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# Reply to LeCraw, Montanera, and Mroz on Hospitals' Malpractice Claims and Costs

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[LINK TO ABSTRACT](#)

The previous issue of *Econ Journal Watch* features an article by Florence LeCraw, Daniel Montanera, and Thomas Mroz—henceforth, LMM (2023)—examining the 2018 article in *Health Affairs* by us and seven other coauthors on the outcomes of implementing a communication-and-resolution program (CRP) in six Massachusetts hospitals (Kachalia et al. 2018). For a brief explanation of the purpose and nature of CRP programs, we recommend the second paragraph of LMM’s article. As LMM go on to say in their third paragraph, the disagreement between them and us does not amount to different conclusions about the utility of CRPs for addressing patient injuries; both they and we conclude that the CRP approach is promising. Rather, the disagreement is over the analytic approach that should be used to generate evidence concerning CRPs’ effects (LMM 2023, 1–2).

Their paper caps over four years of engagement between our study team and LeCraw and Mroz to address their questions about our study. Their criticisms have been a moving target over time as we have addressed particular points; the latest round alleges “three major methodological errors” but discusses two. Both critiques are misplaced.

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First, LMM assert that when using an interrupted time series design, the hospitals in our sample that did not implement CRPs are inappropriate counterfactuals for those that did. Their concern is that the pre-implementation liability trends in the treatment and comparison hospitals were not identical. LMM (2023, 3) offer an alternative method of determining the outcomes of CRP implementation that directly compares one of the hospitals that implemented a CRP to other hospitals without CRPs. Importantly, we considered and rejected that design. The reason it was deemed inappropriate was that hospitals could not provide all of the data needed to adequately control for the range of ways in which they might differ. Because of the risk of residual confounding, we selected an interrupted time-series design, which uses each hospital as its own control. Non-implementing hospitals were included in the sample, undergoing the same analysis as a way of showing whether the changes observed over time at the implementing hospitals also held true at hospitals that did not implement CRPs.

LMM (2023, 4) report that the pre-implementation slopes of the implementing and comparison academic medical centers (AMCs) are not identical. Though we disagree with LMM's characterization of the nonparallelism as "severe" (2023, 9), it's reasonable to conclude that various differences among the sites should give pause about directly comparing sites. That is why our interrupted time-series approach was selected and is appropriate, while LMM's preferred approach is not appropriate.

As reported in our article's Online Appendix, because our analysis focused on the pre/post comparison within each hospital, we performed testing to verify that the requirements for conducting interrupted time-series analysis held in our data. We verified that the stationarity assumption was met for all the tested outcomes using the Dickey-Fuller test and the autocorrelation and partial autocorrelation functions. We concluded that the series were stationary and therefore correction for differencing was not needed.

Contrary to LMM's suggestion (2023, 9–10), we did not conduct our analysis within a causal modeling framework. Rather, our observational analysis yielded measures of association and is described as such in the article. LMM's claim that interrupted time series was deployed to provide "causal evidence" is incorrect, as is the assertion that we characterized our results as showing improved liability outcomes "due to CRP" (2023, 3, 9).

LMM (2023, 9) worry that we introduced "Difference in Nominal Significance" errors with our design. Specifically, they describe error arising from comparing the changes in outcomes over time between independent groups, where one result is significant and the other null, and concluding that the two groups' outcomes are different. Their critique exports arguments made in an article by Martin Bland and Douglas Altman (2015) to a different context where the argu-

ments have little traction. (The other article that LMM cited to buttress their criticism, by David B. Allison et al. (2016), doesn't discuss the issue.) Bland and Altman make a persuasive case for pooling data across units of analysis in the context of a randomized, controlled trial. But in the absence of randomization, if adequate control for confounding variables cannot be assured, there are strong arguments against an approach that relies heavily on comparing observational units rather than also using each unit as its own control. Further, of the three potential adverse consequences of this type of error described by Bland and Altman, only one—the possibility of declaring a difference significant when it is not—has any potential applicability to our study. Yet for four of the six outcome variables we concluded that there were no significant differences. Our study's core conclusion is not that there were significant improvements in liability outcomes associated with CRP implementation, but that hospitals can implement CRPs without experiencing worse liability outcomes.

