Online Appendix

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Measuring Employer-to-Employer Reallocation

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1 Other variables in the monthly CPS that derive from Dependent Interviewing

The main reason to introduce Dependent Interviewing was (mis)measurement of occupation and industry coding. The EMPSAME question is just an instrument designed to improve this measurement, and produced, as a fortunate by-product, information on EE transitions. Besides mobility between employers, occupations and industries, the only other variables that are potentially affected by Dependent Interviewing are indeed the reason for nonparticipation (retirement and disability) and unemployment duration. We examine them in more detail. To summarize our findings, the main difference vis-a-vis the EMPSAME question is that for nonparticipation and unemployment duration the Census also asks a battery of additional questions, which only pertain to the current month and are not part of Dependent Interviewing (thus, are immune to the RIP). These additional questions allow to detect status of retirement, disability, or unemployment duration to date, by-passing the RIP. In other words, these questions always allow to independently code these three states (unemployment duration, retirement, and disability). This is not possible for employer change. Therefore, the RIP impacts only employer change (and industry and occupation thereof) in a major way. In

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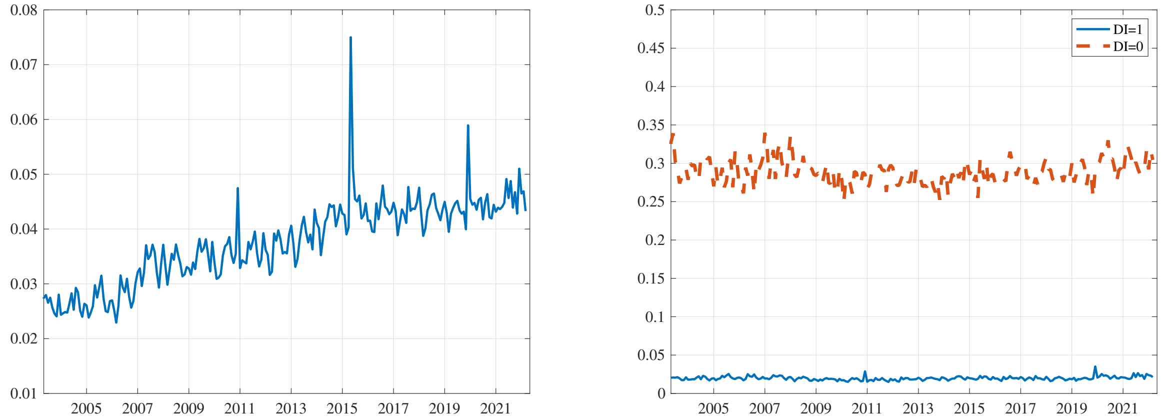
addition, while Self-Employed status is not detected through Dependent Interviewing, we find evidence that its independent coding is noisier when the answer to the EMPSAME question is missing, one more indirect ramification of the RIP.

1.1 Occupation and industry mobility

Missing answers to the EMPSAME question, just like negative answers, trigger independent coding of an employed worker's industry and occupation of employment, which tend to inflate industry and occupational mobility. Indeed, the EMPSAME question was introduced in 1994 precisely to allow dependent coding and deflate spurious mobility. This is an even more serious problem after a missing answer than after a negative answer, because, in the former case, a worker is likely to have stayed with the same employer anyway and changed nothing. We quantify here the implications of the RIP for mobility across industries and occupations.

Independent coding of the worker's occupation and industry, which was the norm pre-1994 redesign and only applied to job switchers post-1994, inflates switching probabilities between occupations and industries by a factor of ten, from about to 3% to about 30%. See Moscarini and Thomsson (2007) for evidence throughout 2006. Most concerning is the impact of independent coding after a missing answer to EMPSAME since 2007, when we know that many job stayers are likely to be censored by the RIP, and the incidence of independent coding soars. As a result, average occupational mobility in the monthly CPS takes off in 2007, as the interviewing changes are introduced and missing answers jump up, and then continues to increase for other reasons that we did not investigate. Figure OA1(a) shows average occupational mobility of consecutively employed workers (eligible for the EMPSAME question), and Figure OA1(b) conditions on valid (left) and missing (right) response to the EMPSAME question (DI=1,0). As is clear from these figures, the probability of transition between occupations that are independently coded (because of missing answer to EMPSAME) averages about 30% per month after 2005, about the same as before 1994 when all occupations were independently coded, and ten times that of dependently coded occupations. The upward trend in measured occupational mobility in panel (a) thus reflects a pure composition effect: the increasing share of the eligible population that belong in the upper line of panel (b).



(a) Overall Switching Probability

(b) Switching Probability by DI Status

Figure OA1: Occupation Switching

1.2 Self-Employment status, and change thereof

In the CPS questionnaires, IO1INT is the question that directly asks if the worker is self-employed or not. There are a few follow-up questions such as IO1INC, which asks whether the business is incorporated or not. The public-use micro data only make available two recoded class-of-worker variables, PEIO1COW and PRCOW1:

PEIO1COW INDIVIDUAL CLASS OF WORKER CODE ON FIRST JOB
NOTE: A PEIO1COW CODE CAN BE ASSIGNED EVEN IF AN INDIVIDUAL IS NOT CURRENTLY EMPLOYED.
VALID ENTRIES:

1 GOVERNMENT - FEDERAL
2 GOVERNMENT - STATE
3 GOVERNMENT - LOCAL
4 PRIVATE, FOR PROFIT
5 PRIVATE, NONPROFIT
6 SELF-EMPLOYED, INCORPORATED
7 SELF-EMPLOYED, UNINCORPORATED
8 WITHOUT PAY

The distinction and transition between incorporated and unincorporated self-employed is subject to change of classification based on tax shield considerations. The second variable provides a coarser classification.

PRCOW1 CLASS OF WORKER
RECODE - JOB 1
VALID ENTRIES:
 1 FEDERAL GOVT
 2 STATE GOVT
 3 LOCAL GOVT
 4 PRIVATE (INCL. SELF-EMPLOYED INCORP.)
 5 SELF-EMPLOYED, UNINCORP.
 6 WITHOUT PAY

We consider the coarser variable PRCOW1 here, because it is the one that the BLS uses to compile the official tabulation of the monthly employment report. For illustration, we examine data in 2018-2019 and report results here.

First, compare the probability of switching from self-employed to employee, and vice versa, conditional on a valid (DI=1) or missing (DI=0) answer to the EMPSAME question among eligible records. In either case, the monthly probability of switching in either direction is over 2% for DI=0 and just .13% for DI=1. Either this change to/from self-employment is strongly correlated with a missing answer to the employer name (DI=0), through an unobserved latent variable affecting both, or the missing answer itself triggers an especially noisy independent coding of self-employment status. Only in the first case self-employment status change could provide independent information that is useful for our imputation of an EE transition. But we do not know which case is more prevalent.

Second, focus on valid answers to the EMPSAME question (DI=1), the .13% of switchers in either direction from/to self-employment for whom we do have an answer to EMPSAME. About 30% of these switchers, in either direction, report EMPSAME=YES, i.e., they work for the same employer as in the previous month. If we took these numbers at face value, we would conclude that almost 1/3 of the workers who leave or join self-employment do so by just changing their formal classification, to and from being a gig worker, but they still do their work for the same company. We find this number implausibly high. We conclude that we cannot use the self-employment status variable to improve our imputation.

1.3 Nonparticipation: Retirement

For inactivity/nonparticipation, the goal of Dependent Interviewing simply seems to be to reduce interview burden: the Census use last month's answer on status in inactivity to impute this month's status. This is from Technical Paper 66, pp. 6-7 (Link):

> Dependent interviewing for people reported to be retired, disabled, or unable to work. The revised questionnaire also is designed to use dependent interviewing for individuals reported to be retired, disabled, or unable to work. An automated questionnaire increases the ease with which information from the previous month's interview can be used during the current month's interview. Once it is reported that the person did not work during the current month's reference week, the previous month's status of retired (if a person is 50 years of age or older), disabled, or unable to work is verified, and the regular series of labor force questions is not asked. This revision reduces respondent and interviewer burden.

