



EJW

ECON JOURNAL WATCH
Scholarly Comments on
Academic Economics

ECON JOURNAL WATCH 20(1)
March 2023: 1–14

Reassessing 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](#)

This paper describes three major methodological errors appearing in a single *Health Affairs* paper, errors that escaped the scrutiny of the reviewers and editorial staff. It also describes how the editors at *Health Affairs* dealt with the errors once we made them aware.

The research topic that concerns us here explores the impacts of a set of practices gaining popularity at medical facilities, chiefly hospitals. This set of practices is called CRP, for Communication and Resolution Program. CRP is used when a patient suffers an unexpected adverse outcome. It addresses patient safety, medical liability, and healthcare professional wellness. The basic premise of CRP is based on honesty among providers, hospitals, patients, and their families after an unexpected adverse outcome occurs. CRP includes an explanation of the reason for the adverse outcome, an apology if an error occurred, a discussion of ways to prevent recurrence of the error upon future patients, and an offer of proactive compensation. CRP includes a program to help healthcare professionals deal with

1. 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. The authors received no financial support for the research, authorship, and/or publication of this article. The authors have no financial or non-financial interests that are directly or indirectly related to the work submitted for publication. We thank William B. Sage, M.D., J.D., for his discussion with us about this topic.

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

3. Georgia State University, Atlanta, GA 30303.

the mental anguish they may experience when their patient experiences an unexpected adverse outcome. CRP is in direct contrast to the common U.S. practice—called Deny, Delay, and Defend by medical-liability experts—in which defense attorneys advise their physician client to not talk to his or her colleagues, the hospital, or the patient and his or her family.

To evaluate CRP, researchers investigate outcomes such as liability, litigation costs, and malpractice claims at CRP-implementing medical facilities. The *Health Affairs* article by Allen Kachalia, Kenneth Sands, Melinda Van Niel, and eight other coauthors (2018) arrives at findings supportive of CRP. We acknowledge the growing body of evidence about the benefits of implementing CRP, but Kachalia et al.'s analysis provides neither supporting nor contradictory evidence. If CRP is to be promoted, the research substantiating its desirability needs to be solid. The thrust of our comment is that the substantiation in Kachalia et al. is fundamentally flawed, and that the case for CRP needs to and, importantly, can be found elsewhere.

We uncovered the errors while studying the impacts of CRP on medical liability outcomes. Whereas Kachalia et al. study CRP as implemented in a Massachusetts hospital, our own involvement with CRP has been chiefly at Erlanger Hospital in Chattanooga, Tennessee. We compared the results from our Chattanooga analysis to those reported in the Kachalia et al. paper. Our detailed re-examination of the results reported in that paper and its online appendix uncovered major deficiencies in their statistical analysis; those authors' primary conclusions rested upon flawed interpretations of their data. None of their primary conclusions or interpretations can be supported after undertaking a comprehensive correction of their statistical analyses. With millions of dollars being spent developing and administering CRP practices, and several states considering legislation to facilitate CRP, e.g., Georgia House Bill 470, 2023 ([link](#)), it is crucial that the empirical evidence regarding CRP's impact on liability outcomes be reliable.

Methods

We used no external data in this reanalysis, nor did we have access to the confidential data used in the original investigators' statistical analyses. We thank the authors for providing (in private communication) corrections to their tables and other assistance as we reassessed the empirical analyses. Because of the confidential nature of their data, we were only able to assess the findings of Kachalia et al. (2018) by reexamining tables and graphs from that paper and its online appendix. Kachalia et al. studied outcomes in two academic medical centers that used CRP. To shorten the following discussion, we focus on their first CRP-implementing academic medical center, AMC-1. Few of our interpretations change when the

analysis is applied to AMC-2, as demonstrated in our online appendix.

We used two approaches to examine whether the control hospitals used by the authors have the potential to serve as adequate counterfactuals for the treated academic medical centers (hereafter simply “centers”). First, using the estimates reported in their appendix, we tested for whether the trends (slopes) in outcomes for treatment and control centers were identical prior to the introduction of the CRP. To perform the test we calculated the standard error for the difference in prior trends as the square root of the sum of the variance of the two estimators, $\sqrt{se(\text{pre} : \text{slope})_T^2 + se(\text{pre} : \text{slope})_C^2}$, where $se(\text{pre} : \text{slope})_T$ is the reported standard error for the slope before CRP implementation at a treated center and $se(\text{pre} : \text{slope})_C$ is the pre-trend standard error for the control centers. We used a standard t-test of equality of slope parameters for each of their six main liability outcomes. Second, we undertook a visual inspection of the trends for these same six outcomes from Exhibits in Kachalia et al.’s paper and its online appendix.

