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Disability and Employment Reevaluating the Evidence in Light of Reporting Errors

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Measurement error in health and disability status has been widely accepted as a central problem for social science research. Long-standing debates about the prevalence of disability, the role of health in labor market outcomes, and the influence of federal disability policy on declining employment rates have all emphasized issues regarding the reliability of self-reported disability. In addition to random error, inaccuracy in survey datasets may be produced by a host of economic, social, and psychological incentives that can lead respondents to misreport work capacity.

We develop a nonparametric foundation for assessing how assumptions on the reporting error process affect inferences on the employment gap between the disabled and nondisabled. Rather than imposing the strong assumptions required to obtain point identification, we derive sets of bounds that formalize the identifying power of primitive nonparametric assumptions that appear to share broad consensus in the literature. Within this framework, we introduce a finite-sample correction for the analog estimator of Manski and Pepper's (2000) monotone instrumental variable (MIV) bound.

Our empirical results suggest that conclusions derived from conventional latent variable reporting error models are being driven largely by ad hoc distributional and functional form restrictions. Moreover, under relatively weak assumptions, we find that an assumption of unbiased reporting is not supported. Nonworkers appear to overreport work limitations.

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Disciplines

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Disability and Employment: Reevaluating the Evidence in Light of Reporting Errors^{*}

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Measurement error in health and disability status has been widely accepted as a central problem for social science research. Long-standing debates about the prevalence of disability, the role of health in labor market outcomes, and the influence of federal disability policy on declining employment rates have all emphasized issues regarding the reliability of self-reported disability. In addition to random error, inaccuracy in survey datasets may be produced by a host of economic, social, and psychological incentives that can lead respondents to misreport work capacity.

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1 Introduction

Measuring employment rates among the disabled has been a matter of intense concern among policy analysts, especially since the passage of the Americans with Disabilities Act (ADA) in 1990. Most studies rely on self-reported health information to analyze relationships between employment and disability. Burkhauser et al. (2002), for example, make extensive use of survey questions of the general form: “Does a health impairment limit the kind or amount of work you can perform?” Evidence from these studies suggests that employment rates between the nondisabled and disabled have widened substantially since the induction of the ADA. Yet reporting errors in disability status contaminate estimates of conditional employment rates. Citing “grave concerns about the accuracy and reliability of widely disseminated information about employment rates among people with disabilities,” the National Council on Disability (NCD, 2002) warns that disability measurement error “could lead to ineffective or even dangerous public policy decisions.”

In this paper, we develop a nonparametric foundation for assessing how different assumptions on the reporting error process affect inferences on the employment gap between the disabled and nondisabled. Measurement error in health status has been accepted as a central problem for social science research (e.g., Institute of Medicine, 2002; Mathiowetz and Wunderlich, 2000; U.S. General Accounting Office, 1997). Twenty years ago, Anderson and Burkhauser (1984) characterized the measurement of work capacity in survey datasets as “the major unsettled issue in the empirical literature on the labor supply of older workers,” and the debates have only intensified over time. Prominent debates about the prevalence of disability, the role of health in labor market decisions, and the influence of Social Security Disability Insurance (SSDI) policy on declining labor force participation rates have all emphasized issues regarding the reliability of self-reported disability information. Bound (1991) provides an illuminating analyses of the econometric issues surrounding disability reporting errors.

There is widespread concern, in particular, about the accuracy of self-reported disability status in survey datasets. While most studies treat self-reports of work limitation as fully accurate, the literature encompasses a wide range of views on reporting errors. Some researchers contend

that disability reporting is largely reliable (e.g., Stern, 1989; Dwyer and Mitchell, 1999; Benitez-Silva et al., 2004), while others contend that strong economic and psychological incentives to misreport disability – coupled with potential difficulties with interpreting the survey questions – render the self-reports nearly devoid of content (e.g., Myers, 1982; Bowe, 1993; Hale, 2001). The psychology literature discusses the potential medical role of “negative affectivity” in respondents’ self-assessments of disability status (see, e.g., Watson and Clark, 1984). The unknown reliability of proxy or imputed responses raises further concerns (Lee et al., 2004).

Others take middle ground positions by formally treating self-reports as reliable for members of certain subpopulations but not others. Many researchers, for example, have emphasized that eligibility for disability transfers is specifically tied to diminished work capacity. Bound and Burkhauser (1999, p. 3446) suggest the possibility that “those who apply for SSDI and especially those who are awarded benefits tend to exaggerate the extent of their work limitations.” More generally, many have suggested that the threshold for claiming disability may be lower for those who find themselves out of the labor force, either voluntarily or involuntarily (e.g., Kerkhofs and Lindeboom (1995), O’Donnell (1999), and Kreider (1999, 2000)). Gordon and Blinder (1980) conclude that their estimated effect of ‘left last job for health reasons’ on early retirement is “too huge to be believed.”

Departing from the existing disability and employment literature, we do not focus on providing point estimates of the employment gap between the disabled and nondisabled. Instead, we derive analytic bounds that allow us to assess the identifying power of different assumptions on the disability reporting error process within a unifying methodological framework. We estimate conditional employment probabilities using information on respondents in the Health and Retirement Study (HRS) and the Survey of Income and Program Participation (SIPP). After describing the data in Section 2, we formalize the identification problem created by arbitrary misreporting in Section 3. New methodological results allow us to assess the sensitivity of the identification problem to variation in the nature and degree of corruption in a regressor, namely disability status. Our approach is similar in spirit to Horowitz and Manski (1995) who assess the problem of identifying a marginal

distribution in corrupt data. We extend their approach to allow for corruption of a binary regressor in a conditional distribution. In this setting, we show how the classical assumption of exogenous or “nondifferential” measurement error considered by Aigner (1973) and Bollinger (1996) can be used to tighten the upper bound on the employment gap.

