Post scriptum

In this post scriptum (September 14, 2020), I briefly make five remarks regarding Karadja and Prawitz (2020) (https://swopec.hhs.se/uunewp/abs/uunewp2020_005.htm) reply to my original comment below (starting on p. 6). See also the second post scriptum on p.4 (added September 20, 2020)

Starting with a discussion of the Swedish emigration data

- KP (2020) argue that I give a biased and incorrect view of the Swedish emigration statistics. For example, they question that my estimate of 1.5 million emigrants during the period 1860 to 1920 is much too large given previous estimates.[1] However, my estimate is based on the officially reported emigration of 1.3 million emigrants (or to be precise 1,272,279) taken from Statistics Sweden (e.g., see link https://www.scb.se/en/finding-statistics/statistics-by-subject-area/population/population-composition/population-statistics/pong/tables-and-graphs/yearly-statistics--the-whole-country/population-and-population-changes/).[2] In addition, I estimate that underreporting was, on average, about 15% since it has been estimated that the official statistics underreports the emigration statistics between 10% to 40% depending on the specific time period (e.g., Sundbärg 1913, Johansson 1976). Thus, my estimate of 1,5 million (≈1.3+0.15*1.3) is therefore consistent with Sundbärgs's estimates of the degree of underreporting in the Swedish statistics.[3]

- Relatedly, KP (2020) argue that there was little or no underreporting period 1885-93, citing Sundbärg (1913). However, Statistics Sweden, i.e., the government agency that produces the official statistics, clearly disputes this claim. Indeed, in a report, "Swedish Population History", published by Statistics Sweden, Hofsten and Lundström (1976) write

  "In 1884 more strict control of emigration began. According to Sundbärg, this had a decisive effect on the emigration statistics. In fact, he was of the opinion that practically all the emigrants were registered in the 1885-93 period (Sundbärg 1910a, p. 251). An official report from 1887, does not quite take the same view. The emigration statistics are not said to be practically comprehensive, but instead it is pointed out that the strict control of the emigrants to countries outside Europe had resulted in a considerably improvement in the emigration statistics. However, where emigrants to European countries were concerned the parish register were still defective (BiSOS A 1885, p. XVII)."[4]

---

[1] Here it is important to point out that Karadja and Prawitz (2019) do not use data from the Swedish official statistics (i.e., "summariska folkmängdsredogörelserna") but rather data collected from the church books by Swedish genealogists combined with ship passenger lists. The data from the church books has a number of additional problems compared to the official statistics, i.e., all geographical units are not covered, data entry typos etc, while the passenger lists often lacks precise information about the place of residence.

[2] Karadja and Prawitz also argue that "the 1.3 million claim is uncertain" when it is not.

[3] Sundbärg's estimates of underreporting ranges between 10-20% for the period 1860-1885. However, these estimates are generally considered to be too low (Johansson 1976, p.170).

[4] Another reason for the underreporting of immigrants after 1884 is provided by Tedebrand (1972, p. 321) since he finds that a large number of emigrants still emigrated illegally to the U.S, i.e., without a change-of-address certificate, via ports in Norway or Denmark.

Moreover, if KP had bothered to compare their migration variable with the official data, then they would have realized that Sundbärg's claim is wrong. In fact, their data includes 4% more individuals over the period 1885-93 than the official statistics,[5] which again suggests that there are still problems with the official statistics even after 1884 despite the claim made by Sundbärg.[6]

To sum up this discussion of the data, the claim by Karadja and Prawitz (2020) that I make a "selective reading" of the literature on the Swedish emigration statistics is false.[7] Instead, it is KP (2020) that once again provide an incorrect description of the problems with the emigration statistics.

Turning to a discussion of the econometric issues

- I note that KP (2020) criticize my solution to the measurement error problem where I use total out-migration, i.e., the sum of true emigration and true internal migration, instead of the error-ridden emigration variable used by KP.[8] They argue that this solution is not valid since internal migration will be correlated with the error term due to "the dynamic relationship between internal and external migration." However, if their argument is correct, then they fail to realize that their IV approach is also going to be flawed for the very same reasons (i.e., they have omitted internal migration from their specification).[9] In other words, KP's instrumental variable estimator is then inconsistent since their instrument is correlated with the omitted variable, i.e., internal migration.[10] Nonetheless, my approach is still valid if we are interested in the effect of total outmigration on labor movement, rather than effect of emigration per se (i.e., the ceteris paribus effect of emigration), since then we do not need to assume that their instrument is unrelated to internal migration; only that it is unrelated to total out-migration.

