# Exchange Rates and Wages

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*Abstract*

The continued globalization of economies increasingly exposes labor markets to international fluctuations. Using Current Population Survey data for the period 1976 through 2000, we explore the implications of exchange rate movements for the wages of U.S. workers in manufacturing and non-manufacturing industries. While the overall impact of exchange rate shocks on wages is modest, the impact for specific groups of workers can be quite sizeable. The most significant wage effects occur at the time of job transitions. By contrast, workers who remain with their same employer experience little if any wage impacts from exchange rates. While exchange rates can affect the incidence of job changing, this is secondary in importance to the impact of these shocks on the wage consequence of job changing. Finally, in the United States the wage effects arising from exchange rate movements vary significantly by skill level — low skilled workers bear the largest burden from an appreciation of the dollar.

JEL codes: F31, F3, F4, J30, E24

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## I. Introduction

As large swings in exchange rates become a regular part of the economic landscape, it is important to understand their implications for labor markets. An evolving literature suggests that the wage effects of dollar movements can be significant, but there remain broad gaps in our understanding of the implications for different groups within the workforce and of the mechanisms for these effects. In this paper, we confront these issues using a rich source of data on labor market outcomes for individual workers employed in U.S. manufacturing and non-manufacturing industries. We confirm that the overall elasticity of wages to the exchange rate is small. However, exchange rate movements can lead to large wage changes for some workers, especially those with lower skill levels. For these workers, large wage adjustments occur primarily at times of job transitions.

Our results unify and significantly expand upon the insights from prior analyses. Prior studies using industry-level aggregates found that exchange rate movements appear to have little effect on jobs, but sometimes sizable effects on wages (Campa and Goldberg 2001).[1] Industries were shown to be heterogeneous in these effects: those with lower price-over-cost markups — interpreted as facing stiffer competitive conditions — have smaller wage responsiveness and larger employment responsiveness than higher-markup industries, while industries with smaller proportions of skilled-labor have less wage and more employment responsiveness. However, the quality of data in these studies, and the level of aggregation, prevented further analysis of the channels for these wage effects. Earlier studies also could not control for individual worker characteristics or identify which types of workers are most impacted by exchange rate movements.

In the present paper we address a number of key open questions about the effects of exchange rate movements on U.S. labor markets. First, we document which types of workers are particularly sensitive to exchange rates, where we focus on individuals delineated by their skill level. We also explore the mechanism through which wages respond to exchange rates, specifically asking whether there are differences in the wage elasticities for workers who remain with their current employer and those who change jobs. Among the individuals who change jobs, we ask whether a move across industries tends to be associated with a different pattern of wage of adjustment. Finally, we consider the extent to which dollar fluctuations influence incidence and wage consequences of job changing and industry-switching by workers.

We tackle these questions using data for male respondents to the March Current Population Survey (CPS) for the period 1977 through 2001. This data presents us with a key advantage over the previous industry-level studies in that we can control for the skill levels of workers and whether they make job transitions. We create a series of two-year panels on wages and employment status of workers by matching workers across adjacent March interviews of the CPS. This approach allows us to estimate the effects of exchange rates on wage growth using an empirical specification that controls for both observed and unobserved heterogeneity in worker skill level.

The paper is organized as follows. Section II provides theoretical underpinnings for the wage and exchange rate interactions to be examined empirically. Section III describes our data, details the criteria used in choosing the estimation sample and discusses issues involved in constructing our job changing measures. Section IV presents our empirical methodology and results. Section V offers concluding remarks.

## II. Theoretical and Conceptual Approach

We use the concept of a worker's expected wage to motivate a decomposition of the overall wage elasticity with respect to the exchange rate into three channels. This decomposition links the growing literature on exchange rate effects on job churning (see Gourinchas (1998), Goldberg, Tracy, and Aaronson (1999), and Klein, Schuh and Triest (2001)) to the literature on exchange rates and wages.

The expected log wage for a worker can be expressed as the probability that the worker remains with his same employer multiplied by the expected log wage conditional on no job change, plus the probability that the worker changes jobs multiplied by the expected log wage conditional on a job change. Letting $P_{it}$ denote the probability of a worker making a job change and $JC_{it}$ an indicator for a job change, we can express the expected log wage $w_{it}$ for the $i^{th}$ worker in year $t$ as follows:

[1]See Branson and Love (1988) and Revenga (1992) for earlier industry studies.

$$E(w_{it}) = (1 - P_{it})E(w_{it} \mid JC_{it} = 0) + P_{it}E(w_{it} \mid JC_{it} = 1) \qquad (1)$$

Differentiating equation (1) with respect to the log real exchange rate ($rer_t$), we derive the three channels through which exchange rates influence the expected wages of individual workers.

$$\frac{\partial E(w_{it})}{\partial rer_t} = \frac{\partial E(w_{it} \mid JC_{it} = 0)}{\partial rer} + P_{it}\frac{\partial[E(w_{it} \mid JC_{it} = 1) - E(w_{it} \mid JC_{it} = 0)]}{\partial rer}$$
$$+ [E(w_{it} \mid JC_{it} = 1) - E(w_{it} \mid JC_{it} = 0)]\frac{\partial P_{it}}{\partial rer} \qquad (2)$$

First, there can be on-the-job wage adjustment, so that exchange rates affect a worker's wage in the absence of any job transition. Second, given the normal frequency of job transitions, exchange rate movements may affect the wage premium or penalty associated with a job change. Third, given the normal wage premium or penalty for job changing, the frequency of job transitions may be responsive to the exchange rate.

Previous theoretical studies provide some guidance for how to formulate an empirical specification in order to estimate equation (2). Campa and Goldberg (2001) and Goldberg and Tracy (2000) show that the sensitivity of labor demand to exchange rates arises primarily through impacts on producer profits. A producer’s revenues are influenced by the exchange rate through the impacts on domestic and foreign market sales, while a producer's costs may also be influenced through the use of imported inputs. This suggests that the competitive structure of an industry and its industry trade orientation will affect the wage and turnover elasticities in (2). Dynamic labor demand models offer a couple of additional insights. First, the within-employer wage elasticity and the sensitivity of the turnover probability to the exchange rate should vary with the size of the adjustment costs associated with hiring and firing workers. These adjustment costs vary by the worker’s skill level. Second, firms should make labor adjustments primarily in response to permanent rather than transitory shifts in the exchange rate.

