A Comment on the Economics of Labor Adjustment: Mind the Gap: Reply

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Russell Cooper and Jonathan L. Willis (2004), henceforth CW, is the latest revision to their critique of our methodology for estimating aggregate nonlinearities when microeconomic adjustment is lumpy. Their case is based on "reproducing" our main findings using artificial data generated by a model where microeconomic agents face quadratic adjustment costs. That is, they supposedly find our results where they should not be found.

The three claims on which they base their case are incorrect. Their mistakes range from misinterpreting their own simulation results to failing to understand the context in which our procedures should be applied. They also claim that our approach *assumes* that employment decisions depend on the gap between the target and current level of unemployment. This is incorrect as well, since the "gap approach" has been derived formally from at least as sophisticated microeconomic models as the one they present. On a more positive note, the correct interpretation of CW's results shows that our procedures are surprisingly robust to significant departures from the assumptions made in our original derivations.

I. Summary of the Case

Throughout, Caballero and Engel (1993, henceforth CE) and Caballero et al. (1997, henceforth CEH) take as an assumption validated in many other studies that at the microeconomic level adjustments are lumpy,[1] and examine whether the implied features of the distribution of microeconomic gaps are useful in explaining aggregate employment fluctuations. Specifically, the basic regression in CE and CEH is:

$$\Delta E_t = \lambda M_t^{(1)} + \gamma M_t^{(3)}, \tag{1}$$

where ΔE represents the rate of growth of aggregate employment, and $M^{(i)}$ is the ith moment of the cross-section distribution of gaps between actual and desired employment at the firm level.

When $\lambda > 0$ and $\gamma = 0$, the equation above simplifies to a linear model where the left-hand-side variable depends only on aggregates (first moments of the cross-section distribution of gaps). This case can be obtained either from a microeconomic model where agents adjust infrequently but with a probability that is independent of their gap (the constant hazard model of Guillermo Calvo, 1983) or from a model where agents face quadratic adjustment costs and adjust all the time (Thomas Sargent, 1978).[2]

When $\gamma > 0$, on the other hand, higher moments of the cross-section distribution of gaps matter for aggregate dynamics.[3] This case can be obtained from a scenario where microeconomic adjustment is lumpy and the probability of such adjustment is increasing in the gap (the increasing hazard model of CE). There is ample microeconomic evidence for this behavior, the question is whether it matters for aggregate adjustment. We find that it does, since our aggregate regressions show a very significant $\gamma >$

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[1] CW (2004) seem to agree with this assumption; in particular, in their conclusion they refer to "overwhelming evidence" in favor of it.

[2] See Julio J. Rotemberg (1987) for a formal proof of the aggregate equivalence of Calvo's lumpy adjustment model and the quadratic adjustment cost model.

[3] The higher moment that matters in specification (1) is the third moment. We focus on this specification because it is simple and shows up often both in our work and in CW's critique. Yet there are other specifications in their and our work that involve higher moments different from the third moment, which explains why we generically refer to "higher moments."

0 *and* a large contribution of $\gamma M_t^{(3)}$ to aggregate employment fluctuations.

CW's critique has changed over time, but as of today, it can be split into three claims, all of them based on applying our procedures to data generated with a model with smooth microeconomic adjustment:

- *Claim 1:* When our measure of microeconomic gaps is computed from their artificial data, there exist parameter configurations for which estimates of γ are similar to ours, even though there is no microeconomic lumpiness or nonlinearities. This has been their main claim, and the common denominator in CW (2001, 2002, 2004).
- *Claim 2:* When the microeconomic gaps are not directly observed but can be estimated with microeconomic data, the procedures used in CEH give nonsensical results when applied to their data.
- *Claim 3:* When only aggregate data are used, coupled with the Kolmogorov equations required to keep track of the simulated cross-section distribution of gaps, as in CE, our estimates can be found even when their (linear) data are used.

Not only are these claims incorrect, as we will argue below, but they also reflect a fundamental misunderstanding of the point of our papers. We developed a methodology to study whether lumpy microeconomic adjustment has aggregate implications, not to infer from aggregate data whether the underlying microeconomic adjustments are lumpy.

