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The Economic and Fiscal Effects on the United States from Reduced Numbers of Refugees and Asylum Seekers

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DISCUSSION PAPER SERIES

IZA DP No. 15317

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Center for Global Development and IZA

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ABSTRACT

The Economic and Fiscal Effects on the United States from Reduced Numbers of Refugees and Asylum Seekers*

International migrants who seek protection also participate in the economy. Thus the policy of the United States to drastically reduce refugee and asylum-seeker arrivals from 2017 to 2020 might have substantial and ongoing economic consequences. This paper places conservative bounds on those effects by critically reviewing the research literature. It goes beyond prior estimates by including ripple effects beyond the wages earned or taxes paid directly by migrants. The sharp reduction in U.S. refugee admissions starting in 2017 costs the overall U.S. economy today over \$9.1 billion per year (\$30,962 per missing refugee per year, on average) and costs public coffers at all levels of government over \$2.0 billion per year (\$6,844 per missing refugee per year, on average) net of public expenses. Large reductions in the presence of asylum seekers during the same period likewise carry ongoing costs in the billions of dollars per year. These estimates imply that barriers to migrants seeking protection, beyond humanitarian policy concerns, carry substantial economic costs.

JEL Classification: F62, H60, J61

Keywords: immigration, immigrant, migrant, refugee, asylum, asylee, costs, wages, employment, labor, market, fiscal, public, coffers, welfare, benefits, GDP, growth

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1 Introduction

From 2017 through 2020, the United States government acted to reduce the number of resettled refugees and asylum seekers in the country. The target of this policy is migrants who claim the need for protection from likely persecution if they return to their home countries. *Refugees* are international migrants outside the U.S. designated by the United Nations as requiring protection. If the U.S. grants protection, they arrive here and become *resettled refugees*. *Asylum seekers* are migrants who have arrived in the U.S. and requested protection from the U.S. government, but have not yet received an answer. If the U.S. grants protection, they become *asylees*.

This policy—to reduce resettled refugees and asylum seekers—succeeded. The government cut the number of refugees mostly by restricting entry, refusing new resettlement admissions requested by the United Nations. Annual U.S. refugee arrivals fell by 86 percent between Fiscal Year 2016 and FY2020. The government cut the number of asylum seekers both by restricting entry and by obliging exit. First, it barred entry to people considered likely to apply for asylum at or shortly after arrival (‘affirmative’ applications). The monthly number of affirmative applications for asylum fell by 68 percent between March 2017 and September 2019. Second, it restricted the criteria for granting asylum both to these affirmative applicants and to people who apply for asylum to prevent deportation (‘defensive’ applicants).¹

Here, I discuss one narrow aspect of how these large changes in policy will affect the United States: that is, the likely magnitude of their effect on the size of the overall economy and on public coffers. Refugee and asylum laws were not, of course, designed with economic or fiscal goals in mind. Offering asylum to those who need it is an obligation under international law; denying asylum regardless of need is a violation of the Geneva Convention and its subsequent amendments. Considering the economic effects of changes to refugee and asylum policy simply investigates a question of descriptive fact, and does not question that moral, humanitarian, and

¹In FY2016 the U.S. refugee resettlement quota was 85,000 and arrivals 84,995. In FY2020 the quota was 18,000 and arrivals 11,841 (*Migration Policy Institute 2020*). The monthly number of affirmative applications for asylum in March 2017 was 16,545, and in September 2019 it was 5,243 (U.S. Citizenship and Immigration Services *Asylum Office Workload* monthly reports posted at <https://www.uscis.gov/tools/reports-and-studies/immigration-and-citizenship-data>), with a steady decline in between (*Dougherty 2020*, 45). Some affirmative applicants have been present in the United States for an extended period but have not been apprehended and placed in deportation proceedings, such as people whose earlier visa granted for other purposes has expired, but such affirmative applicants are not the main target of the policy. More on the efforts to reduce asylum seekers in *Meissner et al. (2018)*.

legal considerations are paramount.

Beyond claiming a need for protection, refugees and asylum seekers are economic actors. All are consumers, most are (or become) workers, and many are (or become) investors. All incur fiscal costs by using public services directly or indirectly, and all generate fiscal revenue either directly or indirectly. A policy of reducing their numbers must have economic ripple effects. Estimating such effects is different from assessing the overall merit of the policy, given its many non-economic effects.

A policy causing large reductions in immigration *in general* creates large negative effects on the overall economy and on the fiscal balance of government. There is no meaningful controversy in the economic literature about this general, qualitative conclusion. The consensus report of a diverse and blue-ribbon commission of economists who study immigration, convened by the National Academy of Sciences, concluded that “immigration is integral to the nation’s economic growth” and that “a new immigrant who most resembles recent immigrants in terms of average age and education creates a positive fiscal balance flow to all levels of government with an NPV [net present value] of \$259,000” (Blau et al. 2017, 6, 434). But neither that report nor other available research estimates the size of these effects for U.S. refugees and asylum seekers specifically.

The findings of this study can be summarized as follows. Today there are roughly 295,000 refugees “missing” from the U.S. population due to the 86 percent reduction in refugee resettlement starting in 2017—those who would be present now if refugee admissions during 2017–2021 had stayed at their 2016 levels. These missing refugees cost the overall U.S. economy over \$9.1 billion *each year* (\$30,962 per missing refugee per year, on average) and cost public coffers at all levels of government over \$2.0 billion *each year* (\$6,844 per missing refugee per year, on average). These costs would continue permanently even if refugee *inflows* this year returned to their 2016 levels—because that would not replace the number “missing” from the population due to earlier reduced inflows. Put differently, relative to 2019 levels, a 10 percent reduction in refugee resettlement to the United States likely causes a loss to the American economy of more than \$1.4 billion, and a loss to public coffers (federal, state, and local) of more than \$310 million, cumulatively over the subsequent five years. Turning to asylum seekers: A 10 percent reduction in affirmative and defensive asylum seekers likely causes a loss to the American economy of more

than \$8.9 billion, and a loss to public coffers of more than \$1.5 billion, cumulatively over the subsequent five years.

These costs are large in one sense, small in another. The costs are large relative to legal limits on policy rules by any U.S. Administration that affect the economy. The Congressional Review Act of 1996 gives the U.S. Congress authority to block certain federal agency rules with an “annual effect on the economy of \$100,000,000 or more”, or similar fiscal effects: “...a major increase in costs or prices for consumers, individual industries, Federal, State, or local government agencies, or geographic regions”—provided that the Government Accountability Office agrees ([CRS 2019](#)). The GDP costs estimated here suggest that any Administration rule reducing refugee arrivals by even 3,300 people in a single year would be subject to Congressional review, placing a check on the arbitrary exclusion of refugees. The costs estimated here are small in a different sense: relative to the U.S. economy. Even the \$9.1 billion GDP cost from refugee exclusion estimated here only amounts to 0.04 percent of the \$21 trillion economy.

The paper begins by critically reviewing the academic research literature that can assist in quantifying the economic and fiscal effects of reducing refugee resettlement. The analysis does not simply list or count studies making various conclusions, but instead discusses why and how different types of evidence are reliable for the formulation of policy. Its main innovation in methodological terms is to question the assumption in much of the research literature that the employment of migrant workers does not produce substantial income to owners of capital, and to explore the quantitative consequences of relaxing that strong assumption. It considers studies of the United States and discusses the extent to which evidence from other countries is informative about the United States. It then separately considers reductions in asylum seekers, who differ from refugees in their characteristics and policy treatment. It concludes by discussing scenarios for the economic and fiscal impacts that are consistent the evidence in the literature, along with several caveats.

2 Refugees

A large literature has documented the economically-relevant characteristics of refugees, and their performance in the labor market. Relatively little research, however, explores their economic impact on other actors in the economy, and their fiscal effects on taxpayers. This section comprehensively and critically reviews the relevant academic research literature to inform plausible economic and fiscal impacts of U.S. refugees consistent with the best-quality evidence.

2.1 Economic effects of refugee resettlement

No academic research in economics directly measures the impact of refugee resettlement on the American economy as a whole. Researchers have, however, estimated 1) the traits of U.S. refugees that shape their economic impact, such as education and labor-market performance, and 2) how immigrants *in general* with the same traits affect the economy. Together, these are informative about the economic impact of refugees.

2.1.1 *Effects on the overall economy*

Relative to other immigrants, refugees resettled into the U.S. have traits that tend to confer higher productivity as workers. Refugees are more educated than non-refugee immigrants: They are more likely to have a high school degree, and as likely to have a college degree (Capps et al. 2015; Fix et al. 2017). U.S. refugees are more likely to be employed than other immigrants, or even native workers (Capps et al. 2015; Bernstein and DuBois 2018; Brell et al. 2020, 102). Historically, refugees have acquired greater education and English skills after arrival than non-refugee immigrants (Abramitzky et al. 2022), and earned 20 percent more on average (Cortes 2004, updated by Chin and Cortes 2015; see Connor 2010 for comparisons shortly after arrival).

In other words, it is reasonable to expect that the economic productivity of average immigrants overall (refugee and non-refugee) is a lower bound on the productivity of refugee immigrants specifically. But economists have produced few direct, transparent estimates of the overall economic effects of *average* immigrants. Such estimates require comparing the state of the US econ-

omy in the presence of a certain group of immigrants to what the state of the economy would have been in the absence of those immigrants. This latter, hypothetical economy (the ‘counterfactual’ economy) is not directly observed. Broadly speaking, economists approximate it via two approaches.

The first approach is the most transparent and most relevant to policy decisions. It measures what happens to the overall economy of different parts of the country (states, counties, or metropolitan areas) when a force majeure—not the local economy itself—causes relatively more immigrants to move there.²

One peer-reviewed academic study uses this method to estimate the effect of overall immigration (non-refugee and refugee) on productivity and overall income per capita in the United States. [Peri \(2012\)](#) measures the response of average income per worker in U.S. states 1960–2006 where relatively more immigrants settled due to such a force majeure. He shows that on average, the arrival of immigrants amounting to 1 percent of employed workers raised average income per worker by 0.88 percent measured on average five years after they arrive. This captures the effect of all income—to labor, capital, and land—not only workers’ wages. The method rests on the assumption that economic outcomes in areas into which many immigrants are drawn by a force majeure would be similar to otherwise similar areas with fewer immigrants if the immigrants had not arrived. The two forms of force majeure used in the study are the facts that immigrant inflows during the period in question can be reliably predicted using 1) distance to the U.S.-Mexico border and 2) pre-1960 immigrant populations from each country of origin, neither of which by themselves cause greater state-level productivity growth. Both of these methods are widely considered valid in the research literature and have been used for decades in numerous peer-reviewed studies.