The first step toward building an empirical specification for the decomposition given in equation (2) is to specify the evolution of an individual worker's wage. To capture observed heterogeneity in skill level across workers, we include a set of individual (*i*) characteristics, $Z_{it}$, containing the worker's education, potential job experience, race and marital status. Within this vector, we interact education and experience so that education is allowed to affect both the level and growth rate of wages. Regional (*r*) cyclical shocks are captured in $V_{rt}$, which contains a measure of local labor market conditions (we discuss the construction of these variables in Section III). Aggregate cyclical shocks are captured in $Y_{jt}$, which contains real GDP growth and an industry-specific index for the real exchange rate.

We allow for a potentially rich error component structure to capture the various sources of unobserved heterogeneity at the individual, industry and regional levels. Through the error component term $v_{ijrt}$, individual and industry unobserved heterogeneity is allowed to affect both the level and growth rate of wages ($\mu_{i1}$ and $\mu_{i2}t$, and $\mu_{j1}$ and $\mu_{j2}t$). Regional unobserved heterogeneity is allowed to affect the level of wages $(\mu_{r1})$. Our empirical model for individual *i*'s wages can be summarized as follows.

$$\begin{aligned} w_{ijrt} &= Z_{it}\beta + V_{rt}\gamma + Y_{jt}\delta + v_{ijrt} \\ v_{ijrt} &= \mu_{i1} + \mu_{i2}t + \mu_{j1} + \mu_{j2}t + \mu_{r1} + \varepsilon_{it} \end{aligned} \tag{3}$$

Since aggregate industry real wages, industry-specific real exchange rates and real GDP tend to display unit roots, using specification (3) to estimate the wage elasticities would be problematic. To deal with this issue, we first-difference wages across adjacent years to derive one of our key estimating equations:

$$\begin{aligned} \Delta w_{ijrt} &= \Delta Z_{it}\beta + \Delta V_{rt}\gamma + \Delta Y_{jt}\delta + \Delta v_{ijrt} \\ \Delta v_{ijrt} &= \mu_{i2} + \mu_{j2} + \Delta\varepsilon_{it} \end{aligned} \tag{4}$$

Observe that the individual, industry and region-specific error components have dropped out with the exception of the time trends, which enter as level effects in the differenced residual.[2] Equation (4) shows that an individual's wage growth is a function of his education, experience, any change in his marital status, and the growth rates of the macro and regional variables. We control in our estimation for the industry-specific error component, $\mu_{j2}$, using 2-digit industry fixed-effects, but the individual-specific error component, $\mu_{i2}$, that captures any unobserved heterogeneity in wage growth rates across individuals remains part of the composite error term.

We use variations of specification (4) to estimate the components of the wage elasticity decomposition given in equation (2). We can restrict the estimating sample to see how the wage elasticity varies by occupational category and skill level. To estimate the specific channels through which exchange rate movements influence wages, we include in specification (4) an indicator for whether a worker changes jobs over the two-year period, $JC_{it}$, and we interact this indicator with the change in the industry-specific exchange rate, $\Delta rer_{jt}$. We write this expanded specification as follows.

$$\Delta w_{ijrt} = \Delta Z_{it}\beta + \beta_c JC_{it} + \Delta V_{rt}\gamma + \gamma_r \Delta rer_{jt} + \gamma_{cr} JC_{it}\Delta rer_{jt} + \Delta Y_t \delta + \Delta v_{ijrt} \qquad (5)$$

Using specification (5), we jointly estimate the wage elasticity for workers who do not change jobs, $\gamma_r$, the effect of the exchange rate on the average wage growth differential between job-changes and job stayers, $\gamma_{cr}$, and the average wage growth differential between job changers and job stayers, $\beta_c$.

## III. The Data

**A. CPS Data**. The main data source for our analysis are data on individual workers drawn from the March CPS surveys from 1977 through 2001, which provide wage information for 1976 through 2000. We restrict our sample to civilian men between the ages of 18 and 63 who are employed in the private sector outside of Agriculture, Forestry, Fisheries and Mining.[3]

[2] In particular, first-differencing further controls for worker skill differences since the individual specific error component, $\mu_{i1}$, drops out of the specification.

[3] The sample is further limited to workers who were not in school, who were not primarily self-employed and who had positive weeks worked and earnings in both years. We symmetrically trimmed the top and bottom 2 percent of workers based on income (in survey year 1981 we trimmed the top and bottom 2 ½

We need to match workers across March surveys in order to create the short panels required for our empirical work. Given the interview structure of the CPS, half of the households in each March survey are potentially "matchable" at the next March survey.[4] For the years 1977 through 2001, there are 231,504 individuals from the March surveys who meet our sample restrictions and who can potentially be matched.[5] The CPS provides a household identification variable that facilitates the match of households across surveys. The next step is to match individuals within the household across the surveys. Prior to the 1994 survey, we use a set of demographic variables to do this individual matching. Starting in 1994, the CPS provides a unique person identification number that can be used to match individuals across surveys. We continue to verify that the demographic information matches across years. Our "matched" sample consists of 113,612 individuals. We also retain the broader "unmatched" sample to determine whether the matching process leads to sample selection issues that need to be addressed in the estimation.

Characteristics of the unmatched (but potentially matchable) and matched samples are provided in Appendix Table A1. There are notable differences between the unmatched and the matched samples, with the most noticeable being the higher homeownership rate in the matched sample. This difference is expected, since the matched sample is comprised of individuals who did not change residences between March surveys. Moreover, matched individuals tend to be older, more likely to be married and have a lower incidence of job changing in the year prior to the first survey.

