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Does the first job really matter? State dependency in employment status in Japan

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This paper examines whether the failure to obtain regular full-time employment at the time of graduation has a long-term impact on subsequent employment status. Using micro data from the Japanese General Social Surveys and the job opening ratio (*yuko kyujin bairitu*) as an instrument for entry-level employment status, I show that the observed correlation between current and entry-level employment status produces a true causal link, which is not attributable to sorting on unobserved aptitude. I also discuss various underlying mechanisms including social institutions and stigmatization. *J. Japanese Int. Economies* **21** (3) (2007) 379–402. Department of Economics, Columbia University, 420 w118th Street, New York, NY 10027, USA.

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1. Introduction

The recent growth of young people who are neither in full-time education nor in regular employment, the so-called "*freeters*," has been drawing considerable attention as a major social phenomenon in Japan. More importantly, various economists (Mitani, 2001; Sakai and Highchi, 2005) have documented a strong correlation between the employment status at the time of graduation and employment prospects down the road. This observed correlation could in fact be causal, namely, that the employment status of the first job could have a real effect on employment prospects in the future. Nevertheless, the commonly accepted explanation for this correlation is

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that *freeters* are unable to commit to working. In economic parlance, the observed correlation is attributed to sorting by unobserved aptitude to regular employment. Despite this debate about *freeters*, the literature has not explored this important question in a rigorous way. This paper attempts to examine whether the adverse effects of failure to land a regular job at the time of graduation is spurious (due to unobserved heterogeneity) or real (causal).

This paper addresses the endogeneity of employment status by using the instrumental variable method. Specifically, I use micro data of the Japanese General Social Surveys (JGSS), which contains recall data of the employment status of one's first job as well as current employment status. As an instrument for the employment status of a first job, I use the job opening ratio (*yuko kyujin bairitsu*) in the year of completing education. This strategy is similar to Neumark (2002): since the job opening ratio is a macro index of labor demand, it is independent of heterogeneity within a cohort. I also provide evidence against direct effects of the instrument of a cohort level.

Even controlling for potential endogeneity, failure at labor market entry has a negative effect on the current probability of regular full-time employment. More precisely, people who failed to obtain a regular full-time job, due to lack of labor demand at the time of their entry, have a 40–50% less chance of having a regular full-time job at present. Given the aggregate level evidence for employers' preference for new graduates, this result is not surprising. However, most of the previous studies on the negative consequences of being a *freeter*, including Mitani (2001) and Sakai and Higuchi (2005), have treated initial employment status as exogenous. Therefore, they have been subject to the criticism that observed persistency in employment status might be due to sorting on individuals' preferences or abilities. To the best of my knowledge, this is the first paper addressing this point among the studies on *freeters* or youth employment stability in Japan.

People who failed to obtain a regular job at entry continue to miss out on investments in employment-based human capital. Thus, increasing youth joblessness may harm the productivity of the Japanese work force in the long run. At the same time, rapid population aging means that, unless Japanese society accepts a huge number of immigrants, today's young workers must be relied upon to work to their full potential to support the economy. Consequently, deteriorating youth employment emerged as a major public concern in the early 2000s.[1] Under these circumstances, it is beneficial to shed light on the prolonged unstable employment suffered by young people who stumbled at entry to the labor market.

While Japan has been known for stable employment, it is true only for regular full-time employees. There is a clear distinction between regular full-time employees, called *seishain* in Japanese, and provisional employees. Provisional employees receive less protection by law, experience with almost double turnover rates and earn lower wages and lower returns to tenure.[2] In this sense, provisional employment in Japan forms the secondary market defined by Doeringer and Piore (1971), with internal labor markets for regular employees corresponding to the primary market. When there is a barrier between the two labor market, a person's current employment

[1] For further discussion about aging workforce and youth joblessness, see Hashimoto and Higuchi (2005) and Genda (2001).

[2] There is little penalty for terminating the employment contract of fixed-term workers, while it is hard to fire regular workers because of *kaikoken ranyou houri* ("just cause" required in judicial practice); Passet (2003) provides detail. For turnover, the Survey of Employment Trend in 2002 reports that the quitting rates of full-time and part-time workers are 14.2 and 26.9%, respectively. For wages, according to the Wage Census, average wages per hour in 2002 for 20–24-year-old full-time and part-time workers were 1193 yen and 904 yen, respectively. As a rough proxy for returns to tenure, the ratio of average annual payment (including bonus) of tenure 3–4 years to that of tenure 0 year for workers aged 25–29 is 1.36 for full-time the ratio, while for part-time it is only 1.07.

status depends on her past employment status. It implies that access to the regular full-time job market is limited to new graduates and those who are already employed as regular full-time.

Despite bleak prospects for provisional employees, there is some controversy over the cause of the recent increase in number of young provisional workers. As described by Hashimoto and Higuchi (2005), when the *freeter* phenomenon emerged in the 1980s, it was perceived that some youths shunned the life of full time jobs even though they were available. Although the *freeter* phenomenon was increasingly regarded as the result of demand deficiency in the prolonged recession of the late 1990s, there remains a persistent view which attributes the problem to a lack of employability of young people.[3] Genda (2001) casts doubt on such a view and argues that, under the condition of economic slump and aging workforce, employment protection of prime-aged men is putting a strain on young people. The empirical results of this paper supports the argument that the demand condition is an important cause; if the *freeters* were to voluntarily avoid regular employment or they were not qualified to find a full-time job from the beginning, no significant effect of failure at entry would be identified with controls for unobserved heterogeneity.

A related issue is the persistent negative effect of a recession at the time of labor market entry in Japan. There is evidence that leaving school during a recession lowers a cohort's wage (Ohtake and Inoki, 1997), and causes a persistent increase in the probability of quitting (Genda and Kurosawa, 2001 and Ohta, 1998).[4] This paper contributes to the literature on cohort effects in the sense that it shows an indirect effect of a recession at entry on future employment status.

The plan of the paper is as follows. Section 2 discusses possible reasons for persistent negative effects of failure to obtain a regular full-time job at entry. Section 3 outlines the empirical model, clarifying what simple probit and IV methods identify. Details of the data are described in Section 4. I report my findings in Section 5: I begin with the pooled regression in Section 5.1. Section 5.2 examines the exclusion restriction of the instrument, and Section 5.3 conducts further analyses by gender and educational background. Section 6 concludes.

2. Why failure at entry has a persistent effect

Why does failure to obtain a regular full-time job at the time of graduation have a persistent effect on the subsequent likelihood of regular full-time employment? Before answering this question, let me clarify what the term "effect" refers to. I use the term to refer to the difference in likelihood of regular full-time employment between those who obtained a regular full-time job at entry and those who did not, conditional on any characteristics determined by the time of graduation. To put it another way, an effect of failure at entry for a person is the reduction of the likelihood of having a regular full-time job at present if she had not obtained a regular full-time job at entry than if she had.

Thus, the "effect" of failure at entry includes the decline in productivity due to graduating without obtaining a regular full-time job, as well as differential treatment by a potential employer given the same productivity. On the other hand, correlation between the first and the current jobs is spurious when it is due to unmeasured variables that influence both. Such unmeasured vari-

[3] See, for example, Ministry of Labor (2000).

[4] On the other hand, studies on the United States mainly focus on effects of the initial condition of each job tenure or the best condition each cohort has experienced. For example, Beaudry and DiNardo (1991) show that current wages are affected more by the minimum unemployment rate that the cohort has experienced than by the current or initial unemployment rate. For mobility, Bowlus (1995) concludes that slack labor market at the beginning of job tenure persistently increases probability of quitting. Also, effects of within-firm cohort are not negligible in Japan, either (Ariga et al., 2000).

ables must be determined by the time of graduation in order to influence the first job. Therefore, the spurious correlation due to unobserved heterogeneity will disappear if all individual characteristics determined by the time of graduation are controlled for.