The magnitude of this estimate specific to the United States is corroborated by a large number of studies of the effect of economic growth on groups of countries collectively. For example, the

²The importance of using such a force majeure, with little relation to local economic conditions, is that the state of the local economy can cause migrants to move there rather than vice versa. This might be, for example, the fact that immigrants from a particular country have a tendency to settle in places where their compatriots have previously settled, to a degree partially independent of local economic conditions. Using only the portion of immigration exposure explained by such a force majeure assists in separating the effect of migration from the causes of migration, or the coincidental correlates of migration.

arrival of average immigrants amounting to 1 percent of the population in the average advanced economy produces an increase of more than 2 percent in long-run income per person (Jaumotte et al. 2016). This finding has been found to be robust in numerous macroeconomic studies (Felbermayr et al. 2010; Boultane et al. 2013; Ortega and Peri 2014; Aleksynska and Tritah 2015; Alesina et al. 2016; Boultane et al. 2016; d’Albis et al. 2019; Engler et al. 2020, a literature reviewed by Portes and Forte 2017).³

This effect on economic growth arises mostly because immigrant workers raise ‘total factor productivity’. That is, the effect does not primarily arise from changing the *amount* of the economic factors of labor, capital, or land used in the local economy, but by raising how much output those factors produce in combination (Peri 2016; Brunow et al. 2015; Alesina et al. 2016; Engler et al. 2020; reviewed by Lewis and Peri 2015).⁴

For example, U.S. immigration on average causes firms to create more new technologies (Burchardi et al. 2020) and adopt technologies that take advantage of greater labor supply (Lewis 2011); causes native workers to invest in greater skill (Hunt 2017), shift into more complex occupations (Peri and Sparber 2009; Foged and Peri 2016), and even change industries (Labanca 2020); stimulates demand for housing and other goods (Saiz 2007; Bodvarsson et al. 2008; Hong and McLaren 2015; Sharpe 2019; Howard 2020); brings more native professional women into the labor force (Cortes and Tessada 2011); causes the creation of more new businesses (Azoulay et al. 2020); cushions local economies from sudden downturns (Cadena and Kovak 2016); creates greater opportunity for specialization by raising the diversity of tastes and knowledge in the population (Alesina et al. 2016); and creates more overseas investment and trade (Burchardi et al. 2018). This last effect, a stimulus to international trade, has been detected for U.S. refugees specifically (Steingress 2018), but all of them are likely to act through refugees in some measure.

A second approach to estimating the overall economic impact of immigration uses a different and less transparent method to estimate a ‘counterfactual’ economy with fewer immigrants. It

³See also Portes 2022 and Sumption 2022 specifically on the effects of Brexit.

⁴Albert and Monras (2020) use a model-based simulation to estimate that immigration by less-educated Mexican workers during the 1990s caused an increase of 0.46 percent in total factor productivity in U.S. cities. This estimate is not comparable to the estimates by Peri (2012) because the study of Albert and Monras rests on a structural model of numerous assumptions about mechanistic relationships between different parts of the U.S. economy, does not measure the effect on GDP, considers only Mexican immigrants with relatively low education, and considers only the 1990s.

starts by assuming a large number of mechanistic relationships between different parts of the economy—such as labor markets, capital markets, product markets, and housing markets. It then estimates what would happen to this abstract and simplified ‘model’ of the overall economy *if* that long list of assumptions were correct *and* a number of immigrants were inserted into the mechanism. Careful examples include the work of [Fehr et al. \(2004\)](#), [Drinkwater et al. \(2007\)](#) and [Chojnicki et al. \(2011\)](#), who find large positive effects of average U.S. immigration on natives’ overall economic welfare.⁵

This second approach is useful to pure research in economic science. It has improved on an older literature built on highly abstract assumptions such as zero adjustment by capital or technology to the presence of immigrants (e.g. [Borjas 1995](#)). But even the newer estimates are less useful for policy formulation than the more transparent empirical approaches discussed earlier. It is not possible to confidently determine how much the quantitative answer produced by a complex modeling exercise depends upon the numerous mechanisms assumed or ruled out by the model, singly or jointly. For example, no such model allows for the possibility of all the complex mechanisms discussed above by which immigration shapes total factor productivity.

A reasonable conclusion from the overall literature on economic impacts is that on average, each worker resettled into the United States as a refugee raises the income of all other workers (natives and non-refugee immigrants) collectively by an amount greater than 0.88 of the refugee’s own income per year. This magnitude is measured 5–10 years after refugees arrive and thereafter. This figure is a conservative lower bound because it describes the effect of an average immigrant, who by several measures is less economically productive than an average refugee. In sections to follow, the magnitude of this implied impact will be compared to impacts estimated by methods that are more direct and refugee-specific but less comprehensive of ripple effects.

2.1.2 *Effects on the labor market alone*

A different, narrower, and much more common approach to studying migrants’ economic impact focuses on how immigration affects only the labor market: wages and employment. An obvious drawback is that this approach does not capture the effect of immigration on the overall economy,

⁵See also the calibrated theoretical model for multiple countries of [Ehrlich and Kim \(2015\)](#) or the purely theoretical work of [Brezis and Krugman \(1996\)](#).

though it can be informative about effects on the distribution of income.

One study directly estimates the effects of U.S. refugee resettlement on native workers' wages. [Mayda et al. \(2017\)](#) compare the wages of native workers in commuting zones that received relatively high inflows of refugees to those that received relatively low inflows, between 1980 and 2010. Their estimates can be interpreted as a causal relationship, not just correlation, because they focus on refugees that lack prior ties to the U.S.—and who are thus assigned a location of residence that is not plausibly affected by local labor-market conditions. Within reasonable statistical confidence, they do not detect any effect on the wages or employment of native workers at any level of education. [Gunadi \(2021a\)](#) similarly detects no statistically-significant US labor market effects from the arrival of 140,000 people fleeing crisis in Venezuela.

A large body of other research considers the effects on labor markets from overall immigration (refugee and non-refugee). This literature takes two broad approaches.

The most transparent and policy-relevant approach compares local labor markets within the United States that received relatively large immigration inflows to those that did not. In U.S. cities where immigration raised the number of workers by an additional one percent during 2000–2010, average workers' wages rose by an additional 0.64 percent ([Lewis and Peri 2015](#), 671, for earlier evidence see [Friedberg and Hunt 1995](#), 32). This rise in wages, given that labor receives roughly two thirds of all income ([Gutiérrez and Piton 2020](#)), is consistent with an increase in output per worker of approximately 0.9 percent. This corroborates the (lower bound) effect of refugees on GDP per capita estimated in the previous section. A similar positive impact on native wages is seen for the last four decades of average non-European immigrants across U.S. counties ([Burchardi et al. 2020](#)).⁶ This rise in average wages occurs despite small declines in the *relative* wages of the least educated workers for 1–3 years after very large, sudden, and geographically concentrated arrivals of the least-educated immigrants ([Clemens and Hunt 2019](#); [Monras 2020](#)).

Among the findings of studies using this approach, apparent contradictions are often merely superficial. For example, one study finds that the large and sudden influx of Puerto Ricans into

⁶This conclusion is consistent with the findings of [Mayda et al. \(2017, 25\)](#). A wage elasticity of 0.6 implies that the wage effect of an average 0.1 percent increase in refugee prevalence would be +0.06, which falls within the 95 percent confidence interval of their two stage least squares estimates on page 25, column 1: [−0.089, 0.112].

Florida fleeing a 2017 hurricane caused no net decline in native wages or employment (Peri et al. 2020). Another study finds that a sudden influx of Czech workers into the border region of Germany caused a fall in German wages and employment (Dustmann et al. 2016a). Both studies use rigorous methods. But the study of Florida considers migrants who took up *residence* there, thus they could stimulate the local economy with their demand for housing and other services—with offsetting positive effects on the market for native labor. The study of Germany consider day-worker migrants who work in Germany but live (and spend most of their money) in the Czech Republic, without the same offsetting stimulus.⁷ The two studies are estimating different things, and only the study of Florida is relevant to crafting policy toward immigration that typically leads to lasting settlement—such as refugee/asylum immigration—with all its economic ripple effects. This pattern in the literature is accessibly summarized by Dustmann et al. (2016b).

A second and less policy-relevant approach has been the focus of the most influential work in this literature. A large body of work has explored the effect on the price of labor (wages) when the supply of labor rises—*all else equal*—within some categories of workers defined by personal traits (not geographic areas) more than it rises within other categories. These categories are usually defined by education, experience, and gender, and comprise workers nationwide. Precisely measuring that quantity *all else equal* requires purging the estimates of any side effects from raising the quantity of labor available.

For example, these estimates typically remove any effects of immigration on nationwide innovation and capital investment, any effects of less-educated workers on the productivity of more-educated workers (and vice versa), and any effects on natives' relative occupational specialization or investment in education. This is what the most influential research has done in the past. That limitation of prior work is discussed by Dustmann et al. (2016b), Card and Peri (2016, 1341), and Llull (2018), among others. The older work resting on these strong assumptions typically finds impacts of immigration on average native wages that are “very small” and sometimes

⁷The authors highlight this limitation. They write, “There are several reasons for why the inflow of immigrants may have led to more adverse effects on natives in ours than in other situations. First, unlike in many other contexts, commuting workers did not live and consume in the affected areas, thus reducing possible demand effects induced by immigrant consumption. Second, it focuses on the short-term effects of an unexpected and exceptionally large labor supply shock, affecting a region that had not experienced large immigrant inflows or labor supply shocks in the recent past. Third, the labor supply shock may have been viewed as temporary by firms, making them reluctant to expand capital in response to the shock” (Dustmann et al. 2016a, 437). In a related natural experiment in Switzerland, where the policy was announced well in advance and slowly phased in so that firms had more time to adjust, Beerli et al. (2021) find no average negative effects on native workers.

negative (Blau et al. 2017, 5).

Such estimates are of limited use to policymakers because they rest on an opaque concept of what would have happened in the labor market with less immigration. Policymakers wish to know how immigration affects the economy in reality. They do not wish to know how immigration *would* affect a hypothetical economy *if* firms did not adjust capital and technology accordingly; and *if* natives with the same education could not respond with increased occupational specialization; and *if* the productivity of educated professionals were unaffected by the work of less-educated service workers. This abstract and strictly academic question is not “meaningful and policy relevant” (Dustmann et al. 2016b, 52). Such estimates are nevertheless routinely misinterpreted by policy makers as estimating the effects of immigration in reality.⁸

When the same estimates are corrected to include the many side effects of immigration that were purged by design, the estimates from this nationwide approach agree quantitatively with estimates based on geographic areas: Immigration that raises the number of workers by one percent within nationwide categories defined by personal traits is associated with a rise in wages of 0.5 percent within the average category (Card and Peri 2016, 1345). As discussed above, this is consistent with an effect on overall income per worker from *average* immigration of 0.8 percent. This estimate can be reasonably taken as a lower bound on the corresponding effect of refugees in particular. This is because the literature consistently finds more positive labor-market effects for immigrants who, like refugees, have above-average education and earnings relative to other immigrants.

2.2 Fiscal effects of refugee resettlement

Measuring the effect of refugee resettlement on public coffers is feasible, but not simple. The net fiscal effect is a difference between the flow of public expenditures and public revenues generated by the act of resettling a refugee. Every study makes several choices about which expenditures and revenues to count, and how. These choices include how to compare up-front resettlement

⁸For example, the U.S. Department of Labor (2020, 63883) interprets one such estimate as meaning that “a substantial increase in the labor supply due to the presence of foreign workers reduces the wages of the average U.S. worker by 3.2 percent, a rate that grew to 4.9 percent for college graduates”. That is, it incorrectly interprets an estimate that rules out many of the economic side effects of immigration as an estimate of the overall economic effects of immigration in reality.

costs to longer-term costs and benefits, how to account for refugees' ripple effects in the economy, and how to treat the effects of refugees' children.

