

REPLY TO:
Comments on “*Presidential Address: Demand-Side Constraints in Development.
The Role of Market Size, Trade, and (In)Equality*”

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WE THANK Tim Bresnahan, Paula Bustos, and Dani Rodrik for their insightful comments and their engagement with our work. We broadly agree with their comments, which suggest many productive directions for future research. Bresnahan’s characterization of the empirical threshold model as a portmanteau structure, with its associated strengths and weaknesses, is spot on. Indeed, the advantage—and weakness—of our approach is that it does not require us to specify the intermediate steps (firms, industries, policies, etc.) needed to realize scale economies. Importantly, as Bresnahan points out, our results should not be interpreted as a call for industrial policy—they can also justify broader policies, such as investments in infrastructure or human capital. While specific policy recommendations are beyond the scope of this work, we emphasize its two main policy-relevant messages:

First, in early stages of development, growth and equality are complements, not substitutes. This needs to be emphasized in many low-income settings where policymakers still perceive a tradeoff between growth-promoting policies and a more equitable distribution of resources.

Second, the recent rise of economic nationalism and escalation of geopolitical tensions pose a serious threat to the development of low-income countries, especially those with small economic size. The literature has identified many mechanisms through which cross-border trade fosters prosperity. Our work emphasizes a particular one, namely, access to large, ideally high-income, foreign markets. Moreover, we argue that access to such markets is becoming a binding constraint in the current political environment. In this sense, we embrace the strong version of the argument Rodrik outlines.

To be clear, our argument is not that conventional development policies—addressing market failures, improving institutions, investing in human capital and physical infrastructure—are no longer effective. Rather, our argument is that such policies are by themselves not sufficient to spur development and reduce poverty. They need to be accompanied by a vision about job creation—good employment opportunities that will lift people into the middle class. Export-led industrialization provided such a vision in the past, especially for smaller countries. As this paradigm is increasingly coming under strain, the need for—and to date, absence of—a viable alternative is becoming apparent.

Both Bresnahan and Rodrik bring up the issue of competition on the supply side. They are absolutely correct in pointing out that in a world with open borders, international trade may also imply more competition for domestic producers. Just as trade enlarges the market available to domestic firms, it exposes the domestic market to competition

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from foreign firms. In fact, this is precisely what gives rise to arguments not for free trade, but for infant industry protection. The theoretical framework underlying our analysis abstracts from this possibility. The empirical model does not incorporate competition from other countries explicitly either, but accounts for it implicitly, by using a measure of international market size based on relative per capita income. Of particular interest is competition from low-wage countries, such as China, which is mentioned by both Bresnahan and Rodrik as an example. China's entry into the world trading system and the effect that this entry had on African economies illustrate clearly both the concern and how it is handled in our paper. As Figure 4 of the paper shows, until the 2000s, Africa experienced rapid growth in its international market size as African countries were signing trade agreements, such as GATT, with richer countries. Then, in 2001, when China enters the WTO, relative income of Africa's trading partners falls, and accordingly, the size of the integrated market to which Africa has access shrinks. Countries in Africa, and other continents, no longer had just open markets with rich buyers, but a rival in their income bracket. This change in countries' income-based international market size may have had important implications for development—a point also made in a recent paper by [Atkin, Costinot, and Fukui \(2022\)](#), who argued that China's entry into world markets pushed many countries, especially in Africa, towards the bottom of the development ladder.

Of course, China is no longer a low-income country, and it is in principle possible that as advanced economies in the West are turning inward, China takes the place of the United States and Europe as a lucrative destination for developing countries' exports. Experience to date does not, however, provide much support for this view. In recent work on the effects of the U.S.—China trade war on “bystander” countries, [Fajgelbaum, Goldberg, Kennedy, and Khandelwal \(2020\)](#), [Fajgelbaum, Goldberg, Kennedy, Khandelwal, and Taglioni \(2023\)](#) documented that while the trade war reduced (as expected) trade between the two rivals, it increased global trade by boosting exports of bystander countries to both the U.S. and the rest of the world. The only destination to which bystander countries did not increase their exports was China. In fact, exports of third countries to China sharply declined. While the reason for this decline is not completely understood, the pattern of global trade reallocation does not support the view that China is becoming a global importing power.

