



Simpler Evidence on Immigration and Institutions: An Assessment

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[LINK TO ABSTRACT](#)

The “New Economic Case for Immigration Restrictions” has challenged the consensus view that moving from existing restrictive immigration policies to unrestricted, or free, immigration would generate massive global gains in output—the so-called “trillion-dollar bills on the sidewalk” (Clemens 2011). Those making the new case for immigration restrictions generally posit that factors responsible for the low productivity in immigrants’ origin countries could migrate with immigrants and undermine the high productivity in destination countries, thus wiping out the forecasted trillion-dollar gains. The new economic case for immigration restrictions is an empirical conjecture. However, it was presented (by, e.g., Borjas 2014; 2015) as a theoretical possibility without supporting empirical evidence that immigration has, in fact, carried such a negative externality.

The new economic case for immigration restrictions does not specify the channel through which the factors responsible for low productivity in origin countries are transmitted to destination countries, but the potential impact immigrants have on formal or informal institutions (norms) are one plausible channel. As George Borjas (2015, 961) asks, “What would happen to the institutions and social norms that govern economic exchanges in specific countries after the entry/exit of perhaps hundreds of millions of people?” One of us (Powell), along with coauthors J. R. Clark, Robert Lawson, Alex Nowrasteh, and Ryan Murphy, published the first paper examining how immigration impacts one formal institution related to

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productivity—economic freedom (Clark et al. 2015). Numerous papers have since investigated how immigration impacts other formal and informal institutions related to productivity. These papers have used cross-country and cross-state econometric analyses, synthetic control analyses, and analytical narrative case studies.⁴ Our paper (Bologna Pavlik et al. 2019) was one such study and investigated the impact that immigration could have on corruption in a cross-country analysis.

The paper by Garrett Jones and Ryan Fraser (2021) in this issue of *Econ Journal Watch* is framed explicitly as a critique of our paper (Bologna Pavlik et al. 2019) and as a critique of Clark et al. (2015) and Eugen Dimant, Tim Krieger, and Margarete Redlin (2015). Specifically, Jones and Fraser write “Unfortunately, when reporting the relationship between immigration from relatively poor or corrupt countries and 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 the attention to the potential of overcontrol bias—in particular, of controlling for proxies of the dependent variable—to obscure strong, important statistical relationships in data” (2021, 3–4). Unfortunately for Jones and Fraser, their critique seriously misses its mark. If Jones and Fraser (or the editor and referees of this journal) compared their own results with regards to corruption to the findings in our paper, they would find that they reconfirm our result: there is essentially no relationship between immigration and corruption in destination countries. Similarly, if Jones and Fraser had read Clark et al. (2015) more carefully they would have realized that that paper never examines the flows of immigrants from relatively poorer or more corrupt countries as Jones and Fraser claim. Thus, we are unsure of how that paper could suffer from the overcontrol bias that Jones and Fraser claim that they correct for. In short, Jones and Fraser’s central claim that these studies suffer from overcontrol bias is simply false.

Two of the three papers Jones and Fraser (2021) claim suffer from overcontrol bias examine the impact of immigration on corruption—Bologna Pavlik et al. (2019) and Dimant, Krieger, and Redlin (2015)—while Clark et al. (2015) is the only paper that examines the impact of immigration on economic freedom that they claim suffers from overcontrol bias. Jones and Fraser (2021) use the data set from our paper (Bologna Pavlik et al. 2019) to attempt to show how overcontrol bias masks important relationships between immigration and both economic freedom and corruption. Although they use our data set, our paper never

4. See, e.g., DeBacker et al. (2015); Dimant, Krieger, and Redlin (2015); Clemens and Pritchett (2016); Powell, Clark, and Nowrasteh (2017); Padilla and Cachanosky (2018); Forrester et al. (2019); Nowrasteh, Forrester, and Blondin (2019); Arif et al. (2020); Nowrasteh and Powell (2020); Yao, Bolen, and Williamson (2020; 2021).

examined the impact immigration had on economic freedom; Clark et al. (2015) is the only paper cited by Jones and Fraser that examined that relationship. Economic freedom, not corruption, is the dependent variable in most of the statistically significant results reported by Jones and Fraser (2021). In fact, the phrase *economic freedom* appears 20 times in their paper, and the abbreviation for its measure, “EFW,” appears 15 times, while the word *corruption* appears 33 times and the abbreviation for its measure, “COFC,” appears only 5 times. It seems reasonable to conclude that Jones and Fraser (2021) is as much a critique of Clark et al. (2015) as it is of our paper (Bologna Pavlik et al. 2019) and to evaluate it as such.⁵

