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Immigration's Effect on Institutional Quality: The Place of Simpler Evidence

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LINK TO ABSTRACT

The idea that the inclusion of a multitude of control variables will necessarily improve (and will not worsen) causal inference is a methodological urban legend at best.
—Julia Rohrer (2018, 28)

An important question is whether migration affects institutional quality over decades.[3] Since productivity and experience with reasonably competent governance differ enormously across countries, the most important papers (including Dimant, Krieger, and Redlin 2015; Clark et al. 2015; and Bologna Pavlik, Lujan Padilla, and Powell 2019) have focused on whether large migration flows from relatively poor or corrupt countries predict lower institutional quality. These papers have reported regression results that use the stock or flow of a nation's immigrants from such origin countries as a control variable, and then use those migration variables to predict subsequent institutional quality. Unfortunately, when reporting the relationship between immigration from relatively poor or corrupt countries and

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2. U.S. Bureau of Labor Statistics, Washington, DC 20212. All views expressed in this paper are those of the authors and do not necessarily reflect the views or policies of the U.S. Bureau of Labor Statistics.
3. For suggestive evidence that the *threat* of emigration can improve a nation's institutions, see Hall (2016), who reports that institutional quality has tended to improve more in nations where residents can exit the nation more easily. Ease of exit is proxied solely by the ratio of length of national borders to coastline—hence it is implicitly a measure of border crenulation—and so further investigation into proxies for low relative cost of emigration is certainly warranted.

subsequent changes in institutional quality, none of the important papers just cited reports simple correlations or scatterplots to give readers a sense of the underlying data. We rectify that omission here. We draw attention to the potential of over-control bias—in particular, of controlling for proxies of the dependent variable—to obscure strong, important statistical relationships in data.

What feeling does your statistical intuition prompt inside of you when you consider the following investigations?

- A regression of restaurant success on chef expertise that also controls for that restaurant's online rating
- A regression of long-run inflation on central bank independence that also controls for long-run money growth
- A regression of firm output growth on that firm's access to financial markets that also controls for the firm's capital and labor growth

In each of these cases, it's plausible that the conventional, expected correlations between

- restaurant success and chef expertise,
- inflation and central bank independence, and
- firm growth and access to finance

could well be rendered statistically insignificant if the researcher controlled for these extra variables. Overcontrol bias can occur when a regression of an outcome Y on a proposed cause X also controls for other proxies of Y or for other proxies of X. Psychologist W. Joel Schneider (2007) illustrates one version of overcontrol bias with a quip: "I am as tall as the Rocky Mountains! (After controlling for barometric pressure)."

When regressions include as controls a combination of proxies of the proposed causal independent variable, proxies of the dependent variable, or factors that are proposed as intermediate causal channels between proposed cause and effect, such regressions can fall prey to overcontrol bias. Controlling for intermediate channels may seem intuitive, but it can eclipse obvious relationships. Obvious bivariate relationships can be reduced, eliminated, or even see the sign change when controlling for mediating channels. How should economists change their empirical approaches to account for overcontrol bias? Psychologist Julia Rohrer offers simple, practical advice in an influential recent essay:

> Mediators are causally affected by the independent variable… A solid rule of thumb is that researchers should not control for such posttreatment variables. (Rohrer 2018, 34)

Rohrer's essay on overcontrol bias is the best recent overview of subject and is strongly recommended. As she notes:

> Often, the analysis follows the rationale that "more control" is always better than less. Models resulting from such an approach have been labeled "garbage-can regressions." (Rohrer 2018, 28)

An alternative to the mediator-heavy regression often exists: plainness and simplicity in evidence. Economists test their models by checking to see whether the models replicate durable correlations, sometimes called 'stylized facts,' such as:

- the positive relationship between the savings rate and GDP per capita;
- the roughly 2/3 share of national income that goes to workers;
- the positive relationship between central bank independence and inflation;
- the relatively stable return to capital across the decades in rich countries, combined with a rising return to labor; and
- the nearly-perfect, 1-for-1 relationship between long-run money growth and inflation between countries.

Such stylized facts are simple—one might even say simplistic. They are rarely derived from multivariate regressions; instead they are simple patterns, scatterplots, time trends, correlations. Economists check to see whether a particular model can replicate them—if the model fits them, that's not a complete test of the model, but it's a good sign.

We report, to our knowledge for the first time, simpler evidence about the relationship between ten- to twenty-year changes in immigration from relatively poor or corrupt countries and two-decade changes in institutional quality. To reduce concerns about data mining, we solely use relevant data from a *Southern Economic Journal* article on the topic by Jamie Bologna Pavlik, Estefania Lujan Padilla, and Benjamin Powell (2019—henceforth "BLP").

