

Chapter 2

A Tailor-Made Plant Closure Survey

In Switzerland there is no data publicly available for workers who lost their job because their plant shut down. For this reason, we ran our own survey. This chapter presents this survey including its design and the procedure we chose to collect data. The chapter is organized as follows: we first discuss whether using plant closure data may alleviate the problem that unemployment is a selective phenomenon and that particular groups of workers are more prone to lose their job than others. Next, we present our sampling strategy and discuss how potential survey bias may threaten the validity of our data. We address our data collection procedure and explain how we linked survey data to register data. We go on to analyze potential bias in the data that we collected and describe the construction of a control group. We then present the institutional and labor market context of our study. Finally, we discuss the main limits of our study.

2.1 Plant Closure Data as a Way to Avoid Selection Bias

Job loss is a typical non-random phenomenon: workers with particular characteristics such as lower levels of education have a higher probability of losing their job (Balestra and Backes-Gellner 2016: 17). A non-random selection into unemployment would be less a cause for concern if we could control for all of the workers' characteristics that are relevant for reemployment. But important characteristics such as motivation, work performance or social skills are usually not observed by researchers and thus cannot be controlled for. If workers with unobserved characteristics that hinder reemployment are overrepresented in the group of the displaced workers, the negative effect of job loss on average workers will be overestimated. In such a case the results would be affected by selection bias.

It has been argued that a strategy to address this bias is the use of plant closure data (Brand 2006). If the workers of a company are displaced because of economic failure, the reason for job loss cannot be attributed to the workers themselves; it may

thus be exogenous to them. In other words, if the whole workforce of a company is displaced, it may be reasonable to assume that the employer did not dismiss workers based on their performance, motivation or other individual characteristics (Gibbons and Katz 1991: 352). Accordingly, observable and unobservable characteristics are likely to be similarly distributed among workers displaced by plant closure and among workers not displaced – as would be the case in an experiment with random attribution to treatment.

However, more recent research argues that even with plant closure data there may still be a selection bias at work. In fact, workers may self-select into firms with a higher propensity to close down. Belonging to the workforce of a non-profitable plant does not seem to be completely random as a comparison of wages between displaced and non-displaced workers suggests (Hijzen et al. 2010: 254–5). Confronted with a choice, highly qualified workers are likely to avoid employment in a plant with economic difficulties.

Moreover, there may be selection out of the sample. Well-informed and entrepreneurial workers will try to quit the company before the actual shutdown (Eliason and Storrie 2009b: 1397). It has been suggested that those workers with the best labor market prospects have the highest probability of “leaving the sinking ship” early. A study based on Austrian administrative data provides evidence for this assumption: workers with higher incomes had a higher probability of leaving the company up to a year before it closed down (Schwerdt 2011: 99). Moreover, those who left the company one to two quarters before the closure had significantly better labor market outcomes than workers from non-closing plants *ceteris paribus* (Schwerdt 2011: 100).

For our study, we sampled those workers who were employed in one of the five plants at the moment of the announcement of the plant closure. The announcement took place between 3 and 9 months before the actual displacement – except in Plant 2 (Biel), where there was no advance notice. In the light of the finding by Schwerdt (2011) that workers might “leave the sinking ship” up to one year before the plant closed down, we may be confronted with selection out of the sample.

2.2 Sampling

To constitute a sample of workers displaced by plant closure, we would ideally draw a random sample of all workers who experienced this situation within a specific period and geographical space. However, in Switzerland there is no systematic account of workers affected by plant closure. Although the Swiss Labour Force Survey records involuntary job loss, no distinction is made between displacement because of plant closure and dismissal for just cause. For this reason we conducted our own survey.¹

¹The project team consisted of five people. The principal investigator, Daniel Oesch, launched the project, was responsible for the acquisition of funding, supervised the project at all stages, con-