In Section 4, we introduce the notion of partial verification of reports within particular observed subgroups (e.g., workers or disability beneficiaries). By allowing for some classification errors within partially verified subgroups, we depart from both the parametric disability literature (e.g., Kreider, 1999; McGarry, 2004) and nonparametric bounds literature (e.g., Horowitz and Manski, 1998; Dominitz and Sherman, 2004) which assume fully accurate reporting within verified subgroups. Section 5 considers the identifying power of monotonicity restrictions that link employment and disability to certain covariates such as age or the likelihood of being approved for disability benefits. Within this framework, we introduce a nonparametric method for correcting the finite-sample bias of the analog estimator of Manski and Pepper’s (2000) monotone instrumental variables (MIV) bound. Under relatively weak assumptions, our results support contentions in the literature that nonworkers systematically overreport disability. Section 6 concludes.

2 Data

Our main analysis uses data from the Health and Retirement Study (HRS) and the 1996 Survey of Income and Program Participation (SIPP). By coupling detailed information about health and disability, work history, and participation in government transfer programs with a panel design, the HRS and SIPP are perhaps the two most important data sources for studying the effects of health status and public policy on work outcomes. In Section 5, we further check the robustness of our results using the publicly released 5% extract from the 2000 Decennial Census of Population.

The HRS is a nationally representative panel survey of households whose heads were nearing retirement age (aged 51-61) in 1992-93. We use self-reported health and labor force participation information from all 12,503 respondents aged 40 or older. We also record each respondent’s years of schooling, occupation, race, gender, receipt of government assistance for a disability, and whether

the responses came from a proxy respondent. As part of our identification strategy, some of our analysis also incorporates reported health and employment information from the second wave which was conducted two years after the first wave.

The SIPP is a nationally representative longitudinal survey covering the U.S. civilian noninstitutionalized population. We utilize data from the first wave of the 1996 panel, a nationally representative sample of 36,800 households. Because respondents older than 69 were not asked about work limitations, we restrict the SIPP sample to the 29,807 individuals between the ages of 40 and 69.

Table 1A displays means and standard deviations. In the HRS, 21.9% of the sample responded that an impairment limits or precludes paid work, with 66.3% currently working for pay. The corresponding fractions in the SIPP data are 18.8% and 69.5%, respectively. These differences between the two surveys primarily reflect differences in the surveyed age distributions (see the last column in Table 1A).

Table 1B presents labor force participation rates by self-assessed work limitation and age. In the HRS, the employment rate among those reporting to be disabled is 0.294 compared with 0.766 for those reporting to be nondisabled. The difference in employment rates by reported disability status – i.e., the employment gap – is thus -0.472 . The corresponding employment gap in the SIPP is -0.482 .

3 The Identification Problem

To infer the employment gap between the disabled and nondisabled, we consider what self-reports reveal about true disability as measured by current social norms or the particular research question of interest. Clearly, survey designers have an expectation that respondents can place questions about work limitation in a reasonable social context. Some respondents may use thresholds different than those implied by the social norms, but the data do not reveal these respondents.

To evaluate the implications of invalid response in corrupt data, we introduce notation that distinguishes between self-reports and accurate reports. Let $L = 1$ indicate that the respondent is

employed, with $L = 0$ otherwise. Similarly, let $X = 1$ indicate that the respondent reports being limited in the ability to work, and let $W = 1$ indicate that the individual is truly limited in the ability to work relative to social norms (or other specified criterion). Finally, let Z indicate whether a respondent provides accurate information, with $Z = 1$ if $W = X$ and $Z = 0$ otherwise. We are interested in learning how the employment rate varies by true disability status:

$$\beta = P(L = 1|W = 1) - P(L = 1|W = 0). \quad (1)$$

The data reveal $P(L = 1|X)$ but not $P(L = 1|W)$. Therefore, β is not identified by the sampling process. In particular, Bayes' Theorem implies:

$$\begin{aligned} P(L = 1|W = 1) &= \frac{P(L = 1, W = 1)}{P(W = 1)} \\ &= \frac{P(L = 1, X = 1) + P(L = 1, X = 0, Z = 0) - P(L = 1, X = 1, Z = 0)}{P(X = 1) + P(X = 0, Z = 0) - P(X = 1, Z = 0)}. \end{aligned} \quad (2)$$

The data identify the fraction who self-report disability, $P(X = 1)$, and the joint probability of being employed and claiming to be disabled, $P(L = 1, X = 1)$, but they do not reveal the distribution of accurate reporters. Some unknown fraction of respondents, $P(X = 1, Z = 0)$, inaccurately report being disabled (false positives) while others, $P(X = 0, Z = 0)$, inaccurately report being nondisabled (false negatives). In the absence of restrictions on misreporting, the data are uninformative; we only know that the conditional employment rate lies between 0 and 1.

3.1 Nondifferential Classification Errors

The classical prescription used to address these identification problems is to assume that the reporting error process is exogenous. In particular, suppose that reporting errors are independent of the employment outcome conditional on true disability status:

$$P(X = 1|W) = P(X = 1|W, L). \quad (3)$$

This type of “nondifferential” classification error has been studied by Aigner (1973) and Bollinger (1996). When the independence assumption (3) holds, Bollinger’s Theorem 1 applied to a binary outcome can be used to show that β is bounded away from zero (in this case from above) by the

reported employment gap $P(L = 1|X = 1) - P(L = 1|X = 0) (< 0)$ (proof available upon request). This independence assumption clearly confers strong identifying power. Using the HRS data, for example, β is estimated to be less than -0.47 , reflecting well-known attenuation bias associated with random measurement error.

While the nondifferential measurement error assumption is powerful, Bound et al. (2001, p. 3725) note that the assumption is strong and often implausible. In our context, the assumption requires that conditional on true disability status, unemployed respondents are no more likely to report being disabled than employed respondents. This assumption effectively rules out, for example, the possibility that labor market outcomes affect respondents' perceptions of their disability status or that employment outcomes may be associated with perceived disability status in addition to true disability status. We proceed under the premise that the assumption of nondifferential errors (3) may not hold in this application.

3.2 Lower Bound Accurate Reporting Rate

To characterize the identification problem in the absence of the nondifferential classification errors assumption, it is useful to consider what can be learned with a known lower bound on the fraction of respondents that accurately report disability status. In particular, suppose

$$P(Z = 1) \geq v \tag{4}$$

where v is an known lower bound on the accurate reporting rate. Horowitz and Manski (1995) apply this degree assumption when assessing the problem of identifying a marginal distribution in corrupt data.

