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Immigration, Innovation, and Growth[†]

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We propose a novel identification strategy to isolate exogenous immigration shocks across US counties, by interacting quasi-random variations in the composition of ancestry across counties with the contemporaneous inflow of migrants from different countries. We show a positive causal impact of immigration on local innovation and wages at the five-year horizon. The positive dynamic impact of immigration on innovation and wages dominates the short-run negative impact of increased labor supply. A structural estimation of a model of endogenous growth and migrations suggests the increased immigration to the United States since 1965 may have increased innovation and wages by 5 percent. (*JEL* J15, J22, J31, J61, O31, R11, R23)

Does immigration cause more or less innovation and growth? In this paper, we answer this question in the context of international migration to the United States over the last four decades. We find a positive causal impact of immigration on innovation and wage growth at the local level (US counties) over a short horizon (five-year periods) and interpret these findings through the lens of a model of endogenous growth and migrations.

Canonical theories of economic growth suggest a role for immigrants in driving local economic outcomes. Immigrants bring ideas, skills, and effort, and increase demand for new inventions, which stimulates growth (Romer 1990; Jones 1995). In the presence of frictions on mobility, trade, or idea flows, regional models suggest immigrants should have local, not just aggregate, effects on innovation and wages (Desmet, Nagy, and Rossi-Hansberg 2018; Peters 2022). In contrast to these predictions, fierce political controversies surround the economic contribution of migrants: Do migrants drain resources from their host communities and stifle innovation?

A rigorous quantification of the causal impact of immigration on innovation and growth has often proven elusive. The reason is that migrants do not allocate

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randomly across space. Instead, they are likely to choose innovative destinations that offer the best prospects for them, creating a spurious correlation between immigration, innovation, and economic growth.

We make three main contributions to the literature on immigration and growth. First, we propose a formal identification strategy to estimate the causal impact of migrations on innovation and wage growth. We build upon the seminal work of Card (2001) but add one key innovation: instead of using the realized preexisting distribution of foreign origins to predict new migrations, we construct a granular set of instruments for this preexisting distribution, using 130 years of country-county-level migration data. Second, we show that immigration causes a large and persistent increase in local innovations and in the wages earned by natives. Immigration also has distributional effects: more educated natives benefit more from it, and more educated migrants have a stronger positive impact on local wages and innovations. Third, we combine this credibly identified evidence with a regional model of innovation and migrations to structurally estimate the local elasticity of innovation to research labor. This elasticity determines the size of local scale effects and the aggregate response to large-scale immigration.

Our structural model also helps reconcile the seemingly contradictory evidence on the impact of immigration on native wages (Card 1990; Borjas 2003): immigration, a labor supply shock, exerts a negative neoclassical downward pressure on wage; but it also induces dynamic innovations and wage growth. For the United States, on average over the last 40 years, the positive impact from innovation dominates the negative impact from increased labor supply.

We now turn to a description of our three main contributions.

Our first contribution is a granular identification strategy able to isolate quasi-random immigration shocks for each US county in each five-year period starting in 1975. As in the canonical shift-share approach, we rely on the tendency of newly arriving migrants to settle in US counties with large preexisting communities of the same origin. But instead of using past realized immigration (Card 2001) or ancestry (Tabellini 2019) to measure the preexisting distribution of foreign origins, we instrument for the preexisting ancestry distribution (Burchardi, Chaney, and Hassan 2019). Doing so, we guard against the concern that where migrants choose to settle within the United States, both in recent decades (the distribution of immigrants) and in the more distant past (the distribution of ancestry), may be correlated with persistent productivity shocks and other unobserved factors that also affect local innovation and growth.[1]

To isolate quasi random immigration shocks, we proceed in two steps. In a first step, we follow the method in Burchardi, Chaney, and Hassan (2019) to construct a set of instruments for the preexisting distribution of foreign ancestry in 1975. For each period starting in 1880, we predict the number of migrants from a given origin country to a given destination county by interacting the total number of migrants

[1] Card himself notes that past immigration shares may be endogenous (Card 2001, p. 43), and a large literature has since highlighted inference and consistency issues arising from this core issue (e.g., Adão et al. 2019; Borusyak, Hull, and Jaravel 2021; Goldsmith-Pinkham, Sorkin, and Swift 2020; Jaeger, Ruist, and Stuhler 2018). Card further states "[o]ne could potentially overcome this problem by finding a set of instruments that explain the location choices of earlier immigrants from different source countries and using predicted settlement patterns of the earlier cohort to construct the supply push indexes." This is precisely the task we undertake in this paper

arriving in the United States from that country with the share of foreign migrants from other origins who settle in that US county. Iterating this procedure over 100 years, we isolate quasi-random variation in the distribution of ancestry across counties in 1975.

In a second step, we follow the canonical shift-share approach: for each period starting in 1975, we estimate migration into a county from a foreign origin country using only the interaction of *predicted* preexisting ancestry and the contemporaneous inflow of migrants from that origin. Summing over all origin countries, we predict the total number of migrants flowing into each US county at each point in time post 1975 (our panel of immigration shocks).

To further guard against any lingering concerns about identification, we estimate the impact of immigration shocks on *changes* in local innovation and growth, not levels. In many specifications, we even include county fixed effects to control for county-specific trends.

Our second contribution is to quantify the causal impact of immigration on innovation and growth in reduced form. We find a positive and significant causal impact of local immigration on the growth in local patents filed per person: on average, the arrival of 10,000 additional immigrants in a county (close to one standard deviation) increases growth in the flow of patents over a five-year period by 1.22 patents per 100,000 residents, an increase of 25 percent relative to its mean (4.61 new patents per 100,000 residents). Immigration also causes a significant increase in local real income growth. 10,000 additional adult immigrants increase wages per capita by $150 (in 2010 dollars), an 8 percent higher annual wage growth for local workers on average.

The positive effect of immigration on local wages is stronger for more educated native workers: for instance, the same immigration shock increases the wages of college graduates five times more than those of high school graduates. More educated migrants also have a stronger positive impact on local innovation and wage growth: for instance, the increase in the flow of patents caused by immigration is five times larger for migrants with one standard deviation higher education (3.7 more years of schooling) than for migrants of average education (11 years of schooling). We also show that the positive effect of immigration on innovation comes primarily from increased patenting by domestic inventors (about 80 percent), and to a smaller extent from immigrant inventors and mixed teams of domestic and immigrant inventors. Finally, we find evidence that the positive effect of immigration on innovation and wage growth diffuses over space, with surrounding counties within 100 kilometers (km) (60 miles) also benefiting significantly.

Our third contribution is to construct a structural model of endogenous growth and migrations. We use this model to estimate the elasticity of local innovation to research labor, to quantify the aggregate impact of immigration on innovation and growth, and to illustrate the challenges to reduced-form identification. Migrants endogenously choose where to settle within the United States, preferring destinations with higher expected wages and larger communities of their same origin. Workers in local labor markets innovate and produce goods. An immigration shock, a positive labor supply shock, decreases local labor cost, but also stimulates local innovation and productivity growth, making workers more productive. Over a five-year period, the innovation channel dominates so that local wages increase by a small amount initially; over time these positive effects build up, leading to a persistent increase

in local wages and innovation. We structurally estimate our model, targeting the well-identified reduced form effect of immigration on local innovation. We estimate an elasticity of local research output with respect to local research labor equal to 0.8. In addition to governing the distribution of idea production across regions, this elasticity disciplines the magnitude of the aggregate response to immigration shocks. To illustrate this response, we conduct a counterfactual experiment, removing the large rise in immigration to the United States after the 1965 Immigration National Act (a counterfactual reduction of approximately one-sixth of total population growth). This exercise suggests that without the increased immigration between 1965 and 2010, United States per capita patenting and income would be around 5 percent below their current steady state level.

We also show that within our model of endogenous migrations, the identification restriction for a simple shift-share instrument for immigration as in Card (2001) and the subsequent literature are violated. A positive productivity shock increases wages and attracts migrants; because local productivity shocks are highly persistent, the preexisting distribution of foreign origins correlates with contemporaneous productivity shocks for long periods of time. In contrast, within our model, our identification strategy generates instruments that are orthogonal to such shocks and also guard against more subtle, county-country specific, confounds to identification.

Related Literature.—Our paper bridges four strands of the literature.

First, many studies use variants of the canonical shift-share instrument (Card 2001) that takes preexisting foreign-origin shares as given. Recent microeconometrics advances have clarified the conditions under which shift-share designs are valid (Borusyak, Hull, and Jaravel 2021; Goldsmith-Pinkham, Sorkin, and Swift 2020), and why they lead to over-rejection rates (Adão et al. 2019). We build on this literature and isolate exogenous variation in the preexisting spatial distribution of ancestry to construct plausibly exogenous immigration shocks to US counties not subject to these concerns. We also explain, within the controlled environment of a structural model linking local innovation to persistent productivity shocks and endogenous migrations, why conventional shift-share designs are likely biased, while our identification strategy is not.

Second, we contribute to a large empirical literature on the link between immigration, innovation, and technology adoption. This literature has documented large contributions of high-skilled immigrants to innovation and dynamism in the United States (Kerr and Lincoln 2010; Hunt and Gauthier-Loiselle 2010; Stuen, Mobarak, and Maskus 2012; Akcigit, Grigsby, and Nicholas 2017; Arkolakis, Peters, and Lee 2020; Khanna and Lee 2018), spillovers from the arrival of high-skilled scientists and inventors on the productivity of their American peers (Borjas and Doran 2012; Moser, Voena, and Waldinger 2014; Bernstein et al. 2018; Moser, Parsa, and San 2021), and the contribution of migrants to the diffusion of knowledge across borders, local technology adoption, and output and employment (Kerr 2008; Lewis 2011; Lafortune, Lewis, and Tessada 2019; Tabellini 2019; Sequeira, Nunn, and Qian 2020).[2] Our results confirm the disproportionate positive impact of high-skilled

[2] Hanson (2009, 2010) and Lewis (2013) provide early surveys. Lewis and Peri (2015) and Abramitzky and Boustan (2017) give an overview of the broader literature on the effect of immigration on regional economies.

migrants on innovation, but also show that the positive impact of immigration is primarily driven by domestic innovators.

Third, we contribute to the labor literature on immigration and local labor markets (Borjas, 2003; Cortes, 2008; Ottaviano and Peri, 2012; Foged and Peri, 2016; Dustmann, Schönberg, and Stuhler 2017; Monras 2020; Jaeger, Ruist, and Stuhler 2018; Bratsberg et al. 2019). We show that immigration has a nil impact on the wages of the least educated native workers at the five-year horizon, but a strong positive impact for more educated workers. More educated migrants also have a stronger positive impact on native wages.[3] Moreover, our structural model suggests these effects are time varying: immigration increases labor supply which exerts downward pressure on wages on impact; but it also fosters innovation, which increases labor productivity and wages over time. This heterogeneity across groups and over time may explain the seemingly contradictory results the empirical literature has documented in different settings. We show that for the United States over the last 40 years, the average local effect of immigration on wages is positive.

Fourth, endogenous growth theory predicts a positive impact of population growth on economic growth and innovation (Romer 1990), with the nature of these scale effects depending upon the technology for producing ideas and the horizon of analysis (Jones 1995, 1999; Peretto 1998; Young 1998; Laincz and Peretto 2006; Bloom et al. 2020). The local effects of immigration across models depends on frictions to mobility, trade, and idea diffusion (Desmet, Nagy, and Rossi-Hansberg 2018; Peters 2022; Monte, Redding, and Rossi-Hansberg 2018; Giannone 2019; Arkolakis, Peters, and Lee 2020). The quantitative predictions of these models crucially rely on the local scale effect in innovation. Instead of disciplining this object by matching moments of income growth, the usual approach, we structurally estimate its value using our empirical estimates of the link between immigration and innovation. Our counterfactual exercise also shows that local scale effects in innovation govern the aggregate response to immigration, and contributes to a growing literature focused on the interpretation of regional evidence (Nakamura and Steinsson 2014; Guren et al. 2021).

The paper is structured as follows. Section I introduces our data. Section II lays out our identification strategy. Section III estimates the causal effect of immigration on innovation and wage growth. Section IV structurally estimates a model of endogenous innovation and migrations.

I. Data

We collect detailed data on migration, ancestry, migrants' education, patents, and local labor markets. Throughout the paper, we use the subscripts o for origin country, d for US destination county, t for the end year of a five-year interval, and $t-1$ for the end year of the previous five-year interval. Summary statistics are in Table 1, and further details are in Supplemental Appendix A, the data repository, and the replication package's ReadMe file (Terry ⓡ al. 2026).

