mathbf{f}}_C^\top \boldsymbol{\Sigma}^{\boldsymbol{\varepsilon}}_{it} \widehat{\mathbf{f}}_C}_{\text{Weather}} + \underbrace{\widehat{\mathbf{f}}_{C^*}^\top \boldsymbol{\Sigma}^{\boldsymbol{\nu}}_{it} \widehat{\mathbf{f}}_{C^*}}_{\text{Adaptation mismatch}} + \underbrace{\mathrm{Var}(R_2)}_{\text{Curvature}} + \underbrace{\sigma_u^2}_{\text{Economic noise}}} \ll 1. \tag{25}$$

This low signal-to-noise ratio presents a practical challenge to confidently estimating the adaptation component of the climate-economy relationship.

As discussed in Section 2.5, econometricians never use the full vector of environmental conditions $\mathbf{C}_{it}$. Rather, they project this variable onto a a low-dimensional index $\mathbf{c}_{it}$

(temperature, precipitation, etc.), compressing $\mathbf{C}_{it}$ via some matrix $\mathbf{P}$:

$$\mathbf{c}_{it} = \mathbf{P}\mathbf{C}_{it}. \tag{26}$$

Let $\mathbf{\Pi} = \mathbf{P}^\top(\mathbf{P}\mathbf{P}^\top)^{-1}\mathbf{P}$ denote the orthogonal projection operator onto the subspace spanned by $\mathbf{c}_{it}$. With a symmetric and idempotent $\mathbf{\Pi}$, the environmental realization vector can then be decomposed

$$\mathbf{C}_{it} = \mathbf{\Pi}\mathbf{C}_{it} + (\mathbf{I} - \mathbf{\Pi})\mathbf{C}_{it}. \tag{27}$$

This first term is the component of $\mathbf{C}_{it}$ observed in $\mathbf{c}_{it}$, which we can call $\mathbf{C}^{\text{Obs}}{}_{it}$, and the second term is the unobserved residual component, $\mathbf{C}^{\mathbf{r}}{}_{it}$. Similarly, we have

$$\mathbf{\Pi}\mathbf{C}_{it} = \mathbf{C}^{\text{Obs}}{}_{it} = \underbrace{\mathbf{\Pi}\boldsymbol{\mu}_{it}}_{\boldsymbol{\mu}^{\text{Obs}}_{it}} + \underbrace{\mathbf{\Pi}\boldsymbol{\varepsilon}_{it}}_{\boldsymbol{\varepsilon}^{\text{Obs}}_{it}}, \tag{28}$$

and

$$(\mathbf{I} - \mathbf{\Pi})\mathbf{C}_{it} = \mathbf{C}^{\mathbf{r}}{}_{it} = \underbrace{(\mathbf{I} - \mathbf{\Pi})\boldsymbol{\mu}_{it}}_{\boldsymbol{\mu}^{r}_{it}} + \underbrace{(\mathbf{I} - \mathbf{\Pi})\boldsymbol{\varepsilon}_{it}}_{\boldsymbol{\varepsilon}^{r}_{it}}. \tag{29}$$

These observed and unobserved components can be substituted into the Taylor series expansion of the reduced-form model:

$$\dot{y}_{it}/y_{it} = f(\bar{\boldsymbol{\mu}}_{it}, \bar{\boldsymbol{\mu}}_{it}) + (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top([\boldsymbol{\mu}^{\text{Obs}}_{it} + \boldsymbol{\mu}^{r}_{it}] - \bar{\boldsymbol{\mu}}) + \widehat{\mathbf{f}}^\top_C\boldsymbol{\varepsilon}^{\text{Obs}}_{it} + \widehat{\mathbf{f}}^\top_C\boldsymbol{\varepsilon}^{r}_{it} + \widehat{\mathbf{f}}^\top_{C^*}\boldsymbol{\nu}_{it} + R_2 + u_{it}. \tag{30}$$

