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Interpreting Data: A Reply to Professor Pardo

John A. E. Pottow
University of Michigan Law School, pottow@umich.edu
Robert M. Lawless
University of Illinois - College of Law
Angela K. Littwin
University of Texas School of Law
Katherine M. Porter
University of Iowa College of Law
Deborah K. Thorne
Ohio University

See next page for additional authors

Interpreting Data: A Reply to Professor Pardo

by

*Robert M. Lawless, Angela K. Littwin, Katherine M. Porter, John A. E. Pottow, Deborah K. Thorne, and Elizabeth Warren**

Professor Pardo has published a pointed critique[1] to our Report,[2] raising three major complaints. First, he claims that we make two predicating assumptions in our study that are flawed.[3] Second, he contends that we misunderstand the means test and fail to appreciate with sufficient "nuance" its "operative effect."[4] Third, he maintains that our Report suffers from methodological problems.[5]

We can address the two impugned assumptions quickly. The first one - that BAPCPA's means test is the sole causal agent driving 800,000 putative filers from the bankruptcy courts[6] - is not one we make. The second - regarding the income profiles of the missing 800,000 bankruptcy filers[7] - is actually somewhat consistent with predictions Professor Pardo himself makes

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*Professor Pottow is corresponding author for this manuscript. The co-authors' names appear in alphabetical order, however, to reflect the significant contribution made by each in various ways. The co-authors' institutional affiliations follow: Robert M. Lawless, Professor of Law and Galowich-Huizenga Faculty Scholar, University of Illinois College of Law (rlawless@illinois.edu); Angela K. Littwin, Assistant Professor of Law, University of Texas School of Law (alittwin@law.utexas.edu); Katherine M. Porter, Associate Professor of Law, University of Iowa College of Law (katie-porter@uiowa.edu); John A. E. Pottow, Professor of Law, University of Michigan Law School (pottow@umich.edu); Deborah K. Thorne, Assistant Professor of Sociology, Ohio University (thorned@ohio.edu); Elizabeth Warren, Leo Gottlieb Professor of Law, Harvard Law School (cbateson@law.harvard.edu). Acknowledgements of our co-principal investigators and other helpful colleagues, advisers and assistants appear at the outset of the initial Report.

[1]Rafael I. Pardo, *Failing to Answer Whether Bankruptcy Reform Failed: A Critique of the First Report from the 2007 Consumer Bankruptcy Project*, 83 AM. BANKR. L.J. 27 (2009).

[2]Robert M. Lawless, Angela K. Littwin, Katherine M. Porter, John A. E. Pottow, Deborah K. Thorne, & Elizabeth Warren, *Did Bankruptcy Reform Fail? An Empirical Study of Consumer Debtors*, 82 AM. BANKR. L.J. 349 (2008) [hereinafter "*Did Bankruptcy Reform Fail?*"].

[3]Pardo, *supra* note 1, at 28 (faulting two "questionable" assumptions).

[4]*E.g.*, *id.* at 33-34 (lamenting that there is "a need for nuance that is missing from the First Report" and offering a distinction between the "purpose" and "operative effect" of the means test).

[5]*Id.* at 29 (claiming the existence of "several methodological deficiencies").

[6]*Id.* at 30 ("First, the Report assumes that but for BAPCPA's enactment, there would have been slightly more than 1.6 million bankruptcy filings in 2007.").

[7]*Id.* at 31 "(Without this assumption [of similar income], no . . . inference can be made [regarding bankrupt debtors' income profiles].").

elsewhere in his critique.[8] The thrust of Professor Pardo's commentary, however, is his second point - that we simply "don't get" the means test - and so we begin our response by addressing this contention. We then discuss our methodology, which we believe is quite robust, before finally elaborating on why we are sanguine in dismissing his complaints with the two assumptions he claims we make.

I. BAPCPA'S MEANS TEST

Professor Pardo claims that we misconceive the true role and function of the means test and, as such, embark upon a fool's errand with our Report.[9] We begin by repeating our understanding of the means test and then present Professor Pardo's very different one.