In Section II we show that due to a basic interpretation error of their own results, Claim 1 is incorrect. In Section III we argue that since the identification strategy we adopt for estimating gaps with microeconomic data is built on the *observation* that microeconomic data are lumpy, it should not be used if microeconomic data are not lumpy. Therefore Claim 2 is not surprising. Furthermore, the fact that CW find nonsensical results, while we find meaningful and statistically significant results, indicates that our findings do not arise when microeconomic adjustments are smooth.

In Section IV we show that Claim 3 has nothing to do with lumpy vs. nonlumpy microeconomic adjustment. Their finding comes from relaxing to an extreme the maintained assumption of our analysis that the driving forces are random walks.[4] This result is neither new nor quantitatively comparable to what we found with actual data.

Somewhat paradoxically, the work of CW can be used to show that our approach is robust to departures from the random walk assumption. In fact, nothing can be found with the serial correlation of 0.81 used in CW (2002), and (almost) nothing with the low serial correlation of 0.47 assumed in CW (2001). CW (2004) dropped it further to 0.28, and even then the gain in R^2 from adding higher moments is substantially less than half of what we found. Section V concludes.

II. Their Main Critique

In the main part of their critique, CW compute from their artificial data the cross-sectional moments of static gaps and estimate an equation analogous to (1):

$$\Delta E_t^{CW} = \lambda M_t^{(1),CW} + \gamma M_t^{(3),CW}, \tag{2}$$

where ΔE^{CW} and $M^{(i),CW}$ stand for the rate of growth of aggregate employment and the ith moment of the cross-section distribution of static gaps respectively, when the underlying data are generated with CW's quadratic adjustment cost model.

Their main finding is that they estimate a positive and statistically significant γ, not very different from the one we find using actual data. Cooper and Willis then argue that this is evidence that a researcher testing for aggregate nonlinearities on their data would conclude, erroneously, that these are important for aggregate dynamics. It follows, they argue, that our

[4] In our derivations, and as is standard in much of the (S, s)-literature, we assumed that the driving forces follow a random walk, an assumption that cannot be easily rejected in the data. In this case, one can show that the static gap (the difference between current employment and the optimal level of employment if there are no adjustment costs) is equal to the frictionless gap (the difference between current employment and the optimal level of employment if adjustment costs are removed only today) plus a constant that depends on the drift. This is a very useful result since the static gap is straightforward to calculate while its frictionless counterpart involves more complex dynamic calculations.

TABLE 1—ESTIMATION WITH STATIC GAP

	CW (quadratic adjustment)	CEH (lumpy adjustment)
R^2 without nonlinear parameter	0.90	0.65
R^2 with nonlinear parameter	0.90	0.79
Increase in R^2 after adding nonlinear parameter	0.00	0.14
Minimum adjustment speed (nonlinear model)	0.19	0.31
Maximum adjustment speed (nonlinear model)	0.20	0.46
Range of adjustment speeds (nonlinear model)	0.01	0.15

Notes: CW column based on Table 1, panel A, in CW (2004). CEH based on Table 3 in CEH (1997). Maximum, minimum, and range of adjustment speeds are calculated considering adjustment hazards in the range $\mu_G \pm 2\sigma_G$, where μ_G and σ_G denote the mean and standard deviation of the cross section of static gaps for the model under consideration.

methodology is flawed and our results may well be due to misspecification error.

However, finding similar values of γ does *not* mean that a researcher will conclude that the nonlinear term is equally important for aggregate dynamics in the two cases. For this, one needs to also look at whether the regressor that is multiplied by γ has similar variability in the two cases. It turns out that it does not: The variability of $M^{(3)}$ in CW's quadratic adjustment model is *much smaller* than that of the corresponding moment when micro-adjustments are lumpy. Thus, the contribution of $\gamma M^{(3)}$ is minuscule in explaining aggregate employment volatility in CW's simulated data, while it is large and economically significant in our findings. Simply put, the reported values of neither R^2 nor λ change when adding the nonlinear term in CW's comment, while they change substantially in our setting (λ falls and R^2 rises).