Therefore, the variance decomposition of economic growth is

$$\begin{aligned}\mathrm{Var}(\dot{y}_{it}/y_{it}) = &\underbrace{(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\mu},\mathrm{Obs}} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})}_{\text{Observable climate change}} + \underbrace{(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\mu},\boldsymbol{r}} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})}_{\text{Unobservable climate change}} \\ &+ \underbrace{(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\mu},\boldsymbol{r},\mathrm{Obs}} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})}_{\text{Confounding climate change}} \\ &+ \underbrace{\widehat{\mathbf{f}}_C^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\varepsilon},\mathrm{Obs}} \widehat{\mathbf{f}}_C}_{\text{Observable weather}} + \underbrace{\widehat{\mathbf{f}}_C^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\varepsilon},\boldsymbol{r}} \widehat{\mathbf{f}}_C}_{\text{Unobservable weather}} + \underbrace{\widehat{\mathbf{f}}_C^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\varepsilon},\boldsymbol{r},\mathrm{Obs}} \widehat{\mathbf{f}}_C}_{\text{Confounding weather}} \\ &+ \underbrace{\widehat{\mathbf{f}}_{C^*}^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\nu}} \widehat{\mathbf{f}}_{C^*}}_{\text{Adaptation mismatch}} + \underbrace{\mathrm{Var}(R_2)}_{\text{Curvature}} + \underbrace{\sigma_u}_{\text{Economic noise}} .\end{aligned} \tag{31}$$

The signal-to-noise ratio of the climate change adaptation component of the climate-economy relationship deteriorates even further when $\mathbf{C}_{it}$ is reduced to $\mathbf{c}_{it}$:

$$\mathrm{SNR}_{\mu,\mathrm{Obs}} = \frac{\overbrace{(\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})^\top \boldsymbol{\Sigma}_{it}^{\boldsymbol{\mu},\mathrm{Obs}} (\widehat{\mathbf{f}}_C + \widehat{\mathbf{f}}_{C^*})}^{\text{Observable climate change}}}{\text{All other components}} < \mathrm{SNR}_{\mu} \ll 1. \tag{32}$$

D CRU-WDI Dataset

We develop a new temperature dataset (CRU-WDI) from 1960 to 2023 with updated data from the University of East Anglia's Climate Research Unit (v4.09 of Harris et al. (2020)) and GDP data from the 2024 World Bank World Development Indicators (World Bank, 2024). We use the country shapefiles and population weights directly from the Burke et al. (2015c) replication package. We compare CRU-WDI to the data used in Burke et al. (2015c) in a linear regression model with and without fixed effects. Our data is able to explain variation in the BHM dataset with high R^2 (Table D1). To ensure deviations are not heterogeneous,

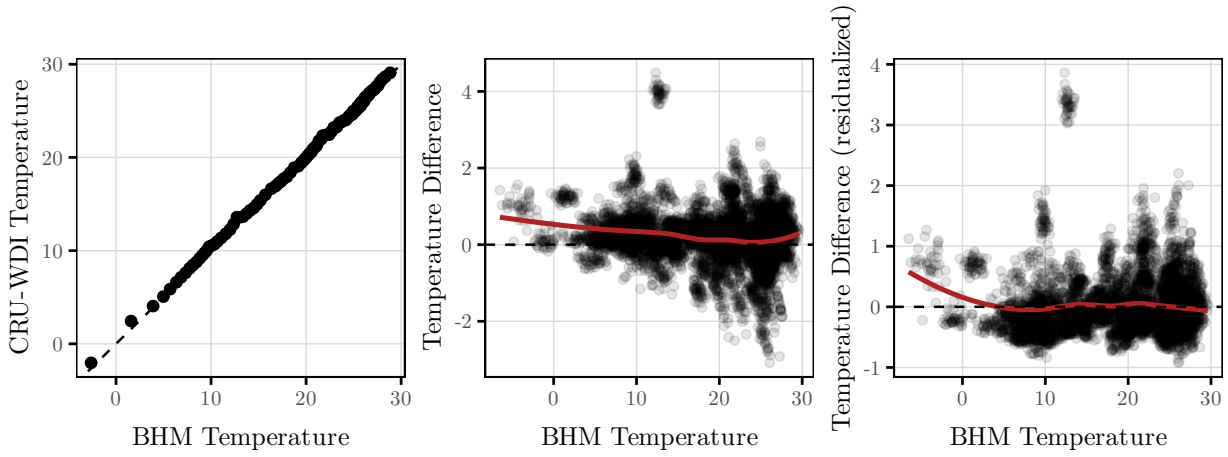
Table D1: Explanatory Regression of Burke et al. (2015c) and Constructed Temperature Data

