EDWARD L. GLAESER   The China shock literature, pioneered by Autor, Dorn, and Hanson (2016), now represents a major strand within two fields of economic thought: international trade and regional economics. As this new paper shows, after twenty years there has been virtually no recovery in employment, manufacturing employment, total earnings, or per capita earnings of the regions experiencing the most impact from the shock. The economists of the 1990s and early 2000s, which sadly includes myself, who were almost unalloyed boosters of free trade rarely imagined that either NAFTA or the admission of China into the World Trade Organization (WTO) could lead to such enduring regional dysfunction. The great puzzle for regional economists posed by the facts documented in this paper, and the broader China shock literature, is why did large parts of America prove so unable to adapt. Why was there so little regional resilience?

In this comment, I highlight four hypotheses about America's regional sclerosis: (1) the shock just keeps going and going, like the Energizer Bunny; (2) the shock struck areas that lacked protective human capital, which can be formal schooling, entrepreneurship, or a diversity of industries; (3) America has acquired local institutions that reduce the ability of repurposing labor and capital; and (4) migration no longer enables areas to rightsize themselves in response to negative shocks. As it stands, the paper superbly documents the persistence of the China shock and provides suggestive evidence on the duration of the shock and the absence of protective human capital. Yet there is a clear need for future research to go forward and help us to better understand why the China shock was so persistent.

The stakes are indeed very high, both for current public policy and for planning for future trade events. Understanding the source of America's regional malaise does not immediately imply a policy solution, but it helps point to potential remedies. For example, if the primary problem was that this shock was extremely persistent, then perhaps it would make sense to focus primarily on short-term relief, as we do now, trusting that eventually the shock would end. If the problem lies in regional human capital, then strategies to improve education and entrepreneurship might be warranted. If the problem lies in regulatory limits on adaption, then this suggests removing the barriers to business formation. If the problem lies in migration, then it may make sense to focus on the barriers to building in America's more productive regions.

The value of resolving this puzzle for future trade policy is even more obvious. Many of us were blindsided by the size of the China shock. If we understood better what makes a region susceptible to trade competition,

then we can predict whether a future shock will be more or less disruptive. We would then be able to make better decisions about when and whether to open, and what policies are needed to deal with the downsides of having new trading partners.

I begin by discussing why the lack of local resilience to the China shock is such a puzzle. I then turn to the four hypotheses, explaining them in more detail and describing the empirical implications of each, some of which are already in the paper. I then discuss a possible route toward a decomposition.

WHY IS THE PERSISTENCE OF THE CHINA SHOCK A PUZZLE? Before 1992, economists who specialize in both international trade and regional economics rarely expected that whole regions of America would lurch into crisis if the United States opened itself more to Mexico (NAFTA in 1994) or China (accession to the WTO in 2001). Trade economists certainly knew that America's low-wage workers could lose out from competition with poorer countries, but they rarely considered the regional implications of that fact. Moreover, the vast literature on the rise in returns to skill was just starting, and there was no consensus (and really still isn't) on the role that globalization played in pushing down the wages of less well-educated Americans. Since Adam Smith, the majority of economists have typically disliked economic nationalism, championed free trade, and consequently downplayed trade's potential downsides in our more popular writing.

As I am a regional economist and no expert on international trade, I will focus on why the persistence of the shock was so surprising to those of us who study local economies. Most obviously, America is a large country, and its tariff walls have been high throughout its history. Consequently, trade has played an outsized role in the success of individual cities, which specialized in shipping and commerce, but negative trade shocks rarely if ever did damage to larger regions of the economy prior to 1970.

Major regional shocks came from transportation innovations, like canals and railroads, new technologies, like the cotton gin and the air conditioner, and political changes, like the right-to-work laws that attracted manufacturing after World War II (Holmes 1997). After 1970, it was obvious that competition with Japan was causing difficulty for some manufacturing sectors, such as automobile production. Yet regional economists were more likely to note continuity than change and to see the decline of industrial cities, like Detroit, as a long-running process reflecting automation and the post–World War II relocation of factories to lower-wage areas.

