



A Rejoinder on the Effects of a Communication-and-Resolution Program on Hospitals' Malpractice Claims and Costs

Florence R. LeCraw¹, Daniel Montanera², and Thomas A. Mroz³

[LINK TO ABSTRACT](#)

There cannot be an effect without a cause.

– Alexander Hamilton, Federalist No. 31

The three pieces that precede the present rejoinder are:

1. Allen Kachalia, Kenneth Sands, Melinda Van Niel, Suzanne Dodson, Stephanie Roche, Victor Novack, Maayan Yitshak-Sade, Patricia Folcarelli, Evan M. Benjamin, Alan C. Woodward, and Michelle M. Mello, “Effects of a Communication-and-Resolution Program on Hospitals’ Malpractice Claims and Costs,” *Health Affairs*, 2018.
2. Florence R. LeCraw, Daniel Montanera, and Thomas A. Mroz, “Reassessing the Effects of a Communication-and-Resolution Program on Hospitals’ Malpractice Claims and Costs,” *Econ Journal Watch*, March 2023.
3. Maayan Yitshak-Sade, Allen Kachalia, Victor Novack, and Michelle

1. Georgia State University, Atlanta, GA 30303.

2. Grand Valley State University, Allendale, MI 49401.

3. Georgia State University, Atlanta, GA 30303. The views expressed here are the authors’ and not necessarily those of the Federal Reserve Bank of Atlanta or the Federal Reserve System.

M. Mello, “Reply to LeCraw, Montanera, and Mroz on Hospitals’ Malpractice Claims and Costs,” *Econ Journal Watch*, September 2023.

It appears that the replying authors agree (at least now) that the characteristics of the data and the circumstances described in the 2018 *Health Affairs* article were wholly inappropriate for advancing a causal interpretation for the original study’s results (Yitshak-Sade et al. 2023, 350). Our primary concern upon reading the original study was misleading statements about causal interpretation. While it is always better to head off such misinterpretation in the original article, the authors should be commended for making the correction now.

In this rejoinder we raise three matters for consideration. First, whether use of causal language, and the inclusion of comparisons to untreated non-CRP hospitals, are likely to induce the audience of *Health Affairs* to infer that the study results are causal. Second, even when examining associations instead of causality, whether large post-treatment spikes in outcomes of interest should be ignored in favor of changes in trend, simply because the spikes are difficult to explain. And, finally, how effectively data sharing agreements insulate original analyses from the scrutiny of reanalyses based on the published information.

Causal misunderstanding

The original authors say they never meant for the non-CRP hospitals to be used as control groups. As they state in their response to our article, the non-CRP hospitals were simply displayed “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” (Yitshak-Sade et al. 2023, 350). This seems, to us, remarkably like the role of a control group in causal inference, and so confusion among the audience about causality seems not only understandable but probable. For future reference, in studies where the authors do not wish the audience to compare CRP and non-CRP hospitals, they should avoid framing their research question by “examining before-and-after trends in claims volume, cost, and time to resolution and *comparing them to* trends among nonimplementing peer institutions” (Kachalia et al. 2018, 1836, italics added).

Their reply states “we did not conduct our analysis within a causal modeling framework” (Yitshak-Sade et al. 2023, 350). We agree and appreciate this clarification for the audience. To prevent future misunderstandings, we recommend further rewrites to the original article:

Original article text (all italics added)	Recommended rewrite
“ <i>Effects of A Communication-And-Resolution Program on Hospitals’ Malpractice Claims and Costs</i> ” (Kachalia et al. 2018, article title)	“Hospitals’ Malpractice Claims and Costs Before and After a Communication-and-Resolution Program”
“We evaluated the liability <i>effects</i> of CRP implementation at four Massachusetts hospitals...” (ibid., 1836)	“We study liability outcomes at four Massachusetts hospitals before and after CRP implementation.”
“implementing a communication-and-resolution program <i>does not expand</i> liability risk and <i>may, in fact, improve</i> some liability outcomes” (ibid., 1843)	“We find no increases in liability risk following CRP implementation. There may, in fact, be decreases in some outcomes following implementation.”

We note and appreciate the replying authors’ attempt to clarify in their response to our comment: “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” (Yitshak-Sade et al. 2023, 351). If the word “can” is used simply to allow for possibility, then the latter offered conclusion is one we can get behind. But if “can” suggests a promising potentiality, then the latter remains causal; and it does little more than add a double negative, implying something like, “We’re not saying they’ll get a good outcome with CRP; we’re saying they won’t get a bad outcome.” More precise wording could resolve confusion about the meaning of this statement.

Spikes at implementation

On the post-implementation spikes, we agree that there should be latitude around leads and lags and, notwithstanding clear justification and the scrutiny of the referees, they are at the discretion of the study authors. Discounting the estimated spikes because they are difficult to explain, or seem implausibly large to have resulted entirely from the treatment, is another matter. Could such spikes be indicative of a model misspecification in the original analysis? What if the treatment caused most of the spike? Or some of it? At what degree of the ‘treatment responsibility’ does the spike become problematic to the conclusion? These are a few reasons why many standard modern methods for inferring causality do not ignore level effects. It cannot be ruled out that the spikes have something to do

with CRP implementation, and so they must be accounted for in the confidence intervals. Similarly, as the original authors mention, the existence of open claims are problematic and have a bearing on estimates and their interpretations. We have encountered these issues in our own original research, and understand the challenges, but the solution cannot be the jettisoning of reliable causal inference methods from our studies, particularly when causal language is retained.

