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# Finding Needles in Haystacks: Multiple-Imputation Record Linkage Using Machine Learning

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**Abstract:**
This paper considers the problem of record linkage between a household-level survey and an establishment-level frame in the absence of unique identifiers. Linkage between frames in this setting is challenging because the distribution of employment across establishments is highly skewed. To address these difficulties, this paper develops a probabilistic record linkage methodology that combines machine learning (ML) with multiple imputation (MI). This ML-MI methodology is applied to link survey respondents in the Health and Retirement Study to their workplaces in the Census Business Register. The linked data reveal new evidence that non-sampling errors in household survey data are correlated with respondents' workplace characteristics.

**JEL Classifications:** C13, C18, C81
**Keywords:** Administrative data, machine learning, multiple imputation, probabilistic record linkage, survey data

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# 1 Introduction

Increasingly, researchers are interested in linking survey and administrative data for measurement and analysis. In most record linkage applications, the units being linked originate from the same frame. For instance, individuals in a given data set are linked to the same individuals in a different data set, or businesses in one data set are linked to the same businesses in another data set. In this paper, we consider the problem of linking across frames. We match individual respondents in household survey data to administrative data on the universe of employers. How does one use a household report of a business to link to the correct employer? It would be possible to build in these linkages from the start, especially where a sampling frame is created from administrative data. In that case, linkage is part of the design. This paper addresses the problem of linking individuals and employers where the linkage is not pre-designed into a survey. This situation typically arises in surveys of households, which are built from sampling frames of household addresses, often without the purpose of linkage as part of the design. Even in an idealized world where the survey and administrative frames were developed in tandem, additional linkages to other administrative data that are not part of the design may be desirable.

We treat record linkage as a missing data problem where true match status is unknown and must be imputed. To impute this information, we must accomplish two related tasks. First, we need to predict whether any given pair of records drawn from the two data sets constitutes a true match. Second, we need to characterize uncertainty in the prediction of true matches and propagate that uncertainty into inferences drawn from the linked data set by subsequent analyses.

The task of predicting true match status is difficult because the size distribution of firms is very skewed. Consider the striking empirical fact that 0.3 percent of all firms employ 53 percent of all workers in the United States.[1] There are, however, more than 6 million firms in the United States, so the flip side of this fact is that most firms are very small. With so many small firms, matching individuals to employers is inherently noisy because many small employers among a set of potential candidates are feasible matches for any given survey respondent. This is our needle-in-the-haystack problem. For example, imagine a set of candidate matches that includes a large insurance company, an independent credit union at the same location that has the same name as the insurance company, and a cafeteria that is also at the same location, is operated by a third-party vendor, and also has the same name as the insurance company. The names and address of the candidate matches are all similar. When presented with this information, a human reviewer may use auxiliary information such as the respondent's industry, occupation, or reported size of the firm where the respondent is employed to guess the correct match. We automate and speed up what a human reviewer would do by using a supervised machine learning (ML) approach to predict the matching firm. ML is particularly valuable for record linkage because it makes flexible use of a very large number of predictors, including auxiliary information, to mimic the heuristics used by a human reviewer. Furthermore, relying on a rich set of predictors lends support to the assumption

[1] 2018 Statistics of U.S. Businesses (SUSB), U.S. Census Bureau.

that imputation errors are ignorable, thereby improving inferences in subsequent analyses of the imputed data. Finally, our cross-validated ML estimator is tuned to deliver high out-of-sample accuracy.

Multiple imputation (MI) allows analysts to propagate match uncertainty when conducting inference. For each record in household survey data, our procedure samples multiple candidates from administrative employer-level data by using ML-based match probability estimates as weights. In cases where the match probability estimates are highly concentrated, household survey records are linked to just one employer. Conversely, for cases where the match probability estimates are highly diffuse, household survey records are linked to many different employers. In the completed (matched) data set, variability between implicates for a given household survey record captures uncertainty associated with the linkage for that household. Subsequent analyses of the completed data set can then combine the multiple implicates for valid statistical inference as in Rubin (1987).

In this paper, we apply our novel combined ML-MI approach to record linkage to match the Health and Retirement Study (HRS), a longitudinal household-level survey of older Americans, and the Census Business Register (BR), an administrative data set that covers the universe of employers in the United States. This new, linked household-employer data set will provide researchers novel ways to investigate wide-ranging questions about the role of employer- and workplace-specific factors in influencing wages, consumption and savings decisions, health outcomes, and retirement choices of older workers. We re-examine the well-known positive gradient between hourly wages and workplace size to provide an example of the type of analysis that the matched data can facilitate. We find that both non-classical measurement error and selective non-response in the HRS survey reports of workplace size generate upward bias in this gradient.

The plan of this article is as follows. Section 2 describes record linkage methodologies in deterministic and probabilistic contexts. Section 3 provides details on the files that we link and explains the three major steps of our record linkage procedure. Section 4 assesses the fit of our match prediction model and evaluates the degree of uncertainty in our linkage. Section 5 illustrates an application of the matched data to shed new light on the incidence and consequences of non-classical measurement error and selective non-response in household survey respondent reports of workplace size. Section 6 concludes.

## 2 Essentials of Record Linkage

This paper builds on an important literature that developed widely used techniques for record linkage. The simplest approaches are non-probabilistic. In these deterministic file-matching applications, researchers accomplish record linkage by isolating a set of variables that are common to a given record in both files. This procedure constitutes both the first and last step in the linkage. It is the first step because it enumerates the set of possible matches. It is the last step because only those records that have exactly one match conditional on variable agreement are retained. In some instances, a sufficiently rich set of accurately measured variables can allow a large fraction of the

original file to be unequivocally matched (see, for example, Lawson et al. (2013) and Setoguchi et al. (2014)). In other cases, the matched file consists of a smaller and potentially non-random subset of the original file that limits the usefulness of the matched data set for analysis. This concern is highlighted in the context of linking historical data, for example, in Bailey et al. (2017).

The Fellegi and Sunter (1969) (FS) method is an early and widely used probabilistic linking approach that picks the best match from the set of multiple potential matches. In this method, researchers estimate the probability that a particular characteristic (such as gender or first and last name) agrees in the two files, given that the records should link (match) and given that they should not link (non-match). To estimate match probabilities, the FS method relies on the strong, and sometimes untenable, assumption that the agreement status of each characteristic is independent conditional on true match status. Next, the data are used to determine high and low log odds cutoffs. Potential matches that land above the high cutoff are coded as true matches, and those that fall below the low cutoff are treated as non-matches. Potential matches that fall between the two cutoffs are evaluated manually, a procedure that has been criticized, for example, in Belin and Rubin (1995) because the error properties of manual review are unknown, may be subject to inconsistent standards across reviewers, and may fail to yield a substantial number of unequivocal matches.[2]

ML methods for record linkage constitute a popular alternative to the FS approach. These methods estimate highly flexible nonparametric functions and classify record pairs into matches and non-matches. For example, Cochinwala et al. (2001) and Elfeky et al. (2002) use decision trees for classification, while Christen (2008a) and Christen (2008b) rely on support vector machines. ML approaches have been implemented with training data (supervised) and without it (unsupervised), with the former typically yielding more accurate linkage (see, for example, Christen (2008b)). The key advantage of the ML-based record linkage approach is its high degree of accuracy. These implementations of ML create a deterministic classifier. Hence, like FS, existing ML-based record linkage applications select the best match among a set of candidate matches. That is, conditional on the matching algorithm, matches are treated as deterministic.

The Bayesian approach to record linkage characterizes uncertainty associated with parameters in the linkage process (Fortini et al. (2001) and Larsen (2004)). In this method, researchers specify prior distributions of parameters governing the mixture of matches and non-matches that generate the comparison vector of agreement status for variables observed in both files. Draws from the posterior predictive distribution of the parameters are then used to produce estimates of pair-specific match probability. One-to-one matching is enforced using the mode of the posterior predictive distribution or by minimizing a loss function. Tancredi and Liseo (2011) refine this procedure by relying on observed discrete matching variables rather than a comparison vector of agreement status for those variables. Steorts et al. (2016) provide a method of linking multiple files, each with potentially duplicated records, within the Bayesian framework. Gutman et al. (2013) and Gutman et al. (2014)

[2]While manual review of the entire set of blocked records has been adopted in some applications (for example, Ferrie (1996)), it is prohibitively expensive in many settings and remains subject to the same criticisms as the manual review step of the FS method.

further develop the Bayesian approach by applying it to situations where variables used in the linkage model are available in both files as well as in cases where variables are available in only one file. Moreover, they jointly model the linkage step and relationships between variables in the linked data set (the analysis step). Then, by repeatedly sampling from the posterior distribution of the linkage step parameters, they generate multiple implicates of linked data sets that are used in the analysis step and combined using the formulas in Rubin (1987). This procedure has the advantage of propagating uncertainty in the linkage step parameters into the analysis step.

Work that is highly germane to the household-employer record linkage problem we consider in this paper began as part of the Longitudinal Employer Household Dynamics (LEHD) program, in two projects that were initiated in the early years of that effort. The first of these projects linked employers to job histories in the 1990–1996 Surveys of Income and Program Participation (SIPP).[3] Abowd and Stinson (2013) evaluate this linkage and use it to compare self-reports and administrative reports of earnings. The LEHD program also links establishments (that is, specific workplaces for a given employer) in the Quarterly Census of Employment and Wages, called the Employer Characteristics File in the LEHD, to individual workers via the state unemployment insurance account number, called the State Employment Identification Number (SEIN) in the LEHD. This linkage starts with deterministic methods using the SEIN. When these methods do not find a one-to-one match, a Bayesian posterior predictive distribution is used to generate 10 implicates linking establishments to the candidate worker's employment history (Abowd et al. (2009)).[4] These 10 implicates are used to connect workplace characteristics to each worker history.[5] The 10 implicate threads are processed according to the Rubin (1987) combining formulas to produce the Quarterly Workforce Indicators (QWI). McKinney et al. (forthcoming) provide a complete assessment of the total variability in the QWIs due to the MI and other edit procedures.

The methodology we develop relies on the accuracy of the ML approach to record linkage while using MI to characterize uncertainty in the linkage and to propagate that uncertainty into subsequent analyses. To our knowledge, this combination of methods has not been employed previously in record linkage applications.[6] Our ML approach allows us to leverage a very large number of predictors to estimate match probabilities that include both discrete and continuous observed variables from either file as well as agreement status variables constructed using both files. Furthermore, the flexibility inherent in this method accommodates rich complementarity between predictors and allows us to dispense with the assumption that predictor variables are independent conditional on true match status, as has been posited in many prior applications. In addition, tuning our prediction models to achieve high out-of-sample accuracy facilitates scalability and precision linkage

[3]This work also developed improved linkages within the 1990–1993 SIPP job histories and integrated data from the Census Business Register into the SIPP (Stinson (2003)).

[4]See Goldstein et al. (2012) for a similar approach applied to medical records.

[5]Other incomplete data in the LEHD infrastructure, such as incomplete data for education, are completed using similar Bayesian methods.

[6]ML methods have been used to improve MI in applications that do not involve record linkage. See, for example, Reiter (2005) for the creation of partially synthetic public use microdata and Burgette and Reiter (2010) for missing variable imputation.

in a way that is difficult to achieve using Bayesian or FS methods. Finally, unlike prior ML-based record linkage methods that use binary classification to select the single best match, the ML model that we use provides a match probability estimate for each record pair. By using a Bayesian bootstrap procedure (Rubin (1981)) to repeatedly sample candidate matches from the estimated match probability distribution when constructing MI linkages, our methodology allows us to approximate parameter uncertainty in the ML model while also characterizing uncertainty regarding latent match status.