[3] Arkolakis, Peters, and Lee (2020) estimate the heterogeneous contribution of European immigrants of different skills on US innovation 1880–1920. We document similar patterns for recent decades.

Table 1—Summary Statistics by County-Year

	N	Mean	SD	IQR
Immigration flows and population change				
$Immigration_{d,t}$	21,987	1.42	12.21	0.22
Δ $Population_{d,t}$	21,986	4.02	19.64	2.54
Immigration Shock ($\hat{I}_{d,t}$)	21,987	−0.00	4.99	0.24
Patents				
Patent flows per 100,000 people	21,987	31.27	85.21	22.08
5-year difference in patent flows (PF) per 100,000 people (assignee)	18,846	4.61	37.77	6.35
5-year difference in PF per 100,000 people (assignee, citation weighted)	18,846	4.02	50.09	5.71
5-year difference in PF per 100,000 people (inventor)	18,846	8.55	46.93	16.99
5-year difference in PF per 100,000 people (inventor, citation weighted)	18,846	8.02	72.53	16.12
Wages				
5-year difference in average annual wage	21,977	18.93	56.52	25.80
10-year difference in average annual wage of native nonmovers aged 25+ (NNM)	6,274	15.00	30.81	37.84
10-year difference in average annual wage of NNM with less than high school	6,274	−6.36	36.51	41.75
10-year difference in average annual wage of NNM with high school	6,274	−1.90	29.86	42.36
10-year difference in average annual wage of NNM with some college	6,274	7.54	32.60	43.81
10-year difference in average annual wage of NNM with B.A.	6,274	22.46	48.31	58.26
10-year difference in average annual wage of NNM with graduate school	6,274	46.97	74.06	96.64
Immigration and education				
$Immigration_{d,t}$ (Age 25+)	21,987	0.80	6.91	0.11
Average Years College$_{d,t}$ (Age 25+)	21,987	1.50	1.41	1.82
Average Years Education$_{d,t}$ (Age 25+)	21,987	10.88	3.65	4.59
Spillovers				
$Immigration_{s(d),t}$	21,987	114.21	216.16	84.90
Neighbors' Immigration$_{n(d),t}$ (inverse distance weight)	21,987	1.15	0.78	0.65
$Immigration_{100km(d),t}$ (other counties within 100 km)	21,987	18.58	64.65	9.21
$Immigration_{250km(d),t}$ (other counties within 250 km)	21,987	74.96	133.50	67.60
$Immigration_{500km(d),t}$ (other counties within 500 km)	21,987	123.10	149.52	143.69

Notes: This table displays the number of observations, mean, standard deviation, and interquartile range for all outcome variables considered, as well as the variables for immigration and the immigration instrument. The first section of the table contains summary statistics for immigration (here we focus only on non-European migration) and population growth in 1,000s of people. The second section lists summary statistics for patenting and differences in patenting per 100,000 people. The third section reports summary statistics for wages ($100). Finally, the fourth and fifth section provide summary statistics on the immigration variables used in the education and spillovers analyses, respectively. Variables for immigration, population growth, and education are all for five-year periods, as are the differenced outcomes except in the case of differences in average annual wage for natives and native nonmovers, which are over 10-year periods.

Immigration and Ancestry.—Following Burchardi, Chaney, and Hassan (2019), our immigration and ancestry data are constructed from the individual files of the *Integrated Public Use Microdata Series* (IPUMS) samples of the 1880, 1900, 1910, 1920, 1930, 1970, 1980, 1990, and 2000 waves of the US census, and the 2006–2010 five-year sample of the American Community Survey. We weigh observations using the personal weights provided by these data sources. I_{odt} is the number of respondents who immigrated from o to d between $t-1$ and t. A_{odt} is the number of respondents in d who claim ancestry from o at t. Our dyadic dataset covers 3,141 US counties, 195 foreign countries, and 10 census waves. Supplemental Appendix A.1 gives additional details.

Innovation.—To measure innovation we use patent microdata from the US Patent and Trademark Office (USPTO) from 1975 to 2010. We match the patent assignee

locations from the USPTO in coordinate form to 1990 US counties, tabulating the number of corporate utility patents granted to assignees in each county in each year of the sample.[4] The patent flow in county d at t is the sum of patents filed in the five-year period ending at t. We normalize this variable by the 1970 county population to measure patent flow per capita. Our primary outcome of interest is the *change* in patent flows per capita between the five-year period ending in t and the five-year period ending in $t-1$. Supplemental Appendices A.2 and A.3 give additional details.

Wages.—We compute from 1975 to 2010 the local average annual wages using the *Quarterly Census of Wages* (QCEW) dataset from the US Bureau of Labor Statistics, deflated by the personal consumption expenditure price index. Our primary outcome of interest is the change in real wages per capita over 5 years (measured in 2010 dollars). We also compute the change in the average annual wages over 10 years, CPI-deflated, for US-born workers (natives) and the subset of natives who have lived in the same county for five years (native nonmovers) using data from IPUMS USA. Supplemental Appendix A.4 gives additional details.

II. Constructing a Valid Instrument for Immigration

Our aim is to estimate the causal impact of immigration on innovation and local wage growth, which can inform a structural model of endogenous growth and migrations. We estimate

$$\Delta Y_{d,t} = \delta_t + \delta_{s(d)} + \beta \cdot \mathit{Immigration}_{d,t} + \epsilon_{d,t}, \tag{1}$$

where $\mathit{Immigration}_{d,t}$ measures the number of migrants flowing into destination county d between $t-1$ and t. $\Delta Y_{d,t}$ is a change from $t-1$ to t in the outcome of interest, usually the change in the number of patents filed per capita in the county. This specification in *changes* ensures any long-lasting differences between counties that are on average more or less innovative are controlled for, and eliminates the skewness of the left hand side variable. δ_t and $\delta_{s(d)}$ are time and state fixed effects, respectively. Our most conservative specifications also include a county fixed effect, δ_d, which controls for any county-specific trend in $Y_{d,t}$, so that for example we exploit only deviations from the county's average growth of patent flows over time.

The main concern with a simple OLS estimate of β is that unobserved factors may affect both immigration and innovation, even though we estimate (1) in differences, and even with fixed effects that absorb state- or county-specific trends. We spell out two identification concerns explicitly, and propose a solution. The first is a simple *reverse causality* concern: local wages are likely correlated with local productivity shocks and innovation, and foreign migrants are in part attracted by higher wages. This induces a spurious correlation between immigration and innovation, where counties that become more innovative attract more migrants over time because they

[4] We use the location of assignees rather than innovators as the majority of recent patents are assigned to corporations, unlike in earlier periods (Akcigit, Grigsby, and Nicholas 2017). For robustness, we also replicate our results using alternative assignments, and various weights to control for patent quality (Hall, Jaffe, and Trajtenberg 2001).

pay higher wages. The second is a *county-country specific omitted factors* concern: workers from a specific country (say India) may disproportionately have specific skills (say engineering) that are well suited for specific sectors (say telegraph, aeronautics, and software development) that are concentrated in a specific county (say Silicon Valley in Santa Clara county). Any time a positive shock to productivity and innovation occurs in that sector (e.g., a shock to the telegraph industry in the 1900's, to aeronautics in the 1960's, or to software development in the 2000's), workers from that country (India) will be drawn to this county (Silicon Valley)—resulting in spurious correlations between local innovation, immigration, and foreign ancestry.[5]

We propose an identification strategy plausibly immune to both concerns. Drawing on the seminal work of Card (2001), we leverage the tendency of incoming migrants to settle in US counties with large preexisting communities from the same ancestry. However, we depart from Card (2001) and the subsequent literature employing the canonical shift-share approach by using only plausibly exogenous variation in preexisting ancestry.

The identification strategy is best described by a stylized example. We predict a relatively large inflow of migrants from o (say Indians relative to other Asians) to d (say Fresno in the central valley of California relative to other destinations on the West Coast) at a point in history τ (say 1900) if the following happens: in 1900, many Indians migrate to the United States including towards regions outside the West Coast (1900 corresponds to the first historical Indian migration wave to the United States) and Fresno county is attractive to foreign migrants from any origin, including from Europe (1900 corresponds to the beginning of oil exploitation in Fresno, and an increase in agricultural production following the construction of irrigation canals in the late nineteenth century). This early settlement of Indian migrants in Fresno partly explains the large community of Indian ancestry in Fresno after 1975.[6] Our identification strategy applies this logic for all periods starting in 1880, all origin countries, and all destination counties. We isolate granular variation in the ancestry composition of US counties that emanates solely from the coincidence of migrants being "pushed" from their country and "pulled" into US counties attractive to the average migrant. We then simply apply the Card (2001) shift-share method using predicted ancestry: any time post-1975 there is a large inflow of migrants from India to the United States, we predict Fresno receives a positive immigration shock, because some newly arriving Indian migrants choose to settle in Fresno with its large (exogenous) preexisting Indian community.

Step 1: Predicting Ancestry: To predict the number of residents with ancestry from o who reside in d at t, $A_{o,d,t}$, we apply the method developed in Burchardi, Chaney, and Hassan (2019). We only give a brief summary here. Burchardi, Chaney, and Hassan (2019) show that a simple reduced form model of

[5] A conventional shift-share design would wrongly assume that preexisting immigration or ancestry shares (e.g., Indians engineers in Silicon Valley) are orthogonal to future innovation shocks (e.g., shocks to the tech sector in Silicon Valley).

[6] Supplemental Appendix Figure 1 shows how the timing of migrations varies between foreign origins; one can notice the early (small) spike in Indian migration in 1900. Supplemental Appendix Figure 2 shows that US counties are attractive to foreign (European) migrants at different points in time; Fresno in 1900 was attractive to Europeans migrants.

migrations driven by "push" and "pull" shocks, combined with a rigorous leave-out strategy, allows to identify variations in $A_{o,d,t}$ that are plausibly exogenous not only to local factors, d-specific, but also to bilateral factors, (o,d)-specific. We develop a structural model of migrations explicitly featuring those two forces in Section IV. Formally, we estimate

$$(2)\quad A_{o,d,t} = \delta_{o,r(d)} + \delta_{c(o),d} + X'_{o,d}\zeta + \sum_{\tau=1880}^{t} a_{r(d),\tau} I_{o,-r(d),\tau} \frac{I_{Europe,d,\tau}}{I_{Europe,\cdot,\tau}} + \nu_{o,d,t},$$

where $I_{o,-r(d),\tau}$ is the total number of migrants arriving from o at τ who settle in counties outside of the region $r(d)$ where d is located,[7] a "push from o" shock. $I_{Europe,d,\tau}/I_{Europe,\cdot,\tau}$ is the share of European migrants who settle in d at τ, a "pull to d" shock. $\delta_{o,r(d)}$ and $\delta_{c(o),d}$ are a series of origin country $\times$ destination region and origin continent $\times$ destination county interacted fixed effects, and $X_{o,d}$ contains a series of time-invariant controls for $\{o,d\}$ characteristics. We estimate (2) separately for each $t = 1980, 1985, 1990, 1995, 2000, 2005, 2010$ using all non-European countries in our sample. From this estimation, we derive predicted ancestry

$$(3)\qquad \hat{A}_{o,d,t} = \sum_{\tau=1880}^{t} \hat{a}_{r(d),\tau} \left(I_{o,-r(d),\tau} \frac{I_{Europe,d,\tau}}{I_{Europe,\cdot,\tau}} \right)^{\perp},$$

where $\hat{a}_{r(d),\tau}$ are the coefficients estimated from (2) and $\perp$ indicates that the interaction of push and pull factors has been residualized with respect to all of the controls in (2), isolating the variation in predicted ancestry exclusively attributable to these instruments.

This reduced form regression captures the intuition above: we expect a large community of ancestry from India living in Fresno in 1980 if many Indian migrants in 1900 settled outside the West Coast ($I_{India,-r(Fresno),1900}$ large) and Fresno in 1900 was attracting a large share of European migrants ($I_{Europe,Fresno,1900}/I_{Europe,\cdot,1900}$ large). Given both the "push-pull" interaction and the restrictive leave-out strategy (we exclude the West Coast from India's push, and exclude all non-European migrants from Fresno's pull), we ensure that predicted ancestry from (2) does not suffer from the endogeneity concerns above. For instance, we leave out Indian migrants with specific skills who may have endogenously chosen where to settle. Had we used realized ancestry instead, we may have included 1980 descendants of 1960 Indian migrants with engineering skills who endogenously settled in Silicon Valley at the time of the early development of aeronautics. This would have induced a spurious correlation between contemporaneous Indian ancestry and productivity and innovation shocks in Silicon Valley, such as software development in 2000.