Moreover, regional economists were largely persuaded by Barro and Sala-i-Martin (1992) that income convergence at the place level was

something of a universal law, true across countries and regions and true for over a century. Blanchard and Katz (1992) also highlighted that unemployment rates do not persist across states, partially because healthy emigration from depressed areas led wages in those areas to rise. Even though Decressin (1994) found that in Europe the process of regional adjustment occurred through the much less healthy process of prime-age men leaving the labor force altogether and Glaeser and Maré (2001) documented strong thirty-year persistence of unemployment rates at the city level, these findings seemed irrelevant for larger American regions, which were dynamic and ever-changing.

Moreover, the prevailing theories of regional economics emphasized the existence of a spatial equilibrium in which welfare levels for equivalent individuals were equalized across space. Alonso (1964) first applied this idea to the land market within the city. Rosen (1979) and Roback (1982) applied the idea formally across metropolitan areas, although the basic idea is also present in Harris and Todaro (1970). The power of movement to iron out differences of welfare across space seemed to be guaranteed by the more than 6 percent of Americans who moved across county lines during every year from 1950 to 1992 (Molloy, Smith, and Wozniak 2011).

Spatial equilibrium implies that even if less skilled workers in America as a whole are worse off because of the change in the terms of trade, these workers should not be disproportionately harmed just because they live in one region or another. Going forward, it would be extremely useful if the China shock literature could document that the residents of shocked areas suffer even more than their level of education or industrial expertise would suggest. Linked census and tax records would be ideal for answering this question.

The Harris-Todaro model explains persistent unemployment at the place level with a high minimum wage. That wage attracts migration to the point that expected income is equalized to earnings in the low-wage region. As wages are fixed in one region above wages in the other region, the probability of employment in the high-wage region must be less than one. Hall (1974) took this model to the US data and showed that high earnings and high unemployment rates went together across the United States, perhaps because midwestern unions were so effective in keeping wages up during the early 1970s.

Gradually models were developed with barriers to migration, such as the place-specific taste shocks that have become ubiquitous in the urban literature since Diamond (2016). Other models emphasized the impact of place on human capital formation, which could induce patterns of poverty across

generations (Benabou 1993; Glaeser 1998). But these papers focused more on small neighborhoods than on whole regions.

The nonmathematical and historical tradition in regional analysis similarly emphasized dynamism rather than stasis. While studies of European regions would often seek explanations for perpetual backwardness, for example, Sicily (Banfield 1958), American regions seem to adapt to shocks. Our Mezzogiorno was the Deep South, and by the 1960s it seemed to change rapidly both culturally and economically. Smith and Welch (1989) showed that the Great Migration of African Americans out of the South explained much of the postwar income convergence between African Americans and white people.

One final fact also encouraged the belief that shocked local economies regularly reinvent themselves: the recovery of some (but hardly all) cities from the urban crisis of the 1970s (Glaeser 2011). Slammed by deindustri-alization, suburbanization, and rising crime levels, cities from New York to Seattle seemed like they were facing an existential threat in the 1970s. By the early 1990s, it was obvious that the better-educated citizens of these cities had reinvented them from cities that made goods to cities that specialized in finance and technology. As these positive stories seemed far more interesting than the persistent poverty of less well-educated cities, like Cleveland, the narrative of their recovery played an outsized role in the thinking of many experts on local economies. Even the fate of Detroit seemed like evidence for the spatial equilibrium model, for while its wages did not recover, its housing costs were extraordinarily low. Detroit provided evidence for the durable housing model of Glaeser and Gyourko (2005), which predicts that urban decline can go on for decades because durable housing remains even after the jobs disappear. But that kind of permanent decline seemed far less plausible for whole regions of the United States.

FOUR HYPOTHESES ABOUT THE PERSISTENCE OF THE CHINA SHOCK The following four hypotheses about why the China shock has been so persistent at the place level seem like the most natural explanations, but they are not the only hypotheses.

