Letter to the Editor


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While a recent paper by Cox in this journal uses as its motivating factor the benefits of quantitative risk assessment, its content is entirely devoted to critiquing Sneeringer’s article in the American Journal of Agricultural Economics. Cox’s two main critiques of Sneeringer are fundamentally flawed and misrepresent the original article. Cox posits that Sneeringer did A and B, and then argues why A and B are incorrect. However, Sneeringer in fact did C and D; thus critiques of A and B are not applicable to Sneeringer’s analysis.

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While a recent paper by Cox in this journal uses as its motivating factor the benefits of quantitative risk assessment (QRA), its content is entirely devoted to critiquing Sneeringer’s article in the American Journal of Agricultural Economics entitled “Does Animal Feeding Operation Pollution Hurt Public Health? A National Longitudinal Study of Health Externalities Identified by Geographic Shifts in Livestock Production.” Cox does not perform QRA in his article; instead, he presents two primary criticisms of Sneeringer’s paper: that Sneeringer misinterprets correlation as causation, and that Sneeringer’s methodology is subject to empirical issues. Cox bases his critique on an ostensible replication of Sneeringer’s results using a short time series of more aggregated data than those used in the original article as well as different empirical techniques, and finds that his data and his methods do not yield robust, consistent estimates.

Cox’s two main critiques of Sneeringer are fundamentally flawed. His first main critique is that Sneeringer has mistaken simple reduced form estimates for causal evidence. This ignores Sneeringer’s methodology and the bulk of her paper that addresses the question whether the correlation between changes in livestock presence and changes in infant mortality is indicative of causation. Cox’s second main critique is that reduced form analyses are temperamental with respect to choice of controls, time period of analysis, and model specification. These are valid worries in general, but Cox ignores the rigorous analyses performed by Sneeringer to address just these concerns. Cox attempts to demonstrate his second point by using less sophisticated empirical methodology and weaker data from the original article to show that the methods and data he uses are subject to a variety of problems. Because he hasn’t used the same methods and data as in the original work, any flaws he finds with this approach are logically not...
reflective of the original work. Cox apparently fails to grasp that problems with his empirical analysis do not mean that all empirical analyses are subject to the same problems. Better data and better empirical analyses can overcome many of the (well-known) problems that he raises; such better data and empirical analyses are used in Sneeringer’s article, and such problems are rigorously addressed.

Given the misreading and misrepresentation of Sneeringer, I offer a reiteration of the original article’s research design, empirical strategy, data, and robustness checks. In doing so, I point out the flaws with Cox’s arguments and analyses.

Sneeringer begins with the following motivation. Livestock production has been widely demonstrated to be associated with pollution. Exposure to the pollutants arising from livestock production has been demonstrated to have human health outcomes. Thus livestock operations will potentially cause pollution that hurts public health. Given concerns over pollution from livestock operations, these facilities face increasing environmental regulation that may raise production costs, hurt profits, reduce employment, and raise food prices. Policymakers weigh these “costs of regulation” against the “benefits” (in the form of reduced pollution and negative health outcomes). Thus federal policymakers would like estimates of the negative pollution/public health effects associated with the industry in order to set appropriate regulations. However, data on the pollutants of interest associated with livestock production have not been gathered on a wide scale over a period of time (a concern that has most recently been voiced by the U.S. General Accounting Office). Thus estimating the relationship between the “input” variable and the “output” variable at any one point in time. At any given point in time, the level of livestock production may be positively correlated with the level of infant health; however, as livestock production increases in that same “good health” location, health may decline. This would show a negative correlation between changes in livestock production and changes in infant health.

The use of natural experiments to estimate plausibly causal relationships is by now common in empirical microeconomics. A well-cited example by Chay and Greenstone uses the recession-induced changes in manufacturing to estimate effects of air pollution on infant health. Given that Sneeringer’s article appeared in an economics journal, it follows that it would ascribe to prevailing methods of the field. Lest Cox now conclude that all empirical microeconomics engaging in this type of research does not understand that correlations do not always reflect causation, he should note that there is also a rigorous literature assessing the problems with this methodology and the various empirical pitfalls associated with it. The literature makes clear that there are degrees of analytical rigor and recommends (often by example) a barrage of methods to assess such analysis.

The natural experiment strategy has been widely adopted in part because modeled predictions of what could happen may not accurately reflect what does happen. Humans are dynamic agents who may do things a modeler does not expect and does not model. Imperfect knowledge of environmental effects also suggests that modeled estimates may yield incorrect conclusions. Further, if parameter estimates used in models are poorly estimated, this may result in incorrect predictions.