Step 2: Predicting Immigration: Having predicted preexisting ancestry, we can now simply apply the canonical shift-share approach by interacting *predicted*

[7] "Region" refers to the nine US census divisions, on average five adjacent states (see Supplemental Appendix Table 1).

preexisting ancestry in a given county with contemporaneous (US-wide) immigration from that origin,

$$(4)\quad I_{o,d,t} = \delta_{o,r(d)} + \delta_{c(o),d} + \delta_t + X'_{o,d}\theta + b_t \cdot \left[\hat{A}_{o,d,t-1} \times \tilde{I}_{o,-r(d),t}\right] + u_{o,d,t},$$

where again the δ's are time, country×region, and continent×county fixed effects, $X'_{o,d}$ observable controls, $\hat{A}^{t-1}_{o,d}$ predicted ancestry from (3), and $\tilde{I}_{o,-r(d),t} = I_{o,-r(d),t}\left(I_{Europe,r(d),t}/I_{Europe,-r(d),t}\right)$ the scaled push factor from o. (Because we leave out from $I_{o,-r(d),t}$ all migrants from o who settle in d's region, scaling by $I_{Europe,r(d),t}/I_{Europe,-r(d),t}$ corrects for differences in region sizes.)

Adding up across foreign origins, we derive our main instrument for the total number of migrants settling in county d in period t, Immigration$_{d,t}$ in (1),

$$(5)\qquad \hat{I}_{\cdot,d,t} = \sum_o \hat{b}_t \cdot \left[\hat{A}_{o,d,t-1} \times \tilde{I}_{o,-r(d),t}\right].$$

This instrument predicts a large immigration shock to Fresno County in 2000 if (i) many Indians migrate to the United States in 2000 (excluding the West Coast), and (ii) we predict a large preexisting community of Indian ancestry in Fresno. The key innovation relative to Card (2001) is to rely on predicted ancestry using historical migrations, instead of realized immigration or ancestry.

Identifying Assumption.—A sufficient condition for the validity of this instrument is that predicted ancestry, $\hat{A}_{o,d,t-1}$, is exogenous in equation (1). With our baseline regional and continental leave-outs, we can write this condition as

$$(6)\qquad I_{o,-r(d),\tau}\frac{I_{Europe,d,\tau}}{I_{Europe,\cdot,\tau}} \perp \epsilon_{d,t} \forall o, \tau \leq t.$$

It requires that any confounding factors that drive temporary increases in a given US county's innovation post-1975 ($\epsilon_{d,t}$) do not systematically correlate with pre-1975 immigration from a given origin to other regions within the United States ($I_{o,-r(d),\tau}$) interacted with the simultaneous settlement of European migrants in that US destination ($I_{Europe,d,\tau}/I_{Europe,\tau}$). If this condition is satisfied, the ancestry variable used to predict immigration in (5) is exogenous.[8]

Performance of the Instrument.—Table 2 shows the results from estimating various specifications of (4). The interaction of predicted ancestry ($\hat{A}_{o,d,t-1}$)[9] with national immigration shocks ($\tilde{I}_{o,-r(d),t}$) predicts immigration flows post-1975 ($I_{o,d,t}$) accurately: The R^2 in column 1 without any controls is 65.6 percent. Importantly, the coefficients on the interaction between predicted ancestry and national immigration are virtually unchanged as we add more controls: controlling for distance, latitude

[8] Exogeneity of ancestry is a sufficient, but generally not a necessary condition for the validity of the shift-share approach (Goldsmith-Pinkham, Sorkin, and Swift 2020). See Borusyak, Hull, and Jaravel (2021) for necessary and sufficient conditions for the validity of the shift-share instrument of Card (2001) and Bartik (1991).

[9] Supplemental Appendix Figure 3 shows a binned scatter plot of predicted ancestry ($\hat{A}_{o,d,2010}$) against actual ancestry ($A_{o,d,2010}$), well-aligned along the 45-degree line.

Table 2—Regressions of Immigration on Push-Pull Instruments at the Country-County Level

	$Immigration^{t}_{o,d}$				
	(1)	(2)	(3)	(4)	(5)
$\hat{A}_{o,d,1975} \times \tilde{I}_{o,-r(d),1980}$	0.0036	0.0036	0.0035	0.0035	0.0035
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
$\hat{A}_{o,d,1980} \times \tilde{I}_{o,-r(d),1985}$	0.0016	0.0016	0.0016	0.0016	0.0016
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
$\hat{A}_{o,d,1985} \times \tilde{I}_{o,-r(d),1990}$	0.0018	0.0018	0.0018	0.0018	0.0018
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
$\hat{A}_{o,d,1990} \times \tilde{I}_{o,-r(d),1995}$	0.0005	0.0005	0.0005	0.0005	0.0005
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
$\hat{A}_{o,d,1995} \times \tilde{I}_{o,-r(d),2000}$	0.0004	0.0004	0.0004	0.0004	0.0004
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
$\hat{A}_{o,d,2000} \times \tilde{I}_{o,-r(d),2005}$	0.0002	0.0002	0.0002	0.0002	0.0002
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
$\hat{A}_{o,d,2005} \times \tilde{I}_{o,-r(d),2010}$	0.0002	0.0002	0.0002	0.0002	0.0002
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
$I_{Euro,d,t}$				0.0109	
				(0.0031)	
$I^{t}_{o,-r(d)} \frac{I_{Euro,d,t}}{I^{Euro,-,t}}$					0.3913
					(0.1558)
Observations	3,583,881	3,583,881	3,583,881	3,583,881	3,583,881
R^2	0.656	0.657	0.709	0.709	0.709
Distance	No	Yes	Yes	Yes	Yes
Latitude dis.	No	Yes	Yes	Yes	Yes
Region-country fixed effects	No	No	Yes	Yes	Yes
County-continent fixed effects	No	No	Yes	Yes	Yes
Time fixed effects	No	No	Yes	Yes	Yes
Concurrent European immigration	No	No	No	Yes	No
Contemporaneous push-pull	No	No	No	No	Yes

Notes: This table reports coefficient estimates for step 2 of our instrument construction, shown in equation (4), at the country-county level. Interpretation: in column 1, $Immigration^{1980}_{o,d}$ loads on $\hat{A}_{o,d,1975} \times \tilde{I}_{o,d,1980}$ with a coefficient 0.0036 while $Immigration_{o,d,1985}$ loads on $\hat{A}_{o,d,1980} \times \tilde{I}_{o,d,1985}$ with a coefficient 0.0016. Moving from column 1 to column 3 we introduce controls for distance and latitude distance and then fixed effects into the regression specification. Column 4 adds contemporaneous European migration as a control while column 5 instead introduces the contemporaneous push-economic pull factor for non-European migration. Standard errors are clustered by country for all specifications.

distance, country and county fixed effects in column 2; adding 14,031 country $\times$ census division and county $\times$ continent interacted fixed effects in column 3; controlling for the (endogenous) total flow of European migrants to the same county in column 4; and controlling for the push-pull interaction that shapes immigration in column 5. Supplemental Appendix Figure 4 provides maps displaying these exogenous "immigration shocks" for each five-year period from 1975 to 2010, $\hat{I}_{\cdot,d,t}$ in (5), with substantial variation over time and space.

III. The Impact of Immigration on Innovation and Growth

In this section, we exploit our quasi-random immigration shocks to quantify the causal impact of immigration on innovation and growth, and probe the robustness of our results.

Table 3—County-Level Panel Regressions of Difference in Patenting on Immigration

	Δ^{5yr} Patent flows per capita			IHS(Patent flows per capita)
	(1)	(2)	(3)	(4)
Panel A. OLS				
$Immigration_{d,t}$	0.200	0.194	0.309	
	(0.096)	(0.096)	(0.197)	
IHS($Immigration_{d,t}$)				1.751
				(0.140)
Observations	18,846	18,840	18,846	21,987
R^2	0.030	0.053	0.190	0.577
Panel B. IV				
$\text{Immigration}_{d,t}$	0.122	0.115	0.181	
	(0.045)	(0.045)	(0.087)	
IHS($Immigration_{d,t}$)				1.652
				(0.150)
Observations	18,846	18,840	18,846	21,987
First stage F-stat	911	807	85	94
AR wald F-test p-value	0.014	0.021	0.013	0.000
Panel C. First stage	$Immigration_d^t$			IHS($Immigration_d^t$)
Immigration shock ($\hat{I}_{d,t}$)	2.119	2.124	1.610	
	(0.070)	(0.075)	(0.175)	
IHS($\hat{I}_{d,t}$)				0.792
				(0.081)
Observations	18,846	18,840	18,846	21,987
R^2	0.762	0.766	0.956	0.541
Geography fixed effects	State	State	County	State
Time fixed effects	Yes	Yes	Yes	Yes
State-time fixed effects	No	Yes	No	No

Notes: Panels A and B of this table reports the OLS and IV results, respectively, of the estimation of equation (1) where the dependent variable is the change in patenting per 100,000 people (population is based on baseline 1970 levels) in county d in the five-year period ending in t and the endogenous variable is non-European immigration (1,000s) in d and period t in columns 1 to 3; column 4 reports results for a comparable regression of the inverse-hyperbolic sine (IHS) of patenting per 100,000 people on the IHS of non-European immigration (1,000s). Panel C reports the results for step 3 of instrument construction, or the coefficient estimates for the first-stage specification for non-European immigration (1,000s) for the instrument described in equation (5). The table includes the first-stage F-statistic on the excluded instrument and the p-value for the Anderson-Rubin Wald F-test for each of the IV specifications. Standard errors are clustered by state for all specifications.

A. *Immigration and Innovation*

Table 3 shows our main results, a test of the hypothesis that immigration *causes* an increase in innovation at the local level (US county), in the short run (five-year period). Our baseline estimate for coefficient β in (1) is in panel B column 1. It measures the impact of an exogenous inflow of immigrants to county d (in 1,000s) on the *change* in the number of patents per 100,000 residents filed over five years, where we instrument for immigration using our immigration shocks (5). The estimated effect is positive and statistically significant (0.122, SE = 0.045). It implies that 10,000 additional immigrants to a county (close to one standard deviation, 12,000) increases the

flow of patents filed locally over a five-year period by $0.122 \times 10 = 1.22$ patents per 100,000 people, from 4.61 patents (its mean) to 5.83, a 25 percent increase.

Panel A shows OLS estimates, panel B our IV estimates, and panel C estimates of the first stage of our IV. Across specifications, our IV estimates are lower than OLS estimates (though not statistically significantly different), suggesting that migrants endogenously sort into destinations that experience an increase in innovation.[10] Consistent with the presence of reverse causality or country-county specific confounding factors, the OLS estimates are unstable as we add more controls, state $\times$ time fixed effects in column 2, and county fixed effects in column 3. By contrast, the IV estimates remain stable across specifications, even when controlling for county fixed effects, so exploiting solely variation in the growth rate of innovation within-county over time. This stability bolsters our confidence that our exogenous immigration shocks are orthogonal to persistent confounding factors at the county-level. Our first stage has a strong F-statistic (always above 85) and Anderson-Rubin Wald F-test (all p-values below 2 percent).

Finally, column 4 shows an alternative functional form, the *elasticity* of innovation to immigration. Instead of estimating the impact of immigration on innovation in levels as in (1), we use the inverse hyperbolic sine transformation, IHS, which approximates the logarithm function,

$$IHS\big(Patents_{dt}\big) = \delta_t + \delta_{s(d)} + \beta_{IHS} IHS\big(Immigration_{dt}\big) + \epsilon^t_d, \tag{7}$$

where we instrument for immigration using the same instrument (5) as in our baseline specification. We find a large and significant elasticity of patenting to immigration shocks, $\beta = 1.652$ (SE $= 0.150$). We interpret this large positive impact of immigration on the flow of patenting through the lens of a regional endogenous growth model in Section IV, and we quantify this model targeting the well-identified reduced-form elasticity β_{IHS}.

B. *Immigration and Wage Growth*

Table 4 tests the hypothesis that immigration *causes* an increase in local wages. Even if immigration has a positive impact on innovation as documented above, the impact on wages is theoretically ambiguous, as we show formally in Section IV. An inflow of immigrants increases the local supply of labor, which depresses wages. At the same time, it induces a rise in local innovation, which increases the marginal product of labor and pushes wages up. The net effect of immigration on local wages in the short run thus remains an empirical question.