A negative shock at entry produces a persistent effect because of several interrelated factors. As evidenced by the fact that typical Japanese firms view the annual hiring of new graduates as their primary means of recruitment, social institutions such as employment practices and the school-to-work transition system are favorable to new graduates. Such social institutions not only make it troublesome to join the recruiting process without being enrolled in a school but also may stigmatize people whose first job was not regular full-time. That is, leaving school without obtaining a regular full-time job works as a bad signal because obtaining a regular full-time job right after graduation is taken for granted. At the same time, this signaling effect reinforces the social institutions. Lack of training opportunities and demoralization may harm the expected productivity of people who failed to obtain a regular full-time job at entry and exacerbate stigmatization.

Japan's employment system has been characterized by the prospect of long-term employment with rising tenure-earnings profile. For example, Hashimoto and Raisian (1985) established that long-term employment is more prevalent and tenure-earnings profiles are more steeply sloped in Japan than in the United States. The practice of long-term employment in Japan implicitly assumes that the main source of recruitment is new graduates and that they start out as full time workers. Although Ariga et al. (2000) find that sizable hirings occur among employees with previous job experience, such mid-career recruitment is mainly to fill vacancies at higher ranks of the job ladder and irrelevant to those lacking sufficient experience. In fact, Japanese firms show strong preference for workers with full-time employment experience in the mid-career recruitment.[5]

Also, schools play a key role in the matching process between new graduates and firms.[6] For high school graduates, it had been common to find jobs through implicit contracts between high schools and firms (*jisseki kankei*), at least until the middle of 1990s. For college graduates, a typical recruiting process starts more than a year prior to graduation, often exclusively targets senior students in colleges, and uses the ranking of universities to screen applicants to interview. In either case, schools have an incentive to make good efforts to support their students because job market performance influences the schools' reputation and quality of incoming students. Since the high school enrollment rate is about 96%, most young Japanese go through either of the two processes.[7]

As a consequence of the well-defined school-to-work transition system, the proportion of new graduates who immediately obtain a full-time regular job is as high as 80–90%.[8] As Ryan (2001) points out, on one hand Japan's school-based recruitment system with high enrollment rates has contributed to relatively stable youth employment in international standards. On the other hand, recessions in the year of graduation may "leave the casualties stranded" (p. 50) even after recruiting for new graduates recovers, because it is hard to rejoin the recruiting process without

[5] The Survey on Employment Management in 2004 reports that only 11.8% of the firms with more than 30 employees hired *freeters* as full-time employee in 2004, while as many as 71.2% recruit mid-career full-time workers.

[6] For details, see Mitani (1999) and Rebick (1998).

[7] 18% (in my data) of high school graduates go to vocational schools called *senshu gakkou*, typically for 1 or 2 years. Some vocational school graduates find jobs through their schools like high school graduates, and others go into similar process to college graduates, depending on type of occupation.

[8] See, for example, Table 1 of this paper.

being in school. The Survey on Employment Trends provides evidence for the advantage of new graduates: 68% of the entrants from non-employment (including temporary unemployed with full-time experiences) to full-time jobs younger than 30 in 2002 were new graduates.[9]

Due to the difficulty in finding a new full-time regular job after leaving school, it takes a long time to catch up through a job search process. Moreover, a person who voluntarily quit a full-time job is not likely to accept a part-time job. However, if all job quitters faced the same available job opportunities, the effect of initial employment status should significantly decline over time, given that 30% of college graduates and 50% of high school graduates quit their first jobs within three years.[10] The empirical results of this paper show much stronger persistence.

A potential reason for the persistence is that whether an applicant's first job after graduation was regular full-time works as a signal of her ability. As Arrow (1973) discussed in the context of statistical discrimination, potential employers believe such signal when the costs to screen the applicant's true aptitude is expensive, and the status of her first job conveys some information about her ability. Obviously, careful examination of the applicant's ability incurs substantial costs. Also, it is natural to think that those who failed to obtain a regular full-time job at entry are adversely selected under the social norm that everyone should find a regular job right after graduation. Then, the employers may not want to pay screening costs to hire those whose expected ability is known to be low.

Since promotion from provisional to regular jobs within a firm is rare in Japan,[11] employers seldom exploit information possibly acquired through on-going employment. This is the key difference from models of wage losses based on adverse selection by the current employer (e.g. Greenwald, 1986 and Gibbins and Katz, 1991). Stigmatization by past employment status can persist for years because potential employers cannot acquire any additional information without hiring them.

It is very important to distinguish between the negative signal due to the lower ability of people who failed at entry *on average* and actual lower ability of *each* person who failed at entry. The high proportion of new graduates who immediately obtain a full-time regular job implies that people with high aptitude could find a regular full-time job even in deep recessions. Then, there may be no difference in aptitude across those dependent on business cycle conditions. Nevertheless, once a worker with relatively low ability obtains a regular full-time job thanks to an economic upturn, potential employers cannot distinguish her from more able workers. Therefore, it can affect her current likelihood of full-time regular employment conditional on her ability.

In other words, whether one was lucky to find a regular full-time job still has an effect after controlling for unobserved heterogeneity in aptitude. This is exactly what the instrumental variable method identifies. To the contrary, obtaining a regular full-time job just by chance would have no effect if the difference in subsequent likelihood of regular full-time employment between people who obtained a regular full-time job at entry and those who did not were completely attributed to the difference in their aptitude.

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[9] Since "full-time" includes provisional jobs with no less working hours than regular the actual ratio of new school leavers in inexperienced entrants to regular full-time jobs could be even higher.

[10] Ministry of Labor (2000).

[11] Although there is no reliable data for the actual number of promotions from part-time/provisional to regular full-time within a firm, proportion of firms with such a promotion systems is only 26.7%, according to the Survey on Employment Management in 2004. Also, gathering from Sato (2004), at least 25% of the firms with such promotion system have not made use of them for more than three years. Note that the government started to encourage promotion/hiring from provisional workers to regular full-time in the early 2000s; therefore, it is likely to have been even more adverse to provisional workers in the 1990s.

A person who experienced full-time employment may also have advantages in that she has already acquired skills thorough on the job training. However, the results of previous studies cast doubt on the quantitative importance of loss of human capital accumulation. While accumulated human capital should be affected by the duration of not having a regular job rather than whether the first job was regular, Mitani (2001) shows that the length of post-school years before getting one's first regular job has no significant effect on the current probability of involuntary non-regular employment. Yet, the perception that lack of experience reduces the average productivity of those who failed to obtain regular full-time employment at entry may exacerbate the stigmatization.

It is also possible that failure at entry discourages job search efforts. An important distinction from the prevailing argument, which states that lack of motivation causes failure at entry, is that failure at entry decreases motivation. Such discouraging effects must be, at least partly, attributed to the fact that it is hard for those who failed at entry to obtain a regular job later. Although it is difficult to assess psychological impact on economic consequences, morale may play an important role. This is also included in the effect identified by the instrumental variable method.

3. Empirical model

Let y_i and x_i be binary indices for the employment status of current job and first job after completing education: 1 if the employment status is regular full-time and 0 otherwise. Consider following binary choice model:

$$y_i = 1(y_i^* = \alpha x_i + \beta' Z_i + \epsilon_i > 0)$$

where Z_i is a vector of covariates and ϵ_i is the error term with mean zero. I assume $\epsilon \sim N(0,1)$ to apply the probit specification.

If ϵ_i and x_i are independent, the probit estimate of α is consistent; however, x_i may positively correlate with ϵ_i because of unobservable characteristics that affect both the current and the past employment status. That is, there may be something determined before graduation that makes one more likely to be a regular employee than another. Then, the probit estimate of α, which merely reflects partial correlation between y_i and x_i, may be overestimated and does not imply a causal effect of x_i on y_i.

Due to this concern of omitted variable bias, I instrument for x_i with v_i, a macro index of labor market condition, following a similar idea to Neumark (2002). Specifically, I use the job opening ratio of the local prefecture in the year of completing education. A valid instrument must correlate with x_i and be independent from ϵ_i. Intuitively, labor market conditions should affect the probability of a new graduate getting a regular job, and it is easy to check its statistical relevance. Also, a prefecture-level aggregate index has no correlation with individual characteristics of people in the same cohort and prefecture. Therefore, the only concern is that the job opening ratio may correlate with the average unobservable aptitude of each local cohort. Section 5.2 provides evidence against such cohort and prefecture effects.

