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ABSTRACT

Econometric Explorations on Bounded Rationality: The Case of Job Changing Behavior*

In this paper we question the hypothesis of full rationality in the context of job changing behavior, via simple econometric explorations on microdata drawn from WHIP (Worker Histories Italian Panel). A rational outcome of the job matching process implies a positive tradeoff between future wages and risk-on-the-job. The main result of this paper is that no "rational" tradeoff is observable after controlling for a variety of possible shifters. However, if we control for individual characteristics and replace wage growth by its predictor net of individual effects, the picture changes with the emergence of a significantly positive tradeoff between wage growth and risk-on-the-job. The interpretation is suggestive: while market forces (net of individual effects) drive towards a rational outcome, individual characteristics, instead of reinforcing the "rationality" of a positive tradeoff, lead towards the opposite direction of confounding good and bad options. Our explanation for these findings is that people act on the basis of bounded rationality à la Simon. If our assessment is correct, the implications are powerful: are there reasons to believe that such patterns are found only in the context of job search and worker mobility and not in other instances of economic behavior? Recent literature on bounded rationality strongly suggests the contrary. Why, then, should economists leave unchallenged and unchallengeable the hypothesis of full rationality? Had our investigation aimed at estimating the elasticities of wage growth and job safety of the workers' utilities, we would have miserably failed. Is this a consequence of a misspecified model or of the wrong behavioral assumptions? Our support unquestionably goes to the latter.

JEL Classification: D8, J63

Keywords: job mobility, bounded rationality, risk on the job

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1. Motivation

This paper is not on job changing behaviour *per se*.[2] Nor do we explain how choices take place. Job changing is the context in which our economic agents operate: we observe worker histories after a relatively long time since the decision to move or stay, and assess *ex post* whether the agents – movers and stayers - performed more or less rationally. Our conclusion is that workers behave according to principles of rationality that seem quite distant from those of "full rationality" assumed in the vast majority of contemporary empirical (and theoretical) studies. The idea of "bounded rationality" à la Simon provides a better fit to our observations. Recognizing the specific principles and/or decision rules that lead individual choices is, for the time being, not within reach. To say the least, however, agents appear to be largely incapable to make predictions over a reasonably long horizon. This, by itself, is a sufficient element to evoke the idea of bounded rationality, but, as we explain in the course of this paper, it is not the only one. [3]

Underlying our exploration is the widely accepted notion that the driving forces of job change are future real wages and expected job quality. We compare the performance of movers and stayers at the end of a three-year time window since the job switch of the movers. Work histories, mobility, job changes and wages are observed in a large employer-employee linked longitudinal panel (WHIP, Work Histories Italian Panel). Job quality *per se* is not: we use as a reasonable proxy an indicator of job stability (denominated "risk-on-the-job") for which appropriate measures can be obtained. Movers are selected in order to exclude all those who have moved involuntarily, i.e. following or pre-empting collective layoffs. A rational outcome of the job matching process - not necessarily implying utility-maximizing individuals – implies a positive tradeoff between future wages and risk-on-the-job.

The main result of this paper is that no "rational" tradeoff is observable after controlling for a variety of possible shifters: individual worker characteristics and career, their belonging to

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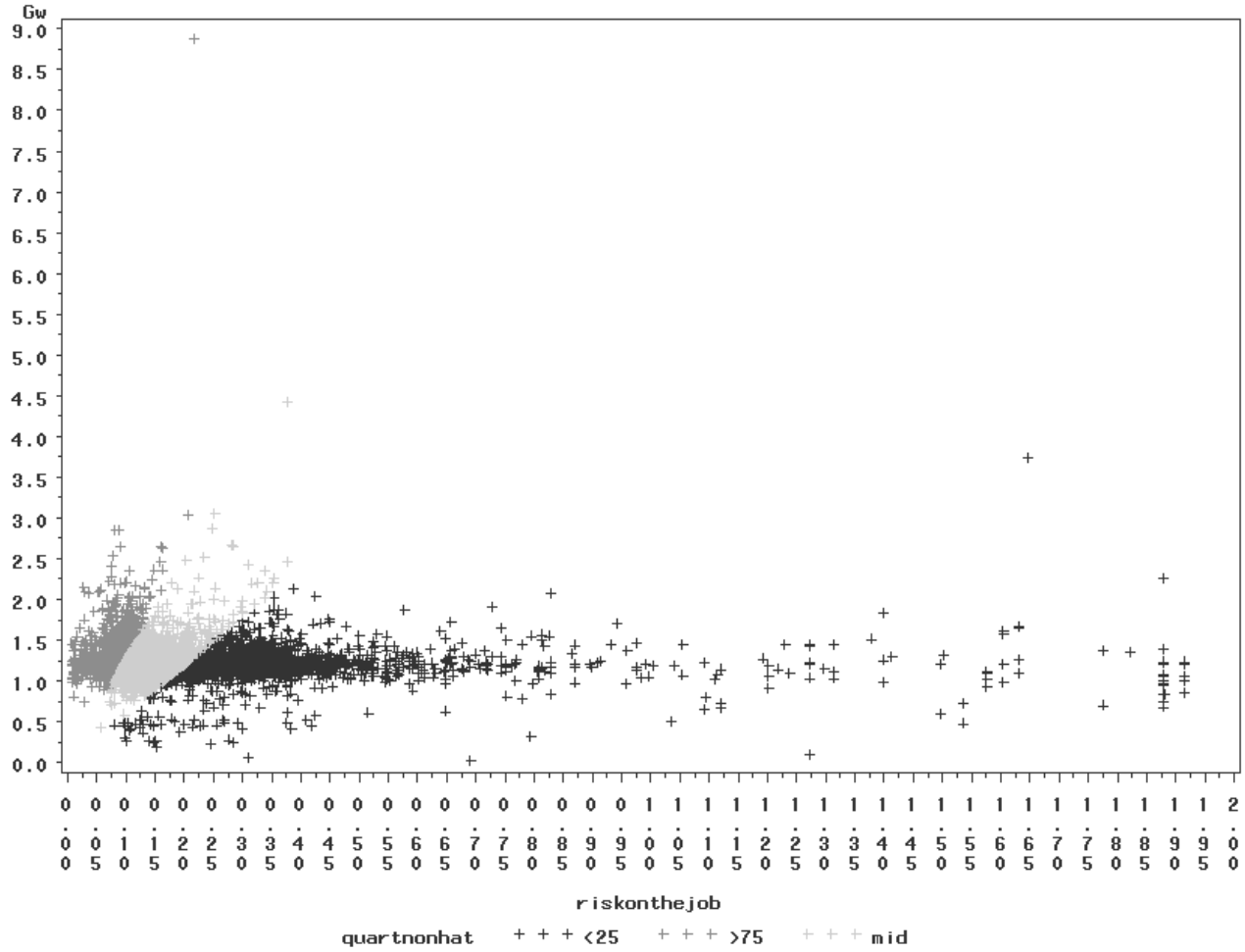
[2] In another paper we have inquired on job changing behaviour *per se*. See Contini and Villosio (2005).

[3] The timing of this exploration may not be ideal: Italy went through a recession in 1993-94, the consequences of which did not spare the labor market. This occurrence may weaken some of our conclusions on the relative performance of movers and stayers, but, in our opinion, the behavioural insights of this paper remain solid.



different industries, employer firm size and age, geographical location, past wage and appropriately defined initial conditions. Instead (and surprisingly), a fragile association between wage growth and risk-on-the-job appears to be negatively sloped. Observations look chaotically sparce in the <wage growth – risk-on-the-job> space, with the vast majority of individual positions deep inside the NW efficiency frontier (the median individual is less than half-way between the origin and the 90th percentile). However, if we control for individual characteristics and replace wage growth by its predictor net of individual effects, the picture changes with the emergence of a significantly positive tradeoff between wage growth and risk-on-the-job. The interpretation is suggestive: while market forces (net of individual effects) drive towards a rational outcome, individual characteristics, instead of reinforcing the "rationality" of a positive tradeoff, lead towards the opposite direction of confounding good and bad options. Our explanation for these findings – as will be discussed in what follows - is that people act on the basis of bounded rationality.

Some results of this investigation are in accord with standard literature, some are not. For instance, as is found in many studies on job changing behavior, movers do somewhat better than stayers in terms of wage growth. But movers are in a much worse position in terms of risk-on-the-job. Therefore, strictly speaking, the comparative performance of movers and stayers, measured by a utility function that embodies both elements depends on the relative weight given to each. Unless risk-on-the-job carries a very small weight compared to wage growth, the stayers appear to be

better performers than the movers. The implication (not surprisingly) is that the movers have a higher risk propensity than the stayers.

The exploration is repeated using a quasi-counterfactual, i.e. restricting the comparison of the movers to that of their matching stayers (co-workers of similar skills in the same firm of origin, who have not moved or have decided not to move), but the main results do not change

In recent years various papers have provided evidence of bounded rationality in a variety of specific case studies reviewed below. The underlying idea here is along similar lines. The novelty of this paper is that we investigate the presence of bounded rationality by means of simple econometric explorations on panel data that have already been used in the recent past to test standard theories of job changing behaviour.[4] Looking at the data from a novel perspective – as we do here - the results appear more consistent with the idea of bounded rationality than those suggested by mainstream approaches.

2. Review of literature

2.1 On job changing behavior

A rich body of empirical studies on various aspects of mobility and wage dynamics has grown in the Nineties. In one of the earlier studies on the United States, Brown and Medoff (1989) find significant differentials attributable to firm size among movers, but none among stayers. P. Gottschalk (2001) suggests that in the United States (1986-93) mean wage growth between jobs is large in comparison to wage growth while working for the same employer, especially for less educated workers. C. Flinn (1986) analyses the intertemporal structure of wages for young workers separately for movers and stayers. He presents evidence that unobserved worker-firm heterogeneity is an important component in the wage growth of young workers. Hartog and Van Ophem (1994) study wage growth of certain groups of employees discriminating between mobile and non-mobile employees, and between voluntary and non-voluntary job changes;

H. Farber (1993 and 1997) looks into the cost of job losses after displacement, finding that job losses adversely affects workers' earnings in many ways. Employment probabilities are reduced and a wide utilization of part-time work yields lower earnings both through shorter hours and lower wage rates. Burda and Mertens (2001) find that wages of displaced workers in Germany decline slightly upon reemployment. The lowest wage quartile (where displacement is concentrated) gains slightly, while losses for the upper three quartiles are comparable to the US evidence. Evidence on

[4] B. Contini and C. Villosio,

real wage losses consequent on unemployment is provided for the UK by S. Nickell et al. (2002): estimated hourly losses amount to 10-20% during the first year from rehiring after the first unemployment spell. Longer duration spells are associated to significantly greater losses.