We show that immigration has a positive impact on local wages over a five-year horizon at the county level, controlling for state fixed effects (column 1) and county fixed effects (column 2).[11] An influx of 10,000 adult migrants (close to one standard deviation, 7,000) increases wages per capita by around 8 percent relative to the mean

[10] Note that the standard errors are lower in the IV specifications than in the OLS specification. This is because we compute cluster-robust standard errors (Cameron and Miller 2015).

[11] For ease of interpretation, we use adult immigration (aged 25+) as the endogenous variable in all of our regressions regarding wages and education.

Table 4—Immigration and Wages

	Δ^{5yr} wages		Δ^{10yr} wages					
	Average annual wage		All native nonmovers	Less than high school	High school	1 to 3 years of college	4 years of college	5+ years of college
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Immigration_{d,t}$	0.149	0.217	0.108	−0.007	0.017	0.029	0.085	0.247
	(0.030)	(0.098)	(0.034)	(0.007)	(0.005)	(0.011)	(0.025)	(0.085)
Observations	21,977	21,976	6,274	6,274	6,274	6,274	6,274	6,274
First stage F-stat	903	37	936	936	936	936	936	936
AR wald F-test p-value	0.000	0.039	0.006	0.323	0.001	0.021	0.003	0.010
Geography fixed effects	State	County	State	State	State	State	State	State
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the results of our IV specification, described in equation (1), for each of our dependent variables with non-European immigration for those aged 25+ (1,000s) to d in t as the endogenous variable. Column 1 reports the results of our IV regression where the dependent variable is the change in the average annual real wage ($100s, at 2010 prices) over the five-year period ending in t. Column 2 repeats the specification of column 1 but adds county fixed effects. Columns 3 through 8 report results where the dependent variable is the change in the average annual real wage ($100s, at 2010 prices) for native nonmovers (those with birthplace listed as a US state or Washington, DC who have not moved in the last 5 years) over the 10-year period ending in t on instrumented non-European immigration for the 10-year period ending in t. Column 3 relies on wage data for all native nonmovers aged 25 and older while columns 4 through 8 further limit the sample of wage data to native nonmovers aged 25 and older with education levels of less than high school, high school, 1 to 3 years of college, 4 years of college, and at least 5 years of college, respectively. We report the first-stage F-statistic on the excluded instrument and the p-value for the Anderson-Rubin Wald F-test for each specification. Standard errors are clustered by state for all specifications.

increase in our sample.[12] This is not mechanically driven by high-earning migrants, nor by a composition effect where low-earning natives may leave in response to the arrival of migrants (Borjas 2003): immigration induces an increase in wages even when restricting our analysis to native nonmovers (column 3).

C. *Immigration and Wage Inequality*

We next show that immigration shocks are intimately linked to the dynamics of wage inequality: an immigration shock has a stronger positive impact on more educated native workers; and more educated migrants contribute more to US innovation and wage growth.

High versus Low Education Natives.—Columns 4 to 8 in Table 4 estimate the impact of an exogenous immigration shock on the change in wages separately for native nonmovers with different levels of education, from high school dropouts (column 4) to a graduate degree (column 8).[13] The impact of immigration on the wage of native workers is monotonically increasing with their level of education: the effect is nil for high school dropouts (column 4), and increases by one order of

[12] Potentially interesting for the interpretation of our results in the context of structural spatial growth models, we find this positive effect of immigration on wages is higher in services (nontraded sector), with a coefficient of 0.429 (SE 0.135), than in manufacturing (traded sector), with a coefficient of 0.211 (SE 0.046).

[13] We use a different horizon, 10 years instead of 5, and data from the US census instead of QCEW wage data.

magnitude going from a high school degree (column 5, $\beta = 0.017, \text{SE} = 0.005$) to a graduate degree (column 8, $\beta = 0.247, \text{SE} = 0.085$). The impact of immigration on college graduates (column 7, $\beta = 0.085, \text{SE} = 0.025$) is roughly the same as the average impact for the entire population of native nonmovers (column 3, $\beta = 0.108, \text{SE} = 0.034$).

High versus Low Education Migrants.—The granularity of our identification strategy in Section II also allows us to separately instrument for high versus low education migrants. To do so, we exploit the fact that the level of education of migrants differs by origin country, and by migration time. For example, Japanese immigrants have, on average, twice the number of years of schooling as those from Guatemala, whereas the education levels of Mexican migrants increased by about 30 percent during our sample period. We disaggregate our baseline instruments in (5) at origin-destination-time level, $\hat{I}_{o,d,t} = \hat{\beta}_t \cdot \hat{A}_{o,d,t-1} \times \tilde{I}_{o,-r(d),t}$, for each of the top 20 origin countries (those that send the most migrants), and generate a set of instruments for the number of years of education embodied in the migration flow from each origin to each destination.[14] The first stage for this additional endogenous variable is

$$\textit{Average Years Education}_{d,t} \times \textit{Immigration}_{d,t} = \delta_{s(d)} + \delta_t + \sum_{o=1}^{20} \kappa_o \hat{I}_{o,d,t} + \nu_{d,t}.$$

Because migrants from different countries at different times have different schooling levels and emigrate to different counties, we are able to isolate exogenous variation in the level of education of migrants across destinations and time. For example, other things equal, an exogenous increase of Japanese migrants to a destination induces an increase in the average education of migrants.

Table 5 panel A shows the positive impact of immigration on innovation increases with the level of education of migrants. Column 1 replicates our standard specification for the age 25+ immigration sample, with a positive—now stronger than baseline—impact of immigration on the growth of patenting per capita.[15] column 2 adds the interaction of immigration with (demeaned) average years of schooling: more educated immigrants cause a larger increase in innovation. 10,000 migrants of average education (about 11 years) cause 2.5 more local patents per 100,000 residents to be filed in a five-year period (10×0.254), while 10,000 migrants with one standard deviation higher education (3.7 more years of schooling) cause 13

[14] We restrict our analysis to immigrants age 25 or older, constructing the endogenous measure of immigration at the county level for this subset of immigrants. We then interact this overall adult immigration flow with the average schooling levels of adult migrants arriving in a given county at a given time from IPUMS, which lists information on the number of years of schooling and the number of years of college education for each respondent. See Supplemental Appendix A.1 for details.

[15] In column 1 of Table 5 (both panels), we consider a specification with a single endogenous regressor and multiple instruments, and therefore report the first-stage *F*-statistic developed in Montiel Olea and Pflueger (2013). The remaining columns in this table report results for specifications with multiple endogenous variables and multiple instruments and, to our knowledge, there is no comparable effective *F*-statistic in this case (Andrews, Stock, and Sun 2019). To nevertheless gauge our ability to identify differential exogenous variation in the separate endogenous variables, the table shows the *F*-statistics from Montiel Olea and Pflueger (2013) after applying the orthogonalization procedure in Angrist and Pischke (2009) to each endogenous variable. In column 2 of both panel A and B those *F*-statistics exceed the critical values for a 10 percent bias. In further columns the *F*-statistics do not always exceed the critical values, indicating possibly weak instruments.

Table 5—Education Analysis

	(1)	(2)	(3)	(4)	(5)
Panel A. Δ^{5yr} *patent flows per capita*					
$Immigration_{d,t}$	0.212	0.254	0.584	0.514	
	(0.081)	(0.082)	(0.356)	(0.128)	
$Average\ Years\ Education_{d,t} \times Immigration_{d,t}$		0.281	0.280		
		(0.094)	(0.128)		
$Average\ Years\ College_{d,t} \times Immigration_{d,t}$				1.076	
				(0.283)	
$\mathbf{1}\{Low\ Avg.\ Years\ Education\} \times Immigration_{d,t}$					−1.671
					(5.620)
$\mathbf{1}\{Medium\ Avg.\ Years\ Education\} \times Immigration_{d,t}$					0.105
					(0.062)
$\mathbf{1}\{High\ Avg.\ Years\ Education\} \times Immigration_{d,t}$					1.705
					(0.830)
Observations	18,846	18,846	18,846	18,846	18,846
Montiel-Pflueger effective *F*-stat	39	38; 13	18; 15	19; 5	4; 37; 3
AR wald *F*-test *p*-value	0.000	0.000	0.000	0.000	0.000
Panel B. Δ^{5yr} *wages*					
$Immigration_{d,t}$	0.243	0.298	0.761	0.424	
	(0.095)	(0.058)	(0.385)	(0.093)	
$Average\ Years\ Education_{d,t} \times Immigration_{d,t}$		0.251	0.238		
		(0.055)	(0.097)		
$Average\ Years\ College_{d,t} \times Immigration_{d,t}$				0.640	
				(0.101)	
$\mathbf{1}\{Low\ Avg.\ Years\ Education\} \times Immigration_{d,t}$					−0.264
					(0.259)
$\mathbf{1}\{Medium\ Avg.\ Years\ Education\} \times Immigration_{d,t}$					0.183
					(0.064)
$\mathbf{1}\{High\ Avg.\ Years\ Education\} \times Immigration_{d,t}$					1.637
					(0.360)
Observations	21,977	21,977	21,976	21,977	21,977
Montiel-Pflueger effective *F*-stat	42	39; 16	15; 18	19; 5	41; 29; 3
AR Wald *F*-test *p*-value	0.000	0.000	0.000	0.000	0.000
Geogrpahy fixed effects	State	State	County	State	State
Time fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: The table reports the results of our IV specification (1) for the change in patenting per 100,000 people (population is based on baseline 1970 levels) in panel A and the 5-year difference in county-level average real annual wages ($100s, at 2010 prices) in panel B. Column 1 repeats our main specification but adjusting the migrant pool to those aged 25+ (1,000s). Columns 2 and 3 then add a second endogenous variable for the interaction of immigration with the (demeaned) average years of education of the migrants arriving in the destination county, whereas column 4 adds (demeaned) average years of college education of those migrants. Repeating the regression in column 2 of the second panel for the 10-year difference in average annual wages ($100s, at 2010 prices) of native nonmovers (US-born working individuals who have not moved outside of the county within the past 5 years) on 10-year migration and corresponding education results in coefficients of 0.249 (0.054) and 0.138 (0.036) on immigration and average years of education times immigration, respectively. Column 5 uses as endogenous variables adult immigration interacted with indicators for the terciles of average years of education of migrants across counties in period *t*. In all specifications, for instrumentation, we exploit the fact that in our initial instrument construction we created quasi-exogenous immigration shocks for each origin country-*o* × destination county-*d* pair in each time period *t*; each specification utilizes the predicted immigration shocks for each of the the top 20 origin nations as a joint set of instruments. For each regression we report the Montiel Olea and Pflueger (2013) effective *F*-statistic. In regressions with multiple endogenous variables we use the orthogonalizaiton method described in Angrist and Pischke (2009, p. 217–218). See the main text for details. We report the *p*-value for the Anderson-Rubin Wald *F*-test for each specification. Standard errors are clustered by state for all specifications.

more local patents to be filed $(10 \times (0.254 + 3.7 \times 0.281))$. Those effects are similar when controlling for county fixed effects (column 3), or when measuring years of college instead of total years of education (column 4). Column 5 uses a nonparametric measure: while the (positive) impact of migrants in the bottom tercile of education on innovation is insignificant, the impact of those in the top tercile is one order-of-magnitude larger than for immigrants with average education. The effect for the middle tercile, while lower than that for the average migrants—about half—is significantly positive. Our results are thus consistent with a large literature on the special role of educated migrants (Kerr and Lincoln 2010; Hunt and Gauthier-Loiselle 2010; Akcigit, Grigsby, and Nicholas 2017; Borjas and Doran 2012; Moser, Voena, and Waldinger 2014; Bernstein et al. 2018), although we show innovation is not exclusively attributable to this elite group.

Table 5 panel B shows similar results for wage growth. A county receiving 10,000 migrants with average schooling would see average annual wages increase by \$300 (in 2010 dollars) over five years $(10 \times 0.298 \times \$100)$, while receiving 10,000 migrants with one standard deviation higher education (3.7 more years of schooling) increases average annual wages by \$1,200 over five years $(10 \times (0.298 + 3.7 \times 0.251) \times \$100)$. The effect for the top tercile of education is also one order-of-magnitude larger than for migrants with average education. This heterogeneity may partly explain the seemingly contradictory findings of the literature on the impact of immigration on wages (Borjas 2003; Cortes 2008; Ottaviano and Peri 2012; Foged and Peri 2016; Dustmann, Schönberg, and Stuhler 2017; Monras 2020; Jaeger, Ruist, and Stuhler 2018; Bratsberg et al. 2019).