An additional consideration is that in open economy settings that are more realistic than the simple theoretical model motivating our analysis, the effect of agricultural productivity growth or commodity price booms on industrialization is ambiguous, as we discuss in Section 3.1 of the paper. This is because such positive shocks can boost comparative advantage in agriculture or the natural resource sector, leading to Dutch disease type effects that ultimately retard industrialization. Whether such effects are relevant in practice is an empirical question. In the empirical exercise, we do not restrict the signs of the agricultural productivity and export growth coefficients. As [Bustos](#) points out, our empirical results do not support the hypothesis that during our sample period, and for the countries included in the sample, agricultural productivity growth contributed to development and poverty reduction. However, export growth did.

In this sense, our empirical approach does not assume away potentially adverse effects of international trade—be it through intensified competition in the domestic market or through a Dutch disease type of mechanism; but our results suggest that—these countervailing forces notwithstanding—on net, trade was on average beneficial for low-income countries in the past decades.

In the quest for a vision for the future, the service sector is often suggested as a potential engine of growth. A recent paper by [Fan, Peters, and Zilibotti \(2023\)](#) indeed argues that

service-led growth drove an increase in living standards in India. The paper's findings are a basis for optimism. Yet, the experience of India as well as that of several other countries that did not rely on export-led industrialization for growth suggest caution.

As a starter, India is a large country (by any measure of market size). Our point of departure has been that the current constraints to growth may be less binding for large economies. That said, even large economies may experience slower development in the absence of international trade.

Indeed, the lack of international integration, failure to participate in global supply chains, and recent inward, nationalist orientation of India have often been blamed for the relatively slow progress of India relative to China. The service sector may lead to growth, but not to growth miracles. And to date, it is questionable whether growth in services will generate the positive developmental impacts that export-led manufacturing growth has had in the past. For instance, Fan, Peters, and Zilibotti showed that the gains from service-led growth in India were concentrated among high-income households, living in urban areas. Hence, it is not clear that they contributed to poverty reduction and sustained, long-term development. Poverty reduction in India has in fact been a black box in recent years due to the lack of poverty data. As we point out in the paper, growth and poverty reduction are highly correlated; but growth does not automatically lead to poverty reduction.

Many of the examples Rodrik gives reinforce this point. For instance, the experience of many African countries that underwent urbanization without industrialization showed that the rise of services does not necessarily lead to sustained development. Along the same lines, the model in Diao et al. suggests that demand-driven growth, led by the service sector, eventually runs out of steam if it does not entail increasing returns to scale. Once again, this is very much consistent with the argument of our paper and the experience of many countries, especially in Africa. There, many countries experienced short-term growth spurts thanks to commodity price booms. Yet most of the growth was driven by consumption services, and it deflated as soon as commodity prices fell. This is precisely our point. Commodity price booms and the rise in consumption services can lead to short-term growth. But growth is not the same as development, and to date, it is unclear that the latter can be achieved without scale economies or some other mechanism generating a virtuous cycle.

An interesting research question in this regard is whether certain types of services are associated with scale economies and positive productivity impacts similar to those documented for manufacturing. Business and ICT services are obvious candidates. If this is the case, then the label "manufacturing" or "services" becomes irrelevant. The same concerns and constraints related to market size identified in the context of manufacturing will apply to services. The difference is that an internationally integrated market in services requires deeper integration than in manufacturing, and this seems even more unlikely in the current environment.