The indices used in the institutional quality literature lack transparent units, and the functional form—linear or non-linear—between immigrant experience and institutional quality is unclear. For those reasons, we emphasize the Spearman and Kendall *rank correlation* results. These two methods test only for the rank-order relationship between pairs of numbers, and hence dramatically reduce the risk that outliers can drive results. Economists are overwhelmingly more likely to use the Pearson correlation coefficient (the familiar *r*), but Pearson's *r* is formally designed for linear relationships where the variables are normally distributed. For cases where there is no particular reason to believe in a linear relationship between the two variables, or where the variables aren't normally distributed, a rank correlation statistic like Spearman's or Kendall's is often recommended. As Wassily Hoeffding noted in 1957 in the pages of *Econometrica*:

> Ranking methods are generally based on fewer assumptions than the usual numerical methods; [and] they are invariant under arbitrary changes of the scale of the measurements… (Hoeffding 1957)

Psychologists are particularly likely to use Spearman correlations. In part they do so because their units of measurement of psychological behavior are often arbitrary, and closer to rankings than true unit measurements; in part they do so because there is little reason to think the relationship between any two measurements will be linear, and little reason to think the measures are themselves normally distributed. Anthony Bishara and James Hittner (2012) review the advice given in widely used psychometrics textbooks:

> By far, the most frequent recommendation was to use Spearman's rank-order correlation—the argument being that Spearman's nonparametric test would be more valid than Pearson's test when parametric assumptions are violated. (Bishara and Hittner 2012)

As noted, we also report the Kendall rank correlation, often denoted by the Greek letter τ. The difference between Spearman and Kendall is somewhat like the difference between horseshoes and basketball: With the Spearman correlation, being off from a perfect rank-order match by a little bit is better than being off by a lot. With Kendall, by contrast, 'an inch is as good as a mile,' since Kendall only tracks whether, for two observations, the two variables have the same (concordant) or different (discordant) rank orders. So Spearman is more like horseshoes (close counts for something), while Kendall is more like basketball (a miss is a miss).[4]

Since we have no idea whether the true relationship between immigration and institutional quality—if one even exists—will be linear or nonlinear, and since the rates of migration are obviously non-normal—both highly skewed and massively kurtotic—we place the most weight on the Spearman and Kendall rank correlation results, not on the Pearson correlations. We do, however, also report conventional Pearson correlations (which presume linearity and normality) and

4. Spearman's ϱ is calculated like Pearson's r, but using rank orders of each variable instead of the actual value of the variable. Thus, Spearman's ϱ is a simple correlation coefficient, but for rankings. Kendall's rank correlation, by contrast, counts up the percentage of all possible pairs of observations where the rank order is concordant (same order) rather than discordant (different order), and then uses that fact to calculate Kendall's τ. With Kendall unlike with Spearman (or Pearson) the magnitude of the concordance or discordance is irrelevant. The magnitude of a Kendall correlation is not directly comparable to that the of the Spearman (or Pearson) correlation.

tentatively discuss magnitudes as well. In addition, we offer, to our knowledge for the first time, scatterplots of such data. This short paper, in other words, is designed to rectify omissions, to fill gaps that in other economics literatures are routinely filled in.

We report the rank correlations between the 10- to 20-year change in the percentage of a nation's population from relatively poor or corrupt countries (in a precise sense defined below) and the 20-year change in the Fraser Institute's Economic Freedom of the World (EFW) index. The Spearman rank correlation between these migration measures and EFW is always in the range of −0.41 to −0.49, while the Kendall rank correlation is always between −0.27 and −0.32. Therefore, in this sample, more immigration from relatively poor or corrupt nations is associated with relative declines in institutional quality. These rank correlation relationships are always significant at the 0.5-percent level. We appear to be the first to report such a negative rank correlation relationship.

In their recent book *Wretched Refuse? The Political Economy of Immigration and Institutions*, Alex Nowrasteh and Benjamin Powell report:

> Regardless of the immigration measure used or the precise regression specification, we have not found a single instance in which immigration is associated with less economic freedom. (Nowrasteh and Powell 2020, 132)

As we show below, a resort to simpler methods rather than heavily controlled regressions might have led to a different statement.