	BHM Temperature	
	(1)	(2)
CRU-WDI Temperature	1.007***	0.939***
	(0.001)	(0.005)
Country fixed effects	No	Yes
Observations	7,351	7,351
R^2	0.991	0.999

Notes: This table depicts the regression results of temperature data from Burke et al. (2015c) on the constructed data. The second model has country-level fixed effects and compares within-country variation. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

we compare binned country-year observations between the two datasets in a scatter plot of differences (Figure D1). We can attribute many of these differences to small countries, where small differences in the temperature grid can drastically impact the weighted-average. For example, the highest cluster of points in the scatter plot is Bhutan. The third plot in Figure D1 depicts the difference in temperature once residualized on a linear model of the log-area of a country.

Figure D1: Comparison of Burke et al. (2015c) and CRU-WDI Data



Notes: The first panel depicts the mean temperature of the Burke et al. (2015c) dataset compared to the CRU-WDI dataset for binned country-year observations. The second panel depicts the difference in temperature between our dataset and the BHM dataset, with a LOESS fitted line. The third panel depicts the difference in temperature once residualized on a linear model of countries' log-areas.

When combined with GDP data, we gain thousands of additional useful observations but lose 129 observations that are used in the BHM regression. This is because the 2024 World Development Indicators lack economic data for certain country-year observations (depicted in Table D2). We are not concerned that losing this small portion of countries is driving the differences we see. These observations are mostly in the beginning of the sample where our dataset produces similar results to BHM.

Table D2: BHM Observations Missing in CRU-WDI Dataset by Country

	Missing Observations
Venezuela	50
Latvia	25
Djibouti	20
Moldova	10
Romania	10
Slovak Republic	6
Serbia	5
Jordan	1
Mauritania	1
Sri Lanka	1

Notes: This table depicts the number of observations used in the BHM regression that are missing from the CRU-WDI dataset.

E Burke et al. (2015) Replication

E.1 Baseline Replication

Using the replication package provided by the authors, we replicate Burke et al. (2015c). Results are depicted in Table E1.

Table E1: Replication of Burke et al. (2015c)

	Burke et al. (2015c)	Replication
Temperature	0.0127***	0.0127***
	(0.0038)	(0.0038)
Temperature^2	−0.0005***	−0.0005***
	(0.0001)	(0.0001)
Precipitation	0.0145	0.0145
	(0.0100)	(0.0100)
Precipitation^2	−0.0047*	−0.0047*
	(0.0026)	(0.0026)
Constant	1.4575**	1.4575**
	(0.6444)	(0.6444)
Observations	6,584	6,584
R^2	0.286	0.286

Notes: This table compares the reported results from Burke et al. (2015c) to our own for their primary regression. Standard errors are reported in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

F Kahn et al. (2021) Replication

F.1 Baseline Replication

Using the replication package provided by the authors, we replicate the baseline findings of Kahn et al. (2021). For all of our replications, we use the standard fixed-effects estimator, rather than the half-panel jackknife estimator, used in KMN because of implementation difficulties. Results are depicted in Table F1.

Our standard errors are slightly different due to software implementation differences in the numeric calculation of the Delta method.