Two key variables provide information about reasons for nonparticipation: PURETOT (RETIREMENT STATUS) and PEMLR (MONTHLY LABOR FORCE RECODE). We describe them in turn.

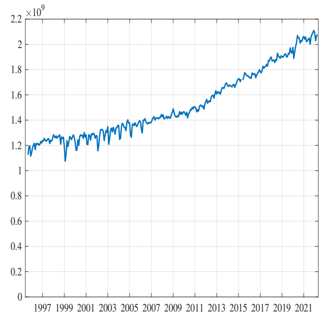
Consider the answer to the following dependent coding question on retirement status in the second month of the matched data.

PURETOT RETIREMENT STATUS
(LAST MONTH YOU WERE REPORTED TO BE RETIRED, ARE YOU STILL RETIRED THIS MONTH?)
VALID ENTRIES:

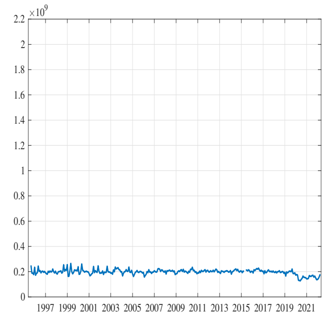
1 YES
2 NO
3 WAS NOT RETIRED LAST MONTH

If the answer is 1=YES, then no more questions are asked about labor force experience, to ease the burden. So, in this case, Dependent Interviewing is designed to reduce the length of the interview, not to increase the precision of the information, as in the occupation/industry case . "Out of the universe", which include individuals who were not retired last month, and missing answers to this question are reported as -1. The answer 3=WAS NOT RETIRED LAST MONTH indicates that the individual had been erroneously indicated as retired last month, so the answer corrects the very premise of the question ("LAST MONTH YOU WERE....").

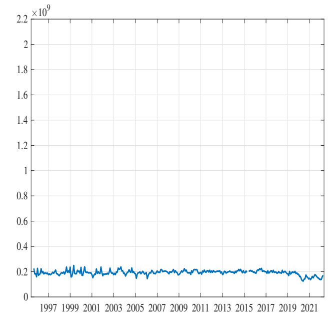
PEMLR is a recoded variable that collects final labor force status information of adult civilians. Possible entries are: 1-2 = employed, 3-4: unemployed, 5-7: nonparticipation. Within the last group, 5 is retirement. This recoded variable is the result of a combination of Dependent Interviewing questions and numerous cross-sectional questions that the CPS



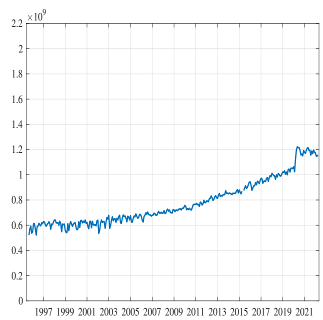
(a) SS



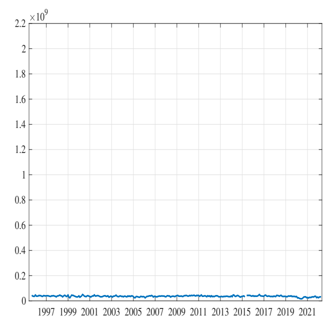
(b) SP



(c) PS



(d) PP

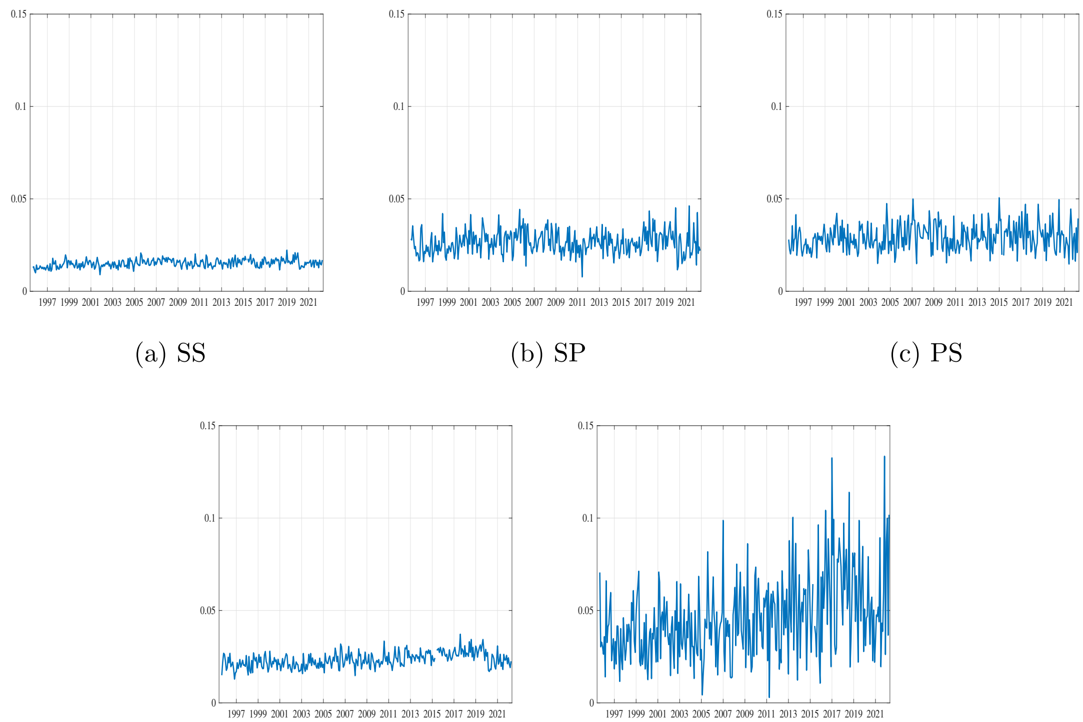


(e) PP'

Figure OA2: Retired Individuals by Respondent Status

interviewers ask about employment status. When the DI question about retirement goes unanswered, the Census can rely on the other questions to fill in the information. As a result, the PEMLR variable has no missing entries. Thus, within the matched records, those who are reported to be "retired" among nonparticipants in the first month can be identified by the main labor force status variable, PEMLR = 5. The number of these people is plotted in Figure OA2 for the five Respondent groups (1 = SS, 2= SP, 3=PS, 4=PP, 5=PP'). There was clearly a seasonality in this series before 2007 which disappeared in 2007 on. But we see no jumps in 2007-2009. Note from the scale of the y axes that respondent turnover for retired/disabled is tiny: most answers are from SS or PP. Plausibly, these retired respondents either are always home to answer or have a caretaker who always answers for them.

To understand how often dependently coded information on inactivity becomes unavailable, possibly because of the RIP, consider all those who are reported as being retired in a given month t (PEMLR=5) and are matched to following month $t+1$. In Fig. OA3, we plot, by Respondent group, $\Pr(\text{PURETOT}_{i,t+1} = -1 | \text{ PEMLR}_{i,t} = 5)$: the share of this sample who report in the second month "PURETOT=-1", namely, are out of universe/missing



(a) SS (b) SP (c) PS

(d) PP (e) PP'

Figure OA3: Share of Missing Answers to the DI Question on Retirement

answers to the Dependent Interviewing question about retirement. These are people whom we know were retired last month, based on $\text{PEMLR}_{i,t} = 5$), thus are eligible for the dependent interviewing question PURETOT on retirement this month, yet do not have a valid answer. These can be cases when the responder either refuses to answer, or does not know the answer, or is not asked the PURETOT question to begin with, because the RIP applies. In the latter case, the share of these records should jump, or at least rise fast, in 2007-2009. We see neither. So, we conclude that measurement of retirement status is not impacted by the RIP.