The first column in Table 1 is based on Table 1, panel A, in CW (2004). It is apparent from their table that the R^2 reported when estimating (2) is the same with or without the third moment of gaps: 0.90. Similarly, the estimated value of the nonlinear parameter γ, even though statistically significant,[5] is economically irrelevant, as the adjustment speed varies by less than 0.013 over the relevant range of gaps.[6] By contrast, in the corresponding exercise in Table 3 of CEH, reported in the second column of Table 1 here, the R^2 increases by 0.15 when adding a nonlinear parameter and the variation of the speed of adjustment over the relevant range is more than *ten times* as large as that in CW's model.

The economic irrelevance of the nonlinearities estimated by Cooper and Willis is even more striking in the 2002 version of their Comment, where they used a more realistic value for the first-order correlation of productivity shocks (0.81 at an annual level).[7] There they report an R^2 of 0.97, both for the model with and without the nonlinear parameter,[8] and the adjustment speed implied by their nonlinear model varies only by 0.005 over the relevant range of values taken by the gap.

III. Estimating Unobserved Gaps with Microeconomic Data

The second and third points of CW's critique stem from the fact that in practice the gaps are

[5] The statistical significance they find possibly reflects the fact that they use time series with 1,000 observations in their simulations, while CEH's estimates are based on 35 observations.

[6] Where the "relevant range" is defined as $\mu_G \pm 2\sigma_G$, with μ_G and σ_G denoting the mean and standard deviation of the cross section of static gaps, respectively. A tedious but straightforward calculation from first principles shows that

$$\sigma_G^2 = \frac{\sum_{k \geq 0} d_k^2}{(1-\alpha)^2}\,\sigma_\varepsilon^2,$$

with:

$$d_k = \frac{G\lambda}{\lambda + \rho - 1}(1-\lambda)^k + \left[1 - \frac{G\lambda}{\lambda + \rho - 1}\right]\rho^k,$$

where $G = (1-\delta)/(1-\delta\rho)$, λ denotes the speed of adjustment in the partial adjustment representation of the quadratic adjustment cost model, δ denotes the discount rate that results in this model when calculating the (correct) dynamic target as a present value of future static targets, σ_ε denotes the standard deviation of firm-specific productivity shocks and α is defined on p. 23 in CW (2002).

[7] As we pointed out the errors in the first and second versions of CW's critique, they reacted by looking for a new parameter configurations and new model specifications that might help their case. Their lack of success, despite two major revisions of their original Comment, possibly is the best evidence of the robustness of our findings.

[8] This is for the benchmark with high adjustment costs. For the benchmark with low adjustment costs, both values reported for R^2 are 1.00.

not observed and hence neither are the cross-sectional distributions of these gaps. They argue that our procedures to estimate these gaps and moments introduce new errors which can lead to false positives and nonsensical results. Again, Cooper and Willis are mistaken.

To explain why, we begin by making a distinction between the procedure in Caballero et al. (1997) (in this section) and that in Caballero and Engel (1993) (in the next section), since Cooper and Willis' specific critique differs between these cases (corresponding to their claims 2 and 3, respectively).

In CEH we observe the microeconomic data but have no direct observation of the gaps. In order to construct the microeconomic gaps, we use information on hours. The idea being that when hours exceed certain normal level, there is a shortage of labor while the opposite is true when hours are below normal. Still, one needs to estimate the mapping from the hours gap to the employment gap, and the equation that does this suffers from classic simultaneity problems. Our way out relies heavily on our *observation* that microeconomic adjustment is lumpy. In this context, the relationship between hours and employment gaps can be estimated if one only uses observations where large adjustments took place; the basic logic behind this procedure being that during these episodes the variability of the regressor swamps the variability of the error term in that regression. Yet if one knows that microeconomic data are not lumpy, as is the case with Cooper and Willis' data, no sensible researcher would use our procedure. Cooper and Willis make the mistake of not understanding that the microeconomic estimation procedure in CEH is *conditional* on the observation that microeconomic behavior is lumpy. Fortunately for us, the latter holds in reality, a fact explicitly acknowledged by CW.[9]

IV. Estimating Unobserved Gaps Using Only Aggregate Data

In Caballero and Engel (1993) we do not observe microeconomic data and hence generate the cross-sectional moments from an internally consistent model. This model starts from the well-established fact that microeconomic adjustment is lumpy, and uses this information to construct the Kolmogorov/Markov functional equation for the evolution of the cross-section distribution corresponding to a given set of parameters. Cooper and Willis apply our procedure to data generated by their quadratic adjustment cost model, and find evidence that γ in equation (2) is positive when it should be zero.

