SHIFT-SHARE INSTRUMENTS AND DYNAMIC ADJUSTMENTS:
THE CASE OF IMMIGRATION

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Abstract

A prominent class of instruments combines local shares with aggregate shifts. We demonstrate that estimates using these "shift-share" instruments are inherently vulnerable to bias from dynamic adjustments to past shocks. To illustrate, we show that estimates of the partial equilibrium impact of immigration on wages employing a frequently-used shift-share instrument are unlikely to identify a causal effect. We propose a dynamic variant of the shift-share estimator that yields consistent and substantially more negative estimates than those using the conventional instrument. Our results are a cautionary tale for a large body of empirical work relying on shift-share instruments for causal inference.

A prominent class of instrumental variables combines local economic shares, usually at some point in the past, with current aggregate shifts to predict variation in an endogenous variable of interest. In a quest for better identification, such "shift-share" instruments have become popular in numerous empirical fields in economics, introducing spatial or other forms of cross-sectional variation to literatures that traditionally relied on time-series analysis.[1] In this paper we show that although shift-share instruments may address static sources of endogeneity, they are inherently vulnerable to identification issues in the presence of dynamic adjustments and a return to equilibrium in the outcome in question. Structural breaks in the components of the instrument that would reduce this concern are unfortunately rare in some of the classic contexts in which the shift-share approach is employed, such as the analysis of labor supply or demand shocks.

One of the most prevalent applications of shift-share instruments is in the estimation of the impact of immigration, where they have been used in dozens of publications in leading journals. Much of this literature relies on spatial variation in immigrant inflows to identify the short-term (i.e. partial equilibrium) effect. Immigrants are likely to locate in areas with favorable labor market conditions, however, and this endogeneity is often addressed with an instrument that interacts the composition of immigrant inflows at the national level with the lagged geographic distribution of immigrants (Altonji and Card 1991, Card 2001). Despite a proliferation of studies, the use of this shift-share, or "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, Card and Peri 2016). Estimates of immigrants' impact on wages that rely only on this instrument tend to be less negative than those from the factor proportions or quasi-experimental approaches (see Monras 2015, Dustmann, Schönberg, and Stuhler 2017, and Llull 2018 for recent examples) and are more variable (Dustmann, Schönberg,

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[1] The classic reference is Bartik (1991), who combines the local industry composition with national changes in employment across industries to isolate local labor demand shocks. 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 (de Roux *et al.* 2017, Piveteau and Smagghue 2017), automatization of routine tasks (Autor and Dorn 2013), and robotization (Acemoglu and Restrepo 2017, Graetz and Michaels 2017). See Goldsmith-Pinkham, Sorkin, and Swift (2018) for additional examples.

and Stuhler 2016), changing sign even when applied to different time periods within the same country (Borjas 1999).

Using a simple theoretical model, we show that the variability of estimates in the spatial correlation approach stems partly from the interplay of two factors. First, local shocks may trigger general equilibrium adjustments that gradually offset their local impact, with a period of positive wage growth following the negative initial effect. Second, the country of origin composition and settlement patterns of immigrants are often persistent, with the same cities repeatedly receiving large inflows. As a consequence, the spatial correlation approach may conflate the (presumably negative) partial equilibrium wage impact of recent immigrant inflows with the (presumably positive) local labor market adjustment to previous immigrant supply shocks. The resulting dynamic adjustment bias can dominate the estimated impact of immigration on wages, resulting in a sign reversal and a positive coefficient on immigrant inflows.[2] We demonstrate that this bias is proportional to the degree of serial correlation in the conventional past settlement instrument.

Our theoretical framework suggests that labor market adjustment processes are a function of lagged immigrant inflows, leading to an omitted variable bias in the conventional instrumental variable estimator. We show that this bias can be addressed by including lagged immigrant inflows in the model and instrumenting for them with a lagged version of the conventional instrument. By instrumenting both current and past immigrant inflows with versions of the past settlement instrument that vary only in their national components, we isolate the variation in inflows that is uncorrelated with current local demand shocks as well as the adjustment to past supply shocks. Because only periods with substantial innovations in the country of origin composition provide sufficient variation to distinguish the effects of current and past inflows, causal inference about the partial equilibrium effect of immigration may be much more difficult than the existing spatial correlation literature would suggest.

We illustrate these issues using decadal data from the U.S. Census and American Community Survey and estimate the impact of immigration on natives' wages in each decade from the 1970s to the 2000s. Because the country of origin mix of the inflow of immigrants to the U.S. is so similar over time, the correlation across metropolitan areas between the usual shift-share

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[2] See Imai and Kim (forthcoming) for a general discussion of biases resulting from dynamics in longitudinal data. We focus instead on serial correlation in the treatment of interest, and how the shift-share methodology can aggravate biases due to adjustment to past shocks.

instrument and its lag is extraordinarily high (between 0.96 and 0.99) from the 1980s onward. Consistent with the implications of this result from our theoretical model, we find that OLS and conventional IV estimates of the short-term impact of immigration vary considerably across decades and are sometimes positive. These results have no clear interpretation, however, because they conflate the partial equilibrium impact of immigration with the adjustment to past immigrant inflows.

Using our dynamic shift-share procedure, we estimate that, following an extraordinary break in the country-of-origin composition in the 1970s, the initial impact of immigration on natives' wages in that decade is more negative than estimates based on the conventional shift-share instrument alone would suggest. We find that the estimated impact of the (lagged) immigrant inflow in 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 therefore suggest that areas with large immigrant flows experience a temporary, but not persistent negative impact on local relative wages. The estimated partial equilibrium 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 estimated adjustment to previous inflows indicates strong but gradual general equilibrium responses.

We show that immigrant inflows to the U.S. after 1980, in contrast, do not permit consistent estimation of their short-run impact. The extraordinarily high multicollinearity between the instrument and its lag leads to underidentification of the first stage equations, making it impossible to separate the partial equilibrium response to current immigrants from the adjustments to previous immigrant arrivals in decadal data. The instrument's impressive ability to predict migration flows is therefore also a potential weakness, and the presence of adjustment processes combined with the high degree of persistence in immigrant locations and countries of origin poses a fundamental identification problem that has not been recognized by the literature. In contrast to our results for the U.S., the exclusion restriction may be more plausible in settings in which the first-stage link is weaker because immigrant inflows have been less stable over time, as is the case in many European countries.

We argue that the shift-share approach is generally vulnerable to these kinds of dynamic adjustment biases. The local "shares" used in the construction of the instrument – such as the demographic or industrial composition of a city – are always highly serially correlated. In the

absence of structural breaks in its aggregate components, shift-share instruments therefore isolate the serially correlated component of the treatment of interest and are unlikely to provide consistent estimates in the static frameworks in which they are commonly employed. While the immigration literature provides a particularly dramatic illustration of the potential consequences, this problem is inherent in the way this class of instruments is constructed and their structure creates its own empirical challenges. This argument is in line with recent research by Adão, Kolesár and Morales (2018), who demonstrate that the regression residuals also may inherit the shift-share structure.

Our framework and solution are relevant for other contexts in which shift-share instruments are employed to address contemporaneous endogeneity issues. In the presence of dynamic adjustments in the outcome and the absence of true structural breaks in their aggregate components, such instruments are unlikely to be exogenous. Our solution, including a lagged endogenous regressor instrumented with a lagged shift-share instrument, solves the identification problem when the national shifts used to construct the instruments are not highly serially correlated. Contexts and periods in which there are sudden innovations at the national level are particularly well-suited to this solution. In other contexts (like immigration since the 1980s in the U.S.) in which the instruments are extremely serially correlated, identification of the partial equilibrium impacts may be extremely difficult or even impossible.

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.[3] 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

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[3] See Peri (2016), Dustmann, Schönberg 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, and Dustmann, Schönberg, and Stuhler 2016).

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, however, and 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.

Following Card (2009), the shift-share instrument can be defined as

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

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 that predates t, 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 and origin groups in M_{ojt^0}/M_{ot^0}. We refer to $\widetilde{m}_{jt}$ 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.[4]

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[4] 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. Wozniak and Murray (2012) and Hunt (2017) remove the area's own inflows from the national inflow rate

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 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.[5] The first part of the table shows 32 papers where labor market outcomes are the primary outcome investigated, while the second part of the table shows 37 papers that examine other incomes. With nearly 70 publications in the last decade alone (and many more not listed here), it is one of the most popular instrumental variables in economics. While most applications focus on questions related to immigration, authors have begun to use the instrument as a convenient way to generate (potentially exogenous) variation in local 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.[6] The instrument is a function of national inflow rates and local immigrant shares and might 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 independent of demand conditions in any particular city.

Two recent papers that were developed in parallel to our own present different perspectives on the source of the exogeneity of shift-share instruments. Goldsmith-Pinkham, Sorkin, and Smith

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to reduce the endogeneity to local conditions.

[5] Most studies listed in Appendix Table 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. Our argument is most relevant for outcome variables that adjust dynamically or even non-monotonically over time.

[6] 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. This distinction is particularly useful with regard to shift-share instruments, which appeal to the notion of externality.