# 3 The Machine Learning, Multiple Imputation (ML-MI) Procedure

## 3.1 Overview

In this section, we describe our ML-MI record linkage procedure for matching household-level survey data to establishment-level administrative data. Our approach acknowledges that many matches are uncertain and is explicit about uncertainty at all steps. It produces a data set of multiply-imputed links that, if used appropriately, will allow analysts to produce statistics and inferences that account for the uncertainty of matches.

We integrate machine learning and multiple imputation using the following steps. First, we enumerate the set of candidate establishments that constitute feasible matches for each survey report about a particular job using a technique known as blocking. Second, we create training data for supervised ML. Third, we estimate an ML model nested within a weighted Bayesian bootstrap (WBB) and use the model to obtain match probabilities for each candidate match. We then draw an implicate using the match probabilities as weights when sampling among the candidates. This step is repeated to create $M$ implicates.

Our procedure accounts for match uncertainty in two ways. First, there is parameter uncertainty in the ML model because it is based on finite data. Second, there is match uncertainty conditional on the parameters because the probability distribution over potential matches is not degenerate. Both types of uncertainty propagate through our procedure since the model is re-estimated and an implicate is drawn using match probabilities as weights within each bootstrap iteration.

At the conclusion of this section, we describe how the multiply-imputed matches should be used in analysis in order to propagate match uncertainty.

## 3.2 Data set structure

Before delving further into the details of our methodology, we briefly describe the data sets that we use in our application. The household survey that we use is the Health and Retirement Study (HRS), which surveys more than 22,000 Americans over the age of 50 every two years. It is a large-scale, longitudinal project studying the labor force participation and health transitions that individuals undergo toward the end of their work lives and in the years that follow. About 70 percent of HRS respondents give permission to the Social Security Administration (SSA) to provide earnings records, which include Federal Employer Identification Numbers (EINs), to the HRS for

purposes of enhancing the HRS data infrastructure.[7] In addition to the EIN provided by the earnings records, the HRS elicits the employer name, establishment address, and telephone number for the respondent's "main job" or the job about which a bulk of work-related survey questions are asked. Respondent reports on employer identity and address are obtained at the survey baseline (that is, when new respondents are enrolled in the study, generally every six years, when a new cohort is added to the study) and in each subsequent wave if the respondent reports having changed jobs.[8] We use EINs along with employer names and establishment addresses to match HRS respondents' employers and workplaces to the Census Business Register (BR), which is the Census Bureau's list of essentially all establishments in the United States. Note that the establishment is the workplace, and a given firm may operate many establishments. The BR contains information on EIN, employer name and establishment address, company affiliation, size, payroll, industry classification, and other employer-level and establishment-level characteristics and can be linked to other Census Bureau survey and administrative data.[9] We refer to the data set created by matching the HRS to the BR and associated Census Bureau data as the CenHRS.

Our procedure includes three cases for linking HRS jobs to the BR: a deterministic match based on the EIN and a probabilistic match with or without the EIN. Table 1 shows these cases and their characteristics.

- In the first case, the respondent consents to SSA linkage and can be deterministically matched to an establishment in the BR. This case occurs when the respondent has just one job (and therefore just one EIN) in a given year, and that EIN corresponds to exactly one establishment in the BR.

- In the second case, the respondent consents to SSA linkage but cannot be deterministically matched to an establishment in the BR. This case happens either because the respondent has multiple jobs in a given year (and consequently has multiple EINs) or because the respondent's EIN does not uniquely identify an establishment in the BR, or both.

- In the third case, the respondent does not consent to SSA linkage, and therefore we do not have their EIN.

For respondents in the second and third categories, which represent about 60 percent of our sample, we implement probabilistic record linkage using ML and MI. In the next section, we discuss how the availability of the EIN affects this procedure.

[7] In addition to earnings records for consenting respondents, the SSA provides retirement and disability benefit claims data.

[8] The HRS collects employer names, establishment addresses, and phone numbers to contact respondents' employers about retirement benefit provisions.

[9] EINs are tax identification numbers; they do not uniquely identify establishments except in two special cases. The first case is for employers that operate just one establishment (single-unit employers). The second case is for employers that operate multiple establishments (multi-unit employers) and have multiple EINs and where a specific EIN points only to one establishment. An example of the latter case would be if Dunder Mifflin Paper Company were a two-establishment firm with one establishment located in Scranton, Pennsylvania, and another in New York City, and if each of those establishments had its own EIN.

### 3.3 Procedure

#### 3.3.1 Blocking

Let jobs in the HRS be indexed by $i = 1, ..., N_{\text{HRS}}$. A job in the HRS is defined as a spell of employment at a unique establishment. Let establishments in the BR be indexed by $j = 1, ..., N_{\text{BR}}$. If we started with the prior that every record in the BR is a potential match for each job in the HRS, we would need to search over a set of $N_{\text{BR}} \times N_{\text{HRS}}$ pairs. This set is of the order $10^6 \times 10^4$.

To reduce the dimensionality of the search problem, we follow a blocking strategy. Blocking groups record pairs that share specific characteristics, wherein pairs that have at least one characteristic in common are regarded as having a positive probability of being matches, while pairs that have no characteristics in common are deemed as non-matches (see, for example, Christen (2012)). That is, the blocking strategy assigns zero probability to candidates outside of the block. If the block has only one candidate, the linkage is deterministic. For HRS respondents who consent to SSA linkage, we block on their EINs. For HRS respondents who do not consent to SSA linkage, we block on their 10-digit phone numbers, three-digit Zip codes, telephone area codes, and city-states.[10]

#### 3.3.2 Training data

The blocking variables we use strongly influence the level of ex ante match uncertainty. Blocking on EINs generates an average of about 400 candidate matches (that is, unique establishments in the BR) per HRS respondent. In contrast, blocking on location-specific variables generates about 30,000 candidate matches per HRS respondent and is therefore associated with a much higher level of uncertainty. See Table 1. Hence, the relationship between predictors and match status varies substantially based on whether the HRS-BR pair is blocked using EINs or not. We account for these differences by creating two different training samples and train separate models: one based on pairs blocked using EINs and the other based on pairs blocked without EINs. Each training sample consists of $N^T \approx 1000$ randomly selected HRS-BR unlabeled pairs. We oversample pairs with a higher likelihood of being true matches using the data and methodology described in Appendix A.

We now specify the procedure for creating the training data by human review to label HRS-BR pairs. Define $\boldsymbol{x}_i^H$ as a vector of individual demographic characteristics, employment-related variables, self-reported employer characteristics, and survey paradata for HRS respondent $i$. Define $\boldsymbol{x}_j^B$ as a vector of characteristics for establishment $j$ drawn from administrative data in the BR. Let $k(ij) = 1, \ldots, N^T$ index HRS-BR pairs in each of the unlabeled samples. These data are examined by reviewers, who observe certain pair characteristics, $\boldsymbol{x}_k$, which are a subset of $(\boldsymbol{x}_i^H, \boldsymbol{x}_j^B)$; Table 2 lists the elements of $\boldsymbol{x}_k$. Each HRS-BR pair is evaluated by two reviewers. Define $y_{k,r} = 1$ if reviewer $r$ scores pair $k$ as a match and $y_{k,r} = 0$ otherwise. To the extent that they disagree, the two reviewer assessments—that is, the $y_{k,r}$—reflect uncertainty about latent match status.[11]

[10] We do not model blocking uncertainty. For respondents who consented to SSA linkage, the EINs come directly from the SSA data (no uncertainty). For respondents who did not consent to SSA linkage, the blocking variables were respondent provided. Accounting for uncertainty in these data is outside the scope of our current models.

[11] A total of seven reviewers conducted these reviews in the Federal Statistical Research Data Center (FSRDC)

For each HRS-BR pair in the unlabeled samples, reviewers consider employer- and establishment-match status separately. An employer match means that the employer identity (for example, Dunder Mifflin Paper Company) in the HRS corresponds to the employer identity in the BR. In contrast, an establishment match implies that, in addition to there being an employer match, the workplace reported by the HRS respondent exactly corresponds to the physical location in the BR (for example, Dunder Mifflin Paper Company, 1460 Main Street, Scranton, Pennsylvania). This distinction is important because workplace characteristics can differ substantially even at different locations of a single employer. For example, different establishments of a given employer may experience differential expansion or contraction, produce different types of goods or services, or employ workers of different skill types or ages. Consequently, we construct four different training data sets: employer match for EIN-blocked pairs, establishment match for EIN-blocked pairs, establishment match for non-EIN-blocked pairs, and establishment match for non-EIN-blocked pairs. In the employer match data set, $y_{k,r}$ refers to employer-match status; in the establishment match data set, $y_{k,r}$ refers to establishment-match status.

Once reviewers complete their assessments, each of the four training data sets can be represented by the following matrix:

$$\boldsymbol{T} = \begin{bmatrix} y_{1,1} & \boldsymbol{x}_1 \\ y_{1,2} & \boldsymbol{x}_1 \\ \vdots & \vdots \\ y_{N^T,1} & \boldsymbol{x}_{N^T} \\ y_{N^T,2} & \boldsymbol{x}_{N^T} \end{bmatrix}. \tag{1}$$

Because there are two reviewer ($r \in \{1, 2\}$) outcomes associated with each HRS-BR pair in the training sample indexed by $k(ij) = 1, \ldots, N^T$, there are $l(ij) = 1, \ldots, 2N^T$ rows in the training data set $\boldsymbol{T}$.

### 3.3.3 Estimating the ML model using weighted Bayesian bootstrap

We estimate the ML models using a set of variables that supplements the information observed by reviewers ($\boldsymbol{x}_l$). The supplemented set of predictors is given by the vector $\tilde{\boldsymbol{x}}_{l(ij)} = f(\boldsymbol{x}_l, \boldsymbol{x}_i^H, \boldsymbol{x}_j^B)$, where the function $f(\cdot)$ supplements and transforms the observed data. Table 3 shows the elements of $\tilde{\boldsymbol{x}}_{l(ij)}$.

The first set of predictors are pair specific. Cubic splines of Jaro-Winkler (JW) scores for employer name and establishment address and a linear JW score for city jointly capture reviewers' assessments of the similarity in the HRS and BR names and addresses.[12] We include cubic splines of the share of employment within the blocking variable accounted for by a candidate BR establishment (or employment share). This variable accounts for the fact that individuals in household surveys are

computing environment.

[12] JW scores, which range from 0 to 1, combine edit distance and $q$-gram-based comparison techniques to measure string similarity. The JW score for establishment address is based on the number and street and does not include information on city, state, or Zip code similarity.

more likely to be employed at larger establishments for any given blocking schema. For EIN-blocked cases, we also include cubic splines of the share of annual earnings accounted for by a candidate EIN (or earnings share). When HRS respondents have multiple jobs, this variable aids in disambiguation by accounting for the fact that the "main job"—about which respondents provide answers in survey questions—is more likely to be associated with a larger share of total earnings. Finally, we include variables measuring the pair-specific agreement status for several characteristics: seven- and ten-digit phone number; three-, four-, and five-digit Zip code; city-state; one-digit industry code; and employer and establishment size class. Some pair-level predictors, such as ten-digit phone agreement, can be highly influential in predicting match probability, but it is very rare for candidate matches to share such granular characteristics. On the other hand, sharing an industry code or a four-digit Zip code is more likely but less predictive of a match.

The second set of variables comes purely from the BR and includes the log size of the employer and whether the BR candidate match is a single-unit (SU) or multi-unit (MU) business.[13] Size and MU-status variables are intended to capture the higher unconditional probability that individuals in a household survey will be employed at larger, MU employers.