By varying the value of v , we can effectively consider the wide range of views characterizing the debate on inaccurate reporting. Those willing to assume that all reports are accurate can set $v = 1$, in which case the sampling process identifies the conditional employment rates. Those believing that all reports are potentially inaccurate can set $v = 0$, in which case the sampling process is uninformative. Middle ground positions can be evaluated by setting v between 0 and 1.

The lower bound in Equation (4) implies restrictions on the unknown joint distributions in

Equation (2). In particular, if the degree of misreporting is no greater than some known fraction, $1 - v$, the following sharp “degree bounds” apply (see the appendix for a proof):

Proposition 1. Let $P(Z = 1) \geq v$. Then $P(L = 1|W = 1)$ is bounded sharply as follows:

$$\frac{P(L = 1, X = 1) - \delta}{P(X = 1) - 2\delta + (1 - v)} \leq P(L = 1|W = 1) \leq \frac{P(L = 1, X = 1) + \gamma}{P(X = 1) + 2\gamma - (1 - v)} \quad (5)$$

where $\delta = \begin{cases} \min\{(1 - v), P(L = 1, X = 1)\} & \text{if } P(L = 1, X = 1) - P(L = 0, X = 1) - (1 - v) \leq 0 \\ \max\{0, (1 - v) - P(L = 0, X = 0)\} & \text{otherwise} \end{cases}$

and $\gamma = \begin{cases} \min\{(1 - v), P(L = 1, X = 0)\} & \text{if } P(L = 1, X = 1) - P(L = 0, X = 1) + (1 - v) \leq 0 \\ \max\{0, (1 - v) - P(L = 0, X = 1)\} & \text{otherwise.} \end{cases}$

To estimate the bounds in Proposition 1, we simply replace the population probabilities with sample analogs. Bounds for $P(L = 1|W = 0)$ are obtained by replacing $X = 1$ with $X = 0$ and vice versa in the proposition. An upper (lower) bound on β can be found by subtracting the Proposition 1 lower (upper) bound on $P(L = 1|W = 0)$ from the Proposition 1 upper (lower) bound on $P(L = 1|W = 1)$. Although these bounds on β are intuitive and simple to compute, they are not sharp. In the appendix, we show how the constraint $P(Z = 1) \geq v$ places further restrictions on β and formalize sharp bounds.

Note that when the lower bound fraction of accurate reporters is relatively small, the bounds on the conditional employment rates are uninformative. For example, when the degree of misreporting can exceed the fraction of respondents reporting to be disabled workers, $(1 - v) \geq P(L = 1, X = 1)$, the lower bound on $P(L = 1|W = 1)$ is zero. After all, despite self-reports to the contrary, all of these respondents may be nondisabled. Similarly, the upper bound is 1 when $(1 - v) \geq P(L = 0, X = 1)$.

The striking feature of the estimates from the HRS sample is that these bounds are uninformative across a large range of values for v . When $v = 0$, the employment gap can lie anywhere between -1 and 1 . The HRS data remain uninformative unless it is known that the accurate reporting rate exceeds 0.41 , and the lower bound remains at -1 unless v exceeds 0.82 . The sign of β is identified as negative (i.e., the data reveal that the disabled are less likely to work than the nondisabled) only

if at least 88 percent of the respondents are known to provide accurate reports. Results are similar for the SIPP data. Under weak assumptions on the degree of accurate reporting, the data provide only modest information on the true employment rates of interest.

4 Nonparametric Partial Verification Model

Concerns about misreporting focus primarily on financial and social incentives for certain types of respondents to exaggerate the extent of lost work capacity. First, eligibility into some government assistance programs (e.g., SSDI) is contingent on being sufficiently work impaired. In addition to monthly cash benefits, Supplemental Security Income (SSI) beneficiaries are immediately eligible for Medicaid benefits, and SSDI beneficiaries become eligible for Medicare benefits after a two-year waiting period. Second, some people may feel social pressure to participate in the labor force until normal retirement age unless their ability to work is impaired (see Bound, 1991). Those who find themselves out of work (or prefer not to work) may feel more compelled to claim that a functional limitation (e.g., difficulty climbing stairs) interferes with the ability to work.

Short of assuming that all respondents provide accurate self-reports, several studies have identified the true disability rate by combining distributional restrictions with assumptions that certain types of respondents provide accurate reports. The existing literature provides a number of restrictions (see, e.g., Bound and Burkhauser, 1999). Kreider (1999) and McGarry (2004), for example, assume that workers provide fully accurate responses, remaining agnostic about the reports from nonworkers. In the spirit of this literature, we evaluate what can be learned about the conditional employment rates given prior information on the degree of misreporting within four observed subgroups: (a) disability beneficiaries (10% in the HRS), (b) respondents who claimed no disability in the second wave of the survey despite being out of the labor force (27%), (c) respondents who were gainfully employed (66%), and (d) respondents who claimed no work limitation in the current wave (78%). For the HRS and SIPP, 94% and 93% of the respondents, respectively, satisfied at least one of these criteria. Although members of these groups may face little incentive to misreport, we allow for the possibility of some reporting errors within verified groups. Note that given the

high thresholds and restrictive screening process used in government disability programs, verifying a work limitation among beneficiaries is not tantamount to assuming that the limitation is sufficiently severe to warrant eligibility into the program.