Specifically, consider the following simultaneous probability model:

$$y_i = 1(y_i^* = \alpha x_i + \beta' Z_i + \epsilon_i > 0), \tag{1}$$

$$x_i = 1(x_i^* = \gamma v_i + \delta' Z_i + \eta_i > 0). \tag{2}$$

The two error components are allowed to correlate: $corr(\epsilon_i, \eta_i \mid Z_i, v_i) = \rho$, while $E(\epsilon_i \mid v_i, Z_i) = E(\eta_i \mid v_i, Z_i) = 0$. If the effect of unobservable inherent characteristics is not negligible, ρ is positive.

In implementation, I primarily use the bivariate probit model assuming joint normality of ϵ_i and η_i. When the joint normality holds, the bivariate probit gives consistent and efficient estimates; however, its consistency hinges on the normality.[12] Thus I also report the linear two stage least squares estimator for the main regression to confirm robustness.

The IV estimator does not reflect unobservable time-invariant characteristics because what is identified by the instrument is how much $\Pr(y_i = 1)$ would change when an increase of v_i raised $\Pr(x_i = 1)$ by a unit. If the correlation between the current and the past employment status comes entirely from time-invariant unobservable characteristics, labor market conditions at the entry will have no effect on current employment status. Therefore, the IV estimator genuinely represents the causal effect of the entry-level employment status.

When α is heterogeneous, the IV may not give the population average of α_i. According to Imbens and Angrist (1994), the linear two stage least squares method identifies a weighted average of treatment effects (α_i) putting higher weights on observations more sensitive to the instruments (i.e. people with high γ_i). It is natural to think that the bivariate probit also puts higher weights on those more sensitive to the instrument. Then, in the present case, the identified causal effect is that for people who are likely to be affected by the aggregate labor market condition.

In other words, the IV method measures the state dependence of people involuntarily unemployed or under-employed at their entry due to a recession. Therefore, even when the IV estimate of α is no smaller than the simple regression ignoring endogeneity, it does not necessarily mean that there is no difference in unobservable aptitudes between those who failed to obtain a regular full-time job at entry and the others. In the phrase of Angrist et al. (1996), the IV estimators do not take into account the "never takers" and "always takers," people who are never influenced by the instrument. Thus, it is still possible that some people are always hired as regular full-time, regardless of the labor market conditions, and actually have high aptitude. For all that, a significantly positive α means that at least those who could not obtain a regular job at entry due to a recession are adversely treated because of their past employment status.

4. Data

My main source of data is the Japanese General Social Surveys (JGSS), a Japanese version of the GSS project conducted by Osaka University of Commerce and the Institute of Social Science at the University of Tokyo, closely replicating the original GSS of NORC at the University of Chicago. I use its micro data pooling surveys in 2000, 2001, 2002 and the second pilot survey in 1999.

Although the JGSS is cross sectional and not a panel data, it contains questions about a respondent's first job after completing education.[13] I exploit this recall data. The original questions break employment status into 13 categories. I summarized those categories as "regular full-time employment" and "others" as follows. Regular full-time employment: executive, six options of regular employment corresponding to different positions on job ladders. Others: part-time, tem-

[12] For the proof of consistency of the maximum likelihood estimates of this system, see Heckman (1978).

[13] Although "one's first job after completing school was regular-fulltime" is not completely equivalent to "one got regular full-time job immediately after graduation," according to 2001 and 2002 surveys which asked whether the respondent got job immediately after graduation, 92% of the respondents whose first job was regular full-time answered they got their first job immediately after completing school. Thus, whether one's first job was regular roughly corresponds to whether one got regular full-time job upon graduation.

porary, self-employment, family-employment, by-work, not employed (for the first job, never employed).[14]

As covariates, I use education, sex, current marital status and years since graduation. Education is expressed as dummy variables corresponding to:

(1) junior high school graduates or high school dropouts,
(2) high school graduates (control group),
(3) junior or technical college (2-year) graduates,
(4) 4-year college graduates or graduate schools,
(5) college dropouts.

Years since graduation (*survey year* − *year of graduation*) are included to control for potential experience that would be accumulated if the respondent had kept working since graduation. Year of graduation is computed from the year of birth and education: *year of birth* + 7 + *years of schooling*; where schooling is 9 years for junior high, 12 for high school, 14 for junior college, 16 for 4-year college and university, and 19 for graduate school. All regressions include survey year dummies to control for current aggregate labor market conditions. I use observations who graduated after 1986, dropping those within 4 years since graduation to prevent α from reflecting temporary inertia of employment status. Observations with missing variables are also dropped.

As the macro index of labor market condition at the entry, I use the active job opening-to-application ratio (*yuko kyujin bairitsu*) in prefecture level taken from the report of the Employment Service Agency (*shokugyou antei gyoumu toukei*). I matched each individual in the JGSS with the job opening ratio of the prefecture of current residence[15] in the year prior to graduation (*year of birth* + 6 + *schooling*). Then, I take the average job opening ratio of subsamples divided by prefecture, subtract it from the raw ratio, and use the residual as an instrument for the first job. In other words, the instrument for the first job is variation in labor demand across years net of prefecture fixed component. Hereafter this variable is referred to as the "normalized local job opening ratio."

The range of the years of entry overlaps that of the Survey of Young Employees (*Jakunensha Shugyo Jittai Chosa*), the main source of data used by previous studies including Mitani (2001) and Genda and Kurosawa (2001).[16] An important distinction of the JGSS from the Survey of

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[14] I treat self-employment and family-employment as non-regular employment together with provisional employees and people not employed. Although self-employment is not usually regarded as a part of *freeters*, I do not drop the self and family employed observations because there are considerable inflows from regular employment relative to the total number of self or family employment: Table A.1 shows more than half of current self/family employed people were regular employees at their entry (both men and women). Some readers may suspect that succession of family business accounts for a considerable part of estimated state dependence. It is true that more than half of people who were initially self or family employed remain in self or family employment. Nevertheless, such people are only 2.1% of the entire sample; therefore, I believe the overall effect of family business is negligible.

[15] Ideally I should use the prefecture where the respondent lived in the year of graduation, but such data is not available. JGSS also asks residence when the respondent was 15 years old, and 80% of the sample answered the same prefecture as current residence. Moreover, the proportion moving is much higher for college graduates, indicating that people tend to move to go to college and obtain jobs there. Therefore, I believe current residence is a good approximation to residence in the year of graduation.

[16] The Survey was conducted in 1997 targeting workers aged 15–29; thus strictly speaking, the oldest part of university and college graduates (176 obs.) in my data are not covered in the Survey because they were older than 30 in 1997.

Young Employees is that the latter covers only current employees (both regular and provisional) of private firms with more than 5 employees. The JGSS covers the entire population, including people currently not employed, so it is free from sample selection of labor force participation.

