SHIFT-SHARE INSTRUMENTS AND THE IMPACT OF IMMIGRATION

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November 2017

Acknowledgements: Jan Stuhler acknowledges funding from the Spanish Ministry of Economy and Competitiveness (MDM2014-0431 and ECO2014-55858-P), the Fundación Ramón Areces, and the Comunidad de Madrid (MadEco-CM S2015/HUM-3444). We thank Michael Amior, Andreas Beerli, George Borjas, Christian Dustmann, Anthony Edo, Jesús Fernández-Huertas Moraga, Tim Hatton, Jennifer Hunt, Joan Llull, Marco Manacorda, Simen Markussen, Joan Monras, Elie Murard, Barbara Petrongolo, Uta Schoenberg, JC Suarez Serrato and seminar and conference participants at the Banco de España, CERGE-EI, Collegio Carlo Alberto, the Frisch Centre in Oslo, Duke University, Gothenburg University, the Helsinki Center of Economic Research, IZA, the London School of Economics, Lund University, the Luxembourg Institute of Socio-Economic Research, the Milan Labor Lunch Series, the Norwegian School of Economics in Bergen, Queen Mary University, Royal Holloway University, Universidad Autonoma de Barcelona, Uppsala University, the University of Navarra, and the 2017 PSE-CEPII Workshop on Migration for comments.

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November 2017
JEL No. C36, J15, J21, J61

ABSTRACT

It has become standard empirical practice to exploit geographic variation in the location of immigrants to identify their impact. To address the endogeneity of immigrants' location choices, the most commonly-used instrument interacts national inflows by country of origin with their past geographic distribution. We present evidence that estimates based on this "shift-share" instrument are subject to bias from a conflation of short- and long-run responses to immigration shocks. If the adjustment process is gradual, subsequent inflows are likely to be correlated with the ongoing response to previous supply shocks. In addition, the spatial distribution of new immigrant flows is often highly stable over time, leading to a first stage that is "too strong." Estimates based on the conventional shift-share instrument are therefore unlikely to identify a causal effect. We propose a "double instrumentation" solution to the problem that — by isolating spatial variation that stems from changes in the country-of-origin composition on the national level — produces estimates that are likely to be less biased. Our results are a cautionary tale for a large body of empirical work, not just on immigration, that rely on shift-share instruments for causal identification.

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Studies of the impact of immigration are often based on spatial variation in immigrant inflows. In the hopes of addressing the endogeneity of the location choices of new immigrants with respect to local labor demand, inflows at an aggregate level are typically combined with the lagged geographic distribution of immigrants to create an instrument (Altonji and Card 1991, Card 2001). With dozens of publications in leading journals, the "past-settlement" instrument is a crucial component of the spatial correlation literature on immigration and has been used to identify supposedly exogenous labor supply shocks. It is also a prominent example of "shift-share" instruments with the same underlying rationale – combining local economic compositions with shifts on the aggregate level to predict variation in a variable of interest. In a quest for better identification, shift-share instruments have become popular in a wide range of literatures, introducing spatial or other forms of cross-sectional variation also to literatures that traditionally relied on time-series analysis.[1]

Despite a proliferation of studies, the past-settlement instrument has not resolved a long-standing dispute regarding the labor market effects of immigration or, more generally, how local labor markets adjust to supply shocks (see, for example, Borjas 2014 and Card and Peri forthcoming). Estimates of immigrants' impact on wages that rely only on the past-settlement instrument tend to be less negative than those from the factor proportions approach, or those that rely on natural quasi-experiments (see, for example, Aydemir and Kirdar 2014; Llull 2014; Dustmann, Schoenberg, and Stuhler 2017; and Monras 2015). Estimates from the spatial

[1] A classic reference is Bartik (1991), who combines the local industry composition with national changes in employment across industries to isolate local labor demand shock. Kovak (2013) interacts the local industry composition with tariff changes to examine the impact of trade reform. Autor, Dorn, and Hanson (2013) interact local industry shares with aggregate trade flows to examine the impact of Chinese imports on labor markets in the US. Shift-share instruments have also been used to isolate exogenous variation in local public spending (e.g. Nakamura and Steinsson 2012, Wilson 2012), foreign aid (Nunn and Qian 2014), credit supply (Greenstone, Mas, and Nguyen 2015), portfolio allocation (Calvet, Campbell, and Sodini 2009), market size (Acemoglu and Linn 2004), judge leniency (Kling 2006), import prices on the firm level (Smagghue and Piveteau 2015, de Roux et al 2017), automatization of routine tasks (Autor and Dorn 2013), and robotization (Graetz and Michaels 2015, Acemoglu and Restrepo 2017). See Goldsmith-Pinkham, Sorkin, and Swift (2017) for additional examples.

correlation approach also appear to be more variable (Dustmann, Schoenberg, and Stuhler 2016), changing sign even when applied to different time periods within the same country (Borjas 1999).

We suggest that these inconsistencies arise partly from the conflation of the short- and long-run responses to immigrant arrivals. The problem stems from the interplay of two factors. First, local shocks may trigger general equilibrium adjustments that gradually offset their local impact. The potentially adverse effect of a local supply shock may thus be followed by a period of positive wage growth. Second, the country of origin composition and settlement patterns of immigrants are correlated over time. These two factors together suggest that the spatial correlation approach may conflate the (presumably negative) short-run wage impact of recent immigrant inflows with the (presumably positive) movement towards equilibrium in response to previous immigrant supply shocks.

A concern in the existing literature is that general equilibrium adjustments occur too quickly, offsetting the (local) impact of immigrant arrivals before the measurement of wages and biasing spatial correlation estimates towards zero (Borjas, 1999, Borjas 2006, Cortes 2008). Our argument suggests, however, that such adjustments are also problematic if they occur slowly, causing the past settlement instrument to violate the necessary exogeneity assumption. This problem is difficult to address, and the resulting bias can dominate the short-term impact of current immigration, resulting in a sign reversal and a positive estimated effect of immigration on wages. We argue that the equilibrium adjustment process poses a problem for estimation of the labor market impact of immigration, regardless of its speed. By placing the past-settlement instrument in a theoretical framework, violations of the exogeneity of the instrument become clearer than in the "ad-hoc" implementations that are common in the literature.

We illustrate how use of the past-settlement instrument exacerbates potential biases using data from the U.S. Census and American Community Survey from 1960 to 2011. Because the country of origin mix of the inflow of immigrants to the U.S. is so similar over time, the correlation between the predicted decadal immigrant inflow rate across metropolitan areas and its lag is consistently high (between 0.96 and 0.99 since the 1980s) and even exceeds the corresponding correlation in actual inflows. As a consequence, the conventional instrumental variable approach captures not only the short-term impact, but also the longer-term adjustment process to previous inflows. The resulting estimates have no clear interpretation, because the respective weights on the short and long term vary across applications. The greatest strength of the instrument, its impressive ability to predict current flows, is thus potentially a weakness. In some sense, if the instrument is "too strong," it is difficult to believe that it is truly separating the exogenous component of immigrant inflows from the endogenous component.

Our results suggest, however, that periods with substantial changes in the country of origin composition may provide variation that can be exploited with a variant of the shift-share strategy. By instrumenting both current and past immigrant inflows with versions of the past-settlement instrument that vary only in their national components, we are able to isolate the variation in inflows that is uncorrelated with current local demand shocks as well as the process of adjustment to past supply shocks. This "double instrumentation" procedure places substantial demands on the data, as the consequences of current and past immigrant arrivals can be distinguished only if there is sufficient innovation in their composition at the national level. We show that in the U.S. the enactment of the Immigration and Nationality Act of 1965, which led to a large break in the country-of-origin composition of immigrants (Hatton 2015), provides sufficient changes in the sources of the immigrant flow to the U.S. to use our procedure. Innovations in the composition of immigrants make the 1970s therefore a particularly interesting case and similar compositional breaks are observed in other countries. Using the inflow of immigrants to the U.S. after 1980, in contrast, is not conducive to such analyses because there is little variation in the country-of-original composition.

We estimate that the initial impact of immigrants on natives' wage in the 1970s is more negative than estimates based on the conventional shift-share instrument would suggest. The estimated impact of the immigrant inflow from the 1960s on wage growth in the 1970s is positive, however, and in some specifications of similar magnitude as the negative impact of the 1970s inflow. Our results suggest that areas with large immigrant flows experience a temporary, but not persistent negative impact on the wages. The short-term response is consistent with a standard factor proportions model, in which an increase in the supply of one factor leads to a reduction of its price. The longer-term adjustment indicates strong but gradual general equilibrium responses.

A slow dynamic adjustment process poses a particular problem for the past-settlement instrument and the immigration literature, but in principle the issue is relevant for many other types of shift-share instruments that combine local "shares" and aggregate "shifts" to generate spatial variation. Local shares are often highly serially correlated, whether constructed from the composition of demographic groups, industries or other characteristics. For shift-share instruments to be valid requires that one of two conditions holds: either the national shifts are not serially correlated, or the variable of interest does not trigger dynamic adjustments in outcomes. In contexts where there are sudden shocks at the national level, shift-share instruments may meet the first condition. In others, like in the immigration literature, care must be taken to ensure that there is sufficient variation over time so that variants of the shift-share methodology, such as the one proposed here, can then be used to isolate variation that is uncorrelated with past shocks and permit a causal interpretation of the results.

I. Spatial Correlations and the Past-settlement Instrument

By number of publications, the spatial correlation approach is the dominant identification strategy in the immigration literature.[2] Its central identification issue is the selection problem: immigrants do not randomly sort into locations, but rather are attracted to areas with favorable demand conditions (Jaeger 2007). A simple comparison between high- and low-immigration areas may therefore yield a biased estimate of the impact of immigration. The problem is notoriously difficult to solve and arises even in those cases in which natural quasi-experiments generate exogenous variation in immigrant inflows at the national level.

To address the selection problem, most studies exploit the observation that immigrants tend to settle into existing cities with large immigrant populations. This tendency, noted in Bartel (1989) and Lalonde and Topel (1991), was first exploited by Altonji and Card (1991) to try to identify the causal impact of immigration on natives' labor market outcomes. Altonji and Card use only the geographic distribution of all immigrants. Card (2001) refined this instrument by exploiting Bartel's observation that immigrants locate near previous immigrants from the same country of origin. For each labor market, he created a predicted inflow based on the previous share of the immigrant population from each country of origin combined with the current inflow of immigrants from those countries of origin at the national level. Card's shift-share instrument then is, specifically,

$$\widetilde{m}_{jt} = \sum_o \frac{M_{ojt^0}}{M_{ot^0}} \frac{\Delta M_{ot}}{L_{jt-1}}, \tag{1}$$

[2] See Peri (2016), Dustmann, Schoenberg and Stuhler (2016), or the National Academy of Science (2016), for recent reviews. The main alternative is to exploit differences in the concentration of immigrants across skill (e.g. education-experience) groups (Borjas 2003). The skill-cell approach identifies only relative effects and can be sensitive to the definition of skill groups and other assumptions (see Dustmann and Preston 2012; Borjas 2014; Dustmann, Schoenberg, and Stuhler 2016).

where M_{ojt^0}/M_{ot^0} is the share of immigrants from country of origin o in location j at reference date t^0, ΔM_{ot} is the number of new arrivals from that country at time t at the national level, and L_{jt-1} is the local population in the previous period. The expected inflow rate $\widetilde{m}_{jt}$ is therefore a weighted average of the national inflow rates from each country of origin (the "shift"), with weights that depend on the distribution of earlier immigrants at time t^0 (the "shares"). The potential advantage of this specification arises from the considerable variation in the geographic clustering of immigrants from different countries of origin, i.e. there is a large amount of variation across areas in M_{ojt^0}/M_{ot^0}.