We believe that BAPCPA's proponents designed the means test to deter high-income ("can pay") debtors from filing for bankruptcy relief under chapter 7 by subjecting them to an onerous statutory eligibility screen.[10] The intended effect is either to force these debtors to file for relief under chapter 13 or to forgo bankruptcy relief altogether.[11] We test that presumption and discover that it is inconsistent with our data.[12]

Professor Pardo chides us for lacking "nuance" in this description of the means test.[13] He instead bifurcates what he calls the "express purpose" of the means test from the "implied purpose."[14] (He then expands his taxonomy to encompass the "operative effect" of the means test - which implements its *express* purpose - and the "deterrent effect," which merely implements its *implied* purpose.)[15] Our description, Professor Pardo complains, merely captures the implied, deterrent purpose of the means test: to deter high-income people from filing for chapter 7. But we miss the (presumably more important) *express* purpose: means-testing people who do in fact file for chapter 7 and then dismissing or converting the cases of the subset who flunk the screen. In his words:

> [T]he express purpose of the means test *was not deterrence* but rather the screening of a particular subset of the con-

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[8]*Id.* at n.21, discussed *infra* at text accompanying note 70.

[9]*Id.* at 32 ("To understand the flaws in the First Report's research question and methodology that preclude it from answering whether bankruptcy reform failed, one must recognize that the Report does not adequately account for the purpose of the means test or the complexity of its provision.").

[10]*Did Bankruptcy Reform Fail?*, *supra* note 2, at 352-53, 356-57.

[11]*Id.*

[12]*Id.* at 363.

[13]Pardo, *supra* note 1, at 28 (bemoaning "[in]adequate account," "incomplete analysis" and "missing" "nuance" of our discussion of the means test); *id.* at 34-35 (lamenting "broad generalizations" that do not "delve into the complexity of the means-test formula" and thus are "quite unfortunate").

[14]*Id.* at 32.

[15]*Id.* at 34, 38 n.47.

> sumer bankruptcy population - that is, the above-median debtors who have filed for Chapter 7 relief. In other words, the means test is meant to affect the *filing* population rather than *the nonfiling* population.[16]

This divergence of conception of the means test - ours focused on its upstream deterrent effects and Professor Pardo's on its intra-bankruptcy "operative" effects - leads to a difference of opinion regarding what we should be looking at to gauge its efficacy. We believe that we should look at whether people are indeed deterred from filing for chapter 7 (or bankruptcy relief more generally) after BAPCPA, which Professor Pardo scolds as focusing "exclusively on the deterrent effect of the means test."[17] By contrast, Professor Pardo believes we should be looking at how the means test is working: how many cases are converting/dismissing debtors' petitions under chapter 7 when they have incomes that are too high. He admonishes that "any evaluation of whether the means test has been a success would, at a minimum, need to consider its effects on the filing population that it has targeted - specifically, *by examining the dismissal and conversion of Chapter 7 cases* under the abuse dismissal framework and ascertaining whether that group of cases statistically significantly differs in a substantively meaningful way from those cases that remain in Chapter 7."[18]

We hold to our understanding of the means test and the concomitant research questions it generates. With respect, Professor Pardo's alternative strikes us as naïve because it assumes that all debtors (and their lawyers) simply file their cases and *then* wait to see if they pass the means test. We think nothing of the sort occurs. Rather, debtors who know they will flunk the means test simply are advised not to file—and do not file—in chapter 7. It is whimsical to suggest that they do file, presumably checking the box to indicate that they flunk the means test, and then wait for their dismissal (perhaps hoping they'll slip through the cracks?). Of the 1,556 chapter 7 filers in our study, exactly three of them (0.2%) filed a chapter 7 petition that on its face failed the means test.[19] Even Professor Pardo eventually concedes, albeit only in a footnote, that the law has *ex ante* incentive effects that might deter such pointless conduct, but he sees that as a sidebar: "In all likelihood, [the means test] does have some deterrent effect on some debtors . . . . That said, the statutory language of the means test clearly operationalizes an aspect of case administration . . . ."[20] And herein lies the crux of the disagreement: in

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[16]*Id.* at 33 (emphasis added).

[17]*Id.*

[18]*Id.* (emphasis added).

[19]These numbers are based on calculations done for purposes of this reply.

[20]Pardo, *supra* note 1, at 38 n.47.

our view, deterring high-income filers from chapter 7 was not a secondary "implied" goal of BAPCPA; it was BAPCPA's animating purpose.