The following are by now widely accepted stylized facts on job changing performance: (i) wage growth (on impact) is often higher among movers, while wage levels are lower compared to the stayers', before and after the job switch; (ii) displaced workers suffer severe wage losses on impact vis-à-vis their more fortunate peers, but often recover after a few years (Lazear, 1998) and Topel (1991).

2.2 On bounded rationality

The concept of bounded rationality and satisficing behavior was introduced by H. Simon since his early works that earned him the Nobel Prize.[5] R.M. Cyert, J. March, O.E. Willamson were among the first to systematically propound the idea in the Sixties. For almost two decades thereafter economists ignored Simon's lesson. Not until the Eighties, much to the merit of the growing experimental literature, has there been a true revival of interest in Simon's work, and the recognition that "..... when choice probems are hard, people often resort to simple rules of thumb to help them cope"[6] .

The experimental and behavioral literatures have documented many aspects of limited rationality: evidence of systematic overbidding in experimental auctions, individuals acting as though they are risk-averse over probabilities when probabilities are ambiguous (Hogart and Kunreuther, 1989), no single preference ordering identifiable when choices are elicited (Tversky and Thaler, 1990).

Bounded rationality is reported in a wide variety of real instances documented in the beautiful survey by J. Conlisk [7]. Investors often appear not to benefit from the possibility to choose portfolios for themselves: a fact that goes against a basic principle of economic theory that expanding the choice set cannot make a consumer worse off (S. Benartzi and R.H.Thaler, 2002). The behavior of US health club attendants is difficult to reconcile with standard preferences and beliefs. Members who choose a contract with a flat monthly fee of over 70 $ attend on average 4.3 times per month, paying a price of $ 17 per visit, even though they could pay $ 10 per visit using a 10-visit pass. On average these users forgo savings of $ 600 during their membership (S. Della

[5] H. Simon,
[6] S. Benartzi and R.H. Thaler (2002)
[7] J. Conlisk, "Why Bounded Rationality?", Journal of Economic Literature, vol. XXXIV (June 1996).

Vigna and U. Malmender, 2006)[8]. Similar findings are also reported in studies on consumer behavior in the credit card industry (H. Shui and L.M. Ausubel, 2004), employee choice of 401(k) plans (B.C. Madrian and D.F. Shea, 2001), purchase of large appliances (J. Hausman, 1979), purchase of flood and earthquake insurance (H. Kunreuther et al. 1978), asset prices (D. Cutler, J. Poterba and L. Summers, 1991), the "winner's curse" in real auctions (A. Roth, 1988; O. Ashenfelter and D. Genesove, 1992).

In yet another context, D. Romer [9] reports that the behavior of American NFL teams departs from the behavior that would maximize their chances of winning in a way that is highly systematic and statistically significant. This is true even in play situations where decisions are comparatively simple. The departure from win maximization are toward conservative, risk-averse behaviour. Perhaps, as Romer puts it, "the decision makers are systematically imperfect maximizers:.....many skills are more important to running a football team than a command of mathematical and statistical tools".

An important review by E. Fehr and J.R. Tyran (2005) [10] opens as follows "At least in their personal lives, many economists recognize that they are surrounded by individuals who are less than fully rational. In their professional lives, however, economists often use models that examine the interactions of fully rational agents". Several factors are indicated as explanation of bounded rationality: overconfidence about self-control and future efficiency, overestimation of future attendance, distaste for psychological transaction costs, limited memory, fallacious committment devices, time inconsistency, deliberation costs. In a series of by now classic contributions G. Akerlof and co-authors (1982, 1984, 1991)[11] introduce concepts well known to sociologists but ignored by his contemporary economists: cognitive dissonance (the bias of fitting beliefs to convenience), salience (the bias of attaching undue weight to recent events), social norms and gift advantages. R.W. Cooper (1999)[12] emphasizes the role of strategic complementarity and strategic substitutability as determinants of aggregate outcomes. Under strategic complementarity (agent i's action leads agent j to follow suit) a small number of individual irrationality may lead to large deviations from the aggregate predictions of rational models; under strategic substitutability (agent i's action leads agent j to act in opposite direction) a minority of rational agents may suffice to generate aggregate outcomes consistent with the predictions of rational models. Thus, whether the rational or irrational players dominate, the aggregate outcome depends on the strategic environment.

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[8] S. Della Vigna and U. Malmender, "Paying not to go the the gym", American Economics Review, June 2006.
[9] D. Romer, "Do firms maximize ? Evidence from professional football", Journal of Political Economy vol. 114, 2006.
[10] E. Fehr and J.R. Tyran , "Individual Irrationality and Aggregate Outcomes", Journal of Economic Perspectives, vol. 19, 2005
[11] G. Akerlof (1984 and 1991).
[12] R.W. Cooper (1999), "Coordination Games – Complementarities and Macroeconomics", Cambridge U.P. 1999).

Imperfect product market competition, thick market externalities in the presence of costly search for trading partners – both present in labor market enviroments - are important determinants of strategic complementarities.

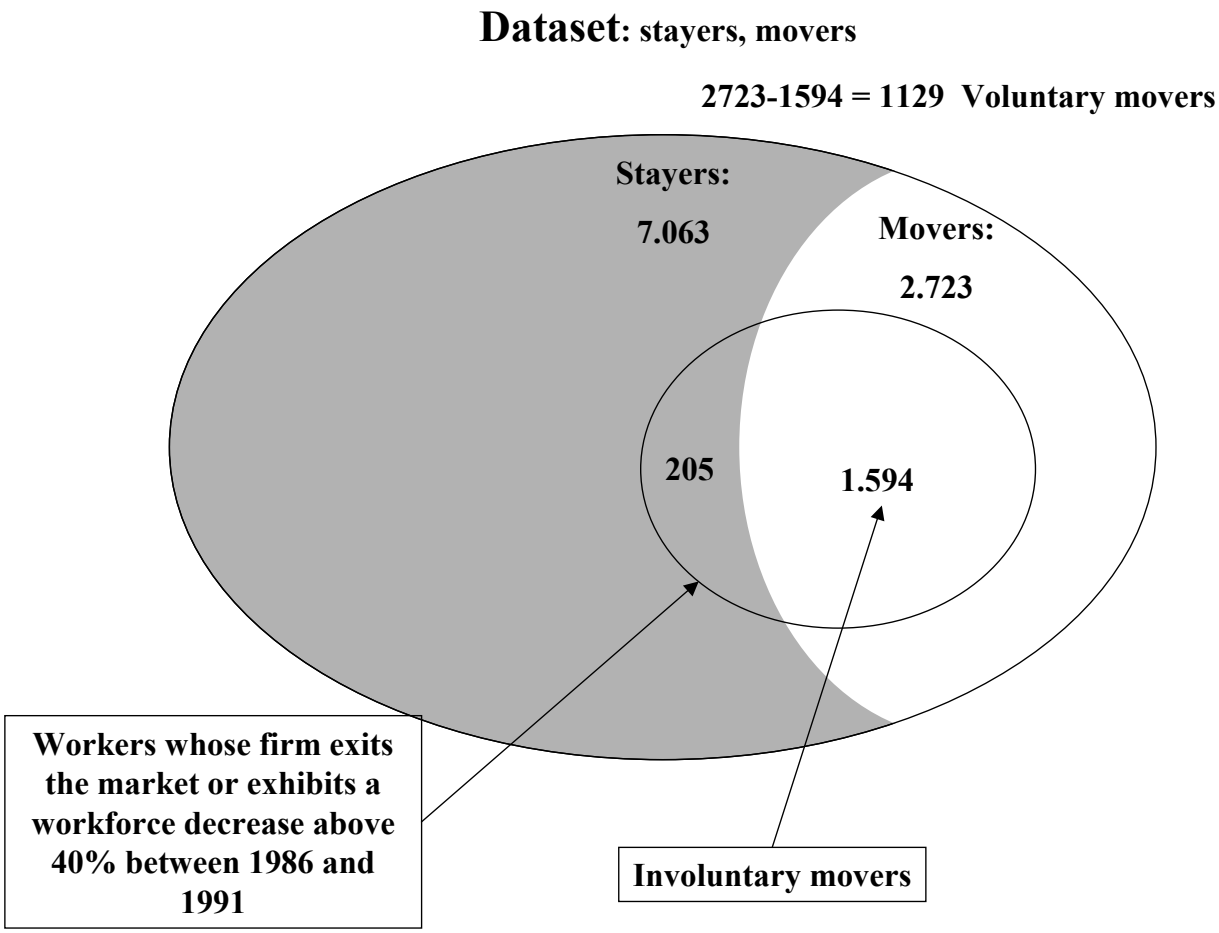
Arguments on learning and adaptation suggest that, if today things have gone wrong, tomorrow they'll go better. Unfortunately there is no general reason to believe that markets automatically render individual decision more rational over time. In long run it may work that way. Learning to improve and re-adapt one's performance – by trial and error – may, however, take minutes or years, depending on the context. In experimental situations adjustment may take place very rapidly (but this evidence involves stylized laboratory settings with small stakes and inexperienced decision makers who devote little effort to their choices); in shopping at the supermarket it may take a few days after buying and tasting a low quality marmelade. But in domains where the planning horizon is long and the stakes are high, like most life cycle decisions as well as technological evolutions of firms, learning and adaptation take time. In the early careers of young persons job changing is frequent for both workers and firms are in search of the right match: here too learning is slow. Finding a job is often costly and risky; "understanding if you like it" could take years; deciding to leave a post for a new one is a hard decision that may have adverse effects on the household's well-being and serenity.