Here CW fail to identify the real reason behind their finding, which is the very low serial correlation assumed in their driving processes.

In our derivations we assumed that the driving forces follow a random walk. As mentioned in the introduction, in this case the static gap is equal to the frictionless gap plus a constant. It is well known within the (S,s) literature that if the random walk assumption is relaxed, the static gap no longer is a sufficient statistic for the probability of adjustment, so that the difference between static and frictionless gap now depends on the state. The first step in CW is to rediscover this result.[10] They then drop the serial correlation of the driving forces from one to around 0.28 (we report all serial correlation coefficients at annualized rates) and go on to generate microeconomic data with a quadratic adjustment cost model. It is only then that they find, under some circumstances, results qualitatively similar to ours. But this is neither new (we already knew that for very large departures from the random walk assumption static and frictionless gaps could not be exchanged) nor quantitatively comparable to our findings.

Paradoxically, the findings in CW are encouraging for the gap approach, since it is only when the serial correlation is dropped to very low levels that things start breaking down. In fact, for the values of serial correlation used in CW

[9] "[There is] overwhelming evidence that plant-level adjustment is nonlinear," CW (2004), first paragraph in the conclusion.

[10] Although they fail to highlight the connection between their sharp departure from the random walk assumption and the difference between both gap measures. Also, beginning in their abstract, they mislead their readers by repeatedly claiming that our approach *assumes* that the optimal policy depends on the gap. In the final sentence of Section II (in CW, 2004) they finally acknowledge that the "gap approach" can be *derived* from optimizing behavior when shocks follow a random walk, yet credit a previous version of their comment for this well-known result (see, for example, Stephen Nickell, 1985).

TABLE 2—ESTIMATION WITH INFERRED STATIC GAP: MACROECONOMIC DATA

Data	Driving force ρ (annual)	Increase in R^2
BLS, as in CE (1993)		*0.13*
Sim. Quadr. Adj.	1.00	0.00
Sim. Quadr. Adj.,	0.75a	0.01
Sim. Quadr. Adj. as in CW (2001)	0.47	0.03
Sim. Quadr. Adj., as in CW (2004)	0.28	0.05

Notes: "Increase in R^2 denotes the difference between the R^2 obtained when estimating a model with a nonconstant hazard and the R^2 when imposing a constant hazard, in both cases using the methodology in CE (1993). "Sim. Quadr. Adj." stands for "Simulated Quadratic Adjustment."

[a] This value of ρ is below both the value in the driving force used in CE and the values used when calibrating RBC models (see Cooley and Prescott, 1995).

(2001), which are already low,[11] there would be no significant false positive finding.

Table 2 reports the gains in R^2 that we found in CE from adding a hazard term increasing in the (absolute) gap, versus those that would be obtained from doing the CW exercise with different degrees of serial correlation in the driving processes.[12] Clearly, there is no risk of false positives (i.e., of finding an increasing hazard when there is none) if serial correlation is not too far from the assumed random walk. CW had to stretch things a lot to find parameters similar to ours, and even then the gain in fit was less than half of the gain we found.

V. Final Remarks

The first paragraph in the conclusion of CW illustrates the flawed logic of their approach. It concludes that "despite the overwhelming evidence that plant-level adjustment is nonlinear, the question of whether this matters for aggregate employment dynamics remains an open issue." But if the goal is to show whether *clearly established microeconomic lumpiness* matters at the aggregate level, then the natural approach is to start from a model with microeconomic lumpiness and determine whether aggregation removes all traces of micro nonlinearities, which is precisely what our methodology is designed to do. Instead, Cooper and Willis start with simulated data that does not resemble actual microeconomic data at all, and test whether a procedure designed to test competing hypotheses that satisfy the microeconomic lumpiness condition provides false positives when applied to their counterfactual data. This is twisted logic at best.

