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Unemployment Insurance Versus Individual Unemployment Accounts and Transitions to Formal Versus Informal Sector Jobs

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Unemployment Insurance versus Individual Unemployment Accounts and Transitions to Formal versus Informal Sector Jobs

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Abstract

This paper analyzes the impact of the unemployment safety net on the speed of returning to employment in 6 major urban areas of Brazil. Distinguishing between formal and informal sector jobs as destinations and controlling for unobserved characteristics that affect transitions, we find that previous formal sector work (which opens eligibility to unemployment insurance (UI) and individual unemployment accounts (FGTS)) accelerates the rate of transition to formal sector jobs and reduces the rate of transition to informal sector jobs. Additional unemployment benefits provided through the UI system do not directly affect transitions to formal sector jobs, although they do slow transitions to informal sector jobs. The results suggest that the unemployment safety net may affect outcomes by reducing search intensity for informal sector jobs, while demand side phenomena may be driving the results for transitions to the formal sector.

Résumé

Dans cet article, je considère l'impact du système d'aide aux chômeurs sur la vitesse de retour en emploi dans 6 zones urbaines majeures du Brésil. Je distingue les transitions vers les emplois du secteur formel de celles vers les emplois du secteur informel en tenant compte de l'hétérogénéité inobservée. Un emploi dans le secteur formel ouvre droit à l'assurance chômage et aux comptes privés du chômage (FGTS). On trouve que le passage par un emploi dans du secteur formel augmente le taux de transition vers des emplois du secteur formel et réduit le taux de transition vers les emplois du secteur informel. Le revenu supplémentaire fourni par l'assurance chômage n'affecte pas la vitesse de retour en emploi du secteur formel mais diminue cette vitesse pour les transitions vers le secteur informel. Les résultats suggèrent que l'aide aux chômeurs affecte les transitions en réduisant l'intensité de la recherche pour des emplois du secteur informel. Des phénomènes qui affectent la demande de travail pourraient être à l'origine des effets observés pour les transitions vers le secteur formel.

JEL Codes / Codes JEL: J64, O54
Key Words : Brazil, Unemployment Insurance, FGTS, Informal Sector, Competing Risks
Mots Clés : Brésil, Assurance Chômage, Comptes privés de chômage (FGTS), Secteur informel, Risques concurrents.

1 Introduction

Brazil's social safety net for unemployed workers is composed of two principal components, unemployment insurance (UI) and personal unemployment accounts (FGTS). Although much attention has been devoted to evaluating the impact of FGTS on labor turnover, relatively little attention has been devoted to studying unemployment insurance in Brazil or its interaction with FGTS unemployment accounts.

This paper attempts to remedy that situation by analyzing the impact of eligibility for unemployment insurance on the speed at which unemployed workers return to work in Brazil's 6 largest metropolitan areas (Recife, Salvador, Belo Horizonte, Rio de Janeiro, São Paulo and Porto Alegre). Given the strong presence of the informal sector in Brazil, we distinguish transitions to jobs in the formal sector from transitions to jobs in the informal sector. Also, since UI is only available to those individuals who were previously employed in the formal sector, it is likely to be endogenous as it is correlated with unobservable characteristics that make workers more or less likely to work in the formal sector (or to work at all).

From an econometric point of view, these considerations lead us to build and estimate a competing risks duration model with one risk being formal sector employment, the other being informal sector employment. We allow for correlation between the risks through the unobserved individual-specific heterogeneity components of the model, which also helps control for the endogeneity of UI eligibility. Our model also accommodates multiple spells of unemployment, time-varying covariates, stock sampling and different baseline hazards according to eligibility profiles.

As in the main previous study on unemployment insurance in Brazil (Cunningham (2000)), we find that there is relatively little difference between receiving FGTS and receiving FGTS and UI, regardless of the length of UI receipt. There is a clear negative effect of benefit eligibility derived from having previously worked in the formal sector on transitions to informal sector jobs, even after controlling for unobserved individual-specific heterogeneity. This effect increases as people become eligible for increasingly longer lengths of UI, but the time profile of transitions does not significantly vary with UI eligibility in the most complete specification. In particular, there does not appear to be a spike in the hazard rates just at benefit expiration (although there is an indication of a spike at 6 months regardless of the profile of benefit receipt). The effect on transitions to the formal sector appears to be dominated by a negative signal associated with previous informal sector work (and not the individual per se) in the eyes of formal sector employers, as these transitions from unemployment to formal sector work for ex-informal sector individuals are the slowest of all those studied in this paper.

The rest of this paper proceeds as follows. After briefly describing the institutional environment and previous research on unemployment insurance in Brazil in section 2, section 3 describes the data used for the analysis and demonstrates the importance of the observability issue : the data provide the information necessary to determine eligibility unemployment insurance but do not provide information on UI benefit receipt. Section 4 briefly describes the econometric specification (the details can be found in appendix A) and section 5 presents the results. Section 6 discusses some caveats that should be kept in mind when interpreting the results and then draws implications of our results for policy. Section 7 concludes.

2 Unemployment Insurance in Brazil

2.1 Brazilian Labor Market

As the World Bank (2002) notes, the Brazilian labor market is characterized by two main features: very high labor turnover and a large informal sector.

Corseuil et al. (2003) show that annual job reallocation averages 33% over the 1991-98 period. Meanwhile, the average yearly job creation rate was about 17.3% and the destruction rate of 15.5%. This evidence is confirmed by several studies, like Maloney (2004) who shows that the average job tenure is among the lowest worldwide (6.3 years with the same employer). Menezes-Filho and Fernandes (2003) and Gonzaga (2003) found the same pattern of job creation and destruction using different data sets.

This feature of Brazilian labor market has two contradictory effects on welfare. First, a flexible labor market can facilitate macro-level job reallocation and could be growth enhancing as it does not perpetuate low productivity jobs. Second, the high degree of income uncertainty can have a direct effect on individual well-being and could also be harmful for efficiency as it encourages unemployed people to accept any offer they receive, independent of the quality of the match between the worker and the employer (thereby crowding out potentially more productive matches). High labor turnover can also discourage workers from investing in human capital, thus reducing economic efficiency. In the context of such high job turnover, the design of the social safety net becomes very important.

The second important feature of the Brazilian job market is the large informal sector, comprising roughly 50% of the labor force. Two main groups are typically counted in this sector, workers *sem carteira de trabalho assinada* and the self-employed. Informal sector workers do not have access to most benefits or social security.

Domeland and Fiess (2002) estimate the probability of becoming unemployed conditional on individual characteristics and find that the group who faces the highest risk of unemployment are young, low educated, informal sector workers. This result holds even after controlling for individual characteristics. Orellano and Picchetti (2001) also study labor turnover, but this time distinguishing between being fired or quitting and using a bivariate probit model. On the re-employment side, Menezes-Filho and Picchetti (2000) mobilize multiple statistical techniques, such as non parametric (Kaplan-Meier) duration estimation, a proportional hazards estimation and a parametric logistic estimation. They find that unemployed people in the São Paulo metropolitan area typically have shorter periods without work.

2.2 Income Insurance in Brazil

The income insurance safety net has two main institutions, unemployment insurance (*seguro desemprego*) and the FGTS (*Fundo de Garantia do Tempo de Serviço*), a fund financed by employers for their employees and available to workers in case of dismissal.

Brazil's unemployment insurance program was created in 1986, among other reforms within the context of the Cruzado Plan. The program was reformed in 1990 and in 1994 extending the coverage of the program. By 1990, 43% of all dismissals from the formal sector were covered by the unemployment insurance according to Cunningham (2000). Eligibility for unemployment insurance requires:

- dismissal without just cause
- lack of other forms of income
- employment in the formal sector for the six months preceding application for unemployment insurance.

The average monthly benefit depends on the average wage of the last three months prior to unemployment. However it can not be higher than twice the minimum wage, implying low replacement rates for some workers. The average benefit level was about 1.36 times the minimum wage in 2005 according to data from the Labor Ministry.

Dismissed formal sector workers also have access to an individualized fund, the FGTS. The employer contributes 8% of the individual's monthly wage to the fund, corresponding to roughly one extra month of wages per year. Workers have access to their FGTS if dismissed without just cause, when they retire, or if they need to provide a mortgage guarantee for a home purchase.

In case of dismissal, the worker can access his entire fund, which is augmented by a fine imposed on the employer in proportion to the FGTS accumulated in the job. This penalty was increased in 2000 to reach a rate of 50%. For many workers, the amount received by FGTS is higher than that received from unemployment insurance.

FGTS is an important cornerstone of the Brazilian income insurance system and has been studied in more detail than unemployment insurance. Many authors argue that the design of the FGTS generates incentives that explain many of the main features of the Brazilian labor market.

When the accumulated FGTS becomes attractive to workers, they may attempt to force a dismissal so as to gain access to the funds. Barros et al. (1999) study the effects of the 1988 constitutional reform which increased to 40% the severance fine in case of dismissal and found that dismissals were multiplied by four. Unfortunately, their differences in differences methodology does not generate conclusive results when considering the impact of the reform on specific groups. For Gonzaga (2003) there are also incentives to negotiate a fake dismissal when workers desire to access their FGTS, since employers would like to avoid paying the dismissal penalty. These authors conclude that the design of the FGTS can explain in part the high labor turnover and some level of informality. Gonzaga (2003) also shows that recent reforms, in particular the 2001 reform which included a firing penalty paid directly to the government, reduced the job destruction rate. This reform increased the firing cost, and duration of employment has increased significantly.