To illustrate the significance of our conceptual difference, we think a hypothetical analogy might be helpful. Imagine (leaving aside constitutional problems) a BAPCPA-like reform called the "Litigation Masculinization Reform Act" that requires any civil complaint filed by a female plaintiff to be dismissed. Professor Pardo would say that the "operative effect" of this law is limited to the universe of people who still file lawsuits after its enactment (one presumes overwhelmingly male). Accordingly, he would insist that academic studies analyzing the new law should look at how well dismissal motions cull women. He might then acknowledge that a secondary, "deterrent" effect would be dissuading women from filing lawsuits in the first place but he would relegate that as the statute's "implied" purpose at best, not its "express" one. By contrast, we would say that the whole purpose of such a law would be to deter women from filing lawsuits and to exclude them from the courts. Scholarly analysis in our view should therefore look at how many women filed lawsuits before and after the statute to understand how many women were excluded - not simply look at how many women who *did* file complaints subsequent to enactment had their actions dismissed. (Indeed, it is not clear to us what we would be looking for in such an inquiry; absent irregularity, we would expect the dismissal rate of such women to approach 100%.) The purpose of such a law is clearly deterrent. That the *mechanism* for so deterring is automatic dismissal of female plaintiffs' complaints is merely the mode of its procedural execution.

Accordingly, rather than apologize for our purportedly "un-nuanced" understanding of the means test, we stand by it. We think we accurately comprehend what the years of legislative squabbling were all about and were consequently correct to focus on deterrence in framing our research questions.[21]

II. OUR METHODOLOGY

Professor Pardo argues in the alternative that even if we are right in our deterrence-based understanding of the means test and the attendant questions to ask, our execution is flawed.[22] He makes two major methodological complaints: first, that a potential sample bias prohibits us from properly com-

[21]We remind readers that the Report explores BAPCPA on the terms framed by its proponents. We accepted at face value the public claims of the means test's champions that it would "screen out the can-pays and high-income crowd who were purportedly abusing the system." *Did Bankruptcy Reform Fail?*, *supra* note 2, at 351-52. As we emphasize in our discussion of and agreement with Professor James J. White's insightful work, we also do not doubt that the sophisticated consumer lenders who lobbied for BAPCPA's passage had unstated private motives to push for the means test. *See id.* at 380-81.

[22]*See* Pardo, *supra* note 1, at 29.

paring our new 2007 data with our previous 2001 data;[23] and second, that the metric we used for comparison - gross income - is inapposite in light of the means test's ultimate consideration of disposable income.[24]

A. Representativeness of CBP Sample

In 2007, thanks to improved online access and the generosity of AACER,[25] we drew a nationwide random sample from all judicial districts. By contrast, in 2001, we physically traveled (or found local help) and sampled from only five judicial districts.[26] Professor Pardo articulates what statisticians would call a "sample bias" concern regarding this approach.[27] It goes like this: the debtors in 2007 were a true random sample from all jurisdictions but the debtors in 2001 were drawn from only five jurisdictions. If those five places were not representative of the nation as a whole, then when we compared the 2001 data with the national sample of 2007, any differences or similarities could result from the change in our sampling method.

To be clear: the problem is *not* with taking a sample from five places per se. If those five jurisdictions are representative of the broader population of cases, it is appropriate and reliable to draw inferences from the sample regarding the population. But if those five are somehow *atypical along dimensions potentially relevant to our analysis*, then the 2001 sample should not be compared to a national sample. For example, if the five states we had chosen in 2001 were Hawaii, California, Alaska, New York, and New Jersey, we would have selected the states that have the five highest costs of living in the United States.[28] Perhaps then the bankrupt debtors in those places were not reflective of the average bankrupt debtor nationally and thus would skew our findings (for example, by having disproportionately higher expenses on Schedule J in chapter 13).

Professor Pardo suggests a concern along these very lines - that the five states we picked in 2001 might be unusual in terms of their income profiles.[29] What if, as he suggests, the 2001 sample came from all low-income states? That would make our 2001 income numbers artificially low and not reflective of the nation as a whole. Thus, we might have not detected an actual

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[23]*See id.* at 38.

[24]*See id.* at 41.

[25]The Automated Access to Court Electronic Records. *See* AACER, http://AACER.com (last visited February 2, 2009).

[26]*See Did Bankruptcy Reform Fail?*, *supra* note 2, at 389-90.

[27]Pardo, *supra* note 1, at 39-40.

[28]*See* Missouri Economic Research and Information Center, "Cost of Living Data Series, Third Quarter 2008," http://www.missourieconomy.org/indicators/cost_of_living/index.stm. (last visited February 1, 2009).

[29]Pardo, *supra* note 1, at 39-40.

change in filers' incomes after BAPCPA (meaning, in turn, that BAPCPA's means test was more effective than we give it credit).