The 'accounting' approach is most commonly used to measure the fiscal effect of any type of immigrant. This approach is to simply to add up the taxes they pay and subtract the public benefits that they receive—either at any given moment in time, or over the course of a lifetime.

One academic study directly estimates the net fiscal impact of U.S. refugees. It does so with one version of this accounting approach. [Evans and Fitzgerald \(2017\)](#) estimate the taxes paid and public benefits received by U.S. refugees who enter between the ages of 18 and 45. For taxes they count state and federal income taxes, Social Security and Medicare taxes, sales tax, and property taxes; for benefits received they count relocation costs, welfare, and food stamps. They identify people likely to be refugees in census data by examining immigrants who arrived from certain countries and in certain years where the majority of the inflow was refugees. They find an average refugee who arrives in the U.S. aged 18–45 is expected to pay a present value of \$21,324 more in taxes than he or she receives in benefits, cumulatively over the first 20 years that such a person spends in the country.

One other accounting study with a similar approach was completed by the federal government. It was ordered by the U.S. Administration in March 2017 and completed by the Department of Health and Human Services in July of that year ([HHS 2017a](#)). The study identifies likely refugees and asylees in census data similarly to [Evans and Fitzgerald](#). It is more comprehensive than that study in that it includes a much broader array of fiscal costs beyond direct assistance—such as criminal justice, education, housing assistance, tax credit refunds, and uninsured hospital patients—though neither study includes the fiscal effects of refugees' and asylees' children. A difference with [Evans and Fitzgerald](#) is that the HHS study accounts for actual (past) fiscal costs and benefits during the fixed period 2005–2014 regardless of when the refugees arrived, rather than the expected (future) costs and benefits of newly-arrived individuals as they age.⁹

The [HHS \(2017a, 30–31\)](#) study finds that comprehensive government expenditures at all levels

⁹The HHS study includes recipients of asylum within its definition of 'refugee', but not asylum seekers. The [Evans and Fitzgerald](#) 'dynamic' method seeks to predict the fiscal balance of a given individual as they age, year by year into the hypothetical future. The HHS 'static' method measures actual fiscal flows for all refugees during a fixed period of time in the past.

totaled \$7,134 per refugee or asylee per year, 75 percent of which come from the federal government. The same people collectively paid \$9,319 in taxes per refugee or asylee per year, 72 percent of which went to the federal government. The net flow into public coffers was \$2,185 per refugee or asylee per year, or \$43,707 per refugee over a 20 year period. Discounted at 2 percent per year for comparability with [Evans and Fitzgerald](#), this implies a present value at the beginning of the period of \$35,728 per refugee or asylee—67 percent larger than the [Evans and Fitzgerald](#) estimate.

The key differences in the two estimates are: 1) The [HHS](#) study includes both resettled refugees and people who received asylum (not asylum seekers) while [Evans and Fitzgerald](#) consider only resettled refugees. 2) The [HHS](#) study includes all people present in the country who arrived as refugees or asylees, no matter how long ago, while [Evans and Fitzgerald](#) consider refugees in the first 20 years after arrival. Considering only the first 20 years after arrival fails to count many years of positive net fiscal contribution during years spent working thereafter, which would tend to make the [HHS](#) estimate more positive, but also fails to count most government outlays during retirement, which would tend to make the [HHS](#) estimate more negative. 3) The [HHS](#) estimate uses a more comprehensive measure of expenditures, which would tend to make it more negative.¹⁰

These studies and all others that use the ‘accounting’ approach suffer from a major limitation that discounts the positive impact of refugee workers on public coffers. This is due to the way that they estimate the effect of refugee workers on tax revenue. Namely, they fail to account for any of the effects of refugees on the rest of the economy, discussed in [subsection 2.1](#). Those other economic activities are also taxed, but any such taxes are not considered in accounts of taxes paid directly by refugees themselves.

This equates to assuming that the employment of a worker has zero effect on the capital income of firms’ owners ([Clemens 2021](#)). That is an extreme abstraction in theoretical terms and

¹⁰The Administration did not publish the [HHS](#) study, though it was complete and the text was leaked to newspapers. The study’s suppression was widely criticized. Nonpartisan experts described the study as “a well-researched, serious piece of work” offering “an unbiased look at hard data” ([Newland and Capps 2017](#)). Within the government, the suppressed study was replaced with a three-page memo counting only [HHS](#) expenditures on refugees, without expenditures by other agencies and without offsetting tax revenue ([HHS 2017b](#)). See Julie Hirschfeld Davis and Somini Sengupta, “[Trump Administration Rejects Study Showing Positive Impact of Refugees](#)”, *New York Times*, Sept. 18, 2017.

is strongly contradicted by empirical evidence that hiring migrant workers raises the value of firms' future stream of capital income (e.g. [Mayda et al. 2020](#); [Bahar et al. 2020](#)). This limitation applies to the literature on fiscal impacts of immigration in general: "In a comprehensive analysis, these ripple effects in the economy would be accounted for; however, due to the complexity of operationalizing a general equilibrium approach into the accounting framework, they typically are omitted" ([Blau et al. 2017](#), 343).

It is possible, however, to estimate the magnitude of such indirect fiscal effects ([Dustmann et al. 2010](#); [Dustmann and Frattini 2014](#)). Economists who do not study immigration have, for decades, studied the subject of how the additional revenue created by a worker's labor is divided between capital and labor. U.S. employers generally do not hire workers as an act of charity, but because the employers earn additional profit and shareholders and creditors earn additional capital income from hiring those workers. In economic terms, workers earn only a portion of the additional revenue they bring in—their 'marginal revenue product'. The rest becomes capital income, which is taxed. No such taxes are included in the accounting exercises described above.

The magnitude of such indirect tax revenue is large, and can be approximated as follows. A conservative estimate is that each additional \$1 of labor income in the United States generates \$0.50 of capital income ([Gutiérrez and Piton 2020](#); [Hershbein et al. 2020](#)). The true value is almost certainly higher ([Naidu et al. 2018](#), 565–568), and with limited exceptions such as the capital-intensive mining sector, it does not vary greatly by industry (e.g. [Elsby et al. 2013](#)). How much public revenue arises from that capital income? The effective tax rate on capital income in the United States is similar to the effective tax rate on labor income: Both are roughly 25 percent ([Saez and Zucman 2019](#), 93).¹¹

Together, these imply that employers, shareholders, and creditors pay additional capital taxes due to the existence of refugee labor, in an amount that is something more than 50 percent of the value of income taxes paid directly by refugee workers. Those additional taxes are counted as zero in existing estimates of refugees' fiscal impact, thus those studies underestimate the tax revenue resulting from refugee labor by more than one third. Zero is certainly not the best estimate that can be made. Omitting this revenue equates to assuming that Americans who employ

¹¹This does not refer to statutory tax rates. The effective tax rate is tax revenue actually collected divided by the value of the income taxed.

refugees sacrifice any income they could have made on their capital by investing it elsewhere. That assumption has no theoretical basis and is contradicted by the fact that firms expanding their workforce, on average, substantially expand the income they earn on capital.

In principle this additional positive effect could be offset if refugees caused other workers to pay less in taxes (such as by reducing their wages) or to claim more in benefits (such as by displacing them from jobs into unemployment insurance). But as discussed above, there is no evidence of negative labor market effects of refugee resettlement on any class of U.S. worker (Mayda et al. 2017).

In all studies of immigrant fiscal impact, a critical assumption is which expenditures to count as costs. Both studies of refugee impact discussed above make the correct decision not to count two major classes of fiscal expenditure: the public schooling of refugees' children, and fixed public expenditures such as national defense or overall government administration. Because some studies of immigrants' fiscal impact mistakenly count such costs, it is worth discussing these two assumptions.

First, the above estimates of positive impact are conservatively low because they omit investment in public schooling for refugees' children. Such investment, if counted, would contribute a further net positive effect on public coffers. This is a consequence of two facts. One is that economists' estimates of the returns to schooling imply that public investment in the schooling of children in America *in general* has a positive fiscal effect.¹² There is no evidence in the economics research literature that public schooling for the average child is a net drain on taxpayers. The other relevant fact is that refugee children and U.S.-born children of refugees have simi-

¹²Card (2001) finds that an additional year of schooling in the United States causes a rise in adult wages of about ten percent. At a conservatively low estimate of median income per worker of \$30,000 per year, this implies that a year of schooling causes \$3,000 in additional labor productivity for the median worker each year. The net present value of that additional productivity from age 18–65, measured at age 10 with the discount rate of 2 percent used by Evans and Fitzgerald is, \$80,108. This additional lifetime labor income would on average produce a further \$40,054 of capital income in net present value, or more, for a total value added of \$120,163 in net present value. Given the approximate effective tax rate of 25 percent on both labor and capital income discussed in the text, this implies a lifetime fiscal benefit of over \$30,040 in net present value for each additional year of schooling. The average present cost of a year of public schooling per pupil in the United States in 2018 was \$12,612 (Bureau of the Census 2020). In this example, the present-value fiscal benefit exceeds the cost by 138%. This illustrative calculation is conservatively low because it counts only fiscal benefits from positive effects of education on labor market performance, not by any other channel such as reducing incarceration rates (Lochner 2020) or reducing the usage of public benefits such as welfare or unemployment insurance when educated children become adults.

lar education outcomes and labor-market performance to average natives (Evans and Fitzgerald 2017). Thus the fiscal effects of schooling for U.S. children in general are representative of the effects for refugee children in particular. Because that effect is positive, estimates of refugees' fiscal impact that exclude such expenditures (such as the two accounting exercises discussed above) underestimate the positive net fiscal effect of refugees. It is nevertheless common in the literature to count only the costs of immigrant children's education while assuming zero fiscal benefit (e.g. Smith 2018), an error of method. This is an important reason why estimates of immigrants' fiscal impact that include both the fiscal costs and fiscal benefits of immigrants' children are uniformly more positive than estimates excluding their children (Blau et al. 2017, 445).

Second, some classes of fixed public expenditure are appropriately omitted from the above calculations. The most important of these are expenditures on national defense, overall government administration, and foreign aid. There is no evidence that the arrival of an additional refugee requires additional expense for such fixed costs. The costs of operating a U.S. nuclear submarine or the U.S. embassy in Moscow, for example, are not affected at all by the arrival of a refugee in the United States. These are textbook examples of *nonrival* goods, meaning that consumption by one resident does not diminish consumption by another resident, so that "the marginal cost of an additional immigrant is, at least in the short run, zero or close to it" (Blau et al. 2017, 8). It is nevertheless common in the research literature to ascribe to refugees or other immigrants a fiscal cost amounting to the average amount spent on such fixed goods per prior resident (e.g. Ruist 2020). This error of method exerts a large negative bias on many existing estimates of immigrants' net fiscal impact (e.g. Blau et al. 2017, 346).

