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Dear Benjamin:

This letter responds to your email request on April 21, 2006 requesting a review of the PENSIM model's forecasts of a proposed change to section 505 of ERISA (CFR 2550.404a-4) that would establish a "safe harbor" that would have the effect of encouraging employers to a) automatically enroll eligible employees in defined contribution pension plans, and b) to invest a larger share of such funds in securities such as equities and mutual funds that are riskier, but have a higher expected rate of return. I have attempted to follow EBSA's charge to reviewers, particulary, to limit my comments to an evaluation of the scientific merit of "the overall analysis of pension effects" predicted by the PENSIM model, and address the four specific questions raised in that charge. This letter provides my comments, and is limited to my evaluation of the scientific merit and reliability/accuracy of the PENSIM forecasts. I do not make any comment on the advisability of the proposed change itself, as directed in the charge to reviewers.

However I would like to make one comment on the draft of 29 CFR 2550 (dated April 12, 2006). I believe that the first sentence of the Background section A, "It is well established that many of Americas (sic) workers are not adequately saving for retirement." is too strong and is actually contrary to a number of recent studies that I cite below. Taking account of these studies, it would be incorrect to claim that it is "well established" that "many" Americans are not saving "adequately" for retirement. There are a number of very influential studies by leading researchers in public finance that show just the opposite, i.e. that that the majority of Americans are saving adequately for retirement. These studies include Alan Gustman and Thomas Steinmeier (1999) "Effects of Pensions on Savings: Analysis with Data from the Health and Retirement Survey" *Carnegie Rochester Conference Series on Public Policy* volume 50, pp. 271–324, Eric Engen, William Gale and Cori Uccello (1999) "The Adequacy of Saving" *Brookings Papers on Economic Activity* volume 2, pp. 65–165, and Karl Scholz, Ananth Seshadri and Surachai Khitatrakun (2006) "Are Americans Saving 'Optimally' for Retirement?" (forthcoming, *Journal of Political Economy*). There are further studies I can cite,

but it I quote from the conclusion of the Scholz *et. al.* study because I think it provides really convincing evidence that "undersaving" is not a huge problem that the government needs to worry about:

"In this paper we develop a rigorous approach for assessing the degree to which a representative sample of households nearing retirement have prepared financially for that event. We find strikingly little evidence that HRS households have undersaved. And because consumption requirements likely fall when households reach retirement (if for no other reason than work expenses fall), our standard may overstate required wealth. We also note that our primary data come from 1992 and 1993, well before the exceptionally strong stock market performance of the 1990s. Because 84.4 percent of households meet or exceed their wealth targets (and most of those who are below miss by a relatively small amount), we are skeptical that the consumption changes around retirement documented by Bernheim, Skinner, and Weinberg (*American Economic Review* (2001)) are due to inadequate retirement wealth accumulation. We also find it striking how much of the variation in observed wealth accumulation can be explained by our life-cycle model. We explain over 87 percent of the variation in wealth for married households, and nearly 80 percent for single, never-married households. And the results presented reflect no tweaking or prior fitting of the model. If we have found major deviations from the model and behavior, it would be difficult to determine whether Americans were preparing poorly for retirement, or we had constructed a poor behavioral benchmark. The fact that our predictions and data closely align suggests two things. First, as mentioned above, Americans are saving enough to maintain living standards in retirement. And second, the life-cycle model provides a very good representation of behavior related to the accumulation of retirement wealth. Of course, we still admit the possibility that Americans are preparing poorly for retirement, our underlying model is poor, and the errors, coincidentally, offset."

"Although the specific measures of undersaving and model fit clearly depend on parameter values, our two main results — that the life-cycle model is capable of closely matching the cross-sectional distribution of wealth in the HRS and that most HRS households are saving more than their optimal targets — are not affected significantly by parameter choices within the range commonly found in the related literature. We also find that life-cycle model does a much better job of matching the cross sectional distribution of wealth in 1992 than a naive model in which households save an income- and age-varying fraction of income. Turning to the question posed in the title of the paper: are Americans saving optimally for retirement? The HRS covers a specific cohort of Americans —households age (sic) 51 to 61 in 1992. Consequently we need to be careful in generalizing our results for the HRS cohort to younger households. This is particularly true if the generosity of social security is reduced in the future. Moreover, saving too much has efficiency costs in the sense that, absent preferences about intergenerational transfers or charitable contributions, reallocating consumption across time could increase lifetime utility. Because we cannot determine whether the systematic oversaving of HRS households reflects bequest motives, the expectation that social security will be reduced in the future, other failures in our characterization of the economic environment, or reflects non-optimal behavior on the part of HRS households, we cannot definitively answer the question posed in the paper title. But the paper provides new, strong support for the life-cycle model as a good characterization of the

process governing retirement wealth accumulation. And more important, it adds considerably to our confidence that the HRS cohort of Americans are preparing well for retirement." (quoted from Conclusions to study, pp. 34–36).