With the research design in mind, the next step is accessing data on which to apply it. Because the research design relies on variation over time and between locations, it is necessary to find data with significant variation across these parameters. Using data with little variation can mean that estimated coefficients are temperamental with respect to inclusion or exclusion of controls and model specification. Thus, Sneeringer uses county-level data for the entire United States covering 20 years. The reproductive health outcome variables are constructed using changes in reproductive health outcomes amounts to a form of “natural experiment.” Notice that using the changes over time in the potentially causal variable (livestock) to estimate changes over time in the outcome of interest (health) is inherently different from estimating the relationship between the “input” variable and the “output” variable at any one point in time. At any given point in time, the level of livestock production may be positively correlated with the level of infant health; however, as livestock production increases in that same “good health” location, health may decline. This would show a negative correlation between changes in livestock production and changes in infant health.
restricted-access data from the National Center for Health Statistics, available to researchers through an application process. As Sneeringer notes, she uses this restricted-access data because public-use data (freely available via the Internet) are censored for individuals residing in less populated areas. As livestock production occurs in more rural areas, omitting these observations from less populated places may not provide accurate estimates.

Note the difference between the data Sneeringer uses and those used by Cox in his purported replication of Sneeringer’s work. Cox uses state-level measures for a one-to-two-year window of time. These data are arguably far more subject to the problems that Cox demonstrates than those used by Sneeringer. The fact that Cox has not attempted to obtain the restricted-use data (or even the public-use data of similar form) suggests that his intent was not to replicate Sneeringer’s analysis and that he fails to recognize the contribution made by using the more disaggregated data.

Sneeringer accompanies the “natural experiment” research design with an empirical strategy making use of what economists refer to as “fixed effects.” The county-level fixed effects included in Sneeringer’s analysis nonparametrically control for average levels over 20 years not only of the outcome variable (infant health) and the variable of interest (livestock presence), but also any potential control variable (infant health) and the variable of interest (livestock presence), but unrelated to each other. Figure 1 in Sneeringer’s paper highlights the within-state movement of livestock, but this feature is ignored by Cox.

Cox’s analysis using state-level data for a brief window of time will mask the within-state changes that are central to Sneeringer’s identification strategy. For example, if several small operations close while one large one opens within a state, this would appear in the state-level data as no change in aggregate livestock production. Similarly, it may appear as no effect on infant mortality at the state level, and thus the two variables appear unrelated. Figure 1 in Sneeringer’s paper highlights the within-state movement of livestock, but this feature is ignored by Cox.

With a “natural experiment” research design, the fixed effects empirical strategy, and more precise data, Sneeringer next considers situations where this scenario may fail to yield causal interpretations. Of primary concern for the topic of interest is that livestock operations may move to or away from places that have some characteristics that correlated with changes in reproductive outcomes. For example, livestock operations may move to or away from places with declining per capita income, which may be correlated with declining reproductive health. Such omitted variable bias would be a major concern with Sneeringer’s work were it not for the fact that the bulk of her paper is devoted to examining whether a plausible case can be made for the existence of such a variable.

Ignoring this large section of the original work and misrepresenting the empirical design, Cox argues that Sneeringer has plausibly ignored a fundamental confounding variable that could be correlated both with animal units and infant mortality. He suggests that changes in animal units might be correlated with demographic characteristics of a less healthy population, and that this is the real event that is driving Sneeringer’s results. This criticism, however, indicates first that Cox completely failed to understand how the county-level fixed effects function. Any characteristic of a county, observable or unobservable, that is fixed over time will not affect changes in infant mortality. For a confounding

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1Epidemiologists’ use of the term “fixed effects” is generally different from economists’ use of this term. Economists use the term “fixed effects model” to refer to a regression model including dummy variables for each variable being “fixed.” Thus, “county fixed effects” refer to regression models that include a vector of dummy variables, one for each county. Epidemiologists often use the term “fixed effects” to refer to nonrandom regression coefficients.
demographic variable to be driving Sneeringer’s results, as Cox wishes to argue, there would need to be a change over time in county-level demographic characteristics that drive infant mortality and are also correlated with the number of animal units. Cox eventually suggests two: the fraction of residents who are black and the fraction of households with incomes below the poverty line. If, in fact, Sneeringer had failed to account for changes in these two variables, that would indeed be a valid criticism of her work. However, these factors are already well controlled for in Sneeringer’s paper.

Sneeringer’s analysis controls explicitly for changes in the percentage of mothers in a county who are nonwhite and for changes in the log of per capita income in the county. Such measures are clearly very similar to the variables suggested by Cox. If Cox wants to argue that measures of the fraction of mothers who are nonwhite is, somehow, an inadequate way to capture the effect of race on infant mortality, he would need to make an argument that it is possible for the fraction of a county’s population that is black to have increased, but the fraction of mothers who are nonwhite to have remained the same. However, he makes no such argument. Indeed, the race of mothers (Sneeringer’s variable) is more closely linked to infant mortality than the race of the population as a whole (Cox’s variable), so it seems clear that Sneeringer’s measure is both highly correlated with the one used in the comment and perhaps also a better control variable than percent black.