In our Reply, however, we have made an effort to take their claims seriously. But there is very little than can be rescued from the sequel of CW's attempts. The results they claim to find are either wrong, or irrelevant, or driven by an extraneous ingredient. Let us recap what they did and the conclusions they should have drawn:

1. CW relax both of our maintained assumptions—that microeconomic adjustments are lumpy and that driving forces follow a random walk—in an extreme fashion. The evidence on lumpy microeconomic adjustment is overwhelming, even Cooper and Willis acknowledge it at times, and our assumption of a random walk is definitely closer to reality than their assumption of an annual first-order correlation as low as 0.28.
2. Correctly interpreted, their main result implies the exact opposite of their Claim 1. When the microeconomic gaps are observed, our methodology does not detect significant nonlinearities when applied to data generated even with the major departures from our assumptions considered by CW.
3. When the microeconomic gaps are not observed but need to be estimated from microeconomic data, one should not use our identification strategy (which relies on microeconomic lumpiness) with their data, where adjustment is known to be smooth. In any event, the parameter estimates they find with their counterfactual data are not statistically significant, in sharp contrast with those we found with actual data.

[11] The annualized serial correlation they use in CW (2001) is close to 0.50. The serial correlation in the actual driving force we used in Caballero and Engel (1993) is above 0.80. Also note that the standard value used to calibrate real-business-cycle (RBC) models (see, e.g., Thomas F. Cooley and Edward C. Prescott, 1995) is 0.81.

[12] We replicate CW's procedure and use 1,000 observations as they did. We also add independently and identically distributed (i.i.d.) normal noise to the aggregate shock in order to calibrate the R^2 of the constant-hazard/quadratic adjustment cost model to match the R^2 in CE for this model (0.75).

4. Finally, when only aggregate data are available and the path of the cross-sectional distribution needs to be simulated, the assumptions about the serial correlation of the driving processes become more important. This is not new. The surprising feature of CW's results is that even after dropping the serial correlation as much as they do in the most recent version of their comment, the explanatory power of the nonlinear terms in their experiments is less than half of what we found in the data. If one adopts more realistic assumptions on the persistence of the driving processes, and uses the values assumed in CW (2002), there is essentially no gain from adding higher moments to their regressions. Again, and contrary to their claim, there is no false positive finding even when applying our methodology to highly unrealistic data.

The other two paragraphs in their conclusion carry the implicit messages that "the gap approach" is voodoo economics and that they are ready to deliver a superior gap-free alternative. First, what they call "the gap approach" has been derived formally by us and many others before us from at least as sophisticated microeconomic models as the one they present [for this, see the extensive literature on the optimality of (*S*,*s*) models].[13] Second, and perhaps more importantly, the methods derived from dynamic optimization that do not "rely on gap measures" already exist in published work. In fact, the difficulties in measuring gaps was the motivation for Caballero and Engel (1994, 1999).[14]

To end on a more positive note, CW's approach contrasts with more constructive and interesting recent developments in the literature on the macroeconomic implications of lumpy microeconomic adjustments. For example, Aubin Kahn and Julia K. Thomas (2003) conclude that within an otherwise standard RBC model, fixed costs of adjusting capital do not have a significant impact on aggregate investment.[15] This finding has been misinterpreted by many as a demonstration that fixed costs do not matter for actual investment. But this is not what they did. In fact, they also show that the aggregate data generated by such a model misses important features of actual aggregate data, such as the skewness caused by investment spikes. And that such spikes can be generated by microeconomic fixed costs if the interest rate is not endogenized (confirming the results in Caballero and Engel, 1999). This finding points to an interesting and fruitful area of research: How does the RBC model need to be modified for it to capture the nonlinearities that are observed in aggregate investment? Let us hope that energy will be spent on this type of question.

[13] The first proof of optimality of (*S*,*s*) policies is in Herbert E. Scarf (1960). For important extensions, relevant to the models discussed in this reply see, among others, Michael Harrison et al. (1983), Sanford Grossman and Guy Laroque (1990), and the pedagogical article by Avinash Dixit (1991).

[14] In these papers we extended the (*S*,*s*) literature to incorporate *stochastic* adjustment costs and estimate a structural model via maximum likelihood.

[15] See Marcelo Veracierto (2002) for a similar conclusion in a model of irreversible investment.

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