(2018) argue that exogeneity of the instrument derives from exogenous variation in the local "shares." In contrast, Borusyak, Hull, and Jaravel (2018) argue that in many settings, identification using shift-share instruments comes instead from the exogeneity of the aggregate "shifts." To us, it is not obvious whether the "exogeneity of the shares" or the "exogeneity of the shifts" condition is more appropriate in the immigration context.[7] The past settlement instrument has traditionally been motivated based on exogeneity of the aggregate shifts.[8] Only a limited number of aggregate shifts (i.e. origin countries) are observed, however, contrary to Borusyak, Hull and Jaravel's assumption that this number grows with the sample size. The dynamic adjustment bias that we describe would invalidate the past settlement instrument under either the "shifts" or the "shares" perspective (as we show below).

To the best of our knowledge, ours is the first attempt to evaluate the validity of the instrument within a simple model of dynamic labor market adjustment, although various concerns have been expressed previously. Borjas (1999) notes that the exclusion restriction may be violated if local demand shocks are serially correlated, leading to correlation between the immigrant 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 bias if immigrant shares are correlated with the unemployment rate. Amior (2017) 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, Wozniak and Murray 2012, Dustmann, Frattini, and Preston 2013, and Orrenius and Zavodny 2015). We argue instead that estimates using the past settlement instrument conflate the partial equilibrium impact of current immigrant inflows with the local labor market adjustment to past inflows. The instrument almost

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[7] The "exogeneity of shares" condition implies that the vector of local shares could be directly used as a potentially large number of valid instruments. Goldsmith-Pinkham, Sorkin, and Smith (2018) acknowledge, however, that "[u]sing the [local] shares as instruments does, however, rely on a stronger identifying assumption than using the Bartik instrument directly." See also the online discussion at https://blogs.worldbank.org/impactevaluations/comment/5042#comment-5042 for a critical assessment of this approach.

[8] For example, Card (2001) assumes that "… *the total number of immigrants from a given source country who enter the United States is independent of … demand conditions in any particular city.*"

surely violates the exogeneity assumption by being correlated with the dynamic response to previous local shocks. As we show, the common strategy of choosing t^0 to be at a substantially earlier point in time offers no protection against this problem 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 examine concerns and proposed solutions raised in the previous literature, and then turn towards problems that stem from the prolonged adjustment in response to local labor supply shocks.

Consider 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 $w_{jt}/\overline{w_t}$ is the relative wage premium offered by labor market j at time t, $\overline{w}_t = (1/J)\sum_j w_{jt}$ is the unweighted average wage across J areas, and 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, 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. 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

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

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 current local demand conditions. The immigration rate in location j can be expressed as a function of the attraction of previous settlements of immigrants from the same country of origin and of labor market conditions:

$$m_{jt} \approx (1-\lambda) \underbrace{\sum_o \frac{M_{ojt-1}}{M_{ot-1}} \frac{\Delta M_{ot}}{L_{jt-1}}}_{\substack{\text{past settlements} \\ \text{pull}}} + \lambda \underbrace{\frac{w_{jt}}{\overline{w_t}} \frac{1}{J} \frac{\Delta M_t}{L_{t-1}}}_{\substack{\text{labor market} \\ \text{pull}}}, \tag{4}$$

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. The traditional shift-share instrument differs from the first term only by choice of the base period and is clearly correlated with immigrant inflows. If $t^0 = t - 1$, the past settlements pull and the instrument are identical.

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

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

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

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

This optimal capital-labor ratio 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 has been the local adjustment process – in particular the responses of other factors of production – triggered by immigrant-induced local labor supply shocks.[9] The spatial correlation approach inherently estimates the short-run, partial equilibrium impact, and the data will be less likely to uncover an impact of immigration on local wages as the elapsed time between supply shock and measurement increases and general equilibrium adjustments occur (Borjas 1999). Researchers therefore assume that estimates exploiting the spatial distribution of immigrants are biased towards zero (e.g. Borjas 2006, Cortés 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). There is little work on the dynamic response to immigration, but the available evidence also points to prolonged adjustment periods. Cohen-Goldner and Paserman (2011) find that high-skilled immigration to Israel lowers native wages in

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[9] Labor supply shocks may affect capital flows (Borjas 1999) and internal migration (Card 2001, Amior and Manning 2017), but may also affect human capital accumulation (Smith 2012, Hunt 2017), 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. From the perspective of our model, the key issue is that all of these adjustments will be a function of previous immigrant inflows and that the adjustment process is part of the disturbance term in the conventional model.

the short run, but that the effect dies out after 5-7 years. Monras (2015) finds a more rapid response in the U.S. in response to Mexican migration after the 1995 peso crisis, while Braun and Weber (2016), examining post-World War II in-migration to Germany, and Edo (2017), examining wage dynamics during repatriation in France following Algerian independence, document more prolonged adjustment periods lasting a decade or more. Local labor markets appear not to fully adjust even long after other types of shocks (e.g. increased trade with China, see Autor, Dorn, Hanson 2016).

Although the relative importance of the underlying adjustment channels is disputed (e.g. Card 2001, Borjas 2014), the argument that we present in the next section is not specific to any particular mechanism. We therefore 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.[10] 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 shock, will only return to its optimal level over time. The coefficient γ measures the share of necessary adjustments to return to the optimal capital-labor ratio that takes place in the subsequent period. Intuitively, γ measures the amount of the adjustment process to shocks in period $t-1$ that occurs in period t, with larger values of γ indicating that the labor market rebounds more completely. As we use decadal data the assumption that labor markets recover nearly completely in the subsequent decade, i.e. $\gamma \approx 1$, 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.

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

Selection and Dynamic Adjustment

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

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

where β_0 represents the secular growth in wages, and β_1, the impact of immigration-induced labor supply changes on local wages, is the object of interest (which in our model is determined by the capital share of production, i.e. $\beta_1 = -\alpha$). Spatial factor mobility will arbitrage away regional wage differences in the long run (LaLonde and Topel 1991) and the object of interest in the spatial correlation literature is the short-run, partial equilibrium response in wages before spatial adjustments occur (Borjas 1999).[11] The quantity in square brackets is unobserved to the econometrician. We will assume that ϵ_{jt} is orthogonal to m_{jt} for all j and t. The "conventional" IV estimator is given by the instrumenting m_{jt} in equation (9) with the past-settlement instrument, $\widetilde{m}_{jt}$.

The first term in brackets 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, OLS estimates of β_1 are presumed to be upward biased estimates of the true short-term impact. The literature largely focuses on how the past settlement instrument, $\widetilde{m}_{jt}$, addresses this selection problem.[12] The instrument will address the selection problem if demand shocks are unrelated to the initial distribution of immigrants used

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[11] Because the spatial correlation approach is fundamentally about partial equilibrium, it cannot be informative about the long-run overall impact of immigrants in the economy. By comparing wages in one area relative to another, however, we can examine if the relative local effect is persistent.

[12] Most of the literature uses first-differenced or fixed-effect specifications (e.g. Dustmann, Fabbri, and Preston 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).

to construct the instrument.[13] 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 demand conditions (as we assume here and as is plausible in our empirical setting) the instrument will be uncorrelated with $\Delta \log \theta_{jt}$.

The IV Estimator with Dynamic Adjustment

We argue that even in the absence of serial correlation in $\Delta \log \theta_{jt}$, however, labor market adjustment can generate endogeneity issues that invalidate the past settlement instrument. The literature has essentially ignored the second component of the disturbance term in equation (9), the dynamic adjustment process. Local labor market shocks 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 will still be ongoing during the subsequent observational period, even at a decadal frequency.[14] If the country of origin distribution of immigrant inflows is highly serially correlated, there is likely to be 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

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[13] Productivity or other labor demand shocks can be serially correlated (Amior and Manning 2017), however, leading to a correlation with the initial distribution of immigrants. The literature has noted this potential 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 used to construct the instrument to minimize its correlation with current demand shifts (e.g. Hunt and Gauthier-Loiselle 2010).

[14] Our analysis focuses on capital adjustments, as these can occur in location j without taking capital away from other areas through international capital flows. Two other channels for adjustment are typically considered: endogenous adjustments in technology (Lewis 2011, Dustmann and Glitz 2015) or internal migration (Filer 1992, Card 2001). Incorporating these channels into the model would not change our general argument, however, as the adjustments would still depend on lagged immigrant inflows in a manner that is similar to equation (10) below.

endeogeneity and conflates the partial equilibrium and local adjustment effects, making it both difficult to interpret and a biased estimator of β_1, the short-term impact of immigration on wages.