We refer to this as the "past-settlement instrument", but other terms are used in the literature (e.g. "network," "supply-push," or "enclave instrument"). Like all shift-share instruments, the past-settlement instrument has intuitive appeal because it generates variation at the *local* level by exploiting variation in *national* inflows, which are arguably less endogenous with regard to local conditions.[3]

It is difficult to overstate the importance of this instrument for research on the impact of immigration. Few literatures rely so heavily on a single instrument or variants thereof. Appendix Table A.1 presents a list of articles published in top general and field journals in economics, plus a number of recent papers that perhaps better reflect current usage of the instrument.[4] With around 60 publications in the last decade alone (and many more not listed here), it is one of the most popular instrumental variables in labor economics. While most applications focus on

[3] Studies vary in their choice of t^0 and how temporally distant it is from *t.* Saiz (2007) predicts national immigrant inflows using characteristics from each origin country to address the potential endogeneity of national inflows to local conditions. Hunt (2012) and Wozniak et al. (2012) remove the area's own inflows from the national inflow rate to reduce the endogeneity to local conditions.

[4] Most studies listed in Appendix Table A.1 use a version of the Card (2001) instrument as their main strategy to address the selection bias, although some use the simpler Altonji and Card (1991) variant. Others combine the past-settlement instrument with other (mostly distance-based instruments) to increase strength of the first-stage or use the instrument for robustness tests or as a reference point for other identification strategies.

questions related to immigration, authors have begun to use the instrument as a convenient way to generate (potentially exogenous) variation in labor market conditions to examine outcomes like fertility (Furtado and Hock 2010) or parental time investment (Amuedo-Dorantes and Sevilla 2014).

The arguments offered in support of the validity of the instrument vary somewhat across studies. A typical motivation is given by Card (2009):

> "If the national inflow rates from each source country are exogenous to conditions in a specific city, then the predicted inflow based on [Card's] equation (6) will be exogenous."

Although this statement captures the instrument's intuitive appeal, the term "exogenous" can be misunderstood.[5] The instrument is a function of national inflow rates and local immigrant shares and may therefore not be exogenous in the sense of satisfying the exclusion restriction required for a valid instrument if the shares are correlated with unobserved local conditions, even if the national inflow rates are unrelated to those conditions (as shown formally in Goldsmith-Pinkham, Sorkin and Swift 2017).

To the best of our knowledge, ours is the first attempt to evaluate the validity of the instrument within a simple model of labor market adjustment, although various concerns have been expressed previously.[6] Borjas (1999) notes that the exclusion restriction necessary may be violated if local demand shocks are serially correlated, leading to correlation between the immigrants shares used in the construction of the instrument and subsequent demand shocks. Pischke and Velling (1997) note that mean revision in local unemployment rates may introduce

[5] Deaton (2010) argues that a lack of distinction between "externality" (i.e. the instrument is not caused by variables in the outcome equation) and "exogeneity" (validity of the IV exclusion restriction) causes confusion in applied literatures. Such distinction would be particularly useful with regard to shift-share instruments, which appeal to a notion of externality.

[6] Our argument is complementary to Goldsmith-Pinkham, Sorkin and Swift (2017) who thoroughly discuss the identifying assumptions underlying the shift-share strategy in a static setting. We focus instead on the complications that arise from repeated shocks and dynamic labor market adjustments.

bias if immigrant shares are correlated with the unemployment rate, and Amior (2016) notes that immigrant shares tend to be correlated with area-specific demand shocks related to the local industry structure.

None of these concerns appear problematic enough, however, to explain the surprisingly varying and sometimes positive estimates produced by using the past-settlement instrument to identify the impact of immigration on local wages. In particular, serial correlation in local labor demand should be addressed if the instrument is constructed using settlement patterns that are sufficiently lagged (e.g. Dustmann, Fabbri, and Preston 2005; Dustmann, Frattini, and Preston 2013; Wozniak and Murray 2012; Orrenius and Zavodny 2015). We argue instead that the past-settlement instrument almost surely violates the exogeneity assumption by conflating short- and long-run responses to local shocks. As we show, the common strategy of choosing t^0 to be at a substantially earlier point in time offers no protection because the violation arises not from correlates of the initial immigrant distribution, but from the endogenous response to immigrant inflows themselves.

II. The Past-settlement Instrument and Local Labor Market Adjustments

We examine the validity of the past-settlement instrument in a model of local labor markets. The core issue can be described in a simple dynamic setting, in which local labor markets adjust in response to spatial differentials in current economic conditions. We first study concerns raised in the previous literature, and proposed solutions, and then turn towards problems that stem from the prolonged adjustment of labor markets in response to local shocks.

Consider first the choice of an immigrant entering the country. A simplified version of the immigrant location choice model (e.g. Bartel 1989, Jaeger 2007) suggests that immigrants choose a location *j* to maximize their utility

$$U_{ojt} = U\left(\frac{M_{ojt-1}}{M_{ot-1}}, \frac{w_{jt}}{\overline{w_t}}\right), \tag{2}$$

where $\frac{w_{jt}}{\overline{w_t}}$ is the wage premium offered by labor market *j* at time *t*, $\overline{w}_t = \sum_j w_{jt}$ is the unweighted average wage across areas, and $\frac{M_{ojt-1}}{M_{ot-1}}$ is the share of the stock of immigrants from country of origin *o* living in location *j* just prior to the immigrants' arrival. Given the results of Jaeger (2007), we assume both first partial derivatives of *U* are positive, so that immigrants are attracted to labor markets with relatively higher wages and to locations with higher shares of previous immigrants from their country of origin. We also assume that amenities across labor markets are equal except for $\frac{M_{ojt-1}}{M_{ot-1}}$. If the national labor market is in spatial equilibrium before immigrants enter the country, implying that the second term in the utility function is zero, then the sole determinant of immigrants' locations will be $\frac{M_{ojt-1}}{M_{ot-1}}$, which motivates the instrument.

The local labor aggregate consists of natives, N_{jt}, and immigrants, M_{jt}, with $L_{jt} = N_{jt} + M_{jt}$ if immigrants and natives are perfect substitutes. The effect of immigrants on the change in labor supply is then

$$m_{jt} \equiv \Delta \log\left(\frac{L_{jt}}{N_{jt}}\right) = \log(M_{jt} + N_{jt}) - \log(M_{jt-1} + N_{jt-1}) - \left(\log(N_{jt}) - \log(N_{jt-t})\right) \tag{3}$$

Holding N_{jt} fixed over time and abstracting from outmigration, internal migration, or death of previous immigrants such that $M_{jt} = \Delta M_{jt} + M_{jt-1}$, where ΔM_{jt} is the flow of new migrants to location *j* between *t*-1 and *t*, the impact of new immigrants on labor supply is then

$$m_{jt} = \log\left(\Delta M_{jt} + L_{jt-1}\right) - \log(L_{jt-1}) \approx \frac{\Delta M_{jt}}{L_{jt-1}}. \tag{3'}$$

If labor markets are not in spatial equilibrium, immigrant arrivals in labor market j will be partly determined by the distribution of previous immigrants and partly by currently local demand conditions. If the arguments of immigrants' preferences over locations in equation (2) are separable, we can express the flow of new migrants to location j as function of the attraction of enclaves and of labor market conditions as

$$\Delta M_{jt} = (1-\lambda)\sum_o \frac{M_{ojt-1}}{M_{ot-1}}\Delta M_{ot} + \lambda \frac{w_{jt}}{\overline{w_t}}\frac{1}{J}\Delta M_t, \tag{4}$$

giving

$$m_{jt} \approx (1-\lambda)\sum_o \frac{M_{ojt-1}}{M_{ot-1}}\frac{\Delta M_{ot}}{L_{jt-1}} + \lambda \frac{w_{jt}}{\overline{w_t}}\frac{1}{J}\frac{\Delta M_t}{L_{t-1}}, \tag{4′}$$

where λ measures the relative importance of labor market conditions in determining immigrant locations and we assume $0 < \lambda < 1$ because both arguments in (1) positively affect utility. As long as λ is strictly larger than zero, immigrants will prefer to locate in areas with favorable labor market conditions, introducing a selection problem.

To place immigrant inflows in the context of labor demand, we assume that output in labor market j at time t is given by

$$Y_{jt} = \theta_{jt} K_{jt}^{\alpha} L_{jt}^{1-\alpha}, \tag{5}$$

where L_{jt} is labor, K_{jt} capital, θ_{jt} is local total factor productivity and α is capital's share of output. Labor is paid its marginal product such that

$$\log w_{jt} = \log(1-\alpha) + \log \theta_{jt} + \alpha \log k_{jt}, \tag{6}$$

with $k_{jt} = K_{jt}/L_{jt}$ denoting the capital-labor ratio. If in the long run capital is perfectly elastically supplied at price r, the optimal capital-labor ratio will be

$$\log k_{jt}^{*} = \frac{1}{1-\alpha}\log\left(\frac{\alpha}{r}\right) + \frac{1}{1-\alpha}\log \theta_{jt}. \tag{7}$$

It will be affected by the local productivity level θ_{jt} but, because of the constant returns to scale assumption inherent in the production technology, not by the local labor aggregate L_{jt}. In the short run, however, the local capital-labor ratio will not adjust completely and will deviate from its optimum.

Local Adjustments to Supply Shocks

A key issue for the spatial correlation approach is the local adjustment process – in particular the responses of other factors of production – triggered by immigrant-induced local labor supply shocks.[7] If other factors adjust quickly, the observed impact of immigration at the local may not represent the impact at the national level. In particular, the longer the time elapsed between the supply shock and measurement, the less likely the data will uncover any impact of immigrants on local wages (Borjas 1999). Researchers therefore assume that estimates exploiting the spatial distribution of immigrants are biased towards zero (e.g. Borjas 2006, Cortes 2008), or argue that only limited spatial adjustments occur in their period of study.

Research on regional evolutions in the U.S. concludes, however, that spatial adjustments can take around a decade or more (e.g. Blanchard and Katz 1992, Ebert and Stone, 1992, Greenaway-McGrevy and Hood, 2016). Recent evidence from the migration literature similarly points to prolonged adjustment periods (e.g. Monras 2015, Borjas 2015, Amior and Manning 2017, Braun and Weber 2016, Edo 2017), and it has been observed that local labor markets are slow to adjust even long after other types of shocks (e.g. increased trade with China, see Autor,

[7] Labor supply shocks may affect capital flows (Borjas 1999) and internal migration (Card, 2001; Dustmann, *et al.,* 2015; Amior and Manning, 2015), but may also affect human capital accumulation (Smith, 2012; Hunt, 2012), the production technology of firms (Lewis, 2011; Dustmann and Glitz 2015), or occupational choice (Peri and Sparber 2009). In principle, the gradual adjustment of any of these factors potentially affects the validity of the shift-share instrument. For simplicity, we have chosen the adjustment process of capital flows to illustrate our points.

Dorn, Hanson 2016). Although the relative importance and speed of individual channels, such as internal migration, is disputed (e.g. Card 200, Borjas 2014), our argument holds in general.

To illustrate our point, we consider an error correction model that allows for wages to respond to contemporaneous supply shocks, and for labor market dynamics in form of the lagged disequilibrium term.[8] For simplicity we focus on capital adjustments and assume that the local capital-labor ratio does not equilibrate immediately in period *t*, but rather adjusts sluggishly in response to labor supply shocks according to

$$\log k_{jt} = \log k_{jt-1} - m_{jt} + \gamma\left(\log k^*_{jt-1} - \log k_{jt-1}\right). \tag{8}$$

The capital-labor ratio declines in response to immigrant inflows but, barring any subsequent shocks, returns to the optimal level over subsequent periods. The coefficient γ measures the speed of this convergence. As we use decadal data the assumption $\gamma \approx 1$ might not be implausible, but our argument also holds if the adjustment process is slow ($0 < \gamma \ll 1$), begins immediately in period t, is triggered by the anticipation of immigrant inflows, or if the recovery is only partial.

Selection and Disequilibrium Bias

Consider now the impact of immigration on wage changes. Substituting equation (8) into a first-differenced version of equation (6) and adding constant and disturbance terms gives

$$\Delta\log w_{jt} = \beta_0 + \beta_1 m_{jt} + [\Delta\log\theta_{jt} - \beta_1\gamma\left(\log k^*_{jt-1} - \log k_{jt-1}\right) + \epsilon_{jt}] \tag{9}$$

where β_1, the short-term impact of immigration-induced labor supply changes, is the object of interest (in our model $\beta_1 = -\alpha$), and β_0 represents the long-term secular growth in wages (i.e. it

[8] Amior and Manning (2017) consider a similar error correction model with regard to population dynamics in the response to labor demand shocks.

would be the coefficient on t in wage levels regression). The quantity in square brackets is unobserved to the econometrician and illustrates the endogeneity problem that the instrument is designed to address. Because wages are affected by local demand shocks (equation 6) and immigrant flows are affected by local wage premia (equation 4), m_{jt} will be correlated with $\Delta\log\theta_{jt}$. Because this correlation is thought to be positive (higher wages lead to more immigrant inflows, e.g. Jaeger 2007), OLS estimates of β_1 are presumed to be upward biased estimates of the true impact.