The China shock just keeps going and going. The most natural way to think about a trade event is essentially as a break in time. Before 2001, the global trading network was in equilibrium without China. After 2001, the global trading network was in equilibrium with China. If everything about the world is static other than that break, then this represents a single shock, and we would expect the world to get to a new equilibrium reasonably quickly. From this perspective, the enduring impact of the China shock on

many American regions is a puzzle. Why aren't they coming to some new equilibrium?

But of course, the China shock does not seem to have been a one-off event. China's entry into the global trading system was followed by massive economic growth accompanied by massive technological development and improvements in education. Consequently, China started with a comparative advantage in goods made with inexpensive labor but then gradually increased its productive edge in a wider range of industries. The shock then was more like rolling thunder than a single lightning strike.

At the regional level, this changing threat may have moved from one industry into another. Firms displaced from a first industry could have moved to a second, which then experienced the full brunt of Chinese competition two years later. Consequently, there may have been considerable adjustment to the China shock, but that adjustment couldn't heal the region because each adaptation was hit by China expanding into a new sector.

The authors do implicitly address this hypothesis at the start of the paper, even if it is not labeled. Figure 1 shows that China's share of world trade continues rising through 2015, which would imply that American regions needed to keep adapting to changing conditions for fifteen years and have had only five years to react to the new equilibrium. If that is the right view of the China shock, then it would seem like this hypothesis is largely correct.

However, the authors embrace Balassa's (1965) measure of revealed comparative advantage for manufacturing, which is a country's share of global manufacturing exports relative to the country's share of world exports of all other goods. As China's total share of global manufacturing exports actually declined between 2010 and 2018, China's revealed comparative advantage in manufacturing relative to the United States stops rising steeply after 2009 as well. Consequently, they see 2010 as the end of China's "productivity growth miracle," which would mean that America has now had over a decade to react, which would make the persistent problems of impacted regions more puzzling.

At this point, I remained unconvinced as to whether the turning point is 2015, as suggested by China's overall share of global exports, or 2010, as suggested by China's revealed comparative advantage in manufacturing. As I am not an international trade economist, I admire Eaton and Kortum's (2002) model immensely, but I do not take it as an article of faith (unlike certain sections of Adam Smith's *The Wealth of Nations*). The figures shown in the paper do not rule out the possibility that China's manufacturing

powerhouse was continuing to expand through 2015, although its non-manufacturing exports were as well.

The authors should address this far more convincingly. Setser (2020) shows that China has continued to rack up ever larger balance of payment surpluses (with the world, not particularly with the United States) through 2015, which is certainly compatible with the shock continuing through that year. The balance of payments surplus is rising, even as exports are stagnant, because Chinese manufacturing imports are declining. This is compatible with a view that China learned how to make a wider range of products between 2010 and 2015 and consequently no longer needed to import them. That could easily have led to more shocks to the already troubled regions in the United States.

I hope that the authors can settle this more decisively going forward, and in the interest of a more exciting puzzle, I hope that they are right that the shock ended earlier. I suggest here a few relatively easy steps to address this topic, but I am sure that they can come up with other, better ways of showing when the China shock came to an end. Most obviously, they can present direct data on annual productivity growth in the manufacturing sector within China and the United States.

A second alternative is to focus on a set of industries within the United States that experienced the greatest impact from the China shock. Was the decline in employment in those industries largely over by 2010, or did it continue for another five years? What happened to total factor productivity in those industries both in the United States and in China over this time period?

A third possibility is to focus on the range of industries in which China expanded. How many different types of goods were being exported successfully by China over time? A simple breakdown of Chinese employment by industrial subclass should work here, with again a focus on the 2007–2016 period that is the subject of contention. Presumably, these facts relate to the decline in China's manufacturing imports, so it might be reasonable to show also the time series of those facts over the same period.