The third set of variables comes purely from the HRS and includes the respondent's age, gender, race and ethnicity, education level, nativity, marital status, survey interview mode (in person or telephone), survey interview language (English or Spanish), log hourly real wage, years of tenure, weeks worked per year, hours worked per week, whether the respondent's employer provides health insurance and/or a retirement savings plan, and the respondent's two-digit occupation and one-digit industry. We include these variables to control for job-specific determinants of match status as well as the quality of identifying information about the employer and establishment reported by the HRS respondent.[14]

To account for the fact that household survey respondents are more likely to be employed at larger establishments, and at employers that provide a larger share of annual earnings, we fully interact the cubic splines of name and address JW scores, employment share, and earnings share.[15] Including this rich set of higher-order interactions substantially increases the number of variables we use to predict match likelihood. Combining HRS-BR pair-specific variables, variables only from the BR, variables only from the HRS, and all the interaction terms, we have a total of 9,200 predictors in the vector $\tilde{\boldsymbol{x}}_{l(ij)}$.

Having defined the set of predictors, we use the logistic function to model match probabilities as:

$$P(y_{l(ij)} = 1|\tilde{\boldsymbol{x}}'_{l(ij)}) = \frac{\exp \tilde{\boldsymbol{x}}'_{l(ij)}\boldsymbol{\beta}}{1 + \exp \tilde{\boldsymbol{x}}'_{l(ij)}\boldsymbol{\beta}}. \tag{2}$$

[13] SU businesses operate only one establishment, whereas MU businesses operate multiple establishments.

[14] We include information on health insurance and/or retirement plan provision because these variables are correlated with employer size, which is often missing in the HRS.

[15] When EINs are unavailable, earnings share cannot be defined. For these cases, we interact the JW scores for name and address with cubic splines of log employer size from the BR.

To approximate posterior uncertainty in $\boldsymbol{\beta}$, we use a weighted Bayesian bootstrap (WBB) (see, for example, Rubin (1981), Newton and Raftery (1994), and Newton et al. (2021)). We construct $m = 1, \ldots, M$ WBB replications of each of the four training data sets by drawing $k = 1, \ldots, N^T$ i.i.d. random variates $\nu_k^{(m)}$ from an exponential distribution with mean 1. The random weight associated with HRS-BR pair $k$ in the training data is $w_k^{(m)} = \nu_k^{(m)} / \bar{\nu}^{(m)}$, where $\bar{\nu}^{(m)}$ is the sample mean of the $\nu_k^{(m)}$. To account for pair-level clustering, each duplicated pair in the respective training data sets receives the same weight.

Define the dimension of $\tilde{\boldsymbol{x}}_{l(ij)}$ as $q$. Since $q \gg 2N^T$, that is, the number of predictors exceeds the number of observations, we rely on the elastic net (EN) shrinkage estimator for model selection and estimation of $\boldsymbol{\beta}$ (Zou and Hastie (2005)). The EN-based parameter estimate for the $m$-th WBB replication is obtained by maximizing the constrained likelihood function:

$$\hat{\boldsymbol{\beta}}^{(m)} = \underset{\beta \in \mathbb{R}^q}{\operatorname{argmax}} \sum_{l=1}^{2N^T} w_l^{(m)} \left( y_l \log \left( \frac{\exp(\tilde{\boldsymbol{x}}_l' \boldsymbol{\beta})}{1 + \exp(\tilde{\boldsymbol{x}}_l' \boldsymbol{\beta})} \right) + (1 - y_l) \log \left( \frac{1}{1 + \exp(\tilde{\boldsymbol{x}}_l' \boldsymbol{\beta})} \right) \right) + \lambda \sum_{p=1}^{q} \left( \alpha |\beta_p| + (1 - \alpha) \beta_p^2 \right). \tag{3}$$

In Equation (3), $l$ indexes observations in the training data, while $p$ indexes predictors. $w_l^{(m)}$ is the random weight attached to observation $l$ in WBB replication $m$. The two tuning parameters, $\alpha$ and $\lambda$, which either zero out or shrink the elements of $\hat{\boldsymbol{\beta}}^{(m)}$, are estimated using tenfold cross validation to optimize out-of-sample predictive performance; see Appendix B for additional details. We plug $\hat{\boldsymbol{\beta}}^{(m)}$ from the respective models (employer and establishment, EIN and non-EIN) into Equation (2) to obtain an estimate of the probability that a human reviewer would regard an unlabeled $i - j$ pair as a match, which we denote by $\hat{p}(\tilde{\boldsymbol{x}}_{ij}; \hat{\boldsymbol{\beta}}^{(m)})$.[16] We iterate this WBB step $M = 10$ times.

Our match imputation relies on the assumption that unobserved determinants of match status are ignorable conditional on the predictors in the model. This assumption can be stated as:

$$P(y_{ij} = 1 | \tilde{\boldsymbol{x}}_{ij}) = P(y_{ij} = 1 | \tilde{\boldsymbol{x}}_{ij}, \boldsymbol{z}_{ij}), \tag{4}$$

where $\boldsymbol{z}_{ij}$ represents a vector of all other variables that influence match status for a given pair. The high dimension of $\tilde{\boldsymbol{x}}_{ij}$ with rich information at the pair-level, from the BR and from the HRS, makes assumption (4) more tenable and facilitates valid inferences for a wide class of subsequent analyses with the linked data.[17] In the next section, we discuss how we use these probabilities to multiply impute matches.

[16] Parameter estimates reflect an average of the two reviewer evaluations. Because we found that reviewer disagreement in the training data was highly unlikely, we do not model between-reviewer uncertainty or propagate it in our MI procedure.

[17] See, for example, the issues of congeniality in imputation models noted by Meng (1994), Rubin (1996), and Murray (2018).

### 3.3.4 Multiple imputation of matches

Existing probabilistic record linkage procedures that either select the highest probability match or enforce one-to-one matching in other ways effectively treat the match, conditional on the procedure, as deterministic. In common with Bayesian record linkage approaches, our procedure captures both linkage uncertainty and the uncertainty in the parameters of the matching model. In the final step, we propagate uncertainty when selecting candidate matches by using multiple imputation.

#### Selecting matches for EIN-based cases

When EINs are available for blocking, we normalize the $\hat{p}(\tilde{\boldsymbol{x}}_{ij}; \hat{\boldsymbol{\beta}}^{(m)})$ to sum to one for each HRS respondent. For each outcome (employer or establishment match), this provides $M$ different estimates of the match probability for each candidate. Then, for each HRS respondent, we draw one implicate from each of the $M$ normalized match probability distributions. We do this separately for employer matches and establishment matches.

#### Selecting matches for non-EIN-based cases

When EINs are unavailable and blocking is based on location-specific variables, there are many more candidate matches to consider, and the task of match selection is substantially more difficult. Consider an example where an HRS respondent is paired with 10,000 BR candidate matches, of which one large-employer candidate is the correct match, and 9,999 small-employer candidates are non-matches. Suppose the large-employer candidate obtains a normalized match probability of 0.5, while each of the 9,999 small-employer candidates receives normalized match probabilities of $\frac{0.5}{9999}$. In this example, random small-employer candidates are as likely to be sampled as the large-employer candidate even though they are three orders of magnitude less likely to be correct.[18]

To mitigate the confounding effect of a large number of candidates, we apply a minimum match probability threshold to eliminate low-quality matches from consideration. For each of the 10 WBB replications, we estimate this threshold by relying on the sample of cases in which EINs yield deterministic matches between HRS respondents and BR establishments. Although we know the true match for these respondents, we proceed as if the EINs were unavailable. That is, we block on location-specific variables and use the match probability estimates to select implicates for each HRS respondent.[19]

The top left panel of Figure 1 illustrates how we determine the match probability threshold at the employer level. The solid circle labeled "Naive" shows match quality without imposing a threshold. Although the entire HRS sample is deemed to be linked, the precision rate—or the share of

[18]The logistic function is bounded below by zero, which causes the model always to return a non-zero match probability. The confounding effect of low probability matches on linkage precision is driven by the large number of candidates, each of which has a trivially small match probability.

[19]This sample enumerates *all* HRS-BR pairs conditional on the blocking variables for a given HRS respondent. This location-blocked validation sample allows us to evaluate the challenge of selecting the right match from a very large number of potential matches. The sample is conceptually different from the training data set, which is a random sample of pairs.

HRS respondents who are correctly matched—is extremely low. Moving away from the naive case, we iterate over progressively higher probability thresholds. For each new choice of threshold, we re-normalize the estimated match probabilities over the set of candidates that survive the threshold and sample matches with probability proportional to the re-normalized match probabilities. Progressively raising the match probability threshold generates movement up and to the left along the solid blue line. For higher thresholds, the share of HRS respondents with no candidate matches survives the threshold increases, and, consequently, the fraction of the sample that can be linked falls. The solid blue line therefore traces out the *realized precision frontier*, which is the trade-off between the precision rate and the share of the sample that can be linked. We define the optimal point on the precision frontier as the threshold that yields a precision rate and a sample link rate that are each closest to their maximum values of 1, or the top right corner of the graph. Formally, the optimal probability threshold obtained using parameter estimates from the $m$-th WBB replication is:

$$\hat{p}^{*(m)} = \underset{p\in[0,1]}{\operatorname{argmin}} \left( \left(1 - \mathcal{P}(\tilde{\boldsymbol{x}}_{ij}; \hat{\boldsymbol{\beta}}^{(m)}, p)\right)^2 + \left(1 - \mathcal{L}(\tilde{\boldsymbol{x}}_{ij}; \hat{\boldsymbol{\beta}}^{(m)}, p)\right)^2 \right)^{1/2}, \tag{5}$$

where $\mathcal{P}$ and $\mathcal{L}$ denote the precision rate and the link rate, respectively. The optimal trade-off is shown with a hollow circle in Figure 1. With respect to the quality of inferences drawn from non-EIN-based linkages, the optimal threshold in Equation (5) places as much weight on controlling selection bias induced by incomplete linkage as it does on controlling incorrect linkage.

The precision rate we estimate is driven by two factors: the extent to which location-based blocking variables aid in isolating true matches and the extent to which the estimated model assigns high match probabilities to true matches and low match probabilities to true non-matches. To parse the relative contributions of these factors, we plot the *limiting precision frontier*, which is shown in the dashed red line. For each probability threshold, the limiting precision frontier shows the performance of a hypothetical linkage algorithm that selects the correct match in every instance where those matches survive the blocking criteria. Because the location-specific blocking variables sometimes eliminate true matches, the limiting precision frontier is always less than 1.[20] The top panel of Table 4 shows that, at the optimal threshold, the limiting precision rate is about 0.79, while the realized precision rate is about 0.59. These rates indicate that 21 percentage points of the loss in precision (1.0–0.79) are due to blocking error, while 20 percentage points (0.79–0.59) of the loss in precision are due to inefficiency in the matching model. Thus, blocking errors and model inefficiencies are each responsible for half of the total loss in precision.[21]

[20] What this means is that some true matches in the BR do not share any location-specific blocking variables with the HRS respondent's report of their workplace address and phone number, thereby causing false negative errors. The availability of EINs for blocking would be sufficient to make the limiting precision frontier equal to 1 for any probability threshold at the employer level.

[21] In addition to precision, Table 4 shows recall at the naive and optimal thresholds. Recall is the proportion of correct matches in the BR that are selected. The limiting precision rate equals the limiting recall rate by definition. The true negative rate (specificity) is trivially small in non-EIN-based record linkage because the number of true negatives is several orders of magnitude larger than the number of true positives. For this reason, we do not consider specificity when choosing the optimal threshold.