We quantify the bias in estimating β_1 using the past settlement instrument by first noting that the labor market adjustment process is a function of all previous immigration and productivity shocks, as shown in Appendix A:

$$\log k_{jt-1}^* - \log k_{jt-1} = \sum_{s=0}^{\infty}(1-\gamma)^s\left(m_{jt-s-1} + \frac{1}{1+\beta_1}\Delta\log\theta_{jt-s-1}\right). \tag{10}$$

Note that the capital stock adjusts completely to technology and immigrant supply shocks only asymptotically as $t \to \infty$. Substituting equation (10) into equation (9) gives

$$\begin{aligned}
\Delta\log w_{jt} = {} & \beta_0 + \beta_1 m_{jt} \\
& + \left[\epsilon_{jt} - \beta_1\gamma\sum_{s=0}^{\infty}(1-\gamma)^s m_{jt-s-1}\right. \\
& \left. + \left(\Delta\log\theta_{jt} - \frac{\beta_1\gamma}{1+\beta_1}\sum_{s=0}^{\infty}(1-\gamma)^s\Delta\log\theta_{jt-s-1}\right)\right]
\end{aligned} \tag{11}$$

where, as in equation (9), the expression in square brackets is assumed to be unobserved to the econometrician. Using the past settlement instrument, $\widetilde{m}_{jt}$, to instrument for m_{jt} and estimating equation (11) by two-stage least squares in a cross section at time t gives

$$\begin{aligned}
\text{plim } \widetilde{\beta}_1^{IV} = {} & \beta_1\left[1 - \gamma\sum_{s=0}^{\infty}(1-\gamma)^s\frac{\text{Cov}(\widetilde{m}_{jt}, m_{jt-s-1})}{\text{Cov}(\widetilde{m}_{jt}, m_{jt})}\right. \\
& + \left(\frac{1}{\beta_1}\frac{\text{Cov}(\widetilde{m}_{jt}, \Delta\log\theta_{jt})}{\text{Cov}(\widetilde{m}_{jt}, m_{jt})}\right) \\
& \left. - \frac{\gamma}{1+\beta_1}\sum_{s=0}^{\infty}(1-\gamma)^s\frac{\text{Cov}(\widetilde{m}_{jt}, \Delta\log\theta_{jt-s-1})}{\text{Cov}(\widetilde{m}_{jt}, m_{jt})}\right].
\end{aligned} \tag{12}$$

The asymptotic bias terms arise from the response of the labor market to past shocks. The first summation in square brackets is the response to immigration-induced supply shocks in the

previous periods. Note that this bias term arises regardless of whether the instrument is motivated from supposed exogeneity of the local shares (Goldsmith-Pinkham, Sorkin, and Smith, 2018) or aggregate "shifts" (Borusyak, Hull, and Jaravel, 2018). Put differently, neither the "exogeneity of the shares" nor the "exogeneity of the shifts" condition will be plausible in the immigration context if the $\text{Cov}(\widetilde{m}_{jt}, m_{jt-s-1})$ terms are not equal to zero (which is an empirical question). The expression in parentheses captures the labor market response to present and past local demand shocks. Both responses raise the marginal productivity of labor and lead to an upward bias in the IV estimate (assuming that β_1 is negative and less than 1 in absolute value).[15]

The terms in parentheses illustrate 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 demand shocks (the first term in parentheses) but also with past demand shocks (the summation term in parentheses). Choosing t^0 to be sufficiently lagged may therefore be advantageous even if the demand shocks themselves are not serially correlated, as we assume in our model. As this is a common strategy in the literature, we assume below that t^0 is sufficiently lagged so that $\widetilde{m}_{jt}$ is uncorrelated with the current adjustment to past demand shocks, i.e. we will assume that the terms in parentheses are equal to zero.

The "dynamic adjustment bias" from lagged supply shocks (the first summation in brackets) is harder to address. Note that we can rewrite the $\text{Cov}(\widetilde{m}_{jt}, m_{jt-s-1})/\text{Cov}(\widetilde{m}_{jt}, m_{jt})$ terms as ratios of the slope coefficients from regressions of lagged and current inflows, respectively, on the current instrument:

$$\frac{\text{Cov}(\widetilde{m}_{jt}, m_{jt-s-1})/\text{Var}(\widetilde{m}_{jt})}{\text{Cov}(\widetilde{m}_{jt}, m_{jt})/\text{Var}(\widetilde{m}_{jt})}$$

This ratio will be small if the instrument predicts current immigrant inflows at time t substantially better than it predicts inflows in the previous periods. As we show below, this is unfortunately

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[15] 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 B we show that when future arrivals are anticipated, the dynamic adjustment 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.

rarely the case in the U.S. context, where this ratio fluctuates around and sometimes exceeds one. The instrument is a good predictor for immigrant inflows in the intended period, but because it is (empirically) highly serially correlated, it is also a similarly good predictor for previous inflows. The bias induced by these quantities is therefore potentially quite large. Lagging t^0 does not address this issue.[16]

The degree of adjustment, γ, may have little influence on the magnitude of the adjustment bias, however, if previous immigrant inflows are highly correlated over time. In the extreme case, if $\text{Cov}(\tilde{m}_{jt}, m_{jt-s}) = \text{Cov}(\tilde{m}_{jt}, m_{jt-1})$ for all $s \geq 1$, and if we ignore the terms involving $\Delta \log \theta_{jt}$, which we have assumed to follow a random walk, then expression (12) simplifies to

$$\text{plim } \tilde{\beta}_1^{IV} = \beta_1 \left[\frac{\text{Cov}(\tilde{m}_{jt}, m_{jt}) - \text{Cov}(\tilde{m}_{jt}, m_{jt-1})}{\text{Cov}(\tilde{m}_{jt}, m_{jt})} \right] \tag{13}$$

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 therefore matter little.[17]

To illustrate the source of the dynamic adjustment bias more concretely, consider the following thought experiment. Imagine that the economy is in a spatial and dynamic equilibrium at some initial period $t=0$ and that immigrants are distributed non-uniformly across labor markets. If immigrant inflows occur at the next period $t=1$, they will be attracted to those labor markets in which the largest share of the initial immigrants from their country of origin live and also those

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[16] Lagging the base period further may reduce the numerators in the ratios $\text{Cov}(\tilde{m}_{jt}, m_{jt-s-1})/\text{Cov}(\tilde{m}_{jt}, m_{jt})$ but, by reducing its ability to predict inflows in period t, also the denominator. In principle, the bias may even be greater if the denominator shrinks more than the numerators. In the recent decades in the U.S., however, the ratio appears to be insensitive to the choice of base period t^0.

[17] What does matter, however, is the assumption that in the long-run equilibrium, immigrant inflows have no persistent effect on local relative wages. Indeed, the spatial correlation approach cannot be used to estimate the long-run overall impact of immigrants in the economy. 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.

areas that experience above-average labor demand shocks. Wages in labor market j will change according to

$$\Delta \log w_{j1} = \beta_0 + \beta_1 m_{j1} + \left[\Delta \log \theta_{j1} + \epsilon_{j1}\right]$$

If the instrument is uncorrelated with current demand shifts, $\Delta \log \theta_{j1}$, the conventional IV estimator will consistently estimate β_1.

In response to the immigrant inflow, wages adjust at $t=2$ according to

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

where the term $\beta_1 \gamma (\log k_{j1}^* - \log k_{j1})$ reflects that local labor markets may still be adjusting to immigrant supply shocks as well as the demand shocks from $t=1$. Using the past settlement instrument, $\widetilde{m}_{j2}$, to instrument for m_{j2} gives

$$\text{plim } \tilde{\beta}_{1|t=2}^{IV} = \beta_1 \left[1 - \gamma \left(\frac{1}{1+\beta_1} \frac{\text{Cov}(\widetilde{m}_{j2}, \Delta \log \theta_{j1})}{\text{Cov}(\widetilde{m}_{j2}, m_{j2})} + \frac{\text{Cov}(\widetilde{m}_{j2}, m_{j1})}{\text{Cov}(\widetilde{m}_{j2}, m_{j2})}\right)\right] \tag{14}$$

As equation (14) makes clear, the bias arising from the adjustment process can by itself cause the IV estimate of the impact of immigration to change from negative to positive if

$$\left[\frac{1}{1+\beta_1} \frac{\text{Cov}(\widetilde{m}_{j2}, \Delta \log \theta_{j1})}{\text{Cov}(\widetilde{m}_{j2}, m_{j1})} + \frac{\text{Cov}(\widetilde{m}_{j2}, m_{j1})}{\text{Cov}(\widetilde{m}_{j2}, m_{j2})}\right] > \frac{1}{\gamma}.$$

Abstracting from any correlation of the instrument with the first period demand shock, we would estimate a positive effect of immigration even if the actual effect was negative if

$$\gamma \frac{\text{Cov}(\widetilde{m}_{j2}, m_{j1})}{\text{Var}(\widetilde{m}_{j2})} > \frac{\text{Cov}(\widetilde{m}_{j2}, m_{j2})}{\text{Var}(\widetilde{m}_{j2})}.$$

We will be more likely to observe a change in sign if more of the adjustment to the first period shocks occurs in the second period (γ is higher) or if the instrument is more highly correlated with past inflows ($\mathrm{Cov}(\widetilde{m}_{j2}, m_{j1})/\mathrm{Var}(\widetilde{m}_{j2})$ is higher), relative to the first-stage coefficient on the instrument.