In view of these studies, I would characterize the scientific "background" that appears to motivate the need for 29 CFR 2550 to be a mischaracterization and overstatement of the economic literature. This is evident from the conclusion, quoted above, of a recently accepted paper to one of the leading journals in economics. If there is a case to be made for 29 CFR 2550, it is not one that is based on compelling scientific evidence that large numbers of Americans are "undersaving". There may be other, normative or "moral" assumptions underlying this change in regulation, (e.g. that saving is a "good thing" in and of itself because it helps to increase US capital stock, and increase the value of the stock market, etc.). However analysis of life cycle models, as well as simple common sense, suggests that there could actually be welfare losses from forcing younger, liquidity-constrained individuals to save more for retirement than they would otherwise choose voluntarily. The insight of the life cycle model is that a huge amount of savings for retirement is actually done in mid-life, and relatively little retirement saving is done at younger ages, especially for individuals who have low wages and little wealth to begin with. These individuals have other more immediate needs for their income, such as paying current bills, saving for a downpayment on a home, and so forth. The fact that young individuals have not accumulated large retirement savings accumulations is not evidence that they are saving sub-optimally for retirement. While it may be possible that baby boomers and younger cohorts are somehow different from the HRS cohort (the experience of the Great Depression may have imprinted a stronger savings ethic on the HRS cohort compared to more recent cohorts), my understanding of the empirical literature is that we do not have an adequate amount of data on more recent cohorts that is comparable in the level of detail and extent that we have collected in the HRS. Also the fact that these more recent cohorts are younger and not yet at the point of retirement means that would have to wait for some time (i.e. until these cohorts reach their pre-retirement years) to determine if their saving really is "optimal" or "adequate" or not. I do not find a strong basis of scientific evidence motivating 29 CFR 2550, especially since the PENSIM model is based on the 1985 cohort, a cohort that we know very little about (these individuals are currently only 21 years old, and many of them are still in college or graduate school). I think the lack of data on this cohort, the lack of scientific evidence that these individuals are undersaving (or the implicit assumption that these individuals will be undersaving in the future) is a major weakness in the draft I read. Thus, I suggest substantially modifying the background section of 29 CFR 2550 to provide a better rationale for this change in policy rather than to base its motivation on something that is more akin to assumption of undersaving on the part of individual rather than a "fact" and then follow this with the a highly controversial claim that it "well established" that individuals are systematically undersaving for retirement.

A number of economists in the "behavioral economics" literature cite a paper by Bernheim, Skinner, and Weinberg ("What Accounts for the Variation in Wealth Among U.S. Households?", *American Economic Review* 2001) as additional compelling scientific evidence that there is undersaving for retirement. Bernheim, Skinner and Weinberg provide empirical evidence that there is a significant drop in food consumption after retirement, and they interpret this as evidence of "undersaving" and/or a lack of "foresightedness" in retirement planning. They also formulate a simple life cycle model and argue that a life cycle consumer "smooths" consumption, and they interpret the drop in food consumption as evidence against the hypothesis that individuals behave in accordance with the life cycle hypothesis. However a number of researchers have shown their conclusions are actually quite dubious for a number of different reasons. First, as the quote from Sholz *et. al.* above suggests, there are good reasons why food expenditures and other work related expenditures should fall after retirement. Recent research by myself, Moshe Buchinsky, and Hugo Benitez-Silva (2006) "Is there a drop in consumption after retirement, and if so, is it consistent with the life-cycle model?" and David Blau (2006) "Retirement and Consumption in a Life Cycle Model" also shows that richer, more realistic versions of the life-cycle model than Bernheim, Skinner and Weinberg considered can, in fact, result in significant drops in *total consumption* after retirement. Our own research measures total consumption using data from all 6 available waves of the HRS survey, and not just food consumption that Bernheim, Skinner and Weinberg focused on. We show that total consumption does fall after retirement, so it is not just an idiosyncratic feature of food consumption.