The top right panel of Figure 1 shows that the extremely large number of candidates per HRS respondent drives the low level of precision attained with unrestricted match selection. For the average HRS respondent, there are $10^4$ to $10^5$ candidates from which to select implicates when we block on location-specific variables. After applying our optimal match probability threshold, however, we reduce the average number of candidate matches by three orders of magnitude and obtain a very large increase in the precision rate. The lower row of Figure 1 and the lower panel of Table 4 show analogous statistics for the establishment-matching model.

For the sub-sample of HRS respondents without EINs, we leave as unmatched those respondents for whom every BR match candidate's estimated match probability is below the optimal threshold. Any matching procedure, of course, should acknowledge the possibility that there is no reasonable match. Our procedure handles this possibility systematically based on the estimated matching model, its uncertainty, and a well-specified objective function estimated using validation data. Hence, determining that a case is a non-match is entirely integrated into the procedure. It does not rely on ancillary determination, for example, when a case is treated as a non-match if the match probability is below an externally specified threshold.

### 3.4 Using the multiply-imputed data set

Our procedure yields $M = 10$ multiply imputed employer and establishment links for each HRS respondent, thereby constituting $M$ completed data sets.[22] For any statistic generated using imputed data, we can combine estimates obtained from each of the $M$ completed data sets using the formulas in Rubin (1987) to compute the variance owing to sampling uncertainty (within-implicate variability) and the variance due to linkage uncertainty (between-implicate variability).[23] For some scalar parameter of interest $\theta$, let $\hat{\theta}_m$ represent estimates derived from the $m = 1, \ldots, M$ completed data sets. Let $\hat{\sigma^2_m}$ represent the variances associated with each of the $M$ parameter estimates. The multiply imputed estimate of $\theta$ is

$$\hat{\theta} = M^{-1} \sum_{m=1}^{M} \hat{\theta}_m. \tag{6}$$

The within-implicate variance is

$$\hat{\sigma^2_W} = M^{-1} \sum_{m=1}^{M} \hat{\sigma^2_m}. \tag{7}$$

[22] When EINs are sufficient to yield a one-to-one BR match for an HRS respondent, record linkage is deterministic and trivial. We include these cases in each of the $M$ completed data sets.

[23] In complementary work in a regression context, potential matches can be aggregated using match probability estimates as weights as in Lahiri and Larsen (2005).

The between-implicate variance is

$$\hat{\sigma_B^2} = (M-1)^{-1} \sum_{m=1}^{M} \left(\hat{\theta}_m - \hat{\theta}\right)^2. \tag{8}$$

The total variance associated with $\hat{\theta}$ is

$$\hat{\sigma^2} = \hat{\sigma_W^2} + (1 + M^{-1})\hat{\sigma_B^2}. \tag{9}$$

# 4 Assessing model fit and linkage accuracy

In this section we implement our record linkage methodology by matching employed respondents in the 2010 wave of the HRS to the BR. We begin by showing selected partial effects of the EN-based employer-matching and establishment-matching models and compare the predictive accuracy of EN-based models with simpler logit models. We then show statistics that quantify the degree of linkage uncertainty under different types of blocking schemes. For non-EIN-blocked matches, we provide evidence that our threshold-based procedure reduces bias in imputed employer and establishment characteristics. Finally, we show characteristics of matched and unmatched respondents in the CenHRS.

## 4.1 ML matching model estimates

### 4.1.1 Partial effects of matching models

Figure 2 shows partial effects of JW scores for name and address on employer (top row) and establishment (bottom row) match probability. These partial effects plot the numerical derivative of the estimated model with respect to each JW score, holding all other predictors at their sample means. The confidence intervals represent posterior uncertainty in the parameters that index the matching model and are based on WBB replications of the training data. The first two graphs in each row show partial effects in EIN-blocked training data, while the second two graphs show partial effects in non-EIN-blocked data. Conditional on having EINs available for blocking, JW scores for name are informative about employer match status, while JW scores for address are not. The reverse is true for establishment match status, where isolating the right workplace from a set of potential workplaces loads more heavily on address information. In the absence of EINs, we see that JW scores for both name and address are important, although they matter only at very high levels of similarity.

The partial effects shown in Figure 2 underscore the value of using the EN estimator and relying on cubic splines with dense interactions to model match status. These higher-order terms capture sharp inflection points in the match likelihood, thereby mimicking nonlinearities in reviewer decisions that would be infeasible to replicate using a simpler parametric approach.

#### 4.1.2 Predictive performance evaluated using cross-validated ROC curves

We illustrate the predictive performance of our models by showing receiver operating characteristics (ROC) curves in Figure 3. For probability thresholds ranging from 0 to 1, the ROC curve plots the true positive rate on the vertical axis against the false positive rate on the horizontal axis. A model that was only as good as chance in classifying matches would have an ROC curve that ran along the 45-degree line, while a perfect classifier would have an ROC curve that hugged the left and top edges of the graph. The area under the curve (the c-statistic) would be 0.5 for the good-as-chance classifier, while it would be 1.0 for a perfect classifier.

The top row of Figure 3 compares employer and establishment match prediction performance using ROC metrics in training data that are blocked on EINs. The lower row shows analogous ROC metrics for training data that are blocked using location-specific variables (that is, in the absence of EINs). In each plot, the blue curve shows the performance of the EN estimator with the full suite of predictors, while the red curve shows the performance of a traditional logistic regression that uses only JW scores for name and address.[24] Each curve is constructed using tenfold cross validation to estimate out-of-sample fit. In all four settings, we see that EN outperforms logit, although the gain is not as pronounced for establishment matching when EINs are available.

### 4.2 Evaluating the linkage

#### 4.2.1 Quantifying match uncertainty for probabilistically linked respondents

Table 5 provides a simple way to summarize the degree of linkage uncertainty in our probabilistically matched sub-samples. The upper panel shows statistics for employer linkage, whereas the lower panel shows statistics for establishment linkage. In each panel, we divide respondents into four different groups. The first group, shown in the first column, refers to respondents who are probabilistically matched using EINs. The next set of columns shows respondents who are probabilistically matched without EINs using three different thresholds: the naive case of no threshold, the optimally chosen threshold, and an extreme threshold that delivers a precision rate of 80 percent.[25]

The first row of the table shows the fraction of HRS respondents for whom a single employer populated all 10 implicates; that is, there is no uncertainty about the linkage. Subsequent rows show the share of implicates associated with successively higher numbers of unique matches. Cases with four or more unique matches are binned together. With EIN-based linkage, almost 90 percent of respondents have no linkage uncertainty. In the absence of EINs, unrestricted sampling generates a very high level of uncertainty where nearly 80 percent of respondents are matched to 10 different employers. Applying thresholds, however, leads to a sharp reduction in linkage uncertainty, moving from what is effectively random matching to near-EIN levels of match quality when extreme thresholds are applied.

[24] The partial effects we show represent averages computed over 10 WBB replications of the training data. The 95 percent confidence intervals reflect posterior uncertainty in the parameters that index the matching models.

[25] Recall from Figure 1 that 80 percent precision is the approximate upper bound of what the location-based blocking variables and matching models can attain.

The statistics in the lower panel of the figure paint a qualitatively similar picture. Unsurprisingly, the overall degree of establishment-level linkage uncertainty is higher due to the added difficulty of finding the correct location in addition to the correct employer. With EINs, we see that approximately 45 percent of respondents have no linkage uncertainty at the establishment level, which is only about half of what we attain at the employer level. Nevertheless, the gains in linkage accuracy are very substantial once we use thresholds, as shown in the non-EIN columns.

The statistics in Table 5 summarize the degree of linkage uncertainty in the CenHRS and quantify the extent to which it can be mitigated using principled, data-driven techniques. Researchers might not be happy with this uncertainty, but making it explicit is clearly superior to choosing a deterministic procedure and proceeding as if the linkage were exact.

#### 4.2.2 Using thresholds reduces bias in imputed variables

In the previous section we showed how we use probability thresholds to improve linkage precision when EINs are unavailable. With an objective function to select optimal thresholds that places equal weight on precision and the linkage rate, we obtain a precision level of about 0.6. Although this is several times larger than what we would find without thresholds, it is still far from 1, which leaves open the possibility that employer- and establishment-level variables may be biased.

In the top panel of Figure 4, we show the relationship between MI-log employer size from the BR and true log employer size in our validation sample. We divide the true log employer-size distribution into 20 equally sized bins and plot the bin-level mean of the true value on the horizontal axis against the bin-level mean of the MI value on the vertical axis. An unbiased imputation procedure would accurately fit each ventile of an unobserved variable and therefore lie along the 45-degree line. We see that non-EIN-based linkage without thresholds generates biased imputations across the entire employer-size distribution and is therefore far from ideal. In contrast, non-EIN-based linkage with an optimally chosen threshold accurately imputes the missing variable of interest.

In the lower panel of Figure 4, we conduct the same exercise but with log establishment size as the target variable. As with the employer-level imputations, non-EIN-based linkage without thresholds is biased across the entire establishment-size distribution. In contrast, MI with an optimally chosen threshold is accurate in the lower half of the establishment-size distribution but understates true establishment size at extremely large workplaces. Although the confidence intervals widen substantially in the right tail, MI-log establishment size is understated, which is driven in part by the difficulty of finding extremely large establishments. In some instances, such as with public school districts or other types of public-sector employers, BR establishment data can represent aggregations of workplaces across different locations, so the meaning and use of the establishment match is less clear. Matches at the employer level are not subject to this problem.

#### 4.2.3 Individual characteristics for matched and unmatched respondents

Table 6 shows selected characteristics of employed HRS respondents in the 2010 wave. The first column shows statistics for the full sample, while the next four columns show statistics for linked

and non-linked sub-samples at the employer level and establishment level, respectively. Because the overall linkage rate is nearly 90 percent at both the employer level and establishment level, successfully linked sub-samples are broadly representative of the full sample of respondents. There are, however, systematic differences between the characteristics of linked and unlinked respondents that are informative about reasons for non-linkage. Moving down the rows of the table, one sees that unlinked respondents are less likely to be white and more likely to be Hispanic and foreign born. On average, they have about one less year of education, 10 to 25 percent lower annual earnings, and two to three fewer years of tenure with their employer relative to linked respondents. Markedly, while 16 to 20 percent of linked respondents are employed in the public sector, only 3 percent of unlinked respondents are employed in that sector. Finally, looking at the paradata, one sees that although there are no differences in the mode of interview, linked respondents are more likely to answer the survey instrument in English. Combined with the higher Hispanic and foreign-born share, this statistic indicates that immigrants are likely to be over-represented in the non-linked sub-sample.

The data presented in Table 6 point at two potential drivers for non-random linkage in the CenHRS. First, it is possible that non-linked respondents simply have less identifying information about their employers and therefore provide lower quality data to the HRS. Second, it is possible that these respondents intentionally withhold identifying information. The second possibility is consistent with the fact that non-linked respondents are non-consenters to the Social Security Administration linkages by construction and may therefore prefer to maintain a higher level of anonymity relative to consenters.

# 5 Application: The wage-size gradient

Using both household- and employer-level survey data as well as administrative employer-employee linked data, several studies establish that larger employers pay observationally equivalent workers higher wages compared with smaller employers (see, for example, Brown and Medoff (1989), Oi and Idson (1999), and Bloom et al. (2018)). In this section, we discuss an application of the CenHRS by re-examining the relationship between wages and establishment size. In particular, our approach reveals that non-sampling errors in survey data are correlated with workplace characteristics and would remain hidden without constructing linkages to administrative data.

