Author's response to reviews

Title: Predicting Inpatient Hospital Payments in the United States: A Retrospective Analysis

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Author's response to reviews: see over
Reviewer 1

Major revisions

1. This paper and the earlier paper refer to payments as “price.” Health care analysts usually think in terms of a payment by sponsor to the hospital, but price is an infrequently used term, perhaps because we do not want to use a term that is used for more homogenous products in more perfect markets. Using the word payment would make this payment for easy to understand. If the authors feel that price is the correct word, it would be helpful if this paper explains exactly what is meant by a price.

> We have changed ‘price’ to ‘payment’ as suggested.

2. It would be helpful if the authors explained more carefully why it is important to know the ratio of payments (prices paid) to charges. It is clear that health care analysts would like to know payments but they are often disclosed. It is not clear why the payment to charge ratio is so important to know. Since charges are rather arbitrary, (high at some sites and low at others), the payment to charge ratio has little intrinsic value. This reviewer understands that charges are often available, and that payments are not. The paper determines how well the PCR can be predicted, but the important question is how well payments can be predicted.

> The introduction now clarifies the role of the PCR in estimating payments and why determining the accuracy of the PCR itself is essential. Because the estimated payment is simply the charge multiplied by the relevant PCR, obtaining an accurate estimated payment relies entirely on the accuracy of the charge and of the PCR.

Minor revisions

3. Page 5, line 1. Is it important to simulate PCRs? It seems that the analysts would like to know whether payments could be predicted. Given the vagaries of charge schedules, the PCR itself is not very interpretable or useful.

> The response to #2 applies here as well.

4. Page 5, line 17. The GLM model uses a log-link function. The previous paper indicated that there were negative values in the PCR, in cases where deductions exceed revenue. Negative numbers could play havoc with a model that is intended to evaluate observations with a lower bound of zero. Were negative observations excluded in this paper as well?

> We dropped encounters associated with non-positive PCR values. We added text at the end of the first paragraph of the Methods section: “Nearly all values are positive and can exceed 1.0. We retained for analysis encounters associated with PCRs greater than zero and no greater than 1.0; the other represent outliers or cannot be modeled with a logarithmic link function.”
5. Page 5, line 22. “A PCR is specific to a payer within a hospital, so the unit of analysis was the hospital.” Since there are separate observations for each payer, isn’t the unit of analysis all discharges sponsored by a single payer within a hospital?

> We have changed the sentence to state that it is the hospital-payer combination that is the unit.

6. Page 6 line 3. Not clear why this paragraph is in the methods section. It discusses endogeniety and earlier studies showing that public payment levels may affect payments levels of other sponsors. How is this information employed to develop the model? Does the method control for endogeneity bias? The cited studies seemed to contradict the conclusion that cost shifting occurs. Can the data be used to test if cost-shifting occurs? What parameter should be evaluated for evidence of this?

> That paragraph motivates why we wanted to model interactions between the 5 equations. Otherwise a reader might wonder why we used Telser’s approach rather than simply treating them as unrelated. We have added a sentence at the start of the following paragraph to link it conceptually to the selection of a modeling approach. (It is the same sentence noted in the response to comment #9 below.)

7. Page 6. Line 12. This paragraph begins with a sentence telling which method was not used. It would be better to explain the method that was used, and why. The sentence about the inapplicability of SUR has different possible interpretations and needs revision.

> We have dropped the sentence about the SUR model.

8. The explanation never states that there is one equation for each type of payer. Confusion also stems from having one equation at page 5, line 17, and not stating that it represents five equations.

> The second paragraph of the Methods section now clarifies that there are five equations: “We predicted the five PCRs in separate equations as functions of…”

9. It would be helpful to explain the problem that the modeling method (putting error terms in the first stage as regressors into the second stage model) is solving. Is the problem the potential correlation between PCR ratio between payers at the same facility? Does the analyst want to allow for the possibility that this correlation differs for each possible pair of payers?

> We have added an explanatory statement: “We wished to capture this within-hospital interaction in order to improve the accuracy of the PCR estimates.”

The answer to the reviewer’s second question is yes, but we felt that it was not necessary to separately state that the correlation could differ between any two pairs. We have not made a
statement that would imply a fixed correlation, and the description of the method (which immediately follows) would clarify that nothing is fixed.

10. Is the approach superior to a simpler model, e.g., a single regression, with multiple observations per hospital (one for each sponsor), with a fixed or random hospital-level effect? One disadvantage of the approach that was used was that was not possible to test if the effect of a specific hospital characteristic differed by payer (i.e., the interaction between sponsor and explanatory variable could have been tested for significance).

> The five-equation approach is superior to the single-equation approach because it allows the coefficients on all regressors to vary across equations. Although we could do a Chow-like test to determine whether the coefficients systematically differ in practice, this is unnecessary given our large sample sizes: the loss of efficiency from an unpooled model should not be a problem.