We believe the reason to be the following. The Census interviewer has PEMLR available to detect retirement status in the previous month. PEMLR is a *publicly available* variable, which is not subject to the RIP, because it is not an explicit answer. It is a variable constructed from multiple cross-sectional questions. Therefore, when the RIP applies and invalidates last month's longitudinal information, the PURETOT question can be based on PEMLR=5 last month, without violating the RIP rules. In contrast, when asking about employer change, the interviewer has no access to a recoded variable that reports the employer name a month before and thus must build on first-hand information directly.

1.4 Non Participation: Disability

Similar results obtain for disability. In that case, the question is

PUDIS DISABILITY STATUS
(LAST MONTH YOU WERE REPORTED TO HAVE A DISABILITY.) DOES YOUR DISABILITY CONTINUE TO PREVENT YOU FROM DOING ANY KIND OF WORK FOR THE NEXT 6 MONTHS?
VALID ENTRIES
1 YES
2 NO
3 DID NOT HAVE DISABILITY LAST MONTH

Again, the share of answers equal to -1 shows no jumps in 2007-2009, and other questions allow to determine disability status. For example:

PUDIS2
DO YOU HAVE A DISABILITY THAT PREVENTS YOU FROM ACCEPTING ANY KIND OF WORK DURING THE NEXT SIX MONTHS?
VALID ENTRIES
1 YES
2 NO

Again, the Census interviewer has access to recoded, and publicly available, variable that measures disability status a month before. So, they can rely on it to circumvent the RIP. We verified that no major rise in disability status appears around 2007.

1.5 Unemployment Duration: no DI question in Survey

Another variable that the interviewer manual claims to be dependent-coded is unemployment duration. But there again, Technical Paper 66 seems to suggest that Dependent Interviewing simply means “impute based on last month’s answer.” In this case, i.e., if someone reports being unemployed in month t, and was also unemployed in month t-1, then the interviewer should not ask about unemployment duration in month t, but just add four or five weeks to the month-(t-1) duration. In this respect, retirement/disability is different from unemployment duration. In the latter, there is no actual question which refers to the information given in the previous month (like EMPSAME and PURETOT do). There is no question in the survey that asks: "Last month, A was reported to be unemployed, is A still unemployed?" So the RIP clearly does not apply. The interviewer knows (without sharing this information with the actual respondent) that a person was unemployed in the previous month, and once the interviewer figures out that the person was still unemployed this month, then duration is updated automatically. So this is just dependent coding without dependent interviewing.

2 Imputation Step 2: Regression results

Tables OA1-OA3 summarize the results of the Step-2 regressions in our imputation procedure described in the paper. Table OA1 presents the coefficient estimates for the 1995-2006 pre-RIP sample, Table OA2 for the interactions with the 2007-2009 cohorts affected by the measurement error of unknown origin, and Table OA3 for the interactions with the RIP, in 2008-2020. We comment on these results in the paper.

3 Relative importance of imputation elements

Our imputed EE transition is the sum of two terms, $(\hat{\alpha} + X_{it}\hat{\beta})$ and the change in the estimated effects of observables X_{it} when we allow for the RIP bias. The two terms are correlated through the presence of observables X_{it} in both the rescaling factor $(\hat{P}_{it}/(1-\hat{P}_{it})$ and the estimated bias $\hat{B}_{it}$. We decompose (in a variance sense) our imputation into these two pieces. There is no natural way to decompose the second term further, into $\hat{P}_{it}$ and $\hat{B}_{it}$, because it is nonlinear.

Figure OA4 plots, for each Respondent group, two time series: the MAR and the series based on imputation $\hat{\alpha} + X_{it}\hat{\beta}$, which is derived by adding back the odds-ratio-rescaled bias to the imputed series. As it is clear from the figures, the two series are extremely similar in every case. Therefore, the difference between the MAR/FF series and our imputed series is largely due to the bias, i.e. to selection by unobservables.

4 Precision of Imputed Transitions.

In the first step of the imputation, for each time t separately, we use all eligible records $i = 1, 2...M_t$ (employed at both times $t-1$ and t) to run a Probit regression of the validity of an answer to the EMPSAME question, $\mathrm{DI}_{i,t}$ on K observables X_t, a $M_t \times K$ matrix, and

Table OA1: Imputation regression results

	$R = SS$	$R = SP$	$R = PS$	$R = PP$	$R = PP'$
BLAISE	−0.087*	−0.115	0.069	−0.095*	−0.472*
RIP	−0.005	−0.025*	−0.013	−0.006	0.021
Rotation Gr. 2−3	−0.002***	−0.001	−0.008***	−0.003***	−0.004**
Rotation Gr. 3−4	−0.003***	−0.002**	−0.012***	−0.003***	−0.009***
Rotation Gr. 5−6	0.000	0.003***	−0.001	0.000	0.000
Rotation Gr. 6−7	−0.002***	−0.001	−0.009***	−0.003***	−0.005***
Rotation Gr. 7−8	−0.004***	−0.001	−0.012***	−0.003***	−0.006***
Sex	0.002***	−0.002***	0.001	0.001*	−0.001
Married Spouse Absent	−0.003***	−0.008***	−0.021***	−0.009***	−0.004**
Widowed/Divorced	0.001**	−0.001	−0.001	0.001	0.006*
Never Married	0.000	−0.004***	−0.007***	−0.001	0.006**
Less Than High School	0.003***	0.003**	0.007***	−0.001***	−0.007***
Some College	0.002***	0.001*	0.000	0.003***	0.003**
College	0.002***	0.001*	0.002*	0.002***	0.006***
Graduate	0.003***	0.004***	0.001	0.002***	−0.002
Ages 16−20	0.025***	0.023***	0.016***	0.016***	0.015***
Ages 31−40	−0.008***	−0.012***	−0.011***	−0.010***	−0.012***
Ages 41−50	−0.011***	−0.015***	−0.013***	−0.012***	−0.023***
Ages 51−60	−0.013***	−0.013***	−0.016***	−0.010***	−0.025***
Ages 61−70	−0.014***	−0.012***	−0.015***	−0.008***	−0.023***
Ages 71−	−0.014***	−0.010***	−0.017***	−0.004***	−0.024***
EE-SS-RG1 Trend	0.781***	1.107***	1.149***	0.969***	2.232***
EE-SS-RG1 Cycle	0.353***	0.124*	0.152*	0.257***	0.382***
Constant	0.007***	0.014***	0.032***	0.015***	−0.018**

Notes: Base groups: Rotation Group 1-2, male, married spouse present, high school, and ages 21-30. Each regression also includes month dummies, 16 major industry and 13 major occupation dummies in the initial month. The full results are available upon request. The sample period is September 1995 - December 2022. The superscripts *, **, and *** indicate significance at 10%, 5%, and 1%, respectively. t-statistics are in parentheses.