D. *Robustness and Additional Findings*

Below, we relate our approach to ongoing debates on the merits of the shift-share approach to identification and show our results are robust to a large array of alternative specifications.

Randomization Tests.—Supplemental Appendix Table 2 implements a randomization test developed by Adão et al. (2019) to gauge whether our instrument suffers from an over-rejection problem typical of shift-share designs:[16] two US counties with similar preexisting ancestry may also have similar exposure to other (unobservable) economic forces, leading to a dependency across residuals not accounted for by conventional clustered standard errors. We randomly generate immigration "shift" shocks for each $\{o, r, t\}$ country-region-time triplet and construct placebo instruments by interacting these random shocks with our predicted ancestry shares,

[16] To clarify the comparison, the shifts are industry shocks in Adão et al. (2019) versus immigration shocks in our case; the shares are employment shares in Adão et al. (2019) versus ancestry shares in our case; the variation is at the sector-commuting-zone level in Adão et al. (2019), versus country-county in our case.

To implement the procedure as in Adão et al. (2019), we replace ancestry in levels with ancestry shares. Formally, using predicted ancestry $\hat{A}_{o,d,t-1}$ from (2), equation (4) becomes

$$I_{o,d,t} = \delta_{o,r(d)} + \delta_{c(o),d} + \delta_t + X'_{o,d}\theta + b_t \cdot \left[\frac{\tilde{A}_{o,d,t-1}}{O_{d'}\tilde{A}_{o,d',t-1}} \times \tilde{I}_{o,-r(d),t}\right] + u_{o,d,t},$$

where the normalization $\tilde{A}_{o,d,t-1} = \hat{A}_{o,d,t-1} - \min\left[0, \min_{d'}\left[\hat{A}_{o,d',t-1}\right]\right]$ ensures predicted shares are in $[0, 1]$.

and run 1,000 placebo regressions of actual immigration on our randomly generated instrument. Column 1 reports the fraction for which we reject the null hypothesis of no effect at the 5 percent statistical significance threshold. We find a false rejection rate of 3.8 percent—close to the theoretical asymptotic 5 percent level, suggesting our inference based on conventional clustered standard errors is valid.

The remaining columns show that using predicted rather than realized ancestry to construct our instruments is key. Column 2 uses *realized* past immigration shares as in Card (2001). The false rejection rate of 27 percent is a sign of unreliable inference. Column 3 shows the same problematic result, a false rejection rate of 25 percent, when using realized ancestry shares.[17] The correlation between local economic factors and ancestry shares invalidates inference with standard shift-share instruments, but not with our predicted-ancestry instrument.

Realized versus Predicted Ancestry, State versus County Aggregation.—We explore in Supplemental Appendix Table 4 the difference in point estimates between using our instrument with predicted ancestry shares (column 1) versus a conventional shift-share instrument with realized ancestry shares (column 2). The estimated impact of immigration on innovation is similar using either instrument, despite the fact that the conventional shift-share instrument suffers from over rejection (Supplemental Appendix Table 2). The estimated impact of immigration on innovation is also similar when we aggregate our data at the US state level as in Hunt and Gauthier-Loiselle (2010), though the estimated standard error using our predicted ancestry instrument (column 3) is again larger than with a conventional shift-share instrument (column 4).

Alternative Instruments.—Supplemental Appendix Table 5 panel A explores the robustness of our main finding to alternative constructions of our instrument: column 1 replaces the push factor in (2), the number of migrants from foreign origin o excluding those who settle in the region where domestic county d is located, with the number of migrants from o excluding those who settle in counties with migrations that are serially correlated with those to d;[18] column 2 replaces the pull factor in (2), the share of European migrants who settle in d, with the share of migrants from a continent different than o's who settle in d; column 3 freezes predicted ancestry at its 1975 level in the construction of our instrument in (5); and column 4 uses non-European immigration until 1960 only to predict preexisting ancestry. Our estimate for β varies little across specifications. Panel B shows the same stability of the estimated impact of immigration on innovation measured as an elasticity, β_{IHS}, using the IHS-IHS specification in (7).

Additional Robustness Checks.—Supplemental Appendix Table 6 shows that our instrument separately identifies significant variation in contemporaneous immigration, even when controlling for lagged immigration shocks. It does so despite the

[17] Supplemental Appendix Table 3 presents additional statistics from the randomization test of Adão et al. (2019).

[18] For each pair of counties, we compute the correlation coefficient over time of total immigration (from all origin countries) and exclude from the push factor at t for county d all migrations to counties d' if their correlation with d is positive and statistically significant at the 5 percent level.

high level of serial correlation in immigration (Jaeger, Ruist, and Stuhler 2018). This pattern is comforting for our identifying assumption and is also consistent with our finding in Section IV that our immigration shocks are not confounded by persistent productivity shocks. Supplemental Appendix Table 7 shows our results are robust to weighting by the citation counts of patents to account for differences in patent quality. Supplemental Appendix Table 8 shows a permutation test, randomly reassigning the baseline instrument within various sets of observations. Across all permutation tests, we find no effect of this permuted instrument on immigration, no reduced-form impact of this permuted instrument on innovation, and the right hand side rejection rate is small (always below 5.2 percent). Supplemental Appendix Table 9 shows results similar to our baseline specification for the impact of population growth on innovation, using our immigration shocks in (5) as an instrument for population growth. Supplemental Appendix Table 10 shows our coefficient is stable to including "bad controls": population density in 1970, patents per capita in 1975, and the share of high school or college educated in 1970. Supplemental Appendix Table 11 shows our results are not driven by individual origin countries. In panel A we repeat the exercise in Table 3 removing one country at a time (for the five largest immigrant origin countries, Mexico, China, India, the Philippines, and Vietnam). In panel B, we use *only* migrants from each one of those five countries. The estimate for the impact of immigration on innovation varies little across samples.

Domestic versus Immigrant Innovators.—Supplemental Appendix Table 12 shows our results are not mechanically driven by incoming immigrant inventors. We define an inventor who files a patent in the United States as "domestic" if their first patent is filed with a US address and as "immigrant" otherwise. Doing so requires assigning patents to counties based on the location of inventors, rather than assignees as in our baseline specification. Column 1 shows this alternative assignment does not alter our baseline finding: our estimate of β in (1) is 0.098 (SE $=$ 0.038) compared to 0.122 (SE $=$ 0.045) in Table 3. Column 2 shows the vast majority of the effect of immigration on innovation, about 80 percent (0.079/0.098), comes from domestic inventors. Columns 3 and 4 show immigrant inventors and mixed teams of domestic and immigrant inventors contribute the remaining shares of the overall effect (5 percent and 10 percent respectively). So while a prolific literature has shown the crucial role foreign inventors play in US innovations (Akcigit, Grigsby, and Nicholas 2017; Arkolakis, Peters, and Lee 2020; Bernstein et al. 2018; Borjas and Doran 2012; Hunt and Gauthier-Loiselle 2010; Kerr and Lincoln 2010; Khanna and Lee 2018; Moser, Voena, and Waldinger 2014; Moser, Parsa, and San 2021; Stuen, Mobarak, and Maskus 2012), we find the aggregate (county-level) response to immigration shocks primarily comes from domestic innovators. Although the contribution of patent flows from immigrant and domestic-immigrant inventor teams induced by immigration is large relative to their overall share of total US patents (1 percent and 4 percent, respectively), it constitutes a small share of the aggregate response.[19]

[19] In Supplemental Appendix Table 13, we further limit to patents in which all domestic inventors have a patent filed in the United States prior to period t (column 3) and further to those with a prior patent in the same county (column 4). The impact of immigration on patenting is similar whether we include innovators who move (column 3) or exclude them (column 4), suggesting our results are not driven by an influx of domestic innovators.

This stylized fact motivates our modeling choice in Section IV, where the impact of immigration on innovation is driven by scale effects in the production of ideas, and where we abstract from differentiation in foreign-born versus native inventors.

Spatial Spillovers.—Supplemental Appendix Table 14 shows positive spatial spillovers of immigration on innovation (panel A) and wage growth (panel B). We consider three concepts of geographic spillovers, and construct distinct instruments for each: within-state spillovers; spillovers that decay smoothly with distance;[20] and immigration within 100 km (60 miles), between 100 km and 250 km (150 miles), between 250 km and 500 km (300 miles), and beyond 500 km. In panel A column 1, we first show that the effect of immigration on innovation is similar with state fixed effects (0.122, SE = 0.045, our baseline in Table 3) and census division fixed effect (0.137, SE = 0.048). Immigration within county d's state, excluding d, has a positive impact on local innovation in d (column 2). Immigration to neighboring counties (inversely weighted by distance) also has a positive impact (column 3). And a one standard deviation increase in immigration within 100 km of d doubles innovation in d relative to the mean; but immigration beyond 100 km has no impact (columns 4). We successfully identify independent variations in immigration at each level of aggregation, one of the strengths of our identification method.[21]

The spatial spillovers for wage growth in panel B are similar.

Interestingly, we also show in Supplemental Appendix Table 16 that immigration causes an inflow of natives (both Whites and non-Whites). This suggests that natives endogenously respond to the anticipated positive impact of immigration on innovation and wages. Our structural model in Section IV features such endogenous internal migrations driven by expected wage differentials.

Dynamism.—Supplemental Appendix Table 17 shows immigration also increases dynamism—the job creation and destruction rates, and the skewness of the job growth rate across sectors—consistent with endogenous growth theories with Schumpeterian creative destruction linked to innovation (Aghion and Howitt 1992; Grossman and Helpman 1991; Klette and Kortum 2004).

IV. Structural Model and Estimation

We interpret our main finding that immigration shocks have a positive causal effect on local innovation and wages at the five-year horizon through the lens of a quantitative regional equilibrium model of endogenous growth and migrations.

A. *Model*

There are O countries and D counties. Upon arriving in the United States, migrants form rational expectations and endogenously select a destination to maximize their

[20] We sum immigration shocks to all counties other than d, inversely weighted by their great circle distances from d computed from county centroids using the census mapping files for county geographies.

[21] Supplemental Appendix Table 15 displays the corresponding first-stage regression results. For all specifications involving multiple endogenous variables, we use the Angrist and Pischke (2009, p. 217–218) first-stage F-statistic, separately testing for each regressor the null of weak identification.

one-period ahead utility, just as natives choose where to live. Time t is discrete, and corresponds to the five-year intervals in our data. Each county produces a nationally traded final good (Y) and patents/ideas (Q).

Goods Production.—The final good $Y_{d,t}$, with price normalized to 1, is produced by a representative firm in county d at time t with technology

$$Y_{d,t} = Z_{d,t} Q_{d,t} L_{Y,d,t}^{\alpha}, \tag{8}$$

where $Q_{d,t}$ is the number (stock) of patents/ideas used in production, $L_{Y,d,t}$ is labor used for production, and $\alpha \in (0,1)$ is the elasticity of output to production labor. $Z_{d,t}$ is a stationary exogenous total factor productivity shock and evolves according to $\ln Z_{d,t} = \rho \ln Z_{d,t-1} + \epsilon_{d,t}$, with autocorrelation $\rho \in (0,1)$ and normally distributed innovations $\epsilon_{d,t} \sim \mathcal{N}(0, \sigma_{\epsilon}^2)$.

Ideas Production.—The stock of patents/ideas in d evolves cumulatively, $Q_{d,t} = Q_{d,t-1} + N_{d,t}$, where $N_{d,t}$ new patents are produced by combining research labor and existing patents,

$$N_{d,t} = L_{N,d,t}^{\gamma} Q_{d,t-1}^{1-\gamma}. \tag{9}$$

$L_{N,d,t}$ is labor used for creating new patents, and $\gamma \in (0,1)$ is our key parameter: the elasticity of innovation to research labor. $N_{d,t}$, the flow of new ideas, corresponds to the total number of patents issued in d at t. The structure of the innovation production function in (9) places our model within a broadly defined class of semi-endogenous growth models (Jones 1995): diminishing returns to past ideas in innovation require that the supply of researchers increases over time to generate sustained growth. In this class of models, the supply of researchers, which can shift through immigration shocks, is the key driver of innovation.