Data darkness ought to raise the bar

As the replying authors mention, agreements disallowing the distribution of data to groups holding original research up to scrutiny are not ideal. Rather than a shield, as the replying authors seem to regard it, we contend that a data sharing restriction places an even greater burden on original studies to acknowledge and accept criticism from reanalysis studies. While the authors (now) do not want the audience to interpret the results as causal, we thought actual causal investigation was warranted given the causal language permeating the original study and its invited comparisons to untreated subjects. In the absence of the original data, we evaluated as best we could, using *their* estimated models' coefficients and some data means,⁴ what their estimation procedure revealed when viewed through a causal lens. The resulting interpretation is quite different. The only way our reanalysis could have been improved was if, instead of us estimating the standard errors, the original authors had supplied them to us (which they declined to do). It is important to note that the replying authors do not dispute our calculations using their coefficient estimates. They merely state that "This result strains plausibility" (Yitshak-Sade et al. 2023, 352). We agree, but those calculations are the implication of *their* estimated coefficients. So, while our reanalysis may be "second-best" to an identical one with access to the original data (*ibid.*), it may still offer more reliable

4. The approximately 200 percent increase in costs we reported in our original article *follows directly* from the coefficient estimates reported by Kachalia et al (2018) in their Online Appendix. Consider, for example, *their* estimate of the post-implementation "change in intercept" for Compensation Cost at AMC 1 reported in their Appendix Exhibit A.9 (and also in their Appendix Exhibit A.10), which equals 1.240. Since the quasi-Poisson, General Additive Model for their estimations uses a log link function, from just this one coefficient "turning on" in the post-CARE implementation period, the predicted mean Compensation Cost shifts gets multiplied by a factor of 3.46 [$3.456 = \exp(1.240)$]. The impact of this post-CARE intercept shift for Compensation Costs at AMC 1 can be interpreted as an increase in Compensation Cost of 246 percent [$245.6 = 100 \times (3.46 - 1)$]. *Their* reported estimates clearly imply a huge increase in predicted mean Compensation Cost post-CARE implementation, even after adjusting for the "change in slope" parameter they report as -0.035 . The comparable change in Compensation Cost at the comparison AMCs from this "one coefficient" is just 16 percent [$15.7 = 100 \times (\exp(0.146) - 1)$]. See LeCraw, Montanera, and Mroz's (2023) Online Appendix for complete details on how we carried out our calculations.

insights than an initial investigation that plays loose with causal language.

In conclusion, our uncontroversial view is that a study making causal statements must hold up to scrutiny based on standard and accepted causal inference methods. If authors are not prepared to suffer this scrutiny, or do not wish to convey a causal interpretation, then the causal language should be removed and an alternative interpretation of the study's contribution must be reinforced. A study cannot have it both ways. We think it is clear to the audience what happened here with the original study, reanalysis, and rejoinder. If causal inference was not intended, why use the causal language? If only before-and-after analysis was intended, why make a comparison to non-CRP hospitals? Why include the non-CRP hospitals at all? We may never receive answers to these questions. The most important outcome of all this is that, with the assistance of the replying authors, any confusion around causal interpretations of their study seems to have been resolved.

Furthermore, flaws in the editorial process (the details of which are apparently not in dispute) at *Health Affairs*, a major health services research journal, have been brought to light and can be used for organizational learning moving forward. While it may be a small step for academia, it is at least taken in the right direction.

References

- Kachalia, Allen, Kenneth Sands, Melinda Van Niel, Suzanne Dodson, Stephanie Roche, Victor Novack, Maayan Yitshak-Sade, Patricia Folcarelli, Evan M. Benjamin, Alan C. Woodward, and Michelle M. Mello.** 2018. Effects of a Communication-and-Resolution Program on Hospitals' Malpractice Claims and Costs. *Health Affairs* 37(11): 1836–1844. [Link](#)
- LeCraw, Florence R., Daniel Montanera, and Thomas A. Mroz.** 2023. Reassessing the Effects of a Communication-and-Resolution Program on Hospitals' Malpractice Claims and Costs. *Econ Journal Watch* 20(1): 1–14. [Link](#)
- Yitshak-Sade, Maayan, Allen Kachalia, Victor Novack, and Michelle M. Mello.** 2023. Reply to LeCraw, Montanera, and Mroz on Hospitals' Malpractice Claims and Costs. *Econ Journal Watch* 20(2): 349–356. [Link](#)

About the Authors



Florence LeCraw was a practicing anesthesiologist for over 35 years. In the later part of her career, she specialized in health economics, which she now pursues full time. She collaborates with economists and other specialists to investigate questions about patient safety, medical liability, and healthcare professional burnout. She is an Adjunct Professor at the Andrew Young School of Policy Studies at Georgia State University in Atlanta. She is on the hospital staff at Northside Healthcare System. Her email address is frlwatts@gmail.com.



Daniel Montanera is an Assistant Professor in the Department of Economics in the Seidman College of Business at Grand Valley State University. His research investigates defensive medicine and the impact of medical malpractice liability costs on health care spending and access to care, as well as auction theory in health insurance exchanges. His work has been published in such journals as *Information Systems Research*, the *Journal of Law, Economics, and Organization*, *Manufacturing & Service Operations Management*, and the *European Journal of Health Economics*. His email address is montaned@gvsu.edu.



Thomas A. Mroz holds the Bernard B. and Eugenia A. Ramsey Chair of Private Enterprise and economics professor at Georgia State University's Andrew Young School of Policy Studies. He is a Research Fellow at the Federal Reserve Bank of Atlanta. He develops econometric models for estimating the impacts of household behaviors on dynamic human development, health, and fertility processes that incorporate incomplete information and factors unobservable to the researcher as key determinants of these processes. His research assesses the reliability of estimators of behavioral economic relationships for explanations of health, labor, and demographic outcomes. His email address is tmroz@gsu.edu.

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