Finally, it deserves recalling that even such "maestri" like A. Goldberger (1989) and K.J. Arrow (1986) noted that the utility maximization hypothesis has little empirical content without strong auxiliary assumptions on the utility function and other model ingredients. And, so they added, stating auxiliary assumptions is often little different from stating empirical predictions outright, as a sociologist might. In this sense, the utility maximization hypothesis merely "packages" the prediction.

3. Data

Our data are drawn from WHIP (Work Histories Italian Panel), an employer-employee longitudinal random sample of all Italian employees of the private sector, observed at monthly frequency (at the time available from 1985 to 1998, now updated to 2003). The sample-population ratio is 1:90. We use a closed panel of male individuals working full-time in the private sector, aged between 30 and 40 in 1986 (over 7000 individuals), and observe their histories and job changes from 1986 through 1996. Gender, age and working hours restrictions respond to the necessity to minimize heterogeneity of behaviour unrelated to job changing activities (maternity and

child care, retirement choices, etc.). The post change performance of movers and stayers is recorded through a sliding three-year window ending in 1996.



4 Unobserved heterogeneity or bounded rationality?

Let us suppose, for the time being, that agents act rationally on the basis of a Cobb-Douglas utility function in two arguments[13]: the observed average real wage growth over a sliding 3-year window, and an appropriate indicator of job safety.[14]

By its very nature, a two-dimensional utility hides everything unrelated to future earnings and job safety within the black box of unobserved heterogeneity. The standard explanation of why one could observe an efficiency frontier in the two arguments reserved to few agents and all the remaining ones dominated inside the frontier – as we do here - is that any tradeoff, however fragile, is an average regression with a large residual variability attributable to unobservable characteristics of the individual workers.

Heterogeneity implies- *inter alia* - that positions strongly dominated in the interior of the wage growth - risk-of-job-loss frontier may correspond to optimal choices derived from unobservable multi-objective individual preferences: Mr. X is a stayer who "loves the amenities of Taormina where he is currently working"; Mr. Y is a mover who has switched to a new job because "he hated his former boss"; Mr. Z does what he does because he is a fool. Unfortunately this argument - allowing any point in the wage growth – risk-of-job-loss space to be the optimum of some unknown preference function - leads us trapped in a black box where any empirical argument aimed at understanding how people make choices becomes irrelevant. Rationality is assumed and cannot be disproved.[15] In our view, considerations other than future pay and job stability may well contribute to explain job changing behaviour: but they simply cannot be so systematically overwhelming as to force any empirical evidence in the black box of unobserved heterogeneity. If one is seriously convinced that additional first order determinants of behaviour enter the picture, then these must be explicitly introduced in the theoretical models of job search. To our knowledge, however, this is seldom been done.

A more fruitful - and less demanding - approach suggests that limited rationality could be the key to this dilemma. Where could full rationality fail in our context ? The first and simplest answer is the length of the planning horizon necessary to drive an adult male's decision of moving or staying on a job (especially where the options are scarce as in Italy during the Nineties). It

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[13] There is no real need, for the sake of this paper, to explicitly introduce the agents' utility function. It does, to some extent, help the exposition, and it is useful in order to illustrate some of the identification problems that arise in empirical estimation.

[14] We may exclude that workers include in their objective function considerations on future pensions, health and fringe benefits. Firstly because under Italian law the rules are identical for all workers. Secondly, because our sample is selected in order to include only people in prime wage and distant from retirement.

[15] Empirical practice neglects this question. Instead of testing the predicted effect of utility maximization against the predicted effects of competing theories, economists tend to test against the non-substantive null hypothesis of no effect. In Conlisk's words (1996), this is "something like wrestling a rag doll; it doesn't prove anything, unless the ragdoll wins".

cannot be too long - certainly a long way from the infinite discounted horizon assumed by theorists of dynamic choice models – nor can it be too short - a one-shot comparison between an outside offer and one's reservation wage, however defined. A three-year horizon – itself an arbitrary choice - is a reasonable compromise and also compatible with the available data. But a three-year horizon implies a much higher difficulty to forecast into the future than a myopic one-shot assessment. This is, in itself, a strong argument in favour of limited rationality.

Are there additional arguments ? Overconfidence of future success, overestimation of one's efficiency in climbing the scale of social mobility, may also lead individuals to take chances that could be disavowed. Risk propensity – which we observe among the movers - requires overconfidence, at least as much as love for risk *per se*: in some cases it pays, in others it does not. Finally, environments leading to strategic complementarity – the labor market is one - will also induce less-than-fully-rational behaviour.

In order to provide convincing evidence in favour of bounded rationality, we must engage in a patient job of data cleaning and removing cross and composition effects. The skepticals must be insured that our reading of the data is not contaminated by prior hypotheses out of line with most of today's mainstream contributions.

5 Measurement issues

(5.a) Defining movers and stayers

Movers and stayers are defined in terms of their move/stay behavior in the 1986-1991 period. After 1991 individuals may move again or stay on the job held at the end of 1991. We intend to assess the performance of movers and stayers in 1986-91 after a sufficiently long (3 years) time horizon.

MOVERS are individuals observed in one firm in 1986 and in a different firm in 1991. Multiple moves between 1986 and 1991 are irrelevant for this definition. If the last relevant job switch occurred before 1991, say in 1990, the time window over which his performance is measured starts in 1990.

STAYERS are individuals observed in the same firm from 1986 to 1991, although their career may have been interrupted by short unemployment (or temporary layoff) spells in between.

The comparison between stayers and movers must involve only the voluntary movers.[16] We recognize the involuntary movers as those who are found on a job in 1991, but have been – as it

[16] In principle the appropriate comparison ought to involve voluntary movers as well as voluntary stayers, i.e. individuals who have received an outside offer and decided to reject it. While selecting the voluntary movers is a relatively easy task, there is no obvious way to select the voluntary stayers. Will this pose a problem of self selection affecting our result ? The answer is no. As will be discussed shortly, our estimates indicate that the stayers' performance is, on the whole, better than the movers'. Therefore if we could select out the involuntary stayers, the result

were -"forced" to leave a preceding position as a consequence of collective layoffs and/or incumbent industry or firm crises. They are singled out as the individuals who are at work in 1991 after having switched jobs in the 1986-91 period: either (i) leaving firms that had closed and exited the market before 1991; or (ii) had undergone drastic workforce reductions before 1991 (> 40% of 1986 workforce, under the assumption that the move aimed at pre-empting future likely layoffs).

(5.b) Measuring wage growth and risk-on-the-job

(b.1) Real (long run) wage growth (G-w)

STAYERS:

w-p =average yearly wage earned during the 3-year spell started in 1990:

(yearly wage / reported working days) * 312

where 312 is the standard number of working days in one year;

w-a = average yearly wage earned at the end of 1990.

MOVERS:

w- p =average yearly wage earned during the 3-year spell after the job switch (which may take place between 1988 and 1992), weighted by the number of working days as above;

w- a = average yearly wage earned at the end of the period preceding the job switch.

Nominal wages are deflated by CPI. Therefore:

real (long-run) wage growth = (G-w) = w-p / w-a

(b.2) Risk-on-the-job (ROJ)

The risk-on-the-job indicator is built on the basis of two elements: the worker-specific predicted likelihood of dismissal in the past 1986-91 time window, and a forward looking firm-specific indicator of employment trend over the subsequent three-year period 1991-94.[17] The former is weighted by the latter as follows:

risk-on-the-job = ROJ = [predicted individual likelihood of dismissal 1986-91, given individual and firm of origin characteristics] / [firm employment trend 1994 / 1991]

Suppose that Mr. X's predicted likelihood of (past) dismissal is 0.30. If Mr. X stays at his firm of origin and such firm increases employment by 50% in the next 1991-94 period. Mr. X's

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would, *a fortiori*, be reinforced as the utility, however defined, of those who lack alternative options will be lower than the utility of those called to express a choice.

[17] The trend is calculated as follows : [E(1994) / E(1991) + 0.10]. Adding 0.10 prevents the ratio (in the denominator of ROJ), from becoming zero when the firm goes bankrupt and/or exits the market.

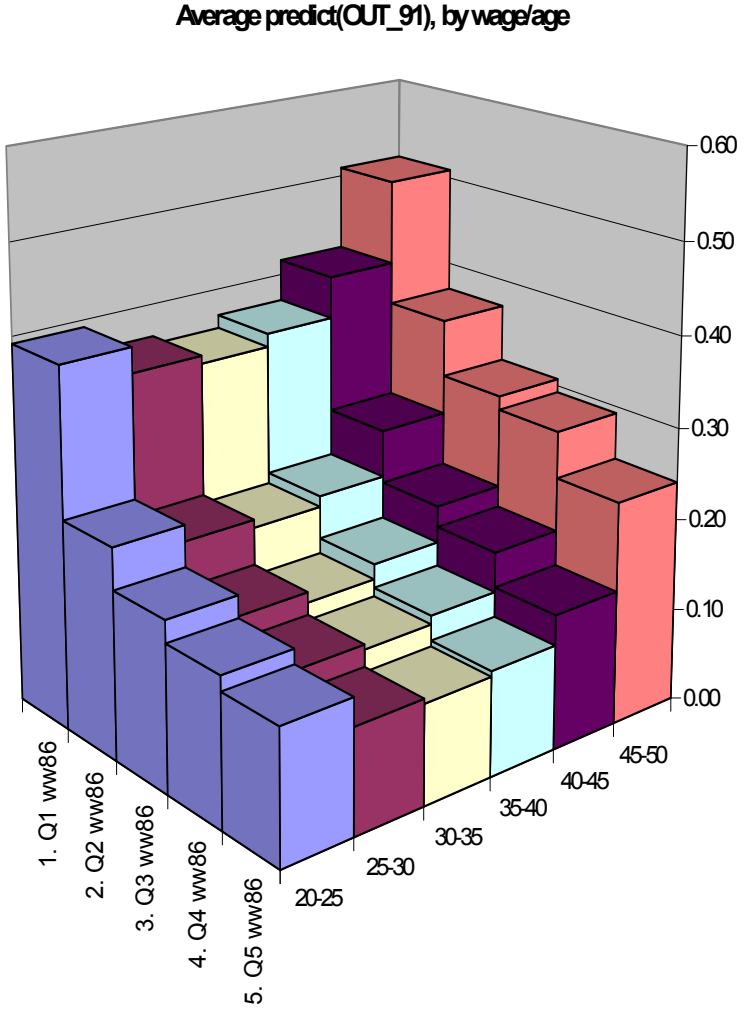
risk-on-the-job is reduced to 0.30/(1+ 0.5) = 0.20. If he moves to a different firm that cuts employment by 20%, his risk-on-the-job increases to 0.375 = 0.30/(1-0.2).