The Pearson correlation (which again, presumes linearity and normality) between the same migration measures and a nation's change in Control of Corruption—one of the components of the World Bank's *Worldwide Governance Indicators*—is always negative and in the range of −0.11 and −0.21, and statistically significant at the 10-percent level in three out of four specifications. We offer a suggestive structural model that could frame future discussions of immigration and institutional quality, then report the stylized facts with correlations and scatterplots to illustrate this heretofore unreported pattern of relationships. As a suggestive extension, we also report regression results that simultaneously control for immigration from relatively poor countries as well as immigration from all other countries. While F-tests cannot reject the equality of the two coefficients, the coefficient on immigration from relatively poor countries is much larger in absolute value and always negative.

Our statistical model

We offer a simple model to frame the discussion of overcontrol bias and to motivate the reporting of change-on-change correlations. The idea is more general than the model below and can be summed up as follows:

> If *levels* of traits of people within a nation contribute to the nation's *level* of institutional quality, then *changes* in those national traits will cause *changes* in a nation's institutional quality, perhaps with a lag.

We do not have data on the traits of people. Instead, we have data on average measures of institutional quality (and income per capita) in an immigrant's nation of origin. We thus test the hypothesis that some traits that shape home-country institutional quality and productivity tend to migrate, to some degree, to an immigrant's destination country, perhaps with a lag. And we do not dispute that causality also works in the opposite direction, that is, that institutions affect the traits of people.

Nation *i*'s level of institutional quality in year t, I_{it}, is generated according to the following linear model. Suppressing the constant, we have:

$$I_{it} = \gamma_i + \theta_t + \alpha L_{it} + \varepsilon_{it} \tag{1}$$

Here, γ_i is a country-specific factor that might represent long-standing cultural and even geographic drivers of institutional quality—factors that won't materially change over a few decades. The θ_t factor captures global average shifts in institutional quality, such as the move toward markets in the 1990s, and also captures worldwide period-level biases in the measurement of the index itself. L_{it} is the critical variable: it represents the percentage of nation *i*'s population who are migrants from countries with 'low' levels of either productivity or transparent governance. In BLP (2019), the standard for 'low' is either one standard deviation below country *i*'s level of productivity or one standard deviation above country *i*'s level of corruption. Of course, that implies that for countries with the very lowest levels of productivity or transparent governance, those values in BLP are mathematically equal to zero. The parameter a, which in principle may be positive, negative, or zero, captures how much migrants from weaker-performing countries shape institutional quality, if at all.

While we do not formally model the social process by which this might occur, one possible channel is especially worth noting. One of us has written recently in the American context about "one particular way immigration is likely already affecting institutions: through a populist backlash channel" (Jones 2018, 342). Hostility to new arrivals is, alas, part of the human experience, and the backlash of a majority to newcomers from relatively poor and corrupt countries may push nations away from economic freedom and neutral competence and t0ward policies and practices that favor insiders. The degree (if any) to which this backlash channel, a shifting median voter channel (e.g., Caplan 2011), or a cultural change channel (e.g., Alesina and Giuliano 2015) might be important we leave to future research.

Finally, ε_{it} captures all other omitted, time-varying country-level drivers of national institutional quality. We do *not* assume ε_{it} is i.i.d., and in particular we do not assume the epsilon term is uncorrelated with L_{it}. However, we do strongly suggest that omitted, country-specific, time-varying institution-improving variables are likely to be *positively* correlated with L_{it}, since national economic success tends to attract migrants of all skill levels. Thus, the modest negative correlations we report below may well be understatements: migrants surely try to go to places that are good, and they plausibly try to go to places that are getting better. It is plausible, therefore, that our omitted variable bias leads to statistical results that understate the relationship we report.

Taking first differences of (1) and collecting nuisance terms into $\tilde{\varepsilon}_{it}$ yields:

$$\Delta I_{it} = \alpha \Delta L_{it} + \tilde{\varepsilon}_{it} \tag{2}$$

Here $\tilde{\varepsilon}_{it} = \Delta \varepsilon_{it} + \Delta \theta_t$. The first term of $\tilde{\varepsilon}_{it}$ captures changes in country-specific time-varying factors (again, plausibly *positively* correlated with L_{it}, since migrants seek prosperity), and the second term captures global trends in measured institutional quality.