Table F1: Replication of Kahn et al. (2021) Primary Model

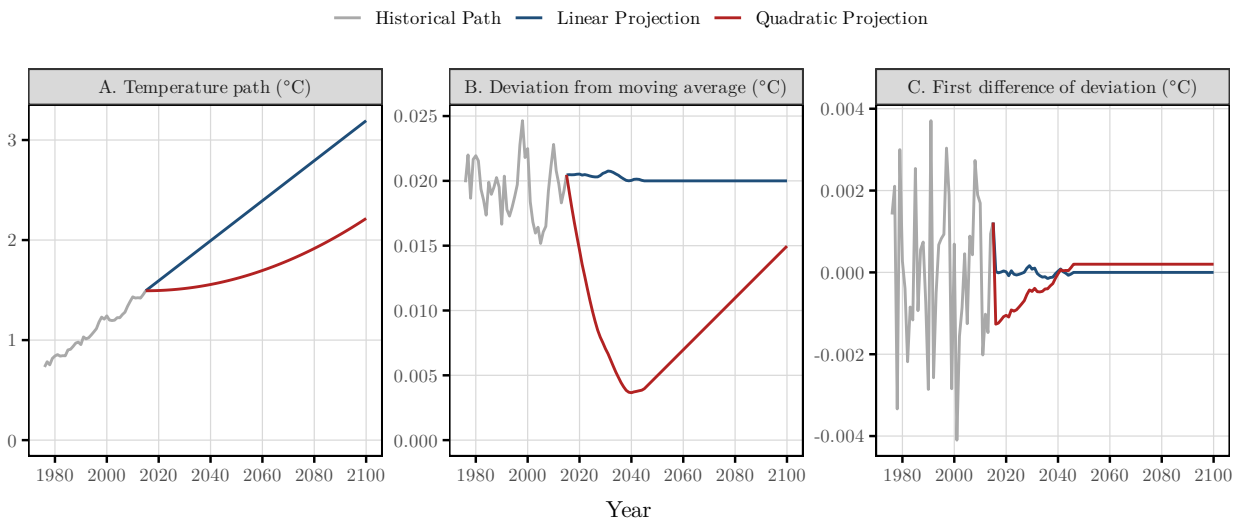
	Kahn et al. (2021)	Replication
ΔTemperature deviation ($\hat{\theta}_{\Delta\lvert\tilde{T}\rvert}$)	-0.582^{***}	-0.582^{***}
	(0.199)	(0.198)
ΔPrecipitation deviation ($\hat{\theta}_{\Delta\lvert\tilde{P}\rvert}$)	-0.032	-0.032
	(0.473)	(0.472)
Persistence parameter ($\hat{\phi}$)	0.671^{***}	0.671^{***}
	(0.049)	(0.049)
Observations	6,714	6,714

Notes: This table compares the reported KMN estimates to our own for their principal model (absolute value of deviations with a moving average length of 30 years). Standard errors are reported in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

F.2 Positive economic projections

We also depict how the counterfactual methodology in Kahn et al. (2021) can lead to economic benefits (Figure F1). Since the authors are not comparing a future with climate change to one without climate change, but rather a future with a quadratic approach to some temperature level to a future with a continuation of the historical trend, there can be a scenario where a particularly low (RCP 2.6) quadratic approach leads to positive effects. This is because the quadratic approach rapidly slows the pace of warming compared to the historical trend. This deceleration translates to positive economic effects, even in the presence of climate change.

Figure F1: Kahn et al. (2021) Projection Methodology—Low Quadratic (RCP 2.6)



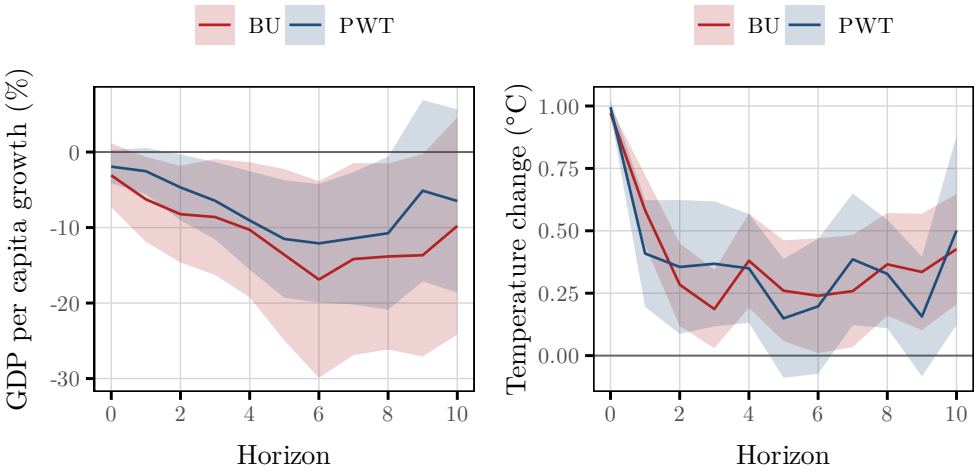
Notes: This figure depicts a conceptual example of the Kahn et al. (2021) counterfactual methodology, like Figure 7. This scenario depicts a lower quadratic projection, such as using a final 2100 temperature from RCP 2.6.