Table OA2: Imputation regression results: BLAISE interaction terms

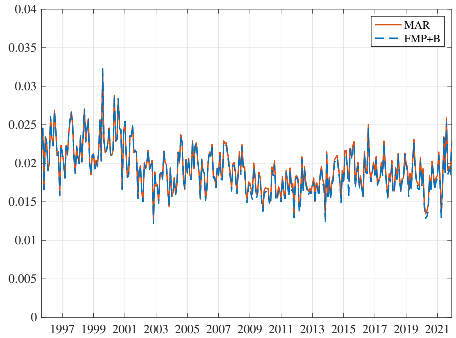
	$R = SS$	$R = SP$	$R = PS$	$R = PP$	$R = PP'$
Rotation Gr. 2−3	−0.001	−0.001	−0.004	0.003**	−0.004
Rotation Gr. 3−4	−0.001	−0.003	0.004	0.001	0.006
Rotation Gr. 5−6	0.000	0.001	0.002	0.002	−0.007
Rotation Gr. 6−7	−0.001	−0.002	−0.001	0.002	−0.006
Rotation Gr. 7−8	−0.002	−0.002	0.001	0.002	−0.001
Sex	−0.001*	0.001	−0.004**	−0.001	0.005
Married Spouse Absent	−0.001	0.007***	0.008**	0.005***	0.006
Widowed/Divorced	−0.001	0.005	0.008	0.000	0.008
Never Married	−0.001	0.006	0.009*	0.000	0.005
Less Than High School	−0.003**	0.003	−0.006	0.001	0.010**
Some College	0.000	0.002	0.000	−0.001	0.003
College	0.001	0.002	0.000	0.003***	0.002
Graduate	−0.001	−0.003	0.003	0.002	0.007
Ages 16−20	−0.006	−0.003	0.010	−0.001	−0.009*
Ages 31−40	0.002	0.006**	0.007**	0.003**	−0.007
Ages 41−50	0.004***	0.005*	0.004	0.002*	0.001
Ages 51−60	0.004***	0.001	0.008**	−0.001	−0.001
Ages 61−70	0.007***	0.001	0.005	−0.003*	−0.004
Ages 71−	0.003*	−0.005	−0.002	−0.005	−0.003
EE-SS-RG1 Trend	4.764*	4.918	−2.935	4.358	24.416*
EE-SS-RG1 Cycle	−0.271**	0.348	0.824**	−0.238*	0.238

Notes: Base groups: Rotation Group 1-2, male, married spouse present, high school dropouts, and ages 21-30. Each regression also includes month dummies, 16 major industry and 13 major occupation dummies in the initial month. The full results are available upon request. The sample period is September 1995 - December 2022. The superscripts *, **, and *** indicate significance at 10%, 5%, and 1%, respectively. t-statistics are in parentheses.

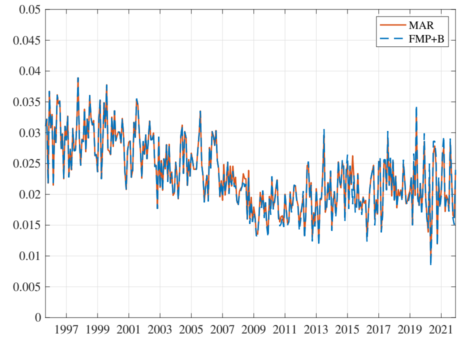
Table OA3: Imputation regression results: RIP interaction terms

	$R = SS$	$R = SP$	$R = PS$	$R = PP$	$R = PP'$
Rotation Gr. 2–3	−0.000	0.001	−0.002	0.001	−0.002
Rotation Gr. 3–4	−0.001	0.002	0.001	0.000	0.007**
Rotation Gr. 5–6	0.001	0.001	0.000	0.001**	0.001
Rotation Gr. 6–7	0.000	0.001	−0.001	0.001**	0.000
Rotation Gr. 7–8	0.000	−0.000	0.000	0.000	0.003
Sex	−0.001*	0.000	0.001	−0.001**	0.002
Married Spouse Absent	−0.000	0.005***	0.008***	0.004***	0.003
Widowed/Divorced	−0.001	0.002	0.005	−0.001	−0.005
Never Married	−0.000	0.002	0.007***	−0.001	−0.007*
Less Than High School	−0.000	−0.004**	−0.008***	0.000	0.003
Some College	0.000	−0.002*	−0.000	−0.001***	−0.002
College	−0.001	−0.002	0.002	−0.000	−0.003
Graduate	−0.001	−0.004***	0.002	−0.000	0.001
Ages 16–20	−0.012***	−0.006	−0.001	−0.005***	−0.007***
Ages 31–40	0.002***	0.006***	0.003*	0.004***	0.004
Ages 41–50	0.003***	0.006***	0.005***	0.005***	0.010***
Ages 51–60	0.004***	0.002	0.006***	0.001	0.011***
Ages 61–70	0.005***	−0.001	0.005**	−0.002**	0.009**
Ages 71–	0.005***	−0.002	0.010**	−0.004***	0.016*
EE-SS-RG1 Trend	0.390**	0.908	0.368	−0.173	−0.529
EE-SS-RG1 Cycle	0.021	0.077	0.043	−0.028	−0.075
N	5468318	984073	979292	4755769	364280
R^2	0.003	0.007	0.008	0.007	0.010

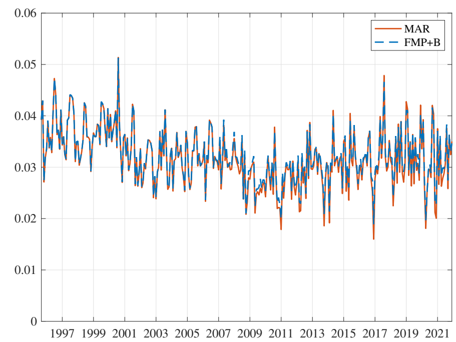
Notes: Base groups: Rotation Group 1-2, male, married spouse present, high school, and ages 21-30. Each regression also includes month dummies, 16 major industry and 13 major occupation dummies in the initial month. The full results are available upon request. The sample period is September 1995 - December 2022. The superscripts *, **, and *** indicate significance at 10%, 5%, and 1%, respectively. t-statistics are in parentheses.



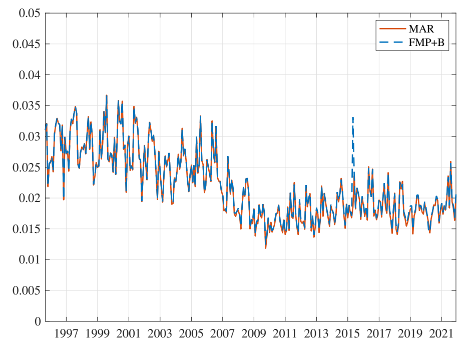
(a) SS



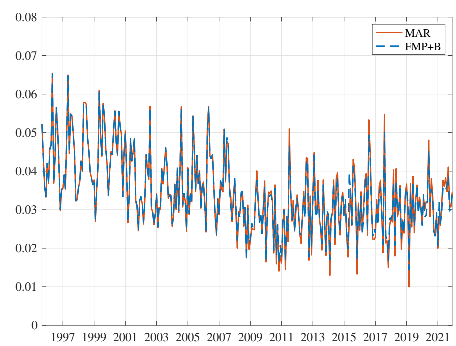
(b) SP



(c) PS



(d) PP



(e) PP'

Figure OA4: Contribution

then predict, for each eligible individual $i = 1, 2 \ldots M_t$:

$$\hat{P}_{i,t} = \Phi(X_{i,t}\hat{\lambda}_t)$$

where Φ is the CDF of the standard normal distribution, $X_{i,t}$ is the $i-th$ row of X_t, with (abusing notation) $k-th$ column element $X_{i,k,t}$, which is the value of the $k-th$ covariate for individual i at time t. Suppose that the true value of the Probit parameters is λ_t and the true probability is $P_{i,t} = \Phi(X_{i,t}\lambda_t) = 0.01$. The Probit estimate is, of course, noisy, so that $X_{i,t}\hat{\lambda}_t = X_{i,t}\lambda_t \pm$ error. Then, the estimated probability will be $\hat{P}_{i,t} = \Phi(X_{i,t}\hat{\lambda}_t \pm \text{ error})$, and with $\Phi(X_{i,t}\lambda_t) = 0.01$, a positive error will move the estimated probability $\hat{P}_{i,t}$ toward 0.5 by more than a negative error of the same magnitude would move it toward 0. Because $P_{i,t}$ is in general a small number, this uncertainty tends to bias $\hat{P}_{i,t}$ upwards.