One of the few evaluations of unemployment insurance was undertaken by Cunningham (2000). She studied the effect of unemployment insurance on unemployment duration, post-unemployment wages and post-unemployment sector of activity. To identify the effect of unemployment insurance, Cunningham (2000) studies the reform of 1995, which modified the number of monthly payments received by some, but not all, groups.

Using a difference-in-differences methodology, Cunningham (2000) finds that unemployment insurance does not modify the post-unemployment wage. If individuals face job search frictions, unemployment insurance theoretically increases the reservation wage and should thus increase post-unemployment wages. This result does not appear to hold in the Brazilian case.

However, as Cunningham points out, jobs are not only defined by their wage level; non pecuniary benefits are also important and formal sector jobs grant access to several benefits. Hence, the author tests the impact of unemployment insurance on the probability of finding a formal or informal sector job. The theoretical framework incorporates important results from development theory concerning the role of the informal sector. Recently, the informal sector has been seen as fully integrated in the economy and is no longer treated as a residual sector. Maloney (2004) and Albrecht et al. (2007) develop models based on this view, in which individuals receive offers from both sectors, compare them and choose the one which provides the highest utility given their preferences.

In order to examine probability of transiting to a given sector, Cunningham estimates a multinomial logit model. The empirical evidence found by Cunningham does not show that unemployment insurance increases the probability of finding a formal sector job, which she interprets as evidence of the integration of formal and informal sectors. She does find, however, that higher unemployment benefits increase the probability of self-employment, as the benefits can provide the starting capital for the project.

Finally, Cunningham (2000) finds that unemployment insurance has no influence on the unemployment duration. Using a competing risk model, however, she does find that the unemployment duration decreases for transitions to the self-employment sector when the duration of unemployment insurance benefits increases.

Concentrating specifically on São Paulo, Menezes-Filho and Picchetti (2000) find (via their logistic regression) that there is an increasing hazard rate out of unemployment during the first six months of an unemployment spell for an ex-formal sector worker. This pattern corresponds more closely to what one would expect given that unemployment insurance benefits last a maximum of five months.

3 The *Pesquisa Mensal de Emprego* (PME) data

The sample used was taken from the *Pesquisa Mensal de Emprego*, or PME, database, a monthly household survey covering the six main Brazilian metropolitan regions: São Paulo, Rio de Janeiro, Salvador, Belo Horizonte, Porto Alegre and Recife. Each individual answers a maximum of eight interviews, following the American CPS survey structure which allows us to observe (at least partially) individual work histories for a period of 16 months. The PME provides information on many variables including education, age, gender and labor market status. The sample used covers the period from March 2002 to August 2007.

If the individual has been unemployed for less than an year, there is information on his previous job. Specially the PME provides information on the job duration and the formal or informal status, asking if the person had a *carteira de trabalho* assigned. The PME does not ask for information about unemployment insurance benefits directly, but we can simulate the benefits. As the information needed to calculate benefit eligibility is not available for every unemployment spell (in particular, the information is missing when the spell has been in progress for more than a year at the first observation date), we limit our attention to individual whose unemployment spell at the first time they are seen in the data has been ongoing for no more than 12 months.[1] This implies that the maximum observable completed spell in our data is 27 months, although there are very few exits to identify our models after 15 months of unemployment. We then exclude all first-time labor market entrants and individuals with missing values for covariates, but no other selection criteria are applied. Thus, of the 319,402 initial unemployment spell observations[2] in our data, we lost 115,189 because were unable to determine UI eligibility. 10,969 of the remainder had unemployment stocks of more than 12 months at the first survey date[3], 50,274 are eliminated as first-time labor market entrants and an additional 5,068 observations were lost due to missing data. The analysis data set thus contains 137,902 observations, corresponding to 72,897 unemployment spells.

In our analysis sample, 16,445 spells are not right censored. For all of these spells we dispose of information on the the sector of the future work. This information is summarized in table 1. Clearly, workers who came from the informal sector are much more likely to return there than are workers who were employed in the formal sector, and there seems to be a slight increase in the probability of returning to the formal sector with the length of UI benefits. However, these results ignore right censoring and differences in the composition of the each sub-population and thus can not be used to draw conclusions about the role of the unemployment safety net for returning to employment; we need a model (such as that described in section 4) to do that.

Table 1: Transition Rates by Unemployment Insurance Eligibility

	Formal	Informal
Ex-Informal Sector	17.42%	82.58%
FGTS Only	52.28%	47.72%
3 months UI	51.60%	48.40%
4 months UI	52.82%	47.18%
5 months UI	53.87%	46.13%

[1]Note that our estimation procedure, described in section 4 and appendix A, controls for stock sampling and thus this selection criterion will not bias our results.

[2]Since our estimation procedure allows for time-varying covariates (see appendix A), a given unemployment spell will generate an additional observation each time any of the covariates, in particular the segments of the piecewise-constant baseline hazard, change.

[3]We can still determine the eligibility of some people with a unemployment spells stock sampled at durations longer than 12 months because there is an additional question that allows us to identify those who have never worked. These people have no eligibility for the unemployment safety net even though they may have been looking for more than a year.

Table 2 gives the main characteristics of unemployed individuals for whom unemployment insurance eligibility can be calculated at the last observation of each spell. The average unemployment duration is about 5.3 months, a result in line with the average duration found by Menezes-Filho and Picchetti (2000). Some other important features of the data appear in the analysis of table 2. As found elsewhere, young people and people without a *carteira de trabalho* in the previous job are overrepresented in the stock of unemployed. A large fraction of unemployed say that they are currently in school, but the causality link is not clear.

Table 2: Descriptive Statistics for Spells of Unemployment

	Mean	Std. Dev.
Total Observed Duration	5.251	4.756
Demographic Characteristics		
Male	0.532	0.499
Age	29.772	10.912
Previous Training	0.228	0.420
Currently in School	0.190	0.392
1-3 Years of School	0.060	0.237
4-7 Years of School	0.274	0.446
8-10 Years of School	0.242	0.428
11 or More Years of School	0.394	0.489
Previous Job Characteristics		
Formal Sector	0.425	0.494
Eligible for 3 Months UI	0.081	0.273
Eligible for 4 Months UI	0.083	0.276
Eligible for 5 Months UI	0.161	0.368
Salaried	0.786	0.410
Indefinite Term Contract	0.381	0.486
Metropolitan Labor Market Conditions		
Unemployment Rate	11.192	2.708
Average Real Wage	1028.096	170.835
Participation Rate	56.307	3.113

4 The Econometric Specification

In this section we describe the econometric model used to estimate the effects of unemployment insurance on the rate of return to employment. After highlighting the key econometric issues that need to be considered, we lay out our approach to estimating the effect of the UI safety net on unemployment exit rates. The details of the model and its notation are presented in appendix A.

4.1 Data-Induced Econometric Issues

Given the complicated sampling / data availability configuration and the possibility of multiple destinations, the model needs to account for several important factors.

1. There are two possible destinations, formal and informal sector employment. A competing risk model with correlated risks is the most appropriate means of accommodating this setting.

2. The factors that determine the duration of an unemployment spell can change with time, thus the component duration models in the competing risk structure should allow for time-varying covariates.

3. Individuals can have several unemployment spells within a 16 month observation window. The model therefore needs to be able to accommodate multiple spells.

4. Given the relatively short length of the panel, restricting attention to flow sampled unemployment spells (spells that begin within the observation window) would result in too-small sample sizes. We thus need to accommodate stock sampling (spells that were already in progress at the start of the observation window).

5. The duration models that make up the competing risks need to be a function of unemployment insurance and FGTS receipt. However, neither of these is available in the PME data, so one must apply the eligibility criteria and make assumptions about implementation and take up rates. Here we will assume that the UI administration applies the rules correctly and that all eligible workers take up their benefits.[4]

6. Applying eligibility criteria requires knowing the length of the spell of employment immediately preceding the unemployment spell and the sector (formal or informal) in which the job was held. Questions concerning the spell of employment preceding the unemployment spell are only asked of those individuals who have been unemployed for no more than a year.

    - There is the potential for selection bias with respect to the individuals for whom this information is available, implying that one needs to incorporate a correction for the observability of unemployment insurance eligibility into the model.

[4] To the best of our knowledge, there has been no research on the determinants of takeup of UI benefits in Brazil on the basis of which we could correct our estimates.

- The duration models in the competing risk framework provide the selection equation: a duration of longer than 1 year at the first observed date of the spell implies an absence of data. The probability of this can be determined as a function of the duration models.
- This implies that the stock sampling correction serves two roles: to eliminate bias related to the fact that longer spells are disproportionately observed (stock sampling bias) and that the set of those for whom we can observe the covariates may not be exogenous (observability/selection bias).

4.2 The Approach to Estimating the Effect of Unemployment Insurance on the Speed of Return to Employment

The objective of this paper is to evaluate the impact of UI benefits on the speed at which unemployed workers return to employment.[5] There is an extensive literature in labor economics on the disincentive effects of unemployment insurance on job search[6] and in most cases one expects to find that higher benefits lead to slower exit from unemployment, while there tends to be a spike in many cases in the rate of exit from unemployment just as benefits expire.

The same economic reasoning applies to the Brazilian case, but with some caveats.

1. The presence of FGTS, which is typically a much higher amount than UI, might lead one to suspect that most of the disincentive effects of the social safety net are associated with FGTS receipt and that the marginal contribution of UI to job search disincentives may be small.