A reasonable conclusion from this literature is that an additional refugee resettled in the United States causes a net flow into federal, state, and local public coffers of \$6,844 per year. This is the value of tax revenue estimated by HHS (2017a) augmented by 50% to include a conservative estimate of additional tax revenue caused by refugees' labor but not paid directly by them, for a total tax revenue per refugee of \$13,979 per year, minus benefits received of \$7,134 per year. It is conservatively low because it omits the fiscal impact of refugees who arrived as children or the U.S. born children of refugees, which are likely to be more positive than the impact of adult refugees (Blau et al. 2017, 404).¹³

¹³This estimate is consistent with an independent estimate from a different method: Using Peri's (2012) estimate that an additional immigrant raises GDP by 1.88 times that immigrant's income, assuming median personal income of

3 Asylum seekers

No study in the economics research literature estimates the economic or fiscal impact of U.S. asylum seekers specifically. This section thus reviews the literature on the effects of 1) asylum seekers in non-U.S. settings and 2) average U.S. immigrants from the countries that are the origins of most U.S. asylum seekers. It proceeds to discuss the degree to which these impacts can reasonably be expected to differ between asylum seekers and refugees. The principal reason that the economic effects of asylum seekers would differ from those of refugees is that asylum seekers tend to have relatively less education and work in lower-wage jobs. The principal reason that the fiscal effects would differ is that asylum seekers are categorically ineligible for the relocation assistance, welfare, and food stamps that constitute the bulk of benefits received by refugees. (Asylum seekers who later receive asylum would be included in the group of refugees and asylees described by the [HHS 2017a](#) fiscal analysis above.)

3.1 Economic effects of asylum seekers

Economists have not specifically studied the effects of asylum seekers on the U.S. economy. A large body of research has, however, studied the economic effects of asylum seekers elsewhere. Many of these studies use the term ‘refugee’ to describe the migrants they consider, particularly in Europe. But the studies are included in this section because most of the people they describe—migrants arriving without protected status who seek protection from the destination-country government—would be described as asylum seekers in a U.S. context.

The most transparent and policy-relevant study of the effects of asylum seekers on destination economies considers Western European countries collectively from 1985 to 2015. [d’Albis et al. \(2018\)](#) use real economic data to estimate that a persistent increase of 0.1 percentage points in the *annual inflow* of asylum seekers to 15 Western European countries produced an increase in average income per capita of 0.59 percent five years after the flow increased. Crucially, the empirical method in the study eliminates the reverse effect of prior high economic performance

\$30,000 per year, and assuming an effective tax rate on labor and capital income of 25%, the gross direct and indirect tax revenue per immigrant worker would be \$14,100 per year. Subtracting the benefits received by the average refugee of \$7,134 in the [HHS \(2017a\)](#) study, the net fiscal effect would be \$6,966 per year. This magnitude is consistent with the estimate of \$6,844 reached by an alternative method in the text.

on asylum seeker arrivals. It measures the evolution of GDP per capita after a positive shock to asylum-seeker arrivals in countries relative to its trend in countries that did not receive such a shock but otherwise have nearly identical trends in both GDP per capita and asylum-seeker arrivals across several previous years. The estimate implies that an increase in the *cumulative total* of arrived asylum seekers across one decade amounting to 1 percent of the destination-country population raises GDP per capita by 0.59 percent in the middle of that decade.¹⁴

This estimate is comparable to, but smaller than, the estimate by [Peri \(2012\)](#) discussed earlier for immigration overall to the United States. [Peri](#) estimates an increase of 0.88 percent in GDP per capita caused by an increase in the cumulative total of immigrant arrivals during a decade amounting to 1 percent of the *labor force* (not population). In Europe the working-age population is roughly two thirds of the total population. Thus in comparable magnitudes, the [d’Albis et al. \(2018\)](#) estimate of the effect of asylum seekers on GDP per capita in Western Europe is 0.39 percent, roughly half the size of the [Peri \(2012\)](#) estimate for immigrants overall in the United States. This is sensible, given the much lower labor force participation of asylum seekers in Europe compared to the United States ([Poutvaara and Wech 2016](#), 42; [Zwysen 2019](#); [Brell et al. 2020](#)).¹⁵

The rest of the research literature on the economic effects of asylum seekers 1) focuses almost entirely on the labor-market effects of asylum seekers, not the overall economy, 2) in individual destination countries other than the United States. [Cengiz and Tekgüç \(2022\)](#) find no effects from the sudden arrival of roughly three million Syrian asylum seekers in Turkey on native workers at any skill level, arguing that pure labor supply effects are offset by an accompanying stimulus to construction and entrepreneurship. [Fallah et al. \(2019\)](#) independently reach a similar

¹⁴[Aiyar et al. \(2016\)](#) address the hypothetical impacts of asylum seeker arrivals on GDP per capita in Europe using a computer model of the European economy. Studies of this kind are less transparent and useful for policy formulation, because it is impossible to know to what degree the predictions of the model depend on the numerous assumptions used to build the model, singly or jointly. For example, the model used by [Aiyar et al.](#) assumes that asylum seekers are perfect substitutes for other workers in the destination economies, which may be responsible for its prediction that asylum seeker inflows would produce a rise in native unemployment. Such a rise is contradicted by the real data on European economies in the study by [d’Albis et al. \(2018\)](#) and has not been observed in the countries most affected by real asylum seeker inflows (e.g. [Scharfbillig and Weißler 2019](#)). While [Aiyar et al.](#) predict falling GDP per capita in Europe due to increased asylum seeker inflows, the more sophisticated model of [Busch et al. \(2020\)](#) makes the opposite prediction for Germany.

¹⁵In a study not yet published in a peer-reviewed academic journal, [Weiske \(2019\)](#) uses a method similar to the method of [d’Albis et al. \(2018\)](#) to estimate the effect of a change in *overall* immigration on output and productivity in the United States, reaching estimates qualitatively and quantitatively similar to the results of [Peri \(2012\)](#).

conclusion for Syrian asylum seekers in Jordan. Foged and Peri (2016) find that large inflows of Iraqi, Afghani, and Balkan asylum seekers to Denmark caused an increase in native wages and employment by promoting native specialization in tasks that complemented asylum seekers' labor. Studies of the recent mass-exodus of Venezuelans have found no effect of their arrival on average workers in Colombia (Santamaria 2020) or in Ecuador (Olivieri et al. 2020), and no effect of the regularization of half a million informal Venezuelan workers in Colombia on other workers in Colombia (Bahar et al. 2021). Clemens and Hunt (2019) review and reanalyze several earlier studies of the effects of asylum seekers on destination countries, including one large inflow of asylum seekers in Miami, and find small or no significant effects on native workers (see also Peri and Yasenov 2019). In sum, the literature on labor-market impacts does not offer a compelling reason to adjust the above estimates of overall economic impact.

Asylum seekers can continue to have economic impacts after ceasing to be asylum seekers. There are three ways for this to happen. First, they could receive asylum. In this case they become asylees, and are included in the group of refugees and asylees considered by the (HHS 2017a) study discussed above. Second, they could be denied asylum and depart the country, in which case they would cease to have significant economic impacts. Third, they could be denied asylum but remain in the country without authorization.

This last possibility is allowed for by the studies discussed above. First, the estimates by d'Albis et al. (2018) discussed above measure the effects of asylum seeker *arrival*, regardless of whether or not they receive asylum, and whether or not they subsequently depart. Second, the estimates of Peri (2012) address the effects of all immigrants, with or without authorization, since the census data he uses do not distinguish immigrants by legal status.

Beyond this, the limited literature on the specific economic effects of unauthorized immigration does not offer compelling evidence that such effects would substantially differ from the effects of asylum seekers. That literature is qualitatively informative, though it does not provide quantitative estimates of impact directly applicable to policy formulation. Abstract modeling exercises, relying on multiple untestable assumptions about mechanistic relationships between different parts of the economy, have investigated the overall effect of unauthorized immigration. These typically find a positive effect of unauthorized immigrants on natives' overall capital and labor

income per capita (Palivos 2009; Edwards and Ortega 2017), though this positive effect is lower than the effect of authorized immigrants, in part because the lack of legal status by itself reduces workers' productivity (Amuedo-Dorantes and Bansak 2011; Pan 2012). Evidence on the strictly labor-market impacts of unauthorized immigrants is inherently limited by the fact that unauthorized work is concealed. Rare evidence from the state of Georgia shows that firms employing more unauthorized immigrants pay higher wages to other workers (Hotchkiss et al. 2015) and are more likely to stay in business, thus continuing to employ other workers (Brown et al. 2013). Nationwide evidence on stricter enforcement of laws prohibiting the employment of unauthorized workers finds no effect on average natives' wages or employment (Orrenius and Zavodny 2015).

A reasonable conclusion from the existing research literature is that an increase in the number of asylum seekers in the United States equal to one percent of the population would raise income per capita for all other residents by an amount greater than 0.4 percent within five years. This likely represents a lower bound on the true effect because it is based on the estimate of d'Albis et al. (2018) for Western Europe, where labor force participation by asylum seekers—the largest determinant of their overall economic contribution—is much lower than in the United States.

3.2 Fiscal effects of asylum seekers

No study in the research literature specifically measures the net fiscal effect of the presence of asylum seekers in the United States. The HHS (2017a) study discussed above measures the overall fiscal effect of both resettled refugees and asylees (asylum seekers who received asylum), but 1) it does not separate the effect of asylees from the effect of refugees, and 2) it does not address the effect of asylum seekers who did not receive asylum. There are means, however, of estimating the fiscal effect of asylum *seekers* overall—whether or not they receive asylum.

The most reliable analysis of the fiscal impact of asylum seekers estimates what has happened to real data on the fiscal balance of destination countries after they have received large inflows of asylum seekers. This method overcomes the limitation of 'accounting' approaches that fail to include tax revenue that is not paid directly by asylum seekers themselves. The method also overcomes the limitation of model-based studies that make numerous, untestable assumptions

about how asylum seekers affect a simplified, abstract economy. A reasonable conclusion from the limited research using the most direct and transparent analysis is that asylum seeker inflows to the United States representing one percent of the labor force cause an increase of more than one percentage point in net fiscal balance as a percentage of GDP. The true number is likely to be greater than this lower bound because the bound comes from estimates of European countries where benefits paid to asylum seekers are greater and tax revenue from their work is lower.

I explain the reasoning for using the fiscal impact approach, and the resulting estimate, in detail below. As to the fiscal impact approach, the most closely related study using real, recent data is the aforementioned study by [d'Albis et al. \(2018\)](#) on the effects of asylum seeker arrivals in Europe. They find that in 15 Western European countries between 1985 and 2015, a sustained additional inflow of asylum seekers that cumulatively amounted to 1 percent of the host population over a decade caused the net fiscal balance as a percentage of GDP to rise by 1.5 percentage points after five years. Because the labor force in Europe represents roughly two thirds of the population, this implies that additional asylum seekers across a decade cumulatively amounting to one percent of the destination-country labor force caused the net fiscal balance as a percentage of GDP to rise by one percentage point. This is likely to be an underestimate of the corresponding effect in the United States, given that European expenses on asylum seekers are systematically larger (due to larger social transfers including unemployment insurance) and revenues systematically smaller (due to lower labor force participation and thus tax revenue generated by asylum seekers) than in the United States.¹⁶

A second approach to estimating the fiscal effects of asylum-seeker arrivals is accounting studies of taxes paid by and benefits received by particular types of immigrants in the United States, including both asylum seekers and others. [Blau et al. \(2017, 445\)](#) present such fiscal accounting for foreign-born U.S. residents in general (with or without lawful immigration status) by education level. [Blau et al.](#) find that the net present value of lifetime taxes paid by an average-age recent immigrant with less than high school education and their descendants is \$503,000, while

¹⁶[d'Albis et al. \(2019\)](#) use a similar method to reach a similar conclusion for OECD countries collectively—including the United States—but for immigration overall rather than asylum seeker inflows specifically. [Storesletten \(2003\)](#) finds that the fiscal impact of migrants in a European country depends heavily on their labor force participation, which is typically low. Labor force participation by asylum seekers is generally recognized as much higher in the United States than in Europe. For example, the rate of labor force participation by Central American immigrants to the United States, regardless of legal status, is 72 percent—higher than U.S. natives at 62 percent ([O'Connor et al. 2019](#)).

the net present value of lifetime benefits received is \$619,000—a negative balance. For an immigrant with some college (but less than a bachelor’s degree), the same figures are lifetime taxes of \$844,000 and lifetime benefits of \$583,000—a positive balance.¹⁷

Applying those findings to learn about the fiscal effects of asylum seekers, then, requires knowing the average education level of asylum seekers. No public data source reports the average education level of applicants for asylum in the United States. There is evidence from Europe that asylum seekers from a given country are more positively selected on education than migrants not seeking protection (reviewed by [Hatton 2020](#), 84–87, see also [Aksoy and Poutvaara 2021](#)). But evidence of this kind is not available for the United States.