The literature largely focuses on this correlation and how the past-settlement instrument addresses the selection problem.[9] Using the past settlement instrument $\widetilde{m}_{jt}$ solves this endogeneity problem if demand shocks are unrelated to the initial distribution of immigrants used to construct the instrument. If productivity or other labor demand shocks are serially correlated (Amior and Manning 2017), this assumption might be violated. The literature has noted this problem (Borjas 1999, Hunt and Gauthier-Loiselle 2010, Aydemir and Borjas 2011, Dustmann, Frattini and Preston 2013, Dustmann and Glitz 2015, among others) and has addressed it by testing for serial correlation in the residuals of the wage regression (e.g. Dustmann, Frattini and Preston 2013) or by lagging the base period t^0 of the instrument to minimize its correlation with current demand shifts (e.g. Hunt and Gauthier-Loiselle 2010). Since our concern is not about time dependence in external processes, we abstract from this issue by assuming that $\log\theta_{jt}$ follows a random walk. If, in addition, the flow of immigrants at the national level is unaffected by local

[9] Most of the literature uses first-differenced or fixed-effect specifications (e.g. Dustmann et al. 2005). The instrument is unlikely to address selection in wage levels. OLS estimates are biased by non-random sorting of recent arrivals with respect to wage levels, but IV estimates would suffer from non-random sorting of immigrant stocks. There is little reason to expect that the latter is much less of a concern since the past-settlement instrument suggests a close relationship between stocks and new arrivals, and spatial differences in wage levels are persistent (Moretti 2011).

demand conditions (as we assume here and as is plausible in our empirical setting) the instrument will be uncorrelated with $\Delta \log \theta_{jt}$.

Our fundamental point is, however, that even in the absence of serial correlation in $\Delta \log \theta_{jt}$ immigration can generate endogeneity issues that invalidate the past settlement instrument. The literature has essentially ignored the second component of the error term, the dynamic adjustment process, which creates an endogeneity problem for the usual shift-share instrument. Local labor market shocks (like immigration) trigger general equilibrium adjustments that gradually offset the initial negative wage effect and lead to subsequent recovery and positive wage growth. If these adjustments are slow enough, they may still be ongoing during the subsequent observational period, even at a decadal frequency. Because the country of origin distribution of immigrant inflows to the U.S. is highly serially correlated, there is a high degree of correlation over time in the locations of new immigrants. The past settlement instrument aggravates this issue, as it is predicated on the existence of some degree of serial correlation in immigrant inflows – it isolates that part of the variation that is predictable by the cumulative inflows up to time t^0.

The combination of the slow adjustment process and the high degree of serial correlation in the country-of-origin distribution of immigrants means that the short-term response to new immigrant arrivals may overlap with the lagged response to past immigrant inflows. The conventional shift-share IV estimator used in the literature does not address this source of endogeneity and conflates these short- and long-term responses, making it both difficult to interpret and a biased estimator of the wage impact of immigration.

To illustrate, consider the following thought experiment. Imagine that the economy is in a spatial and dynamic equilibrium at time t=0. If immigrant inflows occur at the next period $t=1$, wages change according to $\Delta\log w_{j1} = \beta_0 + \beta_1 m_{j1} + [\Delta\log\theta_{j1} + \epsilon_{j1}]$. If the instrument is uncorrelated with current demand shifts, $\Delta\log\theta_{j1}$, the conventional IV estimator will consistently estimate β_1.

At *t=2*, however, wages adjust according to

$$\Delta\log w_{j2} = \beta_0 + \beta_1 m_{j2} + [\Delta\log\theta_{j2} - \beta_1\gamma\left(\log k^*_{j1} - \log k_{j1}\right) + \epsilon_{j2}] \tag{10}$$

where the disequilibrium term $\beta_1\gamma(\log k^*_{j1} - \log k_{j1})$ reflects that the local labor market may still be adjusting to the immigrant supply shock from *t*=1. Using the past-settlement instrument, $\widetilde{m}_{j2}$, to instrument for m_{j2} in equation (10) gives

$$\text{plim}\ \tilde{\beta}^{IV}_{1|t=2} = \beta_1 - \underbrace{\beta_1 \frac{\gamma}{1+\beta_1} \frac{\text{Cov}(\widetilde{m}_{j2}, \Delta\log\theta_{j1})}{\text{Cov}(\widetilde{m}_{j2}, m_{j2})}}_{\substack{\text{adjustment to} \\ \text{lagged demand shocks}}} - \underbrace{\beta_1\gamma \frac{\text{Cov}(\widetilde{m}_{j2}, m_{j1})}{\text{Cov}(\widetilde{m}_{j2}, m_{j2})}}_{\substack{\text{adjustment to} \\ \text{lagged supply shocks}}}\ . \tag{11}$$

The two asymptotic bias terms arise from the response of the capital-labor ratio to past shocks. The first is the response to past local demand shocks and the second is the response to immigration-induced supply shocks in the previous period. Both responses raise the marginal productivity of labor and lead to an upward bias in the IV estimate (assuming that β_1 is negative).[10]

The first bias term illustrates that demand shocks can generate bias even if they are not serially correlated. Intuitively, if local demand shocks trigger a prolonged adjustment process, immigrant shares must not only be uncorrelated with current but also with past demand shocks. Choosing t^0 to be sufficiently lagged may therefore be advantageous even if the demand shocks

[10] We have assumed that immigrant inflows occur as a "shock" to which local markets respond only in hindsight. If these inflows occur repeatedly in the same cities, however, their arrival might be anticipated. In Appendix A.2 we show that when future arrivals are anticipated, the disequilibrium bias becomes larger, and the estimates of the wage impact of immigrant are more positive, in the period after compositional changes occurred, when the response to unexpected arrivals in the previous period coincides with the updating of beliefs about future arrivals. In our data, this period corresponds to the 1980s.

themselves are not serially correlated. As this is a common strategy in the literature, we assume below that the instrument $\tilde{m}_{jt}$ is sufficiently lagged and uncorrelated with the current adjustment to past demand shocks, i.e. we will assume that the first bias term is approximately equal to zero.

The bias from lagged supply shocks is harder to address. Its size at t=2 depends on the ratio $\mathrm{Cov}(\tilde{m}_{j2}, m_{j1})/\mathrm{Cov}(\tilde{m}_{j2}, m_{j2})$, which is the slope coefficient in a regression of past immigrant inflows on current immigrant inflows, using the conventional shift-share variable as an instrument. This coefficient will be small if the instrument predicts current immigrant inflows in area j substantially better than it predicts inflows in the previous period. As we will show, this is unfortunately rarely the case in the U.S. context, where the coefficient fluctuates around and sometimes exceeds one. The instrument is a good predictor for immigrant inflows in the intended period, but it is also a similarly good predictor for previous inflows. Choosing t^0 to be temporally distant does not address this bias.[11]

The size of the disequilibrium bias in equation (11) also depends on the speed of convergence γ. In a general setting with repeated immigrant inflows, however, this speed may have little influence on the magnitude of the bias. Ignoring demand shocks, the estimated impact of instrumented immigrant inflows in a generic period t is

$$\text{plim}\, \tilde{\beta}_{1|t}^{IV} = \beta_1 \left[1 - \gamma \sum_{s=0}^{\infty} (1-\gamma)^s \frac{\mathrm{Cov}(\tilde{m}_{jt}, m_{jt-1-s})}{\mathrm{Cov}(\tilde{m}_{jt}, m_{jt})}\right], \tag{12}$$

such that the size of γ will matter little if the predictable component of immigrant inflows is highly serially correlated (see Appendix A.1). In the extreme case, if the covariance between the

[11] Lagging the base period further may reduce the numerator in the ratio $\mathrm{Cov}(\tilde{m}_{j2}, m_{j1})/\mathrm{Cov}(\tilde{m}_{j2}, m_{j2})$ but, by reducing its ability to predict inflows in the intended period, also the denominator. In principle, the bias may be greater if the denominator shrinks more than the numerator. In the recent decades in the U.S., the ratio appears to be insensitive to the choice of base period t^0.

instrument $\tilde{m}_{jt}$ and immigrant inflows is equal for all periods $t\text{–}s$ for $s\geq1$, expression (12) simplifies to

$$\text{plim } \tilde{\beta}_{1|t}^{IV} = \beta_1 \left[\frac{\text{Cov}(\tilde{m}_{jt}, m_{jt}) - \text{Cov}(\tilde{m}_{jt}, m_{jt-1})}{\text{Cov}(\tilde{m}_{jt}, m_{jt})}\right], \tag{13}$$

because $\lim_{t\to\infty}\gamma\sum_{s=0}^{t}(1-\gamma)^s = 1$. This expression does not depend on the speed of convergence γ. Intuitively, it does not matter if a disequilibrium adjustment has been triggered by immigrant inflows in the previous period or in an earlier period if both are equally correlated with the instrument. In the U.S., the serial correlation in immigrant inflows is so extraordinarily high that the speed of convergence may matter little.[12]

As equation (13) makes clear, the bias arising from the slow adjustment process can by itself cause the IV estimate of the impact of immigration to change from negative to positive if $\text{Cov}(\tilde{m}_{jt}, m_{jt-1}) > \text{Cov}(\tilde{m}_{jt}, m_{jt})$. This conclusion holds even if the true wage impact is strongly negative. If the magnitudes of $\text{Cov}(\tilde{m}_{jt}, m_{jt})$ and $\text{Cov}(\tilde{m}_{jt}, m_{jt-1})$ are very similar, $\tilde{\beta}_{1|t}^{IV}$ may be quite close to zero (either positive or negative), even if the true effect is quite large. OLS estimates suffer from selection bias, but are less affected by this disequilibrium bias if the actual inflows m_{jt} vary more than their predictable component $\tilde{m}_{jt}$ across decades (as they do in the U.S. Census). It is therefore not clear, *a priori*, if IV estimates using the shift-share instrument will be less asymptotically biased than their OLS counterparts.[13]

[12] What does matter, however, is the assumption that in the long run, immigrant inflows have no persistent effect on local relative wages. If the local recovery is only partial, the size of the bias in equation (13) would shrink proportionally. If immigration has instead a positive long-run effect on local wages (e.g. via agglomeration and density externalities, Peri 2016), the bias increases accordingly.

[13] Our arguments here mirror those from two recent studies on labor demand shocks. Amior and Manning (2017) argue that persistent trends in labor demand can trigger important population dynamics at the local level while Greenaway-McGrevy and Hood (2016) find that this persistence needs to be accounted for when studying the response to local demand shocks. The problem is even more severe with immigration supply shocks because they are more highly serially correlated than demand shocks.

IV. Revising the Past Settlement Instrument

Our model illustrates the difficulty of consistently estimating the labor market impact of immigration using the past settlement instrument. In the presence of prolonged spatial adjustment processes, we require an instrument that

- is uncorrelated with contemporaneous and past demand shocks,
- is correlated with the current locational choices of immigrants, but
- is uncorrelated with their choices in the previous period.

The last two conditions are testable, while in the absence of information on local demand shifts the first requires a theoretical argument. The past-settlement instrument potentially satisfies the first condition if the base period t^0 is sufficiently lagged, and it quite clearly satisfies the second condition. So the crucial problem is the correlation of the instrument with past supply shocks, which arises because of the slow adjustment of local labor markets.

In periods in which the country of origin composition of migrants changes substantially, the instrument will be less correlated with past supply shocks, and the IV estimator less biased. We show below that the empirical evidence is consistent with this hypothesis. Our model also indicates that the disequilibrium bias is reduced in settings in which the overall rate of immigration is temporarily increased (e.g. Gonzalez and Ortega 2011), or where origin-specific push factors change the inflow rate of a particular origin group, as in recent studies by Aydemir and Kirdar (2013), Llull (2014), Monras (2015), Chalfin (2015), and Carpio and Wagner (2015).[14]

[14] The use of push factors is typically motivated by the desire to break the potential endogeneity of national inflows to local conditions – for example, more Mexicans may enter the United States if the California labor market is strong. They may under some conditions also reduce the problems that we describe here, however, if the push factors trigger immigrant flows that are very different from previous inflows.