Formally, let $Y = 1$ indicate that a respondent belongs to a “verified” subgroup, with $Y = 0$ otherwise. At least some fraction v_y of the self-reports in such groups are assumed accurate: $P(Z = 1|Y = 1) \geq v_y$. No other restrictions are imposed on the error process within verified groups, and no prior information exists for the error process in the unverified groups. Under these assumptions, we derive the following proposition (see the appendix for a proof):

Proposition 2. Let $P(Z = 1|Y = 1) \geq v_y$. Then $P(L = 1|W = 1)$ is bounded sharply as follows:

$$\begin{aligned} & \frac{P(L = 1, X = 1, Y = 1) - \delta}{P(X = 1, Y = 1) + P(L = 0, Y = 0) - 2\delta + (1 - v_y)P(Y = 1)} \\ & \leq P(L = 1|W = 1) \leq \\ & \frac{P(L = 1, X = 1, Y = 1) + P(L = 1, Y = 0) + \gamma}{P(X = 1, Y = 1) + P(L = 1, Y = 0) + 2\gamma - (1 - v_y)P(Y = 1)} \end{aligned}$$

where

$$\delta = \begin{cases} \min\{(1 - v_y)P(Y = 1), P(L = 1, X = 1)\} & \text{if } \alpha \leq 0 \\ \max\{0, (1 - v_y)P(Y = 1) - P(L = 0, X = 0, Y = 1)\} & \text{otherwise,} \end{cases}$$

$$\gamma = \begin{cases} \min\{(1 - v_y)P(Y = 1), P(L = 1, X = 0)\} & \text{if } \alpha' \leq 0 \\ \max\{0, (1 - v_y)P(Y = 1) - P(L = 0, X = 1, Y = 1)\} & \text{otherwise,} \end{cases}$$

$$\alpha = P(L = 1, X = 1, Y = 1) - P(L = 0, X = 1, Y = 1) - P(L = 0, Y = 0) - (1 - v_y)P(Y = 1),$$

and $\alpha' = P(L = 1, X = 1, Y = 1) - P(L = 0, X = 1, Y = 1) + P(L = 1, Y = 0) + (1 - v_y)P(Y = 1)$.

As before, bounds for $P(L = 1|W = 0)$ are obtained by replacing $X = 1$ with $X = 0$, and vice versa, and we can compute bounds on β by subtracting the appropriate bound on $P(L = 1|W = 0)$ from the appropriate bound on $P(L = 1|W = 1)$. Given verification in Proposition 2, these bounds on β are sharp.

Under the assumption of fully accurate reporting within verified subgroups, $v_y = 1$, these bounds simplify. In particular, it follows that if $v_y = 1$, we have

Corollary 2.1. If $Y = 1 \Rightarrow Z = 1$ (full verification), then

$$\frac{P(L = 1, X = 1, Y = 1)}{P(X = 1, Y = 1) + P(L = 0, Y = 0)} \leq P(L = 1|W = 1) \leq \frac{P(L = 1, X = 1, Y = 1) + P(L = 1, Y = 0)}{P(X = 1, Y = 1) + P(L = 1, Y = 0)}. \quad (6)$$

The width of these bounds depends on the joint distribution of the observed random variables, $\{L, X, Y\}$. An alternative derivation of the result in this corollary is provided in the Horowitz and Manski (1998) bound for regressor censoring.

4.1 Results

Empirical results are presented in Table 2 for the HRS and SIPP data. Columns A and C provide results for the degree bounds (Proposition 1) for selected values of v , and columns B and D present results for the verification bounds with $v_y = 1$. Bootstrapped ninety percent confidence intervals around the point estimates of the bounds are computed based on the bias-corrected percentile method (Efron and Tibshirani, 1993) using 1000 pseudosamples.

As noted above, the estimated Proposition 1 degree bounds are uninformative across a large range of values for v . In contrast, the Proposition 2 verification bounds are always informative for $P(Y = 1) > 0$. Still, the sign of β remains unidentified unless responses for all four groups are verified. In that case, β is estimated to lie within $[-0.472, -0.298]$ for the HRS and within $[-0.482, -0.255]$ for the SIPP. For both datasets, these bounds are 39 points narrower than the corresponding degree bounds.

Intuitively, the bounds widen if respondents in verified subgroups may misreport. Nevertheless, for a sufficiently large v_y , partial verification always improves upon the Proposition 1 bounds in Equation (5). Consider, for example, the Proposition 1 bound where $v = 0.10$, the fraction of disability beneficiaries. If we assume partial verification of beneficiaries alone, then the HRS upper bound is improved if even 27% of beneficiaries are known to provide valid responses.

The existing empirical literature assumes fully accurate self-reports within verified groups and imposes strong structure on the nature of reporting errors within remaining groups (e.g., independence between errors and outcomes). When we relax the usual distributional and functional form restrictions and isolate the identifying power of the verification assumptions, there remains much uncertainty about the true disability rate unless nearly all respondents are known to provide accurate reports.

5 Monotonicity Restrictions

We next formalize the notion that the employment rate may be known to vary monotonically with certain covariates such as age or the likelihood of being approved for federal disability insurance benefits. Suppose, for example, that the conditional employment rate is nonincreasing with age:

$$age_1 \leq age_0 \leq age_2 \implies P(L = 1|W, age_2) \leq P(L = 1|W, age_0) \leq P(L = 1|W, age_1) \quad (7)$$

for all $age_1 \leq age_0$ and all $age_0 \leq age_2$.

With corrupt data, the conditional probabilities in Equation (7) are not identified. However, we can bound these probabilities using the methods described above. Let $LB(age)$ and $UB(age)$ be the known lower and upper bounds, respectively, given the available information on $P(L = 1|W, age)$. Then the monotone instrumental variable restriction (MIV) formalized in Manski and Pepper (2000, Proposition 1) implies:

$$\sup_{age_2 \geq age_0} LB(age_2) \leq P(L = 1|W, age_0) \leq \inf_{age_1 \leq age_0} UB(age_1). \quad (8)$$

There are no other restrictions implied by the MIV assumption. These MIV models are not nested in the usual parametric models (e.g., probit models which impose different assumptions such as homogeneity), nor vice versa.

The MIV bound on the conditional employment rate is obtained using the law of total probability. Assuming the MIV age is a finite set, the following bounds apply:

Proposition 3. If the conditional employment rate is weakly decreasing with the MIV age, then:

$$LB_{MIV} \equiv \sum_{age_0 \in U} P(age = age_0) \left\{ \sup_{age_2 \geq age_0} LB(age_2) \right\} \leq P(L = 1|W) \leq \sum_{age_0 \in U} P(age = age_0) \left\{ \inf_{age_1 \leq age_0} UB(age_1) \right\} \equiv UB_{MIV}.$$

The MIV assumption alone has no identifying power, so we combine this assumption with the previous verification assumptions. In this setting, MIV can have identifying power if either the verification probability or an observed conditional employment rate is not monotonic with age.