As I noted above, there is good reason to expect food consumption would fall after retirement, since some share of food consumption may be work-related, and since higher expenditures for restaurants, convenience foods, etc. may be associated with an attempt to conserve on scarce leisure time while working, whereas the greater level of leisure time after retirement enables retired households to allocate more time to "household production". Indeed, a recent study by Mark Aguiar and Erik Hurst (NBER working paper 11601, Sept 2005) "Lifecycle Prices and Production" provides evidence of this important type of intertemporal substitution, and concludes "we use the estimated elasticities for shopping and home production to calibrate an augmented lifecycle consumption model. The augmented model predicts the observed empirical patterns quite well. Taken together, our results highlight the danger of interpreting lifecycle expenditure without acknowledging the changing demands on time and the available margins of substituting time for money."

My own study with Buchinsky and Benitez-Silva avoids the problems associated with food consumption by studying total consumption expenditure. We still find a significant drop in total consumption, by approximately 20%, after retirement, but we show that this drop in consumption can be explained by the life cycle model. This theoretical finding is also independently confirmed by Blau's study. Thus, the mere fact that even total consumption falls after retirement is not necessarily evidence that individuals are myopic and have prepared poorly for retirement: even far sighted life cycle consumers choose to reduce consumption after retirement since by doing do they do not need to save as much prior to retirement, can retire earlier than otherwise, and can accumulate precautionary buffer stock of wealth that is less costly that attempting to re-enter the labor market post-retirement in situations where

unexpected bad health outcomes and medical costs have unexpectedly depleted their saving levels.

Thus, my reading of the available scientific evidence, together with my own research in this area over the last 20 years suggests that the scientific basis motivating 29 CFR 2550 is weak, or at the very least, the statements that the statement on page 2, quoted above, that appear to be the key motivation for adopting it is an overstatement of the scientific evidence. I wish to emphasize that I am not expressing any support or criticism for or against 29 CFR 2550 itself, as required by the charge to reviewers. There may well be other good economic reasons why this change in the ERISA law is a good idea. However I believe it is a misstatement of the scientific evidence to motivate 29 CFR 2550 based on the apparently paternalistic desire to increase savings of American workers, particularly younger workers who may have other more pressing needs for savings (e.g. to pay off college loans, save for a downpayment on a home, or for college for their children, or to invest in their own businesses or continuing education, etc.). In my opinion, the case for 29 CFR 2550 is weakened to the extent that it makes a false claim that inadequate saving by individuals is "well established."

The remainder of my comments will focus on the use of the PENSIM model to predict the impacts of 29 CFR 2550 for a cohort of individuals born in 1985. My answer to the first question that I was asked to address, "Is PENSIM adequate to estimate aggregate 401(k) balances, pension income, the distribution of pension income, pension plan participation rates, and employer and employee contributions? " is NO. The PENSIM model does not provide a convincing forecast of the impact of adopting 29 CFR 2550 because it lacks a behavioral foundation and makes a number of poorly supported and undocumented behavioral assumptions. Since it is not based on an underlying model of individual behavior or any significant empirical analysis of how workers and firms would react to this policy change, I do not have much confidence in its predictions. Also, since the PENSIM model lacks an underlying behavioral model, the analysis is not able to say much about the individual welfare impacts of adopting 29 CFR 2550, and thus the cost/benefit analysis the DOL provides as a rationale for 29 CFR 2550 is unconvincing and incomplete.

Fundamentally the PENSIM model relies on an *assumption* rather than any behavioral model, or experimental or empirical evidence of how workers' *long term retirement savings* would be affected by a default policy of automatic enrollment, and of investing their defined contribution pension contributions in riskier but higher yielding securities. This is not to say that the PENSIM model's forecasts are not useful, but they are based on a largely mechanical "accounting calculation" that fails to account for a number of different behavioral responses by individual workers and by employers that could largely undo or mitigate the forecasted impacts 29 CFR 2550.