## 5.1 Wage-size gradient in household-survey data

Consider the following statistical model for the relationship between worker wages and establishment size in the cross section:

$$w_{ij} = \gamma_0 + \gamma_1 s^*_{ij} + v_{ij}, \tag{10}$$

where $w_{ij}$ is the log hourly wage of worker $i$ employed at establishment $j$, $s^*_{ij}$ is an error-free measure of the log of the size of worker $i's$ establishment, and $v_{ij}$ is an error term that captures other factors influencing worker wages.[26] The HRS provides household- survey-based measures of hourly wages, $w_{ij}$, as well as establishment size. $s_{ij}$ is often missing and is potentially error ridden when it is reported. Survey-based measures of log establishment size can be written as

$$s_{ij} = s^*_{ij} + u_{ij}. \tag{11}$$

Under the classical measurement error model, discrepancies in survey reports are not systematically related to the underlying variable of interest, implying that $Cov(s^*_{ij}, u_{ij}) = 0$. Furthermore, reporting errors are not systematically related to the error term in equation (11), implying that $Cov(u_{ij,}v_{ij}) = 0$. Given this framework, it is well known that the presence of added noise in the explanatory variable attenuates the ordinary least squares (OLS) estimate of $\gamma_1$. Alternatively, if discrepancies in survey reports are systematically related to the underlying variable of interest—that is, if the measurement error is non-classical—then the OLS estimate of $\gamma_1$ may be either amplified or attenuated depending on the sign of $Cov(s^*_{ij}, u_{ij})$ and its magnitude relative to $V(u_{ij})$.

In the following subsection, we use our MI measures of establishment size to assess the relative importance of survey non-response and measurement error for the wage-size gradient and determine whether the measurement error is classical or non-classical.

## 5.2 Using MI variables from administrative data to assess bias in the wage-size gradient

Define $\hat{s}^{*(m)}_{ij}$ as the $m$-th implicate of log establishment size obtained using our MI-based procedure. We can write the true value of log establishment size under our imputation procedure as

$$s^*_{ij} = \hat{s}^{*(m)}_{ij} + \eta^{(m)}_{ij}. \tag{12}$$

Ignorability as posited in Equation (4) implies the following moment conditions:

$$Cov(\hat{s}^{*(m)}_{ij}, \eta^{(m)}_{ij}) = 0 \tag{13}$$

$$Cov(\hat{s}^{*(m)}_{ij}, v_{ij}) = 0; \tag{14}$$

that is, the imputed variable is uncorrelated with imputation error, $\eta^{(m)}_{ij}$, as well as the error in the regression model, $v_{ij}$.

[26] Although we ignore control variables when writing Equation (10), our empirical implementation includes control variables, which we describe in detail below. Control variables may be subject to measurement errors of their own, which makes the bias in the variable of interest difficult to characterize. We ignore the added effect of measurement errors in the control variables, to the extent they exist, in the discussion that follows.

The $\hat{\gamma}_1$ estimated using the $m$-th implicate of establishment size by OLS is

$$\begin{aligned}\hat{\gamma}_{1,\texttt{MI}}^{(m)} &= \frac{Cov(\hat{s}_{ij}^{*(m)}, \gamma_0 + \gamma_1 s_{ij}^* + v_{ij})}{V(\hat{s}_{ij}^{*(m)})} + o_p(1) \\ &= \gamma_1 \frac{Cov(\hat{s}_{ij}^{*(m)}, s_{ij}^*)}{V(\hat{s}_{ij}^{*(m)})} + o_p(1), \end{aligned} \tag{15}$$

where the second expression follows from Equation (14). Finally, from Equations (12) and (13) it follows that $Cov(\hat{s}_{ij}^{*(m)}, s_{ij}^*) = V(\hat{s}_{ij}^{*(m)})$, which implies that the estimate of $\hat{\gamma}_1$ based on MI-based variables is consistent.

Having shown the conditions under which estimates of $\gamma_1$ obtained from MI-based measures are consistent, we can quantify the relative importance of selective non-response and measurement error in the wage-size gradient. The survey-based estimate of $\gamma_1$ can be written as

$$\hat{\gamma}_{1,\texttt{S}} = \gamma_1 + \texttt{MEB} + \texttt{NRB} + o_p(1), \tag{16}$$

where MEB and NRB are biases due to measurement error and selective non-response, respectively. The wage-size gradient using MI measures, but restricted to the sample of respondents who do report establishment size, can be written as

$$\hat{\gamma}_{1,\texttt{MI|R}} = \gamma_1 + \texttt{NRB} + o_p(1). \tag{17}$$

We can then decompose the respective biases using Equations (15), (16), and (17) as

$$\hat{\gamma}_{1,\texttt{S}} - \hat{\gamma}_{1,\texttt{MI|R}} = \texttt{MEB} + o_p(1) \tag{18}$$

$$\hat{\gamma}_{1,\texttt{MI|R}} - \hat{\gamma}_{1,\texttt{MI}} = \texttt{NRB} + o_p(1). \tag{19}$$

In the equations above, $\hat{\gamma}_{1,\texttt{MI|R}}$ and $\hat{\gamma}_{1,\texttt{MI}}$ average across the $M$ parameter estimates obtained from each of the completed data sets.

Table 7 shows estimates of $\hat{\gamma}_{1,\texttt{S}}$, $\hat{\gamma}_{1,\texttt{MI|R}}$, and $\hat{\gamma}_{1,\texttt{MI}}$. The top panel uses the full sample of linked data, which combines deterministic links and probabilistic links, while the lower panel is restricted to the sample that is deterministically linked. Standard errors for MI-based estimates jointly account for within- and between-implicate variability using Rubin's combining formulas. To control for variation in individual characteristics that affect hourly wages, all the regression models we estimate condition on age, gender, race, Hispanic ethnicity, partnered/coupled status, years of education, tenure, hours worked per week, weeks worked per year, one-digit occupation fixed effects, and one-digit industry fixed effects. We focus on the variable of interest imputed from the BR and do not report coefficients for these control variables in the table.

We see that $\hat{\gamma}_{1,\texttt{S}}$ is significantly larger than $\hat{\gamma}_{1,\texttt{MI|R}}$ in both the full sample and the perfectly matched sub-sample. Amplification bias in the survey-based coefficient is consistent with HRS

respondents' reports of workplace size being subject to non-classical measurement error. Moving next to compare $\hat{\gamma}_{1,\texttt{MI|R}}$ with $\hat{\gamma}_{1,\texttt{MI}}$, we see that selective non-response weakly contributes to amplification bias, as the estimated coefficient either remains unchanged or shrinks when moving from the non-missing self-report sample to the full sample. For each pair of coefficients, we see the same qualitative pattern play out in the sample of all linked respondents and also in the perfectly matched sub-sample, thereby indicating that our findings are not merely artifacts of the linkage process. In the broader sample of all linked respondents, non-classical measurement error is responsible for all of the amplification bias. In the perfectly matched sub-sample, non-classical measurement error and non-response are each responsible for about 50 percent of the amplification bias.

Figure 5 illustrates the nature of non-sampling errors in the full sample (left column) and the perfectly matched sub-sample (right column). In each graph, we divide MI-log establishment size from the BR into 20 equally sized bins and plot the bin-level mean on the horizontal axis. In the top row of the figure, we plot the bin-level mean of log establishment size reported by HRS respondents on the vertical axis. The full sample of linked respondents is shown in the left panel, and the deterministically linked sub-sample is shown in the right panel. The bin scatter plot shows that measurement error in survey reports is *negatively* correlated with the true values; that is, workers at the smallest establishments overstate employer size, and workers at larger establishments understate employer size. In the lower tail, differences between survey and administrative data could reflect seasonal volatility in small establishments.[27] In the upper tail, where errors are more pronounced and size is less volatile over calendar months, the pattern is consistent with the idea that individual employees may be unaware of the full scale of operations and may therefore underestimate workplace size when answering the survey. With respect to non-response, we see that it is largely uncorrelated with size for the broader sample of all linked respondents but negatively correlated with size in the perfectly matched sub-sample.

The estimates we report here provide new evidence on how household survey responses about workplace characteristics are selectively misreported or not reported at all. With linkages to administrative information on workplaces in the CenHRS, we are able to characterize measurement and non-response errors that are not observed in other household-survey data sets.

## 6 Conclusion

This paper describes the construction of a new data set, the CenHRS, which is obtained by linking a household-level survey to an establishment-level frame in the absence of unique identifiers. The between-frame linkage task that we undertake is complicated by skewness in the distribution of employment across firms that makes matching much more difficult. We address these issues by using probabilistic linkage based on supervised machine learning models to estimate the probability that specific employers and establishments in the BR are matches for individuals in the HRS. Our models rely on a rich set of predictors and a high degree of flexibility to replicate important

[27] BR size information is based on payroll tax information reported in March, while three-quarters of the HRS interviews are conducted from May through September.

nonlinearities inherent in human-reviewed training data. Using probabilities estimated from the models, we employ multiple imputation to characterize uncertainty in the linkage. To further refine the set of candidate matches, we estimate probability thresholds that provide the best trade-off between precision and the sample linkage rate. Eliminating candidate matches that fail to meet these thresholds dramatically reduces both linkage uncertainty and bias in the imputed variables. We use these newly linked data to provide new evidence that reporting errors and non-response propensity vary systematically with workplace characteristics.

Beyond issues related to record linkage, the CenHRS opens new avenues for research by extending preexisting measures of activities, experiences, and outcomes for individuals from their family and home context to the work context. These new measures will provide data necessary for a more comprehensive understanding of the determinants of health and well-being over the lifespan.