- Regarding the issue about what type of unobserved geographical heterogeneity that must be controlled for in order for the empirical analysis to be credible, KP (2020)

[5] In fact, KP's data includes even more data for years 1885 and 1886, i.e., 9% and 8% respectively.

[6] The official statistics include 354,215 registered migrants whereas KP data includes 368,858 observations. The most probable reason why KP has more observation is that they include data from ships passenger lists. However, the discrepancy between these two sources might also reflect other problems with KP's variable discussed in footnote 1. Importantly, using KP's migration data instead of the official data does not solve the measurement problem since data on emigrants to European countries are still missing. The measurement error problem in KP is also exacerbated by the fact that emigration to European countries (e.g., Denmark and Germany) mostly occurred from to the southern part of Sweden. As a result, the measurement error will therefore be correlated with parts of KP's instrument, i.e., distance to ports (Göteborg and Malmö).

[7] Interestingly, Hofsten and Lundström (1976) have even a chapter called "defects in the migration data" where they write "it was generally acknowledged that the migration statistics were defective. Statements of this effect can, for instance, be found in reports from the Statistical Commission and the declarations of the clergy."

[8] Using total out-migration also solves the problem of emigrants without a change-of-address certificate since they will be counted as out-migrants as soon as they have been recorded to be absent from the municipality, i.e., when they were recorded in "obefintlighetslängderna" (Johansson 1976). As a result, the sum of total out-migration will be, by construction, equal to the sum of the true emigration and the sum of true internal migration.

[9] In this case, they must also find a second instrumental variable for internal migration since there are now two endogenous variables in this specification.

[10] KP (2020) is right about that the direction of bias of first-stage estimate is not one would expect if there is underreporting, i.e., a downward bias. However, the first-stage effect might be (upward) biased due to violation of the exclusion restriction, i.e., the instrument being correlated with internal migration or unobserved geographical heterogeneity, too, as I discussed in footnote 21 below.

argue that my "suggestion to include weather-station fixed effects is similar but even more demanding than including frost shock fixed effects. This is because, as we use cross-sectional variation, there is (almost) no variation in frost shocks within a weather station" This last statement is clearly wrong since I find significant reduced form effects when I include weather-station fixed effects (see Column 3 in Panel B in my Table 4 below).[11] Moreover, I also find that the first stage effect is zero in this case (see Column 3 in Panel A in Table 4 below). Thus, these two findings can also be interpreted as a falsification test of KP's instrumental variable approach since KP's instrument violates the exclusion restriction if there is no first-stage effect but a significant reduced form effect (e.g., Angrist and Pischke 2009, p. 131). Interestingly, KP (2020) seem to be unaware of that the new results they present in Table 2 also strongly suggest that the exclusion restriction is violated. Indeed, if one controls for a full set of frost shocks then there is no first-stage effect but still a significant and large reduced form effect that is similar across almost all other specifications (see Column 4 in their Table 2). In addition, that the reduced form effect is still significantly different from zero and completely unaffected by the inclusion of a full set of indicators for frost shocks also refutes their claim that controlling for frost shocks in a "saturated way", "would drastically reduce the available identifying variation and was never considered as an option."

- Finally, KP (2020) criticize that I present results from a "bare bone" specification that includes what I consider to be the key control variables used by KP (i.e., Shocks, Port, the initial population size in 1865). In this specification the reduced form effect is very small and even negative, i.e., -0.0002, and thus very different from KP's (2019) results in Table 4, where they report a reduced form effect in the range 0.0014-0.0017. They argue that my specification does not accurately reflect their identification approach since I have omitted regional fixed effects from the specification. However, I find it worrisome that they need to condition on a set of control variables in order to find a significant reduced form effect since these control variables might act as colliders and thus introduce collider bias. Indeed, collider bias induces associations where there is no true causal effect. In addition, I find it disturbing that KP (2019) did not comment on that they needed to condition on a large number of variables in order to find a significant reduced form effect while, at the same time, the first-stage effect became increasingly smaller the more control variables they added to their specification. In my view, this information should have been provided by KP, since it reveals whether their design is likely to be credible or not, but they failed to do so. However, I have provided this information in Table 4 below where I also make the statement "Taken together, these results in Table 4 strongly suggest that there does not exist a causal relationship between emigration and labor movements since there is no consistent relationship between the first-stage and reduced form results in KP's analysis."