We proceed briefly as follows. The next section illustrates how Jones and Fraser (2021) reconfirm the findings of Bologna Pavlik et al. (2019). The section after illustrates how their critique does not apply to Clark et al. (2015). We then critically examine the potential value in Jones and Fraser’s analysis as a standalone empirical contribution to the literature on the relationship between immigration and economic freedom.

Immigration and corruption

In Bologna Pavlik et al. (2019) we studied the relationship between immigration and corruption between 1995 and 2015. Our baseline results examined the relationship between the initial immigrant stock, the subsequent immigrant inflow, and the interaction of the stock and inflow with no contemporaneous controls and only a control for the initial 1995 level of corruption (Bologna Pavlik et al. 2019, 1249 Table 2). Jones and Fraser (2021, 10) note that this is precisely the “simpler statistical evidence” they would like to see. Bologna Pavlik et al. (2019) find no general relationship between immigration and changes in corruption. The paper then goes on to look at the same relationship with contemporaneous controls and the results remained largely unchanged (2019, 1250 Table 3). That still remained true after we used another measure of corruption (International Country Risk Guide (ICRG) in place of the World Bank measure) that allowed us to control for prior trends in corruption for the decade preceding our analysis (2019, 1257 Table 9). The paper goes on to look at the effect of immigration at different levels of corruption and economic freedom in destination countries and the effect of immigrants from origin countries with income that is a standard deviation lower or corruption that is a standard deviation higher than in their destination countries.

5. We will not directly evaluate Jones and Fraser (2021)’s merits as a critique of Dimant, Krieger, and Redlin (2015) though, to some extent, we do so indirectly by examining the relationship between immigration and corruption.

These later regressions based on immigrant origin are the ones that Jones and Fraser take issue with.

Having already shown that the “simpler” evidence in our baseline results did not change when we add controls, we opted to use the fully specified model when further breaking down our data—a fairly standard practice. However, Jones and Fraser are correct that we did not report baseline results without controls for the impact of immigrants from only lower income or more corrupt countries. But what did we find in fully controlled regressions? Our results examining the impact of these two groups over a 20-year period report no statistically significant relationship (Bologna Pavlik et al. 2019, 1253 Table 6a). When we switch to an alternative measure of corruption (ICRG) and control for the trend in corruption for prior periods our results are a little more mixed. Those results find no relationship between immigrants from lower income countries and changes in corruption but do find that immigrants from more corrupt origins are associated with decreased corruption in destination countries at conventional levels of statistical significance. Overall, that finding is an outlier in our paper. How do we write up our results? In our results section we state, “Overall, our results indicate that there is no general long-run association between immigration and corruption. ... Finally, we do not find support for the idea that immigrants from poorer or more corrupt countries will import their origin country’s corruption to their destination country” (ibid., 1254). We reiterate that in the conclusion though we do acknowledge our one outlier finding: “We find limited evidence that increased migration from countries with more corruption may actually reduce corruption in the destination country” (ibid., 1259). Overall, we find a null result.

What do Jones and Fraser find in their simpler and not ‘overcontrolled’ analysis of the relationship between immigration from poorer or more corrupt countries and subsequent changes in corruption? Essentially, a null result. In their Tables 2, 3, and 4 they look at simple correlations (Spearman rank, Kendall rank, Pearson) of the 20-year change in corruption score with two measures (’95–’05 and ’95–’15) of the change in the immigrant population from poorer countries and one measure (’95–’15) of the change in the migrant population from more corrupt countries (Jones and Fraser 2021, 13, 14, 16). None of their nine correlations are statistically significant. Jones and Fraser also run two regressions on the impact of immigrants from poorer countries on corruption in destination countries in Table 5 and similarly report no statistically significant relationship (ibid., 17). Tables 2–4 do report something labeled “Control of Corruption, 1995–2005 annual mean to 2005–2015 annual mean” for the measure of immigrants from poor countries from ’95–’05 but not for their other two measures. It is not clear what these results are measuring as the labels are unclear and they are not described in text of their results, but two of the three measures are statistically significant at the 10 percent level. It

would be safe to characterize their overall result as finding no relationship between immigrants from poorer or more corrupt countries and changes in corruption in destination countries.