A fourth area is China's nonmanufacturing exports. In a purely static model, of course, China cannot be gaining ground relative to the United States in both manufacturing and nonmanufacturing sectors. In a model with substantial short-term trade deficits, it is quite possible for China to be displacing American exports across the board. Consequently, it would be quite useful to see some data on the time series of nonmanufacturing exports from both countries and whether it is possible that the surge in China's nonmanufacturing exports after 2010 could have also contributed to the malaise of America's underperforming regions.

I focused here on approaches that should be straightforward to implement and collectively more convincing about the time when the China shock came to an end. Others may have much better ideas about how to make this point more clearly. Yet I think it is vital to do a better job of determining whether the China shock ended in 2010 or before or in 2015 or after.

The dearth of human capital hypothesis. Eriksson and others (2021) takes a product cycle approach to regions in which industries move from innovative, high-education regions to low-wage, low-innovation regions and found that "the correlations by 1990 suggest that the China shock industries were no longer leading industries concentrated in high-education areas, but had moved through the product cycle" (6). A hypothesis, associated above all with T. W. Schultz and Finis Welch, is that education or other adaptable skills enable people and regions to adapt to changing conditions. The fact that the China shock hit regions with less-educated labor forces may have meant that these regions lacked the skills needed to reinvent themselves. Eriksson and others (2021) do indeed find that the China shock had far more severe consequences on labor force participation in areas with a less-educated workforce; and by contrast, they found that "the Japan shock was on average hitting communities that had a population with a higher level of education and innovative capacity" and consequently "this may have given those communities a better opportunity to pivot to other industries and tasks" (11).

The regional and urban literature has interpreted local human capital broadly to include formal education, entrepreneurial human capital, and a diversity of industrial skills. For example, Glaeser and Saiz (2004) examine the idea that education mediates the negative effect of cold weather on urban population growth from 1940 to 2000 and find a strong negative interaction between years of schooling and warmth, meaning that education appeared to enable reinvention. Similarly, they found that education in 1940 and the interaction between education and manufacturing in 1940 both predicted the decline in manufacturing between 1940 and 2000, suggesting that education enabled cities to find postindustrial areas of specialization.

Chinitz (1961) emphasized entrepreneurial, rather than formal, human capital in explaining why New York seemed to be more resilient than Pittsburgh. New York had been dominated by the garment industries, which had weak returns to scale and few barriers to entry. Pittsburgh because of its proximity to coal mines, specialized in steel, which at its heyday was practically a natural monopoly. If entrepreneurial human capital is particularly fungible across industries, then it will enable places to develop new lines of business after an event like the China shock. Typically, entrepreneurship

is measured with either average establishment size or share of employ-ment in new establishments at the beginning of the time period, and it is sometimes instrumented for with natural variables, such as proximity to historical mines.

Industrial diversity is also seen as a potential form of local human capital, which both engenders creativity and enables reinvention. Jacobs (1969) emphasized cross-industry leaps of imagination, which she claimed were particularly prevalent in diverse cities like New York, and Weitzman (1998) formalized that idea. The literature on local diversity is less well developed than the work on years of schooling or entrepreneurship, but papers do find that initial industrial diversity does predict subsequent employment and population growth at the local level.

The authors have already taken the first steps to test this hypothesis in figures 9 and 10, which seem to show support for the importance of local human capital. Figure 9 focuses on the interaction between the China shock and formal years of education. The first panel looks at the ratio of manufacturing employment to working-age population, which seems to decline equivalently in both educated and less educated regions. By con-trast, the ratio of employment to population drops substantially in the less educated regions and actually increases in more educated regions. This may be the single most striking and surprising fact within the paper, and it strongly suggests that local skills shaped the impact of the China shock. The third panel shows that the China shock led to a decline in population in the regions with more educated populations, but not in the less educated areas. The fourth panel shows that the China shock had a milder impact on income in less educated areas, but that the impact persisted and if any-thing became more severe over time. In educated areas, the initial income drop was severe, but it had essentially disappeared by the end of the time period.