Specification (2), which we use below, certainly has its limitations, but take a moment to compare it to specifications used in BLP. They have the following general form, where the circumflex, inverted-circumflex, and bar versions of I_{it} indicate alternative institutional quality measures, and the betas indicate their coefficients:

$$I_{it} = \beta_1 \hat{I}_{it} + \beta_2 \check{I}_{it} + \beta_3 \bar{I}_{it} + \beta_4 I_{i(t-1)} + \alpha L_{i(t-1)} + \varepsilon_{it} \tag{3}$$

For example, in their Tables 6a, 6b, 8, and 10 (BLP, 1253, 1254, 1256, 1258), paired with their discussion in the associated subsection (ibid., 1252–1254), the authors *always* control for all of the following: the Fraser Institute's Economic Freedom of the World index, the Center for Systemic Peace's Polity Score, Freedom House's Freedom of the Press score, the size of the shadow economy, and average GDP per capita. The dependent variable is included as a lagged dependent variable, while the other institutional controls in their regressions are averaged over a 20-year period or even a five-year period depending on the specification, and hence include contemporaneous data in those averages. Thus, there is substantial risk—even a presumption, to our mind—of overcontrol bias in BLP's results, since recent immigration levels, $L_{i(t-1)}$, could influence all of these potential mediators, and since institutional change is usually slow. In the aforementioned language of Rohrer, these institutional and income controls are potentially "posttreatment variables;" and so these specifications in BLP appear quite likely to run afoul of her (2018) aforementioned "solid rule of thumb" that one should not control for such variables.

BLP do report some simpler results, but only for a different set of immigration measures. In their Table 2, they provide what they call "Baseline Results" of the relationship between various "Immigrant Stocks" and corruption—but only when the "Immigrant Stocks" are broken down into immigrants from OECD countries versus those from non-OECD countries (BLP, 1249). They report:

> The only statistically significant association between immigration and corruption that we found, in these baseline regressions, was that a higher immigrant stock, in 1995, that originated from OECD origin countries was associated with a lower level of corruption in 2015. (BLP, 1249)

It is perhaps notable that they find that greater immigration from OECD countries predicts lower future corruption relative to immigration from non-OECD countries. These are stocks rather than flows—levels rather than medium-run changes—but nevertheless they are examples of simpler statistical evidence, and Tables 6a and 6b (BLP, 1253, 1254) *never* include the extra controls for near-contemporaneous institutional quality and productivity. Such statistical clarity is welcome. However, the authors do not report such simpler baseline results for the immigration measures we discuss below.

Data

All data are from Bologna Pavlik, Lujan Padilla, and Powell (2019), and were generously provided by the authors. The data in levels were all drawn from their "Data" worksheet and transformed into changes accordingly. BLP report complete data for 110 countries. Summary statistics for the change variables are reported in Table 1.

TABLE 1. Summary statistics: changes in migrant population and changes in institutional quality

	Change in percentage of population who have immigrated from poorer countries, 1995–2005	Change in percentage of population who have immigrated from poorer countries, 1995–2015	Change in percentage of population who have immigrated from more corrupt countries, 1995–2015	Change in Economic Freedom of the World, 1995–2015	Change in Control of Corruption, 1995–2015	Change in Control of Corruption, 1995–2005 annual mean to 2005–2015 annual mean
Mean	0.33	1.07	1.67	0.665	−0.036	−0.028
Median	0.01	0.06	0.04	0.505	−0.018	−0.038
Standard deviation	1.34	3.24	3.70	0.93	0.37	0.22
Skewness	2.55	3.30	2.74	0.68	0.26	0.52
Kurtosis	15.22	15.21	11.13	4.81	5.29	5.24

Note: All data are derived from Bologna Pavlik, Lujan Padilla, and Powell (2019).

The three migrant change variables all report changes in the percentage of nation *i*'s population that came from countries where either the level of income is one standard deviation *below* that of country *i* or where the score on the corruption index (described below) is one standard deviation *worse* than that of country *i*. Migrant population data are from the World Bank's *International Migrant Stock by Destination and Origin* data set.

Notice that these migrant change variables are all focused on lower-performing countries, around the *bottom* 15 percent of the global sample—and BLP did not report simple "Baseline Results" looking at immigration from this group of countries. By contrast, the above-mentioned OECD results in BLP, for which simple "Baseline Results" *were* reported, focus on immigrants from the world's *best performing* economies. BLP then compared immigration from OECD countries (of which today there are 37) to immigration from all non-OECD countries, arguably a version of 'the best versus the rest.' While BLP's OECD results checked to see whether large numbers of immigrants from countries with institutional quality well *above* the global mean predicted better institutional quality in the destination country[5]—finding that, on average, the answer was yes—the results below focus