Since Jones and Fraser (2021, 9–10) take issue with the possibility of “over-control bias” in our Tables 6a, 6b, 8, and 10, it would have been more straightforward to test for such bias by replicating our results and then removing the variables that they suspect are responsible for overcontrol bias. We do that here for our baseline (Bologna Pavlik et al. 2019, 1253 Table 6a) in this article’s Table 1.

TABLE 1. The effect of immigration from relatively ‘worse’ origin countries on corruption, with and without basic controls

	Dependent variable: Corruption in 2015							
	Lower-income migrants				More-corrupt migrants			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
‘Worse’ immigrant stock	0.527 (0.760)		0.097 (1.033)		0.828 (0.587)		0.800 (0.640)	
‘Worse’ immigrant net inflow		0.543 (1.766)		-2.053 (2.109)		0.538 (1.215)		-0.939 (1.525)
Corruption 1995	0.927*** (0.028)	0.929*** (0.029)	0.807*** (0.074)	0.810*** (0.078)	0.908*** (0.032)	0.924*** (0.034)	0.805*** (0.078)	0.819*** (0.082)
GDP per capita			-0.050 (0.066)	-0.008 (0.063)			-0.079 (0.065)	-0.028 (0.068)
EFW			0.214** (0.084)	0.246*** (0.091)			0.188** (0.085)	0.230** (0.090)
Polity			-0.008 (0.015)	-0.014 (0.015)			0.001 (0.014)	-0.010 (0.015)
Shadow economy			-0.002 (0.001)	-0.002 (0.001)			-0.002 (0.002)	-0.002 (0.002)
Human capital			0.097 (0.078)	0.039 (0.092)			0.113 (0.069)	0.063 (0.084)
Freedom of the press			0.002 (0.004)	0.002 (0.004)			0.003 (0.004)	0.002 (0.004)
Constant	-0.037 (0.036)	-0.030 (0.037)	-1.214** (0.550)	-1.633** (0.628)	-0.058 (0.038)	-0.032 (0.037)	-0.904* (0.543)	-1.435** (0.672)
Observations	110	110	100	100	110	110	100	100
R ²	0.888	0.887	0.902	0.903	0.892	0.887	0.905	0.902
Adjusted R ²	0.886	0.885	0.893	0.895	0.890	0.885	0.896	0.894

Notes: Robust standard errors given in parentheses. *, **, *** denote statistical significance at the 10-, 5-, and 1-percent levels respectively. All ‘basic’ controls are averaged using all available values from 1995–2015.

All contemporaneous controls have been removed and we control only for initial levels of corruption at the start of the period such that we are focusing on *changes* in corruption over a 20-year period. One might be able to construct an argument that controlling for initial levels of corruption leaves a collider bias when examining how accumulated stocks of immigrants impact the subsequent 20-year change in

corruption but is implausible to argue that subsequent 20-year flows of immigrants somehow impacted initial levels of corruption. Table 1 compares this relatively uncontrolled regression with the results reported in Table 6a in our original paper.⁶ When we remove the additional controls there is still no statistically significant relationship between immigrants from lower-income countries and corruption in destination countries though one measure does change signs to become positive (less corrupt). Similarly, the results when jettisoning the controls in the regressions examining the impact of immigrants from relatively more corrupt origins remain statistically insignificant and one measure also changes signs to become positive (less corrupt). Thus “overcontrol” is not biasing our previously reported results.

So did our paper overcontrol and thus hide a harmful impact of immigration on corruption, as implied by Jones and Fraser? Most of our results find no relationship between immigration and corruption. That’s the conclusion we emphasized. When Jones and Fraser look at the relationship between immigrants from only lower-income or more corrupt countries and subsequent changes in corruption with no controls, most of their results find no relationship. When we modify our main table that they took issue with and report uncontrolled results, we again do not find the bias claimed by Jones and Fraser. Jones and Fraser’s simple data and our update to Table 6a are one more robustness test on our null results. These results remain largely the same. Thus, there was no overcontrol bias masking an otherwise significant harmful relationship between immigration and corruption. Thanks.