It is *prima facie* unclear whether average asylum seekers in the United States have more or less education than average immigrants overall, or even average natives. The top countries of origin for asylum applicants include both countries whose average U.S. immigrants are relatively more educated and those that are less educated. For example, in Fiscal Year 2019, the top three origin countries for affirmative asylum applications were Venezuela, Guatemala, and China ([Baugh 2020](#)). Among immigrants *overall* in the labor force who arrived in the past 10 years (not just asylum-seekers), 78.6 percent of those from Venezuela have postsecondary education. For those from Guatemala, the same figure is 13.8 percent. For those from China it is 68.1 percent ([Ruggles et al. 2020](#)). The same figure for U.S. natives in the labor force is 60.5 percent. The rest of the top ten countries of origin for affirmative asylum applicants includes Haiti and Honduras, whose recent U.S. immigrants are relatively less educated than natives, and it includes India and Nigeria, whose U.S. immigrants are relatively more educated than natives.

Even if the average education of asylum seekers were known, however, the estimates using this approach on asylum seekers would materially differ from the results for average immigrants. This is because asylum seekers are ineligible for many of the benefits received by the average immigrants studied by [Blau et al.](#) They are ineligible to receive major government transfers such as welfare and food stamps 1) while awaiting a decision on their asylum case, and 2) if they

¹⁷As discussed earlier, these estimates appropriately exclude the value of fixed, nonrival public goods expenditures such as national defense and foreign aid. [Orrenius \(2017\)](#) explains why this assumption is the most realistic one. The large literature on the net fiscal impact of immigration in general, including in the United States, is reviewed by [Liebig and Mo \(2013\)](#), [Nowrasteh \(2015\)](#), [Blau et al. \(2017\)](#), and [Hennessey and Hagen-Zanker \(2020\)](#).

receive a negative decision and nevertheless remain in the country unlawfully.

Additionally, this type of accounting exercise—as discussed above—omits tax revenue generated indirectly by capital income accruing to asylum seekers’ employers and their shareholders. The earlier discussion noted that a reasonable lower bound on that additional revenue is 50 percent of the tax revenue paid directly by the average immigrant. Omitting this additional revenue from the accounting, again, equates to assuming that employers who could have invested their capital elsewhere choose instead to invest it in business activities that earn them no money but employ asylum seekers. There is no theoretical or empirical basis for that assumption. There is no reason in the economic research literature to believe that employers of asylum seekers react differently to the incentive to earn money on their capital than other employers in the U.S. economy.

For asylum seekers with an education level less than that of the average immigrant, the value of 50 percent would be even more conservative. This is because wage markdowns—the gap between the marginal revenue created by workers in the U.S. and their wages received (and thus taxed)—are somewhat larger for workers with less education and lower wages. Moreover, wage markdowns are larger for foreign-born workers.¹⁸ Thus the amount of capital income per dollar of labor income is higher for a less-educated, foreign born worker than for an average U.S. worker.

Finally, standard accounting exercises furthermore omit additional tax revenue from the *labor* of other workers complemented by migrant worker. This effect would be more pronounced for asylum seekers than for refugees, given that asylum seekers have lower average levels of education than refugees or natives. An intuitive example of this effect is when a less-educated asylum seeker working in a daycare center allows a more-educated female U.S. worker to participate in the labor force (Cortes and Tessada 2011) and thus pay additional taxes—income taxes on their labor, excise taxes on their consumption, and so on. Colas and Sachs (2020) estimate that the lifetime present value of these additional taxes created by the marginal U.S. immigrant with

¹⁸Bassier et al. (2020, 51, Table 6) find that the ratio of wages to marginal revenue product is 0.74 in the bottom quartile of earnings and 0.82 in the top quartile, given that firm-labor supply elasticity $e = -2 \times$ separation elasticity, and $\frac{\text{wage}}{\text{MPL}} = \frac{e}{1+e}$. Arin Dube pointed out the relevance of this markdown estimate. Qiu and Sojourner (2019) and Tortarolo and Zarate (2018) likewise find that less-educated workers receive larger markdowns than more-educated workers. Qiu and Sojourner (2019) furthermore find that foreign-born workers receive larger wage markdowns than natives.

less than a high school education—including the additional taxes paid by *other workers* whose productivity is raised by immigrant workers' labor—is \$23,713.¹⁹

A third approach to estimating the fiscal effects of asylum seeker arrivals is to build an economic model of the destination-country economy. Such a model equates to a long and intricate list of mechanistic assumptions about how different parts of the economy relate to each other. Numerous studies of this kind have been carried out for European countries. These often predict a negative overall fiscal effect of asylum seeker inflows conditional on the truth of the model's assumptions—a literature reviewed by [Gál \(2019\)](#). One major limitation of this evidence for policy formulation is that every model abstracts heavily from the real economy, and the results are sensitive to its countless assumptions in unknown measure. For example, [Holler and Schuster \(2020\)](#) predict negative fiscal effects from refugee migration to Austria. The model used to reach this conclusion assumes only one type of firm producing one aggregate final good in the economy, and the degree to which foreign and native labor can substitute for each other in the production of that single good does not vary by skill level—an assumption which is known to bias empirical results on the economic impacts of immigration ([Ottaviano and Peri 2012](#), 176). These assumptions restrict by design the degree to which asylum seekers could specialize in industries, firms, and tasks that complement native labor. The effect of these and countless other assumptions on the model's predictions is unclear.

In short, the fiscal impact of asylum seekers in the United States depends substantially on their average level of education, which is unknown. The approach I take below is to make a very conservative assumption: that 80 percent of asylum-seekers have less-than-high-school education, and the rest have high school only. That is, the analysis assumes that all asylum seekers are much less educated than average recent U.S. immigrants from the Northern Triangle of Central America, in order to arrive at a lower bound on the true fiscal impact.

¹⁹This is the present value at age 18, using a 3 percent discount rate for comparability with [Blau et al. \(2017\)](#), of a stream of \$940 payments annually through age 65. \$940 is the mean of the range \$660–1,220 estimated for less-than-high-school workers by [Colas and Sachs \(2020, 28\)](#).

4 Scenarios for the United States

The research literature critically reviewed above allows quantitative bounds on the overall effect on the U.S. economy and fiscal balance that could be expected from large reductions in the number of resettled refugees and asylum seekers present in the country. Collectively, the research literature suggests that reducing inflows of refugees and asylum seekers has a negative impact on GDP and on public coffers. This effect cumulates over time, as reduced inflows lead to a smaller and smaller extant population of refugees and asylum seekers relative to a scenario without reduced inflows.

Below, I estimate bounds on the negative impact that reducing inflows of refugees and asylum seekers has on GDP and public coffers. These estimates are constructed to be conservatively small. That is, the true impact is likely to be more negative than the numbers presented in the tables, for reasons that I explain below in [subsection 4.3](#).

These scenarios are agnostic about the mechanism by which the reduced inflow is achieved. Take, for example, the scenarios for the economic impact of a 10 percent reduction in net inflows of asylum seekers. This reduction could occur by various mechanisms: It could arise due to a restriction in the number of people crossing the border. It could separately arise due to a rise in the rate of rejection for asylum applications, either because this causes fewer people to apply in the first place (e.g. [Neumayer 2004](#); [Toshkov 2014](#)) or because applicants denied asylum are more likely to leave. It could also arise by other, more complex mechanisms. The scenarios address the impact of any and all policies that produce a given reduction of inflow.

The first step is to translate scenarios for percentage reductions in refugees or asylum seekers into reductions in their absolute numbers. This is done in two ways below, using two different base years. One way takes inflows in Fiscal Year 2019 as the base inflow from which declines are calculated. This creates ‘forward looking’ scenarios useful for estimating the effect of further declines in arrivals, beyond the declines that have occurred under recent policy. In Fiscal Year 2019 the number of resettled refugee arrivals was 29,916, the number of affirmative asylum applications was 96,952, and the number of defensive asylum applications was 210,752 ([Baugh 2020](#)).

The second way takes inflows in either 2016 or 2017 as the base year, as described below. These estimates are useful for estimating the effect of recent policy as a whole, over the last 3–4 years and continuing beyond that.

To estimate effects over the last 3–4 years, I begin by estimating effects of percentage reductions on the absolute number of each type of migrant. In Fiscal Year 2016, 84,995 resettled refugees arrived in the United States. If an identical number of refugee arrivals had occurred during FY2017–2020, 339,980 refugees would have arrived during that period. The real number of arrivals during the same period was 118,074 ([Migration Policy Institute 2020](#)). That is, cumulative refugee arrivals during those four years fell 65.3 percent from what they would have been without the decline, causing there to be 221,906 fewer refugees in the country than otherwise would have been present now. Each year that refugee admissions continue at the FY2020 level of 11,841 rather than the FY 2016 level of 84,995, an 86 percent reduction in annual arrivals, means that there are 73,154 fewer refugees present in the country than there would have been without the reduced inflow.

Turning to asylum seekers, the number of asylum applicants (affirmative and defensive) in FY2017 was 284,579 ([Mossaad 2019](#); [Baugh 2020](#)). This is a suitable ‘base’ year because asylum applications, unlike refugee resettlement, are not directly set by a policy lever that can be immediately adjusted by government but are the outcome of individual decisions linked to migration processes with inertia.²⁰ This total comprises 139,917 affirmative applications, usually made at or shortly after arrival; and 144,662 defensive applications, often made during deportation proceedings long after arrival.²¹ A policy of reducing arrivals in the short term would tend to affect affirmative applications quickly but would affect most defensive applications with a longer lag. For this reason, the estimates to follow will give a range of numerical equivalents of percentage reductions in asylum seekers. For example, a 50 percent reduction in arrivals of asylum seekers,

²⁰2017 was the recent peak year for affirmative asylum applications to U.S. Citizenship and Immigration Services, usually made at or shortly after arrival, and tend to fall with stricter regulation of physical entry by migrants—as they have in the years since. Defensive applications can *rise* with stricter internal migration enforcement because by definition, defensive applications occur with deportation proceedings. Indeed, defensive applications have risen in each year since 2017. In other words, both affirmative and defensive applications are indicators of the presence of asylum seekers, but defensive applications respond with a longer lag to stricter migration enforcement, and short-term rises in defensive applications do not capture the medium- to long-term effects of stricter enforcement on applications.