**B. Who is a Job changer?** In order to investigate the role of job changing in affecting the elasticity of wages to exchange rates, we construct an indicator for whether the worker changed jobs over the two-year period.[6] The CPS data has the advantage of allowing us to construct a reasonably consistent measure of job changing over a long

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percent). This effectively eliminates the problem of top-coding of earnings data. We also eliminate observations with allocated or missing values for the variables we use in the analysis.

[4] In the CPS, a household is interviewed for four consecutive months, is rotated out of the survey for eight months and is reinterviewed for four more months. Half of the sample in any March survey are on their first four months rotation of interviews [and are potentially matchable in the following March survey], while the other half of the sample are on their last four months rotation of interviews.

[5] Due to survey limitations, we are unable to match workers for the survey years 1985/86 and 1995/96.

[6] We focus on a two year window (rather than the more traditional one year window) since this matches up with the period we use to measure a worker's wage growth.

sample period. Our key indicator for whether a worker switches jobs is based on a question introduced into the CPS survey in 1977 regarding how many primary employers a worker had over the prior year.[7] We classify a worker as a job changer if he reports more than one primary employer in either of the two years. This question identifies 88 percent of the job changers.

A job changer can answer the he had only one primary employer in each year if the job transition was preceded by a spell of nonemployment that spanned the end of the first year of the two-year period (and the worker makes no other job changes over the two-year period). The way we resolve these cases depends on whether the worker is reemployed as of the first March survey. If the worker is employed as of the first March survey, then we use the industry classification of the worker's current job and primary job last year to determine if a job change occurred. During our sample period, the CPS uses "dependent" industry coding. If the interviewer ascertains that the current job is the same as the primary job last year, then the same industry code is assigned to the current job and to the primary job last year. If these codes differ, then we classify the worker as having changed jobs.[8] Dependent industry coding identifies 8 percent of the job changers.

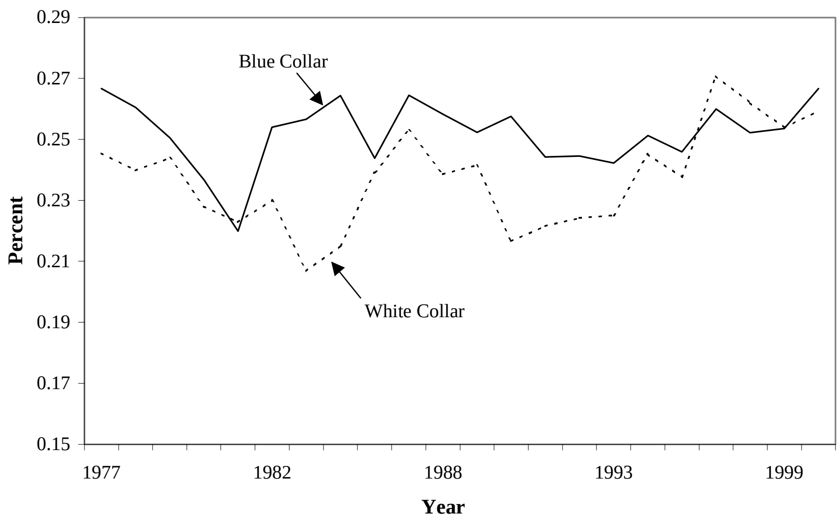
Finally, we consider the case where a worker responds that he has only one primary employer in each year, but was not working as of the first March Survey. Starting in 1994, the CPS ascertains whether the worker is actively looking for employment. We classify workers who were coded as actively looking for employment as job changers. For workers who were not actively looking for employment, we assume that they are on a temporary layoff and are subsequently re-employed by their same employer. For the pre-1994 period, we use information on what a worker indicated was his primary use of time in the prior week (CPS variable “major activity last week”) and a series of questions on methods of job search to classify the workers as either actively looking for work or not. Workers categorized by their answers to this final set of questions account for the remaining 4 percent of the job changers.

On average, job changers have different characteristics than “job stayers” (Appendix Table A1). Job changers are younger, less likely to be married and are less

[7] Workers with dual jobs are instructed to consider these as one job for the purpose of this question.
[8] We will fail to classify a worker as a job changer if he changes jobs at the end of the year and his new job is in the same detailed industry category as his primary job last year.

likely to own their home. Figure 1 shows the average 2-year job-changing rate for blue-collar and white-collar workers over the mid-1970s through 2000. Overall, there is a 25 percent probability that a worker changes jobs over a 2-year period. Blue-collar workers have higher job-changing rates than white-collar workers, although the job-change rate by white-collar workers has trended upward since the early 1980s. By the end of the 1990s, white-collar workers experienced the same degree of job instability as blue-collar workers.[9]

**Figure 1. Two-Year Job Changing Rates: By Occupation**




*Notes:* 2-year job turnover rates. Turnover rates could not be calculated from for 1985 and 1995 and were imputed using linear interpolation.

**C. Industry-Specific Real Exchange Rates**. The theoretical literature suggests that exchange rates affect workers' wages through shifts in labor demand. These demand shifts arise from the revenue and cost implications of exchange rate movements on domestic sales, foreign sales and the use of imported inputs. Due to differences in international product destinations and sources of import competition, a single exchange rate measure for the U.S. may be a poor proxy for the relevant exchange rate fluctuations facing firms in a given industry. This suggests using an industry-specific exchange rate

[9] See Farber (1997) for a detailed discussion of trends in job stability based on the CPS Displaced Worker Surveys.

series. For a given industry and year, we construct each industry-specific real exchange rate as a weighted average of the bilateral real exchange rates of U.S. trading partners (bilateral real exchange rates are from the IMF's *International Financial Statistics*). The weights used in this construction vary across industries and over time, and are the shares in industry trade of each U.S. trading partner.[10]

Theory suggests and prior empirical work finds that the wage and employment implications of exchange rates, at least in aggregated industry data, are strongest when the permanent component of the exchange rate is used (Campa and Goldberg 2001). By contrast, the transitory component has been found to have more of an effect on overtime hours and wages. There are many available methods of decomposing a macro time-series into its permanent and transitory components. Two common approaches are the Beverage and Nelson (1981) (BN) and the Hodrick and Prescott (1997) (HP) filters. Below we apply the BN filter where we assume that the first difference of the quarterly log exchange rate follows an AR(4) process (see Appendix description).[11]