Our survey was conducted among the workforces of five recently closed plants. We defined three criteria for the selection of the companies and then proceeded with convenience sampling, i.e. chose the plants that agreed to participate in our survey. The selection criteria were the following: (1) The plants had to have closed down about 1–2 years before the survey was conducted. This strategy aimed at capturing long-term unemployment and the exhaustion of unemployment insurance benefits.² Using this strategy implies that our data is right-censored, i.e. that some of the workers have not experienced exit from unemployment at the moment when we conducted the survey. (2) We targeted medium-sized and large plants with more than 100 employees. The rationale behind this choice was to avoid reverse causality: in the case of small firms, closure may be caused by the workers' performance, which would blur our analysis of the cause effect of plant closure on workers' ensuing lives. (3) We focused on the manufacturing sector. In this sector plant closures are particularly frequent, which points to its outstanding social relevance (Cha and Morgan 2010: 1141).

Based on these three criteria we made an inventory of closed plants through a screening of the national and regional online press and a short survey among the cantonal employment offices. We identified ten plants, contacted them by mail and telephone, and succeeded in persuading five plants to participate in the survey. For two plants, access to the workers' addresses was given by the plant's management. In two other plants, the access was provided by the cantonal employment offices that accompanied the closing process and in one plant by the works council.

Plant 1 was part of a multinational corporation with headquarters in Switzerland and was active in the sector of machine tool manufacturing. Between October 2009 and August 2010 it relocated its production site from an industrial area outside Geneva to another part of Switzerland and abroad and subsequently displaced 169 workers. Fifteen production workers remained in the factory to provide the plant's machine repair service. A small number of workers helped to assemble the machines in the new production site in Switzerland but without being continuously employed there and five workers went abroad to work at the new production site. In addition, employees in the research and development department and the administration continued to operate on the site. The closure of the production department was announced about 4 months in advance.

ducted data analysis and published results. The author of this study, Isabel Baumann, was involved in all stages of the project, prepared and managed the survey, collected the data, conducted data analysis, and published results. The student assistants, Jessica Garcia and Lorenza Visetti, were responsible for the data entry and coding process and Jessica Garcia conducted telephone interviews with a particular group of survey participants. Katrina Riva was responsible for authoring a data documentation codebook that describes the content and structure of the dataset used for this study – which will be made publicly available on the platform DARIS. Certain tasks were outsourced such as the printing and the sending of the questionnaire. In addition, numerous colleagues helped us with some of the tasks.

²In Switzerland, workers are normally entitled to unemployment benefits of 18 months (that is, after having worked for at least 18 months (AVIG 2012, Art. 27, Ziff. 2).

Representatives of the employees or trade unions and the employer negotiated a redundancy plan. The plan included the set-up of an outplacement center with particular structures to promote the reemployment of workers with disabilities. Workers had the right to leave the plant immediately if they found a new job. They received a termination payment of at least CHF 10,000 and additional benefits depending on their tenure and age. Workers who had to move house or commute at least 40 km longer distances were entitled to an additional payment of CHF 3500. An early retirement plan allowed female workers to retire at 61 and male workers at 62, 3 years before the regular retirement age, on condition that they signed up for unemployment benefits.³ Swiss residents were guaranteed a replacement rate of 70 % of their former wage. For French residents – who were numerous in this plant – the early retirement plan covered up to 60 % of their former wage. In addition, the plant continued to pay the contributions to the company's old-age pension fund until the regular retirement age in order to avoid a reduction in pension benefits.

Plant 2 was a Swiss company located in the agglomeration of the city of Biel and active in the printing sector. At the end of November 2009 the company announced that it was unable to pay the salaries. The cantonal employment office then informed the workforce that the plant would be closed down completely because of insolvency. The 262 employees – who had accepted wage cuts 1 year earlier in order to prevent a closure of the plant – became unemployed almost overnight. Not only was there no redundancy plan, but the workers lost money since the plant was incapable of reimbursing overtime and the workers' shares of the retirement fund.