2. The existence of a large informal sector means that many unemployed workers do not even have access to UI benefits. If workers are sorted by unobserved characteristics such that more productive workers are disproportionately found in the formal sector, then ex-formal sector workers will receive more job offers than ex-informal sector workers. Since these are also the only ones who receive UI benefits, it is unclear whether the net effect on unemployment duration of observing someone with eligibility for UI benefits will be positive or negative. In other words, the simple fact of having been a formal sector worker in the past may serve as a signal about unobserved worker quality for employers and thus these workers might have faster exit rates from unemployment even if they

[5]The PME data do not provide enough information to calculate benefit amounts, so our analysis is limited purely to an analysis of benefit eligibility.

[6]Among the literally hundreds of papers on the subject, Meyer (1990) is probably the reference for most people.

reduce their search intensity simply due to the fact that they receive more, and better, job offers.[7]

3. The sensitivity of job offer arrival rates to job search intensity may be different when looking for formal versus informal sector jobs. For example, it may be the case that getting a formal sector job offer requires following specified application procedures, while informal sector job offers require soliciting one's network of contacts, friends and family, either to find initial capital for starting a new firm or to find a place in an existing one.

4. It may be the case that individuals engage in fraud with respect to the unemployment authorities when they find informal sector jobs, not declaring that they found the informal sector job so as to continue to draw benefits. Such behavior is less straightforward for the individual when a formal sector job is found, since the person knows that the formal sector employer must declare him or her, and thus any attempt to continue to claim unemployment benefits could easily be controlled by the UI authorities. The impact this might have in our data depends on the reliability of the declarations of employment status for people who might otherwise be attempting to defraud the UI authorities.

Each of these considerations has implications for how one should adapt the econometric model

1. One should estimate separate effects for individuals who have no access to the unemployment safety net (ex-informal sector workers), those who can access their FGTS accounts but have no UI eligibility and those who have both FGTS and UI eligibility (broken down by the number of months of UI eligibility).

2. One should allow for separate destinations (formal and informal sector jobs); this is done with a competing risks proportional hazards model. One should also control for unobserved heterogeneity and allow it to influence the speed of return to employment differently for formal and informal sector jobs.[8]

[7] In a classical job search model, an increase in the offer arrival rate will lead to an increase in the reservation wage, which decreases the exit rate from unemployment. However, if the wage offer distribution is log-concave, the increase in the rate of exit due to a higher offer arrival rate more than offsets the decrease due to a higher reservation wage (Kiefer, 1988).

[8] Note that, in models with discrete unobserved heterogeneity distributions, the distribution of unobserved heterogeneity in the population is almost always considered to be independent of the covariates in the proportionality factor, i.e. although we can have different heterogeneity distributions for exits to formal and informal sector jobs, it is assumed that the individuals who came from the formal sector have the same probability of being a high or low type as individuals who came from the informal sector, and the value of each type is assumed to be the same as well. Although there exist nonparametric selection models with endo-

3. One should allow the effects of FGTS and UI receipt to differ by the sort of job found (formal or informal).
4. One should allow the unobserved determinants that affect the speed of transition to each type of job to be correlated.

We focus on the hazard rate of returning to employment for unemployed individuals,[9] and thus a higher hazard rate means a faster exit from unemployment and a shorter expected unemployment spell. In order to evaluate the effect of UI receipt on unemployment duration, we will thus focus on the differences in hazard rates out of unemployment between workers with different rights to the unemployment safety net and see how these differences vary when looking at transitions to jobs in the formal and informal sector. As obtaining unbiased estimates of these exit rates requires estimating a rather complicated model (see appendix A for details), and since these models generate a lot of results that are difficult to interpret, we present our key empirical results in graphical form. We consider several different specifications of our model in order to examine how sensitive our results are to heterogeneity in the structure of the unemployment safety net and to provide results that are more easily comparable to those found elsewhere in the literature. More precisely, we estimate the following specifications:

- A common baseline hazard that is shifted depending on whether the worker was previously employed in the formal sector or not;
- a common baseline hazard that is shifted differently for people with only FGTS eligibility, 3 months of UI eligibility, 4 months of UI eligibility and 5 months of UI eligibility;
- separate baseline hazards for workers previously employed in the formal and informal sectors, with the ex-formal sector baseline hazard being shifted by the type and length of benefit eligibility;
- separate baseline hazards for each type and length of benefit eligibility.

geneity in the literature (Das et al., 2003), the duration literature with unobserved heterogeneity has only resolved the endogeneity issue in the parametric case. The identification demonstrations of Elbers and Ridder (1982) and Heckman and Singer (1984) imply that one could not separately identify semi-parametrically the coefficient on the formal sector identifier and the distribution of the unobserved heterogeneity term conditional on coming from the formal sector. Some progress has been made toward using instrumentation as a solution (Bijwaard, 2007) but the literature has yet to arrive at a consensus on how to handle the issue with nonparametric unobserved heterogeneity.

[9]Recall that the hazard rate is rate at which a person finds a job in a small interval of time, given that the person was still unemployed at that point, i.e. $h(t) = f(t)/S(t)$, where $f(t)$ is the density of unemployment durations and $S(t)$ is the survivor function (1 minus the cumulative distribution function).

This last specification is the most flexible, as it allows spikes in the exit rate to potentially appear at different durations according to the length of benefit eligibility.[10] Appendix B discusses the main differences between the model and data used in this paper and the closest paper in the literature, Cunningham (2000).

5 Empirical Results

The main results of our analyses can be found in tables 3 and 4. Although there are 4 specifications for 2 destinations in each model, it is relatively straightforward to summarize the implications of our estimation results for the speed of exit from unemployment by reconstructing the implied hazard rates and drawing figures.

Table 3: Model Results: Baseline Hazard Components

	Model Specification							
	1		**2**		**3**		**4**	
	Formal	Informal	Formal	Informal	Formal	Informal	Formal	Informal
Constant (Low Mass Point)	2.944**	4.952***	3.008***	4.971***	5.600***	5.080***	5.609***	5.083***
	(1.357)	0.731	(0.963)	(0.704)	(0.986)	(0.761)	(0.983)	(0.697)
Constant (High Mass Point)	8.011***	6.859***	6.501***	7.312***	8.858***	7.322***	8.879***	7.336***
	(1.357)	0.815	(1.111)	(0.787)	(1.182)	(1.071)	(1.118)	(0.866)
Ex-Formal Sector Worker	1.098***	-0.460***	1.143***	-0.425***	1.119***	-0.765***	1.137***	-0.611***
	(0.118)	0.083	(0.089)	(0.082)	(0.095)	(0.090)	(0.103)	(0.098)
Eligible for 3 Months UI			0.239***	-0.015	0.005	-0.022	0.036	-0.247**
			(0.061)	(0.067)	(0.063)	(0.064)	(0.102)	(0.119)
Eligible for 4 Months UI			0.324***	0.043	0.108*	0.020	0.132	-0.215*
			(0.061)	(0.067)	(0.063)	(0.065)	(0.104)	(0.123)
Eligible for 5 Months UI			0.325***	0.005	0.117**	-0.011	0.048	-0.233**
			(0.055)	(0.059)	(0.057)	(0.057)	(0.090)	(0.102)
2 Months	0.266***	0.237***	0.206***	0.251***	0.416***	0.263***	0.419***	0.266***
	(0.078)	(0.041)	(0.055)	(0.039)	(0.091)	(0.055)	(0.090)	(0.046)
3 Months	-0.041	-0.153***	-0.068	-0.113***	0.061	-0.096**	0.065	-0.093**
	(0.083)	(0.046)	(0.057)	(0.042)	(0.099)	(0.048)	(0.098)	(0.047)
4 Months	-0.241***	-0.285***	-0.301***	-0.246***	-0.116	-0.252***	-0.111	-0.249***
	(0.088)	(0.048)	(0.061)	(0.044)	(0.109)	(0.052)	(0.108)	(0.052)
5 Months	-0.310***	-0.411***	-0.343***	-0.374***	-0.366***	-0.490***	-0.361***	-0.486***
	(0.096)	(0.055)	(0.066)	(0.051)	(0.133)	(0.065)	(0.132)	(0.064)
6 Months	-0.157	-0.309***	-0.206***	-0.259***	-0.205	-0.283***	-0.202	-0.279***
	(0.097)	(0.057)	(0.067)	(0.053)	(0.138)	(0.067)	(0.137)	(0.066)
7-12 Months	-0.960***	-1.095***	-0.994***	-1.060***	-1.012***	-1.202***	-1.009***	-1.201***
	(0.072)	(0.039)	(0.048)	(0.035)	(0.085)	(0.042)	(0.084)	(0.041)
13-24 Months	-2.292***	-2.428***	-2.569***	-2.455***	-2.402***	-2.674***	-2.404***	-2.676***
	(0.091)	(0.050)	(0.068)	(0.048)	(0.116)	(0.064)	(0.115)	(0.060)
More than 24 Months	-2.507***	-2.311***	-3.136***	-2.352***	-2.538**	-2.286***	-2.537**	-2.285***
	(0.637)	(0.361)	(0.640)	(0.351)	(1.013)	(0.430)	(1.011)	(0.428)
Formal Sector, 2 Months					-0.449***	0.325***	-0.383**	0.197
					(0.114)	(0.080)	(0.154)	(0.132)
Formal Sector, 3 Months					-0.326***	0.416***	-0.325**	0.151
					(0.120)	(0.086)	(0.164)	(0.144)
Formal Sector, 4 Months					-0.455***	0.342***	-0.442**	0.149
					(0.134)	(0.092)	(0.185)	(0.155)
Formal Sector, 5 Months					-0.162	0.564***	-0.166	0.398**
					(0.156)	(0.102)	(0.214)	(0.173)

Continued on next page...