To fully address the disequilibrium bias we consider all immigrant arrivals, but isolate innovations in their local inflow rates that are uncorrelated with past inflows. Intuitively, this can be accomplished by first regressing the instrument $\widetilde{m}_{jt}$ on its lag $\widetilde{m}_{jt-1}$ (and potentially further lags), and then using the residual from this regression to instrument current immigrant inflows.[15] By construction, this residualized instrument captures innovations in the spatial distribution of immigrant arrivals that are (i) predictable and (ii) uncorrelated with the predictable component of previous inflows. If the usual requirement that the instruments are uncorrelated with local demand shocks is also met, the residualized instrument satisfies the exclusion restriction. To implement this intuition in one step, we simply add $\widetilde{m}_{jt-1}$ as a control variable to proxy for the adjustment process in our standard estimating equation,

$$\Delta \log w_{jt} = \beta_0' + \beta_1' m_{jt} + \beta_2' \widetilde{m}_{jt-1} + \eta_{jt}', \tag{14}$$

continuing to instrument the endogenous actual inflows m_{jt} with $\widetilde{m}_{jt}$.

While adding $\widetilde{m}_{jt-1}$ as a control variable may suffice to "fix" the spatial correlation approach, we can gain additional insights by using it as a second instrumental instead of as a control variable. We address two problems by regressing local wage growth on both current and past immigrant inflows,

$$\Delta \log w_{jt} = \beta_0 + \beta_1 m_{jt} + \beta_2 m_{jt-1} + \eta_{jt} \,, \tag{15}$$

and instrument the two endogenous variables with the two instruments,

$$\widetilde{m}_{jt} = \sum_o \frac{M_{ojt^0}}{M_{ot^0}} \frac{\Delta M_{ot}}{L_{jt-1}} \quad \text{and} \quad \widetilde{m}_{jt-1} = \sum_o \frac{M_{ojt^0}}{M_{ot^0}} \frac{\Delta M_{ot-1}}{L_{jt-2}},$$

in the two first-stage equations,

$$m_{jt} = \pi_{10} + \pi_{11} \widetilde{m}_{jt} + \pi_{12} \widetilde{m}_{jt-1} + u_{jt} \tag{16}$$

[15] One lag appears sufficient in our setting, as the national origin shares did not change much in the decades before the 1970s (see Table 1, Panel C). The number of lags would be important in settings in which the origin and spatial distributions shift repeatedly.

and

$$m_{jt-1} = \pi_{20} + \pi_{21}\widetilde{m}_{jt} + \pi_{22}\widetilde{m}_{jt-1} + v_{jt} \tag{17}$$

By using m_{jt} to instrument for $\widetilde{m}_{jt}$ (equation 16), we address the selection problem. By including m_{jt-1} and using $\widetilde{m}_{jt-1}$ as an instrument (equation 17), we address the disequilibrium bias.[16] The coefficient β_1, the usual coefficient of interest in the literature, captures the wage impact of immigration in the short run and is likely negative, while the coefficient β_2 captures the longer-term reaction to past supply shocks and is expected to be positive.[17]

If the local immigrant stocks at t^0 used for construction of $\widetilde{m}_{jt}$ and its lag $\widetilde{m}_{jt-1}$ are the same, the difference between the two instruments comes only from variation over time in the composition of national inflows. If this composition changes little from one period to the next, the instruments will be very highly correlated, and there may be little distinct variation in each to identify separately both first stage equations, which may suffer from a (joint) weak instrument problem in finite samples (Sanderson and Windmeijer 2016). The "double instrumentation" specification in equations (15) through (17) is therefore more demanding on the data, but has two potential advantages compared to the simpler specification (14). By allowing for $\pi_{21} \neq 0$, we permit the lagged inflows, m_{jt-1} to be correlated with $\widetilde{m}_{jt}$ conditional on $\widetilde{m}_{jt-1}$, While it is not obvious why π_{21} should be non-zero, such a correlation would not be partialled out in equation (14) and instead would be reflected in the estimate of β_1. If instead $\pi_{21} = 0$, the two models give

[16] As another alternative, our model could be transformed into an autoregressive-distributed lag model to then apply dynamic panel data methods (Bond 2009). We do not observe a sufficient number of lags of the dependent variable for the 1970s, however, and our model allows for the more direct estimation via equation (15).

[17] Specifically, in our model β_1 should be negative while β_2 should be positive and of similar magnitude if lagged adjustments are completed within about one decade *or* if immigrant inflows are highly serially correlated. Other models, for example those with frictions (Chassambouli and Peri 2015, Amior 2016) would predict other relative magnitudes.

the same estimates for β_1.[18] In addition, by including m_{jt-1} instead of $\widetilde{m}_{jt-1}$ as a regressor, the double instrumentation specification yields not only an estimate of the short-term wage impact of recent immigrant arrivals, but also a consistent estimate of the response of local wages to previous inflows due to the recovery process.

Other, seemingly more direct, strategies to control for the adjustment process would not yield consistent estimates. Most importantly, controlling for actual lagged immigrant inflows, m_{jt-1} (i.e. without instrumenting with $\widetilde{m}_{jt-1}$) would introduce a mechanical relationship with previous local demand shocks and therefore reintroduce the selection problem.[19] Lagging the instrument further, a common strategy for other reasons, also would not address the problem. Finally, the validity check recently proposed by Peri (2016) to test if the past-settlement instrument correlates with lagged wage growth, while useful from other perspectives, would not reliably detect the disequilibrium problem. The absence of such a correlation is precisely one of the possible consequences when the short-run wage impact and longer-term wage recovery to immigrant inflows overlap.[20] For the same reason, controlling for past wage growth in the wage regression does not address the issue.

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[18] Intuitively, the "right" instrument should predict each endogenous variable, i.e. $\pi_{11}\neq 0$ in equation (16) and $\pi_{22}\neq 0$ in equation (17), while the "wrong" instrument would not have an effect, i.e. $\pi_{12}=0$ in equation (16) and $\pi_{21}=0$ in equation (17). If we are willing to impose such restrictions we can estimate equation (15) using a systems estimator, with potential efficiency gains compared to the 2SLS procedure. We focus on 2SLS results, however, because this would require a structural interpretation of our first stage equations and immigration location choices may be more complicated than our model suggests.

[19] Note that the residual from a regression of the past-settlement instrument $\widetilde{m}_{jt}$ on past immigrant inflows m_{jt-1} is a linear function of the latter,

$$\hat{\epsilon}_{jt} = \widetilde{m}_{jt} - a - bm_{jt-1}.$$

m_{jt-1} depends positively on local demand shocks in that period, however, introducing bias (see also equation (11)).

[20] In our model, a regression of lagged wage growth on the past-settlement instrument $\widetilde{m}_{jt}$ estimates $\alpha[\gamma \sum_{s=0}^{\infty}(1-\gamma)^s \operatorname{Cov}(\widetilde{m}_{jt}, m_{jt-2-s}) - \operatorname{Cov}(\widetilde{m}_{jt}, m_{jt-1})]/\operatorname{Var}(\widetilde{m}_{jt})$, and the term in brackets can be approximately zero if immigrant inflows are highly serially correlated.

V. Data and Descriptive Statistics

To demonstrate our solution, we use data from the 1960-2000 U.S. Censuses and the merged 2007-2011 American Community Surveys (ACS), all obtained through IPUMS (Ruggles, *et al.* 2015). For convenience, we will refer to the merged ACSs as the year 2010.[21] We define an immigrant as a person born in a country other than the U.S. (excluding outlying U.S. territories) and a newly-arrived immigrant as a foreign-born person that immigrated during the last decade. We divide immigrants into 39 countries and regions of origin.[22] In descriptive results that use data that goes back to the 1940 Census, we use the same 17 countries and regions that were used by Card (2001) because of the limited information on countries of origin in those data.

The entire immigrant populations by origin and local area are used in the construction of the past-settlement instrument. We conduct our analysis across metropolitan statistical areas (MSAs).[23] MSAs are the standard unit of analysis in the existing literature and, because of their better comparability over time, are also the baseline unit in our analysis. We include in the analysis all MSAs that can be identified in all Censuses, use data on finer spatial units to make their boundaries as consistent over time as possible, and finally exclude three MSAs in which boundary changes were particularly large between the 1960, 1970, and 1980 Censuses, and for

[21] We use 2007-2011 rather than, for example, 2008-2012, because the MSA definitions changed with the 2012 ACS.

[22] We separately include each country of origin with at least 5,000 observations in the 1990 census, except Cambodia, Iran, Laos, Thailand, and Vietnam, which were not separately coded in all Censuses. All remaining countries of origin are merged into the regions Latin America, Western Europe, Eastern Europe, Asia, Africa, Australia and New Zealand, and Others. Countries that split or merged after 1970 (the USSR, Yugoslavia, Czechoslovakia, and Germany) are coded as the merged unit throughout (e.g. the separate states of the Russian Federation continue to be coded as one unit after the breakup as the USSR, and West and East Germany are merged prior to 1990). Hong Kong and Taiwan are coded as part of China.

[23] Results using Commuting Zones as the geographic unit of observation are shown in the Appendix. The definition of commuting zones is based on Tolbert and Sizer (1996), and applied to Censuses using codes provided by Autor and Dorn (2013).

which finer information cannot be used to make them more consistent.[24] This leaves us with a sample of 109 MSAs.

Our outcome variable is the average log weekly wage in the native labor force in an area. We restrict our wage sample to those who are 18-64 years of age and have 1-40 years of potential experience (age minus expected age at completion of formal schooling) and drop those who currently attend school, who live in group quarters, or who are self-employed. To reduce the influence of outliers (some wages are as low as, or below, one dollar per week) we drop individuals who wages are in the bottom and top percentile in each census year. Dropping the top percentile matters little, while the choice of cut-off point at the bottom has a non-negligible but, as we show, limited, effect on our estimates. To address composition bias from changes in the skill and demographic characteristics of workers, we residualize wages using separate national-level regressions for each census year that control for six education levels (high school dropout, high school degree, some college but no degree, bachelor degree, master degree, and professional or doctoral degree), 40 potential experience levels, gender interacted with marital status, three races (white, black, and other), and nine U.S. Census divisions.

We show the characteristics of immigrant inflows by decade in Table 1. The first row shows the immigrant share of the population, which has risen steadily from its low of 5.2 percent in 1970 to 13.6 percent in 2010. In Panel A, we show the share of new arrivals (those who entered the U.S. in the 10 years prior to the year of observation), the average share of new arrivals in 109 MSAs, as well as the standard deviation and coefficient of variation in new arrivals shares across those same MSAs. The coefficient of variation of the share of recent

[24] These are Bridgeport and New-Haven-Meriden, CT, and Worcester, MA. For all three, their total recorded populations more than triple between the 1960 and 1970 Censuses, and then shrink again by more than two-thirds in the 1980 Census. No other MSA comes close to an equally problematic pattern in the data.

arrivals by MSA shrunk by one half between 1970 and 2010, indicating that immigrants were more geographically dispersed in earlier decades.

As noted by Hatton (2015), after the enactment in June 1968 of the Immigration and Nationality Act of 1965, the country of origin composition of immigrant arrivals changed considerably.[25] Since 1970, however, that composition has remained highly stable. These patterns are illustrated in Panel B of Table 1. Among new arrivals in the 1970 Census (i.e. those who arrived in the 1960s, only a small minority of which arrived after the change in admissions policy was implemented), 41 percent were of Canadian or European origin, while in 1980 (those arriving in the 1970s, after the policy change) the corresponding share was only 17 percent. At the same time, the share of Latin Americans and Asians among the newly-arrived rose from 54 percent for those arriving in the 1960s to 75 percent for those arriving in the 1970s. There are no similarly large compositional changes during the subsequent three decades.