In his own words:

> First, the Report fails to address the implications of comparing a nationwide random sample of bankruptcy cases drawn solely from federal judicial districts in California, Illinois, Pennsylvania, Tennessee, and Texas (i.e., the 2001 CBP). That the 2001 CBP sample is not necessarily representative of debtors nationwide, a point expressly acknowledged by the CBP, is problematic. For example, according to the Census 2000, the median national household incomes in 1999 of Pennsylvania, Tennessee, and Texas were below the median household income of the entire United States. If this state of affairs remained unchanged as of 2001, it would mean that 3 out of the 5 states from which bankruptcy cases were drawn for the 2001 CBP were from states with median household incomes below the national median. If the income from the 2001 CBP were skewed toward the lower end of the income scale, the showing that the income data from 2007 CBP was indistinguishable from the 2001 CBP would not establish that the means test had failed to deter high-income debtors from filing for Chapter 7 relief (and, in fact, would possibly suggest the opposite).[30]

Framed thus, it appears Professor Pardo has a legitimate sample bias concern, and one that may well have an impact on the inferences we draw in our article. After all, if three of the five states have disproportionately low incomes in the general population (and we assume that the bankruptcy filers have similarly disproportionately low incomes matching their state trends), perhaps CBP 2001 picked unrepresentatively low-income states.

What Professor Pardo elects not to mention in this critique, however, is that the other two states in the CBP 2001 sample - California and Illinois - had disproportionately *high* incomes in the very same Census. Professor Pardo highlights only the three states with low incomes and creates the impression that we should have concerns with a low-income skew sample bias, while he omits the full picture: three states have low incomes and two have high incomes. With this more accurate presentation, we do not see the skew

[30] *Id.* (footnotes omitted). We should clarify Professor Pardo's reference to our other work. What we "expressly acknowledge" is that a five-jurisdiction sample, *a fortiori*, is "not necessarily" a representative sample, but that of course is a tautology. We do not concede this is "problematic." On the contrary, our prior research is in fact based upon the representativeness of that sample, which as we mention in the text, is empirically sound.

concern. On the contrary, we are heartened that this implies the five states indeed were collectively representative for a set of five states (three low, two high). Because Professor Pardo's ultimate admission that the two other states have higher incomes is confined to a footnote,[31] we believe he has not been entirely forthcoming in his critique of our sample methods. Surely the reader consuming the text only, which mentions the three low-income states, but not the footnote that reveals the other two states as high-income, might get the impression of a possible low-income skew.

Aside from the selective data presentation issue, there is an even greater problem with Professor Pardo's purported concern over sample bias. Being prudent empiricists, *we had already conducted sample bias checks* in analyzing our 2001 data to see if they compared with the 2007 data. They did. And we referenced these tests our article.[32] To be sure, we could have been clearer and more detailed in our mention of what was done.[33] On the other hand, we think we had a reasonable fear of driving away our audience by belaboring arcane methodological points of "non-findings" in a journal whose readers are not primarily empirical researchers. Indeed, part of our decision not to bother reporting our "non-findings" of no bias was that we had no theory why there would be a bias in the first place, raising an epistemologically intriguing question of what unimagined concerns we need to say we already knew about. In any event, for readers who are curious, we reran the numbers from the 2007 filers using just the debtors from the five states that constituted the 2001 CBP sample rather than the full nationwide sample to see if any of our results came out differently. None did. (In fact, one came out slightly stronger for our conclusions, but the overall point was so small that we elected not to mention this in the article.) As we mentioned in the Report, and now repeat for added clarity, it is methodologically sound to compare the 2001 five-jurisdiction CBP sample with the 2007 nationwide CBP sample.

B. Gross Income Metric

The second major complaint Professor Pardo has with our methods is the decision to use gross income as the metric for gauging the implementation of the means test.[34] In his opinion, we should have used the new BAPCPA concept of "disposable income" - the debtor's remaining income after the sec-

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[31] *Id.* at n.54 (citing Census 2000 data).

[32] *Did Bankruptcy Reform Fail?*, *supra* note 2, at 355 n.32 ("Our prior studies were not random national samples but random samples drawn from five judicial districts. We have no reason to believe this affects the comparisons we make to earlier cohorts of bankruptcy filers.").

[33] Professor Pardo faults us for this brevity. *See* Pardo, *supra* note 1, at 40 & n.56.

[34] *See id.* at 41.

tion 707(b)-allowed deductions under the means test.[35] Professor Pardo's critique is again quite pointed, rejecting our use of gross income as "unsuitable"[36] and "odd."[37] He opines: "Thus, [gross] income by itself is too broad of a metric for ascertaining what deterrent effect, if any, the means test has had in deterring individuals from filing for Chapter 7."[38] We disagree on both normative and methodological grounds.