Immigration and economic freedom

Economic freedom is the dependent variable in most of the statistically significant correlations found in Jones and Fraser’s paper. The majority of their textual discussion of their findings is dedicated to discussing economic freedom rather than corruption. Perhaps the bulk of their contribution then is to fix the “overcontrol bias” in the Clark et al. (2015) paper that investigated the impact of immigration on economic freedom, rather than the Bologna Pavlik et al. (2019) paper, which investigated the impact of immigration on corruption. After all, in their opening paragraph Jones and Fraser cite both of these papers and then explicitly state “Unfortunately, when reporting the relationship between immigration

6. Please note that we inverted the Control of Corruption measure in Bologna Pavlik et al. (2019) while Jones and Fraser (2021) did not. Here we do *not* invert it so that Table 1 is more easily comparable to Jones and Fraser’s results. Thus in Table 1 positive coefficients signal greater control of corruption, i.e., less corruption.

from relatively poor or corrupt countries and 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” (Jones and Fraser 2021, 3–4, our emphasis). We challenge Jones and Fraser, the editor of EJJW, or readers to find any regressions in Clark et al. (2015) that break out the relationship between immigrants from origin countries one standard deviation poorer or more corrupt than destination countries as was done in Bologna Pavlik et al. (2019) and Jones and Fraser’s critique. Clark et al. (2015) report 32 different regression results but none break out the data in the way that Jones and Fraser erroneously assert.

The closest that Clark et al. (2015) come to looking at the impact of immigrants from poorer countries is when they separate the stock of immigrants in a destination country by whether they came from OECD or non-OECD origin countries (regressions 2, 7, and 12 in Clark et al. 2015, 327, 328, 329). But note that even in this case “overcontrol bias” is not a problem. Regression 2 in Table 2 of the original article contains no other contemporaneous controls, controlling only for the initial level of economic freedom (*ibid.*, 327). This exact same regression appears in Bologna Pavlik et al. (2019, 1249) with corruption replacing economic freedom and in that case Jones and Fraser (2021, 10) note that these are “examples of simpler statistical evidence” that they would like to see. Thus, this cannot be the result that they take issue with, yet Clark et al. (2015) never attempt to look at any other measure of immigration from relatively poorer origins. Clark et al. do add additional controls as they continue analyzing the impact of immigrants from OECD and non-OECD origins and find that in each case, non-OECD immigrants are associated with larger subsequent improvements in economic freedom. The magnitude of the coefficient and its statistical significance increases as additional controls are added, but it remains that the results are significant in the baseline results with no contemporaneous controls. Scholars can legitimately debate which model most accurately captures the relationship but there is no “overcontrol bias” masking a hidden relationship.

Clark et al. (2015) was the first paper to empirically examine the new economic case for immigration restrictions. As such, that paper focused on the most general relationship between total immigrant stocks and flows and a measure of institutional quality (economic freedom) associated with higher productivity. It is unsurprising that papers building on that contribution have continued to refine how they look at data to examine how different origins, destinations, regions, times, or outcomes contribute to our understanding of the merits or demerits of the new economic case for immigration restrictions. To our knowledge, no one has examined the impact of immigration on economic freedom in a cross-country setting while specifically looking at the impact of immigrant flows from relatively poorer or more corrupt origins as we did in our paper examining the impact of

those immigrant flows on corruption. Rather than issuing misguided claims about overcontrol bias in existing studies, Jones and Fraser could have addressed this gap in the literature. Unfortunately, since they did not make a full analysis of the impact of these immigrant flows on economic freedom the sole focus of their study, their contribution to filling this gap is inadequate at best.

What do Jones and Fraser contribute?

Jones and Fraser (2021) is the first study to look at how immigrants from relatively poorer countries impact economic freedom in destination countries in a cross-country analysis. This is an important topic that is worthy of inquiry. Unfortunately, Jones and Fraser contribute only a scatterplot, a few simple correlations, and two OLS regressions to this inquiry. While scatterplots and correlations are a fine starting point, that is all that they are, a starting point. In this case they are also a misleading starting point, as we will show by replicating and then building on Jones and Fraser's Table 5 OLS regressions (2021, 17).