III.   Addressing the Dynamic Adjustment Bias

Our model illustrates the difficulty of consistently estimating the labor market impact of immigration using the past settlement instrument and suggests that the biases left unaddressed by the instrument are essentially an omitted variables problem. To see this more clearly, we can rewrite equation (11) as

$$\Delta \log w_{jt} = \beta_0 + \beta_1 m_{jt} + \left(\sum_{k=1}^{\infty} \beta_{k+1} m_{jt-k} \right) + \left[\sum_{l=0}^{\infty} \delta_l \Delta \log \theta_{jt-l} + \epsilon_{jt} \right], \qquad (11')$$

where the quantity in square brackets is unobserved and $\beta_{k+1} = -\beta_1 \gamma (1 - \gamma)^{k-1}$ for $k \geq 1$. The adjustment process is a function of lagged immigrant inflows and present and lagged demand shocks. Previous empirical research has omitted the terms in parentheses. But we observe m_{jt-k} at least up to some maximal lag, τ, and can include these in the estimation. Just as m_{jt} is correlated with $\Delta \log \theta_{jt}$, however, each of the m_{jt-k} terms will be correlated with $\Delta \log \theta_{jt-k}$ (at a minimum), which appears in the disturbance term. The same kind of endogeneity issues that affect m_{jt} in equation (11) also therefore affects its lags in equation (11′).

A natural solution to the endogeneity of the m_{jt-k} terms is to instrument for them using lags of the past settlement instrument. As long as the base period, t^0, is sufficiently lagged, the instruments will be orthogonal to all of the demand shocks, $\Delta \log \theta_{jt-k}$, and permit consistent estimation of the β's. In practical terms it is, of course, impossible to include an infinite number of m_{jt-k} terms (and instrument for them), as equation (11′) suggests. The number of lags to include in estimation will depend on the speed of adjustment, γ, as well as data availability, and very likely varies by context (i.e. the frequency of data used, the outcome variable, and the country under

examination). At issue is the time frame in which past shocks can arguably be ignored in equation (11′). Fewer lags may suffice if the included lags are highly correlated with and therefore control for earlier lags, or if γ is large such that a greater share of the adjustment to shocks from period $t-1$ occurs in period t.[18] With higher frequency data, the number of lags to include would surely be higher. Other strategies to address the adjustment bias are likely to be unsatisfactory. As already noted, further lagging t^0 does not address the correlation between the instrument and the lags m_{jt-k}.[19]

As we present results from U.S. data at a decadal frequency, it seems reasonable to limit the number of included lags to 1. In our setting, the included lag will also be approximately sufficient to control for higher order lags because the distribution of country of origin shares has remained so stable in that period. By limiting the inclusion of past immigrant inflows to one lag, our model is now

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

The coefficient β_1, the usual coefficient of interest in the literature, captures the impact of immigration on wages 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.[20] We instrument the two endogenous variables with the two instruments,

$$\widetilde{m}_{jt} = \sum_o \frac{M_{ojt^0}}{M_{ot^0}} \frac{\Delta M_{ot}}{L_{jt-1}} \quad \text{and} \quad \widetilde{m}_{jt-1} = \sum_o \frac{M_{ojt^0}}{M_{ot^0}} \frac{\Delta M_{ot-1}}{L_{jt-2}}, \tag{16}$$

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[18] Imai and Kim (2017) note that the number of lagged treatments is often arbitrarily chosen in dynamic treatment effect settings and rarely justified on substantive grounds. In our context the number of lagged treatments can be motivated based on the presumed speed of local labor market adjustments, or empirically, based on the observed correlation structure of the immigrant supply shocks.

[19] Validity checks that are useful for other reasons, such as whether $\widetilde{m}_{jt}$ is correlated with lagged wage growth (Peri 2016), would not reliably detect the dynamic adjustment problem because the absence of such a correlation is one of the possible consequences when the short-run impact of current immigrant inflows and the longer-term recovery to previous inflows overlap. While testing for parallel pre-trends is useful in a static setting with a one-time treatment, such tests are difficult to interpret in a dynamic setting with repeated shocks. Controlling for past wage growth in the wage regression does not suffice for the same reason.

[20] 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.

in the two first-stage equations,

$$m_{jt} = \pi_{10} + \pi_{11}\tilde{m}_{jt} + \pi_{12}\tilde{m}_{jt-1} + u_{jt} \tag{17}$$

and

$$m_{jt-1} = \pi_{20} + \pi_{21}\tilde{m}_{jt} + \pi_{22}\tilde{m}_{jt-1} + v_{jt} \tag{18}$$

By controlling for m_{jt-1} in equation (15) we address biases introduced by the adjustment process to past immigrant shocks. By instrumenting for m_{jt} and m_{jt-1} with $\tilde{m}_{jt}$ and $\tilde{m}_{jt-1}$ we address the endogeneity of current and past immigrant inflows to current and past labor demand shocks.[21] To avoid a mechanical relationship between m_{jt-1} and $\tilde{m}_{jt}$, that is between the local country-of-origin shares used to construct the instrument at time t and local inflows at time $t-1$, t^0 should be chosen to be strictly prior to $t-1$.[22]

    If $\tilde{m}_{jt}$ and its lag, $\tilde{m}_{jt-1}$, are both constructed using the same base period t^0, 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. The "dynamic shift-share" specification in equations (15) through (18) is therefore quite demanding on the data compared to instrumenting only for current inflows with $\tilde{m}_{jt}$. In periods in which the country of origin composition of migrants changes substantially, the instruments will less correlated with one another and less likely to suffer from the weak instrument problem. Our model also indicates that the dynamic adjustment bias is reduced in settings in which

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[21] It would be possible to transform our model into an autoregressive-distributed lag model to then apply dynamic panel data methods (Bond 2002). This approach is less attractive with low frequency data, however, and we do not observe a sufficient number of lags of the dependent variable for the 1970s. Instead, our model points to a more direct way to address the endogeneity of current and past immigrant inflows.

[22] Goldsmith-Pinkham, Sorkin, and Smith (2018) suggests that the local country-of-origin shares could be used individually as instrumental variables rather than pre-aggregating them into the shift-share instrument $\tilde{m}_{jt}$. This would rely, however, on the rather strong assumption that all country-of-origin shares are exogenous. Put differently, by allowing the first-stage coefficient to vary by origin groups we would trade consistency for efficiency, which does not seem advisable in this application.

the overall rate of immigration has temporarily increased, or where origin-specific push factors change the inflow rate of a particular origin group.[23]

Our dynamic shift-share procedure addresses the dynamic adjustment bias by controlling for its source, lagged immigrant inflows, rather than directly controlling for the individual adjustment channels that contribute to this bias, such as internal migration, inter-city trade, or internal capital flows. There is, unfortunately, little data available on some of these channels. By isolating variation that is uncorrelated with potential confounders, our proposed solution is closer in spirit to the previous literature's approach to the selection problem rather than using a control variable strategy.

IV.   Data and Descriptive Statistics

To demonstrate the problem and 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. 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.[24] 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.

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[23] 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. This was periodically the case in the U.S.(see, for example, Buchardi, et al. 2019)

[24] 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.

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).[25] 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 which finer information cannot be used to make them more consistent.[26] 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 to 64 years of age and have 1 to 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 will show, limited, effect on our estimates. In a procedure similar to e.g. Card (2001), and Card (2009), 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

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[25] Results using Commuting Zones as the geographic unit of observation are shown in Appendix Tables 3 and 4. The definition of commuting zones is based on Tolbert and Sizer (1996), and applied to Censuses using codes provided by Autor and Dorn (2013).

[26] 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.

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

Panel B of Table 1 illustrates changes in the patterns of the country-of-origin distribution, which changed substantially in the 1970s. In addition to push factors like the Cuban Revolution and the Vietnam War that would have affected the origin of refugees, both the enactment in July 1968 of the 1965 Immigration and Nationality Act (Hatton 2015) and the ending of the Bracero agricultural worker program (Massey and Pren 2012) likely changed the ability to emigrate to the U.S. and the incentives for such migration for workers from different countries than had sent migrants previously.[27] 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 in 1968), 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. Since 1970, the country-of-origin distribution has remained highly stable, however, and 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

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[27] 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.

V. 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.[28] 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 instrument as defined in equation (1). The instruments are constructed with t^0 defined as the previous Census year. 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 more negative. The differences are modest, however, and the IV estimate for the 1980s (using the 1990 Census) is still positive and

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

statistically significantly different from zero. The point estimates also differ substantially across the decades.[29] 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 Card's (2001) past settlement instrument, to more recent periods, and to different spatial definitions. While we use decadal data, this variability is unlikely to reflect differences in the timing of immigrant arrivals within each decade, because the share of inflows occurring in the second half of each decade remains quite stable (e.g. 53% for the 1970s vs 50% for the 1980s).

It is only for the 1970s (using the 1980 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 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.[30]

If we limit equation (11′) to having only one lag of immigrant inflows in the components of the disturbance term, we can estimate some of the key components of the dynamic adjustment

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[29] Estimates using Commuting Zones rather than MSAs are presented in Appendix Table 3. and are similar.

[30] While the break in immigrant composition was likely not anticipated (see Hatton 2015, Massey and Pren 2012), worker and firms might have anticipated that the change in composition was permanent, generating a permanent shift in the spatial distribution of arrivals as well. In this case, our argument would 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 B). Because the spatial distribution of immigrants was so similar between the 1970s and 1980s, even naïvely extrapolating from the latter to the former would give quite reasonable predictions of local immigrant inflows.