5. Furthermore, the OECD includes some countries that both have relatively weak institutional quality, and that have a substantial amount of emigration—so percentage of immigrants from OECD countries may be a poor proxy for percentage of immigrants from countries with high institutional quality. For example, Mexico, an OECD member, was in 2015 the country of origin for 6.7 million immigrants according to the UN, the most of any OECD country and double the number of the next-highest OECD source country, Germany. Mexico's 2015 Control of Corruption score was −0.77, compared to Germany's +1.84. Likewise, Turkey and Italy are also OECD countries with substantial amounts of emigration (2.6 million and 3.3 million, respectively), and have Control of Corruption scores of −0.03 and +0.02, respectively. Thus, additional proxies for the institutional quality of emigrant source countries beyond OECD membership appear warranted.

mostly on immigrants from countries with institutional quality well *below* the global mean. BLP reported simple results on whether immigration from *elite* countries has a statistical relationship with corruption. We do the same for immigration from the world's *poorest and most corrupt* countries.

There is high skewness in the migration level data: for instance, in 2015 the median country in the sample has 1.0 percent of its population from such poorer countries, while the mean value is 4.4 percent, and the standard deviation is 11.5 percent. Of course, for the very poorest countries, the number of migrants from countries substantially poorer is mathematically equal to zero (a fact true for both our estimates and for the original BLP estimates). Our correlations use changes not levels, but similarly display that same high level of positive skewness—a sign the data are far from normally distributed. Two of the migrant population measures are the change in the percentage of nation *i*'s population between 1995 to 2005, and between 1995 to 2015, who are immigrants from substantially poorer countries. The third measure is the same change in the percentage of the nation's population between 1995 to 2015 who are from substantially more corrupt countries. These timeframes are used because they are reported in the BLP data set, because none include changes in migrant population that occur *after* the reported changes in institutional quality, and because the 1995 to 2005 measure in particular allows us to take a first look at the possible lag structure between changes in a nation's population traits and changes in its institutional quality.

We also use BLP's data on the Fraser Institute's Economic Freedom of the World (EFW, values ranged from 0 to 10) and the corruption component of the World Bank's *Worldwide Governance Indicators* (with values ranged from −2.5 to +2.5). In both indices, the larger, more positive number is better. Thus a *higher* Control of Corruption (COFC) score implies *greater* control of corruption. Again, we use changes in three relevant measures for which the levels are already calculated in the BLP data set. Two of the three are changes in the EFW and COFC indices between 1995 and 2015. Also, for the COFC, we use their data on the average level of COFC between 1995 and 2005, and again between 2005 and 2015, to calculate the average change between the two periods; this will reduce the effect of year-to-year noise in the estimates, and again allows us to offer a suggestive test of the lag structure.

To be clear about the direction of the relationship, we present a few typical observations. Singapore's EFW score is 8.81, while Venezuela's is 4.92; Switzerland's Control of Corruption score is 2.14, while Pakistan's is −0.81. Higher values accord with conventional expectations of higher institutional quality.

What do simpler methods show?

Scatterplots are reported in Figures 1, 2, and 3, using World Bank codes as data labels; Spearman rank correlations are reported in Table 2, Kendall rank correlations in Table 3, and Pearson correlations in Table 4.

As the first columns of Table 2 and Table 3 make clear, the rank-order relationship between change in migrants from poor countries as a percentage of population and change in economic freedom is reasonably strong, with rank correlations between −0.41 and −0.49 for Spearman and −0.27 to −0.32 for Kendall. Since these two types of correlation coefficients are calculated in quite different ways, the magnitudes of the coefficients are not comparable to each other, though we should note that a Kendall correlation of −0.3 implies that 65 percent of all possible pairs of observations are ranked in the same (negative) direction.[6] All of these relationships are statistically significant at conventional levels, and hence quite unlikely to be pure coincidence. Recall that rank correlations give no weight to the magnitude of a change, and hence give no particular weight to outliers. Ours is the first paper to report this stylized fact, a finding that certainly deserves further theoretical and empirical inquiry. All of the reported rank correlations are strictly negative.

TABLE 2. Spearman rank correlations: change in migrant population vs. change in institutional quality

	Economic Freedom of the World, 1995–2015	Control of Corruption, 1995–2015	Control of Corruption, 1995–2005 annual mean to 2005–2015 annual mean
Poor, ’95–’05	−0.41* [0.00]	−0.17 [0.07]	−0.21* [0.03]
Poor, ’95–’15	−0.49* [0.00]	−0.16 [0.09]	
Corrupt, ’95–’15	−0.42* [0.00]	−0.11 [0.23]	

Note: ‘Poor’ and ‘Corrupt’ denote percentage of the destination country’s total population who are immigrants from countries one standard deviation poorer or more corrupt than the destination country. Asterisk indicates significance at 5-percent level, p-values in brackets.

