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Newsletter

About That Urban Institute Voucher Study: Q&A With Josh Cowen

The Urban Institute released a report on April 22nd that has been gaining excessive attention among advocates of private-school vouchers. In the Q&A below, National Education Policy Center director Kevin Welner asks Josh Cowen of Michigan State University to help us understand that study and why there has been so much interest among folks who have otherwise been shielding their eyes from recent voucher research. Prof. Cowen was lead author of a similar study published in 2013, so we thought he would be the ideal person to comment on this new one. Cowen is the author of the 2024 book, *The Privateers: How Billionaires Created a Culture War and Sold School Vouchers* (Harvard Education Press).

1. **The Urban Institute recently published a study of Ohio's voucher plan that is being trumpeted by voucher advocates, including the *Wall Street Journal*'s editorial board. So I want to ask you to briefly describe the Ohio voucher policy, the new study, why advocates are so excited, and whether they have reason to be? Let's start with that first question: Please give us a brief description of the Ohio voucher policy.**

   The Ohio voucher policy technically has several voucher programs. But what we're talking about here is the suite of "EdChoice" vouchers (not to be confused with the voucher advocacy group founded by Milton Friedman that is also called EdChoice). When this voucher program began, it provided private school funds for low-income

students and those in public schools scoring low on the state's accountability index. But the program recently expanded to near-universal programs for which 96% of students are eligible in some form. The Urban Institute report focuses on the early days of this EdChoice targeted voucher program (2008-2014), not today's version.

2. **Now please tell us about the new study. What did the researchers set out to learn, how did they go about doing that, and what did they find?**

   They basically took data from a study conducted nine years ago, by two of the current study authors, that had focused on student achievement and on voucher impacts on kids who stay in public schools (so-called competitive effects). And they updated the outcomes to see whether voucher users from that study were more likely to attend and complete postsecondary education. They used two statistical techniques—propensity score matching and regression discontinuity—both of which rely heavily on the test scores that a voucher student obtained before receiving the voucher. That score is used in the statistical models to simulate a condition in which students were randomly assigned to use a voucher or not. The first technique, matching, is used by the researchers to attempt to determine whether voucher users had higher educational attainment levels. The second considered whether students who stayed in public school were more likely to graduate when those schools experienced "competitive" pressure from voucher students. As a general matter, from the standpoint of causal inference—whether their techniques appropriately simulate what would have happened had vouchers not existed—the second approach is more convincing than the first. That's because its regression discontinuity design in principle could simulate random assignment for a small portion of students at or near a cut-point (albeit an all-but-arbitrary one) for voucher exposure based on the state's assignment criteria. The study finds very large impacts of voucher use on college attainment for voucher users (between 9 and 15 percentage points), and it finds some evidence for competitive pressures on those near-the-cut-point public school students, who seemed to be between 3 and 6 percentage points to more likely to attain college access if exposed to voucher competition.

3. **Let's combine the next two questions. What are advocates excited about with regard to this new study, and is the excitement merited? What are the strengths of the study, and what are its weaknesses?**

   Well for one thing I'll just note that the study disclaimer says it was funded by the Oberndorf Foundation. That foundation's namesake, Bill Oberndorf, is the chairman of the board for the American Federation for Children (AFC), which is Betsy DeVos's 501(c)(4) voucher advocacy organization. The authors say that didn't influence the results, and I believe them. But it's why AFC is running around on Fox News right now bragging about the study. Now that federal education research spending has been gutted, we're likely going to see a lot more of this sort of thing: Advocacy organizations are going to step into the breach with their own plans and priorities—often using old or previously collected data, as in this report. Those funders can afford to hit and miss on what such reports find. And they don't have to influence the results to influence how the results are distributed later when the data go their way. I assume if Urban had found negative voucher results, you wouldn't have seen AFC on out there on Fox News second-guessing

themselves.