A similar argument can be made with respect to the Cox’s suggestion that changes in the percentage of people below the poverty line is an omitted variable that could be leading to the strong, robust, and positive correlation between livestock production and infant mortality found by Sneeringer. Sneeringer includes the natural log of per capita income in her regressions when the infant mortality rate is the dependent variable. In this model, the log provides a nonlinear control of per capita income. If the comment author is concerned with nonlinearities in the effects of income, and particularly steeper effects in the lowest income strata, then this model will reasonably control for the issue. Further, Sneeringer shows other functional forms of the original regression (with the infant morality rate in logged form), which show the same results, and provide a check as to the functional form relationship; again, these show the same strong, positive association between livestock production and infant mortality. The log–log functional form can accommodate a much more flexible relationship; if the nonlinearities in income were yielding the effects of animal units, then at least one of these regressions should show no effect of animal units. However, none do. Cox’s argument would only be valid if there are such stark nonlinearities in the effect of changes in income that a measure based on the poverty rate contains fundamentally different information from a measure based on log per capita income. If that were the case, then Sneeringer’s results would not be so consistent across functional forms.

If findings are not the result of these two suggested variables, the comment author must propose another omitted but potentially pertinent confounder. Such a factor would need to be systematically positively correlated with trends in livestock presence nationally over a 20-year period, negatively correlated with infant health, and not well controlled for with the included covariates. As is evident from Sneeringer’s work, the coefficients of interest remain positive and statistically significant even after adding covariates for mothers’ average age and age squared, the percentage of mothers who are married, the percentage of mothers who are white, the percentage of mothers who are foreign born, the percentage of births that are male, the percentage of births occurring in each month, the average number of prenatal care visits, the percentage of births that occur in a hospital, the number of hospitals per capita, the number of physicians per capita, the natural log of per capita income, the percentage of the county that is of Spanish descent, the percentage of the county that has no high school degree, the percentage of the county that has a high school degree, the percentage of the county that has some college education, population density, the percentage of the county employed in farm labor, the percentage of houses with well water, the percentage of houses with septic tanks, the percentage of the county in cultivated cropland, the percentage of the county in forest, the percentage of the county in rural transportation land, the percentage of the county that is in other rural land, the level of employment in 15 other industries, mean temperature, number of inches of precipitation, the number of National Pollution Discharge Elimination System permits for livestock in the county, fixed characteristics of each county, fixed characteristics of each time period, and fixed characteristics of each state in each time period. This list seems quite comprehensive: if Cox has a plausible candidate for an omitted variable that is not, in fact, a tiny variation on an item in this list, he did not provide it.
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Even after accounting for all of these potential issues, Sneeringer considers other potential problems with her own research and attempts to assess them. As empirical outcomes may be the result of a number of choices, Sneeringer conducts myriad further analyses to assess whether the results are robust with respect to functional form, time period, and alternate control sets. These tests appear not only in the main body of the paper but also in an appendix. The results are robust with respect to this multitude of tests.

At this stage in the analysis, Sneeringer has found a statistically significant positive correlation between infant mortality and livestock presence. This correlation remains after controlling for many fixed and variable controls and a battery of robustness tests. Sneeringer has thus addressed most of the issues Cox raises with regard to potential statistical problems with this type of empirical analysis. The force and consistency of these results suggests that the correlation between the input and output variables may be causal. Sneeringer then considers alternative hypotheses for the existence of this strong and robust positive correlation.

A first reason proposed by Sneeringer is ecological bias, in which factors may have different effects at the group and individual levels. Notably, Cox’s more aggregate state-level data are more prone to ecological bias than those used by Sneeringer. One method in which ecological bias could explain Sneeringer’s results is if healthier individuals systematically move out of counties at the same time as increased livestock production, leaving less healthy individuals with greater prevalence of infant mortality. To be responsible for the results, this would have to occur systematically across the country for 20 years. To the extent that women do not generally relocate outside of the county while pregnant, this story is less tenable.

A second pattern that could explain results is the case where individuals in a county with increased livestock presence but unexposed to the industry’s effects are more likely to experience infant mortality. One might perceive of an individual case in which livestock industry presence increases in one section of a county while infant mortality increases in a different (unexposed) portion of the county. It is less plausible to perceive of this scenario systematically occurring nationally for 20 years, particularly when contemplating changes in infant mortality correlated with changes in livestock presence. Notably, this type of bias may also work in the opposite direction, biasing results downward such that actual effects are larger than those estimated. This would be the case if the unaffected individuals have no effects but are averaged with the affected individuals.

Sneeringer’s causal argument and the hypothesized pollution mechanism are bolstered by further analyses. First, she shows negative effects on other infant health outcomes besides mortality, which are expected if the relationship between livestock production and infant mortality is causal. Second, she performs two falsification tests examining the relationship between livestock production and two causes of death not expected to be caused by livestock production (accidents and homicides, chromosomal anomalies). As expected, neither of these is associated with livestock production. Finally, she examines livestock production’s association with causes of death related to pollution; the findings show positive correlations with these outcomes. All of these analyses go to building a causal argument, and are the reason that one is made. Cox ignores these aspects of the original work.

In sum, Cox’s article fails to address Sneeringer’s work. He posits that Sneeringer did A and B, and then argues why A and B are incorrect. However, as described above, Sneeringer in fact did C and D; thus critiques of A and B are not applicable to Sneeringer’s analysis. It is certainly worthwhile to engage in academic debate on different methodological approaches, but Cox’s “straw man” method of argument does nothing to further the scholarly dialogue on this topic.

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REFERENCES