Kachalia et al. (2018) uncovered no significant slope changes for the comparison centers after CRP was introduced; separately, they uncovered some significant slope changes at the CRP-treated center. Given those two findings, the authors concluded that there was statistically significant evidence of improvements in liability outcome trends due to CRP. But their doing so is a classic example of a Difference in Nominal Significance (DINS) error (Allison et al. 2016; Bland and Altman 2015). The correct statistical test requires one to test simultaneously, not separately, for slope changes across the treatment and control centers.

We corrected this error by carrying out a test of the hypothesis that the two slope changes, happening after the CARE implementation date, were identical. We used a simple t-test for the equality of the slope change parameters. Similar to the analysis of pre-CRP slopes, we constructed the standard error of the difference in slope changes as $\sqrt{se(\Delta \text{slope})_T^2 + se(\Delta \text{slope})_C^2}$, where $se(\Delta \text{slope})_T$ is the reported standard error for the change in the slope after CRP implementation in a treatment medical center and $se(\Delta \text{slope})_C$ is the standard error of the trend change for the control medical centers. This approach accounts for DINS errors; it does not assume that the statistically insignificant trend changes found for the control centers are exactly equal to zero.

To construct predicted liability outcomes for the centers, we used the parameter estimates contained in Kachalia et al.’s Exhibits A10 and A12, along with a time trend variable and the mean case mix index for the corresponding center type. Our online appendix ([link](#)) contains complete details. It also describes how we infer standard errors for the differences in the predicted effects of trends and estimated upticks (phase) that took place after the time of the CRP implementation, for the treatment and control centers. That appendix additionally presents a set of

comparable results for AMC-2.

Results

The first two columns in Table 1 contain Kachalia et al.’s estimates of the pre-CRP implementation slope parameters, for AMC-1 and the control centers, for six liability outcomes. These slope parameters measure how each liability outcome had trended prior to the introduction of CRP. The differences between the treated center and the control centers in estimated slopes (column 3) are substantial, and in all cases the estimated trends for AMC-1 are lower than those estimated for the comparison centers. The t-statistics and p-values reported in the last two columns indicate that three of the six trend differences are individually statistically significant at the 5-percent level for a two-sided test. Using a Bonferroni bound for an overall five-percent level (each individually tested at the $.05/6=.0083$ significance level), one would reject the null hypothesis that none of the six pre-CRP slope coefficients are different from zero between the treatment center and the control centers. These statistically significant differential trends in liability outcomes during the pre-implementation period suggest that the set of non-implementing centers are unlikely to provide an adequate control group for assessing causal impacts of the CRP at the implementing medical centers.

TABLE 1. Comparison of liability trends in treatment and control centers before the CRP intervention

Outcomes	AMC-1 pre-slope (standard error)	Comp pre-slope (standard error)	Difference (standard error)	t-stat	2-sided p-value
New claims	-0.020 (0.0123)	-0.010 (0.0042)	-0.01 (0.0130)	-0.768	0.442
Claims receiving compensation	-0.026 (0.0188)	0.004 (0.0072)	-0.030 (0.0201)	-1.493	0.135
Compensation costs	-0.077 (0.0262)	0.004 (0.0118)	-0.081 (0.287)	-2.818	0.005
Defense costs	-0.028 (0.0216)	-0.007 (0.0071)	-0.021 (0.227)	-0.925	0.355
Total costs	-0.068 (0.0236)	0.002 (0.0104)	-0.070 (0.0258)	-2.715	0.007
Average compensation	-0.060 (0.0239)	-0.007 (.0125)	-0.053 (0.0269)	-1.968	0.049

Notes: Columns 1 and 2 display point estimates and standard errors reported in Kachalia et al.’s (2018) Exhibits A10 and A12, respectively. The last three columns are our calculations using the estimates and standard errors from the first two columns. The standard errors of the differences assume AMC-1 is independent of the control centers. Standard errors for the differences in slopes are calculated assuming estimators for AMC-1 and comparison centers are uncorrelated. We use the formula: $se \text{ of difference in trend} = \sqrt{se(AMC1)^2 + se(Comp.AMCs)^2}$.