How does this impact our main object of interest, the estimated average EE probability? In the second step of the imputation procedure, we estimate the "EE mobility bias" $\hat{B}_{i,t} = b(X_{i,t}|\hat{\gamma}_t)$. In the third step of our imputation procedure, for each eligible individual $i = 1, 2..N_t < M_t$ who does not have a valid answer to the EMPSAME question ($\text{DI}_{i,t}$=0), we estimate the odds-ratio rescaled bias:

$$Z_{it} \equiv -\frac{\hat{P}_{i,t}}{1-\hat{P}_{i,t}} RIP_{i,t}\hat{B}_{i,t} = -\frac{\Phi(X_{i,t}\hat{\lambda}_t)}{1-\Phi(X_{i,t}\hat{\lambda}_t)} \cdot RIP_{i,t} \cdot b(X_{i,t}|\hat{\gamma}_t). \tag{1}$$

which then determines the imputed mobility (for notational simplicity, we omit the dependence of $\hat{\alpha}, \hat{\beta}$ on the Rotation group):

$$\widehat{\text{EE}}_{i,t} = \hat{\alpha} + X_{i,t}\hat{\beta} + Z_{it}.$$

The estimated average EE probability is then the average of the mobility of the imputed and observed transitions:

$$\widehat{\text{EE}}_t = \frac{\sum_{i=1}^{N_t}(\hat{\alpha} + X_{i,t}\hat{\beta} + Z_{it}) + \sum_{i=N_t+1}^{M_t} \text{EE}_{i,t}}{M_t}$$

Since $Z_{i,t}$ is decreasing in $\hat{P}_{i,t}$, attenuation in the Probit that biases $\hat{P}_{i,t}$ upward also tends to bias $Z_{i,t}$ downward, and thus reduces the positive impact that we find of the imputation on the estimated average $\widehat{\text{EE}}_t$ probability. That is, our positive (upward) correction to the aggregate series may be conservative, due to imprecise Probit estimates $\hat{\lambda}_t$ in the first step of the imputation procedure.

Isolating the impact of the uncertainty in the estimated $\hat{\lambda}_t$ on the uncertainty in the estimated average $\widehat{\text{EE}}_t$ is complicated by the following issue. Note that $\hat{\lambda}_t = \Lambda(X_t)$ is a known and time-invariant function Λ of the time$-t$ covariates X_t, while $\hat{\alpha}(X), \hat{\beta}(X), \hat{\gamma}(X)$ are known and time-invariant functions of the entire time series of covariates $X \supset X_t$, a tensor

$\{X_t\}_{t=1}^T$, and $\hat{B}_{i,t} = b(X_{i,t}|\hat{\gamma}(X))$ is also a function of $X_{i,t} \subset X_t$. Conversely, the dummy $RIP_{i,t}$ is independent of covariates, as the RIP treatment was introduced "exogenously" by CPS cohort, independently of individual characteristics $X_{i,t}$. Therefore, we can make explicit the dependence of our estimated aggregate time series on the sample X of covariates as follows:

$$\widehat{\mathrm{EE}}_t = \frac{\sum_{i=1}^{N_t} \left[\hat{\alpha}(X) + X_{i,t}\hat{\beta}(X) - \frac{\Phi(X_{i,t}\Lambda(X_t))}{1-\Phi(X_{i,t}\Lambda(X_t))} \cdot RIP_{i,t} \cdot b(X_{i,t}|\hat{\gamma}(X)\right] + \sum_{i=N_t+1}^{M_t} \mathrm{EE}_{i,t}}{M_t}$$

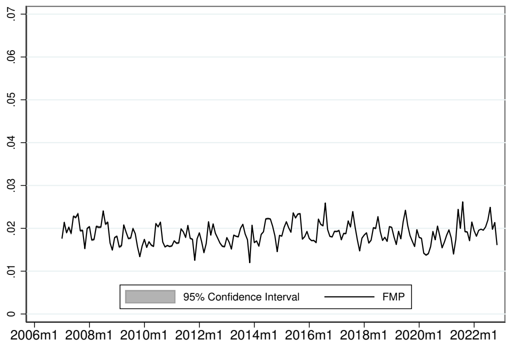
As the uncertainty in each $\hat{\lambda}_t = \Lambda(X_t)$ originates from sampling error in the covariates X_t that are part of X and that include $X_{i,t}$, the joint covariance structure of $\hat{\lambda}_t = \Lambda(X), \hat{\alpha}(X), \hat{\beta}(X), \hat{B}_{i,t} = b(X_{i,t}|\hat{\gamma}(X))$ induced by the common X matters for the variance of $\widehat{\mathrm{EE}}_t$. We do not know how to estimate this covariance structure, thus how to "isolate" the contribution of $\hat{\lambda}_t$.

Nonetheless, we observe that (a function of) each X_t, such as $\hat{\lambda}_t = \Lambda(X_t)$, should have a very weak correlation with (a function of) the entire X, such as $\hat{\alpha}(X), \hat{\beta}(X), \hat{\gamma}(X)$, for two reasons: first, for the same individual i, a few of the K covariates do not persist much over time, so the vector of covariates $X_{i,t}$ for individual i at time t is weakly correlated with $X_{i,t\pm1,2,3..}$; second, and more importantly, each individual is sampled monthly at most 8 times, and often less, while the entire dataset is over 25 years long, so $T > 300$. That is, as time goes by, most covariates pertain to different people, and thus can be treated as independent random variables. Similarly, conditional on the estimated $\hat{\gamma}$, $\hat{\lambda}_t = \Lambda(X_t)$ should have a very weak correlation with each $\hat{B}_{i,t} = b(X_{i,t}|\hat{\gamma})$, because the change in each $X_{i,t}$ has a negligible effect on X_t.

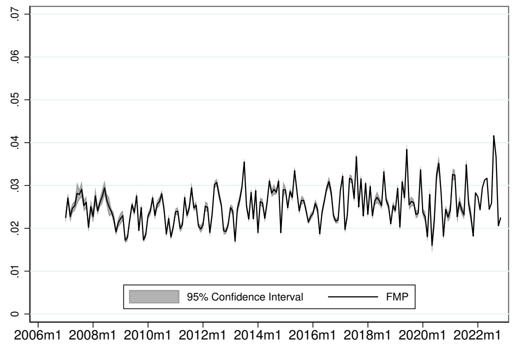
In light of these considerations, we ignore the covariance between $\hat{\lambda}_t$ and the vector $(\hat{\alpha}, \hat{\beta}, \hat{\gamma}, \hat{B}_{i,t})$, a vector that we treat as fixed when studying the variance of *each* $\hat{\lambda}_t$ (for each $t = 1, 2...T$ separately). That is, we estimate the contribution of the (im)precision in $\hat{P}_{i,t}$ on that of the average probability $\widehat{\mathrm{EE}}_t$ by treating only $\hat{\lambda}_t$ as a random variable

$$Var_{\hat{\lambda}_t}(\widehat{\mathrm{EE}}_t) = \frac{Var_{\hat{\lambda}_t}\left(\sum_{i=1}^{N_t} - \frac{\Phi(X_{i,t}\hat{\lambda}_t)}{1-\Phi(X_{i,t}\tilde{\hat{\lambda}}_t)} RIP_{i,t}\hat{B}_{i,t}\right)}{M_t^2} = \frac{Var_{\hat{\lambda}_t}\left(\sum_{i=1}^{N_t} Z_{i,t}\right)}{M_t^2}$$

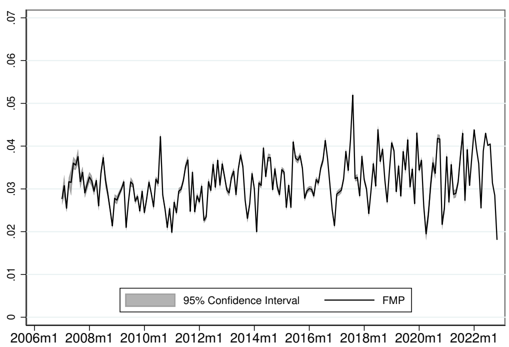
In order to estimate the last expression, we use the Delta method. For each t, we linearize