[10] Since we assume a piecewise constant baseline hazard, we must decide on intervals over which the hazard rate is assumed constant. In order to accommodate the institutional specificities of the Brazilian system, we have chosen the following decomposition for an unemployment spell of length t: $0 < t < 3$, $3 \leq t < 4$, $4 \leq t < 5$, $5 \leq t < 6$, $6 \leq t < 12$, $12 \leq t < 24$, $t \geq 24$.

... table 3 continued

	Model Specification							
	1		**2**		**3**		**4**	
	Formal	Informal	Formal	Informal	Formal	Informal	Formal	Informal
Formal Sector, 6 Months					-0.200	0.371***	-0.404*	0.137
					(0.162)	(0.106)	(0.231)	(0.184)
Formal Sector, 7-12 Months					-0.242**	0.676***	-0.273*	0.242*
					(0.104)	(0.072)	(0.147)	(0.131)
Formal Sector, 13-24 Months					-0.740***	0.638***	-0.938***	0.818***
					(0.153)	(0.099)	(0.250)	(0.153)
Formal Sector, More than 24 Months					-1.418	-0.037	-14.880	-13.090
					(1.433)	(0.716)	(1827.000)	(722.800)
Eligible for 3 Months UI, 2 Months							-0.215	0.099
							(0.200)	(0.204)
Eligible for 3 Months UI, 3 Months							-0.119	0.283
							(0.205)	(0.213)
Eligible for 3 Months UI, 4 Months							0.090	0.225
							(0.219)	(0.224)
Eligible for 3 Months UI, 5 Months							-0.083	0.204
							(0.248)	(0.240)
Eligible for 3 Months UI, 6 Months							0.215	0.469*
							(0.260)	(0.250)
Eligible for 3 Months UI, 7-12 Months							-0.151	0.580***
							(0.180)	(0.181)
Eligible for 3 Months UI, 13-24 Months							0.196	-0.210
							(0.307)	(0.231)
Eligible for 3 Months UI, More than 24 Months							0.005	0.259
							(2641.000)	(1027.000)
Eligible for 4 Months UI, 2 Months							-0.282	0.224
							(0.203)	(0.203)
Eligible for 4 Months UI, 3 Months							0.007	0.392*
							(0.201)	(0.211)
Eligible for 4 Months UI, 4 Months							-0.135	0.367*
							(0.229)	(0.221)
Eligible for 4 Months UI, 5 Months							-0.090	0.212
							(0.244)	(0.243)
Eligible for 4 Months UI, 6 Months							0.146	0.171
							(0.255)	(0.261)
Eligible for 4 Months UI, 7-12 Months							-0.014	0.557***
							(0.175)	(0.183)
Eligible for 4 Months UI, 13-24 Months							0.241	-0.255
							(0.300)	(0.233)
Eligible for 4 Months UI, More than 24 Months							-0.104	0.288
							(2584.000)	(1005.000)
Eligible for 5 Months UI, 2 Months							0.025	0.177
							(0.165)	(0.172)
Eligible for 5 Months UI, 3 Months							0.019	0.366**
							(0.172)	(0.179)
Eligible for 5 Months UI, 4 Months							-0.052	0.212
							(0.193)	(0.191)
Eligible for 5 Months UI, 5 Months							0.059	0.239
							(0.210)	(0.207)
Eligible for 5 Months UI, 6 Months							0.299	0.295
							(0.222)	(0.219)
Eligible for 5 Months UI, 7-12 Months							0.124	0.531***
							(0.150)	(0.157)
Eligible for 5 Months UI, 13-24 Months							0.280	-0.221
							(0.269)	(0.195)
Eligible for 5 Months UI, More than 24 Months							14.220	13.910
							(1827.000)	(722.800)

Sources: Author's calculations from PME data.

Note: All of our models control for the following observable characteristics: Metropolitan region (6 regions), sex, age, relation to household head (5 categories), race (5 categories), previous schooling (5 groups), currently in school, illiterate, previously undertook professional training, currently undertaking professional training, position in previous job (3 categories), ex-civil servant, contract type in previous job (2 categories), local unemployment rate, local labor force participation rate, local average real wage. Model types: 1=common baseline hazard shifted by formal sector status; 2=common baseline hazard shifted by FGTS and UI eligibility; 3=separate baseline hazards, ex-formal sector baseline hazard shifted by the type and length of eligibility; 4=separate baseline hazards for each type and length of benefit eligibility.

Significance levels: $*$: 10% $**$: 5% $***$: 1%

Table 4: Model Results: Proportionality Factor and Unobserved Heterogeneity Components

	Model Specification							
	1		**2**		**3**		**4**	
	Formal	Informal	Formal	Informal	Formal	Informal	Formal	Informal
Recife	-1.996***	-1.388***	-2.017***	-1.362***	-2.198***	-1.345***	-2.197***	-1.343***
	(0.212)	(0.114)	(0.148)	(0.109)	(0.155)	(0.118)	(0.151)	(0.108)
Salvador	-0.986***	-0.875***	-0.903***	-0.848***	-1.199***	-0.845***	-1.199***	-0.845***
	(0.151)	(0.083)	(0.106)	(0.079)	(0.111)	(0.086)	(0.109)	(0.078)
Belo Horizonte	-0.071	0.016	-0.077	0.033	-0.284***	0.034	-0.284***	0.036
	(0.081)	(0.048)	(0.056)	(0.046)	(0.054)	(0.045)	(0.054)	(0.045)
Rio de Janeiro	-0.425***	-0.178***	-0.364***	-0.171***	-0.664***	-0.189***	-0.662***	-0.187***
	(0.096)	(0.055)	(0.067)	(0.053)	(0.067)	(0.052)	(0.066)	(0.051)
São Paulo	0.534***	0.564***	0.501***	0.576***	0.244**	0.585***	0.246**	0.587***
	(0.154)	(0.083)	(0.108)	(0.080)	(0.109)	(0.081)	(0.109)	(0.078)
Male	0.402***	0.119***	0.419***	0.136***	0.170***	0.135***	0.170***	0.136***
	(0.052)	(0.029)	(0.037)	(0.028)	(0.036)	(0.027)	(0.036)	(0.027)
Age	0.002	-0.008***	0.002	-0.008***	-0.005**	-0.008***	-0.005**	-0.008***
	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
Spouse	0.113	-0.204***	0.199***	-0.177***	-0.041	-0.179***	-0.041***	-0.179***
	(0.078)	(0.039)	(0.053)	(0.038)	(0.053)	(0.037)	-(0.005)	-(0.008)
Child	0.089	-0.218***	0.101**	-0.198***	-0.143***	-0.196***	-0.143***	-0.195***
	(0.063)	(0.035)	(0.045)	(0.034)	(0.045)	(0.032)	(0.044)	(0.032)
Relative	-0.003	-0.139**	0.023	-0.119**	-0.200***	-0.111**	-0.198***	-0.112**
	(0.104)	(0.055)	(0.075)	(0.053)	(0.076)	(0.051)	(0.076)	(0.051)
Non Family Member	0.227	-0.798***	0.460***	-0.691***	0.182	-0.659***	0.179	-0.658***
	(0.258)	(0.208)	(0.168)	(0.194)	(0.177)	(0.186)	(0.176)	(0.185)
Black	0.148*	-0.002	0.174***	0.010	-0.069	0.011	-0.070	0.011
	(0.078)	(0.042)	(0.055)	(0.041)	(0.057)	(0.039)	(0.057)	(0.039)
Asian	0.009	0.022	-0.201	0.024	-0.437	0.037	-0.437	0.037
	(0.413)	(0.250)	(0.346)	(0.240)	(0.401)	(0.232)	(0.401)	(0.231)
Parda	0.126**	-0.036	0.149***	-0.018	-0.082**	-0.016	-0.081**	-0.015
	(0.054)	(0.030)	(0.038)	(0.029)	(0.039)	(0.028)	(0.039)	(0.028)
Indigena	0.016	0.387	0.048	0.579**	-0.043	0.587**	-0.033	0.583**
	(0.858)	(0.294)	(0.570)	(0.271)	(0.516)	(0.261)	(0.516)	(0.259)
Illiterate	-0.068	0.044	-0.163	0.067	-0.385*	0.073	-0.388*	0.072
	(0.309)	(0.095)	(0.226)	(0.093)	(0.218)	(0.091)	(0.217)	(0.090)
Currently In School	0.064	-0.124***	0.099**	-0.104***	-0.142***	-0.098***	-0.141***	-0.097***
	(0.062)	(0.035)	(0.045)	(0.034)	(0.048)	(0.033)	(0.048)	(0.033)
1-3 Years of Schooling	-0.025	0.061	0.012	0.069	-0.219	0.071	-0.219	0.071
	(0.266)	(0.089)	(0.189)	(0.086)	(0.179)	(0.084)	(0.177)	(0.083)
4-7 Years of Schooling	0.036	-0.115	0.059	-0.100	-0.198	-0.091	-0.199	-0.091
	(0.228)	(0.086)	(0.174)	(0.083)	(0.161)	(0.081)	(0.161)	(0.081)
8-10 Years of Schooling	0.240	-0.260***	0.283	-0.242***	0.020	-0.228***	0.017	-0.229***
	(0.225)	(0.087)	(0.173)	(0.085)	(0.160)	(0.083)	(0.160)	(0.082)
11 or More Years of Schooling	0.379*	-0.513***	0.425**	-0.487***	0.168	-0.471***	0.166	-0.471***
	(0.225)	(0.089)	(0.173)	(0.086)	(0.159)	(0.085)	(0.159)	(0.083)
Previously Undertook Professional Training	0.359***	0.033	0.402***	0.054*	0.155***	0.057*	0.156***	0.059**
	(0.049)	(0.032)	(0.035)	(0.031)	(0.037)	(0.030)	(0.037)	(0.030)
Currently Undertaking Professional Training	0.368**	-0.039	0.399***	-0.014	0.183*	-0.003	0.187*	-0.003
	(0.143)	(0.102)	(0.103)	(0.097)	(0.110)	(0.093)	(0.110)	(0.093)
Salaried Employee in Previous Job	0.677***	-0.188***	0.722***	-0.166***	0.476***	-0.168***	0.476***	-0.167***
	(0.160)	(0.042)	(0.098)	(0.040)	(0.080)	(0.039)	(0.080)	(0.039)
Entrepreneur/Self-Employed in Previous Job	0.317	-0.171***	0.142	-0.168***	-0.129	-0.181***	-0.129	-0.179***
	(0.196)	(0.052)	(0.137)	(0.051)	(0.116)	(0.050)	(0.116)	(0.049)
Civil Servant in Previous Job	0.080	-0.155	0.158	-0.117	-0.093	-0.139	-0.092	-0.121
	(0.167)	(0.155)	(0.131)	(0.146)	(0.133)	(0.140)	(0.132)	(0.140)
Previous Employment Contract Indefinite Term	0.209*	-0.006	0.218***	0.024	-0.005	0.022	-0.005	0.028
	(0.107)	(0.085)	(0.080)	(0.083)	(0.078)	(0.081)	(0.078)	(0.080)
Local Unemployment Rate	0.020	0.041***	0.022*	0.043***	0.002	0.040***	0.002	0.040***
	(0.017)	(0.009)	(0.013)	(0.009)	(0.013)	(0.009)	(0.013)	(0.009)
Local Average Real Wage	-0.003***	-0.003***	-0.003***	-0.002***	-0.003***	-0.003***	-0.003***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Local Labor Force Participation Rate	-0.094***	-0.076***	-0.093***	-0.075***	-0.099***	-0.075***	-0.099***	-0.075***
	(0.021)	(0.011)	(0.015)	(0.011)	(0.015)	(0.011)	(0.015)	(0.011)
$P\left(\delta^F=0,\delta^I=0\right)$	0.865		0.887		0.883		0.888	
$P\left(\delta^F=0,\delta^I=\overline{\delta^I}\right)$	0.102		0.082		0.094		0.089	
$P\left(\delta^F=\overline{\delta^F},\delta^I=0\right)$	0.002		0.018		0.005		0.005	
$P\left(\delta^F=\overline{\delta^F},\delta^I=\overline{\delta^I}\right)$	0.032		0.014		0.018		0.018	
$\mathrm{cov}\left(\delta^F,\delta^I\right)$	0.263		0.087		0.115		0.116	
Log Likelihood	-55396.014		-55006.001		-54547.109		-54509.511	