We show the serial correlation from one decade to the next in the national composition of inflows in Panel C of Table 1. The first row shows the correlation in the shares of all 38 origins (excluding "Other"). The correlation in country of origin shares between those arriving in the 1960s and those arriving in the 1970s is 0.59 while the correlation is between 0.96 and 0.99 in subsequent decades. In the next row, we find a similar pattern if we exclude Mexicans. In the last row, we show the correlation in immigrant stocks for all decades from 1950 to 2010 (because we cannot identify new immigrants prior to the 1970 Census). These results confirm that the 1970s witnessed a unique break in the country-of-origin composition of immigrants. The immigrant

[25] The Immigration and Nationality Act replaced the national origins quotas, which favored British, German, and Irish immigrants, with a less discriminatory system. Congress did not intend to trigger radical changes in immigration patterns, and did not expect the sudden and dramatic shift in the origin composition (Hatton 2015).

stocks in 1970 and 1980 have a correlation coefficient of 0.65, while the three earlier pairwise correlations are all above 0.94 and those afterwards are at least 0.90.

These patterns are illustrated in Figure 1, where we plot the country-of-origin shares in one decade with the same share in the subsequent decade. In each row, the left-hand graphs show all 39 country-of-origin groups while those on the right exclude Mexico. The first row plots the 1960 arrivals (from the 1970 Census) vs. the 1970 arrivals (from the 1980 Census). The second row plots the 1970 arrivals vs. the 1980 arrivals (from the 1990 Census), and so on. The correlation is clearly stronger after the 1970s.

VI. Estimating the Impact of Immigration on Natives' Wages

Our data allow us to estimate the wage impact of recent immigrant arrivals in the U.S. for five different decades, or four decades when controlling for the lagged inflow rate.

OLS and Conventional IV Estimates

As a benchmark, in Panel A of Table 2 we present OLS estimates of equation (9) where the dependent variable is the decadal growth in residualized log wages of all workers aged 18 to 64 (subject to the other sample restrictions described above) and the units of observation are MSAs. While some of the literature has focused only on men, we include all workers.[26] We estimate the model separately for each decade from 1960s to the 2000s. Panel B presents the corresponding IV estimates, together with the first-stage coefficient on the past settlement

[26] Estimating our results only for men yields similar results. These results are available from the authors by request.

instrument, which is the shift-share variable defined in equation (1) with the reference period being the beginning of the relevant decade. We also report the first stage R^2.

Both the OLS and IV estimates are positive for some decades. Selection may generate an upward bias in the OLS estimates and, once we instrument the immigrant inflow rate using the past settlement instrument, the estimates indeed tend to be smaller and more often negative. The differences are modest, however, and the IV estimate for the 1980s (using the 1990 Census) is still positive and statistically significantly different from zero. The point estimates also differ substantially across the decades.[27] Borjas, Freeman, and Katz (1997) and Borjas (1999) note that the spatial correlation approach yields quite different estimates for the 1970s and 1980s, and this variability extends to IV estimates based on the past settlement instrument, to more recent periods and to different spatial definitions.

It is only for the 1970s (using the 1980s Census) that we find a more than marginally negative IV estimate of the effect on wages. As already noted, this was a period in which changes in the U.S. admission policy created a substantial shift in the country-of-origin composition of immigrant arrivals, leading to their distribution across MSAs being plausibly less related to their spatial distribution in the previous decade. In Panel A of Table 3 we report the correlations between actual immigrant inflows and the past settlement instrument and their respective lags. As expected, this correlation is lower for immigrant inflows in the 1970s than in the later decades: 0.82 compared to 0.92 to 0.96. This gap becomes larger when considering the instrument instead of actual inflows: 0.70 compared to 0.96 to 0.99.

Given these magnitudes, serial correlation is an important issue regardless of the time period under consideration. There is at least some variation in the 1970s while in other decades

[27] Estimates using Commuting Zones rather than MSAs are similar (see Appendix Table A.2).

both the actual inflows and the instrument are nearly perfectly correlated. Our theoretical argument implies that all the IV estimates in Table 2 are upward-biased, but it also suggests that this bias should be smallest in the 1970s (1980 Census) – exactly the period in which we find the most negative estimate.[28]

From Equation (11), we can estimate some of the key components of the disequilibrium bias. In particular, the "supply shock" bias is proportional to the ratio between the two pair-wise correlations of the instrument and lagged and current inflows. One might expect that the correlation of the instrument with current inflows (the denominator) would be larger than the correlation with lagged inflows (the numerator). As we show in Panel B of Table 3, this is unfortunately not the case. In the later decades, the instrument is more highly correlated with past inflows than with the current inflows it is supposed to predict. This is a natural pattern when the national composition changes very little, since past inflows are closer in time to the reference period t^0 used in the construction of the instrument. Lagging the reference period further weakens the predictive power of the instrument relative to time t, but does not substantially change this pattern, as can be seen by comparing the rows using t-2 as the base period (i.e. constructing the instrument from the base immigrant distribution two decades prior to the year of observation). The correlations between the actual inflows at t and the instrument are still weaker than for the correlations when the actual inflows are measured at t-1.

[28] The break itself was likely not anticipated (see Hatton 2015) after the Immigration Act of 1965. Once enacted, however, workers and firms may have expected that it had a permanent effect on the country-of-origin and therefore spatial distribution of immigrant arrivals. In this case, the Immigration Act may also explain why the spatial correlation estimates are most positive in the 1980s (1990 Census). The question of whether workers and firms act on expectations plays a more important role in this argument than the question how expectations are exactly formed (see Appendix A.2). The spatial distribution of inflows in the 1970s were so similar to the inflows in the 1980s that even a naïve extrapolation of the former would accurately predict the latter.

Some studies in the literature combine spatial variation in immigrant inflows across areas with their density across skill groups.[29] Depending on the outcome variable of interest, the explanatory variable may be the rate of immigration in a particular education group (Cortes, 2008; Hunt, 2012), or the relative skill content of immigration (Card, 2009; Lewis, 2011) in an area. Panel C of Table 3 shows the immigration rates of high skilled (with some college or more) and low skilled (high school degree or less) workers, as well as the logarithm of the ratio of high skilled to low skilled immigrants. These measures show the same high degree of serial correlation as those in Panel A. The serial correlation in the skill-specific inflow rates and instruments is close to the corresponding values of the total rate, where it is modest in the 1970s and high in all later decades. The serial correlation in the log skill ratio is high in all periods. The disequilibrium problem will therefore also affect empirical strategies that exploit both spatial and skill-cell variation.[30]

Partialling Out the Past Supply Shock

Our theory suggests that we can address the disequilibrium bias from serial correlation in immigrant inflows by isolating innovations in their predicted inflow rate across cities. In Table 4 we report results from estimating specification (14), in which the lag of the past-settlement instrument is included as a control variable. By partialling out the correlation of the instrument with its lag, we use only innovations in the predicted inflow rate across cities in the U.S. for

[29] See Peri (2016) or Dustmann, Schoenberg, and Stuhler (2016) for an overview. By using both spatial and skill-cell variation, one can difference out unobserved factors that lead to higher or lower wages of all workers in a city (see Card, 2007). Only relative wage effects of immigration across skill groups are identified, however.

[30] The magnitude of the problem may be different, however. The assumption that average wages are mean reverting because labor demand is perfectly elastic in the long run is standard in the literature (even though wage differences between cities are persistent, see Moretti 2010), but differences in local skill-specific wages may be more persistent.

identification of the effect of immigration on wages. We report both the first stage F statistic and the partial R^2 (Bound, Jaeger, and Baker 1995). This "residualized" instrument explains much less of the observed variation in immigrant inflows than the conventional version shown in Table 2. The partial R^2 between the instrument and immigrant inflows is below 0.16 in the 1980s, 1990s, and 2000s (from the 1990, 2000, and 2010 Censuses, respectively). Since the instrument and its lag are so highly correlated in these decades, as shown in Table 3, there is virtually no variation left after partialling out the latter from the former, and the instrument does not do a good job of explaining actual inflows. In all of these years the second stage results vary wildly and have large and uninformative confidence intervals across all three decades.

The exception to this pattern is again the 1970s. Although the past-settlement instrument is highly correlated with its lag in this decade as well, our results suggest that there is sufficient variation to distinguish the effects of the two variables. After partialling out the correlation with its lag, the (residualized) instrument still explains about 37% of the (remaining) spatial variation in immigrant inflows because of the change in the origin-composition of arrivals in the U.S. The first stage F-statistic is sufficiently large to rule out important finite sample biases, but substantially smaller relative to the corresponding statistic shown in Table 2, suggesting that the past settlement instrument appears so powerful in the existing literature largely because of its serially correlated component. The estimated impact of immigration in the second stage is substantially more negative than the corresponding result from Table 2, however (-0.840 vs. -0.342) and suggests that despite the considerable change in the spatial distribution of immigrants flows in the 1980s, the disequilibrium bias in conventional IV estimates in the literature is large.

"Double Instrumentation": First-stage Results

We turn now to estimating the full double instrumentation procedure from equation (15). By instrumenting both the immigrant inflows in the current and previous decades with the corresponding versions of the past-settlement instrument, this procedure gives us not only an estimate of the initial response of local wages to immigrant arrivals, but also of how local wages re-adjust over a longer time period.

In Table 5 we present a set of first stage results from the 2SLS estimation of equation (15).[31] For comparison, columns (1) through (4) show results that follow those in Table 2 by setting the base period in each regression to be 10 years prior to the year of estimation. The pattern for the 1970s in column (1) is what one might expect given the previous results: the 1960s instrument is the main predictor of inflows in that decade, while the 1970s instrument has the largest coefficient estimate for the 1970s inflow. In contrast, columns (2) to (4) illustrate that in the later decades the two instruments carry almost the same information because of the serial correlation in national inflow shares and that the second stages associated with them are unlikely to produce meaningful results on the impact of immigration on natives' wages. In addition to the first stage *F* statistic on the instruments, which measures the overall contribution of the instruments in explaining variation in immigrant inflows, we also report the Sanderson-Windmeijer (2016) *F* statistic. This statistic measures whether there is sufficient information in the two instruments to identify the effects of the two endogenous explanatory variables in the second stage. The low values of the Sanderson-Windmeijer statistic indicate that the model in columns (2) through (4) is likely to be underidentified. The instability of the coefficients on the

[31] To simplify comparison between first-stage coefficients, we rescale the lagged instrument so that both instruments have the same mean. This has no effect on coefficients in the second stage.

instruments between the two first stage equations and across decades also suggests that from the 1980s to the 2000s the instruments do not carry much independent information.

Even for the 1970s some questions remain, as the coefficient for the instrument and its lag in the first stage for the inflow at time *t* have nearly the same size. This is not an unreasonable pattern if we expect that new arrivals are attracted to areas that were popular destinations in previous decades. It is also not an issue for estimation of the second stage, as the estimates of the slope coefficients depend on the (variance-weighted) difference of the two respective first-stage coefficients, which is large and positive.

"Double Instrumentation": Second-stage Results

Because the first stage results for the 1980s, 1990s, and 2000s indicate that there is not enough independent information in the two instruments to identify sufficiently the second stage, to estimate the impact of immigrants we focus on the 1970s, and report our estimates in Table 6.[32] We report different specifications, varying the construction of the instrument, the definition of the outcome variable, the weighting scheme, or the inclusion of control variables in columns (1) to (7). For comparison, we report the conventional IV estimate of the effect of immigrant inflows in Panel A. We then show the estimates of immigrant inflows and lagged immigrant inflows using equation (15) in Panel B and the corresponding reduced-form estimates in Panel C. Our model provides clear predictions on the signs of the coefficients: the (presumably negative) coefficient on the 1970s inflows captures the wage impact of recent arrivals in the short run while

[32] We report heteroscedasticity-robust standard errors in Table 6. These may be downward biased, however, because of small-sample bias. Conventional estimates of the standard error are larger, but the coefficient estimate on recent arrivals remains significant at the 1 or 5 percent level in all specifications.

the (presumably positive) coefficient on the 1960s inflows captures the longer term reaction to local shocks.