5. Findings

5.1. Effects of failure at entry

Table 1 presents summary statistics. In the whole sample, the proportion whose first job was regular full-time is as high as 87.1%. The ratio of current regular full-time employees is lower, reflecting outflow from regular employment due to the weak attachment to the labor force of married women. Tenure is shorter than potential experience even for those who have a regular full-time job at present. "Lifetime employment" is no longer an apt description for the majority of Japanese youth who entered the labor market after 1986; even among those whose first and current jobs are both regular full-time, only 43% have stayed with the same employer since graduation.[17]

Table 1
Summary statistics

	Whole sample		First job was regular full-time ($x_i = 1$)		First jobs was not regular full-time ($x_i = 0$)	
Total number of observations	1406	(100%)	1225	(100%)	181	(100%)
Current employment status						
regular full-time ($y_i = 1$)	818	(58.2%)	774	(63.2%)	44	(24.3%)
part-time/provisional employee or self/family employment	272	(19.3%)	191	(15.6%)	81	(44.8%)
not working	316	(22.5%)	260	(21.2%)	56	(30.9%)
Employment status of first job regular full-time ($x_i = 1$)	1225	(87.1%)				
Education						
Jr. high grads/high sch. dropouts	66	(4.7%)	38	(3.1%)	28	(15.5%)
High school graduates	654	(46.5%)	569	(46.4%)	85	(47.0%)
Jr. and tech college graduates	299	(21.3%)	273	(22.3%)	26	(14.4%)
4-year college or grad school	371	(26.4%)	335	(27.3%)	36	(19.9%)
college dropouts	16	(1.1%)	10	(0.8%)	6	(3.3%)
Currently married	822	(58.5%)	736	(60.1%)	86	(47.5%)
Female	740	(52.6%)	638	(52.1%)	102	(56.4%)
Years since graduation (average)	10.8		10.4		10.9	
Tenure with current employer (if employed)	6.1		6.1		5.5	
(if currently regular full-time)	6.9		7.0		5.4	
Age (average)	30.0		30.1		29.1	
1999 survey respondent	88	(6.3%)	74	(6.0%)	14	(7.7%)
2000 survey respondent	422	(30.0%)	362	(29.6%)	60	(33.1%)
2001 survey respondent	412	(29.3%)	365	(29.8%)	47	(26.0%)
2002 survey respondent	484	(34.4%)	424	(34.6%)	60	(33.1%)
Normalized local job opening ratio in the year of entry* (standard deviation)	0.00	(0.52)	0.02	(0.53)	−0.11	(0.41)

* Job opening ratio of current residence prefecture in the year of graduation less average job opening ratio of the prefecture over 1985–1997; to be used as the instrument for the first job.

[17] Calculated as the proportion with (*years since entry-tenure*) $\geqslant 2$. I regard those with (*years since entry-tenure*) $= 1$ as stayers due to a fear of errors in the year of graduation. Thus, the true fraction of stayers can be even smaller.

Nevertheless, subsamples divided by employment status of their first job show strong persistence in employment status. The ratio of current regular full-time employees is much higher for those whose first job was regular full-time: overall, 63% of those initially employed as regular full-time remain regular full-time at present, while only 24% of those initially not employed as regular full-time have moved to regular full-time employment. At the same time, the difference in education stands out. Especially, the proportion of junior high-school graduates and high-school dropouts in those whose first job was not a regular full-time one is extremely high. This discrepancy brings concerns about differences in unobservable ability correlated with employment status.[18]

To summarize, employment status is actually persistent, while differences in education caution about omitted variable bias in simple regressions. Taking these into account, recall the simultaneous probability model:

$$
\begin{aligned}
y_i &= 1(y_i^* = \alpha x_i + \beta' Z_i + \epsilon_i > 0), \\
x_i &= 1(x_i^* = \gamma v_i + \delta' Z_i + \eta_i > 0)
\end{aligned}
$$

where y_i and x_i are binary indices for employment status of current job and first job: 1 if regular full-time and 0 otherwise, and v_i is the normalized local job opening ratio in the last year of enrollment, the instrument for x_i.

Figure 1 confirms relevance of the first stage. I define a cohort as a group of people who graduated in the same year, and plot the fraction of those with $x = 1$ of each cohort over the year of graduation. The gray line is the average of the normalized job opening ratio of each cohort. The proportion of those whose first job was regular full-time is actually correlated with the instrument. The dashed line is the prediction by probit regression of Eq. (2); it follows the actual trend fairly well.

Table 2 reports basic results. The effect of the first job is large and significant; a person who obtained a regular full-time job at entry has about 50% more chance to be regular full-time at present. Point estimates of simple probit and IV are close, and the Hausman test does not reject the hypothesis that all coefficients in column (2) are equal to those in column (1). Also, the likelihood ratio test does not reject H_0: $\rho = 0$. That is, the real effect of failure at entry is as large as the observed correlation between first and current jobs. Failure to get a full-time job at graduation actually has a significant causal effect on current employment status.

The normalized job opening ratio is statistically significant at the 1% level in the first stage; the t-statistic is 3.6. The coefficient of the normalized job opening ratio is positive but smaller in the reduced form, as expected. More educated people are more likely to have a regular full-time job both at entry and at present. Married women are much less likely to have a regular full-time job at present.[19] The years since graduation have a negligible effect.[20] Moreover, even dropping the years since graduation makes no difference (Table A.2). The last two columns report the linear 2SLS model as a robustness check; the coefficients of x_i and v_i in linear IV estimates are fairly close to the marginal effects in bivariate probit estimates.

[18] Cross tabulations by gender (Table A.1) confirm that people who obtained a regular full-time job at entry are twice as likely to be regular full-time now as the others for both males and females, while the flow to non-employment is much greater for females.

[19] A significant positive effect of current marital status in the first stage may appear odd. I show this is not a crucial problem in Section 5.3.

[20] I tried various specifications (quadratic, cubic, interacted with gender) and confirmed they do not make any difference.

Table 2
Effect of obtaining a regular full-time upon graduation on likelihood of regular full-time employment at present

	Probit IV				Linear IV	
	Simple probit (1)	Structural form (2)	First stage (3)	Reduced form (4)	Structural form (5)	First stage (6)
Dependent variable:	y_i	y_i	x_i	y_i	y_i	x_i
Estimated coefficients						
First job was regular fulltime (x_i)	1.330	1.490			0.514	
	[0.126]***	[0.784]*			[0.336]	
Normalized local job opening ratio in the year of entry (v_i)			0.370	0.111		0.058
			[0.103]***	[0.077]		[0.017]***
Years since graduation	−0.003	−0.003	0.013	0.001	−0.002	0.003
	[0.013]	[0.013]	[0.013]	[0.012]	[0.003]	[0.003]
Jr. high grads/high school dropouts	−0.402	−0.354	−1.040	−0.665	−0.031	−0.310
	[0.203]**	[0.313]	[0.172]***	[0.186]***	[0.114]	[0.042]***
Jr. & tech college graduates	0.068	0.059	0.303	0.137	0.009	0.057
	[0.106]	[0.115]	[0.126]**	[0.103]	[0.033]	[0.024]**
4-year college or graduate schools	0.214	0.210	0.156	0.248	0.048	0.025
	[0.106]**	[0.108]*	[0.115]	[0.101]**	[0.026]*	[0.022]
College dropouts	−0.286	−0.244	−0.906	−0.626	−0.043	−0.258
	[0.381]	[0.434]	[0.330]***	[0.351]*	[0.128]	[0.082]***
Female	−0.163	−0.155	−0.219	−0.196	−0.039	−0.049
	[0.121]	[0.127]	[0.133]*	[0.114]*	[0.037]	[0.028]*
Currently married	0.661	0.651	0.245	0.675	0.140	0.043
	[0.132]***	[0.142]***	[0.136]*	[0.125]***	[0.035]***	[0.026]*
Female* married	−2.174	−2.170	−0.020	−2.045	−0.651	0.002
	[0.170]***	[0.173]***	[0.181]	[0.161]***	[0.042]***	[0.036]
Marginal effect of x_i^{*1} or v_i^{*2}	0.48	0.51	0.07	0.04		
	[0.05]***	[0.27]*	[0.02]***	[0.03]		
ρ		−0.089				
		[0.434]				
LR chi^2/Wald chi^2/F	681.93	565.05		561.41	71.72	8.58
(Pseudo) R^2	0.36			0.29	0.41	0.07

$y_i = 1$: current job is regular fulltime
$x_i = 1$: first job was regular fulltime
Hausman test
H$_0$: Coefficients in the column (1) = those in column (2).
chi^2(12) = 1.03, prob > chi^2 = 1.0000.
H$_0$ is not rejected.
Notes. (1) $\Pr(y = 1 \mid x = 1) - \Pr(y = 1 \mid x = 0)$ evaluated at mean of other covariates.
(2) $\mathrm{d}\Pr(x = 1)/\mathrm{d}v$ evaluated at mean of other covariates.
(3) Standard errors are in brackets. All regressions include survey year dummies and constant. Number of observations = 1406.
* Significant in 10%.
** Significant in 5%.
*** Significant in 1%.