6. Professor of Health Research Methods Ronán Michael Conroy in 2015 helpfully pointed out the relevant formula on a ResearchGate discussion of the Kendall rank correlation (link); it’s just a reorganization of the definition of Kendall’s τ.

TABLE 3. Kendall rank correlations: change in migrant population vs. change in institutional quality

	Economic Freedom of the World, 1995–2015	Control of Corruption, 1995–2015	Control of Corruption, 1995–2005 annual mean to 2005–2015 annual mean
Poor, '95–'05	−0.27* [0.00]	−0.12 [0.07]	−0.14* [0.03]
Poor, '95–'15	−0.32* [0.00]	−0.11 [0.08]	
Corrupt, '95–'15	−0.28* [0.00]	−0.08 [0.21]	

Note: 'Poor' and 'Corrupt' denote percentage of the destination country's total population who are immigrants from countries one standard deviation poorer or more corrupt than the destination country. Asterisk indicates significance at 5-percent level, p-values in brackets.

The first two scatterplots—where each of the 110 observations is a country—illustrate the negative relationship—obviously non-linear, and closer to an L-shaped hyperbolic—between the change in the percentage of each nation's population who are immigrants from relatively poor countries and the change in that nation's EFW score. The dozen or so outliers that are essentially irrelevant to Table 2 and 3's rank correlation estimates are obvious in the scatterplots, but of course that is a reason to place more weight on the rank correlations, and less on the visible outliers. The third scatterplot illustrates one of the weakest correlations—between the change in percentage of a nation's population from poor countries and the change in Control of Corruption.

Figure 1. 10-year change in migrant population from poor countries and 20-year change in Economic Freedom of the World

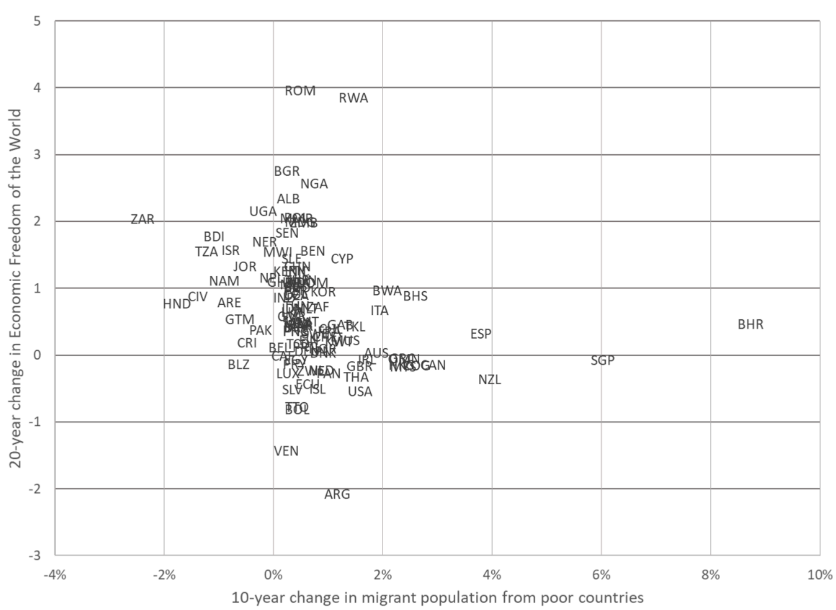

Figure 2. 20-year change in migrant population from poor countries and 20-year change in Economic Freedom of the World