(a) likelihood of dismissal prior to 1991

In order to estimate the likelihood of dismissal, we resort to the open panel 1986-1991, including all full-time male workers aged 20-50. In 1986 the number of workers on payroll is 36,114; of these, only 15,394 are left by 1991. We estimate a logit separately for white and blue collars, against a set of covariates including age and age-square, wage, industry, location, firm size and firm employment trend, initial conditions and various interactions. All the main covariates are highly significant (results are available).

Fig. B displays the predictor of dismissal as a function of age and initial pay (by wage quintiles): the predicted likelihood is U-shaped in age and decreasing in wage. Not surprisingly prime-age workers are those at least risk of dismissal, while at high risk we find the low paid independently of age. Under the plausible assumption that wage and productivity are correlated, this strongly suggests that firms in need of downsizing tend to retain their most productive workers. [18]

[18] P. Gautier et al. (2002) investigate firm downsizing in the Netherlands. Their findings are similar to ours: at each job level it is mainly the lower educated workers who leave during downturns.



(b) projected employment trend

Firm employment histories are observed through 1996. The ratio between total employment on each firm's payroll at the end of 1994 and in 1991 provides a simple indicator of firm-specific trends. The movers' ratio E(1994) / E(1991) is measured at the firm that made him a successful offer (around 1991). Nearly two thirds of the observable firms reduce their workforce in the 1991-94 period that falls around the 1993 recession: this is in line with well known trends of the Italian labor market. A striking 16% of the movers who switched jobs around 1991 end up in firms that exit the market before the end of 1994, while only a modest 0.6% of the stayers (who did not make the switch) are in the same position. As will be reflected in ROJ, the movers are much more exposed to the risk of job loss than the stayers: this is an interesting and novel result for which we find no precedents.

6 Measuring performance: a pseudo-utility function

Workers accept job offers on the basis of two criteria:

1 if the wage offer is "sufficiently high" (i.e. higher than some unknown reservation wage);

2 if the offered position is subjectively perceived to be "sufficiently stable" (i.e. with a low probability of being dismissed or forced to leave).

Both arguments imply a subjective judgement on the future evolution of earnings and on the quality of the job. It is convenient, although not strictly necessary for our argument, to assume a Cobb-Douglas utility function (U) in two arguments: the observed (ex-post) real wage growth over the future 3-year window (G-w), and a proxy of risk-on-the-job (ROJ) defined above:

U = (G-w) / RISK.

6.1 Different performance of movers and stayers

Previous research on these data – relative to the 1986-91 period - established the following results[19] : (i) the mean initial wage (1986) as well as the mean final wage (1991) of the stayers is higher than that of the movers; (ii) the wage growth of the movers is slightly higher than the stayers'. Movers do better than stayers at young age (20-30), but the difference tends to vanish thereafter; (iii) mover-stayer differentials are larger among white-collars than among blue-collars.

The following differences are found here:

- Wage growth (G-w)

Movers do better than stayers in terms of wage growth in the 3-year window following 1991, but only beyond the median. In the low tail of the distribution we find a slight prevalence of the stayers. The same pattern holds for both blue and white collars (fig. A). The variance of the movers is slightly larger than the stayers'.

- Risk-on-the-job (ROJ)

The situation is reversed, with the stayers facing a much lower risk-of-job loss than the movers. Movers appear to be risk-prone, willing to accept a higher pay at considerable cost in terms of job safety. At P50 the stayers' ROJ is 0.12 against 0.16 for the movers among white-

[19] B. Contini and C.Villosio (2005), " Worker mobility, displacement, redeployment and wage dynamics", ch. 16 in B. Contini and U. Trivellato (2005)

collars; 0.12 against 0.20 among manual workers. At P75 the difference increases to 10 p.p. (0.18 vs. 0.28) and 14 p.p. (0.18 vs. 0.32) respectively. Beyond P75 the differences explode (fig.B). The ROJ variance is much larger among the movers.

	Sample size	ROJ –Mean	ROJ - Median	ROJ – Interquartile Range
Movers	1028	0.30	0.18	0.23
Stayers	6766	0.16	0.13	0.09

More specifically, a simple OLS regression (not included here) indicates the following p.p. differentials of ROJ due to firm size, mover-stayer status, skill level and industry (manufacturing (M) vs. public utilities, banking and communication (PUBC)):

	STAYERS				*MOVERS*			
	M		*PUBC*		*M*		*PUBC*	
	small	*large*	*small*	*large*	*small*	*large*	*small*	*large*
BLUE	*+ 60*	*- 20*	*+ 30*	*- 80*	*+ 150*	*+ 30*	*+ 80*	*- 35*
WHITE	*+ 60*	*- 30*	*+ 60*	*- 125*	*+ 130*	*- 5*	*+ 100*	*- 20*

Employees of large PUBC firms are much more protected from ROJ than M–workers; not surprisingly, the large firms offer more protection than the small ones; likewise, the white-collars are less exposed to ROJ than their blue-collar colleagues. The risk-on-the-job of the job changers who leave small M-businesses is more than twice as high than the average ROJ.

Utility

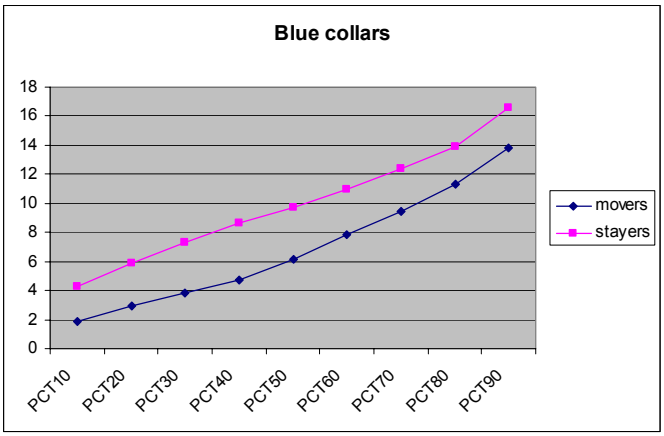
The assumption of unit elasticities (+1 and –1 respectively for numerator and denominator) leads to the dominance of the stayers over the movers, with the ROJ differential driving the result (tab. AA). About 43% of the movers are found in the first quartile of the U-distribution, against 22% of the stayers. Conversely, 26% of the stayers belong to the upper quartile against less than 20% of the movers. These results depend on the adopted measurement scale. With much larger wage growth elasticities - for instance U* = (G-w)***3* / ROJ - the stayers' dominance is reversed only beyond P90.

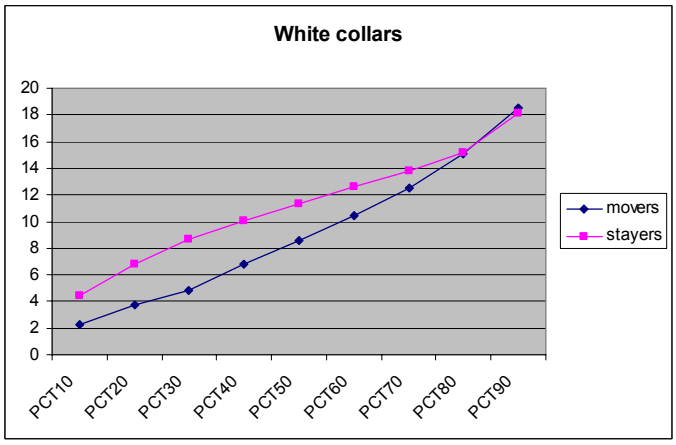
Tab. AA - Distribution of stayers and movers in the U = (G-w) / ROJ - percentile bands

	< P10	P10-P25	P25-P50	P50-P75	P75-P90	> P90	total
Stayers	7.64	14.47	25.73	26.32	16.04	9.80	100
Movers	25.58	18.48	20.14	16.34	8.17	11.28	100

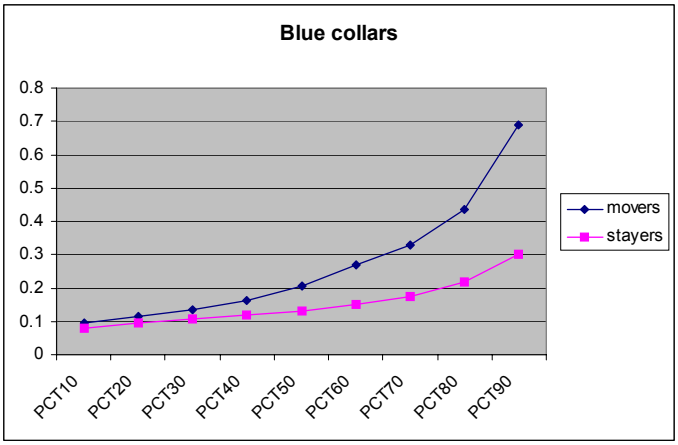
The following graphs show the percentiles of U and its two components, separately for movers and stayers.