There are hundreds of papers that have used economic freedom as an explanatory variable over the last 25 years, but more recently a sizable literature has developed examining economic freedom as the dependent variable.⁷ The most consistent finding across studies that examine changes in economic freedom is that higher initial levels of economic freedom are negatively associated with subsequent increases in economic freedom. In other words, the higher the initial economic freedom level, the harder it is to improve. This is dictated, at least in part, by the construction of the index itself. The earlier literature studying economic freedom as an independent variable has found it strongly correlated with higher income levels, and it is well known that immigrants tend to immigrate from poorer countries to richer countries.⁸ This all implies that Jones and Fraser (2021) have built-in omitted variable bias in their simple correlations. We rectify this by replicating their OLS regression and then controlling for the initial level of economic freedom in Table 2. Since their OLS analysis analyzed the impact of subsequent flows of immigration, it is not plausible that initial levels of economic freedom were somehow caused by these subsequent flows. Thus, overcontrol via collider bias is not possible.

Note that Jones and Fraser (2021) claim in their text that they are following the definition of relatively poor as used in Bologna Pavlik et al. (2019): “Relatively

7. See Hall and Lawson (2014) for a survey of the literature using economic freedom as an explanatory variable and see Lawson et al. (2020) for a survey of the literature examining economic freedom as a dependent variable.

8. Additionally, Ashby (2010) finds that even once per capita GDP is controlled for higher economic freedom is still associated with higher immigration.

poor countries' are defined as before: countries one standard deviation poorer than the destination country" (Jones and Fraser 2021, 17). However, in examining their data and code it turns out that they mistakenly coded relatively poor as simply immigrants from the poorest 50 percent of countries. Our Table 2 uses their stated definition—one standard deviation—but in our appendix we include a table conducting an identical exercise with their mistaken coding of relatively poor as bottom 50 percent and show essentially the same results.

Regressions 9 and 10 in Table 2 replicate Jones and Fraser's result and, like their Table 5 (Jones and Fraser 2021, 17), find that greater immigrant flows are negatively associated with subsequent changes in economic freedom. Regressions 11 and 12 in Table 2 repeat these same regressions while adding only a control for the initial level of economic freedom. As can be seen, in both regressions the impact of immigrant inflows from relatively poorer countries loses its statistical significance, the coefficients decrease substantially in magnitude, and in one case changes sign to become positively associated with subsequent increases in economic freedom. Regressions 13 and 14 in Table 2 repeat this same exercise while changing our immigrant inflow variable to include the entire 20-year time period. The net inflow of immigrants from poorer countries remains statistically insignificant in both regressions. However, now both measures have positive coefficients indicating a greater immigrant inflow would be positively associated with larger improvements in economic freedom.

TABLE 2. The effect of immigration from relatively 'worse' origin countries on economic freedom

	Dependent Variable: Change EFW 1995–2015					
	Inflow 1995–2005				Inflow 1995–2015	
	(9)	(10)	(11)	(12)	(13)	(14)
'Poor' immigrant net inflow	-19.682** (7.590)	-15.690* (8.510)	2.714 (6.252)	-0.011 (6.658)	0.595 (2.265)	0.540 (2.271)
'Non-poor' immigrant net inflow		-8.101 (6.483)		7.224 (6.148)		2.369 (3.406)
Economic Freedom 1995			-0.489*** (0.054)	-0.507*** (0.056)	-0.483*** (0.052)	-0.501*** (0.058)
Constant	0.755*** (0.094)	0.748*** (0.094)	3.671*** (0.330)	3.786*** (0.344)	3.645*** (0.322)	3.742*** (0.352)
Observations	110	110	110	110	110	110
R ²	0.059	0.068	0.466	0.473	0.466	0.468
Adjusted R ²	0.0499	0.0505	0.456	0.458	0.456	0.453
<i>Notes:</i> Standard errors given in parentheses. *, **, *** denote statistical significance at the 10-, 5-, and 1-percent levels respectively.						

We do not view these results as definitive in the least. The impact of immigrants from poorer (or less free, more corrupt, etc.) countries on destination

country institutions is a subject worthy of a much more thorough investigation than is appropriate as a response to Jones and Fraser. Our results here only indicate that their simple analysis sheds little light on the relationship.