For example, at one extreme, a default policy of investing an entitled worker's DC contributions could be undone if the worker chooses to immediately cash out these funds. In fact, the worker might be worse off under this scenario compared to one where the equivalent DC contribution were given to the worker at a cash supplement, to be invested in a DC plan or not at the worker's discretion. The reason is that many tax deferred DC savings plans (IRAs, etc.) have substantial early withdrawal penalities. So if a worker elects to immediately "cash out" funds that were invested by default, these workers will be subject to a large tax that he/she could have avoided if the equivalent amount were provided to them as cash, and they retain the option of whether to invest these funds or not.

The proposal for 29 CFR 2550 notes "the right of participants and beneficiaries to direct investments out of the default investment alternative without penalty." (summary to "Default Investment Alternatives under Participant Directed Individual Account Plans"). In general, from the perspective of a rational, life cycle utility maximizer, default enrollment policies and portfolio allocation rules for which the worker receives advance notification should have no behavioral impact. A fully informed worker will either refuse the default of investing the funds after receiving the 30 days advance notice (as stipulated in 29 CFR 2550), or if they would have desired to invest these funds in a DC plan anyway, then they will not refuse the default of having these funds invested. Similarly for default portolio allocation: either the worker would have chosen this default for him/herself anyway (and thus would not change the default allocation), or they will simply instruct the pension fund manager to invest their pension contributions in the manner they deem best. In either case the final outcome is the same regardless of whether or not 29 CFR 2550 is in place, and this regulation would not affect the ultimate portfolio allocation decision of a rational life cycle utility maximizer. In other world, in the absence of transactions and information costs, a rational individual can completely "undo" the effect of 29 CFR 2550, and so it would be predicted to have no behavioral effect on enrollment levels, pension accumulations, or portfolio allocations.

However how would I reconcile this theoretical "policy neutrality prediction" with the findings of Choi, Laibson and Madrian (2004) "Plan Design and 401(k) Savings Outcomes" (*National Tax Journal*) and other related published finding? Choi *et. al.* find that the three companies they studied that had initially had a "standard enrollment regime" (i.e. where worker's have to actively elect to participate in the 401(k) plan) and switched to an automatic enrollment plan during the 1990s all experienced an increase in 401(k) participation after the switch, but the gap in enrollment levels between these two enrollment regimes declined over time, with tenure at the company, and the gap differed across the three companies studied. As they note, "Under a standard enrollment regime, 401(k) participation is low initially and increases with employee tenure at a decreasing rate." (p. 3 of working paper version of paper). In fact, figure 2 of their paper shows that for Company D, after 42 months of tenure the difference in the fraction of workers who ever participated in the firm's 401(k) plan is the same for active enrollment and standard enrollment after 42 months. For companies A, B and C there still is a gap in the fraction of workers who ever participated even after 36 months of tenure, but the gap between standard enrollment and active enrollment 401(k) participation rates is steadily narrowing over time. It seems possible to extend a life cycle model to account for transaction and information

costs of becoming informed about pension plans, and such a model could explain why standard enrollment rates are initially lower, but I am not aware yet of an explicit attempt to do so.

Thus, I would not deny that there is an effect of active enrollment (of the type to be encouraged by 29 CFR 2550) over "standard enrollment, but it is important to note that a) the magnitude of the effect is highly variable over different companies, and b) the gap between the two regimes tends to disappear over time, and c) the analysis tells us relatively little about whether employees with 401(k) balances "cash out" when they leave the firm. As Choi *et. al.* note, "When an employee leaves a firm, he or she may request a cash distribution, a direct rollover of 401(k) balances to an IRA, or a rollover to another employer's 401(k) plan. If the terminated employee does not make an explicit request, the balances typically remain in the 401(k) plan. Under current law, however, if the plan balances are less than $5,000 and the former employee has not elected some form of rollover, the employer has the option of compelling a cash distribution. Anecdotally, most employers exercise this option." (p. 19). Further, they note that "previous research suggests that the probability of receiving a cash distribution and rolling it over into an IRA or another 401(k) plan is very low when the distribution is small. Instead these small distributions tend to be consumed." (p. 21). Thus, even if automatic enrollment leads to a transient increase in 401(k) participation rates, it is much more uncertain that this increase will translate into a significant increase in long run retirement savings, as opposed to a significant share being cashed out by workers if they change jobs, or decide that they have some need for the money at some point in the future. While the early withdrawal penalty does discourage some early withdrawals, the lack of clear assumptions of early withdrawals and the reliance on a single estimate of the impact of automatic enrollments is a major weakness of the PENSIM model forecasts.