In short, figure 9 suggests that the dysfunction occurred almost entirely in less educated parts of America. Educated workers reacted by leaving the region. That out-migration combined with whatever other adaptations were going on led the initial terrible shock to income to right itself. By contrast, the response in less educated areas was to leave the labor force entirely. The growth of American joblessness is one of the most terrible correlates of the China shock, although that trend preceded the shock by thirty years.

These findings can be expanded and made slightly more formal (i.e., more *t*-tests between the two groups or systematic interactions in regressions). Again, it would be good to know if this was about the people or the place,

which requires some form of correction for individual attributes. Finally, it would be nice to know about other forms of adaption such as changes in the industrial composition at the local level.

Nonetheless, these findings at least suggest that the China shock may have been so persistent because it disproportionately struck less-skilled places. In these areas, the less well educated were closer to the margin of not working and the shock pushed them over the edge. That caused earnings to fall further because of reduced demand for local services, and more people may have lost their job. There was also less human capital that would enable new business formation. In this view, the surprising persistence of the China shock represents the fact that it struck the most vulnerable parts of America. Those regions then developed a form of the unemployment hysteresis that was first observed in Europe in the 1980s and 1990s. America's formal and informal safety nets (i.e., friends and family) are not generous enough to keep the skilled unemployed from trying to get another job, but maybe they have become generous enough so that the unskilled who are laid off just leave the labor force altogether.

The results on specialization shown in figure 10 suggest that the shock was worse for more-specialized metropolitan areas. Yet it is not clear if this is because these geographic areas were smaller, or if the same size shock was effectively more severe in the more specialized areas. It is also difficult to understand the interaction between a shock based on industrial composition and another measure of industrial composition. As much as I appreciate the transparent figures produced by the authors, this is a case where it would make sense to do this in a regression framework where we can tell if an apparent interaction with specialization is actually an interaction with the size of the agglomeration. Presumably it would also make sense to know if there is an interaction with specialization controlling for the interaction with education.

If the concentration interaction remains robust, then a few further steps seem warranted. First, it is worthwhile distinguishing between concentration in industries that were particularly struck by the China shock versus industries that were not. The level of concentration in China shock industries might well have determined the size of the initial blow. The concentration of alternative industries would presumably influence the ability to branch into multiple different new industries.

Finally, just as it makes sense to look at whether education is helping produce more of an industrial shift in response to the China shock, it makes sense to see whether diversity is allowing more of a reaction. Again, in this case, the natural outcome is some measure of new growth in export

industries—exportable from the commuting zone (CZ), not from the United States—that are not shocked by China. Naturally, it would also make sense to look at horse race regressions with this alternative measure.

The last measure of local human capital is entrepreneurship. In this case, the authors could just take share of employment in new firms or average establishment size or both in 2000 or earlier and again look at interactions between these variables and the China shock. Presumably the same steps should be taken for all three concepts of local human capital, which should be relatively straightforward.

The barriers to adjustment hypothesis. The third hypothesis is straightforward to describe but difficult to test. March and Olsen (1998) argued that small well-organized interest groups gradually manage to acquire power in peaceful societies. Consequently, societies accrete rules that protect those insiders at the expense of outsiders, and these rules limit growth in the country as a whole. He saw Britain in the 1970s as a prime example of a nation where insiders, including the clubby financiers in London and union members, had locked out newcomers and held down the country's performance. Of course, the big bang that deregulated London's stock exchange in 1986 seems to prove both that these rules need not be an absorbing state and that Britain's pre-1986 financial rules were limiting the growth of its financial sector.

In the United States, occupational licensing has expanded dramatically since the 1950s and that can make it more difficult for people to switch occupations or to move across state lines within the same occupation (Kleiner and Krueger 2010). In many cities, a vast number of regulations make it difficult to open new businesses that sell physical goods. Indeed, America seems to regulate the entrepreneurship of the poor, which largely involves physical products, far more tightly than it regulates the entrepreneurship of the rich, which often occurs in cyberspace. The barriers to new home construction can also be seen as rules that protect insiders at the expense of outsiders, but they are part of the next hypothesis.