**D. Aggregate and Local Demand Variables**. In order to estimate the effect of exchange rates on wages we need to isolate these effects from other time-series factors such as those generated by industry trends, aggregate cycles and local cycles. Industry wage trends are captured in our wage growth specifications by a set of 2-digit industry effects. We control for aggregate cyclical effects on wages by including real GDP growth in the specification given by (5). Local labor market demand conditions are controlled for using a methodology developed by Topel (1986). For each state, we regress the logarithm of state private-sector non-agricultural employment on a quadratic time trend. The residuals from these regressions, $\gamma_{rt}$, measure the deviations of actual state employment from trend employment. Similarly, we regress the logarithm of national private sector

[10] We used three year averages lagged by two years of trade shares for bilateral transactions with 34 U.S. trading partners. Using lagged trade shares avoids the potential issue of endogeneity of the composition of U.S. trade flows with respect to exchange rate movements. Our methodology for the weighted export plus import share exchange rate parallels the approach implemented by the Federal Reserve Board of Governors. See Goldberg (2002) for an extensive treatment of the properties of industry-specific versus aggregated exchange rate series.

[11] For a discussion of the relative merits of the BN and HP filters, see the exchange between Pedersen and Cogley in the August 2001 issue of the *Journal of Economic Dynamics and Control*. For background on the statistical properties of real exchange rate series see Beveridge and Nelson (1981), Huizinga (1987) and

non-agricultural employment on a quadratic time trend. The residuals from this regression, $\gamma_t$, control for the aggregate business cycle. The proxy for local relative demand conditions, $y_{rt}$, is given by:

$$y_{rt} = \gamma_{rt} - \gamma_t \tag{6}$$

The local relative demand conditions variable measures in a given year the state employment residual as a deviation from the national employment residual. Larger values represent relatively tighter conditions in that local labor market than for the country as a whole.

**E. Sample Selection**. As is clear from the descriptive statistics given in Appendix Table A1, our matched-CPS sample is not a random subsample of male workers from the March CPS surveys. To be matched across consecutive March surveys, a worker must remain in the same house over the intervening year. If the entire household moves or if the worker leaves the household, he cannot be matched. The CPS is a household based survey, and it makes no attempt to track down individuals who change residences. A second restriction imposed on our estimation sample is that a worker must have reported earnings in each of the two years in order for us to be able to compute his wage growth. If someone is out of the labor force for an entire year, or if he is employed but refuses to answer the earnings question, then we cannot compute his wage growth.

The extent of sample selection or attrition bias can be estimated by comparing the matched-CPS data to a similar survey where movers are followed. Neumark and Kwaguchi (2001) compare the matched-CPS to the Survey of Income and Program Participation (SIPP) data. The SIPP uses a very similar sampling frame and design as the CPS, but makes a substantial effort to track people who change residences between surveys. They compare results from the matched-CPS and the SIPP data for empirical specifications of the union wage premium and the marriage wage premium. They find negligible and statistically insignificant evidence of attrition bias arising from the

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Campa and Goldberg (1999).

inability to follow movers as in the matched-CPS.

While the Neumark and Kawaguchi (2001) findings do not point to any likely problems with sample selection issues when using the matched-CPS data, their findings might be sensitive to the empirical question being asked. As a precaution, we proceed and model the two sources of selection for our estimation sample. The first step toward controlling for these two sources of sample selection is to empirically model the processes for matching an individual across March surveys and for having reported earnings. Given that non-matches are primarily due to geographic mobility, our empirical specification borrows from the migration literature.[12] This literature shows that mobility is strongly tied to many individual characteristics such as age, education, and marital status. In addition, mobility is reduced by factors that increase the transaction costs of moving. We proxy these transactions costs with indicator variables for whether the individual owns a house and for whether children are present in the household. We use these same demographic factors to help control for whether a worker has reported earnings.

Specifically, to control for possible sample selection biases we begin by letting $I_{1it}^{*}$ denote an unobserved index of the desire by an individual *i* to stay in the same house during period *t*. We use the normalization that if this index exceeds zero, then the man does not move and we can match him across surveys. Let $I_{1it}$ denote an indicator that takes a value of one if we match the individual, and zero if we cannot match him. Similarly, let $I_{2it}^{*}$ and $I_{2it}$ denote the unobserved and observed indices for whether a man has reported earnings. Let $X_i$ denote a vector of demographic characteristics for individual *i* that affect his mobility and propensity to report earnings. Combining these indices with our earlier wage growth equation, we have the following joint specification where we have relabeled the composite residual in the wage growth equation for ease of exposition, i.e. $v_{3ijt} = \mu_{2i} + \mu_{2j} + \Delta\varepsilon_{it}$ .

[12] See for example, Bartel (1979).

$$
\begin{aligned}
&I^*_{1it} = X_{it}\theta_1 + v_{1it} \\
&I_{1it} = 1 \text{ if } I^*_{1it} > 0, \text{ 0 otherwise} \\
&I^*_{2it} = X_{it}\theta_2 + v_{2it} \\
&I_{2it} = 1 \text{ if } I^*_{2it} > 0, \text{ 0 otherwise} \\
&\Delta w_{ijrt} = \Delta Z_{it}\beta + \Delta V_{rt}\gamma + \Delta Y_t\delta + v_{3ijt} \\
&\begin{pmatrix} v_{1it} \\ v_{2it} \\ v_{3ijt} \end{pmatrix} \sim N(0,\Sigma) \text{ where } \Sigma = \begin{bmatrix} 1 & \sigma_{12} & \sigma_{13} \\ \bullet & 1 & \sigma_{23} \\ \bullet & \bullet & \sigma_3^2 \end{bmatrix}
\end{aligned}
\tag{7}
$$