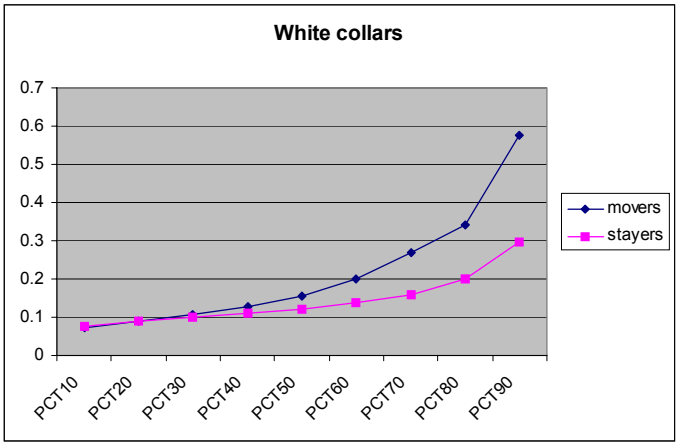
Utility = U = (G-w) / ROJ



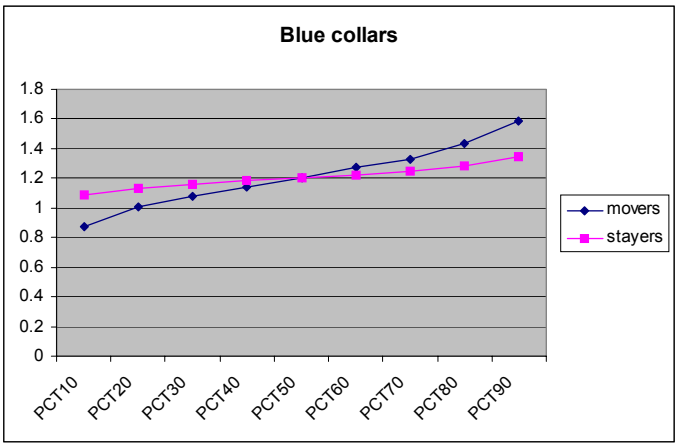


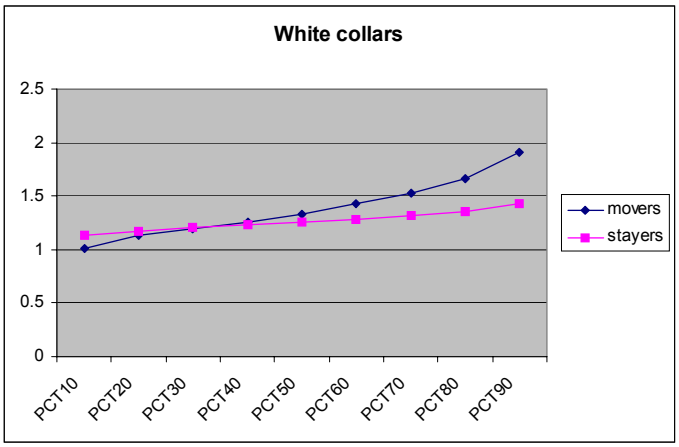
Risk-on-the-job = ROJ





Wage growth = (G-w)





	wage growth = (G-w)		risk-on-the-job = ROJ	
	stayers	movers	stayers	Movers
Mean	1.23	1.30	0.16	0.31
Median	1.22	1.25	0.13	0.18
IQ	0.13	0.36	0.09	0.23

6.2 Within-group performance (distance from the U-frontier)

The relative performance of each group (the blue-collar movers and stayers, the white-collar movers and stayers) is also measured by the distance from its own frontier, placed at the 90% percentile of the group specific U-distribution.

U – Percentile ratios
(distance from the 90% percentile of the distribution)

	BLUE collars		**WHITE collars**	
	movers	**stayers**	**movers**	**stayers**
P25/P90	**0.20**	**0.40**	**0.25**	**0.39**
P50/P90	**0.38**	**0.59**	**0.48**	**0.56**
P75/P90	**0.62**	**0.79**	**0.76**	**0.75**

This measure is very conservative, as it puts the frontier at the 90% percentile of the distribution, a drastic truncation of the top 10% individuals. The lower the ratio, the larger the distance.

The median mover is well below half way from the frontier among the blue-collars, and slightly below among the white-collars. The median stayer is somewhat closer to the frontier, with a percentile ratio of 0.59 and 0.56 among the blue-collars and the while-collars respectively.

This finding is an additional reflection of the higher variability of the movers' performance compared to the stayers'.

7 Estimation of the trade-off

(i) OLS estimation

We estimate the trade-off between real wage growth and risk-on-the-job with the following linear model:

(1) $$(G-w)(i) = a + b\,ROJ(i) + c\,X(i) + d\,I(i) + e\,INTER(i) + u(i)$$ [20]

[20] This model is similar to the specification derived from theoretical equilibrium conditions of job search theory:

with X(i) numerical covariates: age & age-square, last wage (in the firm of origin for the movers, in 1991 for the stayers), number of working days reported during last job spell, number and timing of job switches before the last move, length of unemployment spells between jobs, initial conditions 1986 (proxied by the ratio of individual wage to average firm wage); I(i) dummy indicators: 2 skill groups, 9 industries, 4 geographical areas, 3 firm sizes, mover-stayer status; INTER(i): all relevant interactions. There are no endogeneity problems with ROJ as it is estimated from worker-specific covariates prior to 1991, and forward looking firm-specific elements.

(ii) Removing individual fixed effects from wage growth

Equation (1) is a cross-sectional sample describing wage growth and risk-on-the-job in the 1991-94 time window. It would be inappropriate – in addition to being unfeasible given the nature of the data - to perform fixed individual effects estimation on this specification. A "within" estimate would show the tradeoff of the individual agents through time, with no explanation left for the enormous dominance relations that we observe across individuals. What we need is a "between" estimator that emphasizes such differences.

There are, nonetheless, reasons to suspect that fixed individual characteristics are present in the l.h.s. variable (G-w) *per se*: individual wage growth is, to some extent, "negotiated" between employer and employee, and the latter's own characteristics will affect the bargain. Fixed individual effects are, instead, under control in ROJ: ROJ is the ratio between the individual likelihood of dismissal in the 1986-91 window, and the firm-specific employment trend between 1991 and 1994. The numerator has been estimated with appropriate controls for initial conditions, while the denominator contains only firm-specific elements.

Removing fixed effects from wage growth is feasible as individual (G-w) is observable in the 1991-94 window as well as in the previous 1986-91 period.. We use first difference estimation on a standard specification of (G-w) that includes many of the covariates present in (1), but not ROJ. Let (G-w) (0,i) and (G-w) (1,i) be the wage growth of the i-th individual in the time windows 1986-91 and 1991-94 respectively. X and I indicate the same covariates as in (1). Taking differences, we obtain

---

ln wage (i) = f (B, theta, nu, r) + g (X(i)-controls) + residuals (i)

ln individual wage is the l.h.s. In the r.h.s. we find B = unemployment benefits, a proxy of the reservation wage, theta = labor market thickness (or arrival rate of a job offer); nu = bargaining strength, a shift factor; r = a discounting factor incorporating all future dynamics. B, theta, nu, r are estimated and/or calibrated cross section and/or time varying average values. "Theta" and our "risk-on-the-job" convey similar (but opposite) concepts of job stability / instability. Thus, the implicit tradeoff between wages and job stability is negative, while it is positive according to our formulation.

$$(G\text{-}w)(0,i) = \alpha(i) + \beta * X(0,i) + \gamma * I(0,i) + res(0,i)$$

$$(G\text{-}w)(1,i) = \alpha(i) + \beta * X(1,i) + \gamma * I(1,i) + res(1,i)$$

(2) $\Delta [G\text{-}w(i)] = (G\text{-}w)(1,i) - (G\text{-}w)(0,i) = \beta * [X(1,i) - X(0,i)] + \gamma * [I(1,i) - I(0,i)] + [res(1,i) - res(0,i)]$

which yields β and γ coefficients non contaminated by individual effects affecting wage growth. Let $\beta^{\wedge\wedge}$ and $\gamma^{\wedge\wedge}$ be the non-contaminated estimates. We retrieve the non-contaminated predictors of (G-w) (1,i) as follows:

(3) $[G\text{-}w(1,i)]^{\wedge\wedge} = \beta^{\wedge\wedge} * X(1,i) + \gamma^{\wedge\wedge} * I(1,i) + mean[(G\text{-}w)(0,i)]$.

At this stage we are in a position to obtain a new OLS estimate of a specification identical to (1), except for the dependent variable which is now net of individual effects[21]:

(4) $[G\text{-}w(1,i)]^{\wedge\wedge} = a' + b' ROJ(i) + c' X(i) + d' I(i) + e' INTER(i) + u'(i)$

We display here the OLS estimates of the two trade-offs, the first one with (G-w) in the l.h.s., and the second one with the non-contaminated $(G\text{-}w)^{\wedge\wedge}$. Both dependent variables, as well as ROJ have been normalized.

	(G-w)		**(G-w)^^**	
Intercept	2,28769		-0,47116	
white	**0,29658**	**+++**	**0,42832**	**++++**
ROJ = risk-on-job	**-0,04828**	**+**	**0,06355**	**++**
ggretr_mean	-0,000431		-0,00107	+
R1	0,19278	++	0,28243	+++
R2	0,09967	+	0,19292	+++
R4	-0,0056		0,01454	
R6	-0,01691		0,05781	+
R7	0,0556		0,7485	++++
R8	0,22283	++	-0,05827	+
NOV	0,02716		-0,11535	++
NES	0,04564		-0,16989	++
SUD	0,00088767		-0,13829	++
ISO	-0,18855	+	-0,22715	++
SMALL	**-0,16395**	**+**	**-0,40274**	**+++**

[21] If the tradeoff were estimated regressing ROJ against $(G\text{-}w)^{\wedge\wedge}$ and other covariates, the procedure would be equivalent to TSLS estimation. The results are available and yield few additional insights: stayers are more exposed to ROJ if they are on the payroll of small-size firms. Large firm workers are slightly more protected from ROJ, while – *ceteris paribus* – movers are less protected than stayers.