We find that the impact of recent immigrant arrivals on natives' wages is indeed negative and statistically significant. In our baseline specification in Panel B, column (1), the impact of a one-percent (as a share of the local labor force) immigrant inflow is estimated to reduce average wages by about 0.7 log points. This estimate is substantially more negative than the corresponding conventional IV estimate in column (1), Panel A (which repeats the estimate from Table 2), consistent with our expectation that estimates that do not control for the adjustment to past immigrant shocks are biased upward. The coefficient is similar in size to the corresponding estimate from our simpler procedure in the first column of Table 4, in which we used the lagged past-settlement variable as a control instead of a second instrument. In column (1), we also find a positive and statistically significant coefficient on the predicted lagged immigrant inflow, in keeping with our expectation that this coefficient captures the longer-term adjustment of local labor markets to local supply shocks. In absolute terms, this coefficient is nearly as large as the coefficient on current inflows, suggesting that local wages largely recover from an immigration-induced supply shock within one decade. These estimates capture only the impact on local wages relative to other areas, however, and immigration may have a positive or negative effect over time on the national labor market as local labor markets spatially equilibrate.

Both conventional IV estimates in Panel A and the double IV estimates in Panel B are potentially sensitive to specification choices. One common choice in the literature is to lag the base period further. In column (2), we change the base year in the instrument to 1960 rather than 1970, which only strengthens both the conventional and double IV estimates.[33] We get similar

[33] With the 1960 Data Restoration Project and publication of a 5% sample, the computation of immigrant shares on the MSA level for the 1960 Census has recently become feasible. We did not use the 1950 Census, where MSA

results when we trim an additional 4 percent from the bottom of the wage distribution in column (3). Other choices related to the construction of our variables, such as the use of current or lagged population as denominator when measuring the immigrant inflow rate, yield similar results and are available from the authors by request.

To this point we have weighted both small and large MSAs equally in our analysis. Some spatial correlation studies (e.g. Borjas 2006, Card 2009) weight MSAs by population, however. Solon, Wooldrige, and Haider (2015) note that the justification for weighting by absolute populations is not clear, as it may neither help in the estimation of population-average causal effects nor increase efficiency.[34] In column (4), we present results where we again use 1960 as the base period for our instruments, but weight the regressions by the population. This does somewhat reduce the standard errors, but also reduces both the conventional IV estimates in Panel A and the double instrument results in Panel B, such that none of the estimates are statistically significant. More appropriate may be to weight using the log of population, as we do in column (5), because the variance of the dependent variable declines approximately linearly in this quantity. Here the results are nearly identical to the unweighted results in column (2). We conclude that (properly) weighting makes little difference to the results.

A further concern is different industry structures across MSAs leads to a potential correlation between the past settlement instrument and changes in local labor demand from industry-specific or sectoral demand shifts. In column (6) we include as a control variable a Bartik (1991) shifter to control for local wage changes as predicted by the lagged 2-digit industry composition. The results change little as do those that include the local manufacturing or other

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definitions are less comparable with later years.

[34] Since all but three MSAs in our analysis have populations above 100,000, individual-level uncertainty is unlikely to be an important factor in our sample, and heteroscedasticity of the error term with respect to population size appears limited. We do use weights in the commuting zone analysis in Appendix Tables A.2 and A.3, as many commuting zones have quite small populations.

industry shares, which are not shown but are available from the authors by request.[35] Controlling for Census division fixed effects in column (7), which would net out region-specific industry trends, only strengthens both the first stage and second stage effects. Because our wage measure already is net of Census division fixed effects, the difference between column (2) and column (7) is solely due to controlling for region-specific trends in the regressors.[36]

Our results suggest that the estimated short-term effect of immigration is substantially more negative once we control for the adjustment to previous immigrant inflows and that they are generally robust to common specification choices. They support our core argument that estimates based on the conventional shift-share instrument are upwardly biased estimates of the short-run effect, arising from the high correlation between current and past immigrant inflows.

Second-stage Results: Heterogeneity Across Subgroups

The distributional consequences of immigration are a common concern (Borjas, Freeman, and Katz 1992, Jaeger 1996, Card 2009). Immigrant inflows are not uniformly distributed across skills, and the effects on natives are likely to be concentrated in those skill groups that more directly compete with immigrant arrivals. In the U.S., immigration had a bigger effect on labor supply at lower skill levels (Jaeger 1996), in particular once we take into account that new arrivals tend to work in systematically less skilled occupations than natives with the same observed education and experience levels (e.g. Borjas 1985, Dustmann 1993).[37] With regard to

[35] A particular concern could be the large swings of prices and wages in the oil industry. While its local employment share is a highly significant predictor, it does not have an important effect on the coefficients on immigrant inflows.

[36] We present second-stage results using Commuting Zones in Appendix Table A.3. The pattern of coefficients is generally comparable to those using MSAs, although they are estimated less precisely.

[37] Dustmann, Schönberg and Stuhler (2016) impute the *effective* skills of U.S. immigrants based on their observed distribution across occupation-wage cells. While immigrant arrivals in the 1970s had similar observed skills as

our model, we would also be concerned if we estimated the largest impact on wages among workers who are less likely to face labor market competition from immigrants.

We report IV estimates of the impact of immigration on native's wages in the 1970s for various subgroups using our double instrument procedure in Table 7. In all results, we use 1960 as the base year for constructing the instruments. For comparison, the first row repeats our estimate for all workers from Table 6. In the second row, we restrict the sample only to male workers, which yields point estimates that are similar to those for all workers, but are statistically significant only at the 10 percent level (p-value=0.053). In the third and fourth rows, we stratify by education and find that the short-term impact on wages is greater for natives with a high school degree or less and in rows and in rows 5 through 7 we find that young workers are most affected. Focusing on young and less educated workers in row 8, the estimated short-run impact is even higher.

While we do not want to emphasize any of the point estimates as representing a definitive estimate of the impact of immigration, the overall pattern of results is consistent with the expectation that we should see the greatest impact on wages in those groups in which immigrants are most prevalent. By isolating recent immigrant arrivals from previous inflows we use a substantially narrower source of variation than the previous literature, and some estimates are relatively imprecise. These results provide some support, however, that our empirical strategy captures the short run impact of immigration and not other local shocks that happen to have a similar spatial distribution.

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natives, their effective skills are substantially lower (results available from the authors).

VII. Conclusions

Estimating the impact of immigration is notoriously one of the most difficult exercises in empirical economics. Immigrants' locational choices are not random, and the economy may adjust in many different ways to a change in local factor supplies. To establish causal identification in spite of these issues, many of the existing studies of the short-term wage response use the past-settlement instrument, a shift-share instrument that combines national inflows with the locational patterns of immigrants in a previous period. We showed that this approach is unlikely to identify a well-defined causal effect of interest when there is only limited change in the country-of-origin composition of immigrant inflows at the national level. In such a setting, the inflow rates of immigrants across cities will tend to be highly serially correlated. In recent decades in the U.S., the rates have been nearly perfectly correlated, with the same cities repeatedly receiving large inflows. As a consequence, the shift-share instrument predicts not only recent arrivals, but is also a great (and often better) predictor for arrivals in a previous decade.

The conventional IV estimator does then not only capture the short-term response to recent immigrant arrivals, but also the longer-term adjustment processes that such arrivals may trigger. This compound effect is hard to interpret. How the estimator weights the short- and longer-term wage response will differ across applications, as the correlation of the instrument with its lag will differ. The longer-term estimates of the response of local wages itself is hard to interpret, as it may reflect spatial adjustment processes that eventually affect also "control" areas that were not directly exposed to immigrant inflows.

The greatest strength of the past-settlement instrument, its ability to predict current flows to local labor markets, is possibly also its greatest weakness. In some sense, if the instrument is "too strong," it is difficult to believe that it can plausibly separate the exogenous from the endogenous variation in the actual immigrant inflows. The flipside of this argument is that the prospects to satisfy the exclusion restriction may be better in settings in which the first-stage link between past settlements and inflows is weaker because the source countries of these inflows has been less stable over time, as is for example the case in many European countries.

To address these issues systematically we proposed a revised estimation procedure, which isolates the variation in local immigrant inflows that is uncorrelated to inflows in the previous period. The "double instrumentation" procedure captures and separates both the initial wage response, and the longer-term adjustment of local relative wages to immigrant inflows. The idea to decompose immigrant inflows by origin groups rather than considering the overall inflow (Card, 2001) is crucial for this strategy. While this decomposition has – in our data – little effect on the conventional IV estimator, it allows us to isolate innovations in local immigrant inflows that are caused by compositional changes at the national level.

Our proposed approach places a substantial demand on the data, as the two instruments will typically be highly collinear. In the U.S. in recent decades there are not sufficient innovations in the location choices of immigrants to distinguish the short and long-term response. Only in the 1970s did we find a sufficient change in the composition of immigrant inflows to allow us to apply our revised estimator. The Immigration and Nationality Act of 1965, which changed the U.S. from a quota-based system to one based on family reunification and employment, substantially shifted the country-of-origin mix. Our estimates are more negative than many in the previous literature, suggesting that the initial wage impact of immigration is potentially natives large. Our results also suggest, however, that this decline is (mostly) reversed in the next period. Cities that received large (predicted) immigrant inflows in the 1960s, but smaller inflows during the 1970s, tend to experience a relative wage increase. Immigration may thus have little, if any, adverse effect on local wages in the longer run.

There are a number of important caveats for our results. While our findings do demonstrate that the serial correlation in immigrant inflows is highly problematic for reduced-form identification strategies from spatial data, our point estimates are somewhat imprecise because our estimator uses only a fraction of the spatial variation used in previous studies. It remains to be seen if our more specific hypotheses – that the short-term wage impact is more negative than the conventional IV estimator suggests, and the longer-term adjustment effect is positive – can be confirmed in situations in which there is even greater variation in the country-of-origin composition.

Our findings illustrate an intrinsic property of shift-share instruments that can be quite problematic. Shift-share instruments impute local shocks by combining aggregate "shifts" with local "shares" of industry, demographic or other compositions. But these local shares will almost always be highly serially correlated. For shift-share instruments to be valid even in the presence of dynamic adjustment processes, their aggregate components should not be highly serially correlated. In contexts where there are frequent changes or a sudden shock on the national level, shift-share instruments may meet this assumption (as, for example in Autor, Dorn, and Hanson 2013). In others, like the immigration literature, care must be taken to ensure that there is sufficient variation over time for results to be plausibly interpreted as causal effects. The variant of the shift-share methodology that we propose here can then be used to isolate that part of the instrument that indeed constitutes an exogenous shock.

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Appendix A.1: ***The Disequilibrium Response***

Using equations (4) and (8) we can express the wage change in area j in period t as

$$\Delta \log w_{jt} = \Delta \log \theta_{jt} - \alpha m_{jt} + \alpha\gamma\left(\log k_{jt-1}^{*} - \log k_{jt-1}\right),$$

where the term $\log k_{jt-1}^{*} - \log k_{jt-1}$ captures the degree to which the local labor market was in disequilibrium. Using equations (4) and (7) we can iterate the term backwards,

$$\begin{aligned}
\log k_{jt-1}^{*} - \log k_{jt-1} &= \frac{1}{1-\alpha}\log\theta_{jt-1} + m_{jt-1} + (1-\gamma)\left(\log k_{jt-2}^{*} - \log k_{jt-2}\right) \\
&= \frac{1}{1-\alpha}\log\theta_{jt-1} + m_{jt-1} + (1-\gamma)\left(\frac{1}{1-\alpha}\log\theta_{jt-2} + m_{jt-2}\right) \\
&\quad + (1-\gamma)^{2}\left(\log k_{jt-3}^{*} - \log k_{jt-3}\right) \\
&= \cdots \\
&= \sum_{s=0}^{\infty}(1-\gamma)^{s}\left(\frac{1}{1-\alpha}\log\theta_{jt-1-s} + m_{jt-1-s}\right),
\end{aligned}$$

giving

$$\Delta \log w_{jt} = \Delta \log \theta_{jt} - \alpha m_{jt} + \alpha\gamma\sum_{s=0}^{\infty}(1-\gamma)^{s}\left(\frac{1}{1-\alpha}\log\theta_{jt-1-s} + m_{jt-1-s}\right).$$

Appendix A.2: ***The Disequilibrium Response with Anticipation***

Topel (1986) explores the idea that labor markets adjust in anticipation (concurrently or even before a demand or supply shift actually occurs). It is difficult to judge how sophisticated expectations are or how strongly households and firms may respond to them. Immigrant arrival rates across cities in the U.S. have been so stable and predictable that some degree of anticipation seems likely. Eberts and Stone (1992) argue, however, that the assumption of households moving years in advance of an anticipated demand shocks (as in Topel 1986) is not realistic and firms and workers may not even respond at all.

