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Improving Climate-Change Modeling of US Migration†

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Understanding how adaptation and mitigation will affect how people and economic activity adjust to man-made climate change (CC) is an urgent concern. For the United States with its very high levels of internal factor mobility, migration has been a primary internal-adjustment mechanism to shocks regardless of their origin. Since Graves' (1976) seminal work, it has been known that climate and other natural factors have been a primary cause of the relocation of US economic activity to the Sunbelt, certain coastal areas, and Mountain West. Thus, given the potential for major climatic changes including hurricanes, storms, droughts, flooding, as well as more frequent and longer heat waves (Melillo, Richmond, and Yohe 2014), we expect that US settlement patterns will largely realign as households and firms relocate to areas that will provide higher profits and utility. This realignment will reduce the costs of CC.

The National Climate Assessment Report states that the average US temperature has increased by 1.3–1.9ºF since 1895, accelerating since 1970 due to human activity (Melillo, Richmond, and Yohe 2014). While there was a respite after 2000, recent years have experienced the highest-recorded average global temperatures. Assuming the current path of emissions, US average temperatures are predicted to rise another 8.5–11ºF, producing catastrophic results. Because firm and household migration is a key adaptive response, accurate migration predictions are needed to assess the costs of climate change as firms and households sort to locations that provide higher levels of utility and profits. Though we emphasize the United States, our points apply more generally (see Feng, Partridge, and Rembert 2016 for a more detailed discussion).

To understand marginal benefits and costs of CC in developing policy for (say) 2099, consider that the (economic) costs of seawater rise in South Florida are not simply the lost output in Greater Miami. Instead, it is the difference between what firms would have produced in Miami without CC versus what they would produce in their most optimal location *after* CC (with associated adaptive responses). Yet, current studies tend to *only* consider the costs of extreme heat in their current locations, using late twentieth and early twenty-first century data (e.g., Dell, Jones, and Olken 2014; Deryugina and Hsiang 2014). We have other concerns with the current state of modeling migration in response to CC that we describe in what follows.

I. The Need for Good Theoretical Foundation in Climate-Change Modeling

A key shortcoming of most subnational US CC studies is the lack of a theoretical model, such as the spatial equilibrium model (SEM), to underpin their analysis. In the SEM, equilibrium occurs when neither households nor firms have incentive to relocate to another location, in which household utility and firm profits are equalized across space. While it is unlikely that a regional system is ever fully in equilibrium, the SEM has proved invaluable to regional and urban economists in understanding US regional growth and net-migration in response to economic shocks and long-run migration due to climate (Glaeser and Gottlieb 2009; Partridge 2010).

The SEM assumes that a household's indirect utility is a function of income and site-specific factors such as climate; and a firm's profit/cost function includes input costs and site-specific factors. In equilibrium, the SEM predicts

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that households trade-off lower real income to live in "nice" places and that nice places have high land prices; whereas productive firm locations have higher land prices and wages. A key feature that determines the validity of the SEM model is the degree that households and firms are perfectly mobile to arbitrage utility and profit differentials, which appears to hold in the United States. The SEM can easily be extended to include other factors such as introducing climate risks and associated protective investments that firms and households make to adapt to CC events (Kahn 2015).

The SEM explains the remarkable post World War II movement of people migrating from high-income, densely-populated core areas in the Northeast and Manufacturing Belt to lower-income, sparsely-populated areas in the Sunbelt and the Mountain and Pacific West (Partridge 2010). Consistent with the model, as national income increased, people moved to "nice" places with better climate and scenery (natural amenities being a normal good).

These past and ongoing migration patterns are the opposite of what we would expect later in the twenty-first century due to CC. In the past, households moved away from hotter locations, but this was more than offset by people moving toward warmer winter climates (Rappaport 2007; Partridge et al. 2008). All else equal, Partridge et al. (2008) conclude that between 1950 and 2000, an average metropolitan county that had winter temperatures similar to Orlando would have experienced about 750 percent faster growth than a metro county with Detroit's winter temperatures. This was partially offset by an approximate 500 percent net population loss due to the different July heat and humidity, with people avoiding heat. The outcome is that households and firms are moving to the exact locations that will experience excessive flooding and seawater rise, storms, heat waves, and long-term droughts as CC takes hold. Such migration patterns are persistent and continued into the twenty-first century after a short pause for the Great Recession.