LARGE	**0,0475**		**1,24615**	**++++**
Mov88	-0,61105		0,00027731	
Mov89	-0,64092	+++	0,09512	
Mov90	-1,14012	+++	0,00949	
Mov91	-1,36665	++++	0,08391	
ETA	-0,07523		0,02556	
eta2	0,00090294		-0,00038642	
moves	0,7002	+++	-0,00777	
INEQ86	**-0,33092**	**+**	**-0,38402**	**++**
born_nw	-0,0726		0,0408	
born_ne	-0,09322		0,06555	
born_so	-0,09959	+	0,11669	++
Born_is	0,07197		0,15501	++
Born_ee	-0,11763		0,04713	
sta	**-0,51657**		**-0,96915**	**++**
ROJ * sta	**0.02996**		**0.06454**	**++**
ggretr_mean_sta	-0,00198	+	0,00398	+++
ineq86_sta	0,50703	++	-0,27873	++
small_sta	0,1688	+	-0,04553	
large_sta	-0,09621		-0,04691	
Adj R-square	0.09		0.61	

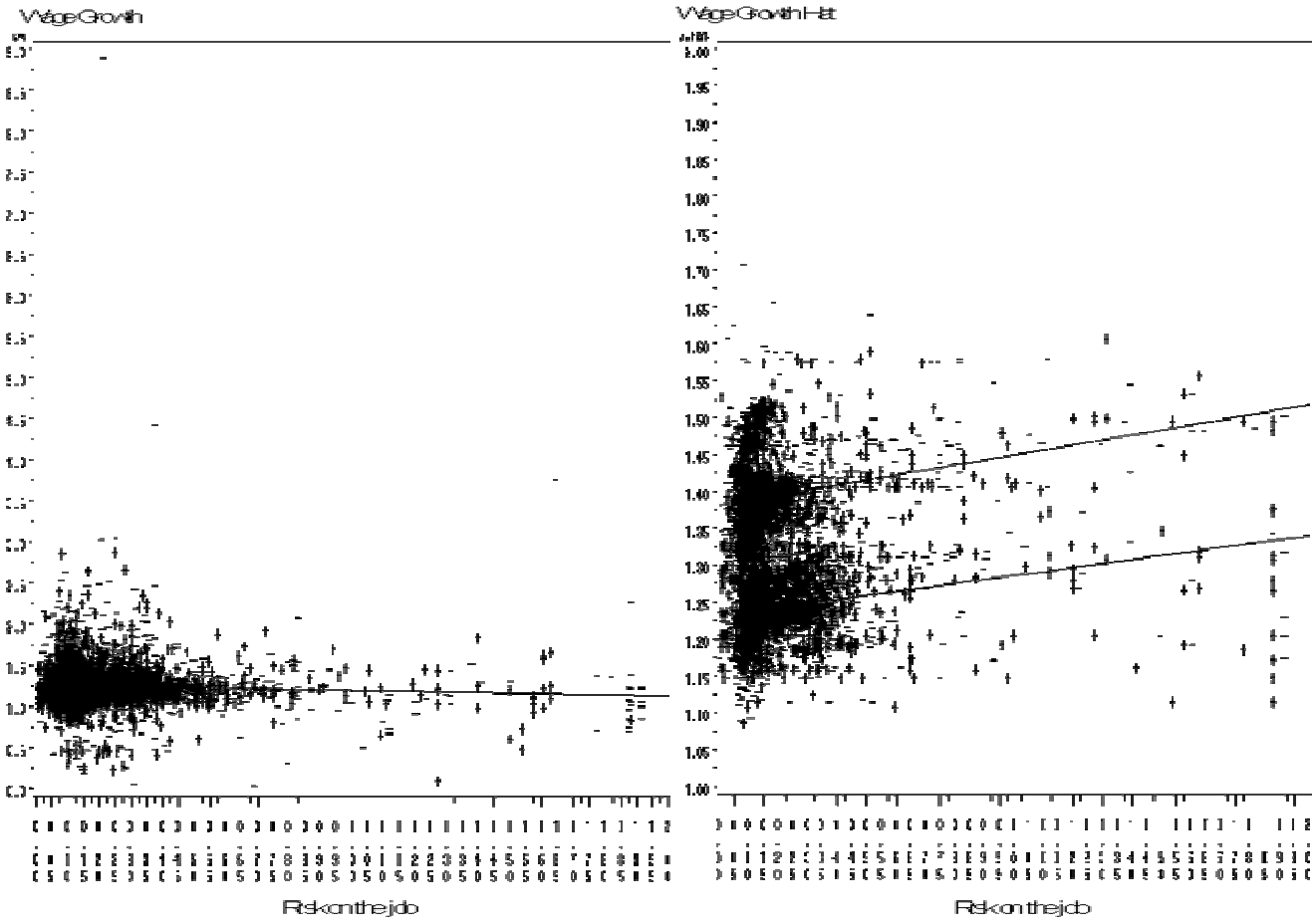
The comparative size of Adj R-square (0.61 vs. 0.09) is a straightforward consequence of the much smaller variability of the non-contaminated predictors (G-w)^^ used as dependent variable, compared to the (G-w) inclusive of individual effects.

The most intriguing result of these estimates is the size and sign of the ROJ-coefficient, which is negative, slightly above significance, in the all-inclusive version (G-w), and robustly positive in the non-contaminated (G-w)^^ version. No "rational" tradeoff is observable in the <wage growth (G-w)> vs. <risk-on-the-job (ROJ)> space defined in terms of the (G-w) variable inclusive of individual effects, after controlling for a variety of possible shifters. The tradeoff consistent with rational behaviour must be positively sloped because higher wage growth compensates for higher risk of job loss: here the observable tradeoff is negatively sloped (although fragile). Observations are chaotically sparce in the <(G-w) vs. ROJ> space, with the vast majority of individual positions deep inside the potential frontier (the median individual is less than half-way between the origin and the 90th percentile – see par. 6.2).

The picture is quite different if we replace wage growth by its predictor net of individual effects: the expected positive tradeoff between wage growth and risk-on-the-job emerges very neatly (the estimated ROJ coefficient is 0.06), and it is stronger among the stayers (about 0.13). The interpretation is suggestive. The market, net of the interference of individual characteristics, operates as theory predicts and leads toward a "rational" outcome. Once individual characteristics enter the picture via their impact on wage growth (G-w), instead of reinforcing the "rationality" of a

positive tradeoff, they push in the opposite direction of confounding good and bad options. What is behind such an unexpected finding ? People may act on the basis of bounded rationality, but how could their actions turn around the outcome suggested by sound principle of rationality ? While recognizing the specific principles and/or decision rules that lead individual choices is, for the time being, within reach only in very specific circumstances (some of which reviewed in par.2.2), a few stylized facts about bounded rationality may be taken for granted. The main one, limited information and computational ability, is the cause of large forecast errors, the more so the longer the time horizon involved.[22] Overconfidence, fallacious commitment devices, cognitive dissonance, salience, slow learning are all additional factors that lead individuals to behave in ways that are not in accord with the economists' full rationality, and that may worsen forecasting abilities. Strategic complementarity (agent i's action leading agent j to follow suit), contagious and herd behaviour, for long time present in the sociological literature, are important determinants of aggregate outcomes and of collective forecast errors. This turns out to be particularly relevant in our exploration: as we explain in par. 9 forecast errors are a serious cause of under-identification that leads to additional fuzzying of the all-inclusive estimated tradeoff.

[22] In addition to the classical Simon's arguments, J.D. Hey (2005) reports experiments aimed at detecting how far subject are looking when tackling simple dynamic decision making problems. He suggests that very few plan right to the end of the problem and many do not look ahead at all.



Left panel: the all-inclusive tradeoff: (G-w) dependent variable

Right panel: the non-contaminated tradeoff: (G-w)^^ dependent variable. The two clusters correspond to small firms (below) and large firms (above).
Vertical scales of the two graphs are different to account for the larger variability of (G-w).

Let us now turn to the remaining notable indications: (i) the negative stayer-dummy is significant in the (G-w)^^ equation, but not in the (G-w) version: for given ROJ the stayers earn a lower wage growth. Recall, however, that movers face a much higher risk-on-the-job; (ii) the white-collar dummy, positive as expected, and very significant in both versions; (iii) the large firm-size effects in the non-contaminated (G-w)^^ tradeoff, explaining the two clusters of individual observations – the large firm size dominating the small one; firm-size effects are instead close to zero in the all-inclusive tradeoff. Here too, this result holds for given ROJ, which is higher at small firm size; (iv) the INEQ86 indicator of initial conditions, pointing to the fact that wage growth is, *ceteris paribus*, lower, the higher one's initial wage relative to the average wage paid by their 1986-firm; (v) the "move" dummies (both "year-of-move" as well as "number-of-moves") are significant only in the all-inclusive (G-w) estimate, but have opposite sign and nearly cancel each other out.

The weakness of the estimated all-inclusive trade-off can be illustrated as follows. We form groups of individuals characterized by attributes that, according to the coefficient estimates, should robustly place them in the upper (lower) part of the scatter in the <(G-w) – ROJ> space and of the resulting U-distribution (above P75 and below P25).

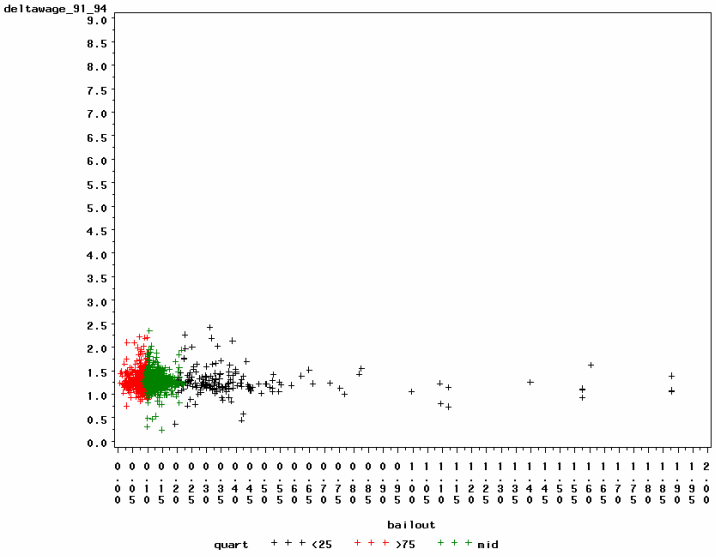
	Sample size	<P25	>P75	P25--P75
U high: above P75				
group 1: PUBC - industries	2450	7	48	45
group 2: white collars at large firms	1576	8	41	52
group 3: large firms & frequent movers	53	28	30	42
U low: below P25				
group 4: stayers in small firms	1044	58	6	36
group 5: frequent movers	278	59	13	28

Group 1 includes 2450 individuals active in the PUBC – industries (1: public utilities, 7: transports and communications, 8: banking and insurance). The majority of firms are large, unionized, sheltered from competition (inspite of the privatizations since the Nineties). The estimated industry dummies in the all inclusive (G-w) trade-off are large and positive, while they are significantly small in the ROJ equation. Workers of these sectors ought therefore to be found in the top percentiles of the U-distribution. The finding is somewhat at odds with expectations: while only 7 % are in the lower quartile, 45% are in the interquartile range and only 48% in the top quartile. For groups 2 and 3 similar comments apply.

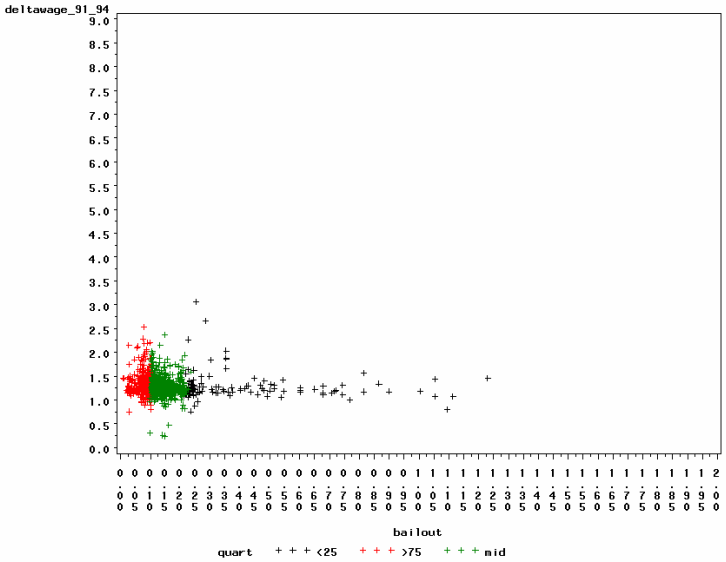
Group 4, on the other hand, is formed by 1044 stayers on the payroll of small-size firms. The relevant dummies are significantly negative in the (G-w) equation and positive in the ROJ version, which should place them in the low percentiles of the U-distribution. Here too the findings are contradictory: 58% are, as expected, in the lower quartile, but 36% are in the interquartile range and 6% in the top quartile. Group 5, formed by the frequent movers (278 workers), is somewhat more in line with expectations: 59% of the individuals are below P25, 28% in the interquartile range and 13% in the top quartile.

The scatter diagrams for the five groups are displayed in fig. XYZ (5 panels): in principle group 1,2,3 workers ought to be predominantly red, groups 4-7 predominantly black. Clearly this is not the case.

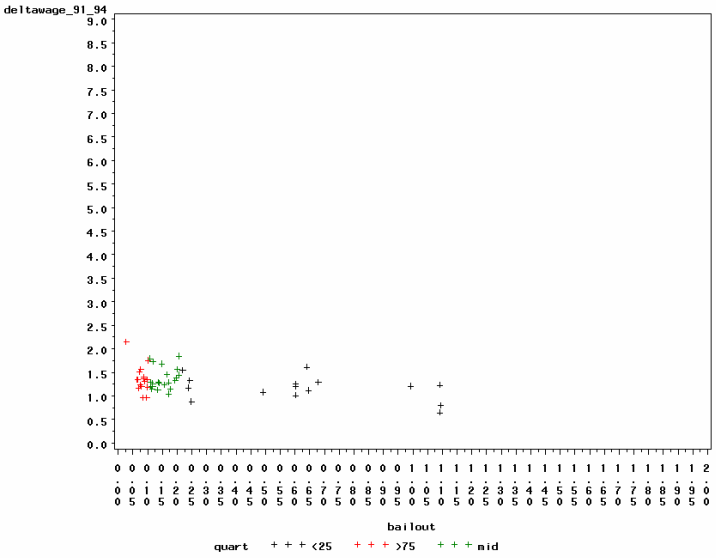
U bands: group 1



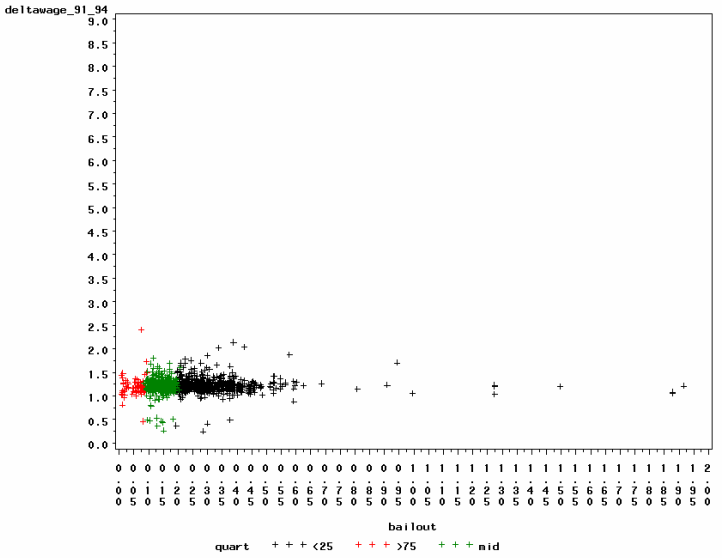
U bands: group 2



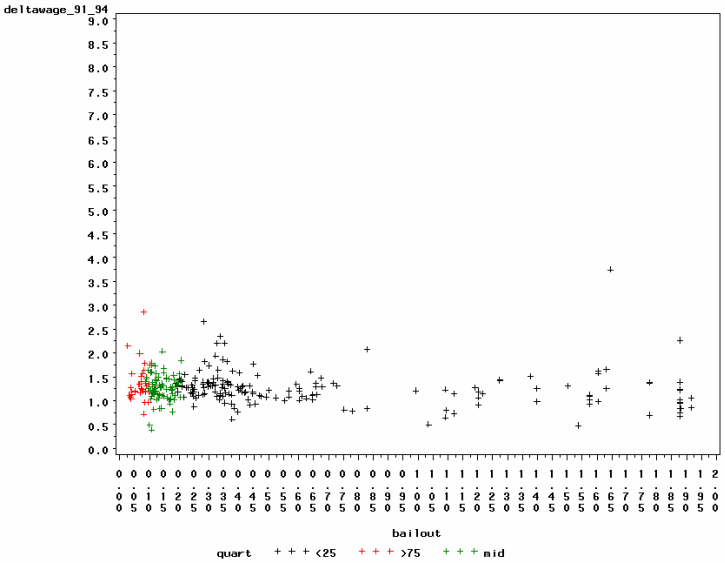
U bands: group 3



U bands: group 4



U bands: group 5



8 Additional evidence

8.1 A quasi-counterfactual analysis: movers vs. matching stayers

It would be enlightening if we could respond to the question "how would the movers have performed had they decided not to move ?". Direct evidence is, obviously, not available. But the data allow to observe the history and performance of a certain number of individuals of the same skill group, co-workers in the firm from which the movers' job switch originated. Observations are unfortunately precluded to firms employing less than 150-200 employees.[23]

We link each mover to his observable co-workers. The linkage yields 220 groups with at least 3 individuals observed contemporarily (out of 1594 movers in the whole panel), and 74 with only 2 (one mover and one stayer). The latter are conservatively excluded from this exploration.

The stayer co-workers ("matching stayers") of the same skill group represent a quasi-counterfactual: they are as similar as possible to the movers at the beginning of the observation period. The underlying assumption is that the average matching stayer has been faced with the same options offered to his colleague mover, and has turned them down. As explained below, this assumption reinforces the conclusion.

The PREMIUM ratio, defined as

$$\text{PREMIUM} = U(\text{movers}) / U(\text{matching-stayers})$$

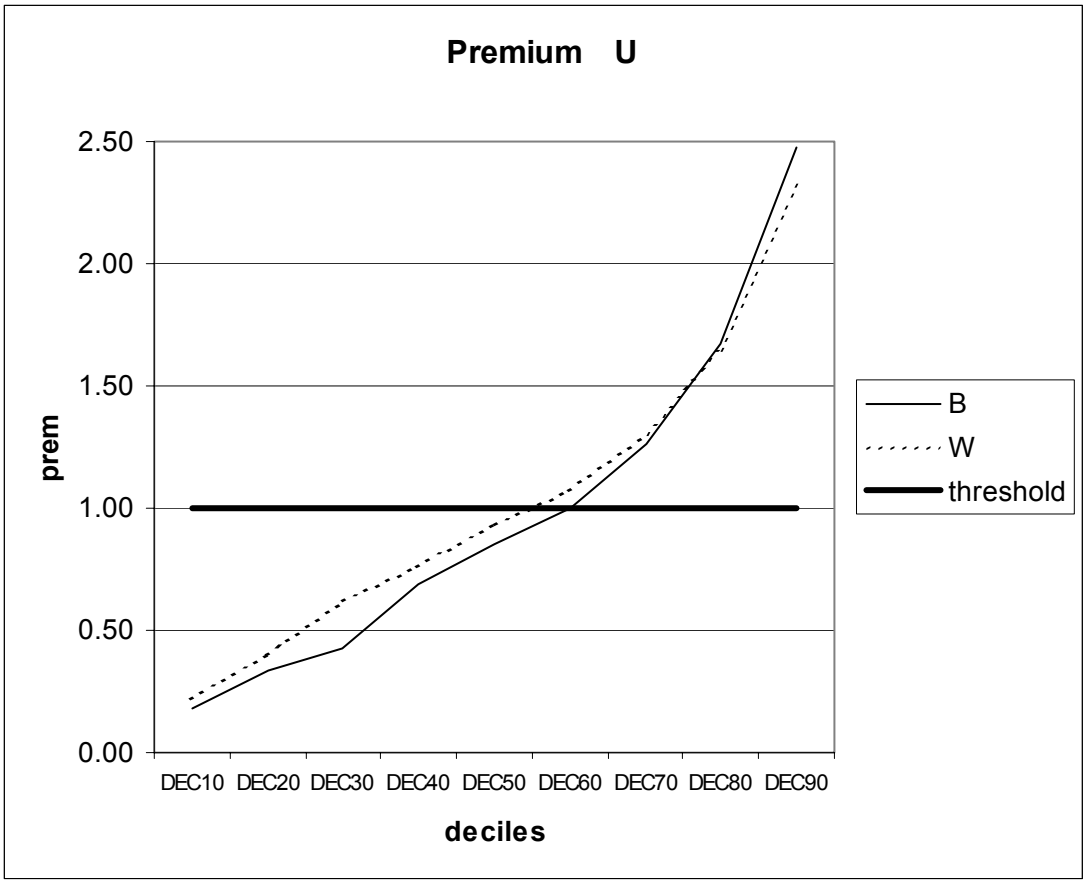
shows the relative performance of the movers vis-à-vis the average matching stayers.

Fig. XA summarizes the information derived from the PREMIUM-percentiles, computed separately for blue and white-collars. Among the manual workers, the median mover performs worse than the median matching stayer: in about 60% of the cases we observe PREMIUM < 1. Among the white-collars, instead, the comparative performance is split at the median (PREMIUM reaches 1 at P50).

In conclusion, we find a weak evidence of "sound" decisions of the movers relative to their matching stayers, whether blue or white-collars. The answer to the question "how would the movers have performed had they decided not to move" would, in the majority of cases, be "they would have performed better".

---

[23] The WHIP sample is drawn from the population of individual workers, the sampling ratio being approximately 1:90. This procedure leads to a modest oversampling of the large firms vs. the small ones: on average we observe 10 workers on the payroll of a company with 900 employees, but only 1 worker employed by firms with less than 50 employees (if at all).



Three arguments reinforce the claim: (i) sample selection allows the match only among the large firms which are relatively more protected from ROJ. A similar experiment on small-size firms would lead to an even worse performance of the movers vis-à-vis their matching stayers; (ii) if the assumption on the matching stayers having received (and declined) job offers is too optimistic, our conclusions are strengthened as the stayers' utility, however defined, would be lower than under the alternative assumption; (iii) if the ROJ elasticity were larger (in absolute value) than assumed here (- 1), the movers' position would look even worse.

8.2. ....But there are also hints of rational response following unpleasant events

Hints of rational response (fortunately) appear among the movers when the events take a downturn after the first job switch. Low wage growth and / or big employment losses at the firm level elicit attempts to search in new directions and make additional moves in the three-year window following the first switch. Likewise behave individuals who have been at (paid) work only a few number of days in recent years. This could be subjectively interpreted as a signal of forthcoming dismissal. A simple probit regression of the probability of job change suggests a robust causal link between low values of (G-w) and high ROJ in the r.h.s., and the likelihood of a new job change among the blue-collars, and even more pronounced among the white-collars. Results are not displayed, but are available on request.

9. Forecasting errors: another source of under-identification

Consider the possibility of estimating the implied U- elasticities, and/or the marginal rate of substitution between wage growth and risk-on-the-job from our empirical data. The natural specification would be (1), or something very similar to the version used to estimate the trade-off between wage growth and risk-on-the-job.

Assume a very simplified world, with two states of nature: G (good) and B (bad), each occurring with probability p and (1-p). Workers are faced with two options, c(G) and c)B), each leading to a trade-off between future earnings and job stability and yielding the following payoff matrix (identical for all individuals)

	states of	Nature
Options	G	B
c(G)	U(GG)	U(GB)
c(B)	U(BG)	U(BB)
probability	p	(1-p)

where U (GG) dominates all other entries, assumed to be known.

If p is "subjectively perceived" sufficiently big, all agents will choose c(G), and the trade-off is identified. Likewise in the opposite case.

Suppose Mr. Smith is offered a job at Fiat: excellent if the auto industry does well (state G), terrible if not (state B). Mr. Smith puts a high probability on state G, chooses c(G) accordingly, but he proves wrong as B occurs. So he is stuck at Fiat, with utility $U(GB) < U(GG)$.

If there are many agents facing choices like the above, some will predict correctly, and some wrong. Ex-post, we observe the state of nature, the choice made by each agent, and the outcome in terms of wage growth and risk-on-the-job. The ex-ante subjective probabilities are not revealed. As a consequence, if agents are unable to make correct predictions, the trade-off may not be identifiable unless the difference between utilities associated to alternative outcomes is sufficiently small (panels A and B). The same argument could be spelled out in somewhat different terms as follows.

In panels A and B, (G,G) and (G,B) denote the outcomes of the selection of c(G) when the state of nature is G or B respectively. In both cases $U(G,G) > U(G,B)$. The shaded areas are strictly dominated by (G,G) with respect to both arguments of the payoff function. In panel A,

the decision maker chooses c(G) thinking that G will prevail, but his prediction is wrong and B obtains; the outcome (G,B) will then fall in the shaded area strictly dominated by (G,G). In such case identification of, say, the marginal rate of substitution between (G-w) and ROJ is impossible. If, on the other hand, the same situation arises as in panel B, identification may be less problematic: here the position (G,B) describes the outcome of c(G) if the decision maker's forecast turns out to be wrong, but, while U(G,B) < U(G,G) still holds, (G,B) falls outside the region of strict dominance of (G,G).[24]

Problems like these will not arise in the textbook example of the choice of a bundle of goods vis-à-vis given prices in a spot market, subject to budget constraint. In this case there are no exit costs. If *ex-ante* price expectations prove wrong, it is always possible to change one's mind once prices are posted and shift to the optimal bundle. Many other real-life examples lead to problems in which the exit cost from a wrong decision may be exceedingly high, once the choice is made: (i) agents facing options between portfolio assets; (ii) people buying a house; (iii) firms or public agencies engaging in irreversible investments.

Imperfect information leads to inefficient resource allocation, unless there are appropriate insurance markets. Forecasting errors, a natural consequence of imperfect information, are the cause of serious identification problems that may affect a large variety of empirical applications.

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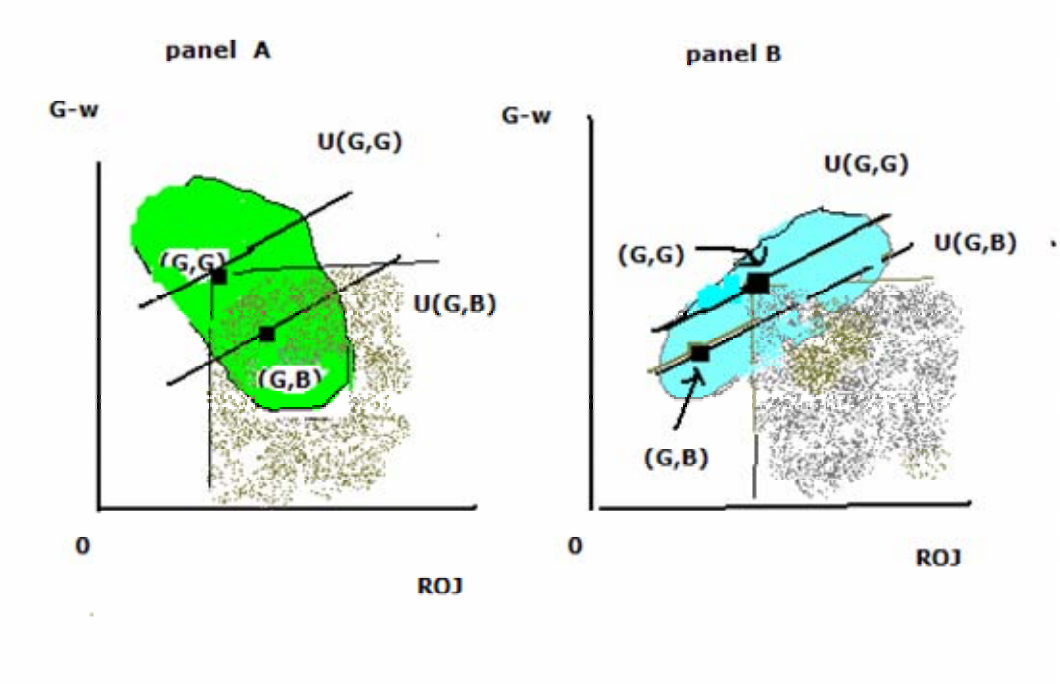
[24] In this exploration the median mover is somewhat better off than the median stayer in terms of wage growth, but in a much worse position in terms of risk-on-the-job. An outcome roughly equivalent to (G,G) is found along the 95-percentile iso-utility U, equal to 19.14 (the top 5% outcomes may be considered near-outliers): we select 186 individual outcomes included between the P94 and P96 percentiles, calculate the mean (G-w), and obtain the correspondent ROJ. The (G,G) coordinates are ROJ= 0.067, (G-w) =1.283.
Such a (G-G) - equivalent strictly dominates 65.57% of the stayers' outcomes and 52.82% of the movers'. This finding is a small step in the direction of improving identification of the movers' m.r.s. between wage growth and risk-on-the-job. It is, however, too small to insure it. Separate estimation of the <(G-w) – ROJ> tradeoff for movers and stayers yields a slightly significant slope coefficient for the former, but not for the latter. A smaller share of strictly dominated outcomes of the movers might have provided better insight.



10. Conclusion

In this paper we explore the performance of Italian workers along a decade (1986-1996) and try to assess the extent to which agents – movers and stayers – are "rational" decision makers. Under full rationality we would expect: (i) a positive tradeoff between wage growth and job safety for both movers and stayers, and (ii) a sufficiently large number of individual positions not too distant from the efficiency frontier. Both priors prove clamorously wrong. The tradeoff is (weakly) negative and only a tiny fraction of individuals are observed in the vicinity of the efficiency frontier. We conclude that workers behave according to principles of rationality that are distant from those of "full rationality" assumed in the vast majority of contemporary empirical and theoretical studies. The idea of "bounded rationality" à la Simon provides a better explanation to our observations.

If this assessment is correct, the implications are powerful: are there reasons to believe that such patterns are found only in the context of job search and worker mobility and not in other instances of economic behaviour ? Our survey of recent literature on bounded rationality strongly suggests the contrary. Why, then, should economists leave unchallenged and unchallengeable the hypothesis of full rationality ? Moreover, are we really learning the right thing (i.e. what we intend to learn) from micro-econometric investigation on structural, micro-founded, specifications if the observed outcomes are distant from the "rational" ones, or if sufficiently rational behaviour is a prerogative of less than 10% of the agents on the efficiency frontier, all the others being dominated by the former ? Had this investigation aimed at estimating the elasticities of wage growth and job

safety of the workers' utilities, we would have miserably failed. Would this be a consequence of a mis-specified model or of the wrong behavioral assumptions ? Our support undoubtedly goes to the latter.

It is our hope to have drawn attention to methodological issues that look important, and that may provide plenty of good food for future and innovative research.

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