"Deep integration" calls for a clarification of our integration measure. Our measure of historical international integration includes very few deep integration provisions, that is, provisions regulating capital, ideas, and labor. Instead, it includes mostly standard trade agreement provisions related to the flow of goods and services. Further, the estimates with and without deep agreements are similar: defining the integration measure using only goods and services provisions, as in column 4 of Table IV, gives very similar results to the baseline specification in column 1 of Table IV, which includes all possible provisions. It is fair to say that, despite potentially adverse effects of international competition, international integration—as measured by these standard provisions—helped many countries

escape poverty. However, when we get to the “full integration” counterfactuals, these do simulate the effects of “deep” integration, which could be materially different. It may be a stretch to say that integrating capital markets will have the same effects as input tariff liberalization in the 1980s. That said, a lot of the remaining integration still has to take place in areas of goods and services, especially in agriculture where developing countries have a comparative advantage. It is not just about capital and ideas. Beef was a big issue in RCEP, for instance; AFCTA may be signed but is not implemented.

In interpreting the historical experience, we know better than to claim that our story fits every single case of poverty reduction around the world. And surely, many other factors and policies have contributed to the success stories. But broadly speaking, the East Asian experience supports our mechanism. We agree with Rodrik that in many ways, the growth miracle of East Asian economies rests on trade—not necessarily *free* trade. Several countries implemented industrial policies that benefited their domestic industries and engaged in active export promotion. Nevertheless, these strategies would not have delivered if countries did not have access to lucrative foreign markets. Vietnam, for instance, has sustained poverty reduction from 2006 to 2015 during which time its (relative) international market increased. It joined WTO in 2007 and signed several regional agreements. Thailand joined GATT in 1982 and has sustained poverty reduction for our whole sample. Exporting allowed these countries to achieve scale. Well-chosen industrial policy may have helped, certainly, but it likely would not have been sufficient without a large international market.

Latin American cases are much more ambiguous, as Bustos points out, but still have elements of our story. Most countries in Latin America are middle-income countries by the time our sample period starts, while the mechanism we highlight applies to low-income settings characterized by high extreme poverty. That said, the import substitution strategies several countries in Latin America pursued in the 1960s and 1970s sheltered the domestic market from foreign competition without giving domestic producers access to foreign markets. The failure of such policies is well documented. In Mexico, extreme poverty measured by the World Bank spiked to 18% around the time they joined NAFTA. But afterwards, poverty has fallen steadily as exports to the U.S. boomed. The 1996 spike in poverty was to a large extent driven by the peso crisis, which was partially linked to international integration but also to domestic policies. On net, it is hard to argue integration with the United States has been bad for the Mexican economy.

The case of Ethiopia is intriguing. An easy answer to Rodrik’s point would be that Ethiopia simply does not fit our story—we do not expect our model to fit every single data point. And the error term allows one to incorporate such examples—a country could be below the threshold, but still get a lucky draw. Still, a study of Ethiopia’s poverty reduction experience provides an interesting angle for evaluating the market size story. One of the factors that have contributed to poverty reduction in Ethiopia was infrastructure investments that improved road connectivity (see [Iimi, Mengesha, Markland, Asrat, and Kassahun \(2018\)](#)). Farmers were subsistence farmers. When roads came, they could start trading, and shops started to open up. So, one might say that this is the same sort of market size mechanism, where domestic connectivity allowed the country to benefit from a large population, even if there was not a middle class. Indeed, low connectivity of domestic markets is another market-size-related constraint that is particularly relevant to African countries and highlighted in the work of [Atkin and Donaldson \(2015\)](#). Rodrik’s comment suggests a fourth channel through which effective market size can be increased: infrastructure investments enhancing transport and connectivity. This channel is also very much in the spirit of Bresnahan’s comment that policies promoting demand growth may

have less to do with picking the “right” sectors, and much more to do with broad investments in physical infrastructure and human capital.

We are in full agreement with Bresnahan that the threshold model does not lend itself to specific policy recommendations—beyond the general principles stated at the outset. We hope that the set of questions he, Bustos, and Rodrik posed in their respective commentaries will inspire future research using a different set of data and methods. Once again, we are grateful for their insightful, constructive comments.

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