The expression for the expected wage growth, conditional on an individual being matched across surveys and having reported earnings, depends on whether there is any correlation between the residuals in the survey matching equation, $I^*_{1it}$, and in the reported earnings equation, $I^*_{2it}$. We measure this correlation, $\sigma_{12}$, by estimating a joint Probit model for whether an individual is matched across March surveys and whether the individual has reported earnings in the base year survey. The joint Probit yields a $\sigma_{12}$ equal to 0.13. Given the low value of $\sigma_{12}$ we assume that the residuals in the survey match and reported earnings equations are independent. In this case, the expression for the expected wage growth, conditional on an individual being matched across surveys and having reported earnings, becomes

$$
\begin{aligned}
\mathrm{E}(\Delta w_{ijrt} \mid I_{1it}=1,\, I_{2it}=1) &= \Delta Z_{it}\beta + \Delta V_{rt}\gamma + \Delta Y_t\delta + \mathrm{E}(v_{3ijt} \mid v_{1it} > -X_{it}\theta_1,\, v_{2it} > -X_{it}\theta_2) \\
&= \Delta Z_{it}\beta + \Delta V_{rt}\gamma + \Delta Y_t\delta + \sigma_{13}\frac{\phi(X_{it}\theta_1)}{\Phi(X_{it}\theta_1)} + \sigma_{23}\frac{\phi(X_{it}\theta_2)}{\Phi(X_{it}\theta_2)}\ ,
\end{aligned}
\tag{8}
$$

where φ( ) and Φ( ) are the standard normal density and cumulative density functions. The ratio of the density to the cumulative density evaluated at $X\theta_1$ and $X\theta_2$ are the Mills ratios which are used to correct for any sample selection.[13]

The first Mills ratio corrects for sample selection effects arising from matching across surveys, while the second Mills ratio corrects for sample selection effects arising from requiring a worker to have reported earnings. Given the lack of correlation between

[13] See Maddala (1983, pg 278-283) for a reference and Ham (1982) for an application.

the match and reported earnings specifications, we calculate these two Mills ratios using $\theta$ 's estimated from univariate Probit models. We use our proxies for the transaction costs of moving to help identify any selection effects.[14]

The Probit results used to calculate the two Mills ratios are summarized in Appendix Table A2. As expected, married men with older children who own their own home are much more likely to be matched across surveys. The probability of being matched also increases with age, and is higher for individuals who have at least a high school education and for nonwhites. These same results carry over to the probability of having reported earnings, except that the probability is lower for nonwhites and for individuals who own their home.

The final component that we need to estimate in order to calculate the elasticity decomposition in equation (2) is the impact of the exchange rate on the probability that a worker makes a job change over the two year period. Here we estimate selection corrected Probit models where the dependent variable is our two year job changing indicator and the control variables are the same as in our wage growth specification.

## IV. Estimation and Results

The empirical analyses reported in this section provide estimates of expected wage elasticities with respect to exchange rates, where we focus on the importance of occupation and educational attainment. We then decompose the channels for these wages effects, identifying whether they arise for workers who remain on the job, through the wage penalty for workers who change jobs, or through a change in the probability of job transitions. We also examine whether the subset of job changers who are industry-switchers have more extreme outcomes than the workers who change jobs but remain within their same industries.

The results for our baseline wage growth specifications covering all workers in the private non-agricultural sector are given in Table 1 (columns 1 and 2). Wage growth is declining in age until a worker reaches his mid-thirties, increases until he reaches his

[14] Three variables are included in these probit specifications, which are excluded from our wage growth specification. These include two variables for the presence of children in the household, and a variable indicator for whether the household head owns or rents the home. In addition, marital status enters in its level form in the probits, and in change form in the wage growth specification. The probit specifications also include year effects that are excluded in the wage growth specification.

early fifties, and then again declines until retirement. College workers experience 3 percent faster wage growth than high school dropouts. While married men earn a wage premium, the transition into marriage does not result in higher wage growth, while the transition from marriage results in roughly a 2% higher wage growth. Finally, wage growth varies positively with both aggregate and local demand conditions.

To show the implications of controlling for sample selection, we present results with and without such controls. Both Mills ratios have positive and significant coefficients, indicating that expected wage growth on average is higher for men who do not move and who report their earnings. However, the impact on the exchange rate elasticity from controlling for selection effects is minimal. This is generally true for all of the subsamples we have examined. The issue of selection or attrition bias in the matched-CPS does not seem critical for our application, consistent with the findings by Neumark and Kawaguchi (2001).

Finally, the baseline job changing Probit results are given in column 2 Table 1. The data indicates that the likelihood of changing jobs is declining with age, declines with a transition to marriage, increases with a transition out of marriage, is lower for high school dropouts and is procyclic with respect to changes in real GDP.

| **Table 1. Baseline Specifications for Wage Growth and Job Changing** | | | |
|---|---|---|---|
| | **Wage Growth** | | **Job-Changing** |
| Variable | (1) | (2) | (3) |
| Age | –0.116** (0.007) | –0.117** (0.007) | –0.254** (0.012) |
| Age squared (x10) | 0.028** (0.001) | 0.028** (0.001) | 0.512** (0.030) |
| Age cubed (x1,000) | –0.022** (0.001) | –0.023** (0.001) | –0.441** (0.025) |
| Becomes married | 0.004 (0.008) | 0.005 (0.008) | –0.054** (0.023) |
| Becomes single | –0.004 (0.009) | 0.018** (0.009) | 0.100** (0.023) |
| High school graduate | –0.005 (0.006) | 0.004 (0.006) | 0.021* (0.011) |
| Some college | 0.007 (0.008) | 0.014* (0.008) | 0.066** (0.012) |
| College graduate | 0.029** (0.007) | 0.034** (0.007) | 0.029** (0.012) |
| % change in real GDP | 1.231** (0.116) | 1.158** (0.097) | 0.351* (0.182) |
| Change in local relative demand conditions | 0.912** (0.159) | 0.914** (0.157) | 0.061 (0.060) |
| % change in real exchange rate | –0.016 (0.047) | –0.000 (0.042) | –0.007 (0.060) |
| Mills ratio – Match across surveys | | 0.057** (0.013) | |
| Mills ratio – Nonmissing wage | | 0.087** (0.029) | |
| 2-Digit industry effects included | Yes | Yes | No |
| N = 113,612 | | | |
| ** denotes significant at the 5% level. * denotes significant at the 10% level. *Notes for columns 1 and 2*: OLS standard errors are given in parentheses. *Notes for column 3*: Probit coefficients with standard errors in parentheses. | | | |