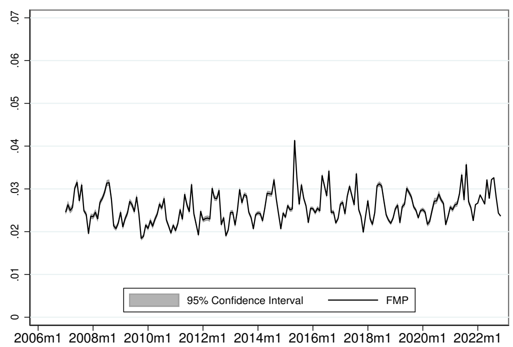
(a) SS



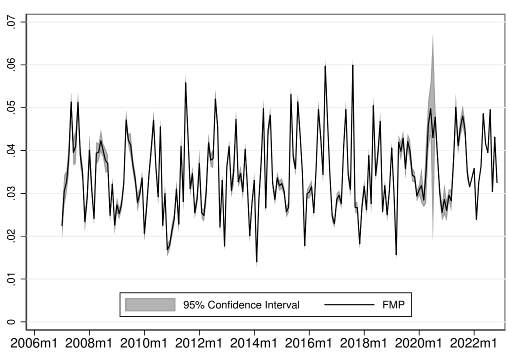
(b) SP



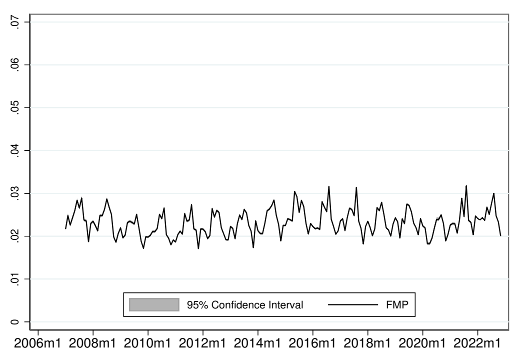
(c) PS



(d) PP



(e) PP'



(f) All

Figure OA5: Average imputed EE transition probability with confidence intervals

(1) with respect *only* to the vector $\hat{\lambda}_t$ around λ_t:

$$
\begin{aligned}
Z_{i,t} &\simeq -\frac{1}{(1-P_{i,t})^2}\phi(X_{i,t}\lambda_t) RIP_{i,t} B_{i,t} \sum_{k=1}^{K} \frac{\partial\,(X_{i,t}\lambda_t)}{\partial \lambda_{k,t}}(\hat{\lambda}_{k,t}-\lambda_{k,t}) \\
&= -\frac{1}{(1-P_{i,t})^2}\phi(X_{i,t}\lambda_t) RIP_{i,t} B_{i,t} \sum_{k=1}^{K} X_{i,k,t}(\hat{\lambda}_{k,t}-\lambda_{k,t})
\end{aligned}
$$

where ϕ is the PDF of the standard normal distribution. So we can write the column vector Z_t with element $Z_{i,t}$ on the $i-th$ row, as

$$Z_t = J_t' X_t(\hat{\lambda}_t - \lambda_t)$$

where J_t is the column vector with

$$J_{i,t} = -\frac{1}{(1-P_{i,t})^2}\phi(X_{i,t}\lambda_t)RIP_{i,t}B_{i,t}$$

on the $i-th$ row. Because $P_{i,t}, \lambda_t, B_{i,t}$ are not observed, we estimate J_t with

$$\hat{J}_{i,t} = -\frac{1}{(1-\hat{P}_{i,t})^2}\phi(X_{i,t}\hat{\lambda}_t)RIP_{i,t}\hat{B}_{i,t}$$

The variance of the sum of $Z'_{i,t}s$ is estimated by

$$Var_{\hat{\lambda}_t}(\sum_{i=1}^{N} Z_{i,t}) = \hat{J}_t' X_t V(\hat{\lambda}_t) X_t' \hat{J}_t$$

where $V(\hat{\lambda}_t)$ is the variance covariance matrix of the coefficient vector of the Probit, taken from Stata directly. Finally, we obtain the desired estimate:

$$Var_{\hat{\lambda}_t}(\widehat{\mathrm{EE}}_t) = \frac{\hat{J}_t' X_t V(\hat{\lambda}_t) X_t' \hat{J}_t}{M_t^2}$$

Figure OA5 shows the time series of average EE mobility by respondent group (including both valid and imputed records), along with (shaded, in gray) confidence intervals estimated according to the procedure illustrated above. The intervals are hardly visible. The impact of uncertainty deriving from the first stage of the imputation appears to be minimal.

5 Other observables that correlate with EE transitions

Earnings. We believe that earnings, as a predictor of job switching, would be problematic. With some probability, the wage surveyed in the reference month 4 will not refer to the job held in months 5-7, in a way that we could not detect, given the 8-month gap between rotations. The sample of apparent stayers in months 5-8 would include workers who experienced job changes, with or without an intervening jobless spell, in the 8-month gap, workers who would then appear to have large earnings changes without an observable employer change.

This is the main reason why we did not use the initial (month 4) wage level in the imputation regression.

Hours. Hours worked, instead, are available every month. We investigate in detail their correlation with employer changes. In 2004-2006, the monthly change in hours is about three times larger for job switchers than for stayers, and is roughly independent of the validity of the answer to the EMPSAME question (DI=0,1), indicating that these answers are indeed missing at random before 2007. In 2007-2009, the difference in hours volatility between job switchers and stayers slightly widens. Now hours are more volatile for the continuously employed who do not answer the EMPSAME question (DI=0), while the variability for the valid answers (DI=1) remains the same as before 2007. To reconcile this discrepancy, we conservatively estimate that the EE mobility rate of the DI=0 answers after 2007 must have been at least three times higher than before 2007, 8.5% vs 2.5%. So changes in hours do contain valuable information to impute unobserved EE transitions.

When we include the change in hours in our imputation regression, while its estimated coefficient is highly significant as predicted by the previous results, the aggregate imputation results barely change. Therefore, the regressors we used before for imputation already contain the same information, and hours worked neither add nor subtract from our main results.

Self-Employment status. Unfortunately, this status appears to be independently coded with significant noise, more so when DI=0. Therefore, it may correlate with employer change in valid records that the RIP censors, but for spurious reasons.

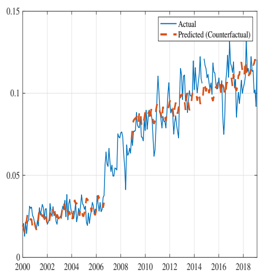
6 Comparison of CPS vs LEHD by demographics

We supplement here material from Section 6.3 in the paper. We consider 10 demographic groups:

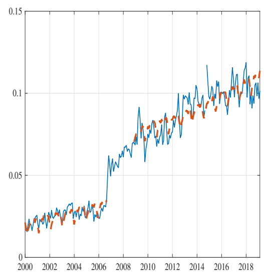
Groups 1-5: males, (19-24) (25-34) (35-44) (45-54) (55+)

Groups 6-10: females, (19-24) (25-34) (35-44) (45-54) (55+)

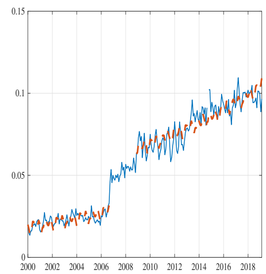
We limit attention to these 10 groups to maintain power, because they are available in the LEHD, too. We first estimate the "impact of Blaise-RIP on (the EE transitions of) each demographic group" as follows. For each of the 10 groups, we run a separate linear regression of the probability of missing answers (DI=0) on month of the year dummies to control for seasonality, linear and square terms of time to capture the trend unrelated to the Blaise-RIP measurement issues, and the dummy that takes zero before Jan 2007 and one after March