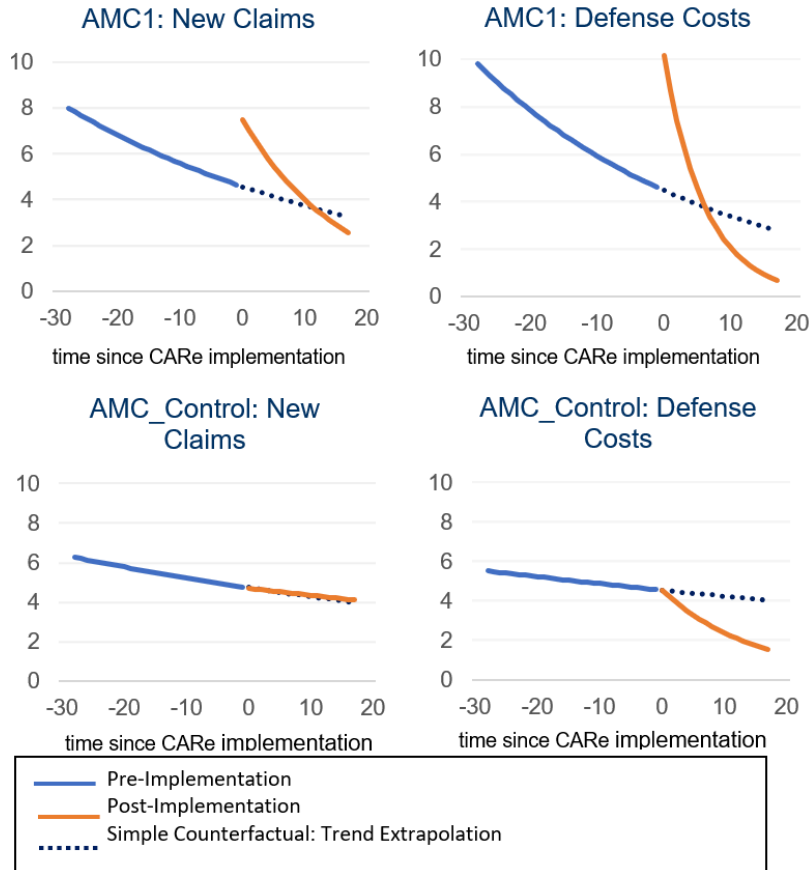
The first two columns in Table 2 contain Kachalia et al.'s estimates of how the slope parameters changed after the introduction of CRP, for AMC-1 and the control centers, for these six liability outcomes. It is the differences between these slope-change parameters that Kachalia et al. interpreted as measuring the impact of CRP on each liability outcome after controlling for time-varying confounding factors. Kachalia et al. introduced a DINS error by assuming that statistically insignificant slope changes for the control centers could be interpreted as being exactly equal to zero. After addressing this DINS issue, the t-statistics and p-values in Table 2 indicate that not one of the six differences between AMC-1 and the control centers is individually statistically significant even at the 10-percent level. Kachalia et al.'s failure to recognize DINS errors led them to exaggerated conclusions about the statistical significance of the CRP implementation's effects.

TABLE 2. Comparison of liability trends in treatment and control centers after the CRP intervention

Outcomes	AMC-1 slope change (standard error)	Comparison centers slope change (standard error)	Difference in slope change (standard error)	t-stat of difference	2-sided p-value
New claims	-0.043 (0.02766)	0.002 (0.01411)	-0.045 (0.03105)	-1.449	0.147
Claims receiving compensation	-0.025 (0.03958)	0.007 (0.02005)	-0.032 (0.044367)	-0.721	0.471
Compensation costs	-0.035 (0.05536)	-0.029 (0.03740)	-0.006 (0.06681)	-0.090	0.928
Defense costs	-0.131 (0.05648)	-0.058 (0.03018)	-0.073 (0.06404)	-1.140	0.254
Total costs	-0.046 (0.05090)	-0.032 (0.03457)	-0.014 (0.06152)	-0.228	0.820
Average compensation	-0.004 (0.05215)	-0.028 (0.04153)	0.024 (0.06667)	0.360	0.719
<i>Notes:</i> Columns 1 and 2 display point estimates and standard errors reported in Kachalia et al.'s (2018) Exhibits A10 and A12, respectively. The last three columns are our calculations using the estimates and standard errors from the first two columns. The standard errors of the differences assume AMC-1 is independent of the control centers. Standard errors for the differences in slopes are calculated assuming estimators for AMC-1 and Comparison centers are uncorrelated. We use the formula: $se \text{ of difference in trend} = \sqrt{se(AMC1)^2 + se(Comp.AMCs)^2}$.					