To conclude, the IV results in KP (2019) is not reliable since the estimated effects are very sensitive to the choice of control variables included in the regressions and the choice of how to measure emigration correctly. Specifically, the reduced form effect can be both significantly positive, zero, or negative depending on the choice of control variables while the first-stage

[11] It is noteworthy that the reduced form effect switches sign but still has exactly the same magnitude as in KP (2019) if one controls for weather station fixed effects instead of region fixed effects. This finding suggests that KP's cross-sectional results are affected by severe omitted variable bias at the weather station level.

effect can be both be close to zero or large depending on how emigration is being measured and the choice of control variables included in the empirical analysis.

Post scriptum II (added September 20, 2020)

I noticed that KP (2020) argue that their results are robust to sample restrictions, i.e., "dropping southern municipalities where European migration is likely more common" as they report in Figure 1. However, there are other reasonable ways of testing whether the first-stage estimates and the reduced form effects are robust to changes of the sample, which also considers not only measurement error, but other, equally problematic, concerns such as unobserved geographical heterogeneity.

In the table below, I have estimated exactly the same first-stage and reduced form regressions as preferred by KP but where I include different municipalities depending on the (log) distance to port, *lproxemiport*, one of the key variables used by KP (2019). In column 1, I have included the estimates from KP (2019) for ease of comparison with my results. In column 2, I only include those municipalities that are closest to port, i.e., those with *lproxemiport* >2 as measured according to their variable. There are 83 such municipalities. The estimated first-stage is then 0.332 and the corresponding reduced form effect is -0.068. Thus, the estimates are very different (both in magnitude and sign) from KP result shown in Column 1. In Column 3, I add those municipalities with *lproxemiport* >1.5, in column 4, I add those with *lproxemiport* >1, in column 5, I add those with *lproxemiport* >0.5, …, and finally, in Column 9, I add those with *lproxemiport* <-1.5. These specifications thus include an increasing number of municipalities with an increasing distance to emigration ports, i.e., 83, 177, 338, 683, 1099, 1527, 2070, 2291 respectively.

As can be seen from this table, both the first-stage estimates and the reduced form effect are both statistically positive or negative depending on the municipalities included in the regressions. It is only in the last two specifications when municipalities furthest way from emigration ports also are included in the estimation (i.e., when at least 2,070 of all the 2,359 municipalities) that the estimates are broadly similar to KP (2019). Again, these results reveal the substantial fragility of the results KP (2019). [12]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
lproxemiport		>2	>1.5	>1	>0.5	>0	>-0.5	>-1	>-1.5
First-stage effect	.062 (.015)	.332 (.75)	-.072 (.116)	.263 (.131)	-.360 (.157)	-.005 (.106)	-.005 (.066)	.091 (.024)	.063 (.017)
Reduced form effect	.0017 (.0004)	-.068 (.011)	.004 (.004)	.013 (.002)	.013 (.006)	-.002 (.005)	-.0014 (.004)	.0021 (.0005)	.0015 (.0006)
Number of municipalities in the sample	2,359	83	177	338	683	1,099	1,527	2,070	2,291

Finally, I also need to comment on KP's choice of indexing the variable *Shocks* with *m* (and by implication also their instrument: Shocks×Ports). *Shocks* is constructed from data at the

[12] I presented these results for KP in the fall of 2019 when discussing their empirical analysis. They choose to ignore my results. Instead, they now argue their results are robust to changes in the sample of municipalities.by only showing the results in Figure 1 in KP (2020).

weather station level (32 units) and cannot therefore vary at the municipality level but only at a more aggregated level. Indeed, the variable *Shocks* only takes 12 distinct values. KP (2020) seem not to understand this point in my original comment below.