While I believe that this remains a valid hypothesis, there are only small steps that can be taken to address it at this point. The simplest approach, which is also less than satisfying, is to use the data that Morris Kleiner has assembled on occupational licensing at the state level and to see whether there is any interaction with the China shock. If there is no significant effect, which I suspect will be the answer, then hopefully future work can provide a possible explanation. A similar step could be taken with the various indexes of the regulation of small businesses at the state and local level (Winegarden 2015).

A far more compelling approach would be to look at individual panel data and movement across states and occupations as a function of the presence of licensing, but that is far beyond the scope of this paper. This would require better data on the state of occupational licensing than I believe that we possess, as well as a data set that enables us to look at the mobility of large numbers of individuals across occupations. Nonetheless, the basic question would be whether the need to license deters people who are hit with the China shock from moving into particular occupations.

Ideally, we could form an overall level of licensing based on the occupations that attract workers who are affected by the China shock. We could ask whether the China shock has a larger impact on joblessness in states where this measure of licensing severity is more extreme. We could also form different indexes for men and women, or other specific groups, and then get more variation in the interaction between exposure to the China shock and exposure to a more stringent licensing regime.

A similar path could be taken with new business regulation. If there was a measure of stringency of regulation by industry and location, then we could form a state or locality level of exposure to this regulation based on the industries that typically attract people who are affected by the China shock. That measure could again be interacted with the China shock, and we could test whether the China shock did more damage in places that regulate small business growth more severely.

The closing of the metropolitan frontier. The final hypothesis is an alternative interpretation of the previous one: insiders have made it impossible to afford housing in high-wage metropolitan areas, which means that displaced workers cannot move to find opportunity. Ganong and Shoag (2017) show that directed migration of poor people from low-wage states to high-wage states largely disappeared after 1980. One plausible reason for this is that high-wage states make it difficult to build low-cost housing, and consequently the costs of living eliminate any wage gains that would come from mobility.

This fact similarly helps explain the rise in the returns for low-skilled workers found in Autor (2019). In fact, the return to urban agglomeration remained as strong in 2010 as it was in 1970 for low-skilled workers who live in high-skill metropolitan areas. However, the returns to living in large, low-skill metropolitan areas have completely vanished, and low-skilled individuals naturally live disproportionately in low-skill and low-wage metropolitan areas. Those metropolitan areas have low living costs, largely because durable housing remains long after an area's productivity vanishes (Glaeser and Gyourko 2005). Conversely, well-educated people are particularly good

at erecting barriers to new building, and consequently the limits on construction are particularly fierce in highly educated metropolitan areas and the prices of homes are also particularly high.

As figure 9 shows, population levels decline in more-skilled CZs and then per capita personal incomes increase. That recovery could reflect a tighter labor market for the remaining workers or selective out-migration by those who were most severely affected by the China shock. Yet there is no population loss from the less-skilled areas that were hit by the China shock, and that may reflect the limited supply of housing in attractive migration sites.

How could we test this hypothesis? The rise in housing regulation is nationwide, but there are stronger migration linkages between certain metropolitan areas. Schubert's (2021) job market paper studies the flow of housing market contagion across sites that are linked by migration networks. As these migration networks have been measured, it is relatively straightforward to measure the migration-weighted housing price index for every CZ. At a point in time, that index would be higher if a particular city is more likely to send people to areas that have higher prices. The index would be easy to adjust over time, and so consequently it would be possible to test whether population out-migration from place A is higher when the average price index of its linked areas is lower. It would also be easy to check if the China shock's impact on population was larger during periods when a place's migration price index was lower.

A somewhat better measure would take into account both the prices and the wages in the migration-linked area. The cost of living could be subtracted from the average earnings of less-skilled households. That total measure could then be weighted by migration links for each CZ. Again, the goal would be to see if there was more population loss when a CZ's migration links offered a better bundle of earnings minus costs.