Figure 1 displays predicted levels of new claims and legal-defense costs using the estimates from the interrupted time series analyses used by Kachalia et al. (2018) and reported in their appendix. The blue lines are the estimated regression lines (predicted values) for claims and defense costs during the pre-CRP (pre-CARE) implementation period. The orange lines are predictions of these outcomes in the post CRP-implementation period. The blue dots are extrapolations of the trends from the pre-CRP period, constructed by assuming the slopes from the pre-CRP period did not change. The top two graphs are the predictions for AMC-1

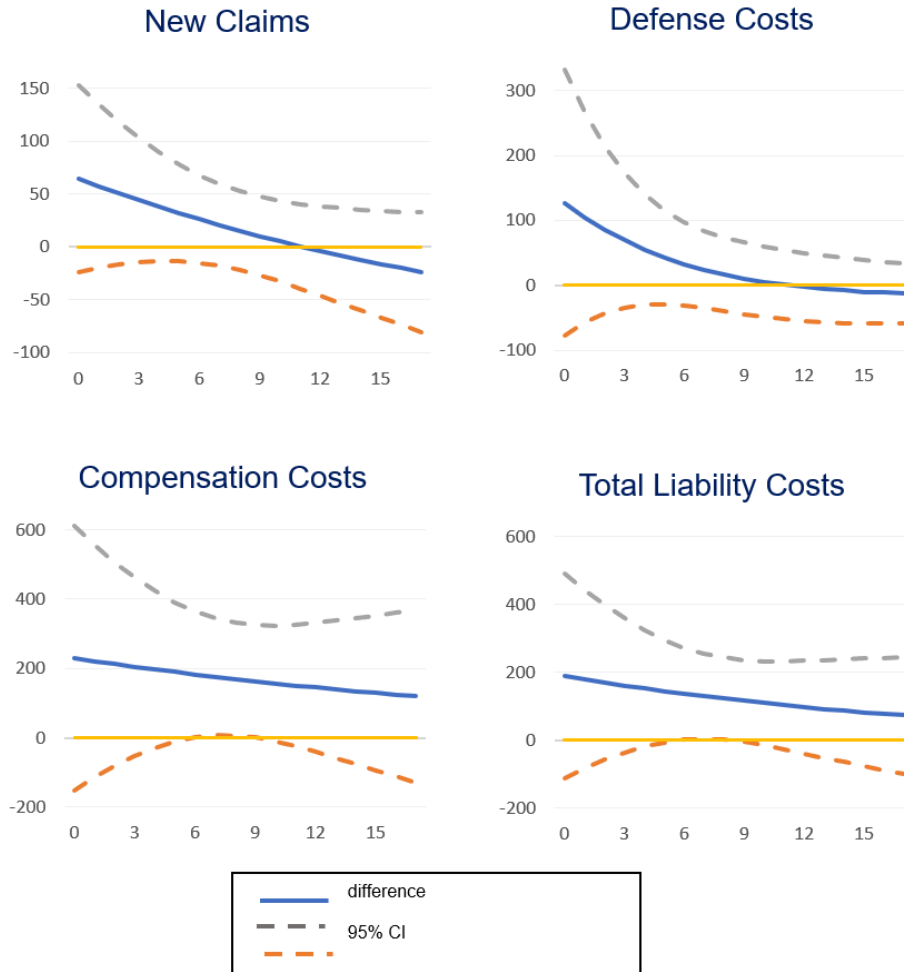
Figure 1. Reanalyzed predicted liability outcomes



Notes: Figure 1 presents the predicted values from Kachalia et al.’s interrupted time series models for two liability outcomes using the estimates reported in their paper and its online appendix. The top two graphs contain the predicted liability outcome values for the CARE treated center (AMC-1) and lower two graphs contain the predicted values of the same two liability outcomes for the set of control centers. All outcomes are measured as the rate per 100,000 clinical encounters. The solid blue line connects the predictions for the time periods prior to the implementation of CARE, and the dotted blue lines are extrapolations using the time trends estimated in pre-CARE implementation time period.