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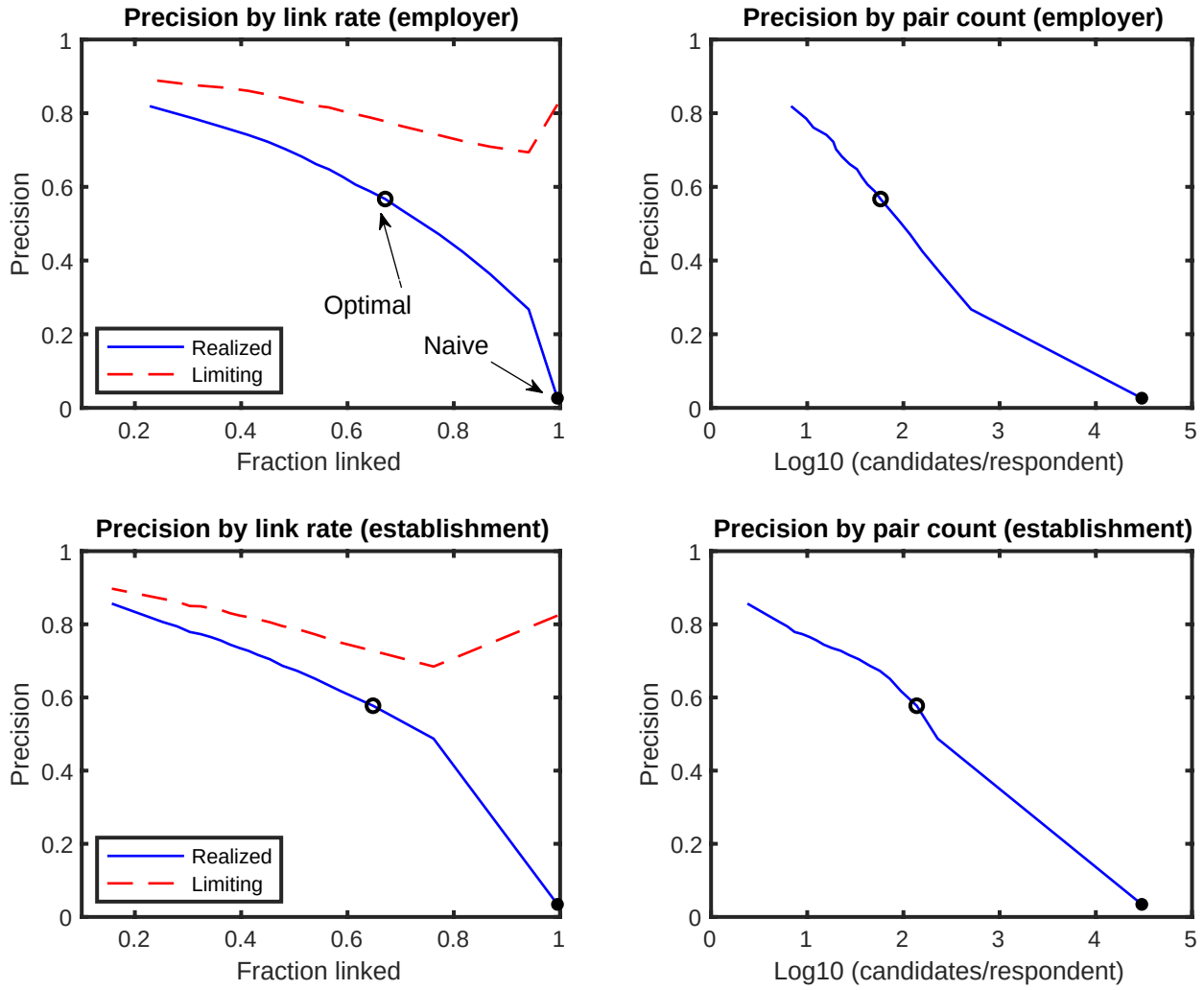
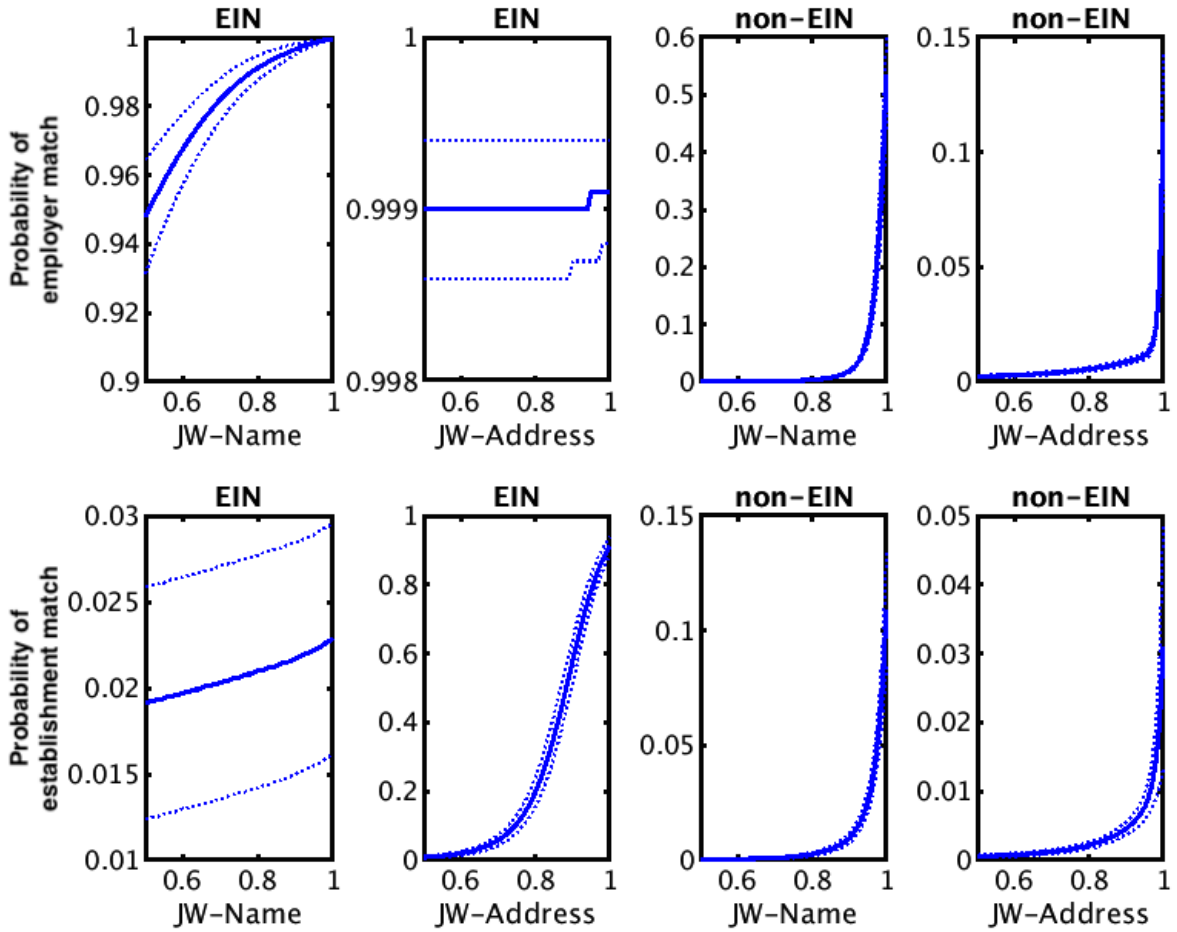
**Figure 1:** Precision rates in non-EIN-based record linkage



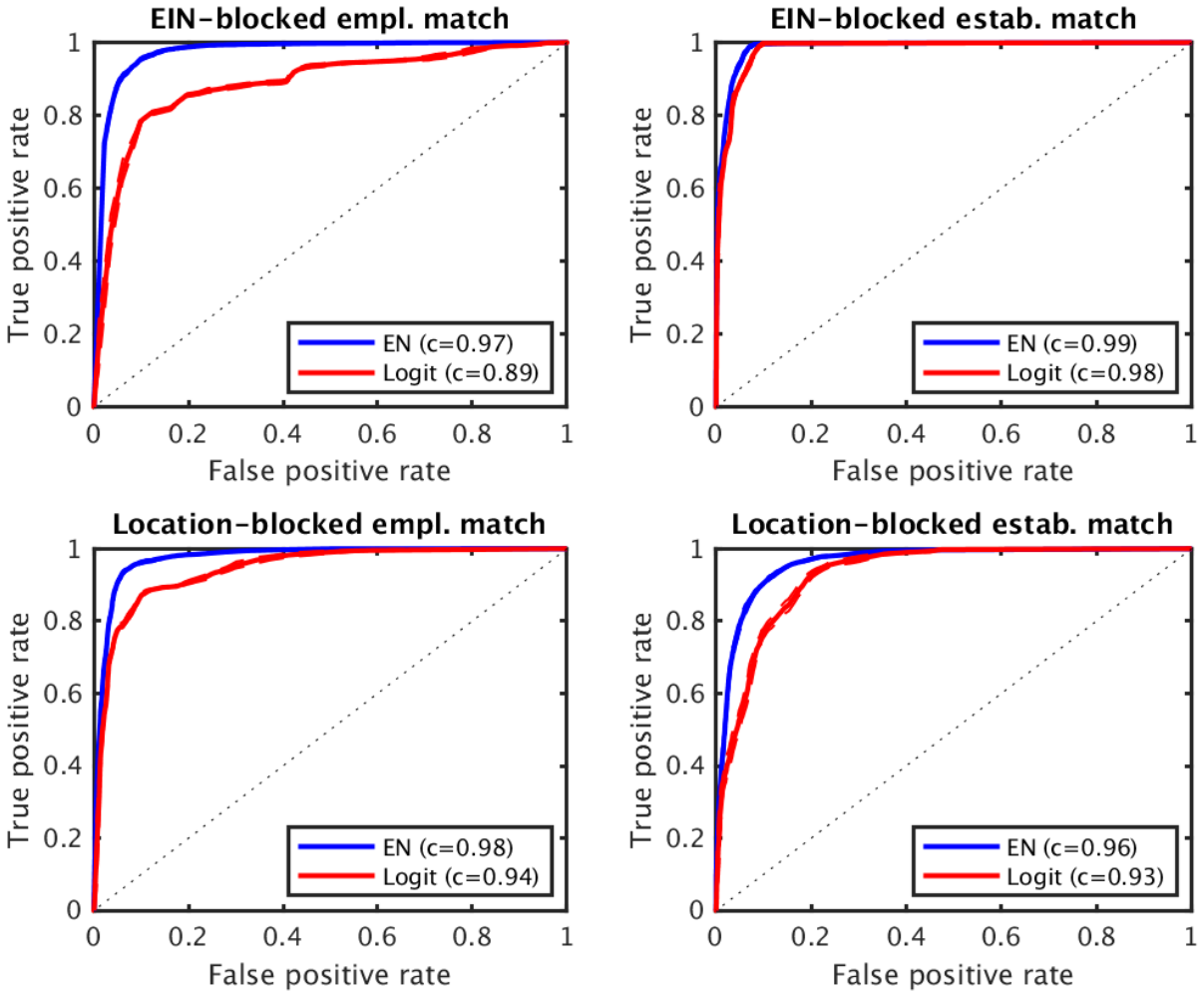

**Notes:** This figure shows the precision rate (that is, the fraction of HRS respondents correctly matched) attained under different probability thresholds in a validation sample. The realized precision rate is the rate achieved by the EN estimator for different probability thresholds. The limiting precision rate is the upper bound on precision attained by a hypothetical estimator that selects all available true matches after blocking is complete. Statistics are averages across 10 WBB replications.