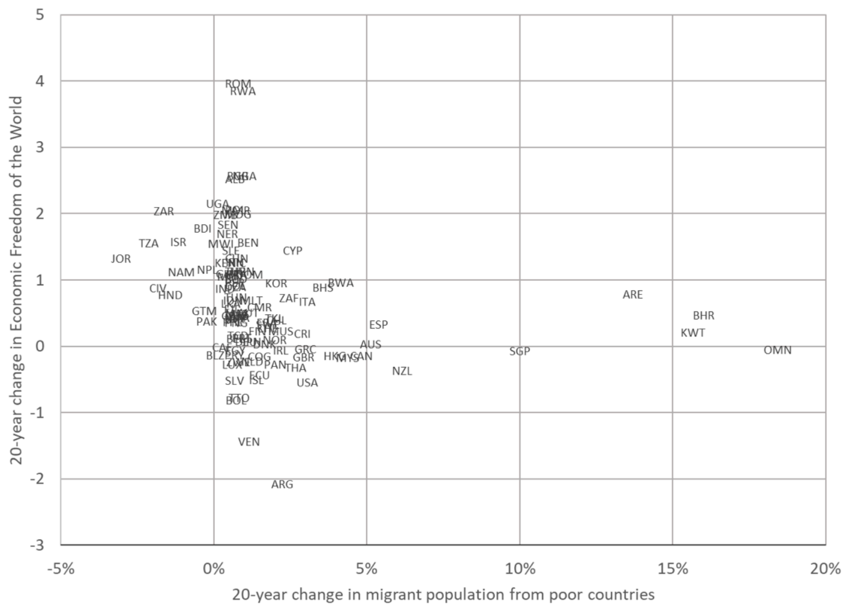

Figure 3. 10-year change in migrant population from poor countries and 20-year change in Control of Corruption

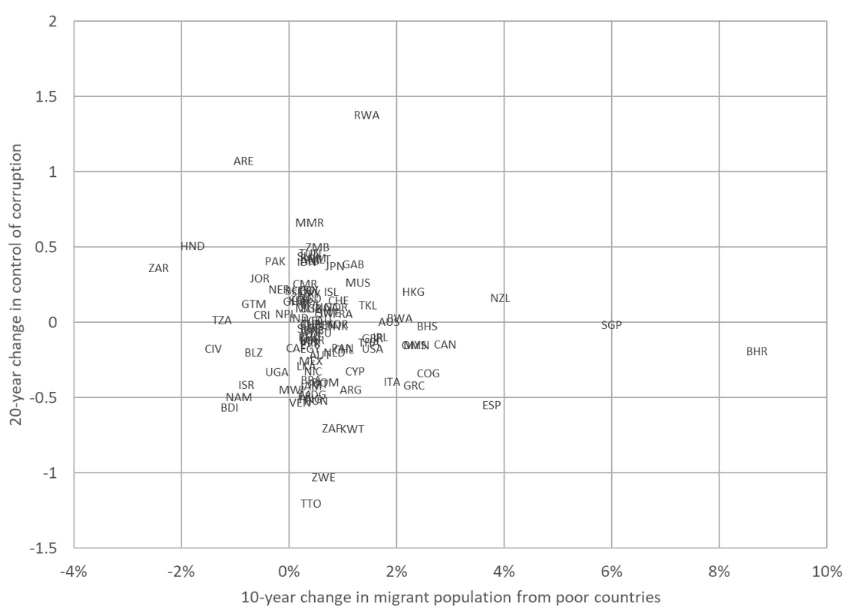

TABLE 4. Pearson correlations: change in migrant population vs. change in institutional quality

	Economic Freedom of the World, 1995–2015	Control of Corruption, 1995–2015	Control of Corruption, 1995–2005 annual mean to 2005–2015 annual mean
Poor, '95–'05	−0.27* [0.00]	−0.11 [0.23]	−0.17 [0.08]
Poor, '95–'15	−0.24* [0.01]	−0.05 [0.64]	
Corrupt, '95–'15	−0.28* [0.00]	−0.05 [0.60]	

Note: 'Poor' and 'Corrupt' denote percentage of the destination country's total population who are immigrants from countries one standard deviation poorer or more corrupt than the destination country. Asterisk indicates significance at 5-percent level, p-values in brackets.

The Pearson correlations, the *r* familiar to economists, are reported to ease discussion of magnitudes, even though the proper functional form is unlikely to have the linear form implied by a Pearson correlation. Consider the −0.27 correlation between the change in migrant share from poor countries between 1995 to 2005 and the change in economic freedom from 1995 to 2015. As a mere matter of prediction—not causation—a linear model would predict that a one standard deviation rise in the change in nation *i*'s population who are from relatively poor countries (1.34 percent of country *i*'s population) would be associated with a decline equal to −0.25 in nation *i*'s EFW score. For 2015 this is approximately the gap between higher-ranked Israel and lower-ranked Iceland, or between higher-ranked Uganda and lower-ranked Indonesia. The United States' value for the change in the U.S. population who are migrants from poor countries between 1995 and 2005 was +1.2 percent, so one would predict a decline in the U.S. EFW score of −0.24.

These results may help explain the tendency toward global convergence in institutional quality reported by Joshua Hall (2016), who notes that there has been substantial convergence in economic freedom around the world since 1980, and hence a decline in the global dispersion of economic freedom scores. Migration from nations with lower EFW scores to those with higher EFW scores would tend to generate both the stylized facts we report here and the stylized fact of a fall in the global dispersion of economic freedom. This is just one suggestion of paths for future work that can build upon the results presented here.