Continued on next page...

... table 4 continued

	Model Specification							
	1		2		3		4	
	Formal	Informal	Formal	Informal	Formal	Informal	Formal	Informal
Observations	137902		137902		137902		137902	

Sources: Author's calculations from PME data.

Note: All of our models control for the following observable characteristics: Metropolitan region (6 regions), sex, age, relation to household head (5 categories), race (5 categories), previous schooling (5 groups), currently in school, illiterate, previously undertook professional training, currently undertaking professional training, position in previous job (3 categories), ex-civil servant, contract type in previous job (2 categories), local unemployment rate, local labor force participation rate, local average real wage. Model types: 1=common baseline hazard shifted by formal sector status; 2=common baseline hazard shifted by FGTS and UI eligibility; 3=separate baseline hazards, ex-formal sector baseline hazard shifted by the type and length of eligibility; 4=separate baseline hazards for each type and length of benefit eligibility.

Significance levels: $*$: 10% $**$: 5% $***$: 1%

5.1 Overall Comparison of Exit Rates

Insert Figure 1 Around Here

Figure 1 makes clear that workers who came from the formal sector exit from unemployment to informal sector jobs slower than those who had previously worked in that sector. If there is indeed sorting along the lines mentioned in section 4.2, then ex-formal sector workers should benefit from positive statistical discrimination in the eyes of employers and be more likely to receive a marginal job offer than ex-informal sector workers. This means that their exit rate from unemployment should be faster. Clearly, this is not the case and the disincentive effects of benefit receipt (the main direct difference between the two sorts of workers) on job search intensity seem far more important, as reflected by the significant negative coefficient of -0.46 in the first specification in table 3.

Transitions to the formal sector seem to follow a different logic, however. First of all, they are slower than transitions to the informal sector, which is understandable given the additional hiring procedures and other formalities that my be associated with these jobs. However, it appears that workers previously employed in the informal sector are at a disadvantage when attempting to enter the formal sector, even conditional on their unobserved heterogeneity. Since there is no reason why the disincentive effects for job search should be less relevant here, such a result suggests a demand-side phenomenon such as a statistical discrimination against with ex-informal sector workers by formal sector employers.

5.2 The Role of UI as Distinct from FGTS

In the previous section we saw transition behavior to the informal sector that was consistent with disincentive effects of the unemployment safety net on job search intensity, although these effects seemed outweighed by demand-side phenomena when considering transitions to the formal sector. In this section we decompose the population of ex-formal sector workers into those with only rights to FGTS, those with rights to 3 months of UI, those with rights of 4 months of UI and those with rights to 5 months of UI. As the different sub-populations receive different lengths of benefits, we might expect to see differential exit rates across these subgroups as well; those with 5 months of UI having the least incentive to search hard for a job and those with only FGTS having a greater incentive to engage in more intensive (and costly) job search.

Insert Figures 2 and 3 Around Here

Figures 2 and 3 show the same results for ex-formal sector workers relative to ex informal sector workers as were visible in figure 1, but they also distinguish between benefit profiles. There does not appear to be any significant difference in the hazard rates by length of benefits, a result confirmed by model 4 in table 3. Although in a simpler specification there appear to be some effects (specification 2) for the transition to formal sector work, these effects disappear once the time profile of the baseline hazard is allowed to vary by benefit length. The shifting coefficients are significantly negative, however, in specification 4 for transitions to informal sector work, suggesting that the extra benefits may further reduce job search incentives. It should be noted that they are not monotonically increasing (in absolute value) with the length of the spell, as one might expect, so this result needs to be interpreted with caution.

It is important to note, however, that the time profile of the baseline hazard is almost never significantly affected by UI benefit eligibility; it is shifted rather than deformed. This is particularly true for transitions to formal sector jobs, where none of the deformation coefficients is significant. For transitions to the informal sector it appears that the main deformation of the hazard curves with respect to the general shape of FGTS-only workers is to make the drop-off after 6 months smaller than that of FGTS-only workers. Again, this may be positive statistical discrimination at work as the workers with UI eligibility were previously employed in the formal sector for longer than those with only FGTS eligibility, and thus have likely acquired more human capital and may benefit more from the positive signal, which would make them more likely to well considered by the labor market.

5.3 Other Covariates and Heterogeneity

The other covariates in table 4 seem to affect unemployment duration as one might expect. Men return to work faster than women, as do heads of households. More highly educated workers seem to be penalized when looking for informal sector jobs, as are those still in school. Previous professional training is also an advantage, while previously salaried employees transit faster to formal sector jobs but slower to informal sector ones.

The effects of the macroeconomic variables are also interesting. A high local unemployment rate seems to push people toward informal sector jobs, whereas higher average real wages in the metropolitan area tend to slow hiring by firms and thereby reduce the speed of return to employment. Lastly, metropolitan areas with high local labor force participation rates tend to be crowded, and the effect is visible on exit rates as people appear to find new jobs slower when the labor force participation rate increases.

Concerning unobserved heterogeneity, the data indicate a mild positive correlation, suggesting that there are some workers ("high" types) who can get a new job quickly in either sector, while others have more problems regardless of where they go. This latter group makes up by far the majority of worker types, with most specifications indicating less than 2% of the unemployed population being the sort of person who can find either sort of job rapidly and over 85% of the population having difficulty in finding both sorts of job.

6 Caveats and Policy Implications

Before discussing the implications of the results found in this paper for policy, it is worth mentioning a few caveats. This discussion is intended to put the later discussion of policy implications into perspective.

6.1 Caveats

The first, and perhaps most important, is that the data do not allow us to identify FGTS or UI receipt, only eligibility. Thus it is entirely possible that individuals who are classified as eligible for 5 months of UI, for example, do not draw any UI in reality and all of the results are driven by formal-sector specific human capital, for example. This issue is due to a fundamental non-identifiability in the unemployment safety net; the same situations that make an individual eligible for FGTS or UI can also generate other effects.

One can imagine two ways to solve this problem. First, one can rely on a natural experiment, as Cunningham (2000) does, in which case a policy change gives people with "identical" labor market histories different sorts of benefits. Unfortunately, no such change occurred in the system during the time period covered by our data.[11]

[11] See appendix B for a discussion of this and other differences between Cunningham (2000) and this paper.

