A Brief Assessment of

Joseph Altonji, et alia, "Employment Effects of Unemployment Insurance Generosity During the Pandemic," July 14, 2020.

John F. Early

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(https://tobin.yale.edu/sites/default/files/files/C-19%20Articles/CARES-UI_identification_vF(1).pdf)

By: John F. Early, Vital Few, LLC

With such a short time frame and so little appropriate data, it is tough to create good measures from scratch, so this team of ten made a college try to evaluate the $600 added to all weekly unemployment insurance (UI) checks by the CARES act. My brief review of the work by this team of 10 is similarly constrained by time and resources, but the results of their research are not compelling, and I wouldn't add $200 to the standard UI benefit based on these results, much less continue the excessive $600.

Specification Problem

The first problem is with the specification of the independent variable being tested – namely an idiosyncratic replacement ratio that measures the so-called "generosity" of the CARES UI benefit as the ratio of the benefit an individual receives under CARES with the benefit they would have otherwise received. But both the ethical and economic questions are about the relationship of the CARES benefit to the earnings from work, not the relationship between the two levels of benefits.

The dependent variable that the authors seek to explain is the level of employment. As a first-order approximation, that would not be bad, but the real variable we want to understand is the rate at which laid-off workers receiving the CARES bonus subsidy do not return to work when their employer is ready to employ them. Total employment may be higher or lower for any number of other reasons, but we need to know the effect of an additional $600 subsidy on the decision to return to work.

The independent variable is "the replacement rate ratio" and is defined by the authors (with some simplification) as follows:

$$repl_{CARES} = UI_{CARES}/w$$
$$= (UI_{base} + 600)/w$$
$$= repl_{base} + 600/w$$

$$\text{replacement rate ratio} = r = repl_{CARES}/repl_{base} = UI_{CARES}/\ UI_{base} = 1 + 600/UI_{base}$$

w = weekly wage

Note that while the UI benefit is a function of the reported wage, that function varies substantially from one state to another.

They then calculate from their database the change in employment over different time periods for different classes of r (≤2.5, 2.5-3.0, 3.0-3.5, 3.5-4.0, 4.0-5.0, and >5.0) and compare them all

to the ≤2.5 class. They find that after the implementation of the CARES premium pay, there was no differences among the classes of r in employment change.

This is the wrong test. Suppose two workers each had a base UI benefit of 400 per week, then their UI_{CARES} benefit would be $1,000 and their replacement rate ratio would be 2.5, the point at which they are defined as a base case to which others are compared to see if there is an effect. (1 + 600/400 = 2.5). If one of those workers had a pre-layoff wage of $800, then going back to work would lose him money ($800 < $1,000). But if the other one had a pre-layoff wage of $1,200, then he would be incentivized to return to work. Yet the authors place both of these cases in the same class for comparison when they have exactly opposite incentives. This misspecification makes the results totally meaningless on any basis.

Ethically, it is reprehensible for government to enable people who are not working to be paid more than people who continue to work, no matter what the reason. Economically, the incentive to return to work is not the ratio of the CARES UI you are getting to the UI you would otherwise have gotten, it is the ratio of the CARES UI to what you would get by going back to work. (There is another more esoteric specification problem here in that it may not be the ratio of that higher UI value, but actually the dollar difference that drives labor supply.)

The illogic of this formulation is demonstrated in the circular reduction of the mathematics. The authors define "reference" workers as those for whom the replacement ratio (as they define it) is less than 2.5. These reference workers are assumed to have no effect from their small ratio, and the behavior of all other ratios are measured in reference to these. This works out to being anybody for whom their pre-CARES benefit would have been less than $400. There is no direct relation to the employment earnings they would be getting by going back to work, as shown above.

Paper Claims

The paper makes a rather modest claim that they could not find any effect: "This note provides preliminary evidence that expansions in UI replacement rates did not increase layoffs at the outset of the pandemic or discourage workers from returning to their jobs over time. We note that our results do not necessarily imply that such responses do not exist – rather, they suggest that expanding UI generosity has not depressed employment in the aggregate."

These claims are made on the basis of only six weeks of data following passage of the CARES Act, so while they might show no increase in the proclivity of employers to lay people off (which is not the primary complaint about the subsidy), the time frame is entirely to early to expect much of an effect on employees being reluctant to return to work while they are getting a substantial subsidy to stay home.

What is particularly puzzling about the result is that in the weeks before the passage of CARES, employment fell more for workers who would later have the highest replacement ratio. After this fall, there was little change. The authors go to great lengths to point out plausibly that because of the speed, churning, and confusion around the passage of the act, it is highly unlikely that firms would have based their hiring and layoff decisions on speculative provisions of the act. Yet here we have the workers who

later received the highest subsidies being laid off in the weeks prior to the act. The authors offer no reason why that should be so.

This pre-passage decline, was, of course, driven by the government shut-down of businesses, but why would the business employing workers who would later get bigger subsidies lay off the most people? One possible factor is that the circular definition of replacement ratio may also inadvertently be segmenting the workforce by the financial strength of their employers. More generally, it means that the model gives economically nonsensical results, which means that the model is likely mis-specified.

The Data – Here's a Tip

The study uses a rather limited private data source that captures mostly small businesses that use a particular type of software for their time keeping. The businesses are not only mostly small, they are heavily skewed toward eating and drinking and other hospitality establishments. The authors are very straightforward about the structure and limitations of the data set and offer plausible arguments and controls to account for some of its limitations. But they entirely miss what may be the most telling flaw. These workers depend heavily on tips. UI benefits are calculated on both base salaries and tips reported to the state and taxed for UI coverage. But small firms in these industries are notorious for systematically understating their tips significantly, thus avoiding taxes for both employer and employee. The analysis is biased when it compares UI benefits against reported earnings rather than earnings with tips, which could be twice or more times as high.

Otherwise, there are more prosaic limitations of the data that make the results unreliable, even if the model were not mis-specified. The very nature of the low-skill, high-turnover industries prevalent in this data set, means that they will, in general, be less likely to be recalled to work than in other more stable industries.

More mechanically, because the authors need historical data to compute UI benefit, they exclude people with less than one year of experience in the firm. That means the sample is biased toward long-term employees who are more likely to be recalled than the newer hires and also more likely to return if asked owing to experience and loyalty.

They wisely try to validate their time-keeping data source with the CPS/ACES data, but while the results do not contradict the first set of findings, they don't support it either. They need to really dig into the CPS more. If I had time, I'd do that.

Final Conclusion

The authors correctly note: "We emphasize that our results do not speak to the disemployment effects of UI generosity during more normal times, which is the subject of a vast literature (Schmieder and von Wachter (2016))." Indeed, they understate the case. The literature unambiguously shows that bigger benefits and benefits paid over longer periods of time introduce significant disincentives to work. In fact, there is a great live example of Denmark that over a number of years substantially reduced the length of time for which it would pay UI benefits. They discovered three relatively equally sized groups of people: one would scramble to find a job as quickly as possible. Another would delay seriously looking until just

before the benefits ran out and then miraculously find a job. The remainder were spread out between the other two. The behavior of the delayers was always the same, no matter how long the benefit period was.

“Higher government subsidies are a disincentive to work” is the law of gravity in labor economics. If someone wants to posit a black hole where that doesn’t apply, then we need far more compelling evidence than provided here. Even in their own words, “These results provide suggestive evidence that, in the aggregate, the expansion in UI benefit generosity did not disincentivize work at the outset, and that high replacement rates did not differentially deter workers from returning to work.” When displacing Newton, one needs an Einstein backed by direct evidence, not a mere “suggestion.”