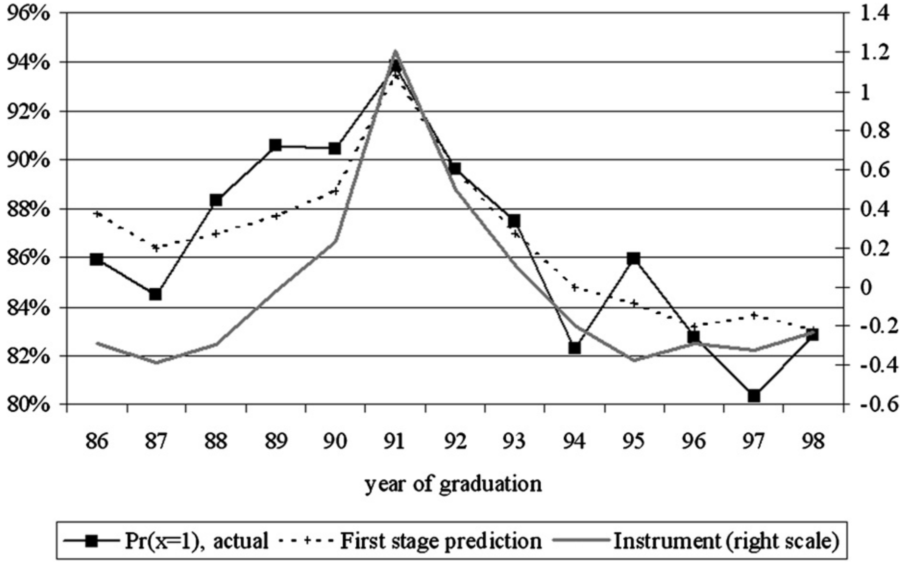

Fig. 1. Fraction of obtaining a regular full-time job upon graduation, by year of graduation.

It may appear puzzling that the IV estimate of α is slightly larger than the simple probit estimate despite the expected negative ability bias. It could be due to larger errors in the IV estimators, since the IV estimates are less efficient in general. In fact, the estimated standard errors of the coefficients in column (2) are much larger than those in (1). The difference in α may merely come from such amplified errors, and the Hausman test shows that the difference between the two estimates of α is statistically negligible.

Also, the IV estimates become larger if the effect of failure at entry is larger for those who are more sensitive to the aggregate labor market condition. Imbens and Angrist (1994) have shown this point formally: an instrumental variable estimate is a weighted average of local average treatment effects (LATE) putting higher weights on individuals more sensitive to the variation of the instrument. To understand it intuitively, recall the probability limit of linear IV estimator of α:

$$plim\,\hat{\alpha}^{IV} = \frac{E(\alpha_i\gamma_i)}{E(\gamma_i)}.$$

This is equal to $E(\alpha_i)$ if and only if $E(\alpha_i\gamma_i) = E(\alpha_i)E(\gamma_i)$, and when $corr(\alpha_i, \gamma_i) > 0$, $plim\,\hat{\alpha}^{IV} > E(\alpha_i)$. On the other hand, the simple probit estimator puts higher weight on observations around median.

Therefore, a naive comparison between the simple probit estimates and the IV estimates may be misleading. The average unobserved aptitude can be lower for those whose first job was not a regular full-time one than the others, although IV estimate is no smaller than simple probit. Then, IV estimates may be upwardly biased in the sense that they may be greater than the population average. Nevertheless, the main question of this paper—whether one obtained a regular full-time job upon graduation matters to one's current employment status—is valid as long as the labor market condition at entry does not affect the current employment status of people who obtained regular full-time employment at the time of graduation. At least, people who failed to obtain a regular job at entry due to lack of labor demand suffer from state dependency in employment status.

Furthermore, Table 3 shows that adding controls for current residence, family background or characteristics of the first job does not change the results. First, it is important to check robustness to adding such variables because I use the job opening ratio of each prefecture as the instrument. Column (2) includes an indicator of living in a large city and dummy variables corresponding to 7 regions; they are jointly insignificant. Second, the specification shown in column (3) examines whether parents' education or their employment status affect the result. They are jointly insignificant, and none of them becomes significant when added independently. Interestingly, the dummy of cohabitation with parents is also insignificant despite the prevailing notion of the '*parasite single*,' young people who live on their parents.[21] The last column shows that controlling for industry and firm size of first job does not weaken the effect of initial employment status much, although industry and firm size of the first job also have significant effects on current employment status.[22]

Table 3
Effect of obtaining a regular full-time job at entry on likelihood of full-time regular employment at present: with various control variables

	(1)	(2)	(3)	(4)
Probit (without IV)				
Marginal effect of x	0.472	0.465	0.488	0.408
	[0.045]***	[0.046]***	[0.056]***	[0.064]***
Significance of added covariates		6.31	4.59	74.71
Chi^2 test statistics (p-value)		(0.39)	(0.47)	(0.00)
Probit IV				
Marginal effect of x	0.512	0.569	0.500	
	[0.269]*	[0.311]**	[0.252]*	
Significance of added covariates		3.41	3.89	
Chi^2 test statistics (p-value)		(0.76)	(0.57)	
Observations	1406	1406	1015* (2)	1192* (3)

Added control variables:

(1) None, the same as Table 2.

(2) Dummy for current residence; metropolitan dummy (1 if living in 13 largest cities) and dummies for 7 regions.

(3) Family background dummies: father went to college, mother went to college, father was self-employed, mother was a regular employee and living with parent(s).

(4) Dummies for 8 industries and 3 firm sizes of first job.

Notes. (1) Marginal effects are $\Pr(y = 1 \mid x = 1) - \Pr(y = 1 \mid x = 0)$ calculated using mean value of other covariates.

(2) Number of observations is smaller due to missing family background data.

(3) Number of observations is smaller because sample is restricted to whose first job was regular or provisional (i.e. self/family employed and never worked are excluded). (4) Standard errors are in brackets. All regressions include the set of covariates used in Table 4.

* Significant in 10%.

** Significant in 5%.

*** Significant in 1%.

[21] '*Parasite single*' is a buzz word made in Japan, like '*freeter*.' For more detail, see a critical review by Genda (2001).

[22] Since there is no information on the first job for those who never worked and no firm size for self/family employed, sample is restricted to those whose first job was regular full-time or provisional. I believe this sample restriction is not critical for simple probit estimates because coefficients of other covariates are similar; however, it surely causes bias on IV estimates. Thus I report only simple probit without IV.

I also tried to include interaction of the first job and years since graduation in the regressions to see how persistent the effect of failure is. However, interacting experience and the first job makes the instrument too weak due to correlation between years since entry and the instrument. Since the IV model does not work, I estimated only the probit model without IV and found that the coefficient of the interaction term is almost zero. Admitting that only suggestive evidence is available, the effect of the first job seems to be highly persistent.

5.2. Exogeneity of the instrument

To be a valid instrument, the normalized local job opening ratio at entry must not correlate with the error term in the structural form, i.e. $E(\epsilon_i \mid v_i, Z_i) = 0$ must hold. It means that the labor market condition in the year of leaving school cannot have a direct effect on current employment status other than through the initial employment status. There is no correlation between the instrument and individual characteristics of people in the same cohort and prefecture because everyone who graduated in the same year and lives in the same prefecture has the same value. Thus, the greatest concern is potential correlation with unobserved components specific to the year of graduation or prefecture.

Assessing the correlation with cohort specific components is especially important because existing studies establish cohort effects on wages (Ohtake and Inoki, 1997) and mobility (Genda and Kurosawa, 2001; Ohta, 1998), which may affect current employment status. Although the ideal way to control for the cohort effect is inclusion of graduation-year fixed effects, adding dummy variables of graduation-year absorbs the explanatory power of the instrument and makes the first stage too weak. Therefore, instead, I include cohort level indices to control for effects of average wages and mobility. In addition, I control for the college enrollment rate because high school graduates may avoid entering the labor market during recessions by going to colleges. In response to the concerns of other trend changes such as downward trend in employability, quadratic trend terms are also added.