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 $t - 1$.

Some studies in the literature combine spatial variation in immigrant inflows across areas with their density across skill groups.[31] Depending on the outcome variable of interest, the explanatory variable may be the rate of immigration in a particular education group (Cortés, 2008, Hunt 2017), 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 and the dynamic adjustment problem will therefore also affect empirical strategies that exploit both spatial and skill-cell variation.[32]

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[31] See Peri (2016) or Dustmann, Schönberg, 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.

[32] 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 2011), but differences in local skill-specific wages may be more persistent.

"Dynamic Shift-Share Estimation": Reduced Form and First Stage Results

The dynamic shift-share procedure we introduced to address the bias due to the conventional shift-share instrument with the ongoing adjustment process of the labor market relies on innovations in the instrument between period $t-1$ and t for identification. Periods in which there is little change in the flow variables from period to period will yield instruments that are highly correlated with one another. To gauge the degree of independent information in the two instruments, in Table 4 we present results from reduced form regressions of residualized wages (as in Table 2) on $\widetilde{m}_{jt}$ and $\widetilde{m}_{jt-1}$ as defined in equations (16) for the 1970s through the 2000s. In creating the instruments here, we choose t^0 to be 20 years prior to the Census year in which we observe wages to avoid, as already noted, a mechanical relationship between $\widetilde{m}_{jt}$ and m_{jt-1}, particularly the disturbance term in equation (15), while also keeping the predictive power of the instruments as high as possible. As in all of the regression results in previous tables, we present heteroskedasticity-consistent standard errors.

For each decade, we also report the Kleibergen-Paap (2006) *rk* LM statistic for underidentification, which tests the null hypothesis that the rank of the matrix formed from the coefficient vectors from the first stage regressions is equal to 1 against the alternative that it is equal to 2. That is, the statistic is informative about the degree of linear dependence between the estimated coefficients in equation (14) and those in equation (15) and indicates how different the predicted values from the two first stage regressions will be. Although the Kleinbergen-Paap statistic along with the reduced form coefficients will be informative about the degree to which the second stage is identified, we also report the first stage coefficients and the conventional first stage *F* test (Bound, Jaeger, and Baker 1995) for the joint significance of the instruments in each equation separately.

We find a statistically significant reduced form relationship only in the 1970s, where the instrument has a negative effect on wages and the lagged instrument has a positive effect on wages. None of the coefficients in the reduced form regressions from the other decades are statistically significant, although looking at the first stage regressions individually would not necessarily lead one to conclude that there are identification problems in the 1990s and 2000s, as the first stage *F* statistics are reasonably large and/or the first stage coefficients are statistically significant. As

expected, only in the 1970s do we reject the null hypothesis of underidentification with the Kleinbergen-Paap statistic. The rest of our analysis is therefore focused on estimating the impact of immigration in the 1970s.

"Dynamic Shift-Share Estimation": Second-stage Results

We estimate the impact of immigration in the 1970s, the only decade in which there is enough independent information in the past settlement instrument and its lag to identify the second stage equation, and report our estimates in Table 5.[33] We report different specifications, varying the definition of the outcome variable, the weighting scheme, or the inclusion of control variables in columns (1) to (6). For comparison, we report the conventional (single) IV estimate of the effect of immigrant inflows in Panel A. These results differ from those for the 1970s in Table 2 because, to be consistent with the dynamic shift-share procedure, we construct the instrument by using immigrant shares in 1960 rather than in 1970. We then show the estimates of the effect of immigrant inflows and lagged immigrant inflows on residualized wages using equation (13) 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 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 0.9 log points. This estimate is substantially more negative than the corresponding conventional IV estimate in column (1), Panel A, and the difference is statistically significant. This is consistent with our expectation that estimates that do not control for the adjustment to past immigrant shocks are biased upward. 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

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[33] As in with all regression results in the paper, we report heteroskedasticity-consistent standard errors in Table 5. These may be downward biased, however, because of small samples. 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.

shocks. In absolute terms, this coefficient is nearly as large as the coefficient on current inflows, suggesting that local wages do eventually recover from an immigration-induced supply shock. 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. We get similar results when we trim an additional 4 percent from the bottom of the wage distribution in column (2). 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 (3), we present results where we 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. Because the variance of the dependent variable declines approximately linearly in the log of population, we present results in column (4) that are weighted by this quantity. We get results that are nearly identical to the unweighted results in column (1). We conclude that (properly) weighting makes little difference to the results.

A further concern is that different industry structures across MSAs might lead to a potential correlation between the past settlement instrument and changes in local labor demand from industry-specific or sectoral demand shifts. In column (5) 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 industry shares, which are not shown but are available from the authors by request.[35] Controlling

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[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 heteroskedasticity of the error term with respect to population size appears limited. We do use weights in the commuting zone analysis in Appendix Tables 3 and 4, as many commuting zones have quite small populations.

[35] Other concerns could be the large swings of prices and wages in the oil industry, or the effects of the substantial labor force entry of women and baby boomers through the 1970s. In Appendix Table 5, we show that our estimates change little when controlling for the employment share in the oil industry, the decadal change in female labor force participation, or the decadal change in labor force participation as predicted by the lagged age structure of the local population. It is also worth noting that the two national immigrant inflows considered are similarly distributed within

for Census division fixed effects in column (6), which would net out region-specific wage 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 (1) and column (6) 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 our results are generally robust to common specification choices. They therefore 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. The results further suggest that the short-run (decadal) impact of low-skilled immigration on average wages is negative, but we do not wish to emphasize any particular point estimate. The confidence intervals on most of the estimates are sufficiently wide to be consistent both with the comparatively small wage impact implied by standard factor proportions model, and with the more substantial responses that have been estimated in some empirical studies (see Dustmann, Schönberg, Stuhler, 2016).

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 our model, we would also

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the respective decade: 57% of the 1960s inflow, and 53% of the 1970s inflow, happened in the latter half of the decade.

[36] We present second-stage results using Commuting Zones instead of MSAs in Appendix Table 4. This spatial definition is ill-suited for our purpose and decade of interest, but the pattern of coefficients remains comparable (although they are estimated less precisely). Only about 400 county groups are identified in the 1970 IPUMS Census, more than 50% of which overlap with multiple Commuting Zones. The measured wage change in a Commuting Zone between the 1970 and 1980 Census may therefore reflect changes in its underlying area definition.

[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 natives,

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 6. For comparison, the first row repeats our estimate for all workers from Table 5. 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 with which immigrants compete more strongly. 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 evidence, however, that our empirical strategy captures the short run impact of immigration and not of other local shocks that happen to have a similar spatial distribution.

VI.   Conclusions

Estimating the impact of immigration is notoriously one of the most difficult exercises in empirical economics. Immigrants' location 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 show that this approach is unlikely to identify a

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their effective skills are substantially lower. These results are available from the authors by request.

well-defined causal effect of interest in the presence of dynamic adjustments and serial correlation in the location of immigrant arrivals. In recent decades in the U.S., inflows have been nearly perfectly correlated, with the same cities repeatedly receiving large numbers of immigrants. As a consequence, the shift-share instrument predicts not only recent arrivals but is also an excellent (and often better) predictor for arrivals in the previous decade, which is effectively an omitted variable in the conventional model.

The conventional IV estimator captures then not only the partial equilibrium impact of recent immigrant arrivals, but also the adjustment processes to previous immigrant inflows. This is particularly problematic when studying the impact of immigration on wages, in which this adjustment is likely to have the opposite sign than the initial impact. To address this issue systematically, we propose a revised "dynamic shift-share" estimation procedure that captures and separates both the initial wage response, and the longer-term adjustment of local relative wages to immigrant inflows. Decomposing immigrant inflows by origin groups rather than considering the overall inflow (Card 2001) is crucial for this strategy. In our data, this decomposition has little effect on the conventional (single) IV estimate but our dynamic shift-share procedure 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 relative to the single instrument procedure, as the current and lagged instruments will typically be highly collinear. In the U.S. in recent decades there are insufficient innovations in the location choices of immigrants to identify both first stage equations in our procedure. Only in the 1970s do we find a sufficient change in the composition of immigrant inflows to allow us to estimate consistently the short-run impact of immigration. Our estimates are more negative than many in the previous literature, suggesting that the initial wage impact of immigration on natives is potentially large. Our results also suggest, however, that much of this decline is reversed in later periods. 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 relative wages in the longer run.

In practical terms, our results suggest that researchers wishing to use spatial variation to estimate the impact of immigration should be aware of longer-term dynamic adjustment processes and to control for them with lagged immigrant inflows instrumented with lagged past settlement

instruments – in particular when studying outcomes such as wages, for which important general equilibrium adjustments are expected. Because the instruments are potentially highly collinear, researchers should check for underidentification or weak identification (e.g., with the Kleibergen-Papp 2006 *rk* LM statistic) and report reduced form results. Our example focuses on a setting in which including only one lag of immigrant inflows seems appropriate, but higher order lags should be included with higher frequency data.

