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Medical Liability Standards and Clinical Practice in the US

Comment

by

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1 Introduction

With their empirical analysis, Michael Frakes, Matthew Frank and Seth Seabury contribute to an important and comprehensive research agenda, extending the work of Frakes (2013) on the role of liability standards in medical malpractice law in the United States (U.S.).

Frakes (2013) investigates how physicians' clinical practice changes in response to alterations of a core substantive component of U.S. medical malpractice law: the standard of care expected of physicians under the law. Based on case and statutory law, he classifies 16 U.S. states as having replaced local standards by national standards between 1978 and 1999, Maryland as having abandoned national standards in 1994, and altogether 16 states as retaining some element of locality. Using a difference-in-differences identification strategy and comprehensive inpatient data on obstetric and complex cardiac care he estimates physicians' responses to the standard-of-care reforms separately for states with clinical intensity above the national average in the prereform period and for those with lower intensity. For selected obstetric and cardiac procedures his empirical results indicate convergence of clinical practice to the national average in association with state-level switches from local standards of care to national standards of care. In addition to this first finding, Frakes (2013) reports no association of changes in health-care quality, using outcome measures related to, for example, neonatal mortality.[1]

The paper by Frakes, Frank, and Seabury (2015) focuses on the first main finding of Frakes (2013), aiming at showing its robustness to using a different type of data. The chosen data provide, in particular, a count of surgeries per county and year that aggregates all kinds of inpatient and outpatient surgical interventions, as well as a count of practicing physicians. Keeping the set of standard-of-care reforms and the observation

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[1]See also Frakes and Jena (2014).

period as in Frakes (2013), the authors adapt the empirical model of Frakes (2013), as well as his identification strategy, in several respects. These adaptations seem, in parts, to be rather ad hoc changes, and I will comment on some of these.

The main estimation results in Frakes, Frank, and Seabury (2015) indicate that local surgery rates converge to the national surgery rate after state-level reforms that replace local liability standards in medical malpractice law by national standards.[2] These results, accompanied by some robustness checks, are in line with the first main finding of Frakes (2013). Taken together, the empirical evidence is consistent with the view that harmonizing liability standards in medical malpractice law can reduce regional disparity in clinical decisions of physicians.

2 Location of physicians and hospital capacity

Frakes (2013) selects a number of specific clinical procedures when estimating the impact of standard-of-care reforms on clinical outcomes, for example the number of cesarian sections normalized by the number of child deliveries. Frakes, Frank, and Seabury (2015) modify the approach by using data on surgical interventions in general. The advantage of the modification is that robustness of the earlier results can be shown for a more broadly defined outcome variable, involving data on surgeries in general, not on specific clinical procedures. This comes, however, with the drawback of complicating the choice of the normalization variable. The equivalent to, for example, the number of child deliveries would be the number of all potentially surgery-inducing incidents, summing illnesses, accidents, child deliveries, and so on. As the latter number is not observable the authors choose to normalize by the number of practicing physicians per county. This decision raises two issues that are not discussed in the paper itself.

First, in contrast to the number of child deliveries, or similar incident-specific measures, the number of physicians is likely to change endogenously in response to malpractice-law reforms. The findings reported in Frakes, Frank, and Seabury (2015) for the county-level number of surgeries per physician seem therefore to be far more difficult to interpret than, for example, the findings of Frakes (2013) for the state-level probability of cesarean section per child delivery. In that respect, the paper by Frakes, Frank, and Seabury (2015) calls for a detailed investigation of the responses of physicians at the extensive margin, namely their responses in the form of location decisions (entry and exit of practicing physicians), in addition to the responses at the intensive margin, namely in the form of the choice between intensive care involving surgeries and nonintensive care.[3]

Second, the authors calculate surgery rates that are specific to a county using only data on surgeries and physicians in the county itself. Taking that approach may, at

[2]The local surgery rate is defined as the number of all surgeries normalized by the number of practicing physicians per county and year. The national surgery rate is proxied by the average surgery rate across all counties of the union during the five year period leading up to the state's national-standard adoption.