Why is Professor Pardo so dismissive of our metric? Again, it comes down to different understandings of the means test. The means test has two parts: the gross income screen[39] and the disposable income screen.[40] The gross income screen lets below-median income debtors avoid the complex statutory test of permissible deductions to determine chapter 7 eligibility; they pass the means test as a matter of law.[41] Above-median income debtors face a different statutory burden. They can be eligible for chapter 7 too, but only after they have run the means test gauntlet in its full detail. If their permissible expense deductions leave them with little enough "disposable income," then - but only then - they pass and escape the presumption of abuse.[42]

Professor Pardo appears to believe that the means test matters only to those who flunk it. That is, for above-median income debtors who face the means test but who ultimately pass, itemizing their expenses in fine detail for creditor and judicial review was no big deal - nothing more than a passing inconvenience of insignificant moment: "High-income debtors need not worry about the means test provided that they have a level of disposable income that is insufficient to trigger the presumption of abuse under the means test."[43]

We disagree strenuously. The means test gauntlet subjects debtors to pervasive judicial and creditor scrutiny and opens the door to objections whose resolutions can render a debtor ineligible for chapter 7. None of us to date has met a practitioner who would be willing to tell an above-median income client seeking chapter 7 relief "not to worry" about the means test because all the client's deductions are ironclad and will lead to a qualifyingly low disposable income.[44] Professor Pardo, by contrast, considers the possibil-

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[35]*Id.* at 42.

[36]*Id.* at 41.

[37]*Id.* at 29.

[38]*Id.* at 41.

[39]11 U.S.C. §§ 707(b)(6), 707(b)(7) (2008).

[40]11 U.S.C. § 707(b)(2) (2008).

[41]11 U.S.C. §§ 707(b)(6), 707(b)(7) (2008).

[42]11 U.S.C. § 707(b)(2) (2008).

[43]Pardo, *supra* note 1, at 41; *see also id.* at 37 ("[T]he concern for an above-median debtor considering Chapter 7 relief is not one regarding total income but rather one regarding disposable income.").

[44]An attorney who files a chapter 7 case that is later determined to be ineligible faces the possibility of

ity that above-median debtors will be perturbed by this heightened judicial and creditor scrutiny "highly unrealistic."[45] Suffice it to say our sense of realism under the means test differs from his; perhaps he has a stronger belief in the legal clarity of BAPCPA's deductions, the ease with which the expenses can be calculated and verified, or the fortitude of debtor nerves. Thus, we maintain it is entirely appropriate to use gross income as a metric to gauge the means test's impact on normative grounds.

There is an equally important methodological reason that we stand by our decision. Because there was no such thing as "disposable income" in the means test sense before BAPCPA, we do not have any 2001 data that are directly comparable with the 2007 disposable income data. Professor Pardo suggests that we could, in his words, "engineer" variables such as *hypothetical* disposable income from 2001 using, *inter alia*, Schedule J expenses.[46] "While this measure of disposable income would not be perfectly congruent with the measure of disposable income set forth in the means test, it would nonetheless be a better metric for evaluating debtor behavior in response to the means test than [gross] income generally."[47]

We applaud Professor Pardo's motivations in trying to construct the most apposite variable. The problem, however, with this idea is endogeneity. Here is the conundrum: if a researcher attempts to work out people's food expenses, car expenses, secured debt, etc., per the means test standards by plucking numbers from Schedule Js in the 2001 cases, it will be necessary to assume that if those people had filed under a BAPCPA regime their reported expenses would have been identical. But we cannot know that. With BAPCPA in place, debtors who know that some expenses will be permissible under the means test have an incentive to shift their discretionary expenses into the permissible categories and shade ambiguous expenses that could fall under multiple plausible classifications as falling under means-test-eligible deductions. A synthetic means-test "disposable income" variable constructed from 2001 cases cannot tell us reliably how those same debtors would have reported (and even incurred) expenses in 2007. This challenge brings us to an important point regarding the conduct of empirical research: investigators must make judgment calls based on logical inference and common sense. Guided by caution, we concluded that we could not be safe in constructing 2001 hypothetical means-test disposable income and so empirical prudence

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sanctions and personal liability. 11 U.S.C. §§ 707(b)(4)(A) & (B) (2008). The almost inescapable conclusion is that Congress wanted attorneys to use caution when filing Chapter 7 cases, and the clearest way for attorneys to exercise caution in this matter is to think twice when the debtor's income is above-median.

[45]Pardo, *supra* note 1, at 44.

[46]*Id.* at 42 n.68.