**Figure 2:** Selected partial effects of the matching models



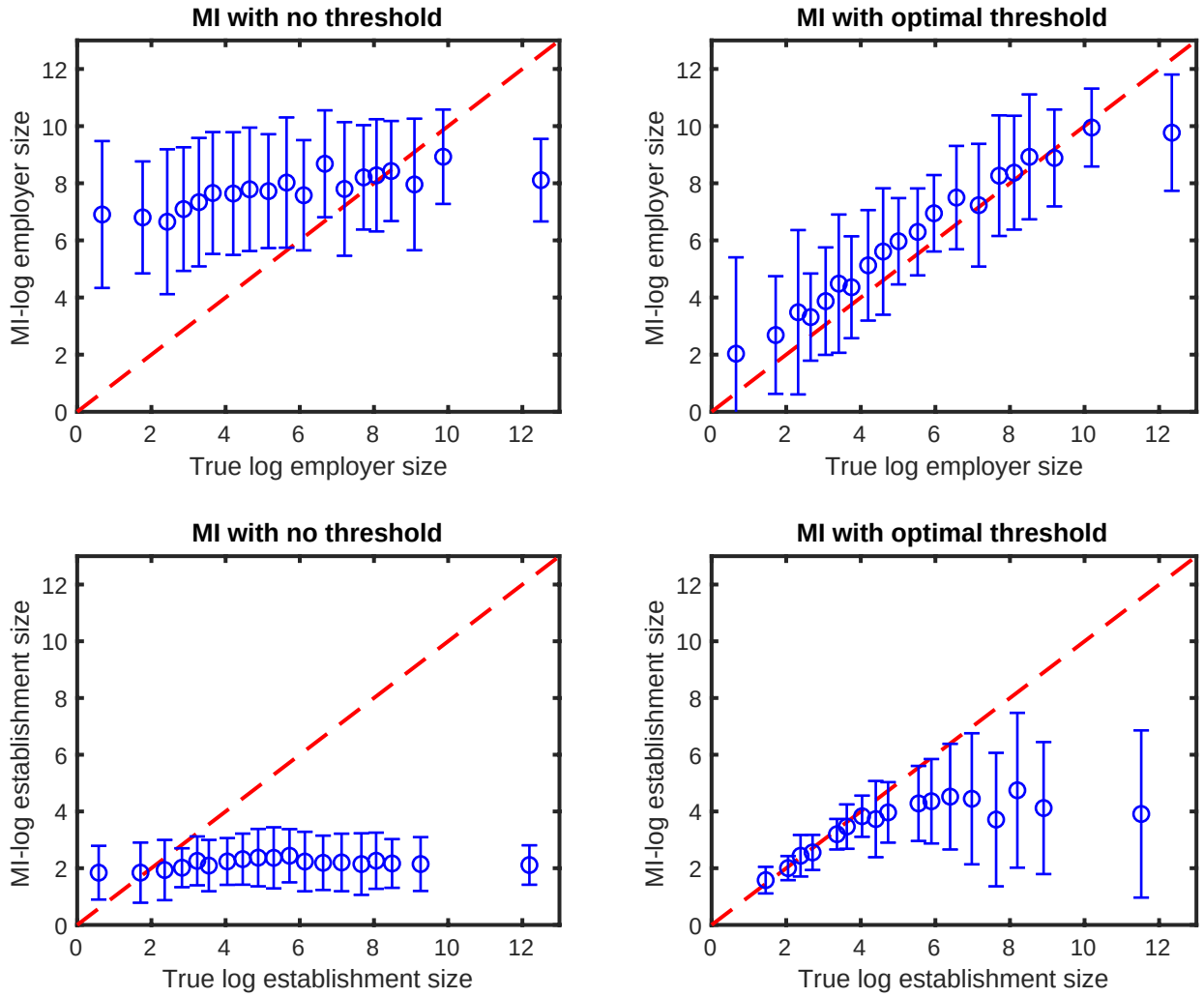

**Notes:** This figure shows partial effects of the matching models for a given predictor, holding all other predictors at their mean. The vertical axes have different scales in each graph. The top row shows the effect of Jaro-Winkler (JW) scores for name and address similarity between the HRS and BR on the probability of employer match status, separately for EIN- and non-EIN-blocked training data. The bottom row shows the effect of the same predictors on establishment match status, separately for EIN- and non-EIN-blocked training data. The 95 percent confidence intervals reflect posterior uncertainty in the parameters of the matching models and are estimated using Bayesian bootstrap replications of the training data.

**Figure 3:** Receiver operating characteristic curves of the matching models



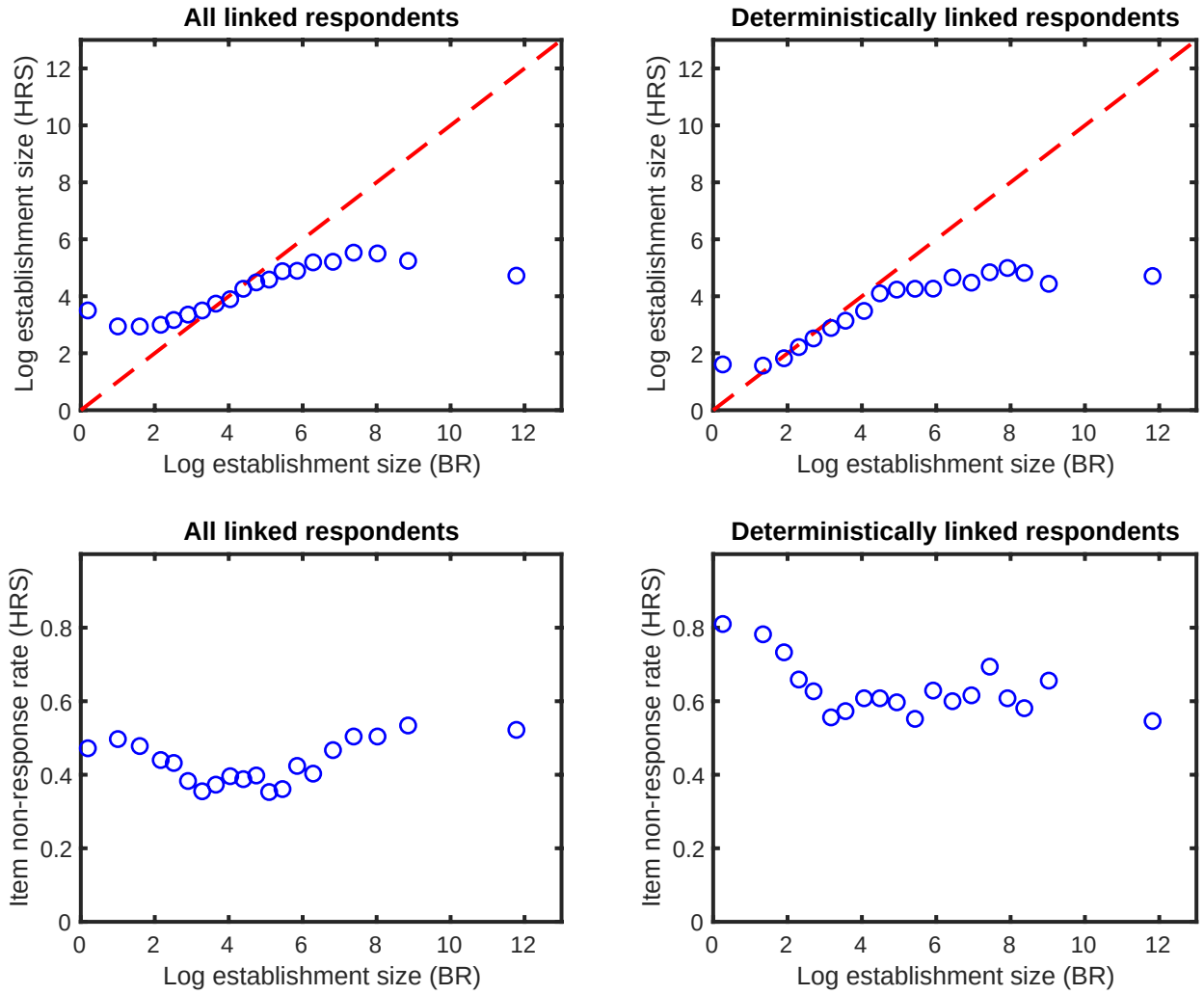

**Notes:** Receiver operating characteristic (ROC) estimates show out-of-sample predictive performance of each model, which is estimated using tenfold cross-validation. EN represents the elastic net estimator, which uses the full suite of predictors, while Logit is the conventional logistic regression estimated using only Jaro-Winkler scores for name and address. C-statistics show the area under each curve, which ranges from a minimum of 0.5 (random classifier) to a maximum of 1.0 (perfect classifier). The 95 percent confidence intervals reflect posterior uncertainty in the parameters of the matching models and are estimated using Bayesian bootstrap replications of the training data.

**Figure 4:** Reducing imputation bias by applying optimally chosen thresholds




**Notes:** This figure shows the relationship between true log size and multiply-imputed log size at the employer and establishment levels for naively and optimally chosen implicates in a validation sample. The 95 percent confidence intervals for MI measures jointly account for within- and between-implicate variability using Rubin's combining formulas. Some cells are suppressed for statistical disclosure limitation.

**Figure 5:** Non-sampling errors in HRS reports of establishment size




**Notes:** This figure shows the relationship between log establishment size from the BR and self-reported log establishment size from the HRS in the top row and item non-response rates about establishment size in the HRS in the bottom row.

**Table 1:** Types of record linkage in the 2010 wave of the CenHRS

| | HRS respondents | Share of respondents | BR candidates per HRS respondent (average number of establishments) |
|---|---|---|---|
| Deterministic match, EIN available | 2500 | 0.415 | 1 |
| Probabilistic match, EIN available | 1800 | 0.295 | 436 |
| Probabilistic match, EIN not available | 1800 | 0.291 | 30,050 |

**Notes:** This table shows the record linkage strategy used for different sub-samples of working HRS respondents in the 2010 wave. Shares may not sum to 1 because each cell is independently rounded. In cases where EINs are not available, the average number of BR candidates per HRS respondent is based on blocking a validation sample (that is, where HRS respondents can be deterministically matched to BR establishments) using location-specific variables.



**Table 2:** Reviewer's information set

| | Source | |
|---|---|---|
| Review variable | HRS | BR |
| Employer's name, establishment address, phone number | ✓ | ✓ |
| Whether employer is single unit or multi-unit | | ✓ |
| Employer and establishment size | ✓ | ✓ |
| Employer's industry description | ✓ | ✓ |
| Respondent's occupation description | ✓ | |
| Provision of health insurance and/or retirement plan | ✓ | |
| Number of EINs in respondent's earnings record | ✓ | |

**Notes:** This table shows the types of variables that reviewers observe when determining match status for an HRS-BR pair in the training data sets. As shown in the source column, some variables are available in both the HRS and the BR, while others are available in only one of the two sources.

**Table 3:** Predictors in the matching models

| Predictor | Description |
|---|---|
| Pair level | |
| Cubic spline Jaro-Winkler score name | Similarity between HRS and BR name |
| Cubic spline Jaro-Winkler score street address | Similarity between HRS and BR address |
| Jaro-Winkler score city | Similarity between HRS and BR city |
| Cubic spline establishment's share of employment within blocking variable | Concentration of employment across candidate establishments |
| Cubic spline EIN share of respondent's total annual earnings | Concentration of earnings across candidate employers |
| Agreement on 7-digit and 10-digit phone number | Agreement status binary variable |
| Agreement on 3-, 4-, 5-digit Zip code, city-state | Agreement status binary variable |
| Agreement on one-digit industry code | Agreement status binary variable |
| Agreement on employer size class | Agreement status binary variable |
| BR only | |
| Log employer size | |
| Whether single-unit or multi-unit employer | |
| HRS only | |
| Age, gender, race, ethnicity, nativity, years of schooling, marital status | |
| Survey interview mode and language | |
| Log hourly real wage, tenure, weeks worked/year, hours worked/week | |
| Provision of health insurance, provision of retirement plan | |
| Two-digit occupation, one-digit industry | |

**Notes:** This table shows the types of predictors used in the elastic net matching models. Pair-level predictors are based on information that is specific to the HRS-BR pair. BR-only predictors are derived purely from the BR, and HRS-only predictors are derived purely from the HRS. Cubic splines for name and address Jaro-Winkler (JW) scores have 10 cut points each. The cubic splines for block share and earnings share have three cut points each. Earnings shares across jobs cannot be computed for respondents who do not consent to SSA linkage. For respondents who do not consent to linkage, we use the cubic spline of log BR size with three cut points. All cubic spline variables are fully interacted with each other. After interaction terms and indicator variables are included to account for missing values of HRS variables, there are a total of 9,200 predictors for the EIN- and non-EIN-based models.

