The Quarterly Journal of Austrian Economics
VOL. 15 | Nº. 1 | 143–147
SPRING 2012

A Critique of MacKenzie, Not an Endorsement of Hoover: Reply to Vedder and Gallaway

Daniel Kuehn

KEYWORDS: business cycle, wage cyclicality, Great Depression
JEL CLASSIFICATION: N12, E32, J30

Vedder and Gallaway's (2011) rejoinder to my comment on MacKenzie (2010) seems to fundamentally misunderstand both my comment's argument and the contribution of Rose (2010). They interpreted my comment as an attempt at proving MacKenzie (2010) "wrong," when I said nothing of the sort. It was simply an application of my view that Austrian scholarship on the Depression ought to maintain its unique perspective without trying to work in a vacuum. MacKenzie's (2010) view of Hoover may or may not be right (I suggest at several points in my comment that we lack critical information for determining whether his view is right), but his *argument for that view* has several blind spots that could not stand uncontested.

Daniel Kuehn (dan.p.kuehn@gmail.com) is a doctoral student in the economics department at American University.

Why is this reply to Vedder and Gallaway (2011) necessary? Aside from setting the record straight on what I claim and what Rose (2010) claims, it is also critical to promote the idea that economists who affiliate with the Austrian school be expected to engage relevant mainstream research. The Austrian school is gaining increasing prominence in the discipline, and if this trend is to continue (something I sincerely hope for) there is no excuse for MacKenzie (2010) to present an article on wage cyclicality that so conspicuously neglects three decades of research on the subject.

The first point to dispatch is the claim that my comment suggests that "MacKenzie (2010) is wrong about Hoover's effectiveness in pushing a high wage policy that caused high unemployment" (Vedder and Gallaway 2011). This is simply not true. I find the idea that labor market policies enacted by Hoover and Roosevelt made the Depression worse quite plausible on their face. I generally have an affinity for Keynesian economics, so a story of rigid wages is one that I find reasonable, if not always the primary factor in explaining business cycles. What I state repeatedly in my comment is *not* that MacKenzie (2010) is wrong, but that he furnishes a weak defense of his own claim. Readers can easily verify that that has been my position, contrary to the claims of Vedder and Gallaway.

Vedder and Gallaway (2011) also admonish me for not citing Murray Rothbard or a number of additional Depression-era economists who support MacKenzie's (2010) argument. This is perhaps the oddest criticism in the entire rejoinder. Critical comments on published research are under no obligation to expand upon and support the original research article.[1] Even given the specificity of my concerns with MacKenzie's research, my comment still takes up a dozen pages. There is no reason to expect me to donate a portion of my own word-count to the cause of buttressing MacKenzie's (2010) argument with an extended literature review. If Vedder and Gallaway believe MacKenzie's argument was inadequately defended with citations of the

[1] I am quite surprised Vedder and Gallaway (2011) even raise this challenge. In their own critical reply to Barnett and Block (2006) in the pages of this journal (Gallaway and Vedder, 2006), they make no effort to buttress Barnett and Block's argument by citing additional sources. It is puzzling, then, that they expect such due diligence from me.

Depression-era literature, this seems like an issue they should take up with MacKenzie himself.

Vedder and Gallaway's attempted refutation of my citation of the wage cyclicality literature actually repeats several of the caveats that I made sure to raise in my comment. For example, most of this research is restricted to the post-war period by data constraints. It is not clear to me why this is posed as a counter-argument, since I actually highlighted precisely this point. My argument is simply that MacKenzie cannot write an article on wage cyclicality and neglect the enormous literature on wage cyclicality that raises concerns about his interpretation of the data. Obviously there are data limitations associated with the Depression. But this is something that MacKenzie (2010) should have brought to the attention of his readers. One cannot simply fail to mention an entire research literature.

Vedder and Gallaway (2011) provide an unusual argument against the application of the wage cyclicality literature to MacKenzie's article. They point out that unions were more prominent in the mid-to-late twentieth century, and therefore the labor market of the 1960s, 70s, and 80s is not comparable to that of the 1930s. One would think, though, that wage rigidity would be more of a problem during periods characterized by powerful unions. If Vedder and Gallaway (2011) insist on exploring how unionization may influence these findings, surely we would expect to find less flexible wages during periods with strong unions than during periods with weaker unions (such as the early Depression years). This would bolster my argument that MacKenzie (2010) needs to at least try to grapple with the wage cyclicality literature and the aggregation bias in his own data.