Substantively, it's also easy to see why voucher advocates love this study. It's been years since a study on voucher participant effects has found good news—whether the outcomes are student achievement or attainment (as in this paper). Those advocates seem indifferent to the fact that the data in this study are now more than a decade old and bear no reflection on today's broader program—I myself have acknowledged that earlier voucher studies on smaller programs were more likely to find positive results. But I guess when you've been batting 0 for 100 for a few years, getting on base once more is exciting.

From the standpoint of study's strengths, I'll say this: The authors did what they could. They had old data that didn't lend themselves to credible causal design—at least for the voucher-user part of the study—and they did all the modeling and analysis that would be expected for such designs, including sensitivity results to consider how important unobservable factors left uncontrolled-for in their analysis might be. It's notable that for college graduation results, the authors find unobservable factors driving both voucher usage and college success are indeed present.

On study weaknesses, I'll explain more below, when I talk about weaknesses in a similar paper that I myself led way back in 2013. But suffice to say, the Urban Institute paper's statistical approaches really are only appropriate when a pre-treatment measure of the dependent (attainment) variable is available. The authors don't have that. They use pre-voucher test scores to proxy for student attainment. This doesn't make a lot of sense in a paper that also argues (as many voucher advocates now do, in the wake of so many studies showing that voucher use harms test scores) that attainment is a more important indicator of success for voucher programs than those very same test scores! Either test scores capture something useful or they don't.

4. What does this research add to the overall body of research about voucher outcomes?

Not much. Even the authors readily acknowledge at the end of the report that "the significant differences between the targeted programs that have produced this encouraging evidence and the universal programs currently being expanded across the country mean that more evidence is needed…" (p. 17). There's a misconception concerning the "achievement vs. attainment" results from voucher studies, with the idea being that even if voucher impacts on test scores are negative, somehow voucher impacts on high school or college enrollment are positive. As I explained in a Brookings Institute primer a couple years ago, those attainment impacts are unquestionably overstated. There was a study from Washington D.C. in 2013 that found big positive voucher impacts on high school graduation. And one from Milwaukee the same year (on which I was lead author) that found small positive voucher impacts on that same measure. Thereafter, we saw a bunch of studies showing no voucher impacts on attainment. This new Urban Institute report sort of adds a positive result to that mix. But it does so in a way that even the authors acknowledge was based on old data from a targeted program that

no longer reflects how vouchers operate today. At best it's an interesting artifact of old public policy—but it also fits well within my broader critique of voucher systems: Older data did from time to time show something favorable; newer data haven't. And we should trust the newer data more because they've been gathered on voucher systems more similar to today's programs. Some of these concerns, as well as others, are also raised in a piece about the new Urban Institute study, written by Stephen Dyer, a former chair of Education Committee of the Ohio House.

5. **You were lead author on a study with a similar statistical strategy and similar findings in 2013. Given that history and background, do you see these attainment studies as valuable contributions, even given the clear methodological and data limitations? Are there ways that future researchers might approach these questions to better address limitations?**

   Right, so at least the Urban Institute authors had a pre-voucher measure of student achievement. We didn't even have a pre-treatment test score and had to use ninth grade test scores (post-voucher) as a control for later graduation and college enrollment. Now, we did have some things the Urban authors didn't—in particular, a measure of parent education and religiosity. Frankly, our approach was considered acceptable 12 years ago but probably wouldn't have been accepted for peer-reviewed publication today. That's okay. That's how the field works: Papers get published, methods get improved. The field moves on.

   By today's standards, the Urban study now and the old 2013 study that I led have similar weaknesses. Namely, propensity score matching designs (and even regression discontinuity approaches like the authors use for their competitive-effects portion of the study) really need to have a pre-treatment measure of the outcome itself. There is really no way to get a true pre-voucher measure of high school graduation or college enrollment. In our 2013 study, we made the argument that our "parent level of education" control variable was as good a measure of a child's prior attainment level as researchers could get. Whether you find that convincing or not is another story, but that's what we did.