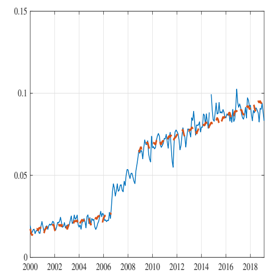
(a) Male, 19-24



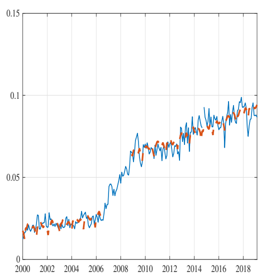
(b) Male, 25-34



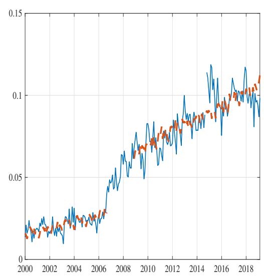
(c) Male, 35-44



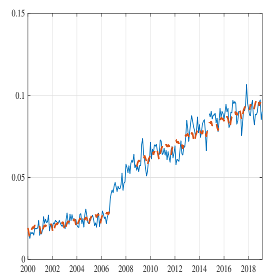
(d) Male, 45-54



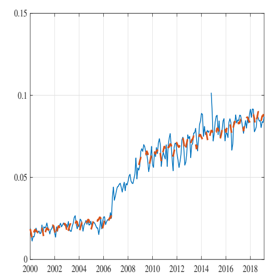
(e) Male, 55+



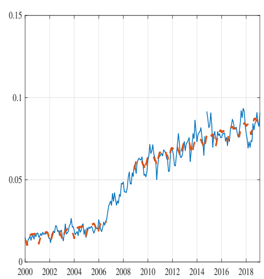
(f) Female, 19-24



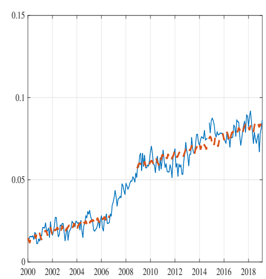
(g) Female, 25-34



(h) Female, 35-44



(i) Female, 45-54



(j) Female, 55+

Figure OA6: Predicted (blue, solid) vs. Actual (red. dashed) Overall Probabilities of $DI = 0$ by Group

2009. Figure OA6 plots the actual shares of missing observations within the eligible records (blue lines) and their predicted values (red dashed lines). We can see that the predicted values track the actual values quite well. Note, again, that the estimation exclude the data for 2007:M1-2009:M3, which are then missing in the figure. Figure OA7 plots the estimated marginal effects of the Blaise-RIP dummy in this regression for each of the 10 demographic groups. Note that in this linear probability setting these numbers roughly correspond to the size of the jump in the predicted probabilities between Jan. 2007 and Mar. 2009 in the previous figure. There are significant variations across groups. Males tend to have higher

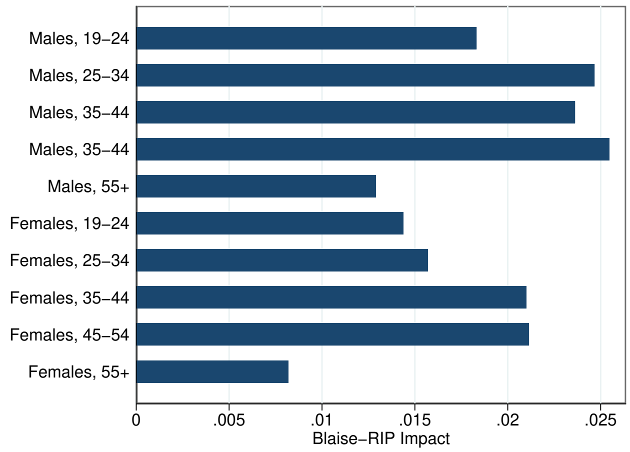
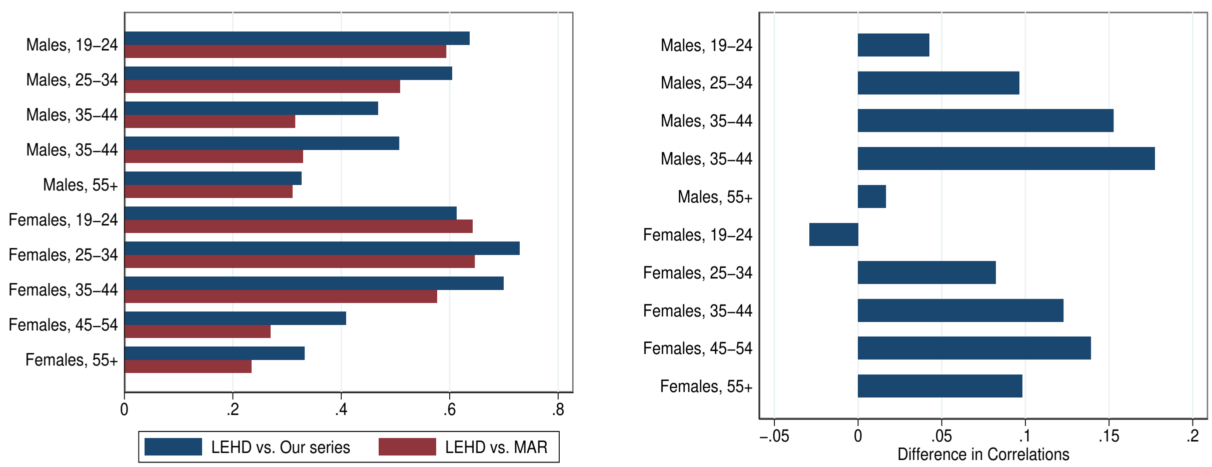


Figure OA7: Impacts of Blaise-RIP Measurement Issues



(a) Correlations

(b) Difference in Correlations

Figure OA8: Correlations with LEHD Series

"propensities" (impact) than women. It is interesting that middle-age individuals tend to have higher propensity values.

Next, we correlate, across these 10 groups, the quarterly levels of EE rates in the LEHD, seasonally adjusted, with those in CPS (MAR, FMP). The data run from 2000Q2 - 2020Q1 (because the LEHD data is available only for this sample period at the time of our analysis), excluding the critical months 2007:M1-2009:M3, we know that through the end of 2006, there was no Blaise-RIP measurement problem, and after March 2009, everybody is subject to the measurement problems, so we want to isolate a clean treatment.

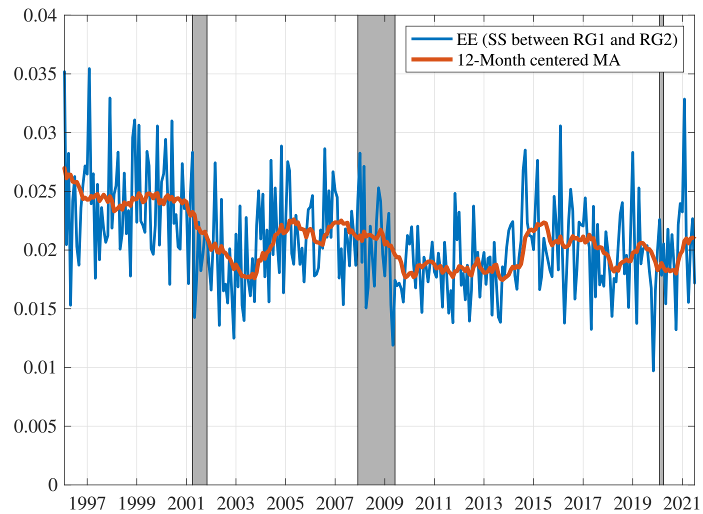


Figure OA9: EE Rate for SS between rotation groups 1 and 2

Figure OA8(a) plots, for each of the 10 demographic groups, the correlation between the LEHD and the (resp.) FMP and MAR series, while Figure OA8(b) plots the difference between Corr (LEHD, FMP) and Corr (LEHD, MAR). We can see that the LEHD series tend to be correlated more strongly with FMP series than with the MAR series.

Figure 19 in the article presents the scatter plot between the the propensities (Figure OA7) and the differences in correlations (Figure OA8(b)) and shows a clear positive association between the two. In words, the higher the estimated incidence of the Blaise-RIP on causing missing answers to the EMPSAME question in a demographic group, the more our imputation will realign the congruence between EE mobility rates in the monthly CPS and the LEHD.