Specifically, column (2) of Table 4 includes the following variables: enrollment rate of colleges (including junior and technical colleges)[23] matched with individual observations using the year of birth; monthly wage of standard workers with the same sex, education and age in the survey year; quitting rate of age 20–29 three years after the entry[24]; and quadratic trend terms. Although including the quadratic trend reduces the explanatory power of the instrument, the difference in the point estimate of α from that in column (1) is negligible, and added cohort variables are all statistically insignificant at the 10% level.[25]

The prefecture specific components are thought to be less problematic because the average job opening ratio of each prefecture is already subtracted. The insignificance of the region dummies in column (2) of Table 3 also suggests that correlation between the instrument and the average characteristics of each prefecture is negligible. To confirm this, column (3) of Table 4 reports estimates using the aggregate job opening ratio as an alternative instrument.[26] Estimated α is almost the same as that in column (1).

[23] Using enrollment rate of 4-year college (excluding junior and technical colleges) makes little difference.

[24] Construction industry is excluded to assure connectivity in time series.

[25] None of them becomes significant when added independently.

[26] Using the unemployment rate for 15–24 years old yields similar results.

Table 4
Checks for cohort and prefecture level effects
Probit IV

	(1)		(2)		(3)	
	Structural form	First stage	Structural form	First stage	Structural form	First stage
First job was regular fulltime (x_i)	1.490 [0.784]*		1.385 [0.939]		1.479 [0.749]**	
Normalized local job opening ratio in the year of entry (v_i)		0.370 [0.103]***		0.315 [0.122]**		
Enrollment rate of colleges			0.028 [0.031]	0.020 [0.030]		
Current Monthly Wages of Standard Workers			0.000 [0.003]	−0.004 [0.003]		
Quitting rate in three years from entry			−4.912 [4.434]	6.082 [5.004]		
Year of Graduation (Linear Trend)			0.111 [0.097]	0.162 [0.095]*		
Squared Trend (Year of graduation 1985)^2			−0.004 [0.006]	−0.010 [0.006]*		
Aggregate (national level) job opening ratio in the year of entry						0.656 [0.166]***

Sources:

Enrollment rate of colleges: The Statistical Abstract of School Education.

Current monthly wages of standard workers: The Wage Census.

Quitting rate in three years from entry: calculated from The Survey of Employment Trend.

Notes.

Other covariates (potential experience, education, marital status, sex and survey year dummies) are included. Standard errors are in brackets.

* Significant in 10%.

** Significant in 5%.

*** Significant in 1%.

If the negative effect of failure at entry comes from stigmatization, the initial labor market condition may affect the magnitude of this effect because the more people fail to obtain full-time employment at graduation the better their average aptitude will be. Consequently, people who entered the labor market during booms might be considered to be less able and have more difficulty in obtaining a regular full-time job. If this were the case, the instrument would no longer be valid. Comparing estimated α of different cohorts is one way of assessing this concern; however, breaking the sample by cohort makes the first stage too weak. Thus, I estimate them without controlling for endogeneity and checked correlation with the instrument. I find no systematic relationship between α and v. I also include dummy variables of year of graduation; they are jointly insignificant. This is additional evidence against cohort effects on employment status aside from employment status of the first job. The estimated α by cohort and the graduation-year fixed effects are reported in Table A.3.

There is no trend change in α either. This is an interesting contrast to the perception that *freeters* in the 1980s are voluntary and could have found full-time jobs if they had wanted, while those in the late 1990s are more like involuntary ones who could not find full-time jobs. For example, Hashimoto and Higuchi (2005) view that "being a *freeter* was at first a voluntary, supply-side phenomenon, a counter-cultural expression, as it were, and not a demand-deficiency phenomenon" (p. 352). Then, the true effect of failure at entry would be stronger for the younger cohort. It is not likely to be the case, although available estimates are subject to potential endogeneity of the first job. Figure 1 also cast doubts on the view that emergence of *freeters* in the 1980s were a pure supply-side phenomenon because the fraction of regular full-time was procyclical in the 1980s as well as in the 1990s. I also tried to estimate the effect of v_i on x_i separately for those who graduates before 1991 and after 1992, and found no significant difference in the two estimates.[27]

5.3. Further analyses by gender and educational background

Coefficients of other explanatory variables in Table 2 show that people with more education are more likely to be regular full-time and married women are much less likely to be regular full-time employees. Thus, aside from employment status of the first job, what mainly affects current employment status is gender and education. Therefore, analyses by gender and educational back ground would be informative, although the results presented in this section are at the best suggestive, limited by the small sample size by gender and the correlation between college enrollment rates and business cycle conditions.

First, Table 5 summarizes analysis by gender. Although employment status of the first job is similar, current employment status is quite different between men and women. This is due to weak attachment to the labor force of married women. Marriage and childbirth takes many women out of the labor force, probably regardless of their past employment status. Consistently, the impact of the first job is smaller for female.

Note that the estimates by gender are less precise due to the smaller sample size than that of the pooled regression. Although the IV estimates are statistically insignificant, the Hausman test does not reject the hypothesis that coefficients estimated with IV are the same as those estimated without IV. It may sound odd that the IV estimate is not distinct either from zero or from the

Table 5
Analysis by gender

	Male		Female	
	(1)	(2)	(3)	(4)
Fraction with $x_i = 1$	88.1%		86.2%	
Fraction with $y_i = 1$	82.1%		36.6%	
Marginal effect of x on y_i^{*1}	Simple probit	Probit IV	Simple probit	Probit IV
	0.48	0.47	0.31	0.28
	[0.06]***	[0.41]	[0.05]***	[0.34]
Hausman test	H_0: (1) = (2) is not rejected.		H_0: (3) = (4) is not rejected.	
Number of observations	666		740	

Notes. (1) $\Pr(y = 1 \mid x = 1) - \Pr(y = 1 \mid x = 0)$ evaluated at mean of other covariates.

(2) Coefficients of the regressions and test statistics are reported in Table A.4.

(3) Standard errors are in brackets.

*** Significant in 1%.

[27] I estimated the probit model of Eq. (2), with an interaction of v and a dummy variable indicating whether the year of graduation $\leqslant$1991. Estimated coefficients (standard errors) of v and the interaction term are 0.343 (0.248) and 0.031 (0.299). The z-value of the interaction term is 0.10. Note that the standard error of v is also much larger than that of the pooled regression in column (3) of Table 2.

simple probit estimate. It means that the estimates are not reliable due to the large standard errors. The sample size is too small to obtain reliable estimates separately for each gender; recall the number of observations whose first job was not regular is only 181 in total. Also, the instrument becomes too weak. Admitting the limitation, I interpret the result as evidence for an effect of the first job for both men and women, since the marginal effects are as large as 47 and 28%.

It is somewhat strange that the marital status has a positive impact in the first stages. As Sakai and Higuchi (2005) point, it is a reverse causality in that employment status affects marital status.[28] Since I cannot think of plausible reasons why marital status matters to men's current employment status, I tried dropping marital status of men to see whether the reverse causality of marital status contaminates the estimates of the first job's impact. It makes little difference, implying no critical bias caused by endogeneity of marital status. Since marital status is thought to have a strong effect on women's labor supply, dropping marital status would not work for women. However, potential bias on α should be negative because the tight labor market at the entry increases the probability of being married, and married women are less likely to have a regular full-time job at present, while the probability of current regular employment should positively correlate with instrument in the reduced form. Hence, potential endogeneity of marital status is not a crucial problem.

Unlike gender, people's choice of education is not independent from the business cycle. Avoiding entry to the labor market during recessions may lead to negative correlation between the ratio of college graduates and the job opening ratio. That is, high school graduates who face a slack labor market may choose to go to college. While the correlation between labor market condition and education choice itself is an interesting finding, it creates bias on the IV estimates when sample is split by education.