Exit, Voice and Political Change: Evidence from Swedish Mass Migration to the United States A Comment*

Per Pettersson-Lidbom#

This version: April 10, 2020

Abstract

In this comment, I revisit the question raised in Karadja and Prawitz (2019) concerning a causal relationship between mass emigration and long-run political outcomes. I find that their analysis fails to recognize that their independent variable of interest, emigration, is severely underreported since approximately 30% of all Swedish emigrants are missing from their data. As a result, their instrumental variable estimator is inconsistent due to nonclassical measurement error. Another important problem is that their instrument is unlikely to be conditionally exogenous due to insufficient control for confounders correlated with their weather-based instrument. Indeed, they fail to properly account for non-linearities in the effect of weather shocks and to control for unobserved heterogeneity at the weather station level. Correcting for the any of these problems reveals that there is no relationship between emigration and political outcomes.

---

* I am grateful to Erik Prawitz for providing a file with the names of the municipalities in order to match my variable to their JPE data set. I am also grateful to Erik Prawitz. Mounir Karadja, Björn Tyrefors and David Strömberg for useful discussions.

Department of Economics, Stockholm University, and Research Institute of Industrial Economics (IFN), E-mail: pp@ne.su.se

1. Underreporting of emigrants

In this section, I will discuss the problem of underreporting of emigrants in the Swedish statistics and how that leads to inconsistency in the instrumental variable estimator used by Karadja and Prawitz (2019) (henceforth, KP) since the measurement error is nonclassical; that is, the variable of interest and its measurement error are *not* uncorrelated, and the expected value of the mismeasured variable is *not* equal to the expected value of the true measure. I will also present a solution to the underreporting problem.

It is well known that the Swedish emigration statistics during the 19th century and early 20th century are unreliable due to the severe underreporting of emigrants. This has been documented and discussed, for example, by Emigrationsutredningen (1909, p. 593), Johansson (1976), Odén (1964, 1971), Ahlberg (1976), Eriksson (1969), and Vernersson Wiberg (2016). These studies show not only that the emigration to the U.S. was severely underreported but also that the emigration to other countries within Europe (e.g., Denmark and Germany) was even more underreported.

It is noteworthy that the studies discussing the problems with the Swedish emigration statistics are not cited in KP.[13] As a result, the discussion in KP that the emigration data are reliable on p. 1876 is incorrect.[14] Specifically, the claim "it is possible to ascertain their [parish reports and ship passenger lists] accuracy by cross-checking the two sources" is erroneous since parish records reported emigration to all countries, while ship passenger lists essentially recorded only emigration to the United States. Indeed, Eriksson (1969) finds that the overlap of individuals between these two sources is only 44%. Part of this discrepancy is due to parish records only registering individuals with a change-of-address certificate.[15] Thus, KP cannot solve the underreporting problem by using a "single emigration variable defined the maximum of either the church book or passenger list data each year" since there will be a very large number of missing emigrants.[16] Moreover, even unifying the two data sets would be insufficient since there would still be a large number of emigrants who are not recorded in

[13] This literature should have been familiar to KP since I had already provided references to this work in 2015 when I suggested that they must address the problem with measurement errors in the Swedish emigration statistics.

[14] KP's claim that their data sets encompass "the universe of registered emigrants during the Age of Mass Migration" is also incorrect since their parish data is estimated to contain only 75% of all emigrants. Data from a number of parishes is also missing in their data (see link https://emiweb.se/?services=emigranter-i-svenska-kyrkbocker/%20).

[15] This problem has been regarded as the chief explanation of the discrepancy between actual and recorded emigration, Johansson (1976).

[16] Importantly, KP lack data from the church books after 1895.

either of these sources, i.e., those who did not apply for a change-of-address certificate and who emigrated to countries other than the U.S. A similar point is also made in Johansson (1976) and Odén (1971).

Most important, I have estimated that KP's emigration variable only includes at most 73% of all emigrants during the period 1860-1920.[17] As a result of this large underreporting of emigration, the KP instrumental variable approach will be inconsistent due to this type of nonclassical measurement error.

To formally illustrate the measurement error problem in KP and how it can be solved, let X^* denote the true emigration. The population regression of interest in KP's analysis can now be expressed as

$$(1) \qquad Y_i = \alpha + \beta X_i^* + u_i,$$

where Y_i is some political outcome in municipality i, and X_i^* is the *true* total sum of emigrants who have emigrated (i.e., moved *outside* Sweden) from municipality i during the period 1867-1920. KP uses an instrumental variable approach in which they replace the true value of X_i^* with a severely underreported measure, X_i, as noted above. Then, they assume that their instrumental variable, Z_i, is uncorrelated with both the population error term u_i and the reporting error $e_i = X_i - X^*_i$. However, because KP replaces the true value in the equation with the underreported value, the instrument variable estimator is *not* consistent since the probability limit of the instrumental variable estimator can now be written as