Alternatively, the American Community Survey could be used to look at actual migration patterns. These could be linked with housing costs and the China shock. An individual migration regression could then be run that tests whether the China shock induces significant migration to high-wage, high-cost areas. The absence of such an effect would also provide evidence for the hypothesis that limited housing supply is preventing people from moving out of depressed areas.

TWO DECOMPOSITIONS OF THE CHINA SHOCK'S PERSISTENCE As the persistence of the China shock is unlikely to reflect any one of these hypotheses alone, it would be nice to have some way of apportioning credit to the

various causes. Two different decompositions might help allocate the quantitative importance of the different hypotheses.

Decomposition 1: Timing, geography, and out-migration. The first decomposition begins with a simple, macro-like equation linking the economic outcome of interest, such as the employment to population ratio, with the China shock and other attributes:

$$y_t^{cz} = \sum_j \left(\beta_{0,j}^{CS} + \sum_a \beta_{a,j}^{CS} X_a^{cz} \right) CS_{t-j}^{cz} + \textit{Other Controls} + \varepsilon_t^{cz}.$$

In this equation, y_t^{cz} is the outcome; $\beta_{0,j}^{CS}$ is the direct effect of the China shock occurring j periods ago; $\beta_{a,j}^{CS}$ are various interactions with local attributes such as education, entrepreneurship, antibusiness regulations, and industrial diversity; X_a^{cz} are the time-invariant levels of the attributes; and ε_t^{cz} is the error term.

In order to take a stand on the first hypothesis some view about how shocks decay over time is needed. One option is to assume that $\beta_{i,j}^{CS} = \delta^j \beta_{i,0}^{CS}$ for all of the attributes and then to use an estimate of δ either pulled from the literature or taken from parallel studies of coal and the business cycle.

The first natural decomposition is to ask how different would America look today if the China shock had been shorter lived or if the China shock had hit different parts of America. How much lower would regional equality be if, instead of hitting the places that had these industries in 2000, the China shock had hit the places that had these industries in 1970 or 1910—at least according to Eriksson and others (2021)? These thought experiments seem straightforward, and if the interactions are to be believed, they would be pretty interesting.

The lightest version of this occurs if only one interaction (education) is considered and timing is not a concern. In this case, one would simply take the basic China shock results and ask how different America would be today if the China shock had struck different geographies. This is close to the authors' comparison figures between more or less educated areas, but it gives us a chance to be somewhat more systematic.

To add in the effects of the closing of the metropolitan frontier, we would need to take a stand on the impact of out-migration on the outcome of interest. The equation might instead take the form:

$$y_t^{cz} = \sum_j \left(\beta_{0,j}^{CS} + \sum_a \beta_{a,j}^{CS} X_a^{cz} \right) CS_{t-j}^{cz} + \theta ln\left(\frac{Pop_t^{cz}}{Pop_{2000}^{cz}} \right) + \textit{Other Controls} + \varepsilon_t^{cz}.$$

In this formulation, θ would presumably be negative for out-migration to be doing any good to the local economy. The right step then is to simulate how many people would have left the CZ if there was more building in the high-value metropolitan areas. Yet to do that properly, a full economic geography model would be needed, perhaps along the lines of Diamond (2016). Consequently, this is surely best left for future researchers.

Decomposition 2. The second decomposition that might be useful is focused just on employment flows, not on the four hypotheses. The goal of this decomposition is to look at the total change in employment in the impacted sectors and then apportion that change to out-migration, employment growth in other sectors (perhaps by broad category), and non-employment. This would only really make sense within a particular age group, either 18–64 (which the authors use) or the standard prime-age group, which is 25–54.