The second alternative is to adopt a "regression discontinuity" type approach, in which one exploits discontinuities in the UI system. For example, if seniority in the previous job could be measured to the day, one can assume that human capital increases continuously with job seniority and thus compare people with 5 months and 29 days of seniority in a formal sector job to those with 6 months of job seniority. The two groups would have essentially the same amount of formal sector human capital and experience, but the latter group also has 3 months of UI and thus any difference in exit rates can be attributed to the perception of UI. Again, our data constrain us in that job seniority is only measured with monthly precision, and given that many studies have found rapid accumulation of human capital at the start of jobs,[12] it might not be justifiable to maintain that all of the difference in exit rates between 5 and 6 months of job seniority is due to UI receipt.

A second caveat relates to the tre atment of unobserved heterogeneity. As was discussed at length in footnote 8, we treat the distribution of unobserved heterogeneity (times and levels) as common to workers coming from the formal and informal sector. This is for nonparametric identification reasons, but it implies that the workers coming from the formal sector can not experience disproportionately fast (or slow) transitions to formal (or informal) sector jobs on the basis of unobserved characteristics. It does not, however, imply an absence of control for unobserved heterogeneity of workers. Although we cannot say whether ex-formal sector workers are more likely to be slower exiters to informal sector jobs along unobserved dimensions than ex-informal sector workers, we can say that coming from the formal sector, and in particular being eligible for UI, slows the exit rate even after conditioning on the marginal distribution of unobserved characteristics that determine the probabilities of having faster or slower transitions to formal and informal sector jobs.

A final caveat comes from another data-related constraint. Our data provide us with information on the job held immediately prior to the unemployment spell in progress, and this poses two problems. First, as Cunningham (2000) notes, individuals have to file for UI within 120 days of losing their formal sector job, so it is possible to have a short, informal sector job during the 120 day period (that the individual does not report when filing for benefits) while having had a long formal sector job immediately beforehand. In this case we would misclassify the individual as ineligible for FGTS or UI, since the unemployment spell was not "immediately" preceded by a spell of formal sector employment. Since Cunningham considers the possibility that such a situation actually occurs in reality as being very small, we will make a similar assumption here.

[12] See Margolis (1996) for an example of a detailed analysis of returns to job seniority.

Nevertheless, this does not exclude a further complication, namely that we underestimate formal sector job seniority if the individual held a formal sector job for less than 24 months immediately before the unemployment spell, but this job was preceded by another formal sector job. Such a problem could only arise in our data for individuals who were previously employed in the formal sector but with less than 24 months of jobs seniority. Of the 30950 unemployment spells that are preceded by formal sector employment in our data, 11765 were preceded by jobs of at least 24 months. The remainder of the spells could potentially suffer from undermeasurement of this type, although the importance of such undermeasurement is difficult to quantify. Such a phenomenon may be the reason why one does not observe monotonically increasing (in absolute value) coefficients on the impact of the length of UI benefits on transitions to informal sector employment in column 4 of table 3.

6.2 Policy Implications

With these caveats in minds, the results from section 5 can serve as a basis for interpreting the impact of UI on the speed of returning to employment. The first key result is that the unemployment safety net slows the rate of transition to informal sector jobs. In the most flexible specification, having been a formal sector worker (and thus being eligible for at least FGTS) results in a significantly slower exit rate to informal sector jobs, and eligibility for UI further slows exit to the informal sector. At the same time, having been a formal sector worker speeds exit to the formal sector, although UI benefits do not further accelerate this transition. In the context of heterogeneity and job search, one can interpret these results as saying that there is both an indirect effect of selection and a direct effect of benefit receipt. The fact that ex-formal sector workers are more likely to find formal sector jobs at any point during their spell than ex-informal sector workers can be seen as indicating that formal sector employers have a preference for workers with previous formal sector experience, and the negative effect on transitions to the informal sector can be seen as other side of the same phenomenon.

Still, eligibility for UI benefits slows transitions to informal sector jobs. It may thus be the case that UI benefits allow unemployed workers to engage in more selective job search, preferring to wait for a good formal sector job offer than taking the first informal sector offer that comes available regardless of the quality of the match. Since ex-informal sector workers do not have the same resources available during their unemployment spell, they may be forced to take the first job offer that comes along, be it from the formal or informal sector, for subsistence reasons. In this sense, although UI increases the length of unemployment spells it may be efficiency-enhancing as it allows workers the freedom to better sort themselves and to refuse job offers that they consider to be of insufficient quality.

It is worth noting, however, that the coefficients on the length of UI benefits do not increase monotonically, as one might expect, nor does one see spikes in the hazard rates at benefit expiration dates in figures 2 and 3.[13] Part of this may be due to measurement error in the length of benefit eligibility (see section 6.1), although there are 2 other possible interpretations. The first is that the extra money provided by UI allows you to improve your job search and slows exit to the informal sector, but if an individual explicitly wishes to transit to the informal sector (for example, with the goal of starting his or her own firm), then such a transition most often occurs in the first 3 months of unemployment, thereby rendering the additional months irrelevant. A second explanation is that once an individual has held a formal sector job for at least 6 months (and the additional job seniority is irrelevant), informal sector jobs come to be seen as less attractive in general and will be refused with a higher probability. Empirically, it is impossible to distinguish between these three explanations, but the key result that UI benefits lead individuals to disproportionately refuse informal sector jobs is an important one for policy analysis.

7 Conclusions

In this paper we have investigated the role of the unemployment safety net on the speed at which people leave unemployment. We have found that there is indeed an effect, but it is negative on transitions to the informal sector and positive on transitions to the formal sector. We have argued that the negative effect on transitions to informal sector jobs reflects the role of benefit receipt on job search intensity, while the positive effect on transitions to the formal sector reflect more employers discriminating against workers who came from the informal sector. The effect on search intensity appears to be slightly reinforced by the addition of UI, although the additional effect is not monotone in the length of benefits.

[13]The spike in the hazard rates at 6 months is also present for ex-informal sector workers, and is thus likely to be do to some other institutional phenomenon independent of the unemployment safety net.

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A Specification of the Econometric Model

In this section we provide the econometric details of the models that were estimated in this paper. The models are based on standard duration econometrics, adapted to the particular institutional and data setting found in the PME data for Brazil.

A.1 Notation and the Base Competing Risk Model

In order to construct the likelihood function, one must define some notation. Let t^F be the (latent) time to reemployment in the formal sector and t^I be the (latent) time to reemployment in the informal sector, with the duration of the unemployment spell being $t = \min\left\{t^F, t^I\right\}$. Each unemployment spell is observed after a duration t^S has already passed, although $t^S = 0$ for flow-sampled spells. Each latent duration is modeled using the proportional hazard framework with a piecewise constant baseline hazard, time-varying explanatory variables X[14] and unobserved heterogeneity, i.e. the hazard rates to formal sector jobs $\left(\lambda^F\right)$ and informal sector jobs $\left(\lambda^F\right)$ can be written as

$$\begin{aligned}
\lambda^F\left(t\left|X_t, \theta^F\right.\right) &= \sum_{j=1}^{J} \alpha_j^F 1_{\{c_{j-1}<t\leq c_j\}} \exp\left(X_j\beta^F + \delta^F\right) \\
&= \sum_{j=1}^{J} \alpha_j^F 1_{\{c_{j-1}<t\leq c_j\}} k_j^F \\
\lambda^I\left(t\left|X_t, \theta^I\right.\right) &= \sum_{j=1}^{J} \alpha_j^I 1_{\{c_{j-1}<t\leq c_j\}} \exp\left(X_j\beta^I + \delta^I\right) \\
&= \sum_{j=1}^{J} \alpha_j^I 1_{\{c_{j-1}<t\leq c_j\}} k_j^I
\end{aligned} \tag{1}$$

where β^F, β^I are vectors of parameters in the proportionality constant, δ^F, δ^I are the unobserved heterogeneity components, $c_0, \ldots, c_J$ are the end points of the various intervals between which the hazard function is assumed constant (with $c_0 = 0$ and $c_J = \infty$) and $\left\{\alpha_1^F, \ldots, \alpha_J^F\right\}, \left\{\alpha_1^I, \ldots, \alpha_J^I\right\}$ the values of the baseline hazard function in each interval. The covariates in X include, in addition to the various sociodemographic and economic environment variables (see section 5), indicator variables for having worked previously in the formal sector (and thus FGTS eligibility), 3 months of UI eligibility, 4 months of UI eligibility and 5 months of UI eligibility. These are the

[14]Note that we can decompose the fixed periods of the baseline hazard into subperiods during which the time varying X variables remain constant. Thus during any interval $\left(c_{j-1}, c_j\right]$, all of the variables in the model remain constant. This approach to discretizing time can not be applied directly to variables that vary continuously with time (such as age), but we assume that the impact of such variables on outcomes is constant between integer values of the variable. For example, we suppose that the effect of age on returning to employment for somebody who is 30.5 years old is the same as that of somebody who is 30.7 years old and somebody who is 30 years old.

covariates that are only observable for individuals with unemployment spells of less than 12 months at the first observation date.