G Bilal and Kanzig (2026) Replication

G.1 Baseline Replication

Using the replication package provided by the authors, we replicate Bilal and Känzig (2026). The impulse response functions are depicted in Figure G1.

Figure G1: Bilal and Känzig (2026) — Baseline Impulse Response Replication



Notes: This figure depicts the replicated GDP per capita and temperature impulse responses of the baseline Bilal and Känzig (2026) PWT model and BU model. Shaded areas depict 95% confidence intervals.

G.2 ENSO Residualization

To residualize the BK temperature shocks on ENSO, we regressed the temperature shocks on a linear model of ENSO lags. We selected the model by minimizing the Bayesian information criterion (BIC) over all model permutations (lags 0-12), with a max parameter number according to BK's specified p. For equal BIC scores, we chose the model with the fewest parameters. For the PWT data, we use lags 1 and 6, and for the BU data, we use lags 0, 1, 2, 5, 7. The results of these models are depicted below in Table G1.

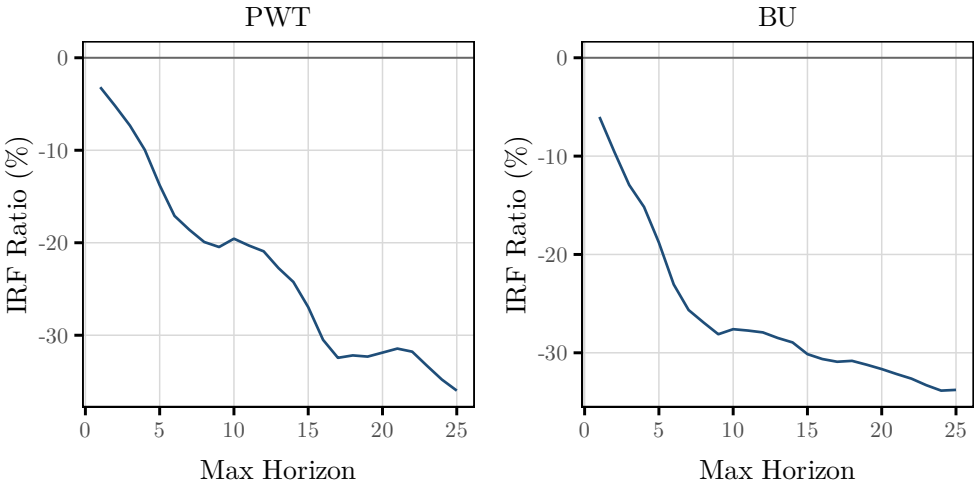
Table G1: Bilal and Känzig (2026) — ENSO Residualization Models

	$T^{\text{shock}}_{\text{PWT}}$	$T^{\text{shock}}_{\text{BU}}$
ENSO_t		0.041^{***}
		(0.013)
ENSO_{t-1}	0.097^{***}	0.073^{***}
	(0.022)	(0.013)
ENSO_{t-2}		-0.059^{***}
		(0.013)
ENSO_{t-5}		-0.048^{***}
		(0.013)
ENSO_{t-6}	-0.055^{**}	
	(0.021)	
ENSO_{t-7}		-0.057^{***}
		(0.013)
Constant	0.003	0.003
	(0.013)	(0.007)
Observations	66	165
R^2	0.307	0.391

Notes: This table depicts the estimated coefficients of the ENSO residualization model of the PWT and BU temperature shocks of Bilal and Känzig (2026). Standard errors are in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