LMM's second line of critique is that when they implemented a different modeling approach, they observed a "large spike in both of the liability outcomes" that occurred "as soon as CRP was implemented" (2023, 6). They allege that our article reports downward trends in liability outcomes but does not acknowledge large, immediate increases in the levels of liability outcomes in the implementing hospital. In our own models, too, some hospitals exhibited upwards shifts in the intercept for some outcome variables. This was reported in our article (Kachalia et al. 2018, 1840–1841), alongside our judgment that readers ought to focus on the changes in slope because what occurs immediately following a CRP's implementation date (i.e., the shift in level) is a less reliable and informative indicator of the program's effect than longer-term trends in the outcomes (i.e., the shift in slope).

There are two reasons why what occurs immediately following a CRP's implementation date is less informative than longer-term outcomes. First, a CRP's effects don't switch on overnight. A go-live date can be designated, but as with any complex quality-improvement program, full implementation takes a period of time. Based on our close monitoring of these programs, a ramp-up period of about three months took place at the implementing hospitals. To account for this, our main analysis imposed a one-quarter lag from the start of the post-implementation period. Second, CRPs take time to bring cases to resolution. Some cases may be resolved in a matter of days, but others—for example, those in which the insurer conducts a second review of the case for possible compensation—may require months. Consequently, one cannot observe the program's effect immediately.

An example of the problem posed by relying too heavily on intercept shifts and of using LMM's modeling approach is their finding of "a 200 percent increase in compensation costs (a tripling) immediately following CRP implementation"

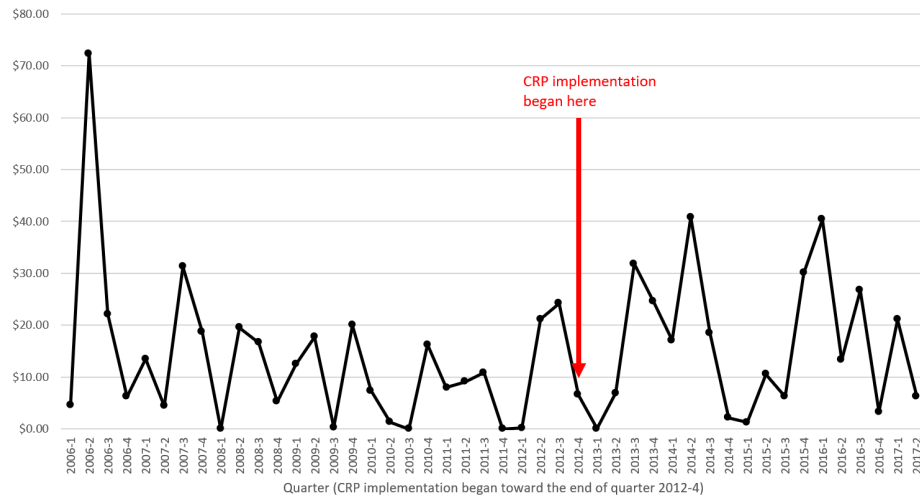
at one of the AMCs (LMM 2023, 8). This result strains plausibility; we know of no mechanism through which it could occur. It arises from a model that LMM constructed from the parameter estimates we reported—not using actual data. As they report (2023, 2), the terms of our data use agreement with the involved liability insurers did not permit data sharing. Such terms are unfortunate because they can leave other scholars with few options for pressure-testing study results, leading to second-best strategies that generate information that is simply wrong.

Simple descriptive plots showing the actual, unadjusted data on mean compensation cost rate in each quarter (Figure 1 here) demonstrate that LMM's model predictions do not match what actually occurred, which was a continuation of that hospital's historical experience of considerable quarter-to-quarter volatility in quarterly compensation cost rates. The same is observable in plots of the actual defense-cost rate (Figure 2) and rate of new claims (Figure 3).