Using the standard result that the survivor function is the exponential of the negative integrated hazard, we can write

$$\begin{aligned} S^F\left(t\left|X_t,\theta^F\right.\right) &= \exp\left(-\sum_{j=1}^{J} k_j^F\alpha_j^F\left(\min\left[t,c_j\right]-c_{j-1}\right)1_{\{t>c_{j-1}\}}\right) \\ S^I\left(t\left|X_t,\theta^I\right.\right) &= \exp\left(-\sum_{j=1}^{J} k_j^I\alpha_j^I\left(\min\left[t,c_j\right]-c_{j-1}\right)1_{\{t>c_{j-1}\}}\right). \end{aligned} \tag{2}$$

With these expressions, we can write the density as the product of the hazard and the survivor functions, i.e.

$$f^F\left(t\left|X_t,\theta^F\right.\right)=\lambda^F\left(t\left|X_t,\theta^F\right.\right)S^F\left(t\left|X_t,\theta^F\right.\right)$$

and

$$f^I\left(t\left|X_t,\theta^I\right.\right)=\lambda^I\left(t\left|X_t,\theta^I\right.\right)S^I\left(t\left|X_t,\theta^I\right.\right).$$

A.2 The Selection Bias - Stock Sampling Correction

As mentioned above, the correction for the selection bias involves exploiting the competing risk model to derive the probability of a stock sampled spell having been in progress for at least a year at the date of the first stock sampling. This involves deriving the survivor function for unemployment spell (both latent durations combined) so that it can be evaluated at 12 months. As noted by Lancaster (1990, p. 100), the hazard function for the unemployment spell will be the sum of the separate transition intensities. Denoting the relevant functions by the index Z, we can thus write

$$\lambda^Z\left(t\left|X,\theta^Z\right.\right) = \sum_{j=1}^{J}\alpha_j^Z 1_{\{c_{j-1}<t\leq c_j\}}k_j^Z \tag{3}$$

$$S^Z\left(t\left|X,\theta^Z\right.\right) = \exp\left(-\left(\sum_{j=1}^{J}\alpha_j^Z\left(\min\left[t,c_j\right]-c_{j-1}\right)1_{\{t>c_{j-1}\}}k_j^Z\right)\right) \tag{4}$$

with $\alpha^Z=\alpha^F+\alpha^I$ and $k_j^Z=k_j^F+k_j^I$.

When constructing the likelihood function we need to be able to express the probability of what we observe, in order to control for selection bias and stock sampling. Given the data constraints, this corresponds to being able to write

$$P\left(T=t_i\left|X_t,t_i>t_i^S,t_i^S\leq 12\right.\right)$$

for those individuals for whom we observe an exit after a duration of t_i and

$$P\left(T > t_i \middle| X_t, t_i > t_i^S, t_i^S \leq 12\right)$$

for those individuals who are right-censored at t_i. Note, however, that t_i^S is observed in the data and is not a random variable, and thus the constraint $t_i > t_i^S$ is not affected by the extra conditioning $t_i^S \leq 12$ since this additional conditioning always holds in the observed data.[15]

The standard correction for stock sampling, namely dividing the likelihood function by the value of the (overall) survivor function evaluated at the stock sampling date, is very similar to the inverse-probability weighting principle described in Wooldridge (2004) that is used elsewhere to establish a representative sample and control for the selection bias. In particular, this principle requires that there be selection on observables, i.e. that conditional on the set of observables used to estimate the sampling probability, the risk of being sampled is independent of the outcome and other observable variables. In our case this property is satisfied trivially in that the sampling model *is* the outcome model, and thus any characteristics that could possibly increase the probability of having an in-progress spell of at least t^S will be in the duration model (as they are related to the duration) and thus in the set of variables used in selection.

A.3 Unobserved Heterogeneity

Unobserved heterogeneity in this model is treated using a correlated discrete heterogeneity approach with two mass points in each direction. For identification, the "low" value of each mass point is fixed at zero, and the probability distribution across mass points for each destination does not assume independence. The following table describes the joint distribution of the unobserved heterogeneity terms δ^F, δ^I with $p_4 =$

Table 5: Joint Distribution of Unobserved Heterogeneity Terms

Formal \ Informal	0	$\overline{\delta^I}$
0	p_1	p_2
$\overline{\delta^F}$	p_3	p_4

$1 - p_1 - p_2 - p_3$, $\overline{\delta^F}$ the value that δ^F takes on when it is of the "high" type and $\overline{\delta^I}$ the

[15] This additional conditioning may, however, be informative about the value of the unobservable heterogeneity in that people with higher values of δ are less likely to have long spells and are therefore more likely to have their covariates observed. In principle, our estimation technique that controls for unobserved heterogeneity corrects for this but the observability constraint may affect the estimates of the joint distribution of unobservable heterogeneity terms.

value that δ^I takes on when it is of the "high" type. Note that the probability of being a "high" type for transitions to the formal sector is $1 - p_1 - p_2$ and the probability of being a high type for transitions to the informal sector is $1 - p_1 - p_3$. Furthermore, the covariance of the two types can be written as cov $\left(\delta^F, \delta^I\right) = (p_1 p_4 - p_2 p_3) \overline{\delta^F \delta^I}$.

B Principal Differences Between This Paper and Cunningham (2000)

As this study is not the first to address the impact of unemployment insurance on unemployment duration in Brazil, it is worth discussing the main differences between our study and that of the primary reference on the subject, Cunningham (2000). It is worth noting that Cunningham addresses the impact of UI along several dimensions (earnings as well as transitions out of unemployment) and that she distinguishes between self employment and other informal sector work, but the discussion here will be centered on differences in estimation methods and data between her paper and ours.

Cunningham (2000) employs a difference-in-differences type of methodology for estimating the effects of UI, focusing on a change in legislation that affected eligibility criteria that occured in 1994. Given the time period spanned by the data used in this paper, there is no comparable "natural experiment" around which our estimation can be based; as a result we control for unobserved heterogeneity explicitly using a two mass point unobserved heterogeneity distribution. Although this is, in principle, a less flexible means of controlling for unobserved heterogeneity (a difference-in-difference estimator is invariant to the specification of the conditional distribution of unobserved heterogeneity, provided its hypotheses are satisfied), it does allow us to add an additional element of flexibility to our estimation that is absent from both the multinomial logit and competing risk specifications in Cunningham (2000); we can allow the rates of transition to different destinations to be correlated. The multinomial logit approach Cunningham uses initially imposes an "independence of irrelevant alternatives" hypothesis, and thus unobservable characteristics that affect the probability of exit to one destination are assumed to be orthogonal to those that affect the probability of exit to any other destination. Furthermore, her competing risk approach based on a Cox-like semiparametric model does not specify the baseline hazard and thus does not allow one to be precise about how unobserved heterogeneity might shift the baseline hazards of different destinations in a correlated manner. Our results suggest that, in our data, unobserved characteristics that affect transitions to informal sector employment are indeed correlated with those that affect transitions to formal sector employment, and thus the orthogonality assumptions implicit in Cunningham's models may not hold in the data.

Cunningham (2000) estimates two sorts of transition models, a multinomial logit model and a competing risk, proportional hazard, semiparametric model, whereas this paper estimates a competing risk, proportional hazard, piecewise-constant model. Relative to the multinomial logit specification, which ignores the length of time spent in unemployment and amounts to imposing a log-weibull distribution on the duration distribution in an accelerated lifetime framework, the model adopted here is qualitatively different. The accelerated lifetime framework models the log of the duration directly, whereas the proportional hazards framework models the probability of transition conditional on having remained in a state up to the date of transition. As a result, the models are not directly comparable and one must rely on measures such as marginal effects and expected duration to generate comparisons, neither of which is available in Cunningham (2000).

Among the proportional hazards models, Cunningham's cox-type model is more flexible than the piecewise-constant specification adopted here. Nevertheless, this paper uses a piecewise constant specification with monthly pieces through the first 6 months of unemployment. As durations are measured with monthly precision, one can not identify the baseline hazard to any finer degree of precision than the that of a monthly transition rate, and thus the piecewise constant specification is not constraining over the first 6 months, when two thirds of the exits in our data occur. The remaining pieces cover more than one month each, and thus do impose some constraints on the form of the baseline hazard, but this seems a relatively small price to pay when the counterpart is the ability to allow for correlated unobserved heterogeneity (see above).

One last modeling issue distinguishes this paper from Cunningham (2000). The competing risks in Cunningham's paper are formal sector employment, self-employment and other informal sector employment, and she estimates separate models for men and women. In this paper we do not distinguish between self-employment and other informal sector employment and we control for sex with covariates rather than estimating separate models for men and women. In this dimension, Cunningham's paper imposes a less constrained model than ours.

Beyond issues directly related to econometric modeling, Cunningham (2000) and this paper differ substantially in the data being used. Cunningham uses data drawn from pooling the 1992, 1993, 1995, 1996 and 1997 National Household Surveys (PNAD), which are repeated cross-sections collected annually by the Brazilian National Statistics Institute, whereas this paper exploits data from the March 2002 to August 2007 Monthly Labor Survey (PME), a rotating 16-month panel. While the PNAD is a national survey, the PME only covers Brazil's 6 largest metropolitan areas and is thus (in principle) less representative of the population. Nevertheless, the PME's panel nature and monthly frequency allows for more precise measurement of unemployment durations, even though the PNAD provides retrospective information that allows one to define unemployment spells with monthly precision. Although the PNAD is a larger survey than each wave of the PME, one might expect our results to be less biased as a result of measurement error in durations (Magnac and Visser, 1999), especially since we limit our attention to unemployment spells that had a maximum duration of 12 months at the first observation date.

The repeated cross-section nature of the PNAD also makes difference-in-difference estimation more problematic since this approach controls for unobserved heterogeneity by differencing outcomes for individuals across time. With panel data this is generally not an issue, but when using repeated cross sections (and pseudo-panels more generally) one must make the additional assumption that the conditional (on observables) distribution of unobserved heterogeneity in the treated population is the same in the "before" period as in the "after" period for the approach to remain valid, and a comparable assumption must be made for the control population. For reasons related to the economic environment during the sample period used by Cunningham (2000) (see below), this seems unlikely. Similarly, Cunningham notes that some sample statistics change between the "before" and "after" periods for her different treatment and control groups, casting further doubt about the credibility of the hypothesis.