**A. Exchange rates and wages**. Table 2 provides a summary of the wage elasticities with respect to exchange rates by occupational category (White Collar versus Blue Collar) and by educational attainment.[15] We report average wage elasticities (with

[15] Blue collar workers include those employed in precision production, craft and repair occupations and

depreciations being upward exchange rate movements).[16] The exchange rate elasticity of wages for the overall population, from the baseline specification, is reproduced in the first data cell of Table 2. Observe that for the private non-agricultural sector as a whole, wages are completely insensitive to movements in the value of the dollar. This finding contrasts with the results in the literature that are based on aggregate wage regressions. For example, Revenga (1992) estimates a significant wage elasticity with respect to import prices of 85 percent. Campa and Goldberg (2001) identify a similar range of significant elasticities, with the wage effects rising over time as industries became more export oriented.

**Table 2. Wage and Job Changing Elasticities with Respect to the Dollar: By Occupation / Education Group and Job Changing Status**

| | Wage Elasticities | | | Job Changing Elasticities |
|---|---|---|---|---|
| | Overall (1) | Job Stayers (2) | Job Changers (3) | (4) |
| Private Non-agricultural<br>113,612 | –0.000<br>(0.042) | –0.047<br>(0.062) | 0.138<br>(0.126) | 0.008<br>(0.073) |
| **Occupation:** | | | | |
| Blue Collar<br>57,342 | 0.086*<br>(0.052) | 0.007<br>(0.060) | 0.309**<br>(0.152) | –0.075<br>(0.098) |
| White Collar<br>56,270 | –0.087*<br>(0.053) | –0.101<br>(0.073) | –0.044<br>(0.123) | 0.067<br>(0.105) |
| **Educational Attainment:** | | | | |
| Less than high school degree<br>19,128 | 0.154*<br>(0.087) | 0.060<br>(0.086) | 0.428*<br>(0.232) | 0.212<br>(0.184) |
| High school graduates<br>46,145 | 0.042<br>(0.041) | –0.014<br>(0.050) | 0.206<br>(0.147) | 0.125<br>(0.109) |
| Some college +<br>48,339 | –0.108*<br>(0.056) | –0.125<br>(0.081) | –0.055<br>(0.112) | –0.217*<br>(0.111) |

*Notes*: Reported coefficients are wage elasticities with respect to a dollar depreciation. Standard errors are given in parentheses and have been adjusted for any non-independence of observations within a year. ** significant at the 5% level.

operators, fabricators and laborers.

[16] These elasticities are based on specifications that do not allow for time-varying trade orientation of industries. In the section on robustness checks, we allow for changing trade orientation of industries.

The other entries in the first column ("Overall") of Table 2 provide elasticities generated by specifications that combine all workers (both job stayers and job changers) in a particular occupational or educational group. The first interesting observation is that the negligible overall impact of exchange rates on wages reflects equal and opposing effects of exchange rates on Blue and White Collar workers. For Blue Collar workers, the data indicate a wage elasticity of 8.6 percent -- which is consistent with the earlier Revenga findings that had been computed over a sample of production workers. However, the estimated wage elasticity for White Collar workers is estimated to be –8.7 percent. Given the roughly equal numbers of each group of workers in the sample, these two effects offset each other when we pool the two groups. Wages *are* responsive to movements in the value of the dollar, but this responsiveness is masked in the aggregate.

Continuing down the "Overall" column of Table 2, we explore whether the dichotomy in results for Blue and White Collar workers reflects average skill differences across these groups. Workers are disaggregated into three broad skill groups based on their education attainment – a low skill group consisting of workers who did not complete a high school degree, a moderate skill group consisting of workers who completed a high school degree but did not continue their education beyond high school, and a high skill group consisting of workers with at least some post high school education.

The estimated elasticities show a sharp contrast between the wage elasticities for low and high skill workers. The wage elasticity for low skill workers exceeds 15 percent (nearly double the Blue Collar elasticity), implying that a ten percent dollar depreciation is associated with a 1.5 percent increase in real wages. In contrast, the wage elasticity for high skilled workers is nearly –11 percent. A ten percent dollar depreciation is associated with a 1.1 percent *decrease* in wages for high skilled workers.

These findings imply that swings in the strength of the dollar will shift the relative wage structure in the labor market. A ten percent dollar depreciation is associated with a 2.5 percent decrease in the relative real wages between high and low skilled workers. Earlier work suggested that dollar movements could affect income inequality primarily through its impact on low skilled wages. Hoynes (1999) documents that less skilled workers have weekly earnings that are more sensitive to general cyclical shocks.[17] Our results suggest that this tendency is reinforced by the impact of dollar movements on the wages of high skilled workers. We speculate later in the paper on what is driving these opposing wage effects.

What role does job changing play in generating these wage effects from exchange rate movements? We also explore this question in columns 2 and 3 of Table 2. For a specific occupation or education group, reading across the associated row in Table 2 contrasts the wage elasticities for workers who stay with their same employer for workers who change employers over the two-year period. In addition, in column 4 we present the elasticity of the rate of job changing to the exchange rate for that occupation or education group.

Consider the results for Blue Collar workers. The overall elasticity of 8.6 percent is driven entirely by workers who change jobs. For Blue Collar workers, the elasticity for job stayers is zero while the elasticity for job changers is over 30 percent. This pattern is mirrored in the results for workers with less than a high school degree. Here we find that the elasticity for job changers is nearly 43 percent. In contrast, the wage elasticity for high skilled workers is larger (in absolute value) for job stayers than for job changers, although these elasticities are imprecisely estimated.