Plant 3 was part of a multinational corporation with headquarters outside Switzerland. Located outside a small town in North-Western Switzerland, it produced various kinds of chemicals. Due to shift-work and weekend-work supplements, the pre-displacement wages paid by Plant 3 were high compared with other firms in the region. The closure was announced about 4 months in advance. In January 2009 its 430 workers were displaced. About 15 workers, who were responsible for tidying up and cleaning the plant, continued to be employed for another 2 years. The sector to which Plant 3 belonged had been experiencing turbulences for many years and high turnover was observed at the intermediate management level of Plant 3 during the years before the closure.

The plant offered a redundancy plan containing termination pay. For a 25-year old worker with 5 years of tenure the termination pay was CHF 8250 and for a 45-year old worker with 20 years of tenure CHF 22,000. While workers had the opportunity to leave the plant before the official end of their contract, those who remained until the end received a premium of CHF 70 for each day worked. The company mandated an outplacement center to provide workers with support for their job search and allowed workers to use its services during their working time. If workers had to move house for their new job and had to commute at least 30 km more than before, they received financial support up to CHF 4000. Older workers at

³The Swiss unemployment insurance entitles workers who become unemployed at the age of 62 to receive unemployment benefits up to their regular retirement age.

2 years from the official retirement age had the option of early retirement. They received pension benefits that corresponded to at least 70 % of their former wage or at least CHF 55,000 per year.

Plant 4 was a Swiss company producing printing machines in the Canton of Bern. When it closed down, 324 workers lost their job in three phases between October 2009 and August 2010. The displacement was announced 5–9 months in advance. Nearly a hundred of the workers affected were relocated to another plant together with the machines on which they were specialized. About 50 displaced workers were employed in a firm that started operating on the production site of Plant 4. However, this firm also closed down about 2 years later.

The plant agreed to a redundancy plan after negotiating with the trade union. For workers who earned less in their new job the company paid the difference for 6–24 months, a measure aimed at encouraging workers to accept lower paid jobs more readily. This measure was, however, little used. In contrast, almost all workers who were eligible for the early retirement benefits included in the redundancy plan accepted the offer. Workers were enabled to take to early retirement from the age of 56.5 years. Workers aged up to 57 were paid their full salary up to age 58 and then received a flat rate of CHF 4000 per month until they retired regularly. Workers aged between 58 and 59 also received a flat rate of CHF 4000 per month until their regular retirement. Workers who were 60–63 at the moment of displacement were paid 90 % of their former salary and those over 63 were paid 100 % of their salary up to the regular retirement age.

Plant 5 produced metal and plastic components and employed about 205 workers in an industrial zone in North-Western Switzerland. It had been sold to a multinational corporation with headquarters in Switzerland about 2 years before this corporation closed the plant. The displacement took place between September 2009 and March 2010 and was announced about 6 months in advance. There was some limited turnover before the closure was officially announced.

The plant offered a redundancy plan including termination pay depending on workers' tenure and age. A 25-year old worker with 5 years of tenure received CHF 11,000 and a 45-year old with 20 years of tenure CHF 33,000. The plan also included the setting-up of an external outplacement center which employees were permitted to use within official working hours. The workers were given priority in the event of vacancies in other plants of the company, but this option was rarely taken up. Workers who found a new job could negotiate to leave the plant before the official displacement date. If workers had to move house or commute longer distances to their new job they received financial support. Finally, the redundancy plan offered the option of early retirement for workers from age 58. Early retirement benefits were calculated based on workers' tenure and were disbursed in the form of payments to the company's old-age pension fund.

None of the five plants offered a training program funded by the companies. However, the workers who enrolled in the public employment offices were entitled, like any unemployed job seeker in Switzerland, to participate in active labor market measures such as training and internships.