Conclusion

Jones and Fraser (2021) fails massively as a critique of Bologna Pavlik et al. (2019) and Clark et al. (2015). Jones and Fraser claim that both studies suffer from overcontrol bias and thus the results of these studies cannot be trusted. When Jones and Fraser attempt to show the simple relationship between immigration from poorer or more corrupt countries and corruption without any controls they find essentially no relationship. That is generally the same conclusion Bologna Pavlik et al. (2019) found and emphasized. Thus, overcontrol bias is not a problem in that study. Jones and Fraser are even more misguided in their claim that they are correcting for overcontrol in Clark et al.'s (2015) examination of the impact of immigration on economic freedom because Clark et al. never examine the relationship between flows of immigrants from poorer or more corrupt countries and economic freedom as Jones and Fraser claim! We wonder how such a misguided framing even made it through a peer review process.⁹

If there is any merit in Jones and Fraser's paper it is that they are the first paper to *start to examine* the relationship between immigrants from poorer or more corrupt countries and the impact they have on economic freedom on destination countries. Unfortunately, we must emphasize 'start to examine' because, as written, that investigation is seriously underdeveloped. As we illustrated in the previous section, once the most standard control from the literature studying changes in economic freedom as a dependent variable is included (the initial level), Jones and Fraser's claimed negative relationship between immigration flows and changes in economic freedom loses statistical significance and reverses sign.

Where does this leave the state of the debate about the empirical relevance of the new case for immigration restrictions? Jones and Fraser have given us no reason to discount the general findings of the two papers they criticize. They do find new results that were previously uninvestigated showing simple scatterplots and correlations between immigration from poorer or more corrupt origins are

9. Our wonder has subsequently decreased after correspondence with EJW editor Dan Klein where he encouraged us to consider removing this sentence because in his view editors and referees wouldn't normally check the legitimacy of citations such as Clark et al. (2015) in Jones and Fraser's paper (2021). In fact, the editor emailed one of us (Powell) asking for a copy of Clark et al. (2015) after we submitted our reply to the accepted Jones and Fraser (2021) critique. Readers can judge for themselves whether an editor or a journal's referees should be familiar with one of the main papers a submission purports to critique.

correlated with subsequent decreases in economic freedom, but that result falls apart with a single standard control. In contrast, a multitude of studies have found either no statistically or economically significant relationship between immigration and economic freedom or a positive relationship.¹⁰ Nowrasteh and Powell (2020) comprehensively document all of the studies to date that empirically assessing the new case for immigration restrictions and conclude that “there is no Q.E.D. here. We cannot rule out that, in some cases, in some places, from some particular immigrant flows, a negative externality that undermines formal and informal institutions or norms related to productivity does exist. However, in general, our findings should make scholars skeptical of how widely relevant the new case for immigration restrictions is” (p. 284). Jones and Fraser have given us no cause to revise that conclusion.

Appendix

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

TABLE A1. The effect of immigration from relatively ‘worse’ origin countries on economic freedom, using Jones and Fraser’s actual measures of immigration inflows (bottom 50 percent of countries defined as poor)

	Dependent Variable: Change EFW 1995–2015					
	Inflow 1995–2005				Inflow 1995–2015	
	(15)	(16)	(17)	(18)	(19)	(20)
‘Poor’ immigrant net inflow	-18.648*** (6.465)	-17.229** (7.143)	0.836 (5.394)	-2.214 (5.620)	0.595 (2.265)	0.540 (2.271)
‘Non-poor’ immigrant net inflow		-4.193 (8.827)		12.025* (6.875)		2.369 (3.406)
Economic Freedom 1995			-0.483*** (0.054)	-0.508*** (0.056)	-0.483*** (0.052)	-0.501*** (0.058)
Constant	0.727*** (0.089)	0.733*** (0.090)	3.645*** (0.335)	3.778*** (0.341)	3.645*** (0.322)	3.742*** (0.352)
Observations	110	110	110	110	110	110
R ²	0.072	0.073	0.466	0.481	0.466	0.468
Adjusted R ²	0.0629	0.0562	0.456	0.466	0.456	0.453
<i>Notes:</i> Standard errors given in parentheses. *, **, *** denote statistical significance at the 10-, 5-, and 1-percent levels respectively.						

10. Powell, Clark, and Nowrasteh (2017); Padilla and Cachanosky (2018); Nowrasteh, Forrester and Blondin (2019); Arif et al. (2020); Nowrasteh and Powell (2020); Yao, Bolen, and Williamson (2020, 2021).

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