As Martin Holmer notes in the document "PENSIM Analysis of Impact of Regulation on Defined-Contribution Default Investments", "The participation probability increase caused by automatic-enrollment procedures is assumed to be such that the overall participation rate would rise from 69.4 percent when none of these plans have automatic enrollment procedures to 90.0 percent when all of these plans have automatic enrollment procedures. This DOL assumption reflects evidence reported in James J. Choi, David Laibson, and Bridgette C. Madrian, "Plan Design and 401(k) Savings Outcomes" written for the *National Tax Journal* Forum on Pensions, June 2004." (p. 7). However as we see from a closer look at the latter paper, this analysis is based on a very small sample of firms, i.e. four firms. There is little reason to believe that these four firms are representative of the huge number of firms and plans in the U.S. and Dr. Holmer provides no justification for the assumption that the *status quo* participation rate is 69.4 percent, and that it would rise to 90 percent when all of these plans have automatic enrollment procedures other than it is a "DOL assumption".

I question the DOL assumption that "As a result of the proposed regulation, in the near future such programs may cover 35 percent to 45 percent of eligible workers rather than 25 percent firms will adopt automatic enrollment procedures." (p. 40 of 29 CFR 2550 proposal). My reading of 29 CFR 2550 is that it *encourages* firms to adopt automatic enrollment, but it does not make this *mandatory.* If this is correct, then there needs to be a separate analysis of what factors might influence firms to adopt automatic enrollment compared to the "standard enrollment" procedures noted above, where workers have to explicitly sign up for a pension plan to become enrolled. If automatic enrollment was something that is very valuable or useful for workers, it is hard to understand why only a minority of firms currently have automatic enrollment policies for the DC pension plans they offer their workers. Indeed the 29 CFR 2550 proposal notes that "However, most surveys suggest that fewer than 20 percent of the employers sponsoring 401(k) plans have adopted an automatic enrollment provision." I would have felt much more comfortable about this analysis if the DOL had undertaken a survey of employers to find out the reasons why they chose not to make enrollment the default for their employees. Perhaps the 80% of the employers that do not enforce this default have good reasons for not doing so. If these firms do have good reasons for not doing so, and if automatic enrollment is not going to be mandatory under 29 CFR 2550, then one can question whether the impact will be very large if firms are not provided with any special incentive to alter their behavior.

However the most dubious assumption underlying the PENSIM model simulations, is that even if we were to accept the DOL assumption that there will be a large increase in the fraction of firms using automatic enrollment if 29 CFR 2550 were adopted, it also relies on an even more unsupported and I think highly dubious assumption that this change by firms will not be undone by workers who choose to opt out immediately from the default, or to eventually cash out their pension when they move to another firm. The DOL's interest appears to be to encourage greater *long term retirement savings and pension accumulations* not just short term pension plan participation and contributions. However none of the studies that the DOL appears to rely on about the "benefits" of automatic enrollment provide evidence on this long term relationship between automatic enrollment at time of employment and long term pre-retirement savings. These studies only show a very short term effect of automatic enrollment on higher pension plan participation rates, but as noted above this effect can be rather transient as pension plan participation rates under the "standard enrollment" policy that 80% of firms have chosen increase over time and often approach the participation rates under automatic enrollment, as I noted from the Choi, Laibson and Madrian study above. Further, even to the extent there are higher long term participation rates and thus pension savings, the long term effect on retirement savings may be very negligible if workers are likely to cash out pension balances when they switch employers. As noted from the Choi, Laibson and Madrian study above, employers seem to have a big role in liquidating small 401(k) balances, and there is little empirical evidence or modeling of pension liquidation and cash out decisions in the PENSIM model. The Choi, Laibson and Madrian study notes that "In three of the four companies, A, C and H, 88 to 91% of terminated participants with prior year-end balances less than $1,000 receive a cash distribution subsequent to termination." (p. 20). To the extent that 29 CFR 2550 is to be directed towards poorer, "vulnerable" individuals whom we might be more concerned that undersaving is a problem (as opposed to richer, more financially sophisticated workers, for

whom perhaps even the DOL might acknowledge are not individuals that the government should be greatly concerned about in regards to undersaving for retirement), that fact that low capital, low income workers tend to have higher job mobility and work at a sequence of lower paying jobs compared to more successful workers with more education and higher human capital who tend to have fewer longer term "career jobs" — this suggests that the issue of cashing out of DC pension balances when job transitions occur could be a major problem undercutting the potential impact of 29 CFR 2550. Thus, I do not find strong scientific support, either from the PENSIM model or any of the studies such as Choi *et. al.* on the effect of automatic enrollment policies that would justify the claim that "A substantial number of individuals will enjoy significant increases in retirement income." (p. 23 of the 29 CFR 2550 proposal).