Table 2.1 Information on the five manufacturing plants included in the survey

Plant	Sector	Workers displaced	Refused address transmission	Inactive address	Active addresses	Official dis-placement dates
Plant 1 (Geneva)	Metal products	169	0 (0 %)	20 (11 %)	149	01.10
Plant 2 (Biel)	Printing	262	3 (1 %)	30 (11 %)	229	12.09
Plant 3 (NWS 1)	Chemicals	430	6 (1 %)	67 (16 %)	357	01.09
Plant 4 (Bern)	Machinery	324	19 (6 %)	17 (5 %)	288	10.09–08.10
Plant 5 (NWS 2)	Metal & plastic	205	8 (4 %)	17 (8 %)	180	09.09–03.10
Total		1390	36 (3 %)	151 (11 %)	1203	

In order to access the workers' postal addresses we had to receive their consent. By means of a letter we informed the workers about our study and asked if they refused to participate. 4% of the total population (n=53) refused to give access to their address.⁴ In addition, about 10% of the addresses (n=133) turned out to be invalid because the workers had moved or – in a few cases – were deceased. From an original population of 1389 workers, this left us with a survey population of 1203 individuals, as presented in Table 2.1.⁵

2.3 Survey Bias

Biases typically associated with data collection are nonresponse bias and measurement error. Nonresponse bias occurs when survey participants differ from nonparticipants in a way that is relevant for the phenomenon under study (Dillman et al. 2009: 17). If the group of nonrespondents were to be composed completely at random, this would reduce the statistical power of the results but not induce systematic bias. Unfortunately, nonresponse is often non-random: individuals not participating in a survey are likely to be less interested in the topic, to have less time to participate

⁴The main reasons for refusal were (i) that workers did not feel concerned by our study, for instance because they were hired on a temporary basis, (ii) that they did not speak the language, or (iii) that they were frustrated with their situation. Note that refusals were very low where the process was managed by the works council (0%), but significantly higher where workers were contacted by the plant's former management (4% and 6% refusals respectively).

⁵For these workers we signed an agreement with the data providers – firm managements, cantonal employment offices and workers' council – guaranteeing the workers' data protection.

or to have lower literacy in the language of the questionnaire (Groves and Couper 1998; Stoop 2005). For Switzerland, earlier findings show that immigrant groups from non-EU countries are usually underrepresented in surveys (Laganà et al. 2011; Lipps et al. 2013).

It is thus important to understand the mechanism behind nonresponse and, if possible, to correct for it. Dillman et al. (2009: 16) introduced the tailored survey design method, an approach that strategically uses survey design to reduce potential bias. A first possibility to address nonresponse bias is to repeatedly contact the population that is surveyed. This measure alone, however, may not be sufficient to reach individuals belonging to subgroups with traditionally low participation rates such as particular immigrant groups. A possible strategy to win the participation of these groups is to alter the survey protocol, for instance by using a shorter questionnaire (Peytchev et al. 2009: 786).

A second technique is the use of a mixed-mode approach (Dillman et al. 2009). Taken on their own, different survey modes each have their advantages and disadvantages. For instance, an Internet survey may be particularly suited for reaching younger cohorts while its coverage is limited, notably among older cohorts (Schräpler 2001: 13; Täube and Joye 2002: 77; Kempf and Remington 2007). Used in combination, these different modes may be a powerful method to increase the respondents' representativeness. A third strategy is the use of financial incentives that encourage respondents to reciprocate by completing the survey. By motivating particularly those respondents with a tendency not to answer survey questionnaires, incentives have proved to reduce nonresponse bias (Dillman et al. 2009: 249). Research in survey methodology indicates that unconditional incentives are more effective than incentives contingent on completing a survey (Harrison 2010: 519; Lipps 2010: 84). In addition, cash and vouchers appear to be more effective than noncash incentives (Harrison 2010).

Once the fieldwork is completed and the researchers have doubts about the representativeness of their sample, an ex-post method to deal with nonresponse bias is to build nonresponse adjustment weights. In order to use this procedure, it is imperative to know at least one characteristic of all individuals (respondents and nonrespondents) in the sample (Corbetta 2003: 227). The more characteristics there are available, the more sophisticated the weighting becomes.⁶ Finally, if other data sources are available, they may provide helpful information about the nonrespondents. Particularly helpful seems to be administrative data since it tends to be comparatively reliable (Corbetta 2003: 196). It is thus valuable for the study's quality to have at least some measures for the nonrespondents.