Firms.—The markets for goods and patents are competitive, with price-taking firms. The final goods firm in d combines new patents N and production labor L_Y to maximize profits,

$$\max_{N, L_Y} Z_{d,t} (N + Q_{d,t-1}) L_Y^{\alpha} - W_{d,t} L_Y - p_{d,t} N, \tag{10}$$

while the research firm optimally chooses research labor inputs L_N to maximize

$$\max_{L_N} p_{d,t} L_N^{\gamma} Q_{d,t-1}^{1-\gamma} - W_{d,t} L_N. \tag{11}$$

The local wage $W_{d,t}$ and the price of a local patent $p_{d,t}$, determined in equilibrium, are taken as given by both types of firms. The research firm gains ownership of the patents it produces for a single period. In the next period, patents expire and become a public good for other firms in the county.[22] This simplifying assumption ensures

[22] We show in Section IIID that our results are similar if we allow patents to fully diffuse nationally.

the research firm makes only static decisions, increasing tractability despite the rich underlying growth dynamics.

Population and Immigration.—In each county d, a mass $L_{d,t}$ of current residents each supplies one unit of labor to their local labor market alone. The local labor force evolves as foreign and domestic migrants arrive and leave, $L_{d,t+1} = (1-\mu)L_{d,t} + \sum_{o=1}^{O} I_{o,d,t} + \sum_{d'=1}^{D} M_{d',d,t}$, where $I_{o,d,t}$ is the number of foreign immigrants from country o who settle in county d at time t, and $M_{d',d,t}$ is the number of domestic movers from d' to d at t. Only a fraction μ of domestic residents are given a chance to move any period, so the total gross outflow of domestic movers from d' is $\mu L_{d,t}$. The total number of migrants from origin o to the United States, $I_{o\cdot,,t} = \sum_{d=1}^{D} I_{o,d,t}$, grows at a rate n, $I_{o\cdot,,t} = (1+n)^t \exp(\nu_{o,t})$, subject to log-normally distributed exogenous shocks, $\nu_{o,t} \sim \mathcal{N}(0, \sigma_\nu)$. These correspond to the origin-specific "push" shocks in our empirical analysis. Note the one-period "time to migrate," which mimics the way we construct our data.

Upon their arrival in the United States at t, migrant i from o forms rational expectations about wages and ancestry compositions and settles in destination d where they derive the highest utility,

$$d = \arg\max_{k} \mathbb{E}_t \left[W_{k,t+1}^{\lambda} \left(\frac{A_{o,k,t+1}}{A_{o,\cdot,t+1}} \right)^{1-\lambda} \right] \exp(-\tau_{o,k,t})\, \eta_{k,t}(i), \tag{12}$$

where $W_{k,t+1}$ is the future wage in destination k, $A_{o,k,t+1}/A_{o,\cdot,t+1}$ is the ancestry share from origin o who will reside in k at $t+1$ (with $A_{o,t+1} = \sum_{d=1}^{D} A_{o,d,t+1}$), $\eta_{k,t}(i)$ are i.i.d. extreme-value distributed preference shocks with dispersion parameter θ,[23] and $\tau_{o,k,t} \sim \mathcal{N}(0, \sigma_\tau^2)$ are i.i.d. normal shocks to bilateral migration costs from o to k.

The stock of residents in d with ancestry from o evolves recursively as migrants and domestic residents with ancestry from o arrive and leave, $A_{o,d,t+1} = (1-\mu)A_{o,d,t} + I_{o,d,t} + \sum_{d'=1}^{D} M_{o,d',d,t}$, where $M_{o,d',d,t}$ are domestic residents with ancestry o who move from d' to d at t (with $M_{d',d,t} = \sum_{o} M_{o,d',d,t}$). Other things equal, migrant i is more likely to settle in d if they expect a high real wage there $(W_{d,t})$[24] and expect d to host a large community of common ancestry $(A_{o,d,t+1}/A_{o,\cdot,t+1})$, if the bilateral migration cost is low $(\tau_{o,d,t})$, and if they draw a high taste shock $(\eta_{d,t}(i))$. $\lambda \in [0,1]$ governs the relative importance of economic versus social factors in this decision.

Assuming a continuum of migrants, the share of migrants from o who chose destination d is

$$I_{o,d,t} = I_{o,\cdot,t} \frac{\exp(-\theta\tau_{o,d,t}) \left(\mathbb{E}_t \left[W_{d,t+1}^{\lambda} \left(\frac{A_{o,d,t+1}}{A_{o,\cdot,t+1}} \right)^{1-\lambda} \right] \right)^{\theta}}{\sum_{k=1}^{D} \exp(-\theta\tau_{o,k,t}) \left(\mathbb{E}_t \left[W_{k,t+1}^{\lambda} \left(\frac{A_{o,k,t+1}}{A_{o,\cdot,t+1}} \right)^{1-\lambda} \right] \right)^{\theta}}. \tag{13}$$

[23] The idiosyncratic taste shocks are distributed Frechet, with $\Pr[\eta_{k,t}(i) \leq \eta] = \exp(-\eta^{-\theta}), \forall i,k,t$.

[24] Given that goods are freely traded on a national US market, wages in units of the numeraire are real wages, and directly comparable across locations.

This expression intuitively links to our reduced-form migration model in Section II. There is a large inflow of migrants from o to d if (i) many migrants from o arrive in the United Sates, $I_{o,\cdot,t}$ large (a "push" factor), (ii) d offers a high expected wage, $W_{d,t+1}$ high, (iii) the migration cost from o to d is low, $\tau_{o,d,t}$ low ("economic pull"-factors, potentially giving rise to the *reverse causality* and *county-country omitted factors* concerns described above), and (iv) there is a large expected group with ancestry from o in d, $A_{o,d,t+1}/A_{o,\cdot,t+1}$ large (a "social pull" factor as in Card 2001).

Domestic resident j from d' of ancestry o, when given an i.i.d. chance to move with probability μ, makes a similar internal migration decision, choosing optimally where to settle,

$$d = \arg\max_{k} \mathbb{E}_t \left[W_{k,t+1}^{\lambda} \left(\frac{A_{o,k,t+1}}{A_{o,\cdot,t+1}} \right)^{1-\lambda} \right] \tilde{\eta}_{k,t}(j), \tag{14}$$

where $\tilde{\eta}_{k,t}(j)$ is again an i.i.d. extreme value distributed shock with dispersion parameter θ. The number of residents with ancestry o who move from d' to d at t is,[25]

$$M_{o,d',d,t} = \mu A_{o,d',t} \frac{\left(\mathbb{E}_t \left[W_{d,t+1}^{\lambda} \left(\frac{A_{o,d,t+1}}{A_{o,\cdot,t+1}} \right)^{1-\lambda} \right] \right)^{\theta}}{\sum_{k=1}^{D} \left(\mathbb{E}_t \left[W_{k,t+1}^{\lambda} \left(\frac{A_{o,k,t+1}}{A_{o,\cdot,t+1}} \right)^{1-\lambda} \right] \right)^{\theta}}. \tag{15}$$

Local labor is allocated to goods and new ideas production, $L_{d,t} = L_{Y,d,t} + L_{N,d,t}$. Given our convenient timing assumptions for patent ownership and migrations, migrants and residents make no dynamic decisions, so we do not model household preferences further.

B. *Equilibrium and Estimation*

We characterize the deterministic balanced growth path equilibrium analytically and numerically solve for dynamics off the balanced growth path with each period equal to five years as in our data. See Supplemental Appendix B.1 for details and a formal definition of the equilibrium.

Equilibrium Properties.—Figure 1 displays theoretical impulse response functions to a temporary, exogenous, inflow of migrants arriving in a given destination (top-left panel), for different values of γ: our baseline estimate $\gamma = 0.781$ (solid line) and a lower value $\gamma = 0.5$ (dashed line). In particular, the endogenous responses in Figure 1 depict deviations of local outcomes from the steady state balanced growth path within any region d after a one standard deviation positive shock $\nu_{o,t}$ to the supply of immigrants from any origin o, a shock which has symmetric effects across our ex ante homogeneous regions starting from the steady state.

The influx of migrants mechanically increases the local labor force (top-right panel). Due to the cumulative impact of immigration, with past immigrant enclaves

[25] Note that since we do not assume any county-specific internal migration frictions, all domestic migrants make similar choices on average. The share of domestic migrants from origin county d' of ancestry o who settle in d is the same as for any other origin county d'': $M_{o,d',d,t}/(\mu A_{o,d',t}) = M_{o,d'',d,t}/(\mu A_{o,d'',t}), \forall (d', d'')$.

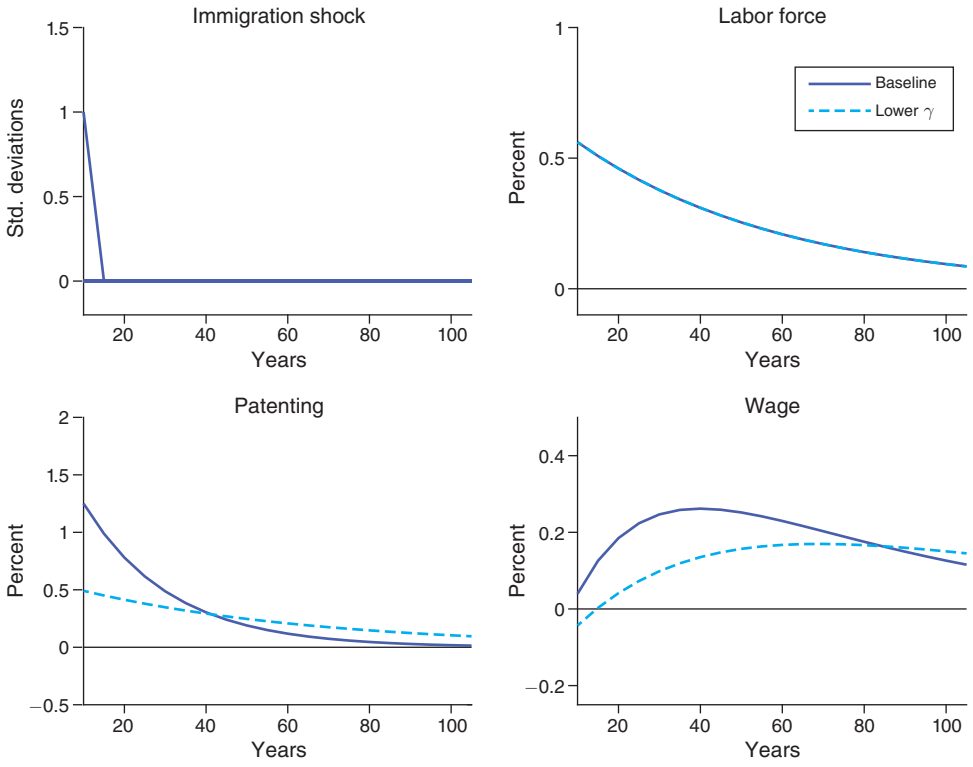

FIGURE 1. THE IMPACT OF AN IMMIGRATION SHOCK

Notes: The figure plots impulse response functions to a one-standard deviation immigration shock in period 1. The top left panel plots the immigration shock $\nu_{o,t}$. The top right panel plots the labor force $l_{d,t}$. The bottom left panel plots patenting $n_{d,t}$. The bottom right panel plots the response of the wage $w_{d,t}$. The immigration shock is from a single origin o, and the responses of the labor force, patenting, and the wage are local responses for a county d. The labor force, patenting, and wage responses are in percentage point deviations from the balanced growth path. The baseline impacts are in solid blue, while the lighter dashed line lowers the parameter γ from its baseline of 0.781 to 0.5.

attracting future migrants, and the sluggish nature of deterministic population growth, this increase is persistent. Those additional workers expand the research sector, and patenting increases for multiple periods (bottom-left panel). The key parameter governing the strength of the innovation response to immigration is the local elasticity of innovation to research labor, γ. The smaller γ (dashed versus solid lines), the weaker the impact of immigration on innovation. This motivates our choice to use the reduced-form causal impact of immigration on innovation to identify γ.

Immigration has two competing effects on wages (bottom-right panel): on the one hand, the increased abundance of local labor exerts a downward pressure on wages, a negative neoclassical labor-supply channel; on the other hand, higher local innovations increase the marginal product of labor and wages, a positive endogenous-growth channel. At our estimated parameters (solid line), the positive effect dominates at the five-year horizon, consistent with our empirical results. For lower values of γ (dashed line), wages initially decline due to the increased labor supply, but eventually increase as innovations accumulate.