Immigration from relatively poor versus other nations

As a robustness check and as an elementary horserace, we report basic regressions that compare whether immigration from the poorest nations has a weaker or stronger relationship with changes in institutional quality than immigration from other nations. We look at the relationship between the percentage change in migrant population from relatively non-poor countries over the period 1995 to 2005 and changes in our institutional variables, EFW and COFC, over the period 1995 to 2015. Our measure for relatively non-poor countries is drawn directly from the BLP data set: it is a residual, a measure of total migrant population minus migrant population from relatively poor countries, as a percent of the destination country's population. Relatively poor countries are defined as before: countries one standard deviation poorer than the destination country. The two immigration measures will simply be called 'poor' and 'non-poor.'

TABLE 5. Regression results: migrant population from relatively poor versus non-poor countries

	Economic Freedom of the World, 1995–2015 [poor alone]	Economic Freedom of the World, 1995–2015 [poor + non-poor]	Control of Corruption, 1995–2015 [poor alone]	Control of Corruption, 1995–2015 [poor + non-poor]
Poor, '95–'05	−18.65* (6.47)	−17.23* (7.14)	−3.13 (2.61)	−3.40 (2.89)
Non-poor, '95–'05		−4.19 (8.83)		0.80 (3.57)
F-Test: Poor = Rich		0.94		0.59
R^2	0.072	0.074	0.013	0.014
N	110	110	110	110

Note: 'Poor' denotes percentage of the destination country's total population who are immigrants from countries one standard deviation poorer than the destination country. 'Non-poor' denotes percentage of the destination country's total population who are immigrants from any non-poor country, using the same one standard deviation cutoff: 'Non-poor' is thus a residual, total immigrants minus immigrants from poor countries as a percentage of the destination country's population. Asterisk indicates significance at 5-percent level, standard errors in parentheses; constant not reported. The 5-percent critical value for the relevant test of F(1,107) is 3.9.

We report OLS regression results in Table 5. Across the board, the migration variables are once again better predictors of economic freedom than of control of corruption—this may well be driven by the fact that EFW is an aggregate institutional quality variable, a combination of many plausibly important institutional quality factors, while control of corruption is just one among many plausibly important institutional quality factors. The EFW measure may thus contain a stronger overall signal of institutional quality relative to control of corruption. When compared to a simpler regression that only controls for migration from poor countries, including a control for immigrants from non-poor countries offers little improvement in fit. The association between migration from poor countries and EFW is statistically significant, but the non-poor-country immigration statistic is not statistically significant. When control of corruption is the dependent variable, no variable is significant. Perhaps more important than the lack of statistical significance for non-poor-country immigration is the fact that in the EFW regression the non-poor-country coefficient is less than one-fourth the size of the poor-country coefficient. When control of corruption is a dependent variable—where again, neither control is statistically significant at conventional levels—the non-poor country coefficient has a flipped sign—consistent with the theory that non-poor-country migration improves institutional quality—but even in absolute value the non-poor-country coefficient is still less than one-fourth of the value of the poor country coefficient.

F-statistics for the test of equality of poor- and non-poor-country coefficients are never rejected at conventional levels—so a skeptic could hold the view that both coefficients had the same value, somewhere between the poor-country and non-poor-country values. However, the best unrestricted estimates indicate that immigration from poor countries predicts a substantially greater change in relative institutional quality than immigration from non-poor countries.

Conclusion

In their famous paper "Central Bank Independence and Macroeconomic Performance: Some Comparative Evidence," Alberto Alesina and Lawrence Summers (1993) never report a correlation, never report statistical significance. Indeed, they only reported data for sixteen countries. That paper helped create a revolution in how economists think about the relationship between institutions and economic outcomes. As they note:

> The results here are not conclusive in the sense that we have looked at the data only in a very straightforward way; more detailed analysis…is warranted… Our results here do, however, create some presumption that the inflation benefits of central bank independence are likely to outweigh any output costs. (Alesina and Summers 1993, 159)

The present paper adds slightly more statistical detail compared to Alesina and Summers (1993), and has a much larger sample, but the point is similar: that looking "at the data…in a very straightforward way" is the beginning of wisdom. The stylized facts presented here will, we hope, lead to further scientific inquiry into the structural causes of cross-country differences in institutional quality.

Appendix

Data and code related to this research is available from the journal website (**link**).

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