While the migration literature represents a particularly dramatic example of the pitfalls of using shift-share instruments, the problem may be nearly as severe in other important contexts that use the shift-share methodology. For example, Amior and Manning (2017) have noted that the classic "Bartik" instrument for local demand shocks is also highly serially correlated.[38] The local shares component of this product will almost always be highly serially correlated, and the problem we highlight will be present to some extent in any shift-share IV context that features dynamic adjustments and limited innovations in the aggregate components of the instrument. When there is sufficient variation over time in the shift component of the instrument, however, our simple solution using multiple lags of the endogenous shock variable instrumented with lags of the conventional shift-share instrument will provide consistent estimates of its partial equilibrium impact as well as the effect of the dynamic adjustment process that follows.

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[38] In Appendix Table 2 we illustrate the degree of serial correlation for Bartik variables constructed using coarse industry classifications at the 1- or 2-digit level. The share component of the variables is measured at the beginning of the decade in question and the instruments are constructed using national employment or wage growth.

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Appendix A: *Labor Market Adjustments*

First differencing equation (7) gives the change in the optimal level of the capital-labor ratio, which is solely a function of labor demand shocks:

$$\log k_{jt}^* - \log k_{jt-1}^* = \frac{1}{1-\alpha} \Delta \log \theta_{jt},$$

Subtracting equation (8) from this expression gives

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

such that

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

Lagging one period, iterating backward, and noting that $\beta_1 = -\alpha$, we have

$$
\begin{aligned}
\log k_{jt-1}^* - \log k_{jt-1} \quad = \quad & m_{jt-1} + \frac{1}{1+\beta_1} \Delta \log \theta_{jt-1} + \\
& (1-\gamma)\left(\log k_{jt-2}^* - \log k_{jt-2}\right) \\
= \quad & m_{jt-1} + \frac{1}{1+\beta_1} \Delta \log \theta_{jt-1} + \\
& (1-\gamma)\left(m_{jt-2} + \frac{1}{1+\beta_1} \Delta \log \theta_{jt-2}\right) + \\
& (1-\gamma)^2\left(\log k_{jt-3}^* - \log k_{jt-3}\right) \\
= \quad & \cdots \\
= \quad & \sum_{s=0}^{\infty} (1-\gamma)^s \left(m_{jt-s-1} + \frac{1}{1+\beta_1} \Delta \log \theta_{jt-s-1}\right).
\end{aligned}
$$

Appendix B: *The Adjustment Process 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. $E[m_{jt+1}] \cong \tilde{m}_{jt+1}$. 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

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

With "naïve" expectations, $E[m_{jt+1}] = m_{jt}$, equation (14) would, for example, change to

$$\text{plim}\tilde{\beta}_{1|t=2}^{IV} = \beta_1 + \ldots - 2\gamma\beta_1 \frac{\text{Cov}(\tilde{m}_{j2}, m_{j1})}{\text{Cov}(\tilde{m}_{j2}, m_{j2})} \tag{14'}$$

where for simplicity we have ignored the terms involving demand shocks. 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 $E[m_{jt+1}] = \tilde{m}_{jt+1}$, the estimates in *t=1* would also be affected, and equation (14) would instead change to

$$\text{plim}\tilde{\beta}_{1|t=2}^{IV} = \beta_1 + \ldots - \gamma\beta_1 \frac{\text{Cov}(\tilde{m}_{j2}, m_{j1})}{\text{Cov}(\tilde{m}_{j2}, m_{j2})} - \gamma\beta_1 \tag{14''}$$

The bias is similar in both anticipation models if $\text{Cov}(\tilde{m}_{j2}, m_{j1}) \approx \text{Cov}(\tilde{m}_{j2}, m_{j2})$. 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.
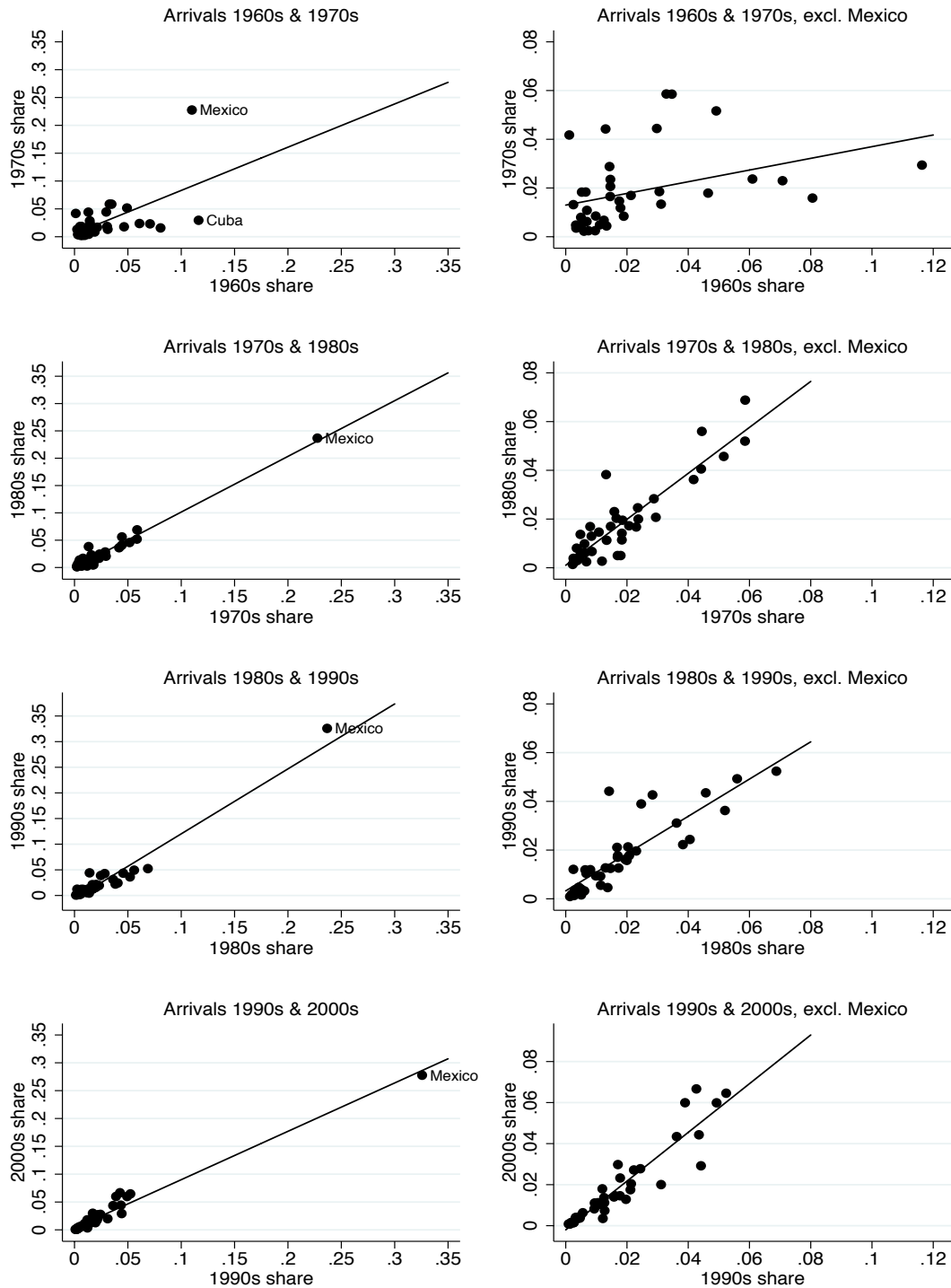
Note: Authors' calculations using U.S. Census (1970-2000) and ACS (2007-2011) data from 39 countries of origin. Each observation is the share of all newly-arrived immigrants that were born in a specific country.

Table 1
Characteristics of Immigrant Inflows

Census Year:	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 (2007-2011) data from 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

Census Year:	1970	1980	1990	2000	2010
Panel A: OLS					
Immigrant Inflows	0.120	-0.156	0.452 **	0.173	0.027
	(0.155)	(0.139)	(0.140)	(0.129)	(0.149)
Panel B: 2SLS (Current Instrument)					
Second stage					
Immigrant Inflows	0.183	-0.342	0.398 **	-0.045	0.017
	(0.211)	(0.184)	(0.114)	(0.113)	(0.144)
First stage					
Past Settlement Instrument	1.121 **	0.686 **	0.976 **	0.629 **	0.749 **
	(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 (1960-2000) and ACS (2007-2011) data from 109 MSAs. The column headings refer to the Census year from which the data were taken. The base year used in construction of the instrument is taken from the Census 10 years prior to the indicated Census year. 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. Robust standard errors in parentheses. ** indicates $p < 0.01$, * indicates $p < 0.05$.

Table 3

Correlations in Local Immigrant Inflows

Census year:	1980	1990	2000	2010
Panel A: Serial Correlation				
Immigrant 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 Immigrant Inflows 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 Lagged Immigrant Inflows 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				
Immigrant 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 (1960-2000) and ACS (2007-2011) data 109 MSAs. 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 10 and 20 years prior to the Census observation year, respectively.