²¹Defensive asylum applications can occur soon after arrival as well, such as at airports, so there is no *necessary* link between the application type and the timing of the application relative to arrival. That said, many defensive applications occur long after arrival.

relative to base year FY2017, would mean 69,959 fewer affirmative asylum applicants in that year. An ongoing, longer-term policy of cutting arrivals by 50 percent would have reduced both affirmative and defensive applications in that year, by 142,290. This is what is meant by the ‘short term’ and ‘long term’ estimates below.

4.1 Scenarios for economic impact

The above scenarios for absolute reductions of refugee and asylum seeker inflows allow conservative estimates of their overall economic effect, starting from estimates of the economic productivity of an average migrant in each category.

Refugees. The average refugee household arrived between 5 and 9 years ago has an income of greater than \$51,902 per year, and comprises 2.5 people,²² only 64 percent of whom are themselves refugees (HHS 2017a, 8). In an average year from 2005 to 2014 there were 4.5 million people in America living in refugee households (including both refugees and non-refugees), with a total income due to refugees of $\$51,902 \times \frac{4.5\text{m}}{2.5} \times 0.64 = \59.8 billion per year. Assuming a conservatively high labor share of 66.6 percent (Naidu et al. 2018, 567) and thus a conservatively low GDP contribution, this implies a contribution to GDP of \$89.8 billion per year by those 2.9 million refugees, or \$30,962 per refugee per year—of which \$10,310 per year accrues to other people in the country. This per-refugee figure includes people of all ages and all employment statuses.

Asylum seekers. No data are publicly available documenting similar income and demographic traits of asylum applicants. But it is possible to carry out a similar calculation for recent immigrants (authorized and unauthorized) from parts of Central America that have contributed disproportionately to recent asylum seekers at the southern border. Data for recent immigrants born in the Northern Triangle of Central America (Guatemala, Honduras, El Salvador) and ar-

²²Refugees with 5–9 years of residence are chosen to balance two objectives. First, a few years should have passed to allow them to realize their potential productivity in the labor market (Cortes 2004; Evans and Fitzgerald 2017), which argues against considering refugees with 0–4 years of residence. Second, the vast majority of their income should come from labor rather than capital, arguing against considering refugees who arrived decades ago who may now be receiving non-negligible income from capital. HHS (2017a, Table 5) reports household income by quintiles for refugees with 5 to 9 years of residency. An underestimate of the underlying mean is a weighted average of the income at each quintile with a weight of 0.2. The average household size in the United States is 2.5, and HHS (2017a, Table 5) shows that the distribution of household sizes in refugee households is similar to that in all U.S. households, though with somewhat fatter tails (both small and large households are overrepresented for refugees).

rived within the past 10 years are taken from the American Community Survey (Ruggles et al. 2020). In an average year 2014–2018, there were 2.02 million people in the United States living in households including at least one person born in the Northern Triangle who arrived within the past 10 years. The average such household earned \$50,975 per year and had 3.99 members, of whom 1.72 (43.1 percent) were themselves recent immigrants from the Northern Triangle. This implies a total income in the average year due to recent Northern Triangle immigrants of $\$50,975 \times \frac{2.02\text{m}}{3.99} \times 0.431 = \11.1 billion per year. Again conservatively assuming a labor share of 66.6 percent (Naidu et al. 2018, 567),²³ this implies a contribution to GDP of \$16.7 billion per year by those 868,316 recent Northern Triangle immigrants, or \$19,194 per immigrant per year—of which \$6,392 per year accrues to other people in the country. This per-asylum seeker figure includes people of all ages and all employment statuses. As discussed above, the approach of letting Northern Triangle migrants proxy for the economic effects of asylum seekers in general yields conservatively low estimates, given that several of the most important countries of origin for recent asylum seekers—such as Venezuela and China—are countries whose U.S. immigrants are typically more educated than U.S. natives and earn much more than Northern Triangle immigrants.

Note that the GDP effects estimated above are the effects of the *presence* of a person in the country in any given year. Thus the effect of a reduced *inflow* on the number of people *present* rises with each year. For example, if 100 people would otherwise have entered the country each year, then after 1 year 100 such people would be present, after 2 years 200 would be present, after 3 years 300 would be present, and so on. If the inflow is reduced by half, to 50 per year, then after 1 year 50 such people would be present, after 2 years 100 such people would be present, after 3 years 150 such people would be present, and so on. The effect of the reduced *inflow* on the number *present* rises with time: In this example, the effect is to reduce the number present by 50 in the first year (100 minus 50), by 100 in the second year (200 minus 100), and by 150 in the third year (300 minus 150). For this reason, the cumulative GDP effect of person-years of *presence* caused by a reduced annual *inflow* by one person across five years is to remove $5 + 4 + 3 + 2 + 1 = 15$ person-years of presence of that person. The five-year cumulative cost is that amount multiplied by the annual economic effect of a person-year of presence in the economy.

²³This plausibly assumes that the share of income that recent Northern Triangle immigrants receive from capital investments is small enough to ignore.

Table 1: ECONOMIC IMPACT: Lower-bound scenarios, reduced refugee and asylum seeker inflows

Percent reduction	Base inflow, annual	Absolute decline, annual	Annual GDP effect \$ per migrant	Annual GDP effect, \$billion (flow change)	Cumulative cost, 5 yrs, \$billion (stock change)	Cumulative direct cost to others, 5yr \$billion
<i>(a) Reduced flows after base year 2016–2017</i>						
Resettled refugees, base year 2016						
10%	84,995	8,500	30,962	0.26	3.95	1.32
25%	84,995	21,249	30,962	0.66	9.87	3.30
50%	84,995	42,498	30,962	1.32	19.74	6.59
75%	84,995	63,746	30,962	1.97	29.61	9.89
Asylum seekers, <i>short run restriction of inflow</i> , base year 2017						
10%	139,917	13,992	19,194	0.27	4.03	1.35
25%	139,917	34,979	19,194	0.67	10.07	3.36
50%	139,917	69,959	19,194	1.34	20.14	6.73
75%	139,917	104,938	19,194	2.01	30.21	10.09
Asylum seekers, <i>long run restriction of inflow</i> , base year 2017						
10%	284,579	28,458	19,194	0.55	8.19	2.74
25%	284,579	71,145	19,194	1.37	20.48	6.84
50%	284,579	142,290	19,194	2.73	40.97	13.68
75%	284,579	213,434	19,194	4.10	61.45	20.52
<i>(b) Reduced flows after base year 2019</i>						
Resettled refugees, base year 2019						
10%	29,916	2,992	30,962	0.09	1.39	0.46
25%	29,916	7,479	30,962	0.23	3.47	1.16
50%	29,916	14,958	30,962	0.46	6.95	2.32
75%	29,916	22,437	30,962	0.69	10.42	3.48
Asylum seekers, <i>short run restriction of inflow</i> , base year 2019						
10%	96,952	9,695	19,194	0.19	2.79	0.93
25%	96,952	24,238	19,194	0.47	6.98	2.33
50%	96,952	48,476	19,194	0.93	13.96	4.66
75%	96,952	72,714	19,194	1.40	20.94	6.99
Asylum seekers, <i>long run restriction of inflow</i> , base year 2019						
10%	307,704	30,770	19,194	0.59	8.86	2.96
25%	307,704	76,926	19,194	1.48	22.15	7.40
50%	307,704	153,852	19,194	2.95	44.30	14.79
75%	307,704	230,778	19,194	4.43	66.44	22.19

The figures of ‘refugees’ represent all refugees present in the country during 2005–2014. The figures for ‘asylum seekers’ represent immigrants from Guatemala, Honduras, and El Salvador of any immigration status, present in the country 2014–2018 and arrived during the previous ten years. The dollar effects in column 4 represent the ongoing annual effect of people present in any given year. A reduced inflow has a larger effect on the *stock* of migrants present that rises with each year that the reduction is sustained. Thus the cumulative impact is estimated according to the number of person-years of *presence* caused by a continuing reduction of inflow in any given year. A reduced inflow by 1 person per year that is sustained for five years removes 15 person-years of presence during those 5 years ($5 + 4 + 3 + 2 + 1 = 15$).

Table 1 reports the results of the scenarios for changes in the overall economic output of the United States using the figures above. Relative to the base years 2016–2017, in part *a* of the table, the assumptions of the scenarios imply that a reduction of 25 percent in the annual inflow of resettled refugees to the United States has a cumulative economic cost of \$9.9 billion at the end of five years. Of this amount, \$3.3 billion accrues directly to other people in the country via increased income on invested capital. The economic cost of a similar 25 percent reduction in affirmative asylum applicants is \$10.1 billion at the end of five years. This is of a similar magnitude to the effect of a reduction in resettled refugees, because while the impact per asylum seeker is smaller, asylum seekers are more numerous than refugees. If a long-term strategy of reducing the number of asylum seekers succeeded in causing a 25 percent reduction in both affirmative and defensive asylum applicants, the economic cost over a five year period would be \$20.5 billion.

Relative to the base year 2019, in part *b* of the table, the assumptions of the scenarios imply that a reduction of 25 percent in the annual inflow of resettled refugees to the United States has a cumulative economic cost of \$3.5 billion at the end of five years. The economic cost of a 25 percent reduction in affirmative asylum seekers is \$7.0 billion at the end of five years, and the cost of a 25 percent reduction in both affirmative and defensive asylum seekers is \$22.2 billion at the end of five years.

4.2 Scenarios for fiscal impact

Refugees. The annual net fiscal impact of a refugee at all levels of government was estimated in [subsection 2.2](#), above, at \$6,844 per person per year.

Asylum seekers. No empirical estimates exist for the net fiscal impacts of U.S. asylum seekers. The federal government has produced hypothetical scenarios for taxes paid by employed asylum seekers, scenarios based on assumptions about earnings and taxes of asylum seekers, not empirical data on such earnings and taxes. These estimates are difficult to interpret, given the strong assumptions they rest on. Some such scenarios assume, for example, that all asylum seekers work at minimum wage, that their employment has no effect on economic activity, and that only federal taxes are collected (e.g. [DHS 2020a](#), 38538, 38593). They also do not contain

empirical estimates of the value of government benefits received by asylum seekers.

As discussed above, [Blau et al. \(2017, 445\)](#) estimate the direct fiscal flows associated with a new immigrant with less than a high school education. The net present value of lifetime government benefits received by the average such person is \$381,000. This is their estimate omitting fiscal flows to and from the immigrant's descendants, for comparability with the above estimates for refugees. The net present value of taxes paid directly by the same people, to all levels of government, is \$272,000.²⁴ Conservatively assuming a labor share of 66.6 percent ([Naidu et al. 2018, 567](#)), and assuming an effective tax rate on both labor and capital income of 25 percent ([Saez and Zucman 2019, 93](#)), this implies additional tax revenue from capital taxation of $(1/0.666 - 1) \times \$272,000 = \$136,041$ per migrant in net present value. An additional \$23,713 in net present value comes from additional tax on the added productivity of more educated labor (the present value of an annual flow of \$920, [Colas and Sachs 2020, 32](#)). The net overall effect is a net present value of $\$431,754 - 381,000 = \$50,754$ per migrant, equivalent to a stream of payments over one working lifetime (age 18–64) of \$1,969 per year.