Taking this limitation into account, Table 6 summarizes the analysis by educational background. As expected, more educated people are more likely to have regular full-time job both at entry and at present. The job opening ratio in the year of graduation is actually negatively correlated with the proportion of college graduates. Thus, I report only the probit estimates of Eq. (1) without IV. The coefficient on x_i of the more-educated group is considerably higher than that of the less-educated group. The signaling effect should be stronger on a group with a higher proportion of regular employment at entry because the reduction in the ex-post expected productivity is larger.[29] Since the proportion obtaining a regular full-time job is higher for college graduates than for high school graduates, the estimated results support this interpretation.

If high school graduates can avoid entering the labor market during recessions by going to college, the job opening ratio should affect college graduates more. Table 6 also reports the effect of v_i on x_i, which correspond to the first stages in the IV estimates. It is actually larger for the higher educated group, although the difference is not statistically significant.

[28] I estimated probit models by gender similar to Eqs. (1) and (2), replacing dependent variable with marital status. I found that men whose first jobs were regular are more likely to be married, and people who graduated during booms are more likely to be married for both men and women.

[29] If potential employers were fully rational, they should exploit information from business cycles over time; however, the previous section shows that α is stable over time. There are several potential reasons for this puzzle. First, employers may not adjust their beliefs to the temporary fluctuation of market conditions. Also, hysteresis in aggregate labor market conditions may cancel out by making a cohort that faced slack labor market at the year of graduation suffer from deteriorated job opportunities in following years. Another possibility is that changes in the composition of educational background, which is systematically correlated with labor market conditions at entry, may have masked such effects.

Table 6
Analysis by educational background

Sample	(1) High school or less	(2) College or more	Wald test H_0: (1) = (2)
Fraction with $x_i = 1$	84.3%	90.1%	
Fraction with $y_i = 1$	55.0%	61.5%	
Marginal effect of x_i on y_i^{*1}	0.415	0.550	Rejected in 10% level
	[0.049]***	[0.058]***	
Marginal effect of v_i on x_i^{*2}	0.053	0.098	Not rejected
	[0.024]**	[0.032]***	
Number of observations	720	686	

Marginal effect of v_i on probability of going to college: -0.059 [0.026]*.

Notes. (1) $\Pr(y = 1 \mid x = 1) - \Pr(y = 1 \mid x = 0)$ evaluated at mean of other covariates, simple probit without IV.

(2) $\mathrm{d}\Pr(x = 1)/\mathrm{d}v$ evaluated at mean of other covariates, first stage probit.

(3) Coefficients of the regressions and test statistics are reported in Table A.5.

(4) Standard errors are in brackets.

* Significant in 10%.

** Significant in 5%.

*** Significant in 1%.

6. Conclusion

Employment status shows strong persistence in Japan. Using the labor market conditions at entry as an instrument, I conclude that the entry-level employment status actually has a considerable causal effect on current status. Furthermore, the instrumental variable exactly identifies persistent negative effects on people who by chance entered the labor force when the market was loose and failed to obtain regular jobs. Thus, this paper challenges the view that the poor performances of *freeters* can be attributed to their unwillingness to work and ineptitude for regular employment. Loss of skill or morale, if any, seems to be rather a consequence than a cause of being a *freeter*. A deteriorating youth employment situation could leave a lasting scar on the aggrieved generation.

At the same time, a persistent effect of failure at entry gives a clue to understanding the known cohort effects on wages and mobility. When the opportunities for regular full-time employment are largely restricted to recent graduates, a large part of the labor market outcomes of each cohort would be determined at the timing of entry. If so, the labor market conditions in the year of entry may well have long-term effects on wages and mobility. Again, the distinction between a causal relationship and spurious correlation is essential because sorting on unobservable abilities within a cohort can also generate a positive correlation between past and current employment status.

There are several potential reasons for the state dependence in employment status in Japan. Among others, I view the social institutions favorable to new graduates and the negative signaling effect as the primary causes, and they are mutually reinforcing. Typical employment practice of Japanese firms and the school-to-work transition system give advantages to new graduates. The social norm that everyone should obtain a regular job right after graduation stigmatizes those who could not obtain a regular job when leaving school. This negative signal prevents employers from hiring those who failed at entry and sustains the social institutions favorable to new graduates. The gap in effects of failure at entry between less educated group and more educated group supports this interpretation, although the small sample size limits ability to perform detailed analysis by demographic group or cohort.

An important reservation is that this paper does not reflect the drastic changes after the economic crash in the late 1990s, because those who left school after 1998 are completely excluded from the data. The sudden fall in the proportion of regular full-time workers might have changed the structure of the labor market. It is ambiguous which direction the change was in the short run; on one hand, it might weaken the signaling effect, on the other hand, lack of job opportunity might rather aggravate the effect. In the longer term, everything else being equal, labor shortage due to shrinking youth population might make it easier to find a regular full-time job even for those who lack experience. However, it is precipitant to be optimistic because the supply-driven positive effect may be canceled out if the demand for regular full-time workers also decreases. The deep recession in the late 1990s triggered various changes in the Japanese employment practice, and their consequences are still undetermined. How the undergoing changes in the labor market have affected the consequence of initial disadvantage will be an important topic for future investigation after further data is accumulated.

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Appendix A

Table A.1
Detailed tabulations of first and current jobs, by gender
Male[a]

	Current job				
First job	Regular employment	Provisional employment	Self/family employment	Not employed	Total
Regular employment	518	23	25	21	587
	(88.3%)	(3.9%)	(4.3%)	(3.6%)	(100%)
Provisional employment	19	15	3	8	45
	(42.2%)	(33.3%)	(6.7%)	(17.8%)	(100%)
Self/family employment	10	0	21	1	32
	(31.3%)	(0.0%)	(65.6%)	(3.1%)	(100%)
Never employed	0	0	0	2	2
Total	547	38	49	32	666
	(82.1%)	(5.7%)	(7.4%)	(4.8%)	(100%)

(*continued on next page*)

Table A.1 (*continued*)

Female[b]

	Current job				
First job	Regular employment	Provisional employment	Self/family employment	Not employed	Total
Regular employment	256	125	18	239	638
	(40.1%)	(19.6%)	(2.8%)	(37.5%)	(100%)
Provisional employment	14	25	5	28	72
	(19.4%)	(34.7%)	(6.9%)	(38.9%)	(100%)
Self/family employment	1	3	9	5	18
	(5.6%)	(16.7%)	(50%)	(27.8%)	(100%)
Never employed	0	0	0	12	12
Total	271	153	32	284	740
	(36.6%)	(20.7%)	(4.3%)	(38.9%)	(100%)

Note. "Provisional employment" consists of part-time, temporary and by work.
[a] Pearson $chi^2(9) = 299.57$, Pr(columns and rows are independent) = 0.000.
[b] Pearson $chi^2(9) = 131.31$, Pr(columns and rows are independent) = 0.000.

Table A.2
Probit IV with and without the years since entry

	Dropped		Included	
Dependent variables	*y*	*X*	*y*	*X*
First job was regular fulltime (x_i)	1.487		1.49	
	[0.793]*		[0.784]*	
Normalized local job opening ratio in the year of entry (v_i)		0.367		0.37
		[0.103]***		[0.103]***
Years since graduation			−0.003	0.013
			[0.013]	[0.013]
Jr. high grads/high school dropouts	−0.353	−1.044	−0.354	−1.04
	[0.316]	[0.172]***	[0.313]	[0.172]***
Jr. & tech college graduates	0.063	0.294	0.059	0.303
	[0.113]	[0.126]**	[0.115]	[0.126]**
4-year college or graduate schools	0.213	0.146	0.21	0.156
	[0.107]**	[0.114]	[0.108]*	[0.115]
College dropouts	−0.245	−0.91	−0.244	−0.906
	[0.435]	[0.330]***	[0.434]	[0.330]***
Female	−0.155	−0.224	−0.155	−0.219
	[0.127]	[0.133]*	[0.127]	[0.133]*
Currently married	0.645	0.272	0.651	0.245
	[0.141]***	[0.134]**	[0.142]***	[0.136]*
Female* married	−2.172	−0.009	−2.17	−0.02
	[0.173]***	[0.180]	[0.173]***	[0.181]

Notes. Standard errors are in brackets. All regressions include survey year dummies and constant. Number of observations = 1406.
* Significant in 10%.
** Significant in 5%.
*** Significant in 1%.