There is a recent literature that explores the role of exchange rates in shifting the rate of job turnover. Gourinchas (1999), for example, found that depreciations tend to reduce both job creation and job destruction in manufacturing industries, so that churning of workers declines when the dollar is weakened. This decline in job-changing was strongest in import-competing industries. As shown in column (4) of Table 4, our data indicate that there is again a dichotomy by skill level in the impact of dollar movements on job churning. Dollar depreciations lower the rate of job changing of Blue Collar and low skilled workers, and increase the rate of job changing for White Collar and high skilled workers. However, the job changing elasticities are generally smaller than the wage elasticities and less precisely estimated.

[17] See also Freeman (1995) and Wood (1994) for detailed discussions of the impact of trade on the demand for low skilled workers.

**B. The Mechanism for Exchange Rate Effects on Wages**. The overall picture that emerges from Table 2 is that during a period when the dollar is weakening there is a decline in the probability that low skilled workers change jobs, and those that do change jobs earn even higher wages. In contrast, there is a increase in the probability that high skilled workers change jobs, and current employers offer these workers lower wages. Using the decomposition in equation (2), we find that job changing affects the overall wage elasticity primarily through the impact of exchange rate shifts on the consequence of job changing and not on the incidence of job changing.

This mechanism of wage consequences through job-changing resonates with findings of a parallel literature on the consequences of job displacement. That literature stresses that whether the worker is re-employed in his same industry or has to switch industries is an important determinant of a worker's post-displacement earnings (see Carrington (1993), Kletzer (1998, 2001), Ong & Mar (1992) and Podgursky & Swaim (1987)). Displaced workers who switch industries on average experience larger wage losses than workers who are re-employed in their same industry. We investigate this channel in the next table.

In Table 3 we present wage elasticities drawn from specifications where each job changer is differentiated according to whether he stays within or changes his 2-digit industry of employment. In addition, we present the elasticity of the probability that a worker both changes jobs and changes industries with respect to the exchange rate. This distinction is intended to shed light on whether dollar movements affect the consequence of job changing primarily by shifting the incidence and wage consequences of switching industries, or by affecting wages for workers who change jobs but stay in their same industry.

Comparisons of entries in the rows of Table 3 show that industry switching *per se* generally plays a minor role in explaining the impact of the exchange rate on the wages of job changers. Among job-changers, exchange rate movements have similar effects on those workers who stay within the same industry, and those workers who change industries. Consider the large wage effect for Blue Collar job changers. The overall wage elasticity for job changers is 0.31. This consists of a wage elasticity of 0.36 for changers who stay in their same industry, and a wage elasticity of 0.23 for workers who both change jobs and switch industry. As the last column of the table indicates, swings in the strength of the dollar also have negligible impacts on the likelihood that a worker who changes jobs will also switch his industry. This pattern of results is mirrored in the results for high school dropouts. The only exception is for White Collar and the more highly educated workers, where dollar depreciations reduce the likelihood of industry-switching conditional on job change. Overall, we do not observe an important role for industry switching in explaining the impact of exchange rates on the wage consequences of job changers.

**Table 3. Wage and Industry Switching Elasticities with Respect to the Dollar, by Occupation / Education Group for Job Changers**

| | Job Changer Wage Elasticities | | Probability of an Industry Switch |
|---|---|---|---|
| | Same Industry | Change Industry | Given a Job Change |
| Private Non-Agricultural<br>113,612 | 0.174<br>(0.122) | 0.087<br>(0.155) | –0.133<br>(0.107) |
| **Occupation:** | | | |
| Blue Collar<br>57,342 | $0.359^{**}$<br>(0.161) | 0.235<br>(0.203) | 0.031<br>(0.384) |
| White Collar<br>56,270 | –0.025<br>(0.133) | –0.071<br>(0.143) | $–0.311^{*}$<br>(0.155) |
| **Educational Attainment:** | | | |
| Less than high school degree<br>19,128 | $0.443^{*}$<br>(0.247) | 0.406<br>(0.337) | 0.049<br>(0.273) |
| High school graduates<br>46,145 | 0.224<br>(0.172) | 0.165<br>(0.153) | –0.185<br>(0.166) |
| Some college +<br>48,339 | –0.012<br>(0.118) | –0.116<br>(0.205) | –0.171<br>(0.166) |

*Notes*: Reported coefficients are wage elasticities with respect to a dollar depreciation. See text for list of control variables. Standard errors are given in parentheses and have been adjusted for any non-independence of observations within a year. Standard errors for the industry switching elasticities take the mean rate of industry switching as data.
$^{**}$ significant at the 5% level.

**D. Robustness checks.** In this section we consider the robustness of our findings to changes in the exchange rate measure used in the regression specifications. We consider both alternative measures for the strength of the dollar, and interactions between the exchange rate and measures of industry trade exposure.

In this paper, our reported results are generated using the permanent components of industry-specific measures of the strength of the dollar. Theory suggests that firm will react more forcefully to "permanent" changes in the value of the dollar. Focusing on our industry-specific exchange rates, we find little difference in our results from using the overall or the permanent component of the bilateral exchange rate shocks. In addition, we find that our results are similar when we use the permanent component of the Board's broad exchange rate measure. Overall, the standard errors are slightly tighter when we use our industry specific measure.

An important related question is whether the extent of these effects have increased over time as U.S. manufacturing and, to a lesser extent, nonmanufacturing industries have become more exposed to trade. Theory suggests that the labor demand response by firms in an industry to exchange rate movements should depend on the "trade openness" of that industry. Trade exposure has steadily increased over our sample period. For example, consider the trade exposure measure defined as the sum of the industry export and import trade shares.[18] This trade exposure measure increased from an average of 6.6 percent in 1977 to 10.6 percent in 1997 (weighted by the number of workers in each industry). When we interact the change in the exchange rate with the industry trade share, we find that the interaction effect is insignificant. To further investigate this finding, we split the sample around 1988. The overall wage elasticity for Blue-Collar workers in the pre-1988 period is 11.1 percent. In the post-1988 period, this Blue-Collar wage elasticity has fallen to –3.8 percent. Clearly, the increasing trade exposure of industries has had less of an effect on overall wage responsiveness than other structural changes that have occurred within United States labor markets.