[47]*Id.*

required staying with the more objective and consistent gross income measure.[48]

Finally with regard to our methodology, we note that Professor Pardo raises some other quibbles.[49] We respond only to the two major ones in this reply, but we do not concede his criticisms on the other points. We will offer by way of example our reaction to one of his additional complaints. At one juncture, Professor Pardo faults us for not excluding certain business cases from our analysis: "In conducting its statistical analysis of differences in income levels, the Report did not exclude cases filed by individuals whose debts consisted of primarily business debts [and hence do not face the means test]. . . . It may be possible that inclusion would not skew the Report's results, but the Report does not address the issue *and thus fails to assuage concerns* on this front."[50]

This is an example of a criticism that sounds like it may raise something important but, upon further reflection, does not. Indeed, Professor Pardo carefully does not say that this omission skews our data, only that it *may* do so - although this potential raises "concern" that in his mind demands "assuagance." So what is the concern? By failing to exclude cases designated as having primarily business debts, we may be getting debtors who distort the income of our sample pool (presumably upward; one predicts businesses have higher incomes than individuals).

We reject this possibility as far-fetched. First, our pool only includes natural persons, so legal entities (such as corporations), which some if not many businesses are likely to be constituted as, are excluded at the outset.[51]

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[48]In revising his draft of this commentary, Professor Pardo now confronts the potential concern of endogeneity in the third paragraph of a multi-paged footnote. *See* Pardo, *supra* note 1, at 42 n.68 ("While possible that the legal significance placed on income and expenses by the means test may have changed the incentives of post-BAPCPA debtors in documenting these amounts . . . it seems reasonable to conclude that this would not be the case . . . ."). We refer interested readers to that lengthy discussion if so inclined; suffice it to say we are less sanguine than Professor Pardo at dismissing the endogeneity issue presented.

[49]*See, e.g., id.* at 43-44 (regarding state variation); *id.* at 44 n.71 (citing one of our earlier works in response to this concern).

[50]*Id.* at 40 (emphasis added). In an adjacent footnote, Professor Pardo faults alternative calculations we ran when we *did* make these exclusions. *See id.* at n.59. Our response to his broader critique in the text above necessarily dismisses this narrower critique.

[51]In any event, the line between a "consumer" bankruptcy and a "business" bankruptcy is not as stark as the nomenclature implies. For example, consider the discussion two of us once wrote in a separate context:

> Entrepreneurs can slip into the personal bankruptcy column, which raises the tantalizing question of whether the prototype small business may be changing from a small shop to a consultant. The small shop would have been widely regarded as a business separate from its owner, a discrete entity that might or might not have been incorporated. In contrast, the consultant who continues the same work that was once salaried and has now been outsourced is a very different kind of entrepreneur. Such a person is someone for whom there is no real separation between the

Second, the historical number of these individual (non-entity) debtors who check the "primarily business debts" box is minuscule: 0.5% in 2001 and 1.8% in 2007.[52] To worry about this tiny subset skewing our large data pool, we would have to imagine these few debtors as having wildly high incomes sufficient to move our overall findings. Yet wildly high figures were also excluded, as we make clear in our methodological appendix when discussing our protocol of removing statistical outliers.[53] Thus, we are nonplussed trying to ascertain just what fact pattern of potential data-skewing Professor Pardo envisions that we "fail[ ] to assuage."[54]

Accordingly, we remain unperturbed by Professor Pardo's methodological challenges. We have every confidence in the representativeness of the 2001 CBP sample and feel heartened by our own (already mentioned) bias checks. We also believe that gross income remains the best and most reliable measure for examining BAPCPA's means test. While Professor Pardo assures that above-median income debtors who list high deductible expenses under section 707(b) "need not worry about the means test,"[55] we maintain that they do worry, and worry quite a bit, at having to face that scrutiny.

III. TWO PREDICATING ASSUMPTIONS

Finally we return to comment on the two assumptions that Professor Pardo claims are foundational to our article yet flawed. Again, he casts the consequences as dire: "Because the [ ] Report does not address the concerns stemming from its assumptions, these concerns remain unresolved and call into question the Report's conclusions."[56] We again disagree with our colleague that our assumptions are flawed and we certainly disagree with the implication that our entire Report rises or falls with these purported assumptions.

The first "questionable"[57] assumption Professor Pardo claims we make is

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> business and the self. The personal nature of the bankruptcy is readily obvious. But this person is an entrepreneur nonetheless, dependent for income on the success of a small business, arranging for business loans, dealing with billing and bad debts, and at the mercy of a marketplace that may be slow or uneven.