Table A.3
Effect of obtaining a regular full-time job at entry on likelihood of full-time regular employment at present: estimates by years, probit without IV

Year of graduation	Effect of x		Graduation-year fixed effects		Average v	Sample size
	coefficient	marginal effect	coefficient	marginal effect		
1986	1.186	0.344			−0.29	149
	[0.255]***	[0.047]***				
1987	1.38	0.37	−0.023	−0.009	−0.39	129
	[0.246]***	[0.036]***	[0.547]	[0.211]		
1988	1.113	0.329	0.597	0.206	−0.30	137
	[0.219]***	[0.043]***	[0.559]	[0.165]		
1989	1.51	0.386	0.439	0.156	−0.02	117
	[0.212]***	[0.028]***	[0.592]	[0.190]		
1990	1.11	0.325	0.043	0.016	0.23	115
	[0.194]***	[0.037]***	[0.600]	[0.227]		
1991	1.22	0.348	1.132	0.335	1.21	130
	[0.183]***	[0.033]***	[0.623]*	[0.119]***		
1992	1.641	0.397	0.463	0.164	0.50	106
	[0.205]***	[0.024]***	[0.632]	[0.200]		
1993	1.252	0.35	0.224	0.083	0.11	120
	[0.192]***	[0.032]***	[0.527]	[0.188]		
1994	1.311	0.356	0.592	0.203	−0.20	113
	[0.208]***	[0.032]***	[0.500]	[0.146]		
1995	1.622	0.394	−0.107	−0.041	−0.37	107
	[0.227]***	[0.026]***	[0.558]	[0.218]		
1996	1.218	0.337	0.057	0.022	−0.29	87
	[0.245]***	[0.039]***	[0.566]	[0.214]		
1997	1.513	0.368	−0.386	−0.152	−0.32	61
	[0.296]***	[0.031]***	[0.670]	[0.266]		
1998	1.418	0.352	0.116	0.044	−0.23	35
	[0.361]***	[0.039]***	[0.689]	[0.255]		

Wald Test 1
H_0: Coefficients of x are the same for all years.
$\text{Chi}^2(12) = 10.98$, prob > $\text{chi}^2 = 0.53$.
H_0 is not rejected.
Wald Test 2
H_0: All graduation-year fixed effects are 0.
$\text{Chi}^2(12) = 8.90$, prob > $\text{chi}^2 = 0.71$.
H_0 is not rejected.
$\text{Corr}(\alpha, v) = -0.02$.
Notes. Coefficients and marginal effects are those of interaction terms of x and year of graduation. Other covariates (potential experience, education, marital status, sex and survey year dummies) are included. Standard errors are in brackets.
* Significant in 10%.
*** Significant in 1%.

Table A.4
Estimated coefficients of regressions by gender

	Simple probit	Male		Simple probit	Female	
		Probit IV			Probit IV	
		y_i	x_i		y_i	x_i
	(1)	(2)	(3)	(4)	(5)	(6)
First job was regular (x_1)	1.483	1.449		1.168	0.98	
	[0.171]***	[1.270]		[0.192]***	[1.205]	
Normalized local job opening ratio in the year of entry (v_i)			0.374			0.422
			[0.162]**			[0.144]***
Years since graduation	0.013	0.013	−0.016	−0.013	−0.012	0.041
	[0.019]	[0.020]	[0.019]	[0.017]	[0.019]	[0.018]**
Jr. high grads/high school dropouts	−0.371	−0.377	−0.645	−1.117	−1.216	−1.808
	[0.233]	[0.321]	[0.226]***	[0.649]*	[0.875]	[0.309]***
Jr. & tech college graduates	−0.283	−0.283	0.04	0.153	0.163	0.329
	[0.231]	[0.231]	[0.253]	[0.121]	[0.135]	[0.150]**
4-year college or graduate schools	0.203	0.205	0.385	0.233	0.229	−0.109
	[0.151]	[0.166]	[0.161]**	[0.153]	[0.155]	[0.167]
College dropouts	0.029	0.017	−1.1	−0.858	−0.864	−0.244
	[0.475]	[0.656]	[0.405]***	[0.796]	[0.796]	[0.656]
Currently married	0.667	0.669	0.295	−1.474	−1.465	0.178
	[0.138]***	[0.162]***	[0.140]**	[0.120]***	[0.141]***	[0.134]
Marginal effect of x_i^{*1}	0.48	0.47		0.31	0.28	
	[0.06]***	[0.41]		[0.05]***	[0.34]	
P		0.0018			0.102	
		[0.671]			[0.633]	
LR chi^2/Wald chi^2/F	142.3	97.57		244.14	236.59	
Pseudo R^2	0.23			0.25		

Dependent variables:
$y_i = 1$ if current job is regular fulltime
$x_i = 1$ if first job was regular fulltime

Hausman Test 1
H_0: All coefficients in (1) are equal to those in (2).
$\text{Chi}^2(10) = 0.00$, prob > $\text{chi}^2 = 1.00$.
H_0 is not rejected.

Hausman Test 2
H_0: All coefficients in (4) are equal to those in (5).
$\text{Chi}^2(10) = 0.41$, prob > $\text{chi}^2 = 1.00$.
H_0 is not rejected.

Notes. (1) $\Pr(y = 1 \mid x = 1) - \Pr(y = 1 \mid x = 0)$ evaluated at mean of other covariates.

(2) Standard errors are in brackets. All regressions include survey year dummies and constant. Number of observations = 666 for male, 740 for female.

* Significant in 10%.
** Significant in 5%.
*** Significant in 1%.

Table A.5
Estimated coefficients in probit regressions (without IV) by education

Dependent variables	$heduc_i$ (1)	y_i (2)	x_i (3)
Normalized local job opening ratio in the year of entry (v_i)	−0.149 [0.066]**		
$x_i * heduc_i$		1.62 [0.212]***	
$x_i * (1 - heduc_i)$		1.163 [0.157]***	
$v_i * heduc_i$			0.513 [0.169]***
$v_i * (1 - heduc_i)$			0.275 [0.126]**
Years since graduation	−0.045 [0.010]***	−0.002 [0.013]	0.012 [0.013]
Jr. high grads/high school dropouts		−0.433	−1.029
Jr. & tech college graduates		[0.202]**	[0.172]***
4-year college or graduate schools		−0.345 [0.260]	0.33 [0.128]***
College dropouts		−0.186 [0.253]	0.187 [0.118]
Female		−0.605 [0.430]	−0.914 [0.331]***
Currently married	0.246 [0.105]**	−0.169 [0.121]	−0.223 [0.132]*
Female* married	0.435 [0.102]***	0.65 [0.132]***	0.242 [0.136]*
Years since graduation	−0.344 [0.138]**	−2.164 [0.170]***	−0.017 [0.180]
LRchi^2	37.57	685.04	84.27
Pseudo R^2	0.02	0.36	0.07

$heduc_i = 1$ if went to college, 0 otherwise.
$y_i = 1$ if current job is regular fulltime.
$x_i = 1$ if first job was regular fulltime.

Wald Test 1
H_0: Coefficients of $x_i * heduc_i$ and $x_i * (1 - heduc_i)$ in (2) are equal.
$Chi^2(1) = 3.06$, prob > chi$^2 = 0.08$.
H_0 is rejected in 10% level.

Wald Test 2
H_0: Coefficients of $v_i * heduc_i$ and $v_i * (1 - heduc_i)$ in (3) are equal.
$Chi^2(1) = 1.28$, prob > chi$^2 = 0.26$.
H_0 is not rejected.

Notes. Standard errors are in brackets. All regressions include survey year dummies and constant. Number of observations = 1406.

* Significant in 10%.
** Significant in 5%.
*** Significant in 1%.

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