[18] Measuring trade exposure by summing the export and import shares is relevant if the primary effects of exchange rate movements are on producer revenues derived from both foreign sales and domestic sales due to import penetration of domestic markets. An alternative measure is the *difference* between the industry export and import shares. Empirically the import penetration in an industry and the industry's use of imported inputs are highly correlated and similarly scaled. See Campa and Goldberg 1997.

## V. Summary and Conclusions

We explore in this paper the magnitude of wage sensitivity to dollar movements and the channels for this sensitivity, through a combination of adjustments to wages by: (1) workers who remain with their same jobs, (2) workers who receive an alternative wages after switching jobs, or (3) induced changes in the frequencies of switching by particular classes of workers in a specific industry groups.

We have demonstrated that the process of aggregation masks large and significant wage responses to dollar movements within particular industry groups**.** While the overall elasticity of wages to the exchange rate is negligible, this reflects offsetting effects for high and low skilled workers. The data indicate that an appreciation of the dollar will exacerbate wage inequality in the labor market. A possible explanation is that a strong dollar creates cost pressures which firms attempt to offset through higher labor productivity. In response to a strong dollar, firms may be induced to invest more in processes that are complementary with high skilled workers. So, while the direct affect of the dollar appreciation is an inward shift in the demand for labor, there also may be a shift in the composition of labor demand toward high skilled workers.[19]

Furthermore, job changers mainly shoulder the effects of dollar movements. Those workers who change jobs experience larger wage changes than the workers who remain on their jobs. While dollar movements do have some effect on job-turnover rates, the size and importance of this channel is generally not substantial for most workers. In addition, we find little role for industry switching as an explanation for the impact of exchange rates on the wages of job changers.

Overall, this paper has demonstrated that dollar fluctuations translate into significant wage effects for specific groups of workers in the U.S. economy. Many of these distributional effects are lost in studies of aggregate data, either because of changes in the composition of the workforce or because of aggregation problems. The results from aggregate studies can give misleading conclusions when applied to the question of the actual disruptiveness of dollar fluctuations for different skill classes of workers.

[19] See Katz and Murphy (1992) and Berman, Bound and Griliches (1994). Feenstra (2001) provides an overview.

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**Appendix**

Computation of Permanent Exchange Rates: The Beveridge-Nelson procedure decomposes an I(1) time series into its transitory and permanent components. We follow Beveridge and Nelson (1981) and Huizinga (1987), and model the exchange rate decomposition using lags of the real exchange rate. In particular, we assume that the first differences of the quarterly (log) real exchange rate follow an AR(4) process, so that the transitory departure of the real exchange rate from its expected long-run equilibrium, $e_t^{BN}$, is given by:

$$E_t\left(e_t^{BN}\right) = -E_t\left(\sum_{j=1}^{\infty} \Delta e_{t+j} \mid \Delta e_t, \Delta e_{t-1}, \Delta e_{t-2}, \Delta e_{t-3}\right)$$

The actual variance decomposition results suggest that the temporary component of exchange rate changes accounts for only a small proportion of the variance of the real exchange rate series.

**Appendix Table A1. Description of the Population of Male Workers**

| | All Observations<br><br>Variable Means<br>(percent of population) | | Job changers Only<br><br>Variable Means<br>(percent of population) | |
|---|---|---|---|---|
| | Unmatched | Matched | Unmatched | Matched |
| Manufacturing | .31 | .35 | .22 | .24 |
| Higher Markup Industries | .14 | .16 | .10 | .10 |
| Lower Markup Industries | .17 | .19 | .12 | .14 |
| Non-Manufacturing | .69 | .65 | .78 | .76 |
| Job changers (1-year rate) | .19 | .15 | | |
| Less than High School Degree | .19 | .17 | .20 | .17 |
| High School Graduates | .40 | .40 | .41 | .41 |
| Some College | .21 | .21 | .21 | .22 |
| College Graduates | .20 | .22 | .18 | .20 |
| Age in years (standard deviation) | 36.9 (11.8) | 38.6 (11.3) | 32.0 (10.7) | 33.8 (10.8) |
| Race (Non-white) | .10 | .09 | .10 | .09 |
| Married | .67 | .75 | .55 | .65 |
| Own home | .66 | .77 | .52 | .66 |
| Number of Observations | 231,504 | 115,094 | 44,392 | 16,799 |

*Notes*: **All statistics for Matched CPS use base year values. Unmatched sample excludes individuals with missing earnings.

| **Appendix Table A2. Probability of Matching Individual Across Surveys & Nonmissing Wage** | | |
|---|---|---|
| | Match Across Surveys | Nonmissing Wage |
| Variable | | |
| Married | 0.144** (0.006) | 0.294** (0.007) |
| Unmarried kids < 18 years old | 0.043** (0.006) | 0.058** (0.007) |
| Unmarried kids < 6 years old | –0.016** (0.008) | 0.105** (0.009) |
| Age of household head | 0.916** (0.026) | 0.403** (0.028) |
| Age squared | –0.037** (0.001) | –0.016** (0.001) |
| Age cubed (x100) | 0.065** (0.002) | 0.026** (0.002) |
| Age fourth (x1,000) | –0.004** (0.000) | –0.002** (0.000) |
| High school graduate | 0.129** (0.006) | 0.102** (0.007) |
| Some College | 0.081** (0.008) | 0.102** (0.008) |
| College Graduate | 0.083** (0.008) | 0.031** (0.008) |
| Nonwhite | 0.037** (0.007) | –0.320** (0.007) |
| Own a home | 0.598** (0.005) | –0.121** (0.006) |
| Year effects included | Yes | Yes |
| 2-Digit industry effects included | Yes | Yes |
| N = 328,663 | | |

*Notes*: Probit coefficients are given with standard errors are given in parentheses. Sample restricted to individuals who are "at risk" of being matched in the subsequent March CPS survey (includes individuals with missing earnings). ** denotes significant at the 5% level