

“‘These Boots Are Made for Walking’”: A Reply

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Erik Craft's comment on our 2000 article takes up a minor point, the impact of no-fault divorce on the gender of the spouse filing for divorce. In the original article, we related the gender of the filing to rent exploitation during marriage, rent appropriation through divorce, and particularly child custody. We tested the hypotheses we generated using a sample of more than 46,000 divorce decrees from the *only* four states collecting all the information we needed. The type of divorce ground was only a control variable, and not a strong one. We argue that Craft's comment misses our essential point.

Erik Craft has written a comment on our 2000 article (Brinig and Allen, 2000), in which we try to explain why so many women file for divorce when, on the surface, the financial costs seem stacked against them. Our article has received a great deal of attention both inside and outside academic circles. This is no doubt attributed in part to the fact that, given the financial and social hardships faced by wives after divorce, most academics have simply assumed that husbands would naturally be the ones who would instigate divorce. It was assumed that the well-known higher filing rates for wives was an aberration or was merely a convention without any economic meaning.

Our article goes through a number of explanations for why an individual might file for divorce. Essentially, parties might file because they are trying to take marital wealth with them, or because they are escaping

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exploitation within the marriage. We called these two cases “rent appropriation through divorce” and “rent exploitation during marriage.” In addition to this, we noted the importance of children as marital assets and sources of power, underscoring the importance they play during and after divorce. Finally, we noted that nothing could be concluded regarding the welfare of divorced parties without knowing the reasons for the divorce.

Using these theories, we developed testable hypotheses based on age, age at marriage, children, human capital, race, and legal factors. We then sought out data in order to determine which factors were important in explaining the filing rate of wives. As it turned out, *only* four states collected information on who filed for divorce and included custody information. This data amounted to information on over 46,000 individuals. Using this data, we found evidence that both men and women file for divorce in order to appropriate assets. We also found evidence that individuals file for divorce when they are being exploited in a marriage. The finding of most significance (statistical, coefficient size, and political interest), though, was that the spouse anticipating child custody was most likely to file. Since women more often than men receive custody, this explained most of the difference in filing behavior. To miss this point is to miss the major finding of the work.

Craft’s comment, in focusing in on a minor aspect of the article (basically, on a sentence in the conclusion), seems to miss the point of what we were attempting to explain and what we found. Craft is concerned about the fact that we use a control variable called “no-fault,” which is intended to control for the two states that have “no-fault” divorce, since in these two states transaction costs for obtaining a divorce should be lower. His concern is that we have only four states and one of these (Connecticut) is very small. As a result, the coefficient has a problem of interpretation (it may only be a crude measure of regional difference) and is likely to suffer from being biased. Craft’s major point is simply that we require “either some panel data set . . . or a cross-section data set with more regional variation” (Craft, 2002, p. 374).

In one respect Craft’s point is well taken. Unfortunately, though, to read this comment and not the article itself, one might get the impression that we were concerned only with the effect of no-fault divorce on the rate of women filing for divorce. The comment states the following: “Brinig and Allen *argue* that no-fault divorce law has raised the percentage of all

divorces filed by women. . . .” and “Brinig and Allen seek to differentiate among the preceding alternative divorce motivations in explaining divorce filings. *In particular*, do no-fault divorce laws encourage any of the above three motivations more than the others? One *key* element in this story is how no-fault divorce laws change the percentage of all divorces filed by women” (pp. 371–72; emphasis added). The comment then proceeds to discuss only this tangential point in our article.

Even a casual reading of our article shows that we make no argument for the no-fault coefficient in our regression. Our model makes no prediction on the sign of this coefficient. Nor is the effect of no-fault divorce the particular or key thing we wish to study. This variable was simply a control variable, and one that we acknowledge as a weak one. In fact, we actually say on pages 149–150 that “despite its importance, we can make no prediction on the sign of this [the no-fault] variable.”

As a matter of record, we happen to find that women increased their filing in no-fault states, we say this is interesting in its own right, but we immediately note that “it must be kept in mind that we are only testing this using four states, none of which have fault laws that resemble the standard fault laws of a generation ago, since even fault-retaining states such as Virginia and Connecticut allow divorce after no-fault separation periods.” “Furthermore,” we note, “none of these states (and, in fact, no modern states) base property division on title, in which most of the transfer of wealth took place” (p. 150, n. 40). Craft seems to be focused on a sentence from our concluding remarks, where we state “[t]he legal ramifications of the no-fault variable are perhaps the most interesting” (p. 158). In this we did not mean the no-fault variable was statistically reliable or an implication of our model, only that we found the outcome interesting from a policy point of view. To take this remark and suggest it is a hidden subtext or that it is the focus of the work is to completely take it out of context.

To repeat, we included the no-fault variable (along with others) simply as a control, fully recognizing that in the current legal context it represents only a marginal difference in law (see p. 150, n. 40). However, the Craft comment is almost exclusively concerned about this variable, and the fact that it may not be robust. The author states, “This may be a spurious interpretation” (p. 372): “the no-fault variable could just as easily be capturing regional heterogeneity” (p. 372) or “approximates perhaps

nothing more than a dummy variable for states (Oregon and Montana, in this case) other than Virginia” (p. 373). The bulk of the comment simply resurrects an old debate between Allen (1992) and Peters (1986) over the merits of including regional variables in a cross-sectional analysis. This point is simply moot because the author is ignoring the fact that we use no-fault only as a control variable. Had the focus of the article been on the effect of no-fault divorce on filing rates, we would have sought out a sample larger than four and we would have used “either some panel data set (ideally including different policy regimes in the same state during different years as in Friedberg [1998]) or a cross-section data set with more regional variation” (p. 374). Since the focus of the work was on *individual* differences and how they influence marriage rents, we feel the sample of 46,000 individuals was adequate.

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