Using the equality $Pop_t^{cz} = MEmp_t^{cz} + OEmp_t^{cz} + NEmp_t^{cz}$, where $MEmp_t^{cz}$, $OEmp_t^{cz}$, and $NEmp_t^{cz}$ stand for manufacturing employment, other employment, and nonemployment, we get:

$$\frac{MEmp_{2000}^{cz} - MEmp_{2020}^{cz}}{Pop_{2000}^{cz}} = \frac{Pop_{2000}^{cz} - Pop_{2020}^{cz}}{Pop_{2000}^{cz}} + \frac{OEmp_{2020}^{cz} - OEmp_{2000}^{cz}}{Pop_{2000}^{cz}}$$

$$+ \frac{NEmp_{2020}^{cz} - NEmp_{2000}^{cz}}{Pop_{2000}^{cz}}.$$

These three categories can then be related both to the China shock and the various interactions. This is not far from what the authors have done, but it helps us to see clearly how much of the manufacturing change is related to nonemployment, out-migration, and the growth of other industries.

At the least, I would again separate out other employment into local service employment (which should certainly decline) and other export industries. This decomposition is useful if readers want to think about whether the local attributes are influencing the growth of employment in other sectors.

CONCLUDING THOUGHTS The work of Autor, Dorn, and Hanson on the China shock is of first-order importance both to economic science and public policy. It matters for trade policy and it matters for policies that relate to the differing fortunes of America's regions. We are at a point where regional heterogeneity feels particularly painful, especially in the political realm, and so this work is incredibly useful.

Looking forward, I think it is important that future regional work on the China shock differentiate three broad classes of outcomes: (1) those outcomes that directly relate to welfare (such as income and nonemployment rates), (2) those outcomes that capture the direct impact of the China shock itself but do not have any immediate welfare interpretation (employment in the most impacted industries or employment in manufacturing), and (3) attendant outcomes that help to test hypotheses about why the China shock persists or why it has such significant impacts on welfare. All of these outcomes are interesting, but not all of them are equally important. Some form of model is needed to produce coherent estimates of welfare losses or total impact.

For example, total area population fits in the third category unless you want to take a stand on a spatial model where population loss shows that the region as a whole has become less attractive. In that case it should be always joined together with housing prices. Income, population, and housing prices are typically used together in regional work that attempts to sort out whether local changes are coming from declining productivity, changing local amenities, or more abundant labor supply. Traditional regional work assumed perfect factor mobility for labor and capital. Social welfare was captured by the returns to and value of the geographically fixed factors of production and consumption, such as land.

More sophisticated modern work on economic geography has assumed limited mobility of labor and idiosyncratic tastes for particular locations. With those assumptions, labor becomes quasi-fixed, and consequently workers can suffer when their region goes into economic distress. Hopefully, future work in this area will use the new tools of spatial economics and structural estimation to better understand the welfare consequences of place-based shocks.

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GENERAL DISCUSSION Austan Goolsbee first offered a summary of the paper, noting that the persistent shocks documented in the paper are tied to communities that were highly concentrated in lower-skill manufacturing industries and that the impact that comes from the China shock specifically needs to be separated from the long-term trend of decline in manufacturing employment—was a decline inevitable and merely happened sooner from the shock? Goolsbee also wondered what it was that seemed to make adjustment in US low-skill manufacturing locations so much worse than elsewhere in the world. Goolsbee noted that Chinese competition was predominantly in lower-skill manufactured goods; but the US manufacturing share of employment is actually quite low, and lower-skilled manufacturing is only a fraction of that, whereas in a country like Mexico, manufacturing is a large share of employment and lower-skill manufacturing is the largest component. Goolsbee argued that with so much overlapping of what China exports and what Mexico manufactures, it would seem like the China shock should have been substantially larger on Mexico than on the United States. But Goolsbee then referred to a paper by economists at the Inter-American Development Bank that suggests only a modest impact of the China shock on Mexican labor markets.[1] He said he could not understand why US labor markets would be less flexible than in Mexico following a smaller shock.

1. Juan Blyde, Matias Busso, Valeria Faggioni, and Dario Romero, "The Impact of Chinese Competition on Mexican Labor Outcomes," working paper, 2017, https://www2.gwu.edu/~iiep/waits/documents/Blyde.pdf.