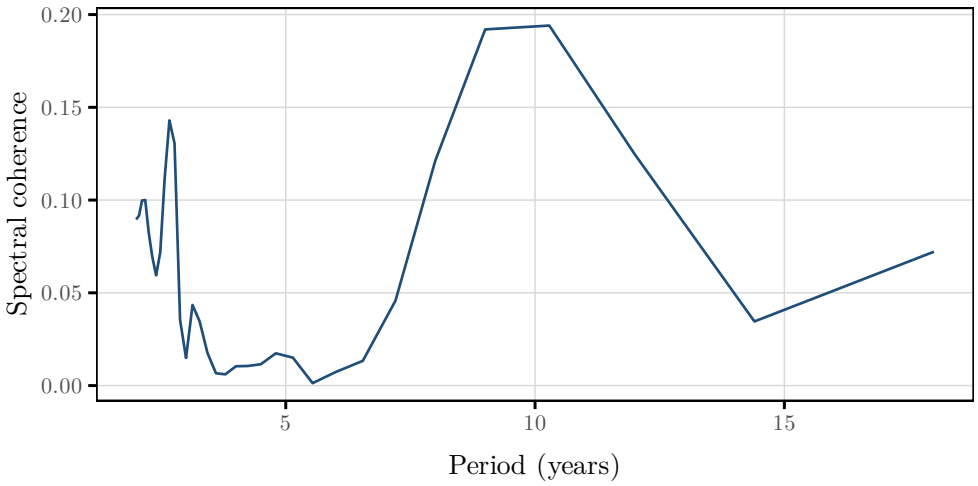
G.3 Supplementary Figures

Figure G2: Bilal and Känzig (2026) — IRF Ratio per Max Horizon



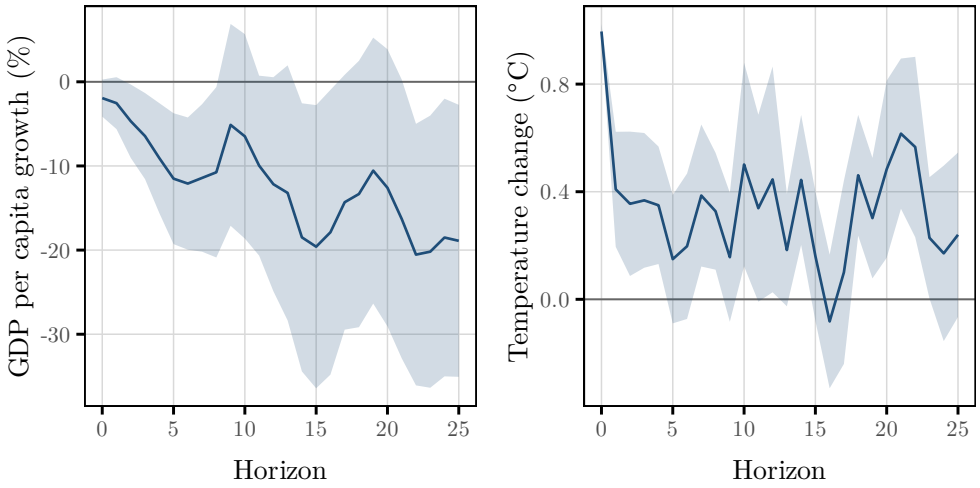
Notes: This figure depicts the calculated IRF ratio over different horizon lengths for both the Bilal and Känzig (2026) PWT and BU datasets out to 25 years.

Figure G3: Bilal and Känzig (2026) — Spectral Coherence of GDP per Capita Growth and Temperature Shocks (PWT)



Notes: This figure depicts the spectral coherence of the GDP per capita growth and temperature shock timeseries for the PWT dataset.

Figure G4: Bilal and Känzig (2026) — 25 Year Horizon Impulse Responses (PWT Dataset)



Notes: This figure compares the GDP per capita and temperature impulse responses of the baseline Bilal and Känzig (2026) PWT model out to a horizon of 25. Shaded areas depict 95% confidence intervals.