   Think of the problem by first asking this question: Whether a voucher scheme is universal (funds available to all students) as Ohio almost is now, or targeted by income as it was during the Urban authors' data collection period, what do students have to do to actually use that voucher? They have to find a school. They have to apply to a school. They have to gain admission to that school. They have to pay any additional tuition costs beyond the voucher scholarship. They have to remain in school. Does that process sound familiar? It should: It's the same set of skills a student must have to go to college and persist toward a college degree. And a strong case can be made that those are very different skills than performing on a standardized test. So controlling for a pre-voucher standardized test score doesn't do much to account for those skill sets that might determine whether a student is a voucher user and whether they go to college later.

   Researchers working with the voucher lobby discard any voucher results on standard-

ized tests that don't come from an experimental study where voucher use is randomly assigned. That conveniently removes from consideration the 2016 achievement study of voucher Ohio vouchers conducted by two of the Urban authors, and especially the Indiana voucher results—all showing awful voucher effects on math achievement. If they were being consistent, they would also throw out most of their studies on non-test score outcomes—including this new one from the Urban Institute. In fact, this attainment study has the core weakness that those achievement studies avoided, since those studies at least had the true pre-voucher measure of the outcome (the state standardized exam) in place. So really, the argument that "we should only focus on experimental design results" in voucher research is far more compelling for student attainment studies like this one than it is for those using test scores as outcomes. The only time you really don't need a pre-treatment measure of the outcome—at least in theory—is when you have a randomized design. By that standard it remains true that only one study based on randomized design has ever shown positive voucher attainment impacts. And it's now more than 12 years old (focusing on the then-tiny DC program).

6. **Over the last several years, through your new book and other efforts, you have been a national leader in speaking with legislators, journalists, and others about the national push to expand vouchers. Do you think this new study fits within that push, and if so, how?**

   The horrific test score impacts for vouchers in the first few years of Indiana, Louisiana, Ohio, and Washington, D.C. vouchers from 2015-2020 should have been enough to end vouchers as an education reform strategy. It's rare to see a reform actually hurt student learning, as we've seen with vouchers. The research field had plenty of evidence to recommend that vouchers scale back or die as a reform strategy—researchers make policy recommendations all the time based on far more ambiguous data—but many folks didn't in this case for one reason or another.

   Instead, the goalposts kept shifting. And I would argue that's because with vouchers, they aren't an economics debate. They're not even an education debate. At least not anymore. Vouchers are a political issue. For the voucher lobby, it's not about the data. They basically say, "ignore the test scores, since we should still have vouchers for other reasons." I firmly disagree, but that's a political position, and they're entitled to it.

   But you know what? That works both ways. People other than voucher advocates should also be looking beyond the data. For instance, we know—and my book covers this at length—vouchers in practice mean that the private schools do the real choosing. Not parents. Today's voucher systems allow private schools to turn away students with disabilities. Or kids with single moms. Or kids from LGBTQ families. Maybe a kid becomes pregnant and is asked to leave her private high school. The whole theory of action behind vouchers is built on private schools (and private religious schools, in particular) being allowed to do that. So the student selection issues in the Urban Institute report (as well as my 2013 study) aren't just econometric problems. At some point we have to ask ourselves what we think is right and what we think is wrong.

Let's wrap up with a thought experiment to get at what I mean here. Whether you're in business or policymaking, one question decision-makers ask is about benefits versus costs. Sometimes the costs are financial tradeoffs. Sometimes they're political. Sometimes they're moral. I've already tried to explain in this interview that I think there are limitations with the Urban Institute report. But let's pretend it was a gold standard study. Let's even say for the sake of argument that the negative voucher impacts on student achievement—the largest we've ever seen on any question in the history of education research—didn't happen. Let's say some positive gains had been made over the last decade instead.

I'd still ask: Is it worth it? How big would achievement or attainment results have to be to justify taxpayer investments in private schools that can choose to exclude kids with disabilities, kids from non-traditional family structures, or anyone else those schools decide aren't a good fit? If there's an answer to that question, it's going to have to be a heck of a lot more convincing than what's presented in the Urban Institute report.

NEPC Resources on Vouchers

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