5.1 Finite Sample Bias

Estimation of the MIV bounds is complicated by the fact one must impose the monotonicity restrictions in Equation (8) over collections of various estimates. In finite samples, estimators that take sups and infs are systematically biased. In this setting, moreover, this bias is especially concerning in that it leads the estimated bounds to be too narrow, rather than too wide, in finite samples. The sup of the lower bound estimates is biased upward and the inf of the upper bound estimates is biased downward. The literature, however, has not attempted to correct or assess the finite sample bias of the MIV estimator.

To address this concern, we present a modified MIV estimator that directly measures and accounts for this finite sample bias using the nonparametric bootstrap correction (see Efron and Tibshirani, 1993). To illustrate the basic idea, let T_n be a consistent analog estimator of some unknown parameter θ such that the bias of this estimator is $b_n = E(T_n) - \theta$. Using the bootstrap distribution of T_n , one can estimate this bias as $\hat{b} = E^*(T_n) - T_n$, where $E^*(\cdot)$ is the expectation operator with respect to the bootstrap distribution. A bootstrap bias-corrected estimator then follows as $T_n^c = T_n - \hat{b} = 2T_n - E^*(T_n)$. This bootstrap bias correction has been found to effectively reduce finite sample bias (in monte-carlo simulations) and be asymptotically efficient at higher orders in a variety of different settings. See, for example, Parr (1983), Efron and Tibshirani (1993), Hahn et al. (2002), and Ramalho (2005).

In our setting, the finite bias is simulated from the bootstrap distributions of the estimated Proposition 2 bounds for each age group. To estimate these bounds using the HRS, we divide the sample into 25 age groups containing 500 respondents per group (503 in the oldest group). For the SIPP sample, each age represents its own MIV group with cell sizes ranging from $n=642$ to $n=1692$ (mean=994). Then for each cell, the verification bounds – which are functions of various nonparametrically estimable probabilities – are estimated and the MIV restrictions in Equation (8) are applied. Figure 1, for example, displays the lower bound estimate and bootstrap distribution of $P(L = 1|W = 1)$ found using the HRS sample under the assumption that workers' responses are valid. The bias of the MIV estimator is estimated from these bootstrap sampling distributions.

To clarify the mechanics of our approach, let the parameter of interest, θ , be the Proposition

3 lower bound on $P(L = 1|W = 1)$ (other cases are analogous), let $LB_n(j)$ be the estimated Proposition 2 lower bound on $P(L = 1|W = 1, Age = j)$ for each age group $j = 1, \dots, J$ (see Figure 1, for example), and let T_n be the MIV lower bound estimate across all age groups. In particular, $T_n = \sum_{j \in U} P_n(j) \{\sup_{j' \geq j} LB_n(j')\}$, where $P_n(j)$ is the fraction of respondents in age group j . The bias b_n is estimated using the bootstrap sampling distribution of $LB_n(j)$. The first step is to randomly draw with replacement from the empirical distribution to obtain K independent pseudo-samples of the original data. Then, using these samples, compute a set of K lower bound MIV estimates of $P(L = 1|W = 1)$. Let T_n^k , $k = 1, \dots, K$, be the K lower bound bootstrap estimates and let $E^*(T_n) = \frac{1}{K} \sum_{k=1}^K T_n^k$ be the expected lower bound from the bootstrap distribution. Finally, compute the estimated bias, \hat{b} , and the bias-corrected MIV estimator, $T_n^c = 2T_n - E^*(T_n)$.

5.2 Results

Table 3 presents bias-corrected MIV bounds, confidence intervals, and estimated finite sample biases for the HRS and SIPP samples. For each of our verification groups taken in isolation, the improvements in the MIV bounds compared with the verification bounds are generally modest. When only workers are verified (Figure 1), for example, the MIV estimate of the lower bound for β using the HRS is -0.824 , a small improvement compared with the analogous verification bound of -0.839 . The improvement from the age MIV is somewhat larger for the SIPP, with the lower bound improving five percentage points from -0.842 to -0.794 . Notice also that in the case where workers are verified the finite sample bias plays only a modest role in both the HRS and SIPP samples. In the HRS, the bias is estimated to be 1.4 percentage points. Reflecting the larger sample sizes, the bias found in the SIPP is estimated to be 1.0 percentage point.

A more striking result emerges for the case when respondents within all four verification groups are assumed to provide accurate reports. In this case, the estimated MIV bounds for the true employment gap estimated from the HRS do not contain the self-reported employment gap, -0.472 , nor do the 90% confidence intervals overlap. A similar result holds for the SIPP as the confidence interval lower bound, -0.441 , exceeds the self-reported value of β , -0.482 . This finding was also confirmed using the publicly-released 5% extract of 3,806,011 individuals aged 40-69 from the 1990 Decennial Census (even without longitudinal information that might verify some self-reports based

on responses from a subsequent wave). About 15.2% of Census respondents reported being limited in the ability to work. The 90% confidence interval lower bound for β is -0.411 which exceeds the self-reported value of -0.474 . With age cell sizes averaging nearly 130,000 observations in the Census, the estimated finite sample bias for the standard MIV lower bound estimator is nearly negligible at less than 0.001. In contrast, the estimated bias is 2.9 percentage points in the HRS and 2.5 percentage points in the SIPP.

Thus, if employment weakly decreases with age, these findings suggest that conventional models which presume valid self-reports are likely to be misspecified. Since the unverified group consists of nonworkers who claim to be disabled, these findings support concerns in the literature that members of this group may systematically over-report disability. In addition, notice also that this finding is inconsistent with the nondifferential independence assumption, $P(X = 1|W) = P(X = 1|W, L)$, discussed in Section 3.1.