$$(2) \quad plim\, \beta^{IV} = \frac{Cov(Y,Z)}{Cov(X,Z)} = \frac{Cov(\beta X^* + u, Z)}{Cov(X^* + e, Z)} = \frac{\beta Cov(X^*, Z) + Cov(u, Z)}{Cov(X^*, Z) + Cov(e, Z)}$$

and, due to nonclassical measurement errors, i.e., $Cov(e, X^*) \neq 0$, the instrument will also be correlated with the reporting error, i.e., $Cov(e, Z) \neq 0$. Thus, $plim\ \beta^{IV} \neq \beta$ even if $Cov(u, Z)=0$ holds. In fact, equation (2) shows that the estimate from the instrumental variable method will be biased upward if the measurement error is negatively correlated with the true value, i.e., $Cov(e, Z)<0$.[18] In the case of underreporting of emigrants, this correlation will most likely be

[17] This calculation is partly based on the official statistics, i.e., those with a change-of-address certificate, which recorded 1.3 million emigrants during the period 1860-1920. I have also estimated that a minimum of 0.2 million emigrants were not recorded during this period due to various sources of errors discussed by Johansson (1976) and Eriksson (1969), among others. Thus, at least 1.5 million individuals emigrated from Sweden during the period 1860-1920. Consequently, a minimum of 0.4 million emigrants are missing from KP's data since they only include 1.1 million emigrants.

[18] The expression for the IV estimator in equation (2) does not recognize that there may be errors in both Y and Z, that other included variables may be measured with error, and that all these errors may be corelated with each other. In such a general model, it is virtually impossible to sign the direction of the bias of the IV estimator. For

negative since there cannot be any underreporting if there is no emigration (i.e., emigration cannot be negative!), whereas there will be underreporting if there is emigration, and the greater the size of emigration is, the higher the underreporting. Stephens and Unayama (2019) also make a similar point that underreporting of data will lead to upward bias when using instrumental variables methods to correct for endogeneity and measurement errors.[19]

The inconsistency problem in PK's instrumental variable approach can, however, be solved by finding a measure of emigration that has classical instead of nonclassical measurement error.[20] In fact, the registered total outmigration, i.e., the sum of the true emigration, X_i^*, and the true internal migration, I_i^*, fulfills the classical assumption since internal migration is *excluded* from the explanatory variables in KP's population regression model. In other words, KP have (implicitly) assumed that their instrument Z_i is unrelated to internal migration I^* since it is subsumed in the population error term.[21] As a result, it is possible to replace X_i^* in equation (1) with total outmigration, i.e., $X_i^*+I_i^*$, and still obtain a consistent estimate of β since $Cov(I^*, Z)$ is assumed to be zero in KP's analysis.

I have collected data on total outmigration from the Swedish National Archives for the period 1860-1950 as part of my ERC-financed historical database project. With these data, it is possible to assess to what extent the results in KP are affected by the problem of underreporting of emigrants. Interestingly, the yearly reported municipal emigration used by KP only makes up, on average, 7.6% of the total outmigration (the median value is 2.4%) during the period 1867-1920, and the share is never larger than 20% for any individual year. Thus, this value must be considered a low share given the very large Swedish emigration during this period since it has been estimated that at least 1.5 million people emigrated, out of an average population of only 4.8 million, during the period 1860-1920. Thus, this finding further underscores the problem of underreporting of the Swedish emigration in KP's data.

Turning to the result of the solution of the underreporting problem, Table 1 presents the results from two first-stage regressions. For comparison, Column 1 shows the first-stage

example, KP's instrument could be measured with an error that is correlated with *e,* which then must be taken into consideration.

[19] See also Bound et al. (2001) for a general treatment of measurement errors.

[20] Bound et al (2001, p. 3729) write that "strategies for obtaining consistent estimates of the parameters of interest work if the measurement error is classical, but do not, in general do so otherwise."