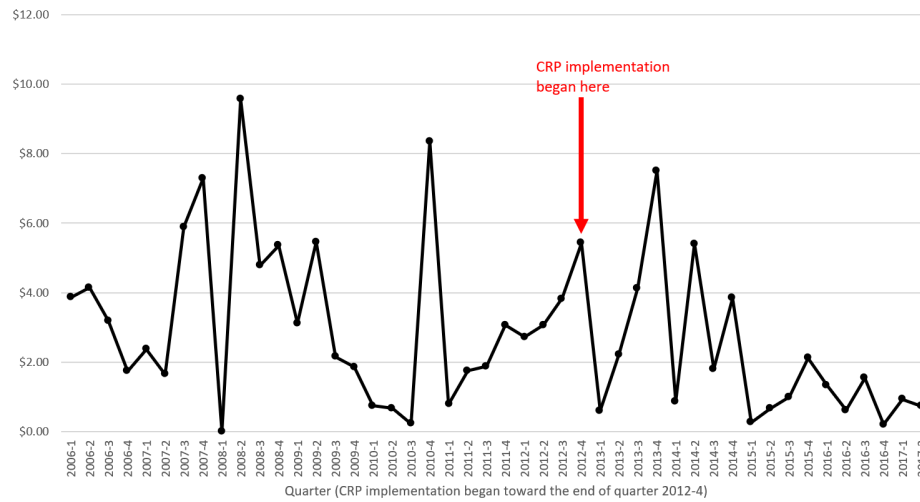
Another problem with the approach taken by LMM relates to open claims—that is, claims that had been initiated but not yet resolved at the time data collection concluded. The fact that 16 percent of the claims in our sample remained open creates analytical difficulties, mostly affecting the post-implementation period. As described in our article, the problem is that if we were to impute costs for these unresolved claims using data from the past, it is likely those imputed costs would be too high, because they would be based on the hospital's old (pre-CRP) approach to resolving claims. If so, it could obscure the savings from CRPs. For example, in a sensitivity analysis (Kachalia et al. 2018, Appendix p. 18) we saw that for one academic medical center, excluding open claims increased the magnitude of the change in trend for new claims ( $-0.098$  without open claims vs.  $-0.066$  with open claims) and decreased the upward shift in level ( $0.547$  without open claims vs.  $0.766$  with open claims). Importantly, our sensitivity analysis (ibid., 18–20) showed that the exclusion of open claims affected the results differently across implementing and non-implementing hospitals. As we reported, excluding open claims did not affect the significance of changes in cost trends for implementing hospitals, but it did for non-implementing hospitals. Therefore, the open-claims problem may have affected the LeCraw team's findings, which arise from a direct comparison between the implementing and non-implementing hospitals.

We agree that performing rigorous analyses of the effects of CRPs—using designs that fit the intervention, study questions, and data—is important because of the broad interest in these programs among U.S. healthcare organizations. Unfortunately, the work by LMM in the March 2023 issue of *Econ Journal Watch* is not such an analysis.

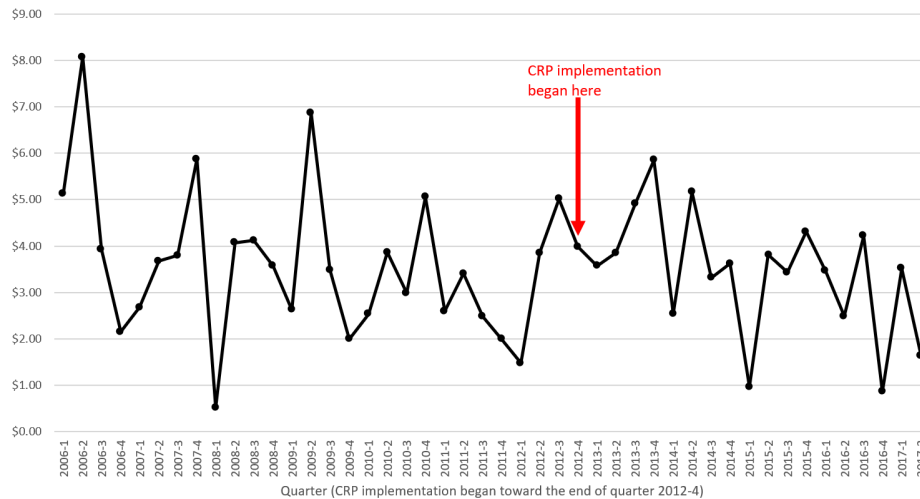
**Figure 1.** Compensation cost rate at AMC1 (unadjusted data)



**Figure 2.** Defense cost rate at AMC1 (unadjusted data)



**Figure 3.** New claims rate at AMC1 (unadjusted data)



## References

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