To further assess the sensitivity of this finding, we applied two other MIV assumptions in the HRS sample. First, we treated age as an MIV in disability instead of employment. Second, instead of age, a natural MIV that exploits information from a variety of individual characteristics in the HRS data can be constructed as the outcome of a respondent’s Disability Insurance application decision. In particular, let the categorical variable A equal 0 if the respondent has not applied for disability benefits, 1 if a disability application was rejected, 2 if an application was accepted after appeal, and 3 if an application was accepted immediately. Using A as the dependent variable, we constructed an MIV as fitted values from an ordered probit model of the application outcome. The specification includes indicators for a large set of physician-diagnosed health conditions and ADL limitations, an indicator for subsequent mortality (died before wave 2), an indicator for ideal body mass, age, education, race, gender, marital status, veteran status, and asset level (details from this regression are available upon request). We define the ideal range to be 20-25 kilograms per meter squared following Fahey et al. (1997). In both of these cases, we find that the lower bound MIV estimator exceeds the self-reported employment gap. For example, given full verification within the previously discussed subgroups, the 90% confidence interval for β narrows to $[-0.443, -0.289]$ after the disability application MIV is imposed (500 observations per cell).

Consistent with the past literature, we have maintained the assumption that all verified respondents provide accurate reports of disability status. While these verified subgroups may not have economic or social incentives to systematically misreport, there may still exist some inaccurate responses: respondents may have difficulties in answering subjective questions, valid reports can be miscoded, and so forth.

Proxy reports among verified groups may be especially concerning. Conceptual difficulties in answering questions about disability status may be compounded for respondents answering on behalf of others. Still, while proxy respondents may have less information about the extent of an impairment or its changing dynamics, they may also have less incentive to misreport. Lee et al. (2004) compare estimates of the number of disabled by respondent type in an environment in which self-response versus proxy was randomized. Among their primary findings, self-respondents and proxy respondents were equally likely to report disability during the initial interview, but proxy respondents were less likely to report disability in the second wave of the survey. The type of proxy mattered as spouses tended to give more consistent responses. This consistency could signify less misreporting among spouse proxies, or it could signify that misreporting among individuals tends to spill over to the spouse's report. In our HRS sample, less than 5% of the responses came from proxy respondents. Of those cases, the vast majority (over 90%) were spouses. In our SIPP sample, nearly 30% of the responses came from proxies (of undocumented type).

We examined the sensitivity of our results to varying degrees of misreporting among proxies within the four verified groups. Specifically, let $P(Z = 1|Y = 1, proxy = 1) \geq v'_y$. When $v'_y = 1$, all proxy reports within the verified groups are known to be accurate. When $v'_y = 0$, all proxy reports may be inaccurate. For the HRS, the 90% confidence interval for β expands from $[-0.449, -0.281]$ when $v'_y = 1$ to $[-0.470, -0.255]$ when $v'_y = 0$, a 4.7 percentage point increase in the widths of the bounds. The confidence interval for the SIPP expands from $[-0.441, -0.241]$ to $[-0.583, -0.105]$, a 28 percentage point increase in the width. Our earlier conclusion that the 90% confidence interval does not contain the self-reported value of β still holds in the HRS even if all of the proxy reports may be inaccurate. The conclusion still holds in the SIPP if v'_y exceeds about 0.75.

More generally, using Proposition 2 we can allow for the possibility of reporting errors from

other sources within verified groups. For $P(Z = 1|Y = 1) \geq v_y$ and arbitrary reporting errors, the 90% confidence interval does not contain the self-reported value of β if v_y exceeds 0.95 in the HRS and 0.92 in the SIPP. These critical values fall substantially, however, if invalid response among the verified can be treated as random error attributable to difficulties in answering subjective questions, coding mistakes, and so forth. In that case, disability self-reports within verified groups are *contaminated* (Horowitz and Manski, 1995):

$$P(W = 1|Y = 1) = P(W = 1|Y = 1, Z). \tag{9}$$

When all four subgroups are verified, the bias-corrected MIV bounds do not contain the self-report of the employment gap, β , as long as invalid response within each observed verified subgroup does not exceed about 15% in the HRS ($v_y = 0.85$) or about 30% in the SIPP ($v_y = 0.70$). Using the disability application index MIV, the confidence interval does not contain the self-reported value of β unless more than about 25% ($v_y = 0.75$) of respondents in the verified groups may misreport.

In summary, evidence that some respondents systematically overreport disability is replicated across different data and MIV assumptions and is robust to departures from the assumption of fully accurate reporting within verified groups.

6 Conclusion

Concerns over the validity of self-reported disability measures have been central in the many debates about the labor market outcomes of older persons. Given arbitrary errors in disability reporting, there is a critical and long-standing gap in our knowledge about how different data and assumptions affect inferences. The Institute of Medicine (2002) highlights the lack of information on reporting errors and calls for more research on the nature and consequences of these errors. The usual approach has been to identify parameters of interest by imposing strong distributional and functional form assumptions on the nature of misreporting. Most studies assume fully accurate reporting, while others have modeled the nature of misreporting in the context of conventional latent variable models.

This paper develops and applies a unifying nonparametric methodology that allows us to as-

sess the power of different assumptions about the error process in self-reported measures of work limitation when inferring the employment gap between the nondisabled and disabled. We began by extending Horowitz and Manski’s (1995) univariate setting to the case of a corrupt variable in a conditional distribution. We then examined the identifying power of “partial verification” and monotonicity restrictions on reporting errors. Within this framework, we introduced a method for correcting the finite-sample bias in Manski and Pepper’s (2000) Monotone Instrumental Variables estimator.

While our approach cannot resolve decades of uncertainty tied to disability reporting errors, the analysis takes an important step in formalizing the identification problem and highlighting the identifying power of a variety of primitive assumptions. Much of our analysis reveals the uncertainty created by arbitrary reporting errors. When we isolate the identifying power of popular verification assumptions without the usual distributional restrictions, there often remains much uncertainty about the true conditional employment rates. This important negative result supports concerns that conclusions derived from conventional latent variable models are being driven largely by ad hoc parametric restrictions. Moreover, some of the estimated bounds under the MIV restrictions do not include the employment gap based on self-reported data, thus casting doubt on the validity of treating self-reports as fully accurate.