Table 4
Dynamic Shift-Shares: Reduced Form and First Stage Results

Census Year:	1980	1990	2000	2010
Reduced Form:				
Past Settlement Instrument	-0.382 **	0.016	-0.469	-0.005
	(0.107)	(0.446)	(0.295)	(0.951)
Lagged Past Settlement Instrument	0.200 **	0.263	0.469	0.052
	(0.073)	(0.448)	(0.345)	(0.943)
First Stages:				
Kleibergen-Paap rk LM statistic	6.720	2.170	0.960	0.072
	[0.010]	[0.141]	[0.326]	[0.397]
Immigrant Inflows				
Past Settlement Instrument	0.186 *	-0.283	-0.781	1.142
	(0.090)	(0.452)	(0.431)	(0.584)
Lagged Past Settlement Instrument	0.501 **	0.984	1.429 **	-0.599
	(0.117)	(0.535)	(0.463)	(0.581)
F	216.9	5.6	26.3	21.4
Lagged immigrant inflows				
Past Settlement Instrument	-0.313 **	0.047	-0.913	1.578
	(0.066)	(0.229)	(0.505)	(0.868)
Lagged Past Settlement Instrument	0.947 **	0.426	1.898 **	-0.704
	(0.091)	(0.255)	(0.523)	(0.866)
F	139.7	13.6	39.7	28.6

Note: Authors' calculations using U.S. Census (1960-2000) and ACS (2007-2011) data from 109 MSAs. The dependent variable is the change in residual log wages by MSA. The column headings refer to the Census year from which the data were taken. Both instruments are created using country-of-origin immigrant shares from 20 years prior to the Census year. The reduced form reports the slope coefficients from an OLS regression of the change in residual log wage on both instruments. First stage results are from regressions of the immigrant inflow rate and lagged immigrant inflow rate on both instruments. Robust standard errors in parentheses. *p*-values in square brackets. ** indicates $p<0.01$, * indicates $p<0.05$.

Table 5

Dynamic Shift-Shares: Estimated Impact of Immigration on Natives' Wages , 1970s

Notes:	(1)	(2) Trim Bottom 5% of Wages	(3) Weight: Population	(4) Weight: log(Population)	(5) Bartik Control Var.	(6) Division Fixed Effects
Panel A: 2SLS (Current Instrument)						
Immigrant Inflows	-0.430 *	-0.440 *	-0.193	-0.407 *	-0.454 *	-0.684 *
	(0.199)	(0.180)	(0.117)	(0.195)	(0.205)	(0.279)
Panel B: 2SLS (Current and Lagged Instruments)						
Immigrant Inflows	-0.898 **	-0.850 **	-0.406	-0.869 **	-0.941 **	-1.591 **
	(0.314)	(0.271)	(0.263)	(0.315)	(0.330)	(0.518)
Lagged Immigrant Inflows	0.687 **	0.602 **	0.308	0.669 **	0.714 **	1.123 **
	(0.239)	(0.199)	(0.232)	(0.240)	(0.260)	(0.356)
Panel C: Reduced Form (Current and Lagged Instruments						
Past Settlement Instrument	-0.382 **	-0.346 **	-0.316 *	-0.381 **	-0.400 **	-0.517 **
	(0.107)	(0.093)	(0.128)	(0.108)	(0.116)	(0.187)
Lagged Past Settlement Instrument	0.323 **	0.320 **	0.233	0.325 **	0.331 *	0.385 **
	(0.117)	(0.105)	(0.157)	(0.116)	(0.128)	(0.176)

Note: Authors' calculations using 1960-1980 U.S. Census data from 109 MSAs. The dependent variable is the change in residual log wages by MSA between the 1970 and 1980 Census. Both instruments are created using country-of-origin immigrant shares from 1960. In column (2), the bottom bottom 5% of wages are trimmed. In column (3) observations are weighted by lagged total population in the MSA. In column (4) observations are weighted by the lagged log population. In column (5) observations include a "Bartik" variable to control for changes in industry composition (see text). Column (6) includes Census division fixed effects. Robust standard errors in parentheses. ** indicates $p < 0.01$, * indicates $p < 0.05$.

Table 6
Dynamic Shift-Shares:
Estimated Impact of Immigration on Natives' Wages for Subgroups, 1970s

Subgroup	Imm. Inflows		Lagged Imm. Inflows	
	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

Note: Authors' calculations using 1960-1980 U.S. Census data from 109 MSAs. The dependent variable is the change in residual log wages by MSA between the 1970 and 1980 Census. Both instruments are created using country-of-origin immigrant shares from 1960. 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.

Appendix A: *Labor Market Adjustments*

First differencing equation (7) gives the change in the optimal level of the capital-labor ratio, which is solely a function of labor demand shocks:

$$\log k_{jt}^* - \log k_{jt-1}^* = \frac{1}{1-\alpha}\Delta\log\theta_{jt},$$

Subtracting equation (8) from this expression gives

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

such that

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

Lagging one period, iterating backward, and noting that $\beta_1 = -\alpha$, we have

$$
\begin{aligned}
\log k_{jt-1}^* - \log k_{jt-1} \ &= \ m_{jt-1} + \frac{1}{1+\beta_1}\Delta\log\theta_{jt-1} + \\
&\quad (1-\gamma)\left(\log k_{jt-2}^* - \log k_{jt-2}\right) \\
&= \ m_{jt-1} + \frac{1}{1+\beta_1}\Delta\log\theta_{jt-1} + \\
&\quad (1-\gamma)\left(m_{jt-2} + \frac{1}{1+\beta_1}\Delta\log\theta_{jt-2}\right) + \\
&\quad (1-\gamma)^2\left(\log k_{jt-3}^* - \log k_{jt-3}\right) \\
&= \ \dots \\
&= \ \sum_{s=0}^{\infty} (1-\gamma)^s \left(m_{jt-s-1} + \frac{1}{1+\beta_1}\Delta\log\theta_{jt-s-1}\right).
\end{aligned}
$$

Appendix B: *The Adjustment Process 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

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

With "naïve" expectations, $E[m_{jt+1}] = m_{jt}$, equation (14) would, for example, change to

$$\mathrm{plim}\, \tilde{\beta}_{1|t=2}^{IV} = \beta_1 + \ldots - 2\gamma\beta_1 \frac{\mathrm{Cov}\left(\widetilde{m}_{j2}, m_{j1} \right)}{\mathrm{Cov}\left(\widetilde{m}_{j2}, m_{j2} \right)} \tag{14$'$}$$

where for simplicity we have ignored the terms involving demand shocks. 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 $E[m_{jt+1}] = \widetilde{m}_{jt+1}$, the estimates in *t*=1 would also be affected, and equation (14) would instead change to

$$\mathrm{plim}\, \tilde{\beta}_{1|t=2}^{IV} = \beta_1 + \ldots - \gamma\beta_1 \frac{\mathrm{Cov}\left(\widetilde{m}_{j2}, m_{j1} \right)}{\mathrm{Cov}\left(\widetilde{m}_{j2}, m_{j2} \right)} - \gamma\beta_1 \tag{14$''$}$$

The bias is similar in both anticipation models if $\text{Cov}(\tilde{m}_{j2}, m_{j1}) \approx \text{Cov}(\tilde{m}_{j2}, m_{j2})$. 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.

Appendix Table 1

Selected Publications using the Past Settlement Instrument

Authors	Year	Journal	Outcome
		Labor Market Outcomes	
Altonji and Card	1991	Book chapter	Native labor market outcomes
Dustmann, Fabbri, and Preston	2005	*Economic J.*	Native labor market outcomes
Reed and Danziger	2007	*AER: Pap. & Proc.*	Native labor market outcomes
Kugler and Yuksel	2008	NBER Working Pap.	Native labor market outcomes
Card	2009	*Am. Econ. Rev.*	Native labor market outcomes
Peri	2011	*J. Int. Econ.*	Native labor market outcomes
Dustmann, Frattini, and Preston	2013	*Rev. Econ. Stud.*	Native labor market outcomes
Facchini, Mayda, and Mendola	2013	IZA Discussion Pap.	Native labor market outcomes
Ottaviano, Peri, and Wright	2013	*Am. Econ. Rev.*	Native labor market outcomes
Aydemir and Kırdar	2014	IZA Discussion Pap.	Native labor market outcomes
D'Amuri and Peri	2014	*J. Eur. Econ. Ass.*	Native labor market outcomes
Ortega and Verdugo	2014	*Labour Econ.*	Native labor market outcomes
Cattaneo, Fiorio, and Peri	2015	*J. Human Res.*	Native labor market outcomes
Del Carpio, Özden, Testaverde, and Wagner	2015	*Scan. J. Econ.*	Native labor market outcomes
Monras	2015	IZA Discussion Pap.	Native labor market outcomes
Foged and Peri	2016	*AEJ: Applied Econ.*	Native labor market outcomes
Moreno-Galbis and Tritah	2016	*Eur. Econ. Rev.*	Native labor market outcomes
Özden and Wagner	2016	Unpub. Manuscript	Native labor market outcomes
Amior	2017	Unpub. Manuscript	Native labor market outcomes
Llull	2018	*J. Human Res.*	Native labor market outcomes
Piyapromdee	2017	Unpub. Manuscript	Native labor market outcomes, welfare
Fairlie and Meyer	2003	*J. Labor Econ.*	Native self-employment
Ottaviano and Peri	2005	*J. Urban Econ.*	Native wages and employment
Ottaviano and Peri	2006	*J. Econ. Geog.*	Native wages and housing market
Farré, Gonzéalez, and, Ortega	2011	*B.E.J. Econ. A&P*	Female labor supply
Forlani, Lodigiani and Mendolicchio	2015	*Scan. J. Econ.*	Female labor supply
Malchow-Møller, Munch, and Skaksen	2012	*Scan. J. Econ.*	Firm-level wages
González and Ortega	2011	*Labour Econ.*	Labor market outcomes
Card	2001	*J. Labor Econ.*	Labor market outcomes, internal migration
Cortés and Tessada	2011	*AEJ: Applied Econ.*	Labor supply, household work and services
Beaudry, Green, and Sand	2012	*Econometrica*	Wage determination
Smith	2012	*J. Labor Econ.*	Youth employment