This simple intuition may explain the mixed results in the empirical labor literature on immigration and wages (Borjas 2003; Cortes 2008; Ottaviano and Peri 2012; Foged and Peri 2016; Dustmann, Schönberg, and Stuhler 2017; Monras 2020; Jaeger, Ruist, and Stuhler 2018; Bratsberg et al. 2019): whether the negative neoclassical impact on wages of a labor supply increase, or the positive endogeneous growth effect dominates, depends on the size of local scale effects and the time horizon.[26]

Estimation Results.—We use a simulated method of moments (indirect inference) to estimate the model. In addition to a range of conventional moments, including the volatility and persistence of immigration, output, and patent flows, we target the IV estimate of the elasticity of innovation to immigration, β_{IHS} (column 4 in Table 3). We use the same identification strategy to construct an instrument for immigration in our simulated model as in the data. In particular, we first unconditionally simulate a panel dataset at the county level over $T = 1{,}000$ periods for $O = 10$ foreign regions and $D = 9$ domestic regions. Then we construct the variable $\hat{I}_{d,t}$ in the simulated data and estimate specification (7) with $IHS\left(\hat{I}_{d,t}\right)$ as our instrument. Note that (7) contains time effects, so our target IV coefficient β_{IHS} is identified off of relative variation across regions with different shock histories.

Table 6 presents results from our structural estimation, including point estimates, standard errors, and the model's fit.[27] We match our target moments well (panel A). In particular, the elasticity of innovation to immigration β_{IHS} is close in our simulated model and in the data (1.641 versus 1.652).[28] Our structural estimate for the local elasticity of patenting to research labor ($\gamma = 0.781$, SE $= 0.086$ in panel B) lies at the upper end of the range of values typically used in calibrated models of aggregate endogenous growth and firm innovation (Acemoglu et al. 2018; Bloom et al. 2021; Akcigit, Pearce, and Prato 2020; Blundell, Griffith, and Windmeijer 2002; Terry, Whited, and Zakolyukina 2020).

Table 6 also reports our estimates of other parameters, which are intuitive. We estimate highly persistent local productivity processes with an annual autocorrelation of $\rho^{1/5} = 0.971$, matching the high persistence of GDP and patenting at the local level. This high persistence of local shocks represents a major threat to identification of conventional shift-share instruments, even when they rely on ancestry or immigration shares from the distant past. We also estimate that shocks to county-level productivity are less volatile ($\sigma_\epsilon = 0.020$) than shocks to origin-level immigration flows ($\sigma_\nu = 0.595$) or bilateral immigration costs ($\sigma_\tau = 0.520$), matching the relative volatilities of immigration at the county, origin, and county-origin levels.

[26] This intuition is complementary to Jaeger, Ruist, and Stuhler (2018), where the gradual inflow of capital in response to a labor shock plays a role similar to endogenous innovation in our model. The key qualitative difference is that capital accumulation induces a temporary rise in real income and a subsequent decrease, while the innovation response in our model yields permanent gains.

[27] We estimate five parameters $(\gamma, \rho, \sigma_\epsilon, \sigma_\nu, \sigma_\tau)$, targeting six moments (our IV coefficient β_{IHS}, the SD of origin immigration $I_{o,\cdot,t}$, the SD of destination immigration $I_{d,t}$, the SD of origin-destination immigration $I_{o,d,t}$, the autocorrelation of output per capita, the autocorrelation of patenting). We set the population growth rate to $n = 2\%$, the labor elasticity in production $\alpha = 0.8$ to match average markups of 20 percent, $\lambda\theta = 0.5$ from Caliendo et al. (2019) with $\lambda = 0.5$ and $\theta = 1$, and the five-year mobility shock $\mu \approx 0.25$ to match the annual county-to-county gross mobility rate of 5 percent within the United States. Further technical details are in Supplemental Appendix B.2.

[28] In Supplemental Appendix Table 20 we provide a separate exhibit with our IV estimation of the target coefficient β_{IHS} from (7) using model simulated data.

TABLE 6—PARAMETERS AND MODEL FIT

Panel A. Moments	Data	Model
IV coeff., $patenting_{d,t}$ on immigration $I_{d,t}$	1.6519	1.6410
	(0.1500)	
Standard deviation, o immigration $I_{o,t}$	0.4061	0.3975
	(0.0284)	
Standard deviation d immigration $I_{d,t}$	0.1794	0.1655
	(0.0110)	
Standard deviation, o-d immigration $I_{o,d,t}$	0.0716	0.1138
	(0.0117)	
Autocorrelation, output per capita $Y_{d,t}/L_{d,t}$	0.9611	0.9646
	(0.0057)	
Autocorrelation, $patenting_{d,t}$	0.9309	0.8925
	(0.0065)	
Panel B. Estimated parameters	Symbol	Value
Elasticity, patenting to labor	γ	0.7807
		(0.0857)
Autocorrelation, county TFP	ρ	0.8631
		(0.0230)
Standard deviation, county TFP shocks	σ_{ϵ}	0.0203
		(0.0090)
Standard deviation, immigration push shocks	σ_{ν}	0.5951
		(0.0793)
Standard deviation, bilateral immigration shocks	σ_{τ}	0.5200
		(0.0707)

Notes: The top panel A reports targeted data moments versussimulated model moments. The bottom panel B reports the estimated parameters. The standard errors, in parentheses beneath moments and estimates, are clustered by state.

The Dynamics of Innovation and Wages.—Figure 2 shows the elasticity of patenting to research labor allows us to qualitatively match the *untargeted* dynamic response of both innovation and wages to a local immigration shock at longer horizons. The top panel A shows the impulse response of innovation and wages to a one period inflow of migrants in our model. The bottom panel B shows the response of patenting and wages to an exogenous immigration shock over different horizons (5, 10, and 15 years), controlling for intermediate immigration shocks. The elasticity of wages to immigration is approximately equal to 0.13.[29] In both model and data, immigration has a positive and persistent impact on local innovation and wages, even though we only target the contemporaneous response of innovation, and we do not target the wage response at all.

C. *Quantification of the Aggregate Impact of Immigration*

We compute a simple aggregate counterfactual to gain intuition for the magnitudes involved. We simulate the trajectory the United States might have followed had the Immigration and Nationality Act of 1965 (INA) not been passed. This act lifted many immigration restrictions. Comparing a high realized migration path with

[29] Supplemental Appendix Table 18 shows the corresponding estimation results.

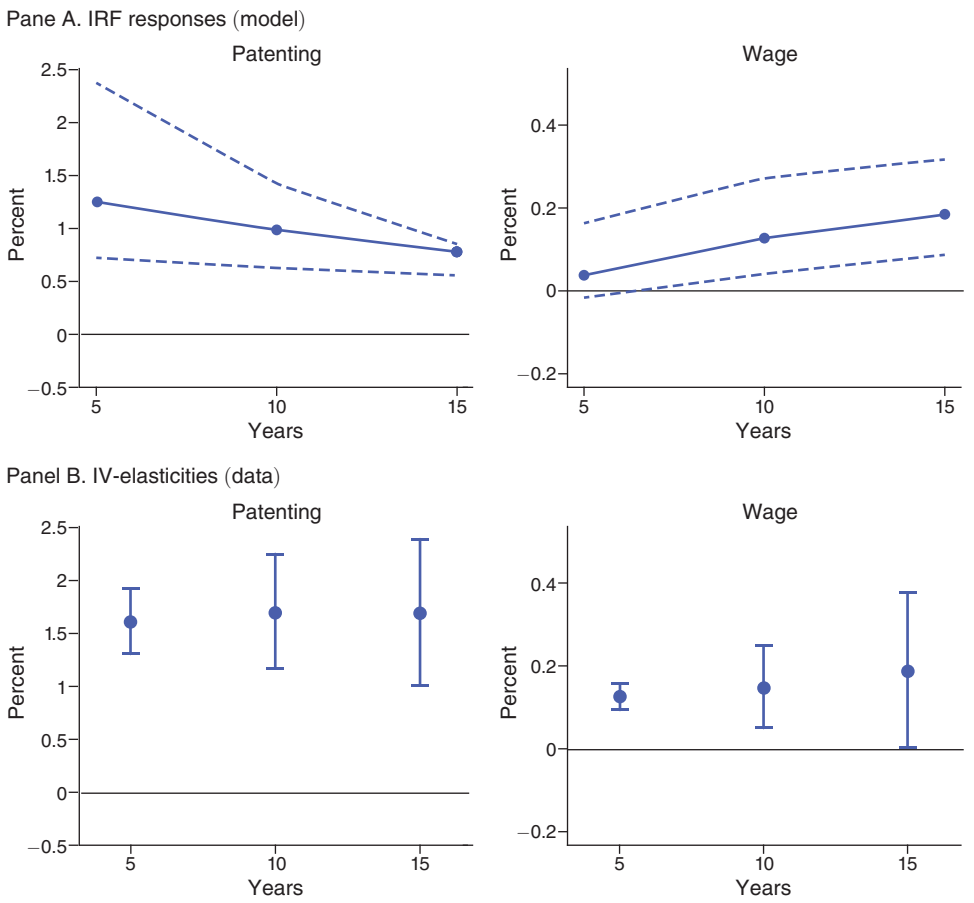

FIGURE 2. IMMIGRATION, INNOVATION, AND WAGES: MODEL VERSUS DATA

Notes: Panel A plots model-estimated impulse response functions for patenting (left figure) and wages (right figure) to a one-standard deviation immigration shock in period 1 (with dashed lines representing standard error boundaries associated with γ). Panel B displays the results of estimating equation (1), where the endogenous variable is the inverse hyperbolic (IHS) of non-European immigration (1,000s) to county d at time t and the dependent variable is the IHS of 5-year patent flows per 100,000 people (left figure) or IHS of wages (right figure). The figures plot the coefficient estimate and 95 percent confidence intervals on the endogenous variable for separate regressions where the outcome is measured in period t, $t+1$, and $t+2$ (for the latter two regressions, we include controls for the immigration shock in $t+1$ and the immigration shocks in $t+1$ and $t+2$, respectively).

the INA, to a counterfactual lower migration path without, we offer suggestive evidence on the quantitative impact of immigration on innovation and growth.

To simulate the dynamics of the economy in a hypothetical world where the INA would not have passed, we feed negative immigration shocks each period $(\nu's)$ such that the total population growth rate is 16 percent lower than in our calibrated model. This 16 percent reduction is computed to approximate the lower contribution of immigrants to US population growth over 1860–1960, before the INA, compared to 1970–2010, after the INA (see Supplemental Appendix B.3 for details).[30]

[30] We do not claim to have quantified the causal impact of the INA on immigration. This exercise is solely meant as an illustration of the quantitative magnitudes in our model. We use demographic data only to get a plausible magnitude for reduced immigration in a hypothetical world without the INA.

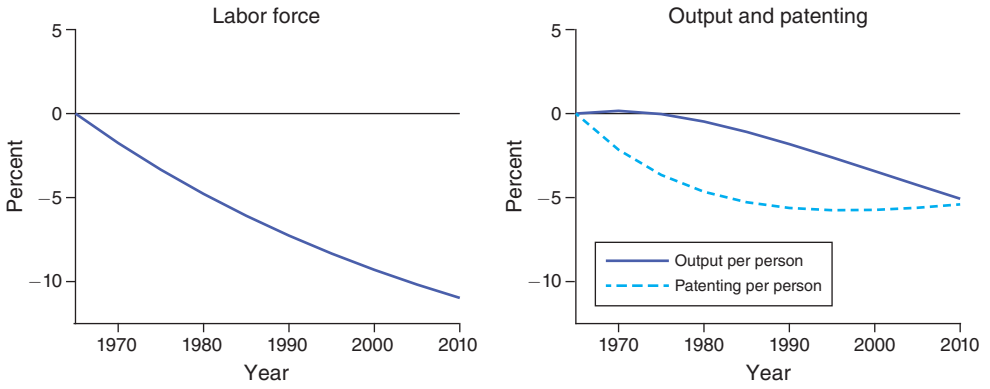

FIGURE 3. REMOVING THE POST-INA IMMIGRATION INCREASE

Notes: The figure plots the simulated counterfactual impact to US economic outcomes from removing the increase in US population growth due to the foreign born empirically observed after the Immigration and Nationality Act of 1965 (INA). Each panel plots the percent deviation of the indicated outcomes from the path of the economy without removal of the post-INA immigration contributions.

The resulting cumulative deviations of macroeconomic aggregates from the balanced growth path are presented in Figure 3. The left panel shows the cumulative reduction in the labor force. Our estimates suggest this counterfactual reduction in immigration would have caused a sharp reduction in patenting per capita, reaching a 6 percent drop by 1990 (right panel, dotted line). Interestingly, the impact on output per capita is close to zero until 1980, as the large stock of patents inherited from the pre-INA period, and the reallocation of labor away from innovation due to the reduction in immigration, allow for more goods production; eventually, as the stock of innovation falls, output per capita falls by 5 percent by 2010 (right panel, solid blue line).