7 Zeroing on to the Great Recession

Our imputed EE measure peaks in early 2007, shortly after UE, but then remains almost flat for all of 2007 (Fig. 15a in the revision). The FF/MAR series peaks prematurely, well before the other two. It is still possible that our imputation over-corrects in 2008, delaying the cyclical decline too much. Technically, the "hump" in our series in 2007, before the financial crisis, originates from the cyclical indicator that we use in the imputation, namely, the EE probability of the SS respondents in the first and second Rotation Group (SSRG1). See Figure OA9 below. Fig. A.2 in the revision plots the same series (without the MA smoother overlayed). Beyond the noise, one can see in Figure OA9 that the EE probability

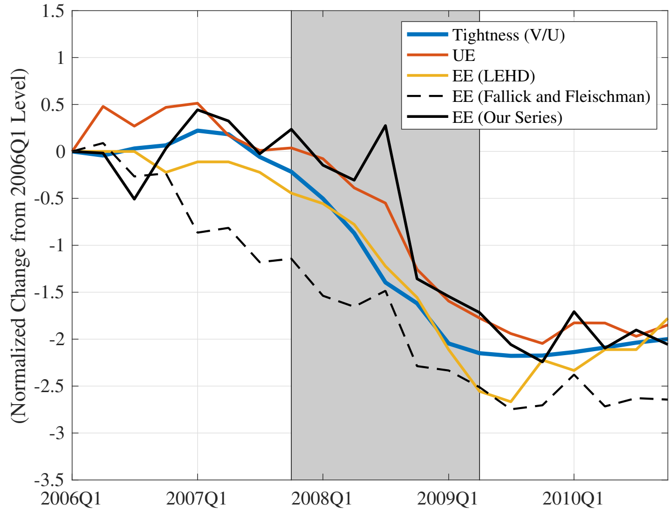


Figure OA10: Labor market flows around the Great Recession

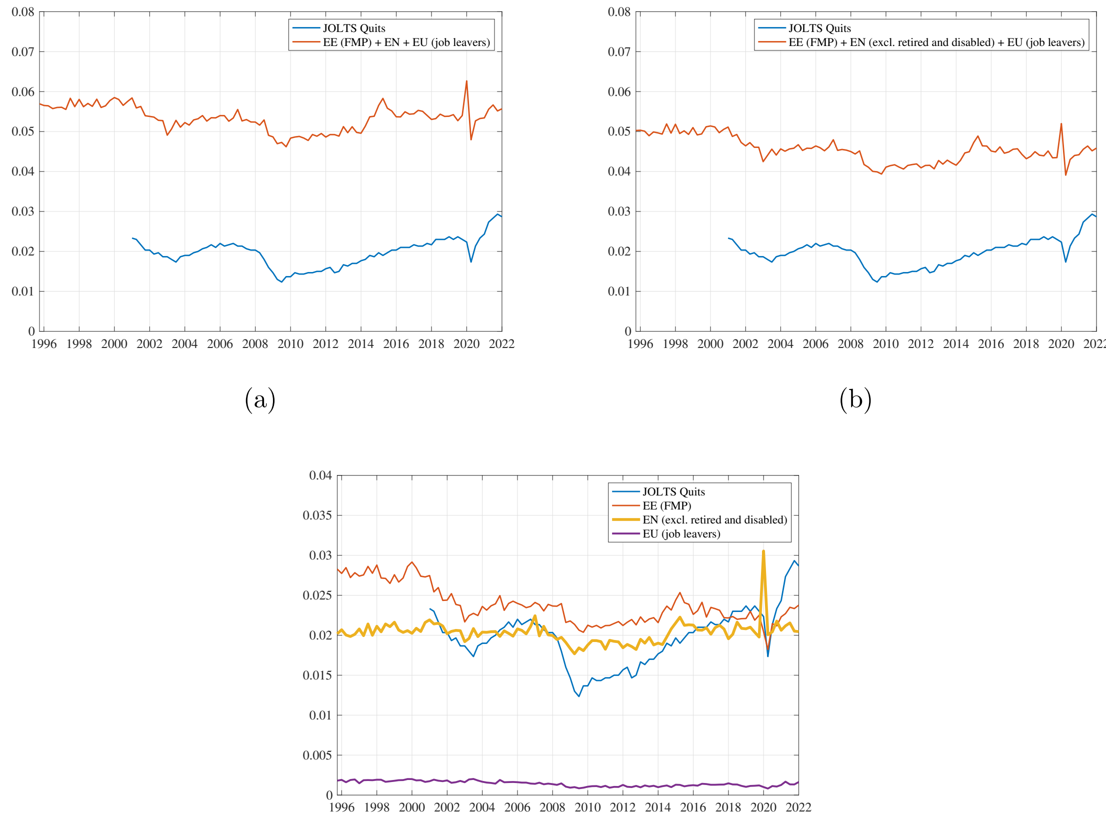
of that SSRG1 group remained elevated in the first three quarters of 2008, while the rest of the economy entered the recession. We do not know whether this episode is genuine or due to other measurement issues. A very similar pattern holds for the EE rate among the overall SS group (all rotation groups; Fig. 13 in the revision).

To shed some more light, in Figure OA10, we plot the CPS EE series (FF and our imputed series), the UE probability, the LEHD-based EE probability, and job market tightness (JOLTS vacancies divided by civilian unemployment). To ease the comparison, we start all series in 2006Q1, which is when the FF series peaks and starts declining, through 2010. We normalize each series to 0 at the beginning and divide it by its standard deviation over that short period. The purpose is to compare the timing of the cyclical decline. Time aggregation introduces a bias in the cyclical amplitude of the LEHD-based series, but it should not affect its phase. As is clear from the figure, our EE series is much closer to the other measures than the FF series. The temporary spike in the middle of the recession runs counter the series from other datasets, but is visible also in the FF series, so it is not the result of our imputation.

The disconnect between UE and EE appears also after 2015. EE stalls and even starts declining before the pandemic, even more so for the FF/MAR measure (so this is not just a result of our imputation), while UE keeps rising. As explained above, we see this as evidence of “mismatch depletion” after many years of tight labor market.

8 EE Transitions in the CPS vs Quits in JOLTS

For JOLTS, quits include those to non-employment, which are likely to be procyclical (because of the wealth effect from stock market returns and early retirement not fully captured as such in JOLTS, or because one is more confident quitting when there are other jobs available). Figure OA11 plots the JOLTS quit rate with several CPS-derived series, described as follows. In Panel (a), we add to our EE measure the transition rate from Employment to Nonparticipation and the transition rate from Employment to Unemployment due to quits (Job Leavers). Panel (b) is the same, but we exclude EN transitions due to retirement and disability, since these separations are not part of quits but included in "other separations" in JOLTS. Note that, in both cases, transition rates out of Employment can be added, because they are competing hazards of exit from employment last month. In Panel (c), we present the individual components of Panel (b). The CPS series in (b), which should capture the total exit rate from Employment excluding layoffs and retirements, is always significantly higher than the JOLTS quit rate. Some of the difference can be due to measured EE that are either EUE, through monthly time aggregation in the CPS, or EE initiated by an involuntary separation, when the worker is able to line up a new job to start right when the old job ends or is terminated. We have no way to correct for these either. Some of the difference may also be due to JOLTS, which is a survey of pre-existing establishments, followed for up a year before being rotated into the JOLTS panel for two years, 1/24 of the sample renewed every month. The BLS uses a birth-death model to correct for the high quit rates from new establishments, but administrative data are notoriously slow to detect entry. Most importantly, the JOLTS and CPS series, either individual or aggregated, evolve very similarly over time until 2020, when they diverge. None of the CPS-based series exhibit the extraordinary recent spike in the JOLTS quit rate.



(a) (b)

(c)

Figure OA11: JOLTS and CPS