Another problem that impairs data quality and that typically occurs in data collection is measurement error (Antonakis et al. 2010: 1095). Measurement errors may have random or systematic causes (Phillips 1981: 400). They are random if

⁶After the identification of the under- and overrepresented groups based on the known characteristics a weighting coefficient is calculated for each respondent (Little and Vartivarian 2005). This weighting coefficient is attributed to every individual while statistical operations are carried out.

they have no systematic pattern and if the data measured sometimes over- and sometimes underestimates the true value of a variable.

Social desirability may systematically bias respondents' answers (Bound et al. 2001: 3746, 3784). In this context, it has been shown that working hours are regularly overstated. This finding has been explained by the positive connotation of hard work. Similarly, retrospective questions are systematically error-prone. A study assessing the validity of retrospective data by comparing it with longitudinal data finds large differences. Subjective psychological states are remembered with particular inaccuracy, while other measures such as reading skills, height or weight are reported more correctly (Henry et al. 1994: 100). This is likely to be a result of most respondents' imperfect memory, the fact that they can only report what they were aware of at the time (Hardt and Rutter 2004: 260–1). However, while it is untested that longitudinal studies are the best way to examine changes over time, cross-sectional assessments of past events may be the second-best option (Hardt and Rutter 2004: 261).

A technique to evaluate and reduce potential measurement error is to use multiple indicators for the variables measured (Bound et al. 2001: 3740). Particularly appropriate for the validation of survey data is information stemming from registers, for instance from the public administration or from employers (Corbetta 2003: 196). Even this data may, however, not be completely free from error.

2.4 Data Collection

The strategies that we used to handle survey bias are the combination of our own survey with administrative data. The main features of our survey design were multiple contact attempts, mixed modes, incentives and weighting. Our data collection instrument was a questionnaire with about 60, mainly closed-ended questions. Many of the questions were adopted from established surveys such as the Swiss Household Panel or the Swiss Labour Force Survey.

The questionnaire was structured into seven parts: the first part contained questions on the workers' job in the plant from which they were displaced. The second part was about the job search and the third about the workers' new job if they had found one. The fourth part asked questions on workers' well-being and social life and the fifth part questions on their household. In a sixth part workers were asked to indicate their socio-demographic information. In the last part we asked for their consent to access register data, further contacts and whether they wished to be informed about the results of the study. Since the target group consists of individuals living in both the German- and French-speaking regions of Switzerland, the questionnaire was drawn up in two languages. It was first cross-examined by survey experts. Then four workers of the survey population completed a test questionnaire. Their feedback on the intelligibility and other features of the questionnaire was incorporated in the questionnaire.