11. Page 7 line 8. Description of APR-DRG as measure of “loss of function” is insufficient. Isn’t the APR-DRG values of zero to four actually a measure of increasing mortality risk?

> There are two APR-DRG subclasses, one for severity of illness and one for mortality. We have updated the text and added a citation as follows: “The severity levels indicate ‘the extent of physiologic decompensation or organ system loss of function’ (Averill et al. 2003, p. 21). The levels range from 0–4, where 0 indicates no decompensation or loss of function and 4 indicates almost total decompensation or loss.”

12. p. 7 line 15. Definition of rural referral centers needs revision. Don’t rural referral centers support “low-volume” rural hospitals? If there are “high volume” rural hospitals, they are exceptional and probably don’t need a referral center to handle their cases.

> The definition of a rural referral center is quite complicated; see http://www.cms.gov/Outreach-and-Education/Medicare-Learning-Network-MLN/MLNProducts/downloads/Rural_Referral_Center_Fact_Sheet.pdf. The background statement from that online document states the general idea, though: “The RRC Program was established to support high-volume rural hospitals that treat a large number of complicated cases.” The relevant sentence in the manuscript now reads, “Rural referral centers are rural hospitals with relatively high volume that treat a large number of complicated cases and transfers from small rural hospitals.”

13. Page 9, Line 1. The source of the key variable, the payment to charge ratio, is not clearly explained. It is stated that HCUP data have payment data for 46 states. But is this the payment data that was analyzed in this study, or just irrelevant information? Is the HCUP data the source of the hospital-level averages that were used as independent variables?
> We have added sentences on the PCR source at the bottom of page 8: “PCR values. We obtained from AHRQ the PCR values developed by Levit et al. (2013). They represent the dependent variables of the regression models.” We also eliminated the statement about financial data and moved the list of states to page 7.

14. The paper then describes source of market structure and policy data. On line 10, it is stated that 10 states provided information on charges and net revenue by payer category for every hospital. Some clear description is needed of the source of the payment to charge ratio, and how this was linked to hospital characteristics.

> We have dropped that sentence. Earlier in the manuscript we note that Levit et al. (2013) describes how the PCRs were made.

15. Page 11, line 18. It might be worthwhile to note the intuitive meaning of GLM log link regression parameters when Table 3 is introduced. Are parameters are relative to a value of zero for the reference group? Are they approximately equal to a percentage difference from the reference group?

> We have added statements about these on page 12: “Each figure represents the marginal effect on the PCR of a one-percent increase in the independent variable. Figures for age group, race/ethnicity, and APR-DRG severity index are interpreted relative to the omitted categories, respectively ages 56-65, White, and APR-DRG severity level 0 or 1.”

16. The parameters for the effect of critical access hospital on PCR are provided, but without explanation of their intuitive meaning. Since this is a log-link function, does this mean that the critical access hospital parameter of .523 for Medicare patients represents (approximately) a 52% higher PCR than not being a critical access hospital?

> Yes, that’s the idea. We have added a statement to clarify the interpretation of coefficients.

17. The reader might be more interested in knowing if payments differ in hospitals that are critical access, sole provider, teaching, or rural referral. Their affect on PCR is less interesting question, because charges are so meaningless as an absolute measure. There may be good reasons to express all findings in terms of PCR, rather than payments, but this reader doesn’t understand the reasoning for this. Could charges have been an independent variable in a regression with payments as the dependent variable? We would then have much more intuitive idea of what the parameters meant.

> The purpose of the paper is methodological, to show how well PCRs can be estimated in cases (i.e., for hospitals in states) where they cannot be directly computed. For that purpose it’s enough to consider whether and perhaps what types of regressors are significant, and the overall model fit. We do not expected readers to treat it as a health services research study and therefore do not comment at length on the interpretation of the coefficients.
18. What does it mean to have a “higher PCR ratio?” Does it mean, for different sponsors at the same hospital, that they sponsor with the higher ratio is providing greater payment? Can this same interpretation be made over all?
> That interpretation is correct when you are comparing two encounters with the same charge. The same principle of comparison should apply to comparisons across hospitals.

19. Is this reviewer correct in thinking case-mix was represented by “hospital-level averages” and not “hospital averages by payer”? This seems like a limitation that should be described.
> The reviewer was correct, but we have changed it to be hospital averages by payer. The downside of this approach is a greater risk of endogeneity. Over the long run hospitals may react to PCRs by selectively attempting to build business among patients with certain conditions. This would likely affect the demographic mix. We now mention this as a limitation.

20. Table 1 is in my version of the paper twice.
> Apparently there was an error in the process of building a review copy in the journal’s online system. We will try to avoid it in the next round.

21. Table 2 should include information about the dependent variable, not just the independent variable. How many observations were there?
> Information on the dependent variable and the number of observations has been added to Table 2. The count of observations within each payer category appears at the base of Table 3.
Reviewer 2

- Major Compulsory Revisions

1. While I understand the value of obtaining prices for commercial and uninsured individuals, I'm not sure what is meant by the “price” for public payers. Can't Medicare payments by DRG, adjusted for geography, be obtained through the CMS website?