which implemented CRP in 2013, and the lower two graphs are for their group of non-implementing comparison centers over the same time periods. All are evaluated at their respective mean value of the case mix index over the observation period. The important takeaway from these graphs is the large spike in both of the liability outcomes that took place at AMC-1 as soon as CRP was implemented. The comparison centers experienced no such increases about the time CRP was

Figure 2. Comparative pre-post implementation changes in outcomes: Re-analysis of Kachalia et al. (2018) AMC-1



Notes: Y-axis is difference in percent changes; a 150, for example, could mean a 200% increase at AMC-1 vs. a 50% increase at comparisons. X-axis is quarters since CARE (CRP) implementation. Figure 2 presents estimates of the impact of the CARE CPR at AMC-1 over the post-CARE implementation period in percentage terms. It uses the estimates reported by Kachalia et al. from their interrupted time series models. Each of the four graphs corresponds to one liability outcome and the estimated impacts over the post-CARE period come from the estimates reported in their paper and its online appendix. The dotted lines provides pointwise 95% confidence intervals for those effects. These estimates compare how the predicted trends for each liability outcome changed differentially for the treated AMC-1 from the pre-CARE period to the post-CARE period, relative to the same change estimated for the group of control centers.

implemented at AMC-1. Additionally, the post-implementation decline in defense costs for the comparison centers indicates that the simple slope changes observed for the treated AMC (e.g., using the treated center as its own control) would be inadequate for assessing the impacts of the CRP.

Figure 2 illustrates the most essential element of this re-analysis. It displays how the impact of CRP on four liability outcomes at AMC-1 evolved over the four and a half years (18 quarters) following its inception in January 2013. The solid lines in this figure measure the complete effect of CRP by considering how both the levels of the outcomes and their slopes, in percentage terms, changed beginning in 2013. Figure 2 uses the estimates reported by Kachalia et al. from their interrupted time series models. Each of the four graphs corresponds to one liability outcome. The estimated impacts over the post-CRP period come from the estimates reported in their paper and its online appendix. The dotted line provides pointwise 95-percent confidence intervals for those effects. These estimates compare how the predicted trends for each liability outcome changed differentially for the treated AMC-1, from the pre-CRP period to the post-CRP period, relative to the same change estimated for the group of control centers. They adjust for common post-2012 changes in liability conditions that affected both the treated and comparison centers. Our online appendix ([link](#)) contains complete details on how we constructed the graphs in this figure.

Consider, for example, compensation costs. While the slope did become more negative, as emphasized by Kachalia et al., the complete impact of CRP implied a 200 percent increase in compensation costs (a tripling) immediately following CRP implementation at AMC-1 (relative to the changes measured for the comparison centers). Over the entire study period, the predicted compensation costs at AMC-1 (implied by their estimates) remain at least twice as large as they would have been in the absence of CRP. For none of the four outcomes in Figure 2 was there any evidence of statistically significant reductions in liability outcomes that could be attributed to CRP.

Discussion

Kachalia et al. (2018) studied the effect of CRP on liability outcomes at two centers. Their abstract states that “CRP implementation was associated with improved trends in the rate of new claims and legal defense costs at some hospitals, but it did not significantly alter trends in other outcomes. None of the hospitals experienced worsening liability trends after CRP implementation” (Kachalia et al. 2018, 1836). These conclusions would seem to provide compelling evidence that hospitals can enjoy the benefits of CRP without significant cost. Such findings

would doubtless be encouraging to the proponents of CRP and be used to support CRP implementations. We have determined, however, that the study does not provide evidence of improving liability outcomes or even no worsening of liability outcomes. We uncover less sanguine interpretations of the impacts of CRP in the settings they studied.

The evidence on the absence of comparable trends in liability outcomes between the treatment and comparison centers contained in our Table 1 strongly suggests that the comparison centers are not valid to use as a control sample. The slopes before CRP came into effect are very different between the treatment and control centers. It is, therefore, difficult to imagine why the post-CRP slope change at the comparison centers would be a suitable counterfactual for the slope change that would have taken place at the treated center had CRP not taken place.

This conclusion becomes apparent upon visual inspections of Kachalia et al.'s Exhibits 2 and 3, and online appendix Exhibits A5 and A6. For most liability outcomes displayed, the lack of agreement in slopes between the treatment and comparison centers is severe. For new malpractice claims (their Exhibit 2), the slopes for AMC-1 and the comparison centers are somewhat similar for the first part of the pre-implementation period. Over the 12 quarters immediately preceding the CRP implementation, however, the slopes diverge. For total malpractice liability claim cost rates (their Exhibit 3), there is never any similarity in the slopes during the pre-implementation period among the centers. Only for new claims (in Exhibit A5) and defense costs (in Exhibit A6) is there any semblance of parallel slopes prior to the implementation of CRP. Their set of comparison centers should not be considered as an adequate control group for uncovering causal impacts without additional justification.

Focusing on only changes in slopes, which is the main causal evidence discussed by Kachalia et al. (2018), our Table 2 uncovers no evidence of statistically significant reductions. This finding is in stark contrast with their claim of significant decreases in slopes for new claims and defense costs. For an interrupted time-series analysis with a control group to provide causal evidence, one should compare the changes in slope after implementation between the treatment and control centers. The authors do make that comparison. However, they assumed implicitly that the insignificant slope changes for the comparison group meant that the comparison group's slope change is exactly zero. Consequently, they only considered the change in slope for the treated center and its standard error.

That implicit assumption made by Kachalia et al. is known as a Difference in Nominal Significance (DINS) error, and it indicates that they used an inappropriate baseline for causal inference (Allison et al. 2016; Bland and Altman 2015). Even using conventional thresholds of significance, DINS errors can "inflate the false-positive rate from 5% to as much as 50%" (Allison et al. 2016). More simply,

DINS errors can make estimated causal effects look considerably more statistically significant than they actually are. The tests for differences in slope changes between the treatment and control centers in Table 2 properly account for the sampling variability in both the treatment and control groups. Those tests do not contain DINS errors, so the correct interpretation of the estimates Kachalia et al. reported should have been that there were no statistically significant improvements in slopes as a result of CRP.

Additionally, Kachalia et al. (2018) focused almost exclusively on changes in the trends for liability outcomes over time between treatment and control hospitals to reach their conclusion of improvements in liability outcomes post-CRP. There were, however, large increases in adverse liability outcomes at the CRP-treated hospitals immediately following the implementation of CRP. In some instances, these increases implied that estimated liability outcomes more than tripled immediately after CRP commenced. The investigators' analysis abstracted from these large and important deteriorations in liability outcomes. More importantly, after factoring in these increases, there was never a significant reduction in any of the liability outcomes we examined at the treated hospitals (relative to the control hospitals) after the introduction of CRP.

Limitations

Since we did not have access to the data used by the authors, and they were unable to perform some additional statistical analyses to help resolve issues, we were impelled to make several statistical assumptions. The measures of statistical significance in Tables 1 and 2 and Figure 2 rely upon the assumption that the samples of treatment and control centers are independent. Additionally, we could not retrieve enough information to allow us to construct accurate confidence intervals in Figure 2. We did, however, use information on how time slopes and intercepts covary in regression models to make informed guesses. See our online appendix ([link](#)) for more details.

The graphs in Kachalia et al. (2018) and their online appendix were based on a smoothing technique called LOWESS. In the presence of seasonality in liability outcomes (e.g., more claims tend to be filed in the fourth calendar-year quarter than the first quarter), a LOWESS graph can confuse seasonality and smoothing and exhibit excessive variability. The visual inspection we made relied upon their LOWESS graphs, and the extreme variability in slopes could reflect more the naive LOWESS approach and less the true slope changes over time.

Interactions with *Health Affairs*

Kachalia et al. (2018) implicitly overstated the relevance and accuracy of their results for an assessment of the impact of CRP on legal-defense costs and the rate of new claims at centers that implemented CRP. We agree with their statement that the trend coefficients for defense costs and rate of new claims “improved” at the implementing centers (Kachalia et al. 2018, 1841), but our reassessment of their data demonstrated no significant effect of CRP on the actual level of defense costs or rate of new claims when compared formally to the trends at the non-implementing centers. Their failure to assess in detail the quality of the set of centers used as controls, their primary focus on the statistical significance of *individual* coefficients, and their disregard of statistically insignificant coefficients all led to misleading conclusions.