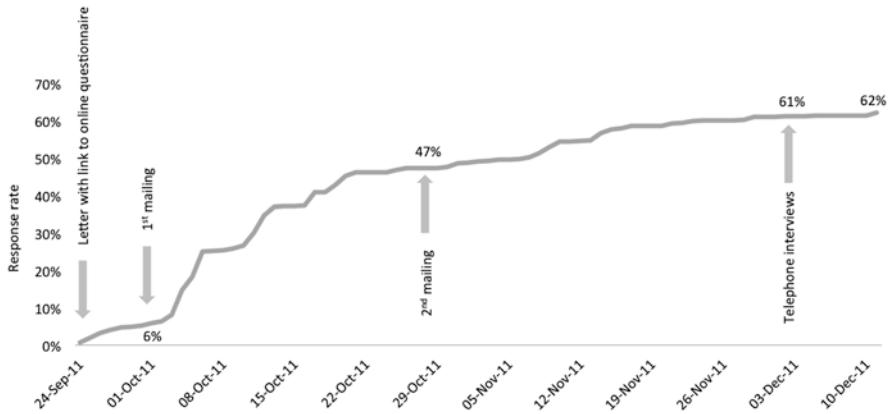


Fig. 2.1 Timeline of the survey with response rate and contact attempts

The survey was started at the end of September 2011 and completed in December 2011 (see Fig. 2.1). We first sent out a pre-notice letter that presented the purpose of our study and announced the imminent questionnaire. A Web link given in the letter provided workers with access to the online version of the questionnaire and allowed them to start participating in the survey immediately. A recommendation letter issued by the Swiss State Secretariat of Economic Affairs (SECO) accompanied the pre-notice letter.⁷ The purpose of this letter was to enhance the survey's legitimacy by showing governmental support. One week later, the workers received the paper-and-pencil version of the questionnaire. This mailing was accompanied by an unconditional financial incentive in the form of a voucher for 10 Swiss Francs (about 8 €) for Migros, Switzerland's biggest retail company. About 1 month later, at the beginning of November, those workers who had not yet participated received the paper-and-pencil questionnaire a second time. The control of response was possible since our survey was not anonymous. An individual identification number affixed on every questionnaire allowed us to track responses back to the participants.

This strategy also allowed us to evaluate the respondents' representativeness while the survey was still running. Since previous research from Switzerland found an underrepresentation of particular immigrant groups, we analyzed nonresponse bias according to national origin. Information about the nationality of the whole survey population was not available in our data. We therefore created a proxy for national origin on the basis of workers' surnames. Thereby, we distinguished between four groups: (1) Switzerland, France and Germany, (2) Spain and Portugal, (3) Italy, and (4) other countries, notably ex-Yugoslavia and Turkey. When taking this proxy – an admittedly rough indicator for immigration background – and looking at the response rate of these four groups, we observed differences in nonresponse rates as predicted by previous research: Group 1 had a response rate of 66%, Group 2 56%, Group 3 55% and Group 4 40%. Accordingly, in order to increase

⁷This institution also partially funded our study.

the response rate of Group 4 we drew a sample of this group and succeeded in completing the survey questionnaire with 15 individuals from non-EU member countries by telephone. This measure led to a final response rate of 52 % for Group 4, similar to those of the other proxy immigrant groups.

Of all the respondents 76 % used the paper-and-pencil questionnaire, 21 % responded online and 3 % by telephone.⁸ It is not surprising that paper-and-pencil was the most frequently used mode since we had workers' postal addresses at our disposal, but not their email addresses. The repeated contact attempts seem to have been worthwhile. The access to the online questionnaire at the start of the survey resulted in a response rate of 6 %. After the first mailing of the paper-and-pencil questionnaire the response rate rose to 47 %. The second mailing led to an increase in responses of another 14 percentage points and the telephone interviews contributed one more percent. Figure 2.1 shows the timeline of our survey. Whether the use of incentives helped improve the response rate cannot be tested since a control group without incentives would have been needed.

The overall response rate of the survey was 62 %, which is equal to 748 workers. Almost two out of three displaced workers thus responded to our questionnaire, a relatively high response rate as compared to an earlier plant closure study for Switzerland which had response rates between 20 and 31 % depending on the company (see Weder and Wyss 2010: 9–13). Workers' motivation to participate in this study may be due to a number of factors such as multiple contacts, mixed modes of surveying, financial incentives and an official recommendation letter from the State Secretariat for Economic Affairs. In addition, workers possibly felt strongly concerned by the topic of the survey and were interested in the goals of the study (Sweet and Moen 2011: 9). Comments that we received with the questionnaires let us assume that the workers were relieved to be able to inform us about their experiences after plant closure. After completing the survey, we adjusted for nonresponse by weighting the data provided by the respondents. We used a technique that is based on a "missing at random" assumption (see Baumann et al. 2016).⁹

Very important for our study was the fact that we were able to link the survey data to register data from the public unemployment insurance. The unemployment

⁸In addition to the non-EU immigrants, we conducted seven telephone interviews with workers who called because they did not want to participate in the survey. We were able to persuade them to give us some basic information about themselves. We therefore conducted a total of 22 telephone interviews.

⁹In this case, subgroups based on variables available for respondents and nonrespondents are created, assuming that non-participation happened at random within these subgroups. Accordingly, we created subgroups based on information that we received from the address providers for all displaced workers. