

Disentangling Stigma from Firm Effect on Wage Losses for Laid-off Workers

(Preliminary Version)

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Abstract

The present paper extend the analysis of [Gibbons and Katz \(1991\)](#) on stigma based on wage losses of laid-off workers. We incorporate a time-varying firm effect on workers' wage, both in the theoretical framework and in the empirical exercises. It is shown that once such effect is encompassed the identification strategy used by [Gibbons and Katz \(1991\)](#) is no longer valid, and their reported results for the stigma effect tends to be overestimated. We rely on a rich matched employer-employee database to propose an improvement in the identification strategy encompassing firm effects. Instead of comparing wage variation between any laid-off and any worker displaced due to plant closing, we restrict this comparison (across the cause of displacement) for workers displaced by the same firm at the same moment. Estimations under this strategy point in fact to significantly lower stigma than pointed by estimations based on [Gibbons and Katz \(1991\)](#) strategy.

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1 Introduction

An influential paper by [Gibbons and Katz \(1991\)](#) presented a theoretical framework of asymmetric information in the labor market with respect to workers' productivity in which laid-off workers experience a higher wage loss than workers who lose their jobs due to plant closing. The intuition is that laid-off workers signal inferior levels of intrinsic productivity. Using U.S. data, the authors confirm the existence of a stigma effect for laid-off workers in a framework where there is no firm-specific effect on wage determination.

The present paper aims to extend their analysis by incorporating a time-varying, firm-specific effect on workers' wage, both in the theoretical framework and in the empirical analysis. It is shown that once such effect is included in the model the identification strategy typically used in the literature is no longer valid, as it tends to overestimate the stigma effect. This casts some doubts on the reliability of the empirical results reported in [Gibbons and Katz \(1991\)](#), as well as those reported by other authors relying on the same identification strategy.¹

The inclusion of a time-varying, firm-specific effect in this type of analysis may be motivated in two steps. First, there are evidences in the literature of significant influence of firms' effect on workers wage ([Abowd et al. \(1999\)](#)). This may be particularly relevant in the context we are analyzing. [Guiso et al. \(2005\)](#) point that enduring disturbances to output are only partially insured by the firms with respect to workers wage, which on the other hand are fully insured against temporary idiosyncratic shocks. In accordance with this result, it is shown ([Stevens \(1997\)](#)) that workers experienced a large wage reduction prior to displacement when this is due to plant closing episodes as opposed to no significant change for laid-off workers. So, we have that wages evolve heterogeneously among displaced workers, and, if the reason for plant closing is a downward movement in its productivity², this heterogeneity reflects the influence of firms' effects on wages.

Second, this heterogeneous effect of firm productivity on wages may be confounding the estimation of the stigma effect. If firms' productivity level is significantly lower in episodes of plant closing, the pre-displacement wage of plant-closing workers will tend to be lower than the wage of laid-off workers. Hence, assuming the former group of workers is not stigmatized in the labor market, we should observe an increase in their post-displacement wage. This implies that at least part of the comparison of wage

¹For instance [Dorion \(1995\)](#) and [Grund \(1999\)](#) conduct similar empirical analysis using data from Canada and Germany, respectively.

²This is a standard result in IO models that incorporate endogenous exit decisions.

variations between plant-closing and laid-off workers will be influenced by the rise in the wage of plant-closing workers. The main implication of this is that the stigma effect may end up being overestimated due to the fall in productivity when plants are closing.

As a matter of fact the inclusion of firm size, which is positively correlated with firms' productivity level, among explanatory variables tends to attenuate or even eliminates the stigma effect (Krashinsky (2002)). However Abowd et al. (1999) point that unobserved firm characteristics influence wages in a rather distinct way as done by firm size. This point is particularly relevant in our context since it is quite likely that unobserved firm characteristics (e.g. management practices) do play a role in explaining the failure of some firms that are otherwise observationally identical to surviving firms. In our empirical analysis we will consider the influence of both observed and unobserved characteristics of the firms when estimating the stigma effect.

We rely on a rich matched employer-employee database to propose an improvement in the identification strategy of the stigma effect. Instead of only comparing the wage variation between any laid-off and any worker displaced due to plant closing, we will also restrict this comparison (across the cause of displacement) for workers displaced by the same firm. This is implemented through a regression model incorporating pre-displacement firm fixed effects and restricting the sample to workers displaced around the moment that the plant closed.

In fact, we estimate the stigma parameter under both methods, the one used by Gibbons and Katz (1991) and the one we propose. The results for the estimation of the stigma parameter are always statistically different from zero. However, comparing the results across the alternative model specifications (which in turn reflect alternative identification strategies), we have obtained a much lower (in module) stigma effect under our proposed method. This result is consistent with our claim that the stigma effect has been overestimated in the literature.

Apart from these methodological aspects that distinguish the present paper from Gibbons and Katz (1991)'s, we also depart from their procedures with respect to the source of information used in the empirical exercises. Song (2007) claims that the use of household survey based on retrospective information for wages in previous jobs, as in Gibbons and Katz (1991), may introduce recall bias in their estimation. The author tries to replicate the empirical results using a more recent U.S. data source that limits the use of retrospective information, and reports lower stigma effects. Recall bias tends to be a minor issue for us, since we rely on administrative files in which establishments provide information to

the Brazilian federal government in the first quarter of each year on all labor relationships they had in the previous year.

The remaining of the paper is structured as follows. The theoretical background is shown in section 2, where we also discuss the implications of the theory for the definition and identification of the stigma parameter. In particular we show the identification problem generated by not considering a firm effect, and consequently how the standard empirical exercise found in the literature produces biased estimates of the effect of interest. Section 3 introduces the database, explains the construction of some variables, and presents some descriptive statistics. Section 4 presents the empirical model and the estimated results based on a sequence of procedures to correct for the confounding factor highlighted in the discussion of the identification problem. Section 5 concludes.

2 Theoretical Framework and Identification Strategy

In search of new workers, prospective employers face the problem of offering contracts compatible with the workers' productivity, which is in general private information. In a world where current employers know more about individual productivity than their prospective competitors, the pool of unemployed workers are likely to have a great proportion of low productivity people, because firms usually try to retain their best staff and either fire their low productivity employees or let them quit. This is a typical context of an adverse selection situation, where in equilibrium market wages are driven down towards low productivity levels. This problem is partially mitigated by the fact that prospective employers usually observe a signal about the worker's productivity, namely his employment history containing the type of past job separations experienced by the worker. This signaling equilibrium is first explored by [Gibbons and Katz \(1991\)](#), who not only formalized a two-period theoretical model that establishes the wage separation between workers who lost their jobs due to plant closing (the non-informative event) and to dismissal (in which case the type of separation would suggest low productivity), but also proposed a way of testing this empirical content in the microdata.

As mentioned in the introduction, a number of articles have been published trying to refine the estimations of [Gibbons and Katz \(1991\)](#) model, and a subset of them ([Stevens \(1997\)](#) and [Krashinsky \(2002\)](#)) have found that ex-ante wages of workers that belonged to next-to-close firms are usually lower than those of workers laid off from firms that continued functioning after their dismissal. As will be demonstrated at the end of this section, this fact pollutes the estimation of the stigma effect in that

higher wage gains of plant-closing workers (as compared to laid off individuals) may in fact be due to lower pre-displacement wages of the former instead of lower ex-post wages of the latter. Attempts of correcting the estimates include adding firm size and other control variables at the firm level to the regressions, aiming to homogenize ex-ante wages, but are arguably imperfect if lower ex-ante wages in next-to-close firms are in fact due to lower (unobservable) firm-level productivity, partially transmitted to wages.

In the remaining of this section, we revisit and extend [Gibbons and Katz \(1991\)](#)'s model to explicitly incorporate an active role for the firm in wage formation. In particular, our model reconciles the stylized fact that pre-displacement wages in next-to-close firms are usually lower with a meaningful comparison between average wages of dismissed workers and workers who lost their jobs due to plant closing. We then show the identification problem generated by the non-inclusion of firm-specific effects. In doing so we show how the standard empirical exercise proposed by [Gibbons and Katz \(1991\)](#) can produce biased estimates of the effect embedded in the information about the cause of displacement from the previous job. Finally, we propose a sequence of procedures to correct for such confounding factor in the data, which is implemented and described in the empirical section.

2.1 Equilibrium in Gibbons and Katz

The set-up is a two period economy ($t \in \{1; 2\}$), where information on worker productivity (η_i) is revealed at the end of the first period. This value is a draw from a known cdf $F(\cdot)$ with finite support, and a log concave density $f(\cdot)$.

Some plants close at the end of period 1 before knowing the productivity of their workers. In surviving firms, the productivity of a retained worker grows by s units from the first to the second period. The (log) concavity mentioned above guarantees that there is only one η^* such that $\eta^* + s = E[\eta_i \mid \eta_i > \eta^*]$. Firms require workers to have a minimum productivity of η_R in order not to be dismissed at the end of the first period, and prospective employers observe whether the worker has been fired or not, which functions as a signal about the worker's productivity in their model. The layoff rule for the current employers is therefore:

$$\begin{aligned} T &= 1 \text{ iff } \eta_i \leq \eta_R \\ &= 0 \text{ otherwise} \end{aligned}$$

It is assumed that no long term contract is feasible between workers and firms. Therefore wages are negotiated at the beginning of each period. Between the two periods, laid off workers receive job offers consistent with their displacement status, $w_m(T = 1) = E[\eta | \eta < \eta_R]$. The market for non-laid off workers is described as a two-step game in which prospective employers play first by making their job offers and then the incumbent responds with its own offer. Market wages in this case must be consistent both with the signal ($\eta > \eta_R$) and with the fact that the incumbent will try to retain its best workers. In this case, the zero-profit wage should be compatible with individuals who, in spite of not being dismissed by the firm, were not good enough for the incumbent to cover the market wage. Available workers for job change would therefore be those with the lowest productivity levels among the non-dismissed, which would drive wages down to η_R . In order to have a non-trivial market wage, the authors assume a fraction μ of non-dismissed workers will always be willing to change jobs, which guarantees that $w_m(T = 0) > \eta_R$. In fact, incumbent firms will cover the bids from the prospective employers whenever $\eta_i + s \geq w_m(T = 0)$, which leads to (unique) non-laid off wage offer implicitly defined by the zero-expected profit expression:³

$$0 = \mu [E(\eta | \eta \geq \eta_R) - w_m(T = 0)] + \\ (1 - \mu) \Pr[\eta + s < w_m(T = 0) | \eta_R \leq \eta] [E(\eta | \eta_R \leq \eta < w_m(T = 0) - s) - w_m(T = 0)]$$

Solving the equation above for w_m , one may see that it can be represented as a weighted average of $E(\eta | \eta \geq \eta_R)$ and $E(\eta | \eta_R \leq \eta < w_m(T = 0) - s)$.⁴ Since both terms are no smaller than η_R (and possibly greater), w_m is no smaller than η_R as well.

Finally, market wages for workers coming from plants that close down are $w_m(C) = E[\eta]$, as the closing down event is not informative about individual productivities. The main empirical implication of Gibbons and Katz (1991)'s paper is that, after displacement, dismissed workers should earn less than workers displaced due to plant closing.⁵ They test this result by looking at the coefficient of a dummy variable associated to the cause of displacement in regressions using variations between pre- and post-displacement wages as the dependent variable.⁶ In the next section we present a model in

³Equation (1) of Gibbons and Katz (1991).

⁴With $\frac{\mu}{\mu + (1 - \mu) \Pr[\eta_i + s < w_m(T = 0) | \eta_R \leq \eta]}$ and $\frac{(1 - \mu) \Pr[\eta_i + s < w_m(T = 0) | \eta_R \leq \eta]}{\mu + (1 - \mu) \Pr[\eta_i + s < w_m(T = 0) | \eta_R \leq \eta]}$ as the respective weights.

⁵Gibbons and Katz (1991) assume employers do not make negative inferences about the productivity of workers displaced due to plant closing. Thus, their wage is $E[\eta]$, which is greater than $E[\eta | \eta < \eta_R]$, the wage of laid-off workers in the second period.

⁶As a matter of fact they provide results based on two types of regressions, one with post-displacement wages as the

which the naive inclusion of this dummy variable in this type of regression is likely to produce biased estimates of the stigma associated to the source of displacement. In the model, pre-displacement wages are lower in next-to-close firms, which magnifies eventual wage gains to those who lost their jobs due to plant closing.

2.2 Introducing the firm component

The key problem in testing [Gibbons and Katz \(1991\)](#)'s empirical predictions is to have groups of workers whose only difference is the source of displacement. If one compares post-displacement wages between laid-off workers and workers who lost their jobs after a plant closing, two sorts of criticisms arise. First, the cause of displacement dummy variable may capture unobserved differences between the two groups of workers, which could bias the coefficient. Second, post displacement wage regressions suffer of a potential selectivity bias coming from the fact that only reemployed workers belong to the sample.⁷ The common alternative to this approach is to compare wage gains between the two groups, instead of the post-displacement wages. The problem in this case is that [Gibbons and Katz \(1991\)](#)'s model is blind with respect to pre-displacement wages. Indeed, the authors assume for simplicity that wages depend only on the workers' expected productivity. This assumption, together with the fact that jobs must be ex-ante identical in equilibrium (otherwise some firms would not attract any worker), imply that pre-displacement wages should be identical across plants. However, though they present evidence of similar pre-displacement wages between laid-off and plant-closing workers, this result is challenged by [Song \(2007\)](#), who, as aforementioned, claims that such equality in pre-displacement wages is affected by recall bias. As it will be shown in the the next section, our data do display a difference in pre-displacement wages between the two groups.

We generalize [Gibbons and Katz \(1991\)](#)'s model to relax the hypothesis of identical pre-displacement wages for laid-off and plant closing workers. The generalized version of the framework will be used, later on, to suggest forms to correctly identify and estimate the impact of the cause of displacement on wages.

dependent variable and the other with wage variations as mentioned above. Their favorite results are those coming from wage variations as the dependent variable, perhaps because they are less likely to suffer from an omitted fixed effect problem (one could imagine, for instance, that workers who were fired in the previous year are in fact individuals who have difficulties in working for pay or in accepting orders. If this is the case the dummy variable just mentioned may be capturing not only the effect of the source of the last displacement, but also the cumulative effect of a history of other dismissals or the unobserved type of the worker).

⁷[Gibbons and Katz \(1991\)](#) and others have used heckit corrections to deal with this problem, which is still vulnerable to criticisms about the functional form and exclusion restrictions used in the correction.

We want to keep our model tractable and as similar as possible to [Gibbons and Katz \(1991\)](#). As in their model, jobs must be ex-ante identically attractive if we want all firms to have a positive number of workers in the first period, but on the other hand we want to have a firm effect that helps to explain the observed differences in wages in the first period. We tackle this problem by assuming wages have a common (across firms) part, related to the firm's expectation about the worker's productivity, and a stochastic part contingent on the global productivity of the firm:

$$w_{ijt} = E[\eta_i | I_{jt}] + \delta_{jt}$$

where I_{jt} denotes the information set of firm j at time t , and δ_{jt} is the firm's productivity (unknown at the beginning of the first period and draw from a zero-mean distribution h_δ in period 1). Workers then decide whether to accept a job offer at firm j , in period 1 iff $E(w_{ijt}) \geq \max_{k \neq j} E(w_{ikt})$. In period 1, expected wages equal to $E[\eta]$ in all firms, as no information is still available and $E[\delta] = 0$, but paid wages may differ.

To capture the fact that pre-displacement wages tend to be lower for plant closing workers, we say that a plant stops its activities at the end of period 1 iff $\delta_{jt} < \Psi$. In words, a firm closes down if its productivity falls below a certain threshold Ψ . In this case, it is clear that the average pre-displacement wage of workers displaced at the time of the interruption in firms' activities is generally lower than that of laid-off workers.

The proposed timeline for the events is such that, in period 1,

- (i) workers are hired with a promise of earning $w_{ij1} = E[\eta_i] + \delta_{j,1}$;
- (ii) $\delta_{j,1}$ is revealed;
- (iii) first period wages are paid;
- (iv) the survival rule mentioned above is applied and some plants eventually close down;
- (v) η_i is revealed in surviving firms;
- (vi) surviving firms decide whether to retain or to lay-off workers;
- (vii) competing firms bid for the available work force.

In this time line, event (vii) deserves some comment. As in [Gibbons and Katz \(1991\)](#), we assume prospective employers can bid for the non-laid off workers (as well as for the unemployed), and after all offers are made the incumbent employer decides whether or not to cover the highest bid received by the workers. Because a worker revealed productivity (η) is equally valuable to all firms but only one of them

knows his true productivity, there is clearly an adverse selection problem in the market for the non-laid off workers. From the firm's decision not to layoff some workers, everybody knows the productivity for those individuals is at least as high as η_R , but the prospective employers also know that if the worker's productivity is high, the incumbent employer will cover its bid. Solving this game backwards, one could see that wage offers from the prospective employers would be driven down towards η_R , and the incumbent would retain all of its non-laid off workers by covering the market offer. [Gibbons and Katz \(1991\)](#) circumvent this problem by assuming that a proportion $\mu > 0$ of the non-laid off workers is always willing to switch jobs, so that the prospective employers can offer more than η_R and still have non-negative profits.

In our case, we claim to have market wages greater than η_R even if $\mu = 0$ (i.e. even if all agents display expected income maximizing behavior). This result comes from the fact that we allow the firm productivity, δ , to vary over time, for instance by following an AR(1) process of the type:

$$\begin{aligned}\delta_{jt+1} &= \rho\delta_{jt} + v_{jt} \\ v_{jt} &\text{ i.i.d. } (j, t); E(v) = 0\end{aligned}$$

We then have two types of non-laid off workers willing to move to a new job: those coming from firms with a bad shock in time t (since part of the worker's wage is contingent on the firm's performance), and those forced to move through μ . To make things clear, any equilibrium must satisfy the participation constraints of the workers, and competition among employers should be such that the market wage satisfies a zero (expected) profit condition. Consider a second period wage for non-laid off workers that consists on a fixed part, $w_{j'}$, and a contingent promise of $\delta_{j't+1}$, coming from firm j' . Then the participation constraint of a risk neutral worker is:

$$w_{j'} + E(\delta_{j't+1}|I_{it}) \geq \max_{k \neq j'} w_k + E(\delta_{kt+1}|I_{it})$$

If $k \neq j$ (j is the incumbent employer), then $E(\delta_{kt+1}|I_{it}) = E(\delta_{kt+1}) = 0$, whereas $E(\delta_{jt+1}|I_{it}) = \rho\delta_{jt}$. Let $j' = m$ be the maximum bid, w_m , received by the worker. The worker is willing to move from firm j to firm m iff:

$$w_m \geq w_j + \rho\delta_{jt}$$

which means the minimum promise for the wage's fixed part the incumbent should offer in order to retain the worker is $w_m - \rho\delta_{jt}$. On the other hand, the firm will only cover the bid if the worker brings it non-negative profits, i.e.:

$$\eta_i + s - w_j \geq 0$$

Putting these two facts together, we conclude that firms will retain workers iff

$$\eta_i + s + \rho\delta_{jt} - w_m \geq 0$$

Therefore, there is room for good workers to be willing to move at market wages, especially if they come from surviving firms with a low value of δ_{jt} . In this case, it is hard for the firm to cover the market bid because the worker requires a premium over w_m in order to compensate for the low expectation regarding δ_{jt+1} . This fact is particularly interesting as we do not need a story about μ to have non-trivial market wages for non-laid off workers as in [Gibbons and Katz \(1991\)](#). The key assumption to reach this result is the one on firm-level productivity shock displaying some persistence ($\rho \neq 0$).

The market wage is then obtained from the zero profit equation of the competitive prospective employers:

$$0 = \mu [E(\eta|\eta \geq \eta_R) - w_m] + (1 - \mu) E_j \{ \Pr[\eta + s + \rho\delta_{jt} < w_m | \eta_R \leq \eta] [E(\eta|\eta_R \leq \eta < w_m - s - \rho\delta_{jt}) - w_m] \}$$

From the equation above it is easy to confirm our claim that we have market wages greater than η_R even if $\mu = 0$. In this case:

$$w_m = \int_{\text{supp}(\delta_t)} \int_{\eta_R}^{w_m - s - \rho\delta_t} \eta dF_\eta dF_\delta$$

Based on the equilibrium found in the model, we suggest in the next section ways to correctly identify the stigma effect provided information about the pre-displacement plant is available.

2.3 Stigma Definition and Identification

So far we have been using the term stigma in a lousy way. The literature has not provided any formal definition for stigma but it is often colloquially referred as the (re-employment) wage difference between those workers displaced due to plant closing and those laid-off. We may define stigma as:⁸

$$stigma = E[\eta_i | \eta_i \leq \eta_R] - E[\eta_i]$$

When the firm component is not considered, one may identify the stigma parameter using the following strategy, as (implicitly) done by [Gibbons and Katz \(1991\)](#):

$$E[\Delta w | T = 1] - E[\Delta w | T = 0] = stigma$$

However once the firm component is taken into account, this strategy is no longer appropriate, as shown below:⁹

$$E[\Delta w | T = 1] - E[\Delta w | T = 0] = \{E[\eta_i | \eta_i \leq \eta_R] - E[\eta_i]\} - \{E[\delta_{j,1} | T = 1] - E[\delta_{j,1} | T = 0]\}. \quad (1)$$

It is possible to show that the second component in the second line of the expression above is always positive, which invalidates the identification strategy of [Gibbons and Katz \(1991\)](#).¹⁰

Therefore we need an alternative identification strategy. We claim that the wage variation comparison among the two groups of workers becomes a suitable strategy once i) it contrasts employees that used to work at the same establishment, and ii) the laid-off group is restricted to include only those who lost their job near the closing date of the establishment.

To be precise, we propose to identify the stigma parameter through the following difference, where t' represents the moment when the plant closed:

⁸Note that in a treatment effect set-up this definition would correspond to the selection bias of the program.

⁹To see this first note that the wage variation can be stated as:

$$\Delta w_{i,j} = T \cdot E[\eta_i | \eta_i \leq \eta_R] + (1 - T) \cdot E[\eta_i] + \delta_{j',2} - E[\eta_i] - \delta_{j,1}.$$

The result follows easily once we apply the expectations conditioned with respect to the value of T , and note that

$$E[\delta_{j',2} | T = 1] = E[\delta_{j',2} | T = 0].$$

¹⁰To see this note that:

$$E[\eta_{j,1} | T = 0] = E[\eta_{j,1} | \eta_{j,1} < \Psi] < E[\eta_{j,1} | T = 1] = E[\eta_{j,1}], \quad (2)$$

where the last equality comes from the fact that the firm productivity component in the first period is independent from the workers component.

$$E[\Delta w | T = 1, j, t' - \varepsilon] - E[\Delta w | T = 0, j, t']$$

In this case, we will be comparing, for the same plant, workers who lost their jobs due to plant closing with laid-off workers who lost their jobs almost at the same time the plant closed down (hence with the same $\delta_{j,1}$). The intuition here is that workers who lost their jobs almost at the same time, in the same establishment, must have pre-displacement wages influenced by the same value of the firm component.

Writing the term above in a way analogous to equation (1) elucidates our claim, as shown below.

$$E[\Delta w | T = 1, j, t' - \varepsilon] - E[\Delta w | T = 0, j, t'] = \{E[\eta_i | \eta_i \leq \eta_R] - E[\eta_i]\} - \{E[\delta_{j,t'-\varepsilon} | T = 1, j, t' - \varepsilon] - E[\delta_{j,t'} | T = 0, j, t']\} \quad (3)$$

The last term in curly brackets should be null in order to obtain the stigma parameter in the right hand side. We claim that this term vanishes as ε goes to zero. That is, we rely on the following identification assumption.

- $\delta_{j,t}$ is locally continuous in t at the closing date.

Another identification assumption implicitly used in this strategy is that the wage offer is a step function in time, valuing $E[\eta_i | \eta_i \leq \eta_R]$ anywhere to the left of t' , and $E[\eta_i]$ at t' .

2.4 Introducing an alternative time line

The time line of our theoretical framework does not accommodate laid-off episodes in plants that eventually close down. However the identification strategy proposed above relies on laid-off workers from plants that close right after displacing them.

In order to reconcile the theoretical framework with our identification strategy we propose now an alternative time line. The proposed time line for the events is such that, in period 1,

- (i) workers are hired with a promise of earning $w_{ij1} = E[\eta_i] + \delta_{j,1}$;
- (ii) (η, δ) are revealed to the incumbent employer and to the worker (but not to the prospective employers);
- (iii) layoff decisions are made and wages are paid;
- (iv) prospective employers make their wage offers to both laid off and non-laid off workers, based on the existing information about the available workers;
- (v) ε is revealed; and

(vi) plants eventually close down.

As it can be seen, layoffs occur before the firm closes. The introduction of a second firm level component, ε , was the key for this. Now, we say that a plant stops its activities at the end of period 1 iff $\delta_{jt} + \varepsilon_{jt} < \Psi$. In words, ε_{jt} is an idiosyncratic shock (e.g. demand shock) that affects profitability of firm j at time t . We say that the firm interrupts its production if the combination of both δ_{jt} and ε_{jt} falls below a threshold Ψ . In this case, it is clear that we maintain our result that firms with low δ_{jt} are more likely to close down, at the same time that average wages in these firms are generally lower.

One may argue that if the plant had chosen some workers to be laid-off, than those workers remaining employed until the plant interruption would carry some information to prospective employers, namely that their individual productivity component is not low enough. This has consequences for our definition of stigma.

In this set-up we may define stigma as:

$$stigma = E[\eta_i | \eta_i \leq \eta_R] - E[\eta_i | \eta_i > \eta_R]$$

The difference between this definition and the previous one relies on second term on the right-hand side: while in the previous case it corresponds to the unconditional mean of η_i , now it corresponds to the mean of the distribution of η_i truncated to the left at the threshold value that defines the lay-off rule.

In terms of the empirical content of the model, it is no longer true that post-displacement wages of workers who lost their jobs due to plant closing should be $E[\eta]$. Now the sequence of events is such that the layoff decision happens before the shock that makes some plants to close down. Therefore, the comparison between post-displacement wages of dismissed workers and individuals who lost their jobs due to plant closing should be seen as the difference between workers who received a bad and a good signal about their productivity (transmitted through the layoff decision of the incumbent employer).

Although the definition of our parameter of interest has changed it is easy to see that our proposed identification strategy remains valid as well as the caveats of the standard identification strategy.

Finally, it should be noted that the use of data from laid-off workers from plants that eventually close down is widespread in the literature. So this tension between the theoretical time line, where no displacement occurs in plants that closes down, and identification / estimation procedures using real world data, with displacement occurring in these plants, is not inherent to our framework.

3 Data

Our data come from a Brazilian administrative file (Relação Anual de Informações Sociais - RAIS) maintained by the Brazilian Ministry of Employment and Labor (Ministério do Trabalho e Emprego - MTE). In Brazil, all registered, tax-paying establishments must send to the Ministry information on all employees who worked anytime during the reference year.¹¹

The information contained in RAIS provides a matched employer-employee longitudinal database, similar to those available in developed countries.¹² The data available in RAIS include information specific to workers (such as gender, age and schooling), to establishments (such as location, and industry), and to the labor contracts (such as wage, working hours, dates of hiring and separation, and reason for separation).

3.1 Sample and construction of variables

The first step in constructing our sample was to identify all separations that took place between 2000 and 2005.¹³ We then check whether the worker obtained a job in the same year of the separation event or in the following year.¹⁴ This gives a reasonably large time span for reemployment of up to 23 months in the formal sector¹⁵. For workers reemployed within this time period, we discard observations from workers who were reemployed at the same month of the displacement. This is to avoid quit episodes in our sample. For workers reemployed within the time period between 1 and 23 months, we discard observations from workers who were reemployed at the same establishment where the original job was held. For the remaining episodes we further restrict the sample by considering only men with age between 25 and 60 years old, where the following employment conditions were met:

- full time contracts (at least 30 hours per week),
- non-agricultural firms in the private sector, and

¹¹The absence of tax evaders prevents us from claiming that the data refer to the universe of Brazilian establishments. In fact, RAIS gathers information on the universe of what is typically called the ‘formal sector’.

¹²See [Abowd and Kramarz \(1999\)](#) for a description of the countries where this type of database was available and how research on labor economics has benefited from such databases.

¹³Due to the possibility of multiple jobs held by the same worker, we keep only the information of the main job and discard all observations related to other ongoing jobs.

¹⁴At this point we remove job separations due to retirement even if the worker decides to return to the labor force. We also remove job-loss events due to a worker’s death.

¹⁵This is the time span for separations which occurred in January of year t followed by a re-employment in December of year $t + 1$.

- permanent contract (i.e. with no expiration date).

Most of the conditions imposed can be justified as attempts to remove from the sample employment relations that have either seasonal characteristics or held by someone not fully engaged in the labor market. Lastly, for reasons to be explained in the next subsection, we removed from the sample all separations from establishments employing less than 55 workers. All these conditions left us with a sample of 5,2 million episodes of job losses.

For the sample defined above, we collected some already available variables from RAIS, and built some others. Among those already available, we collected gender, schooling, age, industry, geographic location (at the State level), date (month and year) of reemployment, tenure, and wage. We then built the number of employees, the date of the separation and an indicator of whether it was due to plant closing or not. The definition of some variables worths some comments. Concerning the wage, we do not have neither the precise information on the last wage before the job loss, nor the first wage after reemployment. The actual information on wages is the average monthly wage of the worker at the establishment where the job loss occurred for the last year available^{footnote}The displacement year for those laid-off and the previous one for those displaced due to plant closing., and the average monthly wage of the worker at the establishment and the year of reemployment. Both averages take into account the actual time interval of workers within the year at the establishments of displacement and reemployment.

The indicator of plant closing was built keeping track of each establishment's identification number over time. We considered that a plant closed at year $t + 1$ when the identification number of a establishment that appeared until the last day of year t and ceases to appear in any of the following years up to 2007.¹⁶ Note that we do not have the precise date that a plant closed down. In order to impute this piece of information, we have developed the procedure described in the next subsection. Before describing this procedure, it is important to keep in mind that we do have the month and year of the job-loss events when the plants do not close in that year.

3.1.1 Imputation of plant closing dates

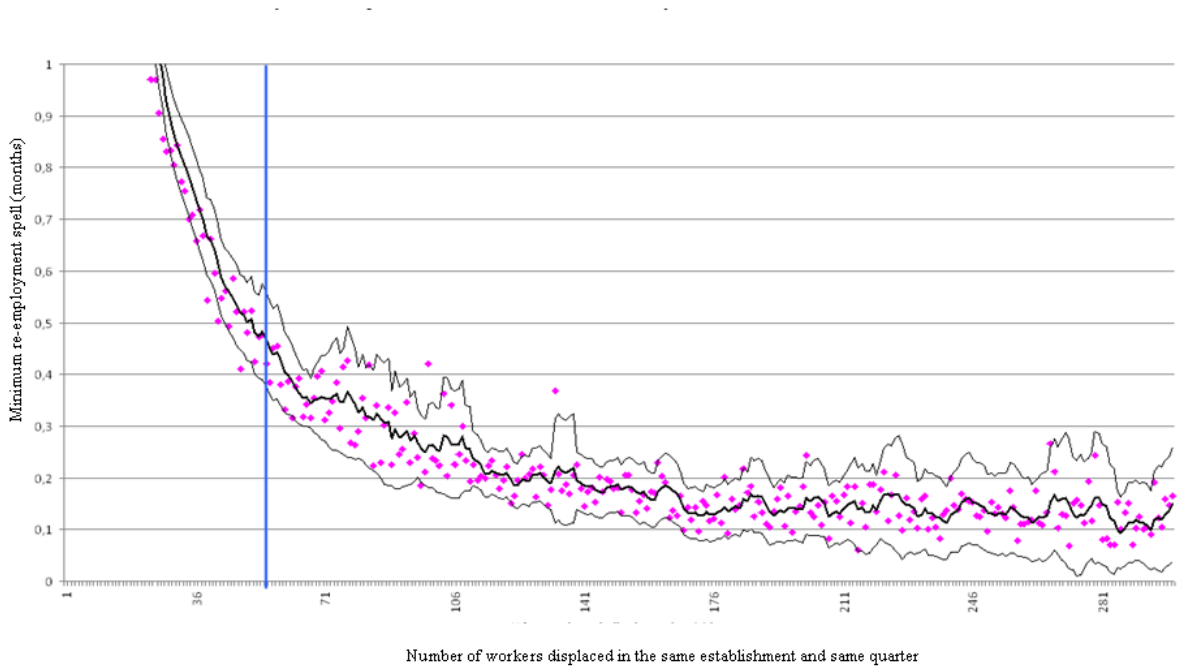
The first step was to count the total number of separations in each quarter among surviving establishments. Within each group of workers who lost their jobs in the same quarter and at the same

¹⁶There were cases where the identification number was not found in one year but reappeared in the next one. We have interpreted this as a random data processing problem, and removed the workers from these establishments from the sample.

establishment we compute the time spell between the job loss and reemployment, and take the minimum value (i.e. the time spell for the worker who was first reemployed).

Our prior was that this minimum value should decrease as the number of workers in the group increases. Figure 1, which presents the average values and confidence intervals for the minimum spell by the size of the group, shows that the minimum value tends to zero as the number of workers in the group increases. Therefore, we claim that, at least in episodes of large lay-offs, the hiring month for the worker who was first reemployed can be used as the plant closing month for all workers of the same group.

Figure 1: Approximation Error of the Plant Closing Data by Plant Size



As it can be seen in Figure 1, the measurement error for episodes of lay-offs of 100 workers is 6 days (0.2 months). The last step was to choose a lower bound for the number of workers, below which we consider that the measurement error becomes too large. We opted for a minimum of 55 employees losing jobs simultaneously for this lower bound. For this number, the measurement error is below 0.5 months with 95% confidence level (see the vertical bar in the Figure). Therefore we consider only plant-closing episodes where the establishment had at least 55 employees. In order to have a fair comparison we also excluded from our sample separations from surviving establishments employing less than 55 workers.

3.2 Descriptive Statistics

In this section we will show that the “raw data” support our claim of a relevant role of firm’s productivity in lowering wages of workers to be displaced due to plant closing.

Table 1 brings some descriptive statistics about workers’ characteristics and some aspects of the terminated employment relationship. The main result is that there is a remarkable distinction between the groups with respect to pre-displacement wage, as shown in the first line. In fact, monthly wages were 22% lower for the group of worker who lost the job due to plant closing ($R\$976,17$) than for the laid-off workers ($R\$1189,09$).

This result contrasts with the insignificant pre-displacement wage gap reported by [Gibbons and Katz \(1991\)](#). On the other hand our result supports the claim made by [Song \(2007\)](#), who suggests that pre-displacement wages comparison in [Gibbons and Katz \(1991\)](#) are likely to be driven by recall bias.

The pre-displacement wage difference reported in table 1 contrasts with the distribution of tenures, which favors the plant closing group. As it can be seen, between the second and fourth lines of the same table, while 63% of laid-off workers had less then 1 year tenure, the corresponding figure for workers who lost jobs due to a plant closing was 40%. In the remaining lines of the table we can see that no other individual characteristic may be pointed as responsible for the higher pre-displacement wage of the laid-off group, as these characteristics are distributed in a similar fashion in both groups. As a matter of fact, there is a slightly higher concentration of younger and less skilled (lower educated) workers among those who lost jobs in surviving establishments. Summing up these results, one may say that with respect to individual observable characteristics pre-displacement wages should be higher, as opposed to lower, for workers displaced due to plant closing.

Table 2 presents some reemployment outcomes for workers according to the cause of separation.

Table 1: Individual and Employment Relation Characteristics by Cause of Displacement

	Cause of displacement	
	Lay-off	Plant closing
Pre-displacement wage (R\$)	1189.09	976.17
Tenure (years)		
Less than 1	0.63	0.40
1 to 3	0.22	0.32
More than 3	0.15	0.28
Schooling (years)		
0 to 7	0.48	0.42
8 to 10	0.26	0.33
Over 10	0.26	0.26
Age (years)		
25 a 39	0.74	0.71
40 a 59	0.26	0.29

Source: RAIS (Brazilian administrative file)

The first line shows that workers who lost their jobs due to a plant closing attains positive (real) wage gains when reemployed, while laid-off workers experiences wage losses. Once again this is compatible with our claim of pre-displacement wages being forced down in closing plants due to downward move in their productivity. If this is the case, the workers who left these plants are likely to experience an upgrade in firm productivity component once re-employed.

The remaining of the table shows that while the re-employment wage is in fact higher for this group, it is not obvious whether it performs better than the other in a broader context since the time spell for reemployment is 50% higher for those who lost their jobs due to a plant closing than for those who were laid-off.

Table 2: Frequency and Reemployment Outcomes by Cause of Displacement

	Cause of displacement	
	Lay-off	Plant closing
Wage gain (%)	-29.8	1.7
Reemployment wage (R\$)	916.24	993.27
Average reemployment spell (months)	6.1	9.8
Number of observations	5,081,128	126,393

Source: RAIS (Brazilian administrative file)

The pattern described above suggests that firm characteristics may be indeed the determinant of the pre-displacement wage difference in favor of the laid-off workers. Table 3 reveals an important

distinction with respect to the industry of establishments before workers' displacements. More than two thirds (68%) of laid-off workers were displaced from the manufacturing sector, whereas less than half (47%) of workers from the other group lost their jobs in this sector. Also, the service industry was the origin of relatively more job losses when displacement is due to plant closing (49%) than when is due to lay-off (28%). Table 3 also shows that no other establishment characteristic should have influenced the pre-displacement wage gap mentioned above, as the two groups were similarly distributed according to the size and regional location of pre-displacement establishments.

Table 3: Establishments' Characteristics by Cause of Displacement

	Cause of displacement	
	Lay-off	Plant closing
Industry:		
Manufacturing	0.68	0.47
Construction	0.02	0.02
Trade	0.02	0.02
Service	0.28	0.49
Establishment size (# of employees):		
From 55 to 249	0.52	0.56
From 250 to 999	0.33	0.33
More than 1000	0.16	0.11
Geographical region:		
North	0.04	0.07
Northeast	0.15	0.13
Southeast	0.56	0.58
South	0.18	0.12
Midwest	0.07	0.09

Source: RAIS (Brazilian administrative file)

4 Stigma Estimation

In this section we develop our empirical procedure to estimate the stigma parameter.

4.1 Basic Specification of the Empirical Model

Let π_{ijt} be the worker i 's productivity at firm j , in period t , which is determined by his observed individual traits, X_{it} , and by the matching quality between the worker and his job, η_{ijt} . In particular, let us assume that:

$$\pi_{ijt} = \eta_{ijt} \exp(X'_{it}\beta),$$

where β is a vector of coefficients.

According to our theoretical framework, firms do not know the matching quality at the moment they hire workers, and therefore offer a wage reflecting the expected quality given the available information. Apart from X , we will introduce two other elements not discussed in the theoretical section. First, we assume that the wage offers are also influenced by another individual (non-productive) characteristic, which will be denoted as $\varepsilon_{i,t}$. This aims to incorporate wage heterogeneity present in the real world among workers with similar observable characteristics. This component is modeled as separable from the productive characteristics, which allow us to write wages as:

$$w_{i,j,t} = E_t[\eta_{i,j,t}] \cdot \exp(X'_{it}\beta + \varepsilon_{i,t}) \quad (4)$$

The second component we aim to introduce in the empirical analysis is a time component for matching quality, which is assumed to be homogeneous across firms (A_t). This aims to capture business cycle effects on wages, which may interfere in the analysis of wage losses, as demonstrated by Nakamura (2008) and Kosovich (2010). This component interacts with both the firm and the worker components as follows:

$$\eta_{ijt} = A_t \cdot \exp(\eta_i + \delta_{j,t}).$$

As considered in the theoretical section, firms observe the labor records of worker i and offer wages according to the following prior about his productivity:

$$\eta_i^p = T_i \cdot E[\eta_i | \eta_i < \eta_R] + (1 - T_i) \cdot E[\eta_i]$$

After taking logs we end up with the following expression with respect to wage offers:

$$\ln w_{i,j,t} = E[\eta_i] + \ln A_t + T_i \cdot (E[\eta_i | \eta_i < \eta_R] - E[\eta_i]) + X'_{i,t}\beta + E[\delta_{j,t}] + \varepsilon_{i,t},$$

which can be re-stated as the following estimable relation:

$$\ln w_{i,j,t} = \alpha + \ln A_t + T_{i,t} \cdot \gamma + X'_{i,t}\beta + E[\delta_{j,t}] + u_i + \nu_{i,t}.$$

α represents $E[\eta_i]$, and γ is the stigma parameter ($E[\eta_i | \eta_i < \eta_R] - E[\eta_i]$). Note that we added one further assumption, namely the decomposition of the non-observable term $\varepsilon_{i,t}$ between u_i and $\nu_{i,t}$. The

first component represents workers fixed effects, justified by sample selection arguments to be detailed below. The second term is assumed to be a noise iid across i and t , as well as having zero mean.

Our sample consists of workers involved in episodes of displacement from the Brazilian formal sector followed by re-employment up to one year after the matching is broken. Therefore we discharged (due to lack of information) those who remained unemployed or that transited either to informal jobs or inactivity.

It seems reasonable to assume that the expected productivity of those re-employed in formal jobs (conditioned on observable characteristics) is higher than that of workers re-employed in informal firms, or the ones that remained with no job. We assume that the fixed effect drives the selection in the sample, and thus the sample selection problem is ruled-out once the analysis is also conditioned on this term.

Notice that we are implicitly assuming that workers always choose formal jobs whenever they receive offers from this sector, leaving inactivity and the informal sector as outcomes of individuals who did not fulfill the (expected) productivity requirements of the formal sector. Although strong, this assumption is necessary since we have no information about informal workers or inactive individuals.

Since we have longitudinal information about workers in different jobs, we can take first-differences across matchings:

$$\Delta \ln w_{i,j,t} = \ln w_{i,j,t} - \ln w_{i,j-1,t'} = \Delta \ln A_t + \gamma T_{i,t} + (\Delta X_{i,t})' \beta - E[\delta_{j-1,t'}] + E[\delta_{j,t}] + \Delta \nu_{i,t}, \quad (5)$$

In this notation, j and t refer to the new job while $j-1$ and t' to the previous one. As pointed out when we discussed the identification of the stigma effect, there is a correlation between $T_{i,t}$ and $E[\delta_{j-1,t'}]$. Therefore OLS estimators, as used by [Gibbons and Katz \(1991\)](#), would deliver biased estimates of γ . Hence, we introduced fixed effect for $j-1$ firms, and restricted the sample with respect to t considering only displacement episodes that occurred very close to the moment that the plant close.

4.2 Augmented specification of the empirical model

Natural extensions to the proposed exercises include investigating whether the stigma effect is heterogeneous across industries or occupations. To motivate why it could be, we may for instance imagine that being fired only signals that the worker is not suited to the type of industry or occupation he is being dismissed from. If this is the case, one would expect that the stigma effect is stronger in situations where the last job and the new job are similar. We can accommodate this argument by including a dummy variable indicating whether the new position is similar (in terms of industry or

occupation) to the previous one, and interacting this dummy with the dummy that indicates whether the worker has been laid-off or lost his job due a plant closing. In the same way, it is possible that some industries/occupations stigmatize more than others. Workers fired from firms in an industry that traditionally displays high turnover rates may not be stigmatized, given that potential employers may see this event as natural. On the other hand, if being fired is unusual for a given the type of position, then being fired may provide a stronger signal about the worker’s productivity. In this case, the inclusion of dummies (and respective interactions with T_{ij}) indicating characteristics of the last job would deal with this sort of heterogeneity. In our estimations we include two dummy variables, one for whether the displaced worker found a new job in the same industry and the other for whether he was reemployed in the same occupation. At this stage, only the latter was interacted with the dummy variable that distinguishes our two groups of interest.

Finally, one could argue that some observable variables on firm characteristics could be enough to capture the influence of the unobserved firm component on wages. In fact, Krashinsky (2002) presents evidence that the difference in wages between laid-off workers and those who lost their job through plant closings vanishes when controls for the size of the establishment are introduced in the model. Since our model is specified in first difference, we included dummies for the occurrence (or not) of a transition across establishment size categories (55-249, 250-999 and more than 1000 employees).

4.3 Results

Table 4 reports the estimates of the models presented in section 4.1 for three distinct samples. For each sample, we report the results of two specifications of the model. Both contain a dummy variable that takes on value one if the worker was laid-off and zero otherwise. The difference between them is the inclusion of additional covariates. Specifically, the second specifications include a dummy for whether the worker was reemployed in the same occupation held in the previous job, its interaction with the lay-off dummy, the workers’ tenure in the previous establishment, a dummy for reemployment in the same industry, and a set of dummies for the occurrence (or not) of a transition across establishment size categories (55-249, 250-999, and more than 1000 employees). Time dummies for the quarter and the year of workers’ displacement are included in all regressions.

The first two columns contain the results when information on *all* displaced workers in our sample is used. The following two columns display the estimates of the model based on the sample of *all* displaced workers (laid-off and plant-closing) that *were previously working in plants that eventually closed down*.

Therefore from the first to the second exercise we expelled laid-off workers from surviving firms. According to our theoretical framework, these are workers whose pre-displacement wage should be higher than the one for the workers remaining in the sample of the second exercise¹⁷. Hence this procedure tends to attenuate the bias term in equation 1.