**Table 4:** Precision and recall for non-EIN-based record linkage

| | Probability threshold | Proportion linked | Realized precision | Realized recall | Limiting precision | BR candidates per HRS respondent (average number of establishments) |
|---|---|---|---|---|---|---|
| Employer-level linkage | | | | | | |
| Naive | 0 | 1 | 0.026 | 0.032 | 0.824 | 30,050 |
| Optimal | 0.39 | 0.648 | 0.587 | 0.745 | 0.788 | 52.3 |
| Establishment-level linkage | | | | | | |
| | Probability threshold | Proportion linked | Realized precision | Realized recall | Limiting precision | BR candidates per HRS respondent (average number of establishments) |
| Naive | 0 | 1 | 0.034 | 0.042 | 0.824 | 30,050 |
| Optimal | 0.095 | 0.661 | 0.569 | 0.786 | 0.724 | 146.8 |

**Notes:** This table shows linkage performance without EINs under the naive case where no probability threshold is applied and the case where the optimally chosen probability threshold is applied. The statistics shown are computed in a validation data set where HRS respondents can be deterministically matched to BR establishments. The top panel shows statistics for employer-level linkage, and the lower panel shows statistics for establishment-level linkage. Precision is the proportion of HRS respondents who are correctly matched. Recall is the proportion of correct matches in the BR that are selected. Limiting precision equals limiting recall by definition. Statistics are averages across 10 WBB replications.

**Table 5:** Concentration of multiple implicates

| Employer-level linkage | | | | |
|---|---|---|---|---|
| | | Non-EIN-based | | |
| $N$ (unique implicates) | EIN-based | No threshold | Optimal threshold | Extreme threshold |
| 1 | 0.89 | 0.1 | 0.44 | 0.83 |
| 2 | 0.09 | 0.05 | 0.23 | 0.13 |
| 3 | 0.01 | 0.05 | 0.14 | 0.03 |
| 4-10 | 0.01 | 0.79 | 0.19 | 0.02 |
| $N$ (respondents) | 1800 | 1800 | 1200 | 350 |
| Establishment-level linkage | | | | |
| | | Non-EIN-based | | |
| $N$ (unique implicates) | EIN-based | No threshold | Optimal threshold | Extreme threshold |
| 1 | 0.44 | 0.01 | 0.27 | 0.69 |
| 2 | 0.17 | 0.01 | 0.19 | 0.14 |
| 3 | 0.06 | 0.01 | 0.13 | 0.08 |
| 4-10 | 0.32 | 0.97 | 0.41 | 0.1 |
| $N$ (respondents) | 1800 | 1800 | 1100 | 300 |

**Notes:** This table shows the concentration of implicates across HRS respondents. For non-EIN-based linkage, the table shows the concentration of implicates across respondents for three different thresholds: no threshold (or the naive case), the optimally chosen threshold, and an extreme threshold, which is associated with a precision rate of 80 percent in the validation data.

**Table 6:** HRS respondent characteristics by linkage status

| | | Employer | | Establishment | |
|---|---|---|---|---|---|
| | Full sample | Linked | Non-linked | Linked | Non-linked |
| Age | 57.61 | 57.63 | 56.92 | 57.72 | 56.54 |
| Male | 0.45 | 0.44 | 0.48 | 0.44 | 0.50 |
| White | 0.67 | 0.68 | 0.57 | 0.68 | 0.59 |
| Black | 0.22 | 0.22 | 0.24 | 0.22 | 0.23 |
| Other race | 0.11 | 0.10 | 0.19 | 0.10 | 0.17 |
| Hispanic | 0.15 | 0.14 | 0.26 | 0.14 | 0.25 |
| Partnered/coupled | 0.73 | 0.72 | 0.73 | 0.72 | 0.73 |
| Years of schooling | 13.24 | 13.43 | 12.06 | 13.45 | 12.11 |
| Native born | 0.85 | 0.87 | 0.69 | 0.86 | 0.73 |
| Annual earnings ($) | 41,800 | 43,160 | 33,330 | 42,620 | 37,660 |
| Hours worked per week | 38.14 | 38.34 | 36.47 | 38.24 | 37.36 |
| Weeks worked per year | 48.77 | 48.95 | 47.04 | 48.81 | 47.98 |
| Tenure (years) | 11.22 | 11.79 | 8.23 | 11.62 | 9.24 |
| Public sector worker | 0.17 | 0.21 | 0.03 | 0.21 | 0.03 |
| Interviewed in English | 0.93 | 0.94 | 0.81 | 0.94 | 0.84 |
| Interviewed in person | 0.74 | 0.75 | 0.76 | 0.74 | 0.76 |
| $N$ | 6100 | 5600 | 750 | 5400 | 850 |

**Notes:** This table shows HRS respondent characteristics for the full sample of working respondents in the 2010 wave and for the linked and non-linked sub-samples at the employer and establishment level. Annual earnings are in 2010 dollars. Case counts are independently rounded.

**Table 7:** Log establishment size effect in log wage

| | A: All linked respondents | | |
|---|---|---|---|
| | | MI-size from the BR | |
| | HRS self-report of size ($\hat{\gamma}_{1,\mathtt{S}}$) | Non-missing self-report sample ($\hat{\gamma}_{1,\mathtt{MI|R}}$) | Full sample ($\hat{\gamma}_{1,\mathtt{MI}}$) |
| | 0.042 | 0.019 | 0.019 |
| | (0.005) | (0.004) | (0.003) |
| $N$ | 2700 | 2700 | 4400 |
| | B: Deterministically linked respondents | | |
| | | MI-size from the BR | |
| | HRS self-report of size ($\hat{\gamma}_{1,\mathtt{S}}$) | Non-missing self-report sample ($\hat{\gamma}_{1,\mathtt{MI|R}}$) | Full sample ($\hat{\gamma}_{1,\mathtt{MI}}$) |
| | 0.044 | 0.033 | 0.023 |
| | (0.009) | (0.006) | (0.005) |
| $N$ | 850 | 850 | 1800 |

**Notes:** This table shows the effect of log establishment size on log wages. Regression samples are restricted to observations where HRS respondents reported hourly wages or provided sufficient information to infer hourly wages from reports of total earnings and total hours. All regression models include controls for weekly hours, annual weeks, tenure, years of schooling, partnered/coupled status, nativity, gender, race, Hispanic ethnicity, age, one-digit occupation fixed effects, and one-digit industry fixed effects.

# Appendix A Constructing the training data set

Our training samples are composed of HRS-BR pairs generated by blocking the 1998 and 2004 waves of the HRS with the BR. The first sample blocks on EIN and is used to fit probabilistic matching models for cases where EINs are available, while the second sample blocks on 10-digit phone number, 3-digit Zip code, telephone area code, and city-state and is used to fit probabilistic matching models for cases where EINs are unavailable. We choose 1998 and 2004 to create the training samples for two reasons. First, these were years in which the HRS drew fresh cohorts of survey respondents. Second, the file structure of the BR changed in substantive ways in 2002. Therefore, using HRS cohorts before and after 2002 to estimate the matching models allows us to account for unobserved variation in the quality of data drawn from the BR.

Simple random sampling of pairs for human review would produce very few true matches and therefore limit the predictive performance of our models. Instead, we follow a stratified random sampling approach to draw candidate matches (see, for example, Christen (2012)). We begin by computing Jaro-Winkler (JW) scores for name and address similarity for each pair. We then divide the JW scores for name and address into four bins each, with grid points spaced closer together at the right tail of the respective JW score distributions. This binning exercise defines 16 strata from which we draw equally sized samples to obtain a total sample size of $N^T \approx 1000$ pairs. Because the bins are concentrated at the top of the JW name- and address-score distribution, this stratified sampling methodology substantially increases the share of true matches in the training data set relative to a simple random sample.

# Appendix B Model selection

Our training data sets consist of approximately 2,000 observations each, which is substantially smaller than the number of predictors available to estimate match probabilities. To solve this dimensionality problem and, more importantly, to avoid over-fitting our model, we use ML tools to aid in prediction. While a complex model with many variables and interactions has the potential of reducing in-sample (training) errors substantially, this improvement is misleading because it considers the wrong model-fit criterion. To ensure that the model generalizes well, we consider out-of-sample (test) error that we estimate using tenfold cross validation.

In our setting, the complexity of the model is indexed by the number of predictors. Reducing model complexity by shrinking the number of predictors increases the bias component of the test error but has the potential to reduce the variance component substantially. In order to obtain a model with the optimal degree of complexity, we employ the elastic net (EN) shrinkage estimator.

The EN estimator solves the constrained maximum likelihood problem posed in Equation (20):

$$\max_{\beta\in\mathbb{R}^q} \sum_{l=1}^{2N^T} w_l^{(m)} \left( y_l \log\left(\frac{\exp(\tilde{\boldsymbol{x}}_l'\boldsymbol{\beta})}{1+\exp(\tilde{\boldsymbol{x}}_l'\boldsymbol{\beta})}\right) + (1-y_l)\log\left(\frac{1}{1+\exp(\tilde{\boldsymbol{x}}_l'\boldsymbol{\beta})}\right)\right)$$
$$\text{st: } \sum_{p=1}^{q}\beta_p^2 \le t_1,\ \sum_{p=1}^{q}|\beta_p| \le t_2, \tag{20}$$

where $l$ indexes observations in the training data set, and $p$ indexes predictors. In Equation (20), the typical maximum likelihood problem is supplemented with two constraints, each of which constitutes a tuning parameter for the estimator. Together, these tuning parameters control the level of model complexity: $t_1$, as in ridge regression, sets a threshold on the sum of squared values of the coefficients. The ridge penalty term has the effect of controlling the variance component of test error by preventing any one predictor from exhibiting too strong of an effect on the outcome. This penalty is important when some predictors are correlated. $t_2$, as in the LASSO, sets a threshold on the sum of the absolute values of the coefficients. When this second constraint binds, some of the coefficients are set exactly to zero, thereby reducing the complexity of the model.

To find the optimal model, we recast the EN estimator in Lagrangian form, as shown in Equation (21). The two tuning parameters discussed above are replaced by a Lagrange multiplier, $\lambda \in \mathbb{R}_+$, and a parameter $\alpha \in [0,1]$ that controls the degree of mixing between the ridge constraint and the LASSO constraint:

$$\max_{\beta\in\mathbb{R}^q} \sum_{l=1}^{2N^T} \overbrace{w_l^{(m)} \left( y_l \log\left(\frac{\exp(\tilde{\boldsymbol{x}}_l'\boldsymbol{\beta})}{1+\exp(\tilde{\boldsymbol{x}}_l'\boldsymbol{\beta})}\right) + (1-y_l)\log\left(\frac{1}{1+\exp(\tilde{\boldsymbol{x}}_l'\boldsymbol{\beta})}\right)\right)}^{\ell(y_l,\tilde{\boldsymbol{x}}_l;\boldsymbol{\beta})}$$
$$+\lambda\sum_{p=1}^{q}\left(\alpha|\beta_p| + (1-\alpha)\beta_p^2\right). \tag{21}$$

We obtain prediction models by implementing the EN estimator using the `glmnet` package in `R`. This particular implementation of the EN estimator takes a given value of $\alpha$ and finds the value of $\lambda$ that delivers the lowest out-of-sample (test) deviance, which is defined as:

$$-2\sum_{f=1}^{10}\sum_{l\in f}\ell\left(y_{lf},\tilde{\boldsymbol{x}}_{lf};\hat{\boldsymbol{\beta}}_{f'}(\alpha,\hat{\lambda})\right). \tag{22}$$

In Equation (22), $f$ indexes 10 equally sized random partitions (folds) of the data. $\ell(\cdot)$ represents the log likelihood as defined in Equation (21). $\tilde{\boldsymbol{x}}_{lf}$ is the vector of predictors for observation $l$ in fold $f$, $\hat{\boldsymbol{\beta}}_{f'}(\alpha,\hat{\lambda})$ is the parameter vector estimated using observations on all folds except for fold $f$, and $\hat{\lambda}$ is the test deviance-minimizing choice of $\lambda$. To obtain the best prediction model, we perform a grid search by iterating $\alpha$ from 0.05 to 0.95 in 0.05-unit increments and select the model with the lowest test deviance across all the values of $\alpha$.