Robert M. Lawless & Elizabeth Warren, *The Myth of the Disappearing Business Bankruptcy*, 93 CAL. L. REV. 742, 791 (2005). If one is worried about the deterrent effect of BAPCPA on the ground, the grey area between a "consumer" and a "business" is an important policy distinction not to be glossed over by relying on checkbox characterizations on the voluntary bankruptcy petition.

[52]*Id.* at 773 tbl. 2 (reporting number of persons who checked the box as having "primarily business debts" in the 2001 Consumer Bankruptcy Project as 0.5%). The 2007 numbers are from our current data and calculated for the purpose of this reply.

[53]*Did Bankruptcy Reform Fail?*, *supra* note 2, at 398.

[54]Pardo, *supra* note 1, at 40.

[55]*Id.* at 41.

[56]*Id.* at 31.

[57]*Id.* at 29.

that the means test played a causal role in deterring the 800,000 non-filers from the bankruptcy system:

> [T]he Report assumes that, but for BAPCPA's enactment, there would have been slightly more than 1.6 million bankruptcy filings in 2007. . . . . What makes this assumption problematic? . . . . [T]he Report needs to provide an account that explains why it is reasonable to assume that the missing 800,000 did not enter the consumer bankruptcy system in response to the existence of the means test.[58]

We are at a loss as to why this proposition is "problematic."[59] On the contrary, it was the animating premise of BAPCPA as championed by its proponents (whose rhetoric we cite extensively in our original article)[60] and has been seized upon by both defenders and detractors of BAPCPA as a near-obvious given: a transformative amendment to the Bankruptcy Code, which had the stated goal of driving legions of deadbeats from the consumer bankruptcy system, was followed by a sharp reduction in the number of people filing for bankruptcy. To us - and apparently to everyone else - causation of some sort is imputed as *res ipsa loquitor*. Indeed, we might put the shoe on the other foot to challenge Professor Pardo to explain to us how BAPCPA did *not* play a causal role in reducing the number of bankruptcy petitioners under these circumstances.

It could be, however, that he is making another, more limited argument: that we attribute the drop in filings to *the means test* in particular, and not BAPCPA in general. "[The CBP Report] casts its research question as one that would characterize the effect of bankruptcy reform as a function of a single provision of the Bankruptcy Code [ie., the means test]."[61] But this is a straw man; we make no such claim - again, as Professor Pardo himself somewhat acknowledges.[62] True, we say that this was the claim of BAPCPA's supporters - and we intended to test that claim - but we then devote a substantial part of the discussion section of our article reflecting on just what could be causing the reduction in filings of 800,000 missing debtors given that it *cannot* be the means test doing all the work.[63] Professor Pardo's suggestion that we do not consider alternatives to the means test is not just mistaken but deeply puzzling in light of our long and detailed discussion of precisely

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[58]*Id.* at 30.

[59]*Id.*

[60]*See Did Bankruptcy Reform Fail?*, *supra* note 2, at 352-53.

[61]Pardo, *supra* note 1, at 30; *see also id.* at 28 ("[The CBP Report] assumes that enactment of the means test deterred 800,000 individuals from filing for bankruptcy in 2007.").

[62]*Id.* at 30 n.19.

[63]*Did Bankruptcy Reform Fail?*, *supra* note 2, at 377-85.

such alternatives.[64]

The second assumption Professor Pardo slights as "questionable"[65] and "problematic"[66] is actually one that we make: that the income profiles of the 800,000 excluded filers in 2007 are likely to be similar to those still filing. Of course, this is a working inference. It is virtually impossible to gather data on deterred consumers who did not actually file bankruptcy,[67] and we acknowledge as much in the Report.[68] So we do what all good investigators do in the absence of data; we analyze the likelihood of the various possibilities. There are three possible income relationships between these two groups of potential filers. The deterred group could have higher incomes than those who actually filed, they could have lower incomes, or the two groups' incomes could be similar.

If the non-filers had lower incomes than the filers in 2007, then that would simply strengthen our misgivings over BAPCPA backfiring and not driving out the high-income abusers it was supposed to. We were loath to make an assumption that would suggest the means test had the reverse effect of *attracting* higher-income debtors to bankruptcy (at least without a strong underlying theory, which we did not have and no one has yet offered).