Over the twenty-first century, the SEM model suggests that CC will increasingly induce a significant relocation of people and capital away from locations that are less desirable/productive to places with greater productivity and better amenities (Kahn 2015). Because people and production will relocate, the effects of CC cannot simply be approximated by examining how firm productivity is affected by CC in their current locations; one also needs to assess differences between existing locations and the future locations to which firms will relocate. It is possible that by the twenty-second century, Alaska will be one of the most populated states just as contemporary Florida (FL) is today, as technological change and CC makes Alaska more hospitable. For instance, in 1900, sultry Florida's population was less than one-fourth that of temperate Iowa (IA) (i.e., 1900 pop., FL = 528,542; IA = 2,231,853). From 1900–2016, Florida grew 3,800 percent compared to 40 percent for Iowa, while the United States grew 324 percent (2016 pop., FL = 20,612,439; IA = 3,134,693). Not only does this show how location preferences can change with technological advances such as air conditioning, but also the important role of US interregional migration.

The lack of a good theoretical foundation can also lead to misinterpreted empirical results such as concluding that in the future, firms and households will prefer very hot weather and other adverse weather events based on past trends. The SEM model explains why people and businesses are "voting with their feet" *toward* regions that experience many of the "adverse" environmental features associated with future CC—even as such regions are associated with *lower incomes* and *higher land prices*. Research that concludes that hot temperatures or heat waves are associated with lower income (e.g., Deryugina and Hsiang 2014) need to be careful in interpretation because firms and households have been moving to these locations for decades. If firms lose from heat waves, why have they moved to regions that have them?

II. Adaptation and Non-Marginal Changes Due to Climate Change

Migration's first-order adaptation response to CC should greatly reduce the US costs of CC. Other adaptations which will also reduce costs include better cooling methods, and shifting outdoor and heat-affected work to cooler winter months (much as better summer weather historically led to higher summer employment). Future CC costs would be overstated if such adaptations are not considered (Kahn 2015). That is, reduced-form CC forecasting models would have structural changes, possibly rendering such estimates unusable (the Lucas Critique).

Two other related empirical issues are that CC reflects a non-marginal change and in many cases, far outside the range of past experience. Standard hedonic models are particularly sensitive to non-marginal changes because their theoretical underpinnings are based on marginal changes around the optimum for utility and profit maximization. Another problem of making predictions outside the range of past experience is that potentially large and catastrophic changes due to CC have unknown migration responses based on current research, and there is the distinct possibility of tipping points that will promote much larger responses. Migration responses to contemporaneous hurricanes, for example, appear to be limited. Yet, while (say) one medium-sized hurricane hitting a region once every decade may have had little migration effect, a medium or major hurricane *every year* (say) in 2075 may have much larger impacts.

To highlight these challenges, imagine building an integrated assessment model (IAM) in 1935 to forecast regional population in 2017 (with 2017 levels of modeling expertise). Such a model would likely have many flaws due to using early twentieth century technological and economic parameters in its construction based on pre-1935 trends. The same issues apply in building a similar IAM in 2017 for predictions in 2099. Even as modern IAMs suffer many of the same problems, they are further compounded by trying to model non-marginal changes in climate. Thus, we concur with Pindyck's (2013) skepticism of the value of IAMs with ad hoc parameters.

III. Finding Relevant Climate Measures for the Latter Twenty-First Century

Traditional work on how US interregional migration responds to climate has long used measures such as: average January and July temperatures; number of sunny days; humidity; number of "extreme" hot days such as being above 90ºF; heating and cooling degree days; annual snowfall; and annual precipitation. More recent work has extended these measures to consider events such as tornadoes, hurricanes, and splitting temperatures into bins of (say) the number of days in a given year that fell between 10–15ºC, 15–20ºC, 20–25ºC, and so on (Dell, Jones, and Olken 2014). Probably the largest contribution of the modern CC work is its assessment of how past and contemporaneous US socioeconomic and health indicators are affected by the annual numbers of extreme cold and heat days, but as noted above, the value of these results is unclear for use in parameterizing IAMs for forecasting the latter twenty-first century.

Current research has not yet assessed some offsetting "positive" events of CC including more temperate winters. (With adaptation, more economic activity will shift to winter). For example, while the roles of extreme heat and cold have been explored, other winter events that shut down regional economies have not. As a resident of Minneapolis can attest, it is not extreme cold that shuts down the city; it is blizzards and ice storms, which are not the same, for instance, as annual snowfall. These offsetting factors should also be explored in the future, which will require new measures of climate and weather. Better measures of the extreme weather events that will increasingly matter under CC are urgently needed, rather than deriving more complex IAMs with weak parameterization. For instance, the regional relocation of economic activity will mitigate many adverse effects of extreme temperature. But other events (e.g., prolonged rain; the intensity of hurricanes; storms; tornadoes; and landscape changes that will make areas more or less attractive) are likely to be key factors that also affect future settlement. Better methods of measuring these factors are needed for empirical work. For instance, is it the number of hurricanes or their intensity that matters? Is there a tipping point where hurricane activity acts as a migration push factor? Indeed, while people are drawn today to areas with severe droughts in the Desert Southwest because of its link to sunny weather, there is likely a tipping point such as the Dust Bowl (Hornbeck 2012) where drought conditions repel people.