All the evidence behind the present reassessment was available in the original article and hence to the editors and reviewers at *Health Affairs*, and two of the three major flaws we uncovered follow directly from only a moderately close examination of the paper’s tables. The review process clearly failed in this instance. Such failure does not appear to be a rare event in health research, as indicated by several other investigations (Ebrahim et. 2014; Khan et al. 2020; Ito et al. 2021).

Most importantly, *Health Affairs*’ response to the uncovering of these errors, even after the external reviewers concurred with our reanalysis, was to offer only a reduced-visibility forum for the reanalysis. Additionally, their editorial staff refused to allow us to publish in *Health Affairs* more than a brief exchange—as Letters to the Editor—with the original authors. More concerning was a senior *Health Affairs* editor telling us that their policy is to not publish replication papers even when peer review errors are identified since their audience is not interested in this type of paper. Unfortunately, burying or ignoring the uncovering of all but the most scandalous errors in published research seems to be the norm in health policy and medical journals. At the 2022 International Congress of Peer Review and Scientific Publication, four editors-in-chief of medical journals told one of us that they also did not publish replication studies. They all said they were concerned that publishing replication papers would negatively affect their impact factors. That editorial boards’ main concerns seem to be monetary or visibility-related, as opposed to improving science or potentially correcting significant errors, is deeply troubling.

Due to the attitudes of the editorial boards described above, most researchers perceive little or no reward for attempts to uncover such deficiencies in medical and healthcare papers. Medical journals, as well as academic institutions,

should strongly encourage and reward replications and re-analyses of published papers. We think of the paraphrasing of a sentiment often attributed to Mark Twain or Josh Billings: “It ain’t what you don’t know that gets you into trouble. It’s what you know for sure that just ain’t so.”

Appendix

Our online appendix ([link](#)) contains Appendix Tables A1 and A2 (comparing the pre-CARe implementation slopes and the post CARe implementation slope changes between AMC-2 and the control centers for the same six liability outcomes examined in Tables 1 and 2 in the main text—again, CARe is a form of a CRP) and an explanation of the method for calculating standard errors for differences in outcomes between treated and control centers when intercept shifts are non-zero.

Data and code

Data and code used in this research is available from the journal website ([link](#)), including:

- Tables 1,2, A1, A2 code for reading the data in the Excel file
- Kachalia et al.’s slopes and changes (Excel file)
- Figure 1 code
- Figure 2 and Figure A1 code
- Kachalia et al.’s A11 Exhibit sent to us

References

- Allison, David B., Andrew W. Brown, Brandon J. George, and Kathryn A. Kaiser.** 2016. Reproducibility: A Tragedy of Errors. *Nature* 530: 27–29. [Link](#)
- Bland, J. Martin, and Douglas G. Altman.** 2015. Best (but Oft Forgotten) Practices: Testing for Treatment Effects in Randomized Trials by Separate Analyses of Changes from Baseline in Each Group Is a Misleading Approach. *American Journal of Clinical Nutrition* 102(5): 991–994. [Link](#)
- Ebrahim, Shanil, Zahra N. Sohani, Luis Montoya, Arnav Agarwal, Kristian Thorlund, Edward J. Mills, and John P. A. Ioannidis.** 2014. Reanalyses of Randomized Clinical Trial Data. *JAMA* 312(10): 1024–1032. [Link](#)

- Ito, Chiyo, Atsushi Hashimoto, Kohei Uemura, and Koji Oba.** 2021. Misleading Reporting (Spin) in Noninferiority Randomized Clinical Trials in Oncology With Statistically Not Significant Results: A Systematic Review. *JAMA Network Open* 4(12): e2135765. [Link](#)
- 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](#)
- Khan, Muhammad Shahzeb, Maaz Shah Khan, Zunaira Navid Ansari, Tariq Jamal Siddiqi, Safi U. Khan, Irbaz Bin Riaz, Zain Ul Abideen Asad, John Mandrola, James Wason, Haider J. Warraich, Gregg W. Stone, Deepak L. Bhatt, Samir R. Kapadia, and Ankur Kalra.** 2020. Prevalence of Multiplicity and Appropriate Adjustments Among Cardiovascular Randomized Clinical Trials Published in Major Medical Journals. *JAMA Network Open* 3(4): e203082. [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.