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Appendix: Proof of Propositions 1 and 2

Proof of Proposition 1. (degree bounds) $P(Z = 1) \geq v$:

Decompose the conditional probability in Equation (2) as follows:

$$P(L = 1|W = 1) = \frac{P(L=1,X=1)+P(L=1,X=0,Z=0)-P(L=1,X=1,Z=0)}{P(X=1)+P(L=1,X=0,Z=0)+P(L=0,X=0,Z=0)-P(L=1,X=1,Z=0)-P(L=0,X=1,Z=0)}.$$

Let $a = P(L = 1, X = 0, Z = 0)$ where $0 \leq a \leq \min[(1 - v), P(L = 1, X = 0)]$, and let $b = P(L = 1, X = 1, Z = 0)$ where $0 \leq b \leq \min[(1 - v), P(L = 1, X = 1)]$. Then, for conjectured values of a and b , it follows that

$$\begin{aligned} & \frac{P(L = 1, X = 1) - b}{P(X = 1) - b + \min\{(1 - v) - b, P(L = 0, X = 0)\}} \\ & \leq P(L = 1|W = 1) \leq \\ & \frac{P(L = 1, X = 1) + a}{P(X = 1) + a - \min\{(1 - v) - a, P(L = 0, X = 1)\}}. \end{aligned} \quad (10)$$

These bounds are identified by finding the values of $\{a, b\}$ which maximize the upper bound and minimize the lower bound. First notice that these extremum are only realized if $(1 - v) - b \leq P(L = 0, X = 0)$ and $(1 - v) - a \leq P(L = 0, X = 1)$, in which case Equation (10) simplifies to

$$\frac{P(L = 1, X = 1) - b}{P(X = 1) - 2b + (1 - v)} \leq P(L = 1|W = 1) \leq \frac{P(L = 1, X = 1) + a}{P(X = 1) + 2a - (1 - v)}. \quad (11)$$

Differentiating this bound with respect to a and b reveals that the lower bound is minimized when $b = \delta$ and the upper bound is maximized with $a = \gamma$. Proposition 1 follows. \square

Proof of Proposition 2. (partial verification) $P(Z = 1|Y = 1) \geq v_y$:

Using Bayes' Theorem, $P(L = 1|W = 1) = \frac{P(L=1,W=1)}{P(W=1)}$. Decompose the numerator as

$$\begin{aligned} P(L = 1, W = 1) &= P(L = 1, X = 1, Y = 1) + P(L = 1, W = 1, Y = 0) \\ &+ P(L = 1, X = 0, Y = 1, Z = 0) - P(L = 1, X = 1, Y = 1, Z = 0) \end{aligned}$$

and decompose the denominator as

$$\begin{aligned} P(W = 1) &= P(X = 1, Y = 1) + P(L = 1, W = 1, Y = 0) + P(L = 0, W = 1, Y = 0) \\ &+ P(L = 1, X = 0, Y = 1, Z = 0) + P(L = 0, X = 0, Y = 1, Z = 0) \\ &- P(L = 1, X = 1, Y = 1, Z = 0) - P(L = 0, X = 1, Y = 1, Z = 0). \end{aligned}$$

Let $b = P(L = 1, X = 1, Y = 1, Z = 0)$ where $0 \leq b \leq \min[(1 - v_y)P(Y = 1), P(L = 1, X = 1, Y = 1)]$, and let $a = P(L = 1, X = 0, Y = 1, Z = 0)$ where $0 \leq a \leq \min[(1 - v_y)P(Y = 1), P(L = 1, X = 0, Y = 1)]$. Then, for conjectured values of a and b , it follows that

$$\begin{aligned} & \frac{P(L = 1, X = 1, Y = 1) - b}{P(X = 1, Y = 1) + P(L = 0, Y = 0) - b + \min\{(1 - v_y)P(Y = 1) - b, P(L = 0, X = 0, Y = 1)\}} \\ & \leq P(L = 1|W = 1) \leq \\ & \frac{P(L = 1, X = 1, Y = 1) + P(L = 1, Y = 0) + a}{P(X = 1) + P(L = 1, Y = 0) + a - \min\{(1 - v_y)P(Y = 1) - a, P(L = 0, X = 1, Y = 1)\}} \end{aligned} \quad (12)$$

Since a and b are unknown parameters, these bounds are not identified. Bounds are identified by finding the values of $\{a, b\}$ which maximize the upper bound and minimize the lower bound. First notice that these extremum are only realized if $(1 - v_y)P(Y = 1) - b \leq P(L = 0, X = 0, Y = 1)$ and $(1 - v_y)P(Y = 1) - a \leq P(L = 0, X = 1, Y = 1)$, in which case Equation (12) simplifies to

$$\begin{aligned} & \frac{P(L = 1, X = 1, Y = 1) - b}{P(X = 1, Y = 1) + P(L = 0, Y = 0) - 2b + (1 - v_y)P(Y = 1)} \\ & \leq P(L = 1|W = 1) \leq \\ & \frac{P(L = 1, X = 1, Y = 1) + P(L = 1, Y = 0) + a}{P(X = 1) + P(L = 1, Y = 0) + 2a - (1 - v_y)P(Y = 1)}. \end{aligned} \quad (13)$$

Differentiating this bound with respect to a and b reveals that the lower bound is minimized when $b = \delta$ and the upper bound is maximized with $a = \gamma$. Proposition 2 follows. \square

Sharp degree bounds on β

Suppose one has prior information on the maximum degree of inaccurate responses, $P(Z = 1) \geq v$. Using the same logic as in Proposition 1, we can also bound $P(L = 1|W = 0)$:

$$\frac{P(L = 1, X = 0) - a}{P(X = 0) - 2a + (1 - v)} \leq P(L = 1|W = 0) \leq \frac{P(L = 1, X = 0) + b}{P(X = 1) + 2b - (1 - v)}. \quad (14)$$

Combining Equations (10) and (14), we have:

Proposition 1a. Let $P(Z = 1) \geq v$. Then

$$\begin{aligned} & \inf_{b \in (0, \min[(1-v), P(L=1, X=1)])} \left[\frac{P(L = 1, X = 1) - b}{P(X = 1) - 2b + (1 - v)} - \frac{P(L = 1, X = 0) + b}{P(X = 0) + 2b - (1 - v)} \right] \\ & \leq \beta \leq \\ & \sup_{a \in (0, \min[(1-v), P(L=1, X=0)])} \left[\frac{P(L = 1, X = 1) + a}{P(X = 1) + 2a - (1 - v)} - \frac{P(L = 1, X = 0) - a}{P(X = 0) - 2a + (1 - v)} \right]. \end{aligned} \quad (15)$$

Over part of the range for v , these bounds differ from the naive bounds obtained directly from Proposition 1. Consider, for example, the lower bound in Proposition 1A. If the value of the unknown parameter b that minimizes the first expression (i.e., the lower bound on $P(L = 1|W = 1)$) differs from the value of b that maximizes the second expression (i.e., the upper bound on $P(L = 1|W = 0)$), the two bounds on β will differ and the Proposition 1A bounds will be tighter. The two bounds will be identical when the lower bound on $P(L = 1|W = 1)$ and the upper bound on $P(L = 1|W = 0)$ are realized at same value of the unknown parameter b .