This estimated impact, about 5 percent lower output per capita over 45 years, lies within the range of recent quantitative estimates in the endogenous growth literature. For example, the recent decline in *total* US population growth contributes about 19 percent over 45 years (Peters and Walsh 2021), increased growth from trade generates 45-year gains of around 7 percent (Sampson 2016), short-termist incentives on US managers cost around 2 percent over 45 years (Terry 2023), and stronger US antitrust policy generate 45-year gains of around 4 percent (Cavenaile, Celik, and Tian 2023). The ratio of the cumulative population effects to cumulative output per capita effects, a bit more than 2 to 1 in our analysis, also roughly matches a similar exercise in an earlier historical period in Arkolakis, Peters, and Lee (2020).

D. *Identification*

Our structural model allows us to compare our reduced-form identification strategy (Section II) to the seminal identification strategy proposed by Card (2001), which predicts contemporaneous immigration shocks by interacting immigration shifters with past immigration shares,

$$\hat{I}^{Card}_{\cdot,d,t} = \sum_{o=1}^{O} I_{o,\cdot,t} \frac{I_{o,d,t-1}}{I_{o,\cdot,t-1}}. \tag{16}$$

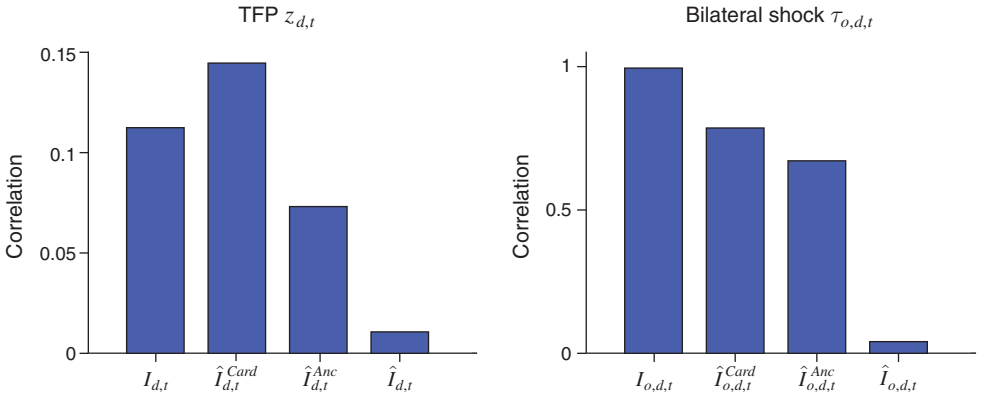

FIGURE 4. TFP, BILATERAL SHOCKS, AND INSTRUMENTAL VARIABLES

Notes: The figure reports correlations in simulated model data linking observable outcomes of interest to underlying model shock processes. In each panel, the observable data is given by the inverse hyperbolic sine of total immigration I, the Card IV $\hat{I}^{Card}$, a version of our baseline IV with realized rather than predicted ancestry shares $\hat{I}^{Anc}$, and our baseline IV $\hat{I}$. The left panel reports correlations from data aggregated to the destination county d by time t level with the log county-level TFP shock $z_{d,t}$. The right panel reports correlations from data disaggregated to the origin country o by destination county d by time t level with (minus) the bilateral immigration shock $-\tau_{o,d,t}$.

Applying our endogenous migration model (13) at time $t-1$, we see that past immigration shares $I_{o,d,t-1}/I_{o,\cdot,t-1}$, and hence predicted immigration shocks in d themselves, $\hat{I}^{Card}_{\cdot,d,t}$, are driven by period $t-1$ expectations of future wages and ancestry terms in destination d. But a positive productivity shock, an increase in $Z_{d,t-1}$ in (8), increases not just contemporaneous wages, $W_{d,t-1}$, but also expected future wages, $W_{d,t}$, because of the persistence in productivity. The same reverse causality logic also applies to innovations: a positive productivity shock, $Z_{d,t-1}$ higher, attracts migrants and triggers innovations, which in turn increases future innovation through the production function (9). The "*Card* instrument" in (16) is thus contaminated by the (simple) *reverse causality* effect of wages and innovation on immigration due to persistent productivity shocks.[31] Our identification strategy instead constructs a set of instruments for ancestry, $\hat{I}_{\cdot,d,t} = \sum_{o=1}^{O} \hat{b}_t \left[\hat{A}_{o,d,t-1} \times \tilde{I}_{o,leave-out,t}\right]$ in (5), which isolate *relative* variations in predicted local ancestry that result exclusively from the coincidental timing of historical push and pull factors—purging the effect of persistent productivity shocks from our immigration shocks.

Figure 4 (left panel) explicitly shows the correlation structure between productivity shocks ($Z_{d,t}$), realized immigration ($I_{\cdot,d,t}$), the *Card* predicted immigration shock ($\hat{I}^{Card}_{\cdot,d,t}$), and our proposed predicted immigration shock ($\hat{I}_{\cdot,d,t}$). For reference, the figure also includes an intermediate "Ancestry" version of our instrument $\hat{I}^{Anc}_{\cdot,d,t}$ constructed by replacing predicted ancestry $\hat{A}_{o,d,t-1}$ with realized ancestry. As expected, because productivity is persistent, realized immigration is correlated with contemporaneous productivity shocks. The "Card" instrument, because it relies on past immigration shares, themselves correlated with persistent past productivity shocks,

[31] As noted earlier, David Card explicitly notes that past immigration shares may be correlated with persistent productivity shocks (Card 2001, p. 43), potentially creating a spurious correlation with contemporaneous wages.

is also correlated with contemporaneous productivity shocks. Using realized ancestry, the "Ancestry" instrument, alleviates this correlation somewhat but not fully. Our predicted immigration shocks $\hat{I}_{\cdot,d,t}$, which exploit only the historically predicted, relative component of ancestry and include a rich leave-out structure, are not correlated at all with local productivity shocks.

The right panel shows our identification strategy is also immune to the more elaborate identification concern regarding *county-country specific omitted factors*. If specific migrants (say Indian engineers) have skills suited for certain industries and destinations (say IT in Silicon Valley), then any shock to the cost of migration correlated with TFP (say $\tau_{India,SantaClara,t}$ correlated with $Z_{SantaClara,t}$) would induce a spurious correlation between innovation, immigration, and ancestry. Figure 4 (right panel) confirms that realized bilateral migration flows ($I_{o,d,t}$), "Card" predicted bilateral migration flows ($\hat{I}^{Card}_{o,d,t}$), and crude "Ancestry" instruments' predicted migration flows ($\hat{I}^{Anc}_{o,d,t}$) are all correlated with the model's underlying bilateral migration cost shocks ($\tau_{o,d,t}$). By contrast, our predicted migration flows ($\hat{I}_{o,d,t}$) are not.

To summarize, within a quantitative model of endogenous growth and migration, our predicted immigration shocks are orthogonal to two variables that would raise endogeneity concerns (persistent productivity and bilateral migration costs). This bolsters our confidence that our identification strategy is well suited to identify the causal impact of immigration on innovation.

E. *Robustness*

We conclude with an exploration of the robustness of our results to various extensions.

Local versus Global Idea Spillovers.—In our baseline model, local innovations only depend on local research labor and the local stock of ideas. Our empirical results suggest some degree of spatial diffusion. Our results are robust to allowing for spatial spillovers: going to an extreme case of full (national) spillovers in ideas after one period, the estimated local innovation elasticity γ in Supplemental Appendix Table 19 changes little and remains around 0.8. Intuitively, spillovers affect the slow-moving stock of ideas, but have little impact on the short-run response of local innovation to immigration shocks (see Supplemental Appendix Figure 5). Spatial spillovers thus do not affect our quantification of the aggregate response to a national symmetric immigration shock. They affect instead the distribution of innovation and wage growth across regions in response to asymmetric shocks, which we do not explicitly study here.[32]

[32] Note that we employ an effective local idea aggregator in Supplemental Appendix Subsection B.1 in our spillover specification of the model which is based on the average of idea stocks across all regions. So our impulse responses starting from the balanced growth path in Supplemental Appendix Figure 5 to an immigration supply shock $\nu_{o,t}$ which is symmetric across regions differs little from the no spillover case. One could imagine that an alternative specification with local idea aggregators based on the *sum* of idea stocks across regions might yield a larger response in the spillovers version of the model. But such an alternative model would need to be calibrated to match the same steady state balanced growth path per capita growth rates, which would quantitatively limit this force.

Decreasing Returns to Labor.—The parameter governing the degree of decreasing returns to scale in production, α, has both a literal labor elasticity role as well as, in some alternative interpretations of the model, a role in governing implied markups. In our baseline we choose $\alpha = 1/1.2 \approx 0.8$ to match a 20 percent implied markup. Supplemental Appendix Figure 6 shows that the impact of an immigration shock is robust to alternative choices for α over the wide range 0.7 to 0.95. The impact of an immigration shock on innovation remains strong in each case.

Constant versus Decreasing Returns in Research.—Our model is a semi-endogenous growth model in the sense that per capita income growth is proportional to population growth in steady state. In our baseline we chose a particular technology for the research sector with constant returns to scale $\left(N_{d,t} = L_{N,d,t}^{\gamma} Q_{d,t-1}^{1-\gamma}\right)$ implying an elasticity of innovation to past ideas of $1 - \gamma \approx 0.2$. We explore alternative specifications for the production function of new ideas ($N_{d,t} = L_{N,d,t}^{\gamma} Q_{d,t-1}^{\zeta}$ allowing $\gamma + \zeta \neq 1$), set ζ to 0.1 and 0.3, evenly spaced around our baseline of $\zeta \approx 0.2$, and reestimate the model in each case to recover γ. The estimated returns to scale to innovation, $\gamma + \zeta$, are near unity in each specification.[33] Supplemental Appendix Figure 7 shows the impact of an immigration shock across specifications. A higher elasticity ζ to the slow-moving idea stock unsurprisingly results in stronger long-run impacts while dampening short-run impacts. Over 45 years, the horizon of our INA analysis in Figure 3, the impact of immigration on innovation is similar across specifications.

V. Conclusion

The economic, social, political, and cultural changes immigrants bring to their host communities are the subject of fierce political controversies. Informing this debate with data has proven difficult, not only because different migrants may affect their host communities in different ways, but also due to an identification problem: immigrants likely choose to settle in host communities that offer the best prospects rather than at random. This generates endogenous correlations between past and present immigration, and local economic outcomes, making it difficult to isolate the causal effects of immigration.

We introduce a novel solution to this identification problem that allows for the construction of local immigration shocks—instruments for the total number of migrants arriving in each US county for each five-year period since 1975. Importantly, these immigration shocks remain valid even if migrations prior to 1975, and thus the county's preexisting ancestry composition, are endogenous to local economic activity, and can be flexibly disaggregated into different instruments for migrations from each origin country to each destination county in each period.

We use these instruments to show that, on average, immigration to the United States between 1975 and 2010 had a positive causal effect on local innovation and average wages of natives. For example, a 1 percent increase in immigration to a

[33] We estimate $\hat{\gamma} = 0.8250$ when $\zeta = 0.1$ and $\hat{\gamma} = 0.6995$ when $\zeta = 0.3$.

given county on average increases the number of patents filed by local residents by 1.6 percent and local wages by 0.13 percent, over a five-year period.

We interpret those empirical results through the lens of a structural model of endogenous migrations and innovation. To quantify this structural model, we target the reduced-form impact of immigration on innovation. This quantification exercise suggests that the elasticity of innovation with respect to research labor, 0.8, is relatively large, implying that labor supply shocks such as those brought by international migrations have strong scale effects on local innovation. This model also explicitly shows that while immigration unambiguously increases innovation, its effect on local wages varies over time: In the very short run, it is possible for a labor supply shock to depress wages, while the positive impact of higher innovation and labor productivity on wages gradually builds over time and becomes dominant.

Beyond our application to immigration, we believe our approach linking preexisting (ancestry) shares to the interaction of historical factors may prove useful in other applications of the canonical shift-share instrument. For example, the cumulative forces that lead to the establishment of migrants of different ethnicities across locations may be similar to the historical forces that generate variation in preexisting shares of industries, occupations, and other specializations across locations. Our procedure for isolating quasi-random variation in preexisting shares may thus prove useful in other settings that have studied the local effects of import competition, the local fiscal multiplier, local supply elasticities, and other important subjects.

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