We consider two cases here that, together with our baseline case in which anticipation plays no role, may perhaps bound the truth. In the first version, the expected inflow of migrants equals the current rate, i.e. $E[m_{jt+1}] = m_{jt}$. In the second version, agents combine the observed composition of immigrants in their city with a correct forecast of the national inflow in the next period, i.e. $E[m_{jt+1}] \cong \widetilde{m}_{jt+1}$. In the first model agents are naïve, simply extrapolating from the current to the next period. In the second they predict as well as an econometrician armed with (*ex post*) Census data.

If the capital-to-labor ratio responds similarly to anticipated and realized shocks, then the error correction model changes from equation (8) to

$$\log k_{jt} = \log k_{jt-1} - m_{jt} + \gamma\left(\log k^{*}_{jt-1} - \log k_{jt-1} - E[m_{jt}]\right). \qquad (8')$$

With "naïve" expectation $E[m_{jt+1}] = m_{jt}$ this would not affect the probability limit given in equation (11), but equation (13) would change to

$$\operatorname{plim} \widetilde{\beta}^{IV}_{1|t=2} = \beta_1 + \ \ldots \ + 2\gamma\alpha \frac{\operatorname{Cov}(\widetilde{m}_{j2}, m_{j1})}{\operatorname{Cov}(\widetilde{m}_{j2}, m_{j2})} \qquad (13')$$

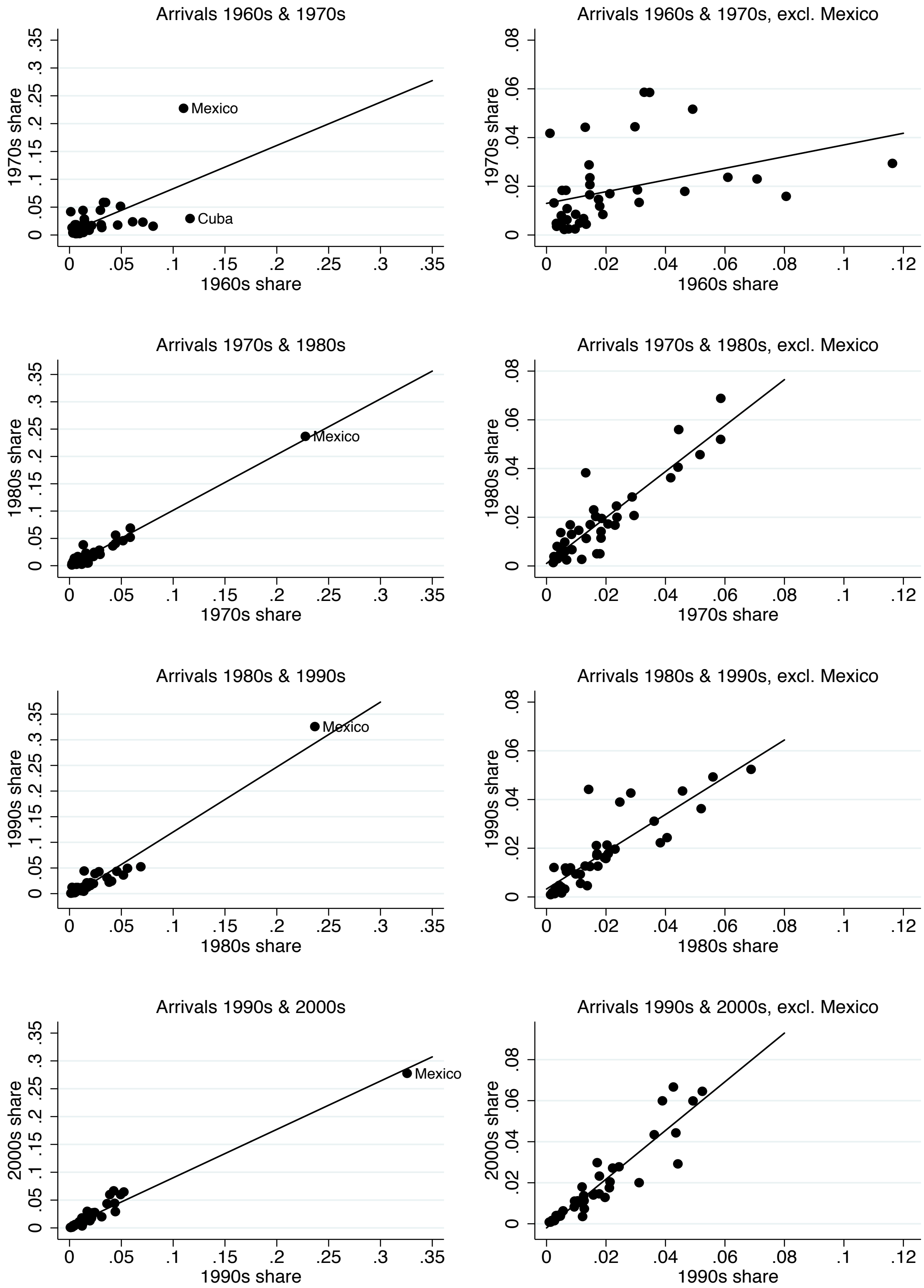
The bias from a response to the supply shock is now twice as large, because the capital-labor ratio responds both to the immigrant inflow in t=1 as well as to the expected inflow in t=2, and the latter is equal to the former. With the "sophisticated" expectation $E\left[m_{jt+1}\right] = \widetilde{m}_{jt+1}$, already the estimates in t=1 would be affected, and equation (13) would instead change to

$$\text{plim}\tilde{\beta}_{1|t=2}^{IV} = \beta_1 + \ \dots \ + \gamma\alpha \frac{\text{Cov}(\widetilde{m}_{j2}, m_{j1})}{\text{Cov}(\widetilde{m}_{j2}, m_{j2})} + \gamma\alpha \qquad (13'')$$

The bias is similar in both anticipation models if $\text{Cov}\left(\widetilde{m}_{j2}, m_{j1}\right) \approx \text{Cov}\left(\widetilde{m}_{j2}, m_{j2}\right)$. Extending these arguments to a generic period t shows that under either anticipation model, the bias term is largest in the period after a structural break in the distribution of immigrants occurs – in our setting, the 1980s – as the response to the unexpected immigrant inflow in the previous period coincides with the response to updated beliefs about their distribution in the future.

Figure 1

Interdecadal Correlation of Composition of Immigrant Arrivals to the U.S.



Note: Authors' calculations using U.S. Census (1970-2000) and ACS (2010) data based on 39 countries of origin. Each observation is the share of all newly-arrived immigrants that were born in a specific country.

Table 1
Characteristics of Immigrant Inflows

Variable	1950	1960	1970	1980	1990	2000	2010
National Immigrant Share	0.076	0.056	0.052	0.067	0.087	0.117	0.136
Panel A: Share of Recent Arrivals							
Nation			0.016	0.025	0.037	0.044	0.032
Average MSA			0.014	0.020	0.029	0.037	0.028
Standard deviation across MSAs			0.018	0.022	0.034	0.030	0.019
Coefficient of variation across MSAs			1.31	1.11	1.17	0.81	0.66
Panel B: Share of Recent Arrivals From							
Canada and Europe			0.414	0.173	0.131	0.164	0.117
Mexico			0.110	0.228	0.237	0.326	0.278
Other Latin America			0.258	0.196	0.236	0.207	0.234
Asia			0.168	0.319	0.319	0.261	0.307
Africa/Other			0.049	0.084	0.077	0.042	0.064
Panel C: Serial Correlation in National Composition							
Recent arrivals, 38 origins (excl. Other)				0.59	0.99	0.96	0.98
Recent arrivals, excluding Mexico				0.37	0.95	0.90	0.95
Immigrant stocks, 16 origins (excl. Other)	0.99	0.99	0.94	0.65	0.90	0.97	>0.99

Note: Authors' calculations using U.S. Census (1950-2000) and ACS (2010) data based on 109 MSAs. The column headings refer to the Census year from which the data were taken. Recent arrivals are immigrants who arrived in the decade prior to the Census year.

Table 2
Estimated Impact of Immigration on Natives' Wages

	1970	1980	1990	2000	2010
Panel A: OLS					
Immigrant Inflow Rate	0.120	-0.156	0.452 **	0.173	0.027
	(0.155)	(0.139)	(0.140)	(0.129)	(0.149)
Panel B: 2SLS					
Second stage					
Predicted Immigrant	0.183	-0.342	0.398 **	-0.045	0.017
Inflow Rate	(0.211)	(0.184)	(0.114)	(0.113)	(0.144)
First stage					
Past Settlement	1.121 **	0.686 **	0.976 **	0.629 **	0.749 **
Instrument	(0.216)	(0.132)	(0.175)	(0.114)	(0.058)
First stage R^2	0.819	0.674	0.775	0.655	0.832

Note: Authors' calculations using U.S. Census and ACS data based on 109 MSAs. The column headings refer to the Census year from which the data were taken. The table reports the slope coefficient in a regression of the change in residual log wage on the immigrant inflow rate in the decade preceding each census year. The reference year for past settlement instrument is beginning of the relevant decade. Robust standard errors in parentheses. ** indicates p <0.01, * indicates p <0.05.

Table 3

Correlations in Local Immigrant Inflows

	1980	**1990**	**2000**	**2010**
Panel A: Serial Correlation				
Acutal Inflows	0.82	0.96	0.92	0.96
Past Settlement Instrument	0.70	0.99	0.96	0.99
Panel B: Cross-Sectional Correlation of Immigrant Inflows and Instruments				
Correlation of Inflow with				
Instrument base period t-1	0.82	0.88	0.81	0.91
Instrument base period t-2	0.73	0.69	0.68	0.78
Correlation of Past inflow with:				
Instrument base period t-1	0.62	0.96	0.93	0.95
Instrument base period t-2	0.51	0.81	0.81	0.83
Panel C: Serial Correlation by Skill Group				
Actual inflows				
High skilled	0.79	0.95	0.94	0.97
Low skilled	0.81	0.95	0.88	0.93
log(High skilled/Low skilled)	0.62	0.80	0.76	0.73
Past Settlement Instrument				
High skilled	0.70	0.97	0.98	0.99
Low skilled	0.72	0.98	0.98	0.99
log(High skilled/Low skilled)	0.88	0.95	0.99	0.99

Note: Authors' calculations using U.S. Census and ACS data based on 109 MSAs. The column headings refer to the Census year from which the data were taken. Each entry is a pairwise correlation across 109 MSAs. Panels A (all immigrants) and C (subgroups and ratios) report the serial correlations in actual inflows and in the past settlement IV. Panel B shows the correlation between the IV and the inflow it is supposed to predict, with that between the IV and the previous inflow. Low skilled are workers with at most a high school degree. High skill workers are those with more than a high school degree. Base period t-1 and t-2 mean that the instrument is constructed using the immigrant distribution in first and second decades prior to the observation year, respectively.

Table 4

Estimated Impact of Immigration on Natives' Wages Controlling for Lagged Inflows

	1980	1990	2000	2010
Second Stage				
Immigrant Inflow Rate	-0.840 *	3.413	0.662	0.116
	(0.395)	(7.077)	(0.643)	(0.774)
Lagged Predicted Inflow Rate	0.619 *	-4.427	-0.483	-0.056
	(0.282)	(10.240)	(0.433)	(0.433)
First Stage				
Past Settlement Instrument	0.415 **	-0.190	-0.557	1.049 *
	(0.115)	(0.456)	(0.288)	(0.400)
Partial R^2	0.367	0.004	0.092	0.158
First stage F	13.10	0.172	3.738	6.689

Note: Authors' calculations using U.S. Census and ACS data based on 109 MSAs. The column headings refer to the Census year from which the data were taken. The table reports the slope coefficient in a 2SLS regression of the change in residual log wage on the decadal immigrant inflow rate, including the lagged past settlement instrument as a control variable. The partial R^2 measures the correlation between the immigrant inflow rate and the instrument after partialling out the effect of the control variable. Robust standard errors in parentheses. ** indicates p<0.01, * indicates p<0.05.