Table 1

Descriptive Statistics

A. Means and Standard Deviations

	HRS (N=12,503)		SIPP (N=29,807)		
	mean	std. dev.	mean	std. dev.	weighted mean [†]
Work-limited (self-reported)	0.216	0.411	0.188	0.391	[0.218]
Disability precludes work	0.094	0.291	0.112	0.315	[0.135]
'Yes' to either of the above (X=1)	0.219	0.414	0.188	0.391	[0.218]
Labor force participant (L=1)	0.663	0.473	0.695	0.460	[0.686]
Current receipt of disability income	0.101	0.301	0.051	0.220	[0.066]
Age	56.0	5.26	54.9	8.62	[55.9]
Years of schooling	12.0	3.27	12.8	3.24	[12.6]
High school graduate	0.707	0.455	0.827	0.377	[0.805]
College graduate	0.175	0.380	0.228	0.368	[0.211]
Nonwhite race	0.280	0.449	0.162	0.382	[0.162]

B. Conditional Employment Probabilities
by Self-reported Disability Status

	All		Work Limitation (self-reported)		No Work Limitation (self-reported)	
	HRS	SIPP	HRS	SIPP	HRS	SIPP
	(N=12,503)	(N=29,807)	(N=2742)	(N=5597)	(N=9761)	(N=24,210)
Age:						
40-49	0.766	0.838	0.487	0.440	0.819	0.896
50-54	0.737	0.785	0.341	0.368	0.828	0.878
55-59	0.662	0.670	0.286	0.284	0.779	0.792
60-64	0.555	0.462	0.245	0.185	0.662	0.570
65-69	0.316	0.257	0.119	0.136	0.409	0.305
70+	0.224	--	0.087	--	0.287	--
All	0.663	0.695	0.294	0.304	0.766	0.786
weighted [†]		[0.680]		[0.290]		[0.789]

[†]Weighted to match HRS age distribution

Table 2

Estimated Bounds and 90% Confidence Intervals for the Employment Gap
Under the Lower Bound Accurate Reporting Rate and Partial Verification Assumptions

Verified Group	HRS Sample (N = 12,503)			SIPP Sample (N = 29,807)		
	(A)	(B)		(C)	(D)	
	v	Degree Bounds	Partial Verification	v	Degree Bounds	Partial Verification
beneficiaries	0.101	[-1.000, 1.000] [-1.000, 1.000] ^a	[-0.976, 0.846] [-0.980, 0.854]	0.051	[-1.000, 1.000] [-1.000, 1.000] ^a	[-0.984, 0.938] [-0.986, 0.940]
wave 2 verification	0.267	[-1.000, 1.000] [-1.000, 1.000]	[-0.793, 0.703] [-0.802, 0.714]	0.175	[-1.000, 1.000] [-1.000, 1.000]	[-0.829, 0.916] [-0.833, 0.920]
workers	0.663	[-1.000, 0.448] [-1.000, 0.458]	[-0.839, 0.361] [-0.849, 0.367]	0.695	[-1.000, 0.396] [-1.000, 0.403]	[-0.842, 0.323] [-0.848, 0.328]
claim no disability	0.781	[-1.000, 0.387] [-1.000, 0.396]	[-0.784, 0.361] [-0.790, 0.368]	0.812	[-1.000, 0.344] [-1.000, 0.349]	[-0.800, 0.323] [-0.804, 0.328]
all of the above	0.938	[-0.819, -0.259] [-0.839, -0.247]	[-0.472, -0.298] [-0.488, -0.277]	0.933	[-0.856, -0.240] [-0.862, -0.232]	[-0.482, -0.255] [-0.493, -0.241]

^abootstrapped 90 percent confidence intervals (Efron and Tibshirani, 1993)

Table 3

Estimated Bounds and 90% Confidence Intervals for the Employment Gap
Under the Age Monotone Instrumental Variable Assumption

Verified Group	HRS [†] (N=12,503)	SIPP [†] (N = 29,807)
beneficiaries	[-0.971, 0.831] ^a	[-0.974, 0.915] ^a
	[-0.977, 0.842] ^b	[-0.980, 0.921] ^b
	+0.013 -0.022 ^c	+0.007 -0.007 ^c
wave 2 verification	[-0.762, 0.672]	[-0.805, 0.881]
	[-0.775, 0.686]	[-0.815, 0.887]
	+0.021 -0.027	+0.012 -0.012
workers	[-0.824, 0.357]	[-0.794, 0.322]
	[-0.838, 0.366]	[-0.807, 0.327]
	+0.014 -0.008	+0.010 -0.004
claim no disability	[-0.767, 0.357]	[-0.785, 0.322]
	[-0.780, 0.367]	[-0.791, 0.327]
	+0.006 -0.008	+0.004 -0.004
all of the above	[-0.431, -0.308]	[-0.438, -0.255]
	[-0.449, -0.281]	[-0.441, -0.241]
	+0.029 -0.033	+0.025 -0.023

^bMIV point estimates, corrected for finite-sample bias

^abootstrapped 90 percent confidence intervals

^cestimated finite-sample bias

[†]for the HRS, each age grouping is constructed so that there are 500 observations per age cell; for the SIPP, each age represents its own MIV group with cell sizes ranging from n=642 to n=1692 (mean=994)

Figure 1

Bootstrapped Age-Specific Histograms for Lower Bounds on $P(L=1|W=1)$
in the HRS when Disability Status is Verified for Workers