[21] Indeed, KP treat internal migration I^* as an additional outcome variable Y in their instrumental variable approach in Column 1 in Table 8. Thus, KP have assumed the following causal chain: $Z\rightarrow X^*\rightarrow Y$, i.e., the instrument Z only has an indirect effect, which only goes through X^*, on the outcome Y, i.e., $Cov\ (I^*, Z)= Cov\ (u, Z)= 0$. If this exclusion restriction is wrong, i.e., that Z has a *direct* effect on both X^* and I^*, then two valid instruments are required for identification, i.e., one for X^* and another for I^*. This may be another reason why KP's empirical analysis is flawed.

estimate from KP's analysis, where they have used the underreported measure of emigration, while Column 2 displays the results when total outmigration is used instead. Table 1 reveals that the first-stage estimate with total outmigration is 0.010, while KP's estimate is 0.062. Thus, KP's estimate is severely biased upward with a factor of more than 6. Moreover, the effect in Column (2) is also a rather precisely estimated zero since it can rule out a first-stage effect larger than 0.026. Consequently, there is no first-stage relationship in KP's analysis when correcting for the problem of underreporting.

Table 1. First-stage estimates

	KP's migration variable (1)	Total outmigration (2)
KP's instrument	0.062*** (0.015)	0.010 (0.008)

Note: Coefficients significantly different from zero are denoted by the following system: *10 percent, **5 percent, and ***1 percent.

2. Inadequate controls for the confounding effect of weather shocks

In this section, I will discuss the problem of insufficient controls for the confounding effects of the weather shocks in KP's analysis.

KP writes "An important feature of our identification strategy is that we control for the direct effects of frost shocks and port proximity in (4). This is beneficial because studies that use weather shocks as instruments are typically marred by the problem that weather may simultaneously affect many variables (Giuliano and Spilimbergo 2014; Sarsons 2015). In our setting, locations that experience more severe frost shocks may obtain weaker government finances, worse public health, or other features that can affect our outcomes without going through emigration."

Thus, the central idea of KP's identification approach is that they can control for the weather shock, *Shocks*, and port proximity, *Port*, in equation (4), while the interaction between these two variables, $Shocks \times Port$, is assumed to be a valid instrumental variable. Thus, KP assume that their instrument is exogenous *conditional* upon the control variables *Shocks* and *Port*. However, there are two problems with this approach of controlling for the confounding effects of the weather shocks using a single control variable, *Shocks*.

The first problem is that KP impose that control variable *Shocks* only has a *linear* effect on the outcome of interest. It is noteworthy that KP index the parameter in front of the variable $Shocks_{mc}$ with s, i.e., weather stations, in equation (4), i.e., $\beta_s Shocks_{mc}$, even though they impose linearity, $\beta_s=\beta$, in their empirical analysis. In addition, the variable *Shocks* is also wrongly indexed since it is measured at the weather station level *s* and not at the county level *c* or municipality level *m*. Thus, *Shocks* should have been indexed *only* by *s* and not *mc*. Thus, these issues with the indexations make it difficult to evaluate KP's empirical approach without analyzing their data and code.

The second problem is that KP do not control for weather station fixed effects. As a result, their instrument may be correlated with unobserved characteristics across the spatial areas where weather shocks occur, i.e., at the weather station level *s*, even after conditioning on the control variable *Shocks*.

Starting with the problem of imposing linearity on the variable *Shocks*, it is possible to completely relax this assumption by estimating a model with a full set of indicators for each level of the weather shock.[22] Since the control variable *Shocks* only takes 12 distinct values, it is sufficient to include 11 dummy variables to completely saturate the model. An *F*-test strongly rejects the assumption of linearity (i.e., $F(11, 31) = 7.1$ and $Prob > F = 0.0000$) in the first-stage relationship. Moreover, the estimated first-stage effect is 0.023 with an s.e. of 0.021 when relaxing the linearity assumption. Thus, this estimate is much smaller than KP's reported first-stage estimate of 0.062 and is not statistically significantly different from zero. Thus, allowing that the effect of the weather shock on the outcome of interest may be non-linear shows that there is no first-stage effect. Moreover, the estimate of the first-stage effect when using total outmigration, when allowing for non-linearities in the control variable *Shocks*, is even smaller, i.e., 0.008 with an s.e. of 0.011. Table 2 displays these results.

Table 2. First-stage estimates when relaxing the linearity assumption in the variable *Shocks*

	KP's migration variable (1)	Total outmigration (2)
KP's instrument	0.023 (0.021)	0.008 (0.011)

Note: Coefficients significantly different from zero are denoted by the following system: *10 percent, **5 percent, and ***1 percent.