A more nuanced point is that Vedder and Gallaway (2011) appear to misunderstand the claims that the wage cyclicality literature has converged on. Compositional change is not simply a matter of "last hired, first fired." Firms maintain workers in which they have made large human capital investments and who have specialized skills. These need not be recently hired workers. Indeed, I cite Martins, Solon, and Thomas (2010) precisely because they find that real wages are pro-cyclical even among newly hired workers. Whole occupations that are lower-wage can be decimated by labor saving technology in a process of creative destruction that is salutary in the long-term but introduces bias in aggregated wage statistics.

The literature on wage cyclicality is massive, and while Vedder and Gallaway have been vigorous in their past resistance to its findings (see Vedder and Gallaway, 1998), I encourage readers of this journal to explore it for themselves. The reference list in my comment on MacKenzie (Kuehn, 2011) is a good place to start.

Finally, my critics here seem to have misread Rose's (2010) empirical work on the Hoover conferences. They suggest that Rose (2010) was "looking at the wage behavior of those firms whose president attended the employment conference of, say, November 21, 1929, as opposed to those not attending," calling such a strategy "very dubious" (Vedder and Gallaway, 2011). I agree such a strategy would be very dubious. The only problem is that this was not Rose's (2010) strategy. It is a fair characterization of Rose's (2010) *first model* (one of many). But reading beyond that initial, motivating model demonstrates that Rose (2010) is quite aware of the point that one cannot simply look at the behavior of conference attendees and compare it to non-attendees. He presents a variety of alternative specifications, including the difference-in-differences models I describe in my comment and industry-wide tests of wage cutting behavior. Based on this evidence at least (which, I should add, uses disaggregated data), the business community appears to have greeted Hoover with a shrug of indifference rather than a shudder of fear.

The model presented by Vedder and Gallaway (2011) merits at least a brief mention. It highlights the fact that during the volatile period of 1901–1929, money wages are well explained by consumer prices and the tariff (among other factors). They go on to project this model into 1930 and 1931 and find exactly what one would expect: after a dramatic deflation and an ill-advised tariff increase, money wages were higher than they should have been, according to the model. Vedder and Gallaway (2011) treat this unsurprising result as definitive evidence in favor of MacKenzie (2010). It seems to me to have far more to say about the market's response to fundamental uncertainty and large shocks, as well as the Federal Reserve's policy response than it has to say about a public relations stunt by the Hoover administration.

Perhaps MacKenzie (2010) is right about Hoover. Perhaps it was much more than a public relations stunt. If this is the case, MacKenzie (2010) did not provide convincing evidence to that effect, and he also omitted discussion of critical limitations on his claim. *That* is the central argument of my comment, and it is not an argument that Vedder and Gallaway (2011) have sufficiently refuted.

REFERENCES

Barnett, William II, and Walter Block. 2006. "Gallaway and Vedder in Stabilization Policy." *Quarterly Journal of Austrian Economics* 9, no. 1: 57–81.

Gallaway, Lowell, and Richard Vedder. 1998. "Government and unemployment: Reply to DeLong." *Critical Review* 12, no. 3: 253–264.

——. 2006. "How Big Is the Austrian Tent? A Reply to Barnett and Block." *Quarterly Journal of Austrian Economics* 9, no. 3: 67¬–69.

Kuehn, Daniel. 2011. "Hoover and Wages in the Depression: A Comment on Douglas MacKenzie." *Quarterly Journal of Austrian Economics* 14, no. 4: 442–453.

MacKenzie, Douglas W. 2010. "Industrial Employment and the Policies of Herbert C. Hoover." *Quarterly Journal of Austrian Economics* 13, no. 3: 101–119.

Martins, Pedro S., Gary Solon, and Jonathan Thomas. 2010. "Measuring What Employers Really Do about Entry Wages over the Business Cycle." *NBER Working Paper* no. W15767.

Rose, Jonathan D. 2010. "Hoover's Truce: Wage Rigidity in the Onset of the Great Depression." *Journal of Economic History* 70, no. 4: 843–870.

Vedder, Richard, and Lowell Gallaway. 2011. "Hoover and Wages in the Depression: A Comment on Douglas MacKenzie: A Rejoinder." *Quarterly Journal of Austrian Economics* 14, no. 4: 454–461.