Indeed, since the PENSIM model relies largely an "accounting calculations" rather than detailed behavioral models of workers and firms, the PENSIM forecasts must rely largely on externally provided behavioral assumptions since as I noted above there are no studies that I am aware of that provide a solid empirical link between adoption of automatic enrollment policies by firms and the increase this causes in the long run retirement savings of the firm's workers. It appears that the PENSIM model in turn relies largely on behavioral assumptions supplied by DOL, i.e. the "DOL assumptions" that Dr. Holmer noted in his reports. Thus, in the end I do not see any substantial, independent scientific basis for conclusions of the effect of 29 CFR 2550 in the proposal document, such as "In the very long run the proposed regulation is predicted to increase aggregate 401(k) plan account balances by between 2 percent and 5 percent, or approximately $45 billion and $90 billion if represented at 2005 levels. The portion invested in equity will increase by between 3 percent and 5 percent, or $27 billion and $48 billion." (p. 23). Although the charge to reviewers suggests that EBSA and DOL are themselves relying on "highly influential scientific assessments" (e.g. the PENSIM model), I do not see the forecasts of the PENSIM model as being "highly influential" and not even clearly "scientific" and it is unclear to me the extent to which its forecasts are "independent" to the extent that some of its key forecasts rely on "DOL assumptions." To state this more directly: it is not clear to me how much of a "highly influential scientific assessment" that the PENSIM model can offer if it relies on DOL assumptions which themselves have not been subjected to independent scrutiny and review. In effect, the "forecasts" in the 29 CFR 2550 proposal appear more akin to "guessitmates" that originate more from the DOL/EBSA itself than from the PENSIM model, and it would be more transparent to simply acknowledge this.

I do think the 29 CFR 2550 proposal reflects an honest appraisal of the great uncertainties in the forecasts, such as the statement "The estimates are highly uncertain. The long time horizon compounds the uncertainty. One of the greatest uncertainties relates to the default contribution rate, which is assumed to be fixed at 3 percent. Higher initial default contribution rates, or default provisions that increase contribution rates as tenure and/or pay increases, might enlarge the positive effects on pension income and reduce the negative effects. But it is unclear whether plan sponsors will adopt such approaches, or if they do, whether they might make other changes to their plans or whether more eligible employees might decline automatic participation. The Department therefore has no reliable basis for estimating the effects of such changes in automatic enrollment programs." (p. 50). I would agree with this assessment,

and to me, this statement itself is inconsistent with the much more confident predictions made elsewhere in the proposal and which I have quoted above.

In summary, based on my review of the PENSIM model and the supporting economic literature, I do not find strong scientific support for the main conclusion of the 29 CFR 2550 proposal, namely, "Based on the foregoing analysis and estimates, the Department is confident that the proposed regulation will increase aggregate retirement savings and pension income substantially. The Department therefore concludes that the benefits of this proposed regulation will exceed its costs by a wide margin, and invites comments on this conclusion." (p. 47). I find that there is almost no scientific support for the hypothesis that adoption of automatic enrollment policies by firms will lead to higher long run retirement savings, and therefore higher pension benefits during retirement. Also, as I noted above, I also question the motivating assumption for this proposal, namely, that workers are undersaving for their retirement. I have provided citations to a number of different recent studies that suggest the vast majority of individuals are saving adequately for their retirements. While these studies apply to older cohorts than the 1985 cohort that is the focus of the PENSIM analysis and forecasts, as I have noted above, there is almost no available scientific data that would enable us to conclude whether the 1985 cohort is substantially different from the older birth cohorts for which we have been able to observe their life cycle work, savings, and retirement outcomes. There may well be other good reasons to adopt 29 CFR 2550, but I do not think this proposal provides a strong scientific case for it.

Sincerely,

John Rust