Turning to the problem that KP's instrument may be correlated with unobserved heterogeneity at the weather station level, it is possible to deal with this issue by including a full set of weather station fixed effects since KP's instrument is an interaction between weather

[22] In a Web Appendix (Table B11), KP estimate models with different polynomials of *Shocks* and *Port*. However, these polynomial regressions still impose strong functional form assumptions. More important, the variable *Shocks* cannot be accurately approximated by a *continuous* function, such as polynomials or splines, since it is a *discrete* variable, measuring the number of frost shocks, that only takes 12 values.

shocks and port proximity.[23] In other words, even when spatial fixed effects at the weather station level are controlled for, there will still be variation in KP's instrument within weather stations. Nonetheless, KP omit fixed effects at the weather station level in their analysis. Instead, they include county-specific effects. However, these geographical fixed effects cannot control for confounders at the level of the weather shocks, since counties comprise an administrative level of the central government consisting of 24 geographical areas, and each individual county fixed effect in KP's analysis will map into municipal data from approximately four weather stations, on average.[24] As a result, county fixed effects are therefore not adequate controls for unobserved heterogeneity at the weather station level. Moreover, the choice of controlling for fixed effects at the county level seems more or less arbitrary since these effects are only partly related to the other key control variable *Port*, which measures the geographical distance to ports. As a result, county fixed effects are not likely to be sufficient controls for the effect of distance to ports not captured by the single control variable *Port*, such as non-linear effects and time-invariant characteristics correlated with *Port*.[25]

Table 3 presents the first-stage results when weather station fixed effects are included instead of the county fixed effects used by KP. Column (1) shows the results for the emigration variable used by KP, while Column (2) displays the results from total outmigration. Again, the first-stage estimates in both columns are small and very different from KP's reported first-stage estimate of 0.062. Thus, controlling for weather station fixed effects shows that there is no first-stage relationship when unobserved heterogeneity at the weather shock level is considered.[26]

---

[23] Dell et al. (2014) discuss the crucial importance of controlling for spatial (area) fixed effects when using a "weather-shock" approach in a panel data setting, i.e., using an exogenous source of variation in climatic variables over time within a given spatial unit. Indeed, they write that "the weather-shock approach has strong identification properties. The fixed effects for the spatial areas, μ_i, absorb fixed spatial characteristics, whether observed or unobserved, disentangling the shock from many possible sources of omitted variable bias." Although Dell et al. (2014) discuss panel data applications, it is also possible to include fixed effects for the spatial areas in which the weather shocks occur even in a cross-sectional approach, as in KP's analysis, as long as the weather shock is interacted with some other variable that varies within the spatial areas.

[24] There are a total of 32 weather stations in KP's data.

[25] KP write (p. 1885) that "By including county fixed effects and using proximity in logarithms (rather than levels), the identifying variation does not disproportionately rely on northern counties." However, a much better approach is to order all 2,359 municipalities based on the distance to ports and define geographical groups accordingly. In this way, it is possible to control much more convincingly for the factors related to distance to ports that seem to concern KP.

[26] The estimated clustered standard errors in Table 3 also become significantly larger than those reported by KP (e.g., 0.046 vs. 0.015), suggesting that KP's clustered standard errors are likely to be biased downward because there is a correlation of the errors within weather stations that is not properly dealt with by only using clustered standard errors when there are only a limited number of clusters (32) and very unequal cluster sizes (i.e., the range is from 2 to 311).

Table 3. First-stage estimates when controlling for weather station fixed effects

	KP's emigration variable (1)	Total outmigration (2)
KP's instrument	0.019 (0.046)	-0.016 (0.012)

Note: Coefficients significantly different from zero are denoted by the following system: *10 percent, **5 percent, and ***1 percent.

3. An illustrative example

In this section, I present an example that clearly illustrates the problem discussed previously with KP's empirical design, i.e., that their IV estimate is likely to be biased due to nonclassical measurement error in their emigration variable, failure to account for non-linearities in the effect of weather shocks, and negligence to control for unobserved heterogeneity at the weather station level.

Table 4 presents the results from first-stage estimates (Panel A) and reduced-form effects on labor organizations (Panel B) using three different specifications with data *only* from KP.