[3]Note that stability of the main findings when using a measure of surgeries per capita, not per physician, is mentioned in footnote 5.

least in part, generate the observed variation of the main convergence pattern across the different subsamples of geographical areas that are considered in Table 3.[4] Why? Because counties need to have a sufficiently high hospital capacity to be included in the estimation sample and the applied restriction is far more likely to eliminate rural counties than more densely populated counties.[5] The resulting subset of rural counties in the estimation sample is small and likely to be selective as rural counties with sufficiently high hospital capacity are likely to attract a large share of the patients that are referred to an out-of-county hospital by physicians in surrounding rural counties that are not sampled due to insufficient hospital capacity. The physicians practicing in these nonsampled counties are neglected when normalizing the number of surgeries in the sampled counties, and this can well contribute to the observation of the more pronounced convergence pattern in the sampled rural counties. Accordingly, it would be good to see how the empirical results of Table 3 change on taking an alternative approach. The county-level data could, for example, be aggregated to a regional level that is sufficient to eliminate the occurrence of regions that have insufficient hospital capacity for getting sampled.

Having mentioned the hospital-related sample restriction, let me add a few further remarks. Hospital capacity is likely to change endogenously in response to malpractice-law reforms, just as the practicing of physicians does. If indeed hospital capacity changes endogenously, the main coefficient estimates for a sample restricted by a hospital-related exclusion restriction will not capture the aimed-at average treatment effects on the treated (ATT), but the ATTs plus possibly large selection bias terms.[6] Accordingly, the estimation results in column 1 of Table 5 are important. These indicate robustness of the main convergence pattern to abandoning the hospital-related sample restriction in two out of three model specifications. In addition, it is equally important that the authors can show robustness in column 3 of Table 2. There they use a model specification without the hospital-related, and thus potentially endogenous, variable that is part of their preferred set of explanatory variables. In line with Angrist and Pischke (2009), both these modifications should, however, be applied at the same time. Therefore, it would be useful to also report the estimates of the model specification without the hospital-related explanatory variable for the unrestricted sample.

3 Multiple regulatory interventions

In the last part of their paper, Frakes, Frank, and Seabury (2015) pick up the concern that their effect estimates could reflect not only responses to the reforms of medical liability standards, but also to a different type of reform, namely the introduction of the

[4]The estimated coefficients for the sub-sample of rural counties in column 1 of Table 3 are more than twice as large as the corresponding ones for any of the other population-density-specific subsamples that are used in Table 3. Counties are defined as rural if the population density is below the 25^{th} percentile of the respective sample distribution.

[5]Counties are eliminated from the estimation sample if less than 50 hospital beds are available on average across years (see also footnote 9 and Table 3).

[6]See chapter 3.2.3 of Angrist and Pischke (2009) for details.

Medicare Prospective Payment System (PPS) in October 1983. The 1983 PPS reform changed hospital reimbursement for Medicare inpatient expenses from a full-cost to a partial-cost system by introducing a fixed, diagnosis-based reimbursement per Medicare patient in the case of operating costs, but not capital costs. The concern is a relevant one, in my view, for the following two reasons. First, many states implemented their reforms of medical liability standards in the years around the introduction of the PPS in 1983 (7 of 17 states in the years 1981 to 1985), and most states underwent the reforms in the period when PPS treated operating and capital costs differently (11 of 17 states in the years 1983 to 2001). Second, Acemoglu and Finkelstein (2008) have shown that the 1983 PPS reform increased capital-labor ratios substantially and encouraged the adoption of new intensive-care technologies, and more so in hospitals with a higher share of Medicare patients in 1983.

The authors report robustness of their main findings to eliminating all observations between 1982 and 1985. This sample reduction does, however, not deal with potential long-term responses to the 1983 PPS reform and the differential treatment of operating and capital costs until 2001.[7] Accordingly, I would like to suggest estimating empirical models that are designed to precisely identify whether and to what extent both types of reforms lead to co-occurring responses in line with expectations. Specifically, do the responses to the 1983 PPS reform add to the responses to the reforms of medical liability standards in counties with low prereform treatment intensity, and counteract the responses in counties with high intensity? Is this pattern more pronounced in counties with a higher prereform share of Medicare patients? In my view, further research that takes regulatory changes to the hospital reimbursement system into account, along with malpractice-law reforms, would be highly interesting.

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[7] Acemoglu and Finkelstein (2008) found, for example, the increase in the capital-labor ratio associated with the 1983 PPS reform to grow across all the postreform years until 1986, the end of their observation period.

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