Authors	Year	Journal	Outcome
		Other Outcomes	
Fulford, Petkov, and Schiantarelli	2017	Unpub. Manuscript	Ancestry composition and county GDP
Bianchi, Buonanno, and Pinotti	2012	*J. Eur. Econ. Ass.*	Crime
Bell, Fasani, and Machin	2013	*Rev. Econ. Stat.*	Crime
Chalfin	2015	*AER: Pap. & Proc.*	Crime
Hunt	2017	*J. Human Res.*	Educational attainment
Orrenius and Zavodny	2015	*J. Labor Econ.*	Educational choices
Furtado and Hock	2010	*AER: Pap. & Proc.*	Fertility
Dustmann and Glitz	2015	*J. Labor Econ.*	Firm adjustment
Ottaviano, Peri, and Wright	2015	NBER Working Pap.	Firm-level trade of services
Machin and Murphy	2017	*J. Econ. Geog.*	Higher education
Saiz	2007	*J. Urban Econ.*	Housing market
Hunt and Gauthier-Loiselle	2010	*AEJ: Macro*	Innovation
Bosetti, Cattaneo, and Verdolini	2015	*J. Int. Econ.*	Innovation
Card and DiNardo	2000	*AER: Pap. & Proc.*	Internal migration
Hatton and Tani	2005	*Economic J.*	Internal migration
Lewis	2011	*Quarterly J. Econ.*	Investment in automation
Ganguli	2015	*J. Labor Econ.*	Knowledge diffusion
Kasy	2015	*J. Urban Econ.*	Location choices with social externalities
Lafortune	2013	*AEJ: Applied Econ.*	Marriage market
Giuntella and Mazzonna	2015	*J. Health Econ.*	Native health outcomes
McHenry	2015	*J. Human Res.*	Native human capital
Amuedo-Dorantes and Sevilla	2014	*J. Human Res.*	Parental time investment
Wozniak and Murray	2012	*J. Urban Econ.*	Population, internal migration
Cortés	2008	*J. Political Econ.*	Prices (goods and services)
Frattini	2008	Unpub. Manuscript	Prices (goods and services)
Peri	2012	*Rev. Econ. Stat.*	Productivity
Fassio, Kalantaryan, and Venturini	2015	IZA Discussion Pap.	Productivity
Peri, Shih, and Sparber	2015	*J. Labor Econ.*	Productivity
Cascio and Lewis	2012	*AEJ: Econ. Policy*	Residential and school segregation
Boustan	2010	*Quarterly J. Econ.*	Residential segregation
Fernandez-Huertas, Ferrer, and Saiz	2017	IZA Discussion Pap.	Residential segregation
Iranzo and Peri	2009	*Rev. Econ. Stat.*	Schooling externalities and productivity
Kerr and Lincoln	2010	*J. Labor Econ.*	Science and engineering, patenting
Ransom and Winters	2016	IZA Discussion Pap.	STEM education and employment
Cortés and Pan	2014	*J. Health Econ.*	Supply of native nurses
Peri and Sparber	2009	*AEJ: Applied Econ.*	Task specialization
Barone, D'Ignazio, De Blasio, and Naticchioni	2016	*J. Publ. Econ.*	Voting behavior

Appendix Table 2

Serial Correlation in Classic Shift-Share Instruments

Census year:	1980	1990	2000	2010
Immigration				
Immigrant Inflows	0.82	0.96	0.92	0.96
Past-Settlement Instrument	0.70	0.99	0.96	0.99
Bartik-Employment				
Employment (percentage growth)	0.31	0.21	-0.14	0.20
Bartik Instrument (2-digit)	0.27	0.24	0.69	0.41
Bartik Instrument (1-digit)	0.39	0.68	0.88	0.89
Bartik-Wage				
Wage (log-growth)	-0.01	-0.44	0.15	0.05
Bartik Instrument (2-digit)	0.23	-0.49	-0.06	0.29
Bartik Instrument (1-digit)	-0.10	-0.25	0.39	0.20

Note: Authors' calculations using U.S. Census (1960-2000) and ACS (2007-2011) data. Each entry is a pairwise correlation across 109 MSAs between the variable in the respective decade and its decadal lag. The Bartik instruments are constructed by combining local 2-digit or 1-digit industry shares in the beginning of the decade with industry-specific employment or wage growth on the national level.

Appendix Table 3
Estimated Impact of Immigration on Natives' Wages, Commuting Zones

Census Year:	1980	1990	2000	2010
Panel A: OLS				
Immigrant Inflows	-0.210 **	0.605 **	-0.014	0.060
	(0.077)	(0.094)	(0.107)	(0.115)
Panel B: 2SLS (Current Instrument)				
Second stage				
Immigrant Inflows	-0.315 **	0.595 **	-0.222	0.060
	(0.095)	(0.089)	(0.170)	(0.091)
First stage				
Past Settlement Instrument	0.782 **	1.017 **	0.602 **	0.678 **
	(0.166)	(0.057)	(0.115)	(0.082)
First stage R^2	0.663	0.891	0.714	0.823

Note: Authors' calculations using U.S. Census (1960-2000) and ACS (2007-2011) data from 741 commuting zones. The column headings refer to the Census year from which the data were taken. The base year used in construction of the instrument is taken from the Census 10 years prior to the indicated Census year. 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. Robust standard errors in parentheses. ** indicates $p<0.01$, * indicates $p<0.05$.

Appendix Table 4

Dynamic Shift-Shares:

Estimated Impact of Immigration on Natives' Wages for Commuting Zones, 1970s

Notes:	(1)	(2) Trim Bottom 5% of Wages	(5) Bartik Control Var.	(6) Division Fixed Effects
Panel A: 2SLS (Current Instrument)				
Immigrant Inflows	-0.294 *	-0.312 **	-0.318 *	-0.520 **
	(0.199)	(0.118)	(0.140)	(0.192)
Panel B: 2SLS (Current and Lagged Instruments)				
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 (Current and Lagged Instruments)				
Past Settlement Instrument	-0.196 **	-0.183 **	-0.212 **	-0.291 **
	(0.059)	(0.054)	(0.061)	(0.070)
Lagged Past Settlement Instrument	0.038	0.027	0.038	0.065
	(0.067)	(0.085)	(0.064)	(0.094)

Note: Authors' calculations using 1970-1980 U.S. Census data from 741 Commuting Zones. The dependent variable is the change in residual log wages by MSA between the 1970 and 1980 Census. Both instruments are created using country-of-origin immigrant shares from 1960. In column (2), the bottom bottom 5% of wages are trimmed. In column (5) observations include a "Bartik" variable to control for changes in industry composition (see text). Column (6) includes Census division fixed effects. Robust standard errors in parentheses. ** indicates $p < 0.01$, * indicates $p < 0.05$.

Appendix Table 5
Estimated Impact of Immigration on Natives' Wages for Subgroups,
1970s with additional control variables

Notes:	(1) Demographic Control Vars.	(2) Oil industry Control Var.
Panel A: 2SLS (Current Instrument)		
Immigrant Inflows	-0.412 *	-0.430 *
	(0.168)	(0.199)
Panel B: 2SLS (Current and Lagged Instruments)		
Immigrant Inflows	-0.787 **	-0.914 **
	(0.257)	(0.317)
Lagged Immigrant Inflows	0.542 **	0.706 **
	(0.193)	(0.247)
Panel C: Reduced Form (Current and Lagged Instruments)		
Past Settlement Instrument	-0.344 **	-0.382 **
	(0.104)	(0.107)
Lagged Past Settlement Instrument	0.248	0.323 **
	(0.148)	(0.120)

Note: Authors' calculations using 1960-1980 U.S. Census data from 109 MSAs. The dependent variable is the change in residual log wages by MSA between the 1970 and 1980 Census. Both instruments are created using country-of-origin immigrant shares from 1960. In column (1), regressions include two demographic control variables: the decadal change in female labor force participation, and the decadal change in labor force participation as predicted by the age structure of the local population lagged one decade ((number of individuals aged 10-54 / number of individuals aged 20-64) - 1). In column (2), regressions include a control variable for the oil industry's share in local