What about higher income for the non-filers? We thought this an unlikely possibility and stated so explicitly. As we explain in our Report:

> We should articulate a necessary assumption: that there was not a trend of rising income among those who tend to file for bankruptcy. If such a hypothetical trend were true, then merely showing similar incomes between bankruptcy filers before and after BAPCPA would suggest that BAPCPA

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[64]We find Professor Pardo's response especially unusual in light of the fact that he acknowledges our discussion. *See* Pardo, *supra* note 30, at 4 & n.19. But his characterization of our discussion is either mistaken or misleading; an extensive multi-page passage of our article is buried in a footnote suggesting it is a minor throwaway rather than a substantive analysis. "Although the First Report acknowledges the possibility that other BAPCPA-related provisions may have had a greater effect in deterring bankruptcy filings,[footnote] it casts the research question as one that would characterize the effect of bankruptcy reform as a function of a single provision of the Bankruptcy Code." *Id.* The footnote proceeds to pincite our references and discussion of Professor Ronald Mann's theory and later reference and discussion of Professor James J. White's theory. It does not, however, explain that *we agree with and embrace those theories. Did Bankruptcy Reform Fail?*, *supra* note 2, at 379-81. This framing of our analysis suggests, at least on our read, that we perhaps drop a begrudging footnote to Mann and White "acknowledg[ing]" the "possibility" that other forces than the means test may be at work in explaining our data but then rampage ahead relying solely on the means test as the explanatory agent. On the contrary, we embrace Mann and White in an extensive discussion and say the exact opposite from what Professor Pardo, inexplicably, faults us for apparently not saying - that many other forces beyond the means test must be at work. *Id.*

[65]Pardo, *supra* note 1, at 29.

[66]*Id.* at 31.

[67]In many cases, they might not even know who they are.

[68]*Did Bankruptcy Reform Fail?*, *supra* note 2, at 375. Professor Pardo cites this acknowledgement but nevertheless proceeds with his criticism. Pardo, *supra* note 1, at 31 n.23.

> succeeded in driving out the [800,000] *even-higher income* debtors that we would have expected to see in bankruptcy pursuant to this trend. In light of the flat incomes of the U.S. population generally, and our prior research suggesting that bankruptcy filers generally have lower (and stably lower) incomes than the general population, we would find it difficult to construct a model predicting such a trend.[69]

We stand by this position, which led us to take the most conservative stance in the face of uncertainty: assuming that the incomes of deterred filers are indistinguishable from actual filers - neither higher nor lower.

Does Professor Pardo offer any theories of his own regarding the income of the excluded - how they might be higher (which would provide support for BAPCPA's defenders) or lower (which would provide support for us)? He does offer, in a footnote, that some of the excluded filers might have been driven away by higher attorney's fees after BAPCPA.[70] If we take that theory as correct, and if we posit that those with lower incomes are the ones least able to pay at the margin when attorney's fees increase, then the income of the excluded 800,000 should be *lower* than the filing population. This would make our findings about the regressive impact of BAPCPA even stronger. Thus, not only is Professor Pardo unable - as were we - to construct a hypothesis under which the income of the excluded debtors would be higher in light of the available income data that we have, but his only speculation on the matter suggests that the income profiles of the excluded might be lower, which buttresses our conclusions.

CONCLUSION

Professor Pardo has written some insightful and helpful pieces of scholarship in the bankruptcy field that we have expressly relied upon in our own individual research projects.[71] This commentary, in our opinion, is not one of them. Instead of offering useful ideas of how to explore the available empirical data or build new data sets, he impugns our methodology, our logical assumptions, and our very understanding of BAPCPA's means test. With respect for our colleague, we do not find these critiques grounded in either a compelling theory of the operation of the bankruptcy system or a thorough

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[69]*Did Bankruptcy Reform Fail?*, *supra* note 2, at 357 n.42 (citations omitted). Professor Pardo was aware that we explain this assumption explicitly in our work because he cites (and quotes) it. Pardo, *supra* note 1, at 31 n.22.

[70]Pardo, *supra* note 1, at 30-31 n.21.

[71]*See, e.g.*, John A. E. Pottow, *The Nondischargeability of Student Loans in Personal Bankruptcy Proceedings: The Search for a Theory*, 2006 CAN. BUS. L. J. 245, 264-65 (discussing findings of Rafael I. Pardo and Michelle R. Lacey, *Undue Hardship in the Bankruptcy Courts: An Empirical Assessment of Discharge of Educational Debt*, 74 CINN. L. REV. 405 (2005)).

understanding of our data. It is unfortunate so much time was spent writing such a commentary (and response). We confess that we would rather have been analyzing more of our data and expanding the collective knowledge about post-BAPCPA debtors, an enterprise to which we (and Professor Pardo) can now return.