In sum, while better climate measures would improve analysis, the climate factors that will drive *future* migration are unknown, much as the appeal of warm winters was unknown in 1935.

IV. Other Migration Issues

Other "known unknowns" will also affect future migration. For one, the pace of future productivity and income growth, as well as innovations in adaptation, will affect whether actors have the resources to take precautionary actions such as relocating or taking other precautions to stay. While future migration will likely be a first-order driver of US adaptation, we now lack key knowledge that would help us forecast its role decades in advance. Similarly, CC costs will be higher if the post-2000 decline in migration responses to economic shocks remains permanent (Partridge et al. 2012). (Yet migration responses to amenities are mostly unchanged.)

V. Other Future Research Needs

There are other institutional features that will affect future migration and CC costs. Probably the most prominent is how the different levels of government support affected areas by constructing infrastructure such as large seawalls. Such expenditures promote moral hazard, slowing needed adjustment and increasing CC costs. Likewise, state and local officials in what will be the most impacted areas have near- and medium-term incentives to promote economic development, including providing favorable land-use policy for residential and commercial development and building infrastructure. Not only are such expenditures potentially wasteful, they encourage more households and firms to locate in these areas, further increasing future CC costs. It is easy to see why today's policymakers could place little weight on CC. For example, if using a private discount rate of 5 percent, \$1 in 50 years is worth only \$0.08 today.

These issues point to urgent research needed to identify institutional features in a federal system that will aid adjustment processes under adverse environmental changes. How can the federal government, for instance, properly support state and local governments while minimizing moral hazard? Also, how can state and local government incentives be tailored to internalize future costs of their actions in order to avoid attracting activity to areas that will be most adversely affected? More research is also needed to investigate the roles of the rural-urban divide and of rising income inequality in not only affecting the ability of lower-income households to take adaptive steps, but also whether these corrode key institutions.

VI. Conclusion

Man-made CC poses dire risks to the planet. This paper assesses the role of migration in the US adaptation process. In most CC work, interregional migration has typically been treated as second-order or less. Yet, with high interregional capital and labor mobility, migration should be a primary adaptation mechanism as households and firms relocate to their preferred location, much as US population realigned from the Northeast and Manufacturing Belt to the Sunbelt and western states after World War II. We paint a guardedly optimistic picture in which Alaska and other northern states could gain economic activity. Yet, this picture only applies to large countries with sizable climate variability, along with high internal factor mobility. Canada, China, and the United States are among the few countries that appear to fit these criteria.

Current US CC research, however, tends to examine how socioeconomic indicators were affected by extreme heat in the late twentieth century and early twenty-first century. Besides reflecting virtually none of the adaptations that will occur, they miss the actual economic costs of CC. Particularly, they do not consider the future difference in economic activity between places where economic activity would have occurred without CC and places where actors relocated as a result of CC. Given current knowledge, we believe there is little value of current US based CC change cost predictions given the paucity of knowledge on how migration will adjust.

Instead of developing more complex IAMs that are poorly parameterized (Pindyck 2013), we propose that current research focus on developing a better understanding of migration's first-order role in CC adaptation. Our first suggestion is that rather than start estimating reduced-form models using past data, researchers should adopt a theoretical framework such as the SEM and better recognize the Lucas Critique that reduced-form parameters change as structural factors change. Second, more research is needed in developing climate measures that better reflect the future features of CC such as number and intensity of storms, heavy rainfall events, (fewer) blizzards, and (fewer) ice storms. We feel that future internal migration can greatly limit the costs of extreme heat, but the effects of the other factors are largely unknown. Third, research should focus on identifying better institutions, improving local resilience, and reducing moral hazard.

Even if our guardedly optimistic view is correct, this does not mean the United States should ignore CC. Steps to properly price carbon and other actions are needed to avoid a catastrophe and to support US trading partners. Even if the US economy suffers little, CC will greatly degrade stunning locations with high option values such as South Florida. Finally, as a major historic contributor to CC, the United States is obligated to help vulnerable countries.

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