Table 5

Double Instrumentation: First Stages

	(1)	(2)	(3)	(4)
Census Year:	1980	1990	2000	2010
IV base period:	1970	1980	1990	2000
First stage for immigrant inflows				
Past settlment instrument	0.415 **	-0.190	-0.557	1.049 *
	(0.115)	(0.456)	(0.288)	(0.400)
Lagged past settlement instrument	0.325 **	1.221 *	1.211 **	-0.303
	(0.048)	(0.510)	(0.265)	(0.415)
F-statistic	166.2	15.15	107.9	103.7
Sanderson-Windmeijer *F*-statistic	47.68	6.05	1.11	0.85
First stage for lagged immigrant inflows				
Past settlement instrument	-0.098 *	0.268	-0.502	1.339 *
	(0.047)	(0.163)	(0.279)	(0.513)
Lagged past settlment instrument	0.719 **	0.376 *	1.43 **	-0.157
	(0.017)	(0.173)	(0.241)	(0.534)
F-statistic	5261.0	53.32	532.5	175.7
Sanderson-Windmeijer *F*-statistic	153.46	6.35	1.10	0.87

Note: Authors' calculations using U.S. Census and ACS data based on 109 MSAs. The column headings refer to the Census year from which the data were taken. For comparability the lagged instrument is rescaled by the mean of the current instrument. Robust standard errors in parentheses. ** indicates p<0.01, * indicates p<0.05.

Table 6

Estimated Impact of Immigration on Natives' Wages for the 1970s: Double Instrumentation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IV base period	1970	1960	1960	1960	1960	1960	1960
Notes:			Trim Bottom 5% of Wages	Weight: Population	Weight: log(Population)	Bartik Control Var.	Division Fixed Effects
Panel A: 2SLS							
Immigrant Inflows	-0.342	-0.430 *	-0.440 *	-0.193	-0.407 *	-0.454 *	-0.684 *
	(0.184)	(0.199)	(0.180)	(0.117)	(0.195)	(0.205)	(0.279)
Panel B: 2SLS using Double Instrument Procedure							
Immigrant Inflows	-0.719 *	-0.898 **	-0.850 **	-0.406	-0.869 **	-0.941 **	.1.591 **
	(0.291)	(0.314)	(0.271)	(0.263)	(0.315)	(0.330)	(0.518)
Lagged Immigrant Inflows	0.515 *	0.687 **	0.602 **	0.308	0.669 **	0.714 **	1.123 **
	(0.202)	(0.239)	(0.199)	(0.232)	(0.240)	(0.260)	(0.356)
Panel C: Reduced Form							
Past settlement instrument	-0.349 **	-0.382 **	-0.346 **	-0.316 *	-0.381 **	-0.400 **	-0.517 **
	(0.116)	(0.107)	(0.093)	(0.128)	(0.108)	(0.116)	(0.187)
Lagged past settlement instrument	0.207 **	0.323 **	0.320 **	0.233	0.325 **	0.331 *	0.385 **
	(0.089)	(0.117)	(0.105)	(0.157)	(0.116)	(0.128)	(0.176)

Note: Authors' calculations using 1980 U.S. Census based on 109 MSAs. The dependent variable is the change in residual log wages by MSA between the 1970 and 1980 Census. In column (3), the bottom bottom 5% of wages are trimmed. In column (4) observations are weighted by lagged total population in the MSA. In column (5) observations are weighted by the lagged log population. In column (6) observations include a "Bartik" variable to control for changes in industry composition (see text). Column (7) includes Census division fixed effects. Robust standard errors in parentheses. ** indicates p <0.01, * indicates p <0.05.

Table 7
Estimated Impact of Immigration on Natives' Wages for the 1970s, Subgroups:
Double Instrumentation

	Imm. Inflows		Lagged Imm. Inflows	
Subgroup	Coeff.	Std. Err.	Coeff.	Std. Err.
All	-0.898 **	0.314	0.687 **	0.239
Male	-0.754	0.394	0.516	0.297
Education				
High School or Less	-0.980 **	0.350	0.705 **	0.268
More than High School	-0.618	0.422	0.615	0.431
Age				
30 or Younger	-1.146 **	0.436	1.026 **	0.325
31-50	-0.615 *	0.278	0.412	0.213
51-64	-0.743	0.644	0.532	0.462
30 or Younger and Low Skilled	-1.313 *	0.561	1.042 *	0.412
Wage Quantiles				
10th	-1.071	0.730	0.726	0.732
25th	-1.377 *	0.596	0.835	0.495
75th	-0.660 **	0.221	0.126	0.293
90th	-0.394	0.282	-0.214	0.382

Note: Authors' calculations using 1980 U.S. Census based on 109 MSAs. . The dependent variable is the change in residualized mean (columns 1-8) or percentile (columns 9-12) of log wages between the 1970 and 1980 Census. Low skilled are workers with at most a high school degree. High skill workers are those with more than a high school degree. Estimation by 2SLS. Base period is 1960 for both instruments. Robust standard errors in parentheses. ** indicates p<0.01, * indicates p<0.05.

Table A.1

Publications using the Past Settlement Instrument

Authors	Year	Journal	Outcome
Altonji and Card	1991	Book chapter	Native labor market outcomes
Card and DiNardo	2000	AER: P&P	Internal migration
Card	2001	JOLE	Internal migration, labor market outcomes
Fairlie and Meyer	2003	JOLE	Native self-employment
Dustmann, Fabbri and Preston	2005	Economic Journal	Native labor market outcomes
Hatton and Tani	2005	Economic Journal	Internal migration
Ottaviano and Peri	2005	JoUE	Native wages and employment
Ottaviano and Peri	2006	J. of Econ. Geography	Native wages and housing market
Reed and Danziger	2007	Am. Econ. Review	Native labor market outcomes
Saiz	2007	JoUE	Housing market
Cortes	2008	J. Political Econ.	Prices (goods and services)
Frattini	2008	mimeo	Prices (goods and services)
Kugler and Yuksel	2008	mimeo	Native labor market outcomes
Peri and Sparber	2009	AEJ: Applied	Task specialization
Card	2009	AER: P&P	Native labor market outcomes
Iranzo and Peri	2009	REStat	Schooling externalities and productivity
Hunt and Gauthier-Loiselle	2010	AEJ: Macro	Innovation
Furtado and Hock	2010	AER: P&P	Fertility
Boustan	2010	Quarterly J. of Econ.	Residential segregation
Kerr and Lincoln	2010	JOLE	Science and engineering, patenting
Cortes and Tessada	2011	AEJ: Applied Econ.	Labor supply, household work and services
Lewis	2011	Quarterly J. of Econ.	Investment in automation
Gonzalez and Ortega	2011	Labour Econ.	Labor market outcomes
Farré, Libertad and Francesc	2011	B.E. J. Econ. A&P	Female labor supply
Cortes and Tessada	2011	AEJ: Applied	Female labor supply
Cascio and Lewis	2012	AEJ: Policy	Residential and school segregation
Beaudry and Green	2012	Econometrica	Wage determination
Bianchi, Buonanno, and Pinotti	2012	J. Eur. Econ. Ass.	Crime
Smith	2012	JOLE	Youth employment
Wozniak and Murray	2012	JoUE	Population, internal migration
Hunt	2012	Working Paper	Educational attainment
Peri	2012	REStat	Productivity (TFP)
Malcho-Moller, Munch and Skaksen	2012	Scan. J. Econ.	Firm-level wages
Dustmann, Frattini and Preston	2013	ReStud	Native labor market outcomes
Lafortune	2013	AEJ: Applied	Marriage market
Ottaviano, Peri and Wright	2013	Am. Econ. Review	Native labor market outcomes
Monras	2013	Working Paper	Native labor market outcomes
Bell, Fasani and Machin	2013	REStat	Crime
Facchini, Mayda and Mendola	2013	Working Paper	Native labor market outcomes
Amuedo-Dorantes, Sevilla	2014	J. Human Res.	Parental time investment
Cortes and Pan	2014	J. Health Econ.	Supply of native nurses
Aydemir and Kirdar	2014	Working Paper	Native labor market outcomes
Llull	2014	Working Paper	Native labor market outcomes
Piyapromdee	2014	Working Paper	Native labor market outcomes, welfare
Ganguli	2015	JOLE	Knowledge diffusion
Orrenius and Zavodny	2015	JOLE	Educational choices
Amior	2015	Working Paper	Native labor market outcomes
Del Carpio, Özden, Testaverde, Wagner	2015	Scan. J. Econ.	Native labor market outcomes
Dustmann and Glitz	2015	JOLE	Firm adjustment
Özden and Wagner	2015	Working Paper	Native labor market outcomes
Machin and Muprhy	2015	Working Paper	Higher education
Chalfin	2015	AER: P&P	Crime
Ottaviano, Peri and Wright	2015	Working Paper	Firm-level trade of services
Forlani, Lodigiani and Mendolicchio	2015	Scan. J. Econ.	Female labor supply
Cattaneo, Fiori and Peri	2015	J. Human Res.	Native labor market outcomes
Kasy	2015	JoUE	Location choices with social externalities
Sharpe	2015	PhD Thesis	Housing market
Ransom and Winters	2016	Working Paper	STEM education and employment
Fernandez-Huertas, Ferrer and Saiz	2016	Working Paper	Residential segregation
Fassio, Kalantaryan and Venturini	2016	Working Paper	Productivity
Foged and Peri	2016	AEJ: Applied	Native labor market outcomes
Fulford, Petkov and Schiantarelli	2017	Working Paper	Ancestry composition and county GDP

Note: The table lists publications that use a version of the past settlement instrument and their outcome of interest. JOLE=*Journal of Labor Economic* s, JoUE=*Journal of Urban Economics* , AEJ=*American Economic Journal* , REStat=*Review of Economics and Statistics* , ReStud=*Review of Economic Studies* .

Table A.2

Estimated Wage Impact of Immigration, Commuting Zones

Panel A: OLS	1980	1990	2000	2010
Imm. inflow rate	-0.210**	0.605**	-0.0138	0.0599
	(0.077)	(0.094)	(0.107)	(0.115)

Panel B: 2SLS	1980	1990	2000	2010
Imm. inflow rate	-0.315**	0.595**	-0.222	0.0597
	(0.095)	(0.089)	(0.170)	(0.091)
First stage	0.782**	1.017**	0.602**	0.678**
	(0.166)	(0.057)	(0.115)	(0.082)
R-squared	0.663	0.891	0.714	0.823
F-statistic	22.28	318.4	27.28	68.53

Note: Based on U.S. Census data and 741 Commuting Zones. The table reports the slope coefficient in a regression of the change in residual log wage on the immigrant inflow rate in the decade preceding each census year. Reference year for past settlement instrument is beginning of decade. Observations weighted by lagged total population. Robust standard errors in parentheses, ** p<0.01, * p<0.05.

Table A.3

Double Instrumentation: Second Stage, Commuting Zones

	(1)	(3)	(6)	(7)
IV base period	1970	1970	1970	1970
		trim 5%	Bartik	region FE
Panel A: 2SLS				
Immigrant Inflows	-0.294*	-0.312**	-0.318*	-0.520**
	(0.134)	(0.118)	(0.140)	(0.192)
Panel B: 2SLS w/ Double IV				
Immigrant Inflows	-0.416	-0.388*	-0.447	-0.889*
	(0.219)	(0.178)	(0.240)	(0.371)
Lagged Immigrant Inflows	0.197	0.123	0.208	0.471
	(0.164)	(0.123)	(0.194)	(0.267)
Panel C: Reduced Form				
Immigrant Inflows	-0.196**	-0.183**	-0.212**	-0.291**
	(0.0588)	(0.0541)	(0.0609)	(0.0695)
Lagged Immigrant Inflows	0.0382	-0.0265	0.0380	0.0649
	(0.0666)	(0.0849)	(0.0642)	(0.0935)

Note: Based on U.S. Census data and 741 Commuting Zones. The dependent variable is the change in residual log wages by commuting zone between the 1970 and 1980 Census. All regressions include lag log population as control variable and are weighted by lagged total population. Bottom 5% of wages trimmed in column (2). Columns (3) and (4) include a Bartik IV or Census Division fixed effects as control variables. Robust standard errors in parentheses, ** $p<0.01$, * $p<0.05$.