Column 1 reports the results from a "bare-bones" specification that includes the key control variables used by KP (i.e., Shocks, Port, the initial population size in 1865) with the exception of county fixed effects. Column 1 shows that the first-stage effect is very large, i.e., 0.189, as compared to KP's reported first-stage of approximately 0.06 in their Table 3. However, the reduced form effect is nonetheless very small and negative, i.e., -0.0002, and thus very different from KP's results in Table 4, where they report a reduced form effect in the range 0.0014-0.0017.

Column 2 reports the results from the addition of county fixed effects to the bare-bones specification in Column 1. Now, the first-stage effect is reduced to 0.063, while the reduced form effect becomes positive and much larger, i.e., 0.0014, and statistically significant. Thus, it is noteworthy that a positive and statistically significant reduced form effect only appears when controlling for county fixed effects.

Column 3 shows the results when weather station fixed effects, instead of county fixed effects, are added to the bare-bones specifications in Column 1. Now, the first-stage effect decreases further, to 0.042, and is no longer statistically significant. However, the reduced form effect becomes negative, -0.0015, and is statistically significant. Thus, controlling for weather station fixed effect led to exactly the opposite reduced form result compared to the specification with county fixed effects in Column 2.

Taken together, these results in Table 4 strongly suggest that there does not exist a causal relationship between emigration and labor movements since there is no consistent relationship between the first-stage and reduced form results in KP's analysis.

Table 4. First-stage and reduced form results from three specifications

	(1)	(2)	(3)
Panel A. First-stage effect			
KP's instrument	0.189*** (0.041)	0.063*** (0.016)	0.042 (0.032)
Panel B. Reduced form effect			
KP's instrument	-0.0002 (0.0006)	0.0014*** (0.0004)	-0.0015** (0.0006)

Note: Coefficients significantly different from zero are denoted by the following system: *10 percent, **5 percent, and ***1 percent.

References

Ahlqvist, Göran (1976), Sydsvensk utvandring till Danmark, Fallstudie av en i kyrkoböckerna registrerad utvandring, [Emigration from South Sweden to Denmark, A Case Study Based on the Registered Emigration from the Church Books]. https://journals.lub.lu.se/scandia/article/view/1204/990

Eriksson, Ingrid (1969). Nordic Emigration: Research Conference in Uppsala Sept. 1969

John Bound, Charles Brown, Nancy Mathiowetz (2001). Chapter 59 - Measurement Error in Survey Data, Editor(s): James J. Heckman, Edward Leamer, Handbook of Econometrics, Elsevier,Volume 5, Pages 3705-3843.

Johansson Rolf (1976). Registrering av flyttare, en källkritisk granskning av kyrkoboksmaterial 1840-90, [Registration of Migrants, a Source Critical Evaluation of the Church Books 1840-90]. Scandia 1976, s. 167-192 https://journals.lub.lu.se/scandia/article/view/1203/989

Odén, Birgitta (1971). Ekonomiska emigrationsmodeller och historisk forskning. Ett diskussionsinlägg, [Emigration Models in Economics and Historical Research: A Discussion] https://journals.lub.lu.se/scandia/article/view/837/622

Odén, Birgitta (1964): "Den urbana emigrationen från Sverige 1840-1872, [The Urban Emigration from Sweden 1840-1872] Unpublished manuscript, Lund 1964.

Emigrationsutredningen (1909): Betänkande. Stockholm. Norstedt & Söner.

Stephens, M., and Takashi Unayama (2019). "Estimating the Impacts of Program Benefits: Using Instrumental Variables with Underreported and Imputed Data", *The Review of Economics and Statistics*, 101:3, 468-475

Vernersson Wiberg, A.-K. (2016). Migration och Identitet: En studie av utvandringen från Blekinge till Danmark och Tyskland 1860–1914. [Migration and Identity: A Study of Emigration from Blekinge to Denmark and Germany 1860-1914] (Licentiate dissertation). Uppsala: Department of Social and Economic Geography, Uppsala University.

APPENDIX (NOT FOR PUBLICATION)

In this appendix, I describe how my variable, the cumulative sum of total outmigration for the period 1867-1920, was merged to KP's data set that I downloaded from JPE's homepage.

I discovered that KP's data files do not include the names of the geographical areas (i.e., municipalities) but only a variable running from 1 to 2,359. Thus, I had to ask KP to send me this information. After some work, I was able to match 2,330 out of the 2,359 municipalities by using the code developed by the Swedish Archives (Riksarkivet).