Starting from our largest sample of displaced workers, the first column of Table 4 displays a statistically significant coefficient estimate of the lay-off dummy of approximately -0.17.¹⁸ This result evinces a negative wage differential between workers that were chosen to be laid-off and workers that were displaced from a plant closing, thus providing evidence of the existence of a stigma effect against the first group.

The inclusion of additional covariates in the second column of Table 4 makes the wage gap between our groups of interest even larger, with the lay-off dummy coefficient decreasing to around -0.19. Note that the interaction dummy coefficient not only has a small point estimate but it is also statistically insignificant. This means that the stigma effect does not seem to vary for workers that got reemployed in the same or in a different occupation of previous employment.

The results in the following two columns show there is a noticeable change in the estimated coefficients of the lay-off dummy. Indeed, as compared to the corresponding estimates in the previous two columns, this change is around 3 log points for each estimate, which represents an attenuation of approximately 17% in the stigma effect. This results are in line with our prediction above.

The last two columns also incorporate the existence of establishment-specific effects and also refer to displaced workers from closing plants, but where we restricted the group of *laid-off* workers to those that were dismissed *in the last quarter of the previous year of the plants' closing dates*.

We believe this comparison is more appropriate for the identification of the stigma effect since this group of laid-off workers are more likely to have been submitted to the same (productivity) shocks that affected the plant-closing workers next to their establishments' closing dates. This should attenuate even further the bias term mentioned above, driving it to a negligent level.

As it can be seen, the estimates of the lay-off dummy coefficients in the last two columns reveal a further attenuation of the stigma effect of around 36% and 17% as compared to the middle columns of Table 4, respectively. These figures are around 47% and 32% if the comparison is made with the first

¹⁷This prediction results from our assumption on persistence of $\delta_{j,t}$ and from the establishment surviving rule, which says that surviving establishments must have higher level of this productivity component than the ones that closes down.

¹⁸Almost all estimated coefficients across samples and specifications are statistically significant at conventional levels. So, unless otherwise mentioned, we will not point to this issue anymore.

Table 4: Coefficients of the Wage Change Equation

Covariates	Displaced workers		All workers displaced from closed plants		All workers displaced from closed plants near the closing date	
	(1)	(2)	(1)	(2)	(1)	(2)
Laid-off (A)	-0.1710 (0.0026)	-0.1927 (0.0038)	-0.1415 (0.0040)	-0.1577 (0.0049)	-0.0902 (0.0336)	-0.1340 (0.0334)
Same occupation (B)		0.0225 (0.0049)		0.0079 (0.0056)		0.0186 (0.0033)
(A) X (B)		0.0053 (0.0050)		0.0060 (0.0056)		-0.0054 (0.0057)
Age squared	-0.0026 (0.0000)	-0.0020 (0.0000)	-0.0032 (0.0000)	-0.0023 (0.0000)	-0.0020 (0.0002)	-0.0012 (0.0002)
Reemployment spell:						
Exactly 1 month	0.1688 (0.0022)	0.1546 (0.0022)	0.158 (0.0021)	0.1459 (0.0021)	0.0528 (0.0063)	0.0504 (0.0063)
2 to 3 months	0.0589 (0.0022)	0.0314 (0.0022)	0.0589 (0.0021)	0.0466 (0.0021)	0.0083 (0.0059)	0.0000 (0.0058)
4 to 6 months	0.0181 (0.0022)	0.0047 (0.0022)	0.0152 (0.0021)	0.0099 (0.0020)	-0.0016 (0.0056)	-0.0036 (0.0056)
7 to 11 months	-0.0018 (0.0022)	-0.0041 (0.0022)	-0.0031 (0.0021)	-0.0032 (0.002)	-0.0388 (0.0056)	-0.0353 (0.0055)
Tenure previous employer		0.0377 (0.0001)		0.0278 (0.0002)		0.0193 (0.0004)
Same industry		0.0231 (0.0008)		0.0130 (0.0008)		0.0013 (0.0026)
Same establishment class size		0.0059 (0.0016)		-0.0162 (0.0019)		-0.0016 (0.0064)
Moved across class sizes:						
1st to 2nd		0.0375 (0.0018)		0.0201 (0.0022)		0.0455 (0.0069)
1st to 3rd		0.0403 (0.0020)		0.0160 (0.0024)		0.0073 (0.0076)
2nd to 1st		-0.0527 (0.0018)		-0.0487 (0.0021)		-0.044 (0.0073)
2nd to 3rd		-0.0121 (0.0021)		-0.0089 (0.0023)		-0.0607 (0.0073)
3rd to 1st		-0.0304 (0.0021)		-0.0357 (0.0020)		-0.0139 (0.0102)
Intercept	0.1694 (0.0038)	0.1980 (0.0049)			0.0931 (0.0345)	0.1256 (0.0348)
	3298876 2902529	8269006 2902529	3298876 2902529	8269006 2902529	185063 138649	5615969 138649

Notes: The dependent variable is log (real) wage variation. Standard errors in parentheses. All specifications include dummies for quarter and year of workers' displacement.

two columns of the Table.

Overall, we take these results as evidence that for the precise identification of the stigma effect one should take into account the presence of unobservable, time varying establishment-specific components. In particular, it seems that this component influences pre-displacement wages driving downwards the value for workers displaced due to plant closing.

Estimates for other coefficients are worth a comment. Firstly, reemployment in the same occupation or in the same industry tends to increase the ratio of post- to pre-displacement wage. Second, as expected, longer reemployment spells tend to monotonically decrease this wage ratio, on average, for all workers. Finally, the presence of dummy variables that attempt to control for the potential effect of workers' transitions across different establishment size categories seems to matter, as in [Krashinsky](#)

(2002), but not enough to attenuate the stigma effect. These results are qualitatively similar across samples.

5 Conclusion

Using linked employer-employee data for Brazil, we find that permanently laid-off workers fare worse in terms of the post- and pre-displacement wage variation than workers who are displaced from a plant closing. Following a model in which the cause of displacement influences the inference potential employers make about workers' productivity, our results show that the wage change faced by the first group can be up to 18% lower than the wage change experienced by the second group.

This finding was maintained when different sets of control variables were used. In particular, it was robust to the inclusion of controls for the size of the establishment, a result that differs from what was found in Krashinsky (2002).

It was also robust for different specifications of the model. This was particularly valid for the first set of estimates from the model that incorporates an unobservable, firm-specific component. However, though qualitatively similar, the estimates were attenuated when a restricted sample of displaced workers was used.

Overall, we believe our results evince the existence of a stigma effect in the Brazilian labor market.

In further versions of paper we intend to investigate why there was an attenuation of stigma effect when a restricted sample of workers was used.

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