This amount does not include the fiscal costs of processing asylum applications such as administrative and court costs. Transparent public data on such costs do not exist. The U.S. Administration has recently, however, proposed a set of fees for affirmative and defensive asylum applications—as a cost-recovery measure—that collectively would not exceed \$1,500 per asylum applicant ([DHS 2020b](#)). A conservative overestimate of the costs underlying such a measure could be set at \$3,000 per person. Including such costs, the above fiscal balance calculation becomes a net present value of \$47,754 per migrant, equivalent to a working-lifetime stream of payments of \$1,853 per year.

The same calculation for immigrants with high school only, again omitting their descendants ([Blau et al. 2017, 445](#); [Colas and Sachs 2020, 32](#)) yields a net present value of net lifetime fiscal revenue of $\$365,000 + (\$365,000 \times (1/0.666 - 1) + \$29,925 - \$354,000 = \$220,480$, equivalent to a working-lifetime stream of annual payments of \$8,554. Including migrants' descendants in either calculation—for workers with less-than-high-school or high-school-only—increases the net positive fiscal impact by raising tax revenue per migrant by more than it raises benefits

²⁴[Blau et al. \(2017\)](#) use a discount rate of 3 percent, which is used in all the calculations to follow, for comparability.

received (Blau et al. 2017, 445).

No public data estimate the fraction of current asylum seekers that have completed high school. Among recent Northern Triangle migrants in the labor force during the years 2014 through 2018, 32.3 percent had completed high school only (no more and no less), and 50.1 percent had less than a high school education. The same figures for all others in America are 33.0 percent of those in the labor force who have completed high school only and 8.2 who have completed less than high school (Ruggles et al. 2020). In other words, among recently-arrived immigrant workers from the Northern Triangle who have a high school education or less, 61 percent have less than high school. A highly conservative assumption would be that among asylum seekers specifically, 1) *all* have a high school education or less, and 2) 80 percent of those have less than high school. Under that assumption, the annualized net positive fiscal flow produced by the work of an average asylum seeker from the Northern Triangle would be $0.8 \times \$1,853 + 0.2 \times \$8,554 = \$3,215$ per migrant, per year.

Table 2 shows the net fiscal effects, at all levels of government, of various scenarios for reduced inflows of refugees and asylum seekers implied by the assumptions above. For the base years 2016–2017, in part *a* of the table, the assumptions imply that a reduction of 25 percent in the annual inflow of resettled refugees to the United States has a cumulative net fiscal cost, at all levels of government, of \$2.2 billion at the end of five years. A reduction of 25 percent in the number of affirmative asylum applicants has a net fiscal cost at all levels of government of \$1.7 billion. If a long-term strategy of reducing the number of asylum seekers succeeded in causing a 25 percent reduction in both affirmative and defensive asylum applicants, the net fiscal cost over a five year period would be \$3.4 billion. Relative to the base year 2019, in part *b* of the table, the fiscal loss from a 25 reduction in refugees at the end of five years is \$0.8 billion, the loss from a 25 percent reduction in affirmative asylum seekers at the end of five years is \$1.2 billion, and the loss from a 25 percent reduction in both affirmative and defensive asylum seekers at the end of five years is \$3.7 billion.

Table 2: FISCAL IMPACT: Lower-bound scenarios, reduced refugee and asylum seeker inflows

Percent reduction	Base inflow, annual	Absolute decline, annual	Annual net fiscal loss \$ per migrant	Annual net fiscal loss, \$billion (flow change)	Cumulative loss, 5 yrs, \$billion (stock change)
(a) <i>Reduced flows after base year 2016–2017</i>					
Resettled refugees					
10%	84,995	8,500	6,844	0.06	0.87
25%	84,995	21,249	6,844	0.15	2.18
50%	84,995	42,498	6,844	0.29	4.36
75%	84,995	63,746	6,844	0.44	6.54
Asylum seekers, <i>short run restriction of inflow</i>					
10%	139,917	13,992	3,215	0.04	0.67
25%	139,917	34,979	3,215	0.11	1.69
50%	139,917	69,959	3,215	0.22	3.37
75%	139,917	104,938	3,215	0.34	5.06
Asylum seekers, <i>long run restriction of inflow</i>					
10%	284,579	28,458	3,215	0.09	1.37
25%	284,579	71,145	3,215	0.23	3.43
50%	284,579	142,290	3,215	0.46	6.86
75%	284,579	213,434	3,215	0.69	10.29
(b) <i>Reduced flows after base year 2019</i>					
Resettled refugees					
10%	29,916	2,992	6,844	0.02	0.31
25%	29,916	7,479	6,844	0.05	0.77
50%	29,916	14,958	6,844	0.10	1.54
75%	29,916	22,437	6,844	0.15	2.30
Asylum seekers, <i>short run restriction of inflow</i>					
10%	96,952	9,695	3,215	0.03	0.47
25%	96,952	24,238	3,215	0.08	1.17
50%	96,952	48,476	3,215	0.16	2.34
75%	96,952	72,714	3,215	0.23	3.51
Asylum seekers, <i>long run restriction of inflow</i>					
10%	307,704	30,770	3,215	0.10	1.48
25%	307,704	76,926	3,215	0.25	3.71
50%	307,704	153,852	3,215	0.49	7.42
75%	307,704	230,778	3,215	0.74	11.13

The figures of ‘refugees’ represent all refugees present in the country during 2005–2014. The figures for ‘asylum seekers’ represent immigrants from Guatemala, Honduras, and El Salvador of any immigration status, present in the country 2014–2018 and arrived during the previous ten years. The dollar effects in column 4 represent the ongoing annual effect of people present in any given year. A reduced inflow has a larger effect on the *stock* of migrants present that rises with each year that the reduction is sustained. Thus the cumulative impact is estimated according to the number of person-years of *presence* caused by a continuing reduction of inflow in any given year. A reduced inflow by 1 person per year that is sustained for five years removes 15 person-years of presence during those 5 years ($5 + 4 + 3 + 2 + 1 = 15$).

4.3 Robustness check with an independent approach

All of the above estimates are conservatively low, thus the estimates in Tables 1 and 2 should be considered as lower bounds on the unknown, true values. This is for several reasons.

First, the estimates of fiscal impact include only people who arrive as refugees and asylum seekers themselves, not their children. The net fiscal impact of the presence of an immigrant becomes more positive when the fiscal impact of their children is included (Blau et al. 2017, 445). Reducing arrivals of refugees and asylum seekers has the effect of removing both the migrants and their children from the economic and fiscal reality that would occur if the migrants had arrived. Thus a fuller, but more uncertain measure of fiscal impact would include migrants' children.

Second, all analysis of overall economic product and of tax income above assumes a conservatively high labor share of income, at 66.6 percent. This assumes almost no monopsony power in the labor market and thus almost no wage markdowns. Substantially greater markdowns and thus a lower labor share would be consistent with the research literature (Naidu et al. 2018, 567), particularly for foreign-born workers like refugees and asylum seekers (Qiu and Sojourner 2019) and moreover for less-educated and lower-wage workers like average asylum seekers from Central America (Tortarolo and Zarate 2018; Qiu and Sojourner 2019; Bassier et al. 2020). That is, the literature implies that a larger share of the workers' marginal product becomes capital income, and is taxed as such, than assumed in the scenarios above. This would mean more dollars of capital income and indirectly-paid capital tax associated with each dollar of labor income and directly-paid labor tax than assumed above.

Third, the scenarios assume that the additional value added by refugee and asylum-seeking workers does not affect activity in the rest of the economy except through payments to owners of capital. For example, there are no scale economies at the firm, state, or national level; no innovation is caused by migrants or the people they work with or purchase from; their labor does not affect natives' decisions regarding labor supply or investment in financial capital or education; no businesses employing other workers are created by refugees or asylum seekers; and so on.

Fourth, the analysis of asylum seekers assumes that 80 percent of working-age asylum seekers

have not completed high school, much higher than the real fraction among recent immigrants born in the Northern Triangle. Beyond this, as discussed above, large shares of recent asylum seekers are from countries such as Venezuela and China, whose U.S. immigrants are on average much more educated than Northern Triangle immigrants.

For all of these reasons, it is useful to check the lower-bound scenarios against empirical impact estimates derived from real U.S. data on migration and economic performance. The advantage of this alternative approach is that the estimates come from studies directly observing what happens in real economic data after inflows of migrants. The disadvantage is that such empirical estimates exist for the United States for migrants in general but not refugees and asylum seekers specifically; and they exist for refugees and asylum seekers specifically but for Europe, not the U.S. The analysis to follow uses the estimates from the literature based on real macroeconomic data to estimate the economic effects of refugee and asylum seeker inflows using an approach entirely separate from the approach in the previous two subsections.

By this alternative method, scenarios for impact on gross domestic product (GDP) from reductions in resettled refugees or asylum seekers can be estimated by the change in GDP,

$$\Delta Y = \left(\frac{r \cdot M_{\text{base}} \cdot \ell \cdot e}{N} \right) \times (s \cdot Y), \quad (1)$$

where Y is U.S. GDP; Δ indicates ‘change’; r is the percent reduction in the number of each type of migrant present in the country; M_{base} is the base level from which the percent r decline occurs; ℓ is the fraction of each type of migrant that is working age; e is the fraction of each type of migrant that is employed; N is the number of U.S. workers in the whole economy; and s is the percent change in GDP per percentage-point decline in each type of migrant as a fraction of the employed workforce. The size of the U.S. nonfarm labor force N is measured in December 2019 at 152.0 million (BEA 2020a), and initial U.S. GDP Y is measured in the fourth quarter of 2019 at \$21.7 trillion (BEA 2020b).

The rest of the parameters in equation (3) are set as follows. For resettled refugees, as discussed above, the base number of migrants per year M_{base} in 2016 is 84,995; the fraction working age ℓ is 0.809 (HHS 2017a, 24); the fraction of working age employed e is 0.64 (Kerwin 2018, 206); and the percentage change in effect on GDP per percentage point change in migrants

as a fraction of the employed workforce s is 0.88 (from [subsection 2.1.1](#) above).

For asylum seekers, as discussed above, the base number of migrants per year M_{base} in 2017 is 139,917 in the ‘short run’ and 284,579 in the ‘long run’, and the percentage change in effect on GDP per percentage point change in migrants as a fraction of the employed workforce s is 0.4 (from [subsection 3.1](#) above). The US government does not publish statistics that separate all asylum applicants by age. But among affirmative applicants granted asylum in FY 2019, 76.8 percent were working age, 18–64 ([Baugh 2020](#), 9). Thus for asylum seekers, ℓ is set at 0.768. The government likewise does not publish data on the fraction of asylum seekers who are employed. But employment rates of Central American immigrants in the U.S. are *over* 65 percent, that is, over five percent higher than natives ([Peri and Rutledge 2020](#)). Thus for asylum seekers, e is set at 0.66.

[Table 3](#) reports the results of the scenarios for changes in annual GDP from equation (3). For the base years 2016–2017 in part *a* of the table, the assumptions imply that a 25 percent decline in refugee resettlement *in one year* causes an ongoing overall economic loss of \$1.4 billion to all U.S. residents collectively *in each subsequent year*. In the same way, the assumptions imply that a 25 percent reduction in asylum seekers in the short term (a reduction in affirmative asylum applicants), *in one year*, produces an ongoing overall economic loss of \$1.0 billion to all U.S. residents collectively in each subsequent year. For the base year 2019 in part *b* of the table, a 25 percent reduction in refugees in one year causes an ongoing overall economic loss of \$0.5 billion in each subsequent year, and a 25 reduction in affirmative asylum seekers in one year causes an ongoing overall economic loss of \$0.7 billion in each subsequent year.