Two other data issues are worth noting. First, and most obviously, Cunningham (2000) uses data from the 1990s, and in particular her "before" period for her difference in differences estimates concerns a time when hyperinflation was rife in Brazil, whereas the "after" period benefited from more price stability. The PME data used here cover the mid-2000s, a period in which prices were relatively stable.[16] Insofar as the different economic environment in the before and after period affects employers' hiring behavior differently when faced with an individual previously employed in the formal versus the informal sector, a difference in difference estimator (which relies on the assumption that macro effects are the same for the treatment and control groups) may be suspect.

The last point concerns the analysis sample. Cunningham (2000) only considers "men and women of working age (14-65) who left a non-agricultural job, spent at least one month unemployed, and found a new job in the formal, informal wage, or self-employment sector in the year of the survey". This paper does not impose the

[16] The average annual inflation rate in the period studied here varied between 3.6% and 14.8%, whereas inflation in the "before" for Cunningham (2000) was between 1022% and 1927% and inflation in the "after" period was between 66% and 7% (International Monetary Fund, 2008).

same sample selection criteria. Here, we do not impose the age or sector constraints, but for data reasons we limit our attention to unemployment spells that have been in progress for at most 12 months at the first observation date (thus the longest observable completed spell is 27 months) and who are not primo-entrants into the labor force. Our stock sampling correction should eliminate the bias due to stock sampling,[17] but it is unclear how the fact that we allow potentially older and younger workers as well as workers coming from the agricultural sector should make our results differ from hers.[18]

[17] It is unclear how Cunningham (2000) handles stock sampling bias in her paper.

[18] Recall that our data are drawn from 6 urban areas, and as such transitions from agricultural employment may not be as prevalent as in the PNAD data.

Figure 1: Relative Speed of Exit from Unemployment by Worker Type and Sector of Job Found

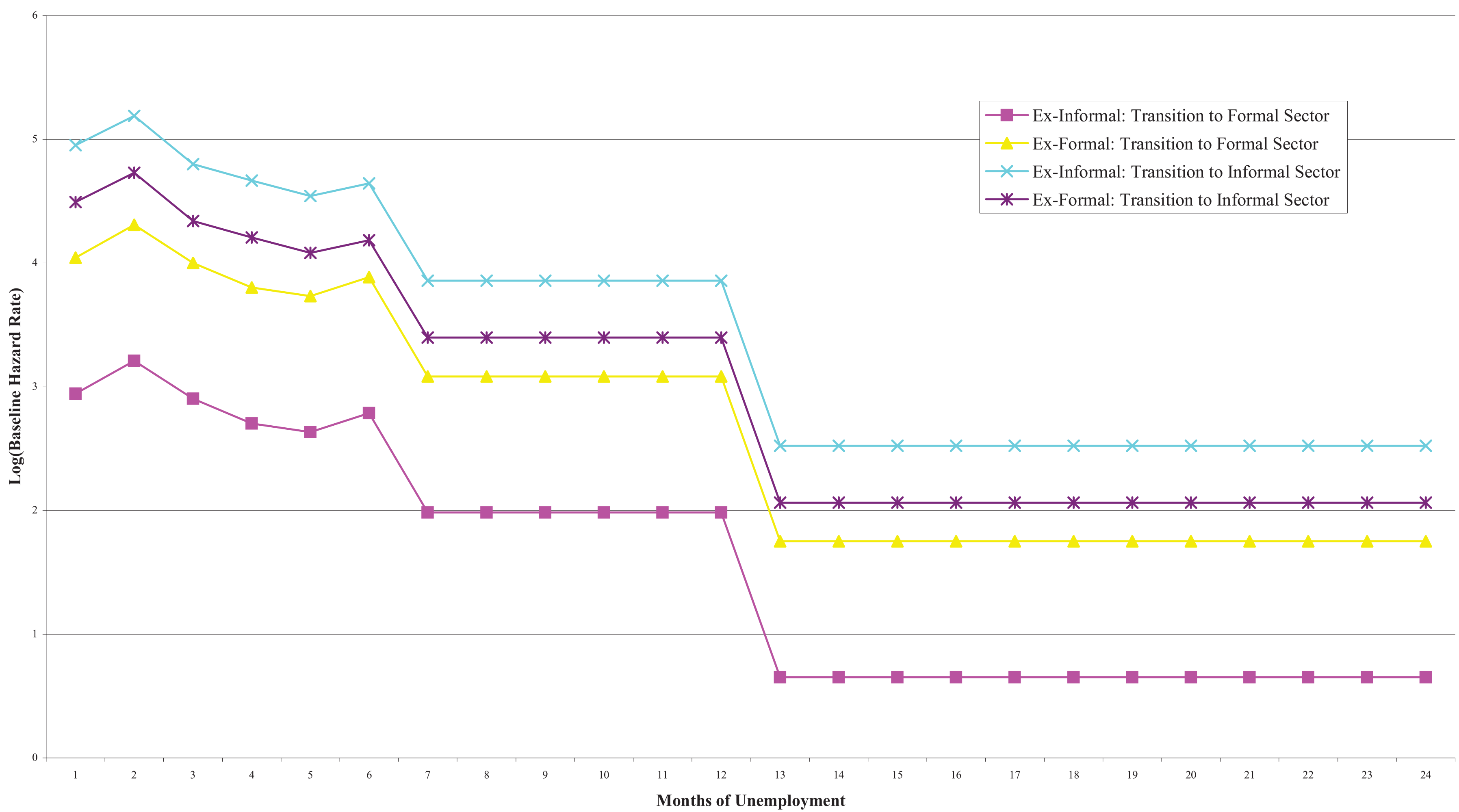

Figure 2: Hazard Rates for Transitions to Formal Sector Jobs

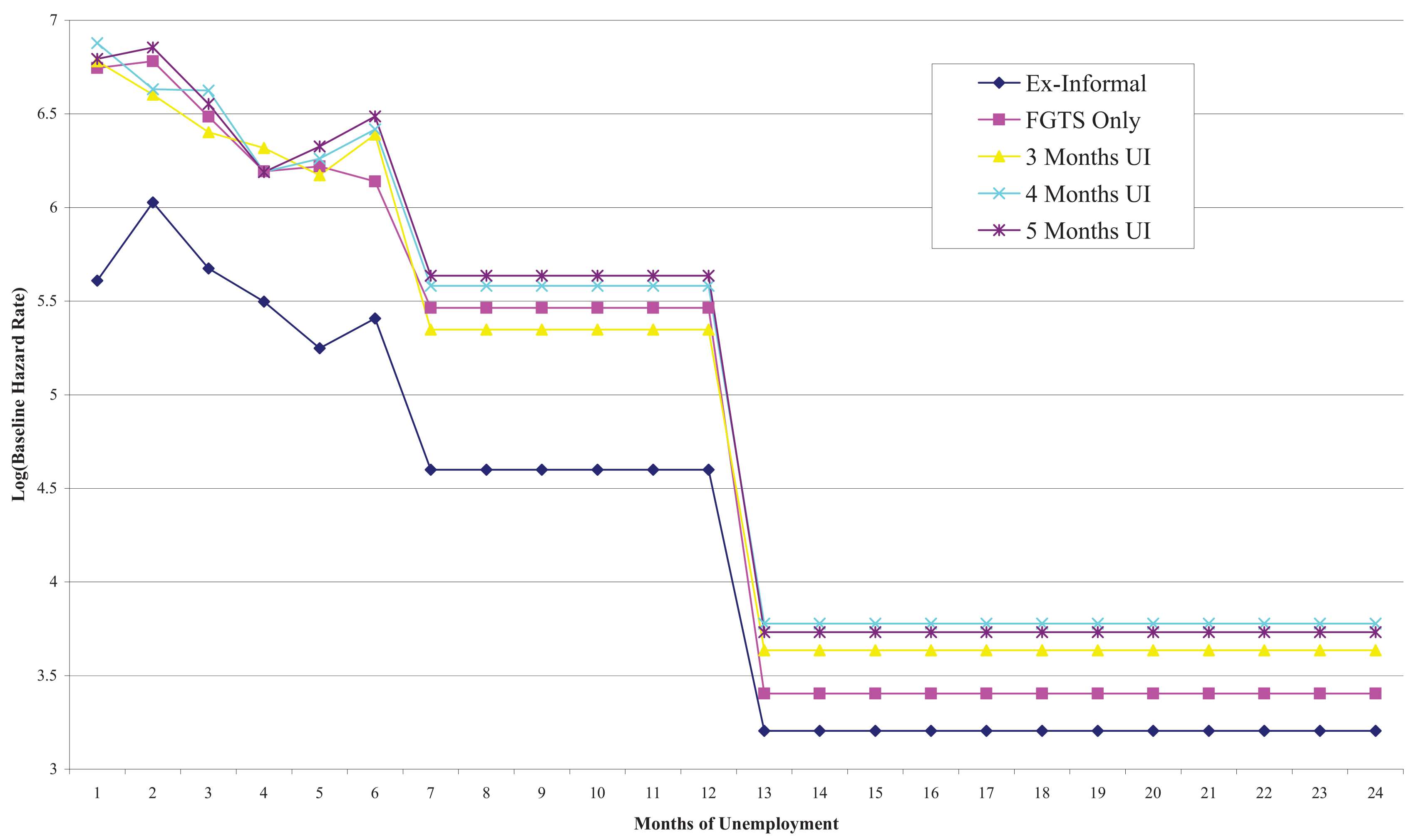

Figure 3: Hazard Rates for Transitions to Informal Sector Jobs