> Yes, Medicare is the one payer for which payments are publicly available without significant cost and effort. (Medicaid payments are available online from at least some states, but using them for a multistate effort is a lot of work because state coverage varies.) That the great majority of studies of community hospitals have been done on Medicare patients illustrates the old adage about searching under the lamppost. If PCRs become readily available, we expect that researchers will no longer focus so heavily on Medicare patients when studying cost.

a. As a side note (not a major revision), I believe this could help explain your negative relationship between market concentration and price for public plans, which set prices, and for which utilization would be a more important driver of cost than price.

> We agree. We added the following statement to the Correlation Between PCRs and CCRs section: It is also plausible to assume that there could be other factors beyond intense competition among hospitals, including the percent of discharges enrolled in managed care plans that can independently negotiate prices for Medicare and Medicaid, that may cause such outcomes.

> We also added the following statement to the Discussion section: Self-pay and other insurance are relatively smaller payer categories and account for a smaller fraction of overall hospital revenues. Our estimates for self-pay and other insurance categories may be less reliable than the estimates for Medicare, Medicaid and Private Insurance because of their small size and the complexity in determining these categories in many states.

b. This error is made again in Discussion paragraph 3 – studies on health care utilization use Medicare claims data, but as Medicare uses administered prices, only commercial claims datasets (which are difficult to obtain) would be used to look at prices. (Thus, this work is quite valuable, and I think should focus on commercial payers and, perhaps, the uninsured)

> That paragraph has been dropped, and material has been added to the prior paragraph.

2. The way you define “price,” as I understand your methods, you might be underpricing patients who have coordinated benefits. In other words, Medicare might pay less for a patient who has commercial insurance or vice versa, and your “prices” would in fact be payments, not the negotiated price between insurer and hospital. This might not be an
issue to you, but note that payments reflect payer and procedure mix, not the pure negotiated “price.”

> We appreciate this point. We now label them payment-to-charge ratios.

3. The endogeneity issue addressed (with regards to cost shifting) does not address a key argument usually made in the industrial organization literature, which is that hospital (patient, etc.) characteristics, especially market share, tend to be a function of the margins received in a market, not just vice versa, since hospitals will locate in high margin areas. This problem is somewhat mitigated by the inclusion of state variables, but without multiple years of data, we can’t be sure whether time shocks explained the relationship between margins and hospital characteristics, although the data were pre-recession and pre-ACA. This is especially problematic given the use of payment, rather than pure price, which is unweighted (e.g., with DRG weights), since hospitals can manipulate high versus low margin services over time and thus change their market share. Multiple years of data would have validated this method, yet anyone who has used HCUP would understand that this is no easy endeavor.

> We have added a paragraph on limitations relating to potential endogeneity in the Discussion section (pp. 15-16). Unfortunately the HCUP Market Structure File, which provides some of our independent variables, is created only every third year. This would make it hard to do a time-series analysis because interpolating the intervening years would add uncertainty and error.

- Minor Essential Revisions

4. Paragraph 1, introduction: payment and price are not always used interchangeably, e.g. if the payment is not the full negotiated price, as discussed above

> The Introduction has been revised. It no longer implies that payment and price are interchangeable.

5. Policy relevance should acknowledge more than just cost shifting as target research for this method.

> We agree. Therefore we added the following statement to the Discussion section: PCRs can help public policymakers to better understand hospitals responses to differential payment amounts for similar services across payers. For example, do hospitals decline to admit new patients with public insurance or provide these new patients with lower quality of care?

6. Methods, paragraph 3, I think you meant to write “hospitals’ margins on publicly insured patients.” Also, FYI, Robinson (2011) cites original concept and research on reverse cost shifting first proposed by Stensland, Gaumer, and Miller (2010)

> Thank you for the correction about ‘publicly’ and for the reference to Stensland et al., which we have added to the manuscript.
7. Does the HCUP have more granular market info that can be linked to discharges, e.g. MSA or county?

> HCUP does not contain hospital information at the county or MSA level.

> What about a sensitivity analysis using state fixed effects, then MSA/county fixed effects? I think at least state fixed effects are necessary to see if you’re capturing all of the state policies that might affect both margins and hospitals/patients

> We did a sensitivity analysis with state fixed effects. The methods and results are now described in the paper.

- Discretionary Revisions

8. I don’t understand the last sentence in the third paragraph in the introduction “…there can be multiple cost reports for a single hospital in one year, and extreme values can occur.”

> We rewrote the paragraph and dropped that reference.

9. Methods, second paragraph. Did you log prices because of skewness in the PCR variable, i.e. your own data?

> Yes, because of skewness in the OLS errors.

10. Table 2: I understand the need for abbreviated tables, but it might be more useful to highlight differences across payer groups rather than describe one group completely

> We have split out the descriptives by payer.