These estimates of [Table 3](#), using parameters from empirical macroeconomic studies in the literature, suggest roughly double the economic impact seen in the estimates based on household-level data in [Table 1](#). The household-level estimates assume no indirect effects of migrants on any activity elsewhere in the economy; the empirical macroeconomic estimates do not. The impact estimates in [Table 3](#) are thus suggestive of the magnitude of what has been omitted by assumption from [Table 1](#).

That said, the estimates of [Table 3](#) cannot be interpreted as precise estimates of the true economic

Table 3: ECONOMIC IMPACT: Alternative scenarios based on macroeconomic empirical studies

Percent reduction	Base level	Absolute decline	Employed working-age	Percent of employment	GDP decline, billions \$/year
<i>(a) Reduced flows after base year 2016–2017</i>					
Resettled refugees, base year 2016					
10%	84,995	8,500	4,401	0.0029%	0.55
25%	84,995	21,249	11,002	0.0072%	1.39
50%	84,995	42,498	22,004	0.0145%	2.77
75%	84,995	63,746	33,005	0.0217%	4.16
Asylum seekers, <i>short run restriction of inflow</i> , base year 2017					
10%	139,917	13,992	7,092	0.0047%	0.41
25%	139,917	34,979	17,730	0.0117%	1.01
50%	139,917	69,959	35,461	0.0233%	2.03
75%	139,917	104,938	53,191	0.0350%	3.04
Asylum seekers, <i>long run restriction of inflow</i> , base year 2017					
10%	284,579	28,458	14,206	0.0093%	0.81
25%	284,579	71,145	35,515	0.0234%	2.03
50%	284,579	142,290	71,031	0.0467%	4.07
75%	284,579	213,434	106,546	0.0701%	6.10
<i>(a) Reduced flows after base year 2019</i>					
Resettled refugees, base year 2019					
10%	29,916	2,992	1,549	0.0010%	0.20
25%	29,916	7,479	3,872	0.0025%	0.49
50%	29,916	14,958	7,745	0.0051%	0.98
75%	29,916	22,437	11,617	0.0076%	1.46
Asylum seekers, <i>short run restriction of inflow</i> , base year 2019					
10%	96,952	9,695	4,914	0.0032%	0.28
25%	96,952	24,238	12,286	0.0081%	0.70
50%	96,952	48,476	24,572	0.0162%	1.41
75%	96,952	72,714	36,857	0.0242%	2.11
Asylum seekers, <i>long run restriction of inflow</i> , base year 2019					
10%	307,704	30,770	15,361	0.0101%	0.88
25%	307,704	76,926	38,401	0.0253%	2.20
50%	307,704	153,852	76,803	0.0505%	4.40
75%	307,704	230,778	115,204	0.0758%	6.59

Calculated using equation (3).

impact of reductions in refugees and asylum seekers. The estimates are lower bounds on the true effect. The estimates of [Table 3](#) for refugees are based on macroeconomic estimate for immigrants in general, not refugees specifically. That is appropriate for the purposes of estimating a lower bound on the true effect, for the reasons discussed in [subsection 2.1.1](#). And the estimates of [Table 3](#) for asylum seekers are based on empirical macroeconomic estimates from Western Europe, not from the United States. That is appropriate for the purpose of estimating a lower bound on the true effect in the United States, where asylum seekers are much more likely to be employed and thus economically productive than in Europe, for the reasons discussed in [subsection 3.1](#).

5 Discussion

In principle, the above conclusions might be altered if either of two conditions hold. First, the entry of a refugee or asylum seeker could hypothetically raise the amount of government expenses for reasons other than the benefits received by the migrant household. Any such expenses, if they exist, have not been counted above. Second, the capital share of firm revenue could hypothetically be lower for firms hiring refugees and asylum seekers than for other firms. Any such difference, if it existed, would reduce the fiscal revenue attributable to capital income taxation in the calculations above. The evidence in the research literature does not support either of these conjectures. I will discuss each in turn.

5.1 Ripple effects on public expenditures

There is no evidence in the research literature that refugees or asylum seekers cause substantial additional public expenditures for reasons other than the benefits received directly by them, their households, and their descendants.

In fact, the literature has tested and ruled out various mechanisms by which such indirect fiscal costs might emerge. The most obvious mechanism would be if the arrival of refugees and asylum seekers displaced large numbers of U.S. citizens and permanent residents onto unemployment insurance. But refugee resettlement has no detectable effect on the unemployment rate of U.S.

workers at any skill level (Mayda et al. 2017). Studies have likewise failed to detect an impact of even large-scale arrivals of asylum seekers on U.S. workers' unemployment. Both Card (1990) and Borjas and Monras (2017), for example, agree that the very large and sudden arrival of Cuban asylum seekers in Miami in 1980 had no detectable effect on U.S. workers' unemployment at any skill level (Clemens and Hunt 2019). New arrivals of asylum seekers in Western Europe cause *reductions* in native unemployment (d'Albis et al. 2018).

Another hypothetical mechanism for indirect public expenditures would arise if refugees and asylum seekers caused an increase in crime rates, which would require additional expenditures on public services such as policing and incarceration.

But the research literature offers no basis for substantial effects on crime from refugees or asylum seekers on average. There is no relationship between crime and the presence of resettled refugees in U.S. counties (Amuedo-Dorantes et al. 2021). Masterson and Yassenov (2019) study the U.S. administration's recent large cuts to refugee resettlement, which reduced refugee presence much more in some U.S. counties than others, and detect no associated divergence of trends in crime rates. There is likewise no evidence in the literature that asylum seekers cause higher crime rates of any kind. An unknown number of asylum seekers remain in the country as unauthorized immigrants, but unauthorized immigrants have lower rates of felony arrests than U.S. natives (Light et al. 2020) and lower rates of incarceration than U.S. natives, explaining the finding that inflows of unauthorized immigrants do not cause rises in the rates of any type of property crime or violent crime (Gunadi 2021b). Ousey and Kubrin (2018) review the entire criminology literature related to immigration in general and conclude that "the immigration-crime association is negative—but very weak". Couttenier et al. (2019) find an association between violent crime and childhood exposure to civil conflict and mass killing among refugees in Switzerland, but also find that this association is absent in Swiss cantons with active policies to integrate refugees into the local labor market and society. More generally, the literature has failed to find substantial effects of immigration from developing countries on social trust and cohesion that could affect the size of the economy, and thus net government revenue, by any set of diffuse mechanisms (Clemens and Pritchett 2019).

A final, hypothetical mechanism could arise if the arrival of refugees and asylum seekers causes

natives to increase their political preferences for income redistribution, raising government expenditures on welfare relative to tax revenue. There is no evidence in the literature for such an effect of refugees and asylum seekers in particular, and the evidence on immigration in general suggests the opposite: that increased ethnic diversity caused by immigration reduces natives' political preference for redistribution (Alesina and Stantcheva 2020). This could be one mechanism by which the arrival of asylum seekers in Western European countries produces a net positive impact on public finances (d'Albis et al. 2018).

5.2 Differences in capital income share

The above calculations make the approximation that the capital share of income is similar in firms that employ refugees and asylum seekers and firms that do not. It was noted above that this approximation could hypothetically prove less accurate if 1) there are large differences in capital share of income across sectors and 2) refugees and asylum seekers concentrate in those sectors. There is no evidence for such a pattern. Capital share of income is similar across sectors of the U.S. economy with minor exceptions such as the capital-intensive mining sector, and there is no evidence that refugees and asylum seekers concentrate in small, exceptional sectors with low capital share.

A second, hypothetical way that the capital share of income could differ for firms employing refugees and asylum seekers would arise if firms employing low-wage, low-education workers exhibited a lower capital share of the additional revenue created by hiring a worker, *and* if refugees and asylum seekers were typically low-wage, low-education workers. But neither of these conjectures has support in the literature. First, as discussed above, wage markdowns are larger for low-wage workers and for immigrants in general, meaning that if anything, the capital share of additional revenue at firms employing low-wage immigrants is higher than at average firms. Second, there is clear evidence that U.S. refugees are neither low-education nor low-wage workers, and there is no evidence that asylum seekers are systematically low-education or low-wage workers. This latter point reflects an absence of evidence more than evidence of absence—because public data on the education and wages of asylum seekers do not exist. But again, even if asylum seekers on average did have lower education and lower wages than otherwise comparable workers, this suggests that the capital share of income at firms employing them

would be *higher*, not lower. Finally, the direct empirical evidence on the changes in fiscal balance following large inflows of asylum seekers to advanced economies provides an independent check: The results show much larger positive effects on fiscal balance than can be accounted for by increased direct taxation of asylum seekers' labor (d'Albis et al. 2018).

A third, hypothetical challenge to this approximation could arise if capital investment adjusts very slowly to the hiring of new workers like recently-arrived refugees and asylum seekers. But the evidence in the research literature contradicts this conjecture. Capital investment reacts to new, large inflows of immigration between 1 and 5 years after their arrival (Peri 2012; Albert and Monras 2020; Engler et al. 2020). These rapid increases are permanent, persisting decades later (Burchardi et al. 2018), and even generations later (Sequeira et al. 2019). In other words, the average capital share of revenue at firms is a good approximation of the new capital revenue created by hiring additional workers, even in the short run. Beyond this, most asylum seekers in the United States, as discussed above, are people applying for asylum in the course of deportation hearings—many of whom are not new arrivals but have already been working in the United States for an extended period.

6 Caveats and conclusion

This analysis has presented estimated lower bounds on the economic and fiscal costs of reductions in U.S. refugee and asylum seeker inflows. Even for reductions of 10–25 percent, after five years, a lower bound on the cost to the overall economy is in the billions of dollars per year, and a lower bound on the fiscal cost is in the hundreds of millions of dollars per year. Both of these estimates would rise with each additional year beyond this arbitrary five-year benchmark. This is because the cost arises from the missing *ongoing presence* of these immigrants in the economy, and each additional year of reduced inflow further decreases the *number present* relative to what it would have been without the reduced inflow of new arrivals.

The quantitative scenarios presented in the tables above are not estimates of the fixed 'effects' of reducing admissions, in two senses. First, they are lower bounds on the true effects, which are likely to be higher. Second, they are not fixed but contingent on other policies that shape

those effects in the context of the underlying studies. The labor market integration of refugees is high on average, but depends substantially on personal networks (Dagnelie et al. 2019) and in particular on where they are settled: They integrate less well when arriving with a large group of compatriots to the same locale at the same time, but integrate better when arriving in a locale with numerous, earlier arrivals of established compatriots (Beaman 2012; Martén et al. 2019; Frattini et al. 2020; Auer et al. 2022). Labor market integration is also contingent on general economic conditions around the time of arrival (Mask 2020; Aksoy et al. 2020). Labor force participation by asylum seekers is reduced by the barriers to working they face upon arrival (Hainmueller et al. 2016; Marbach et al. 2018; Slotwinski et al. 2019; Fasani et al. 2021). Thus, while work bans are likely to deter asylum seekers from coming (Bugge et al. 2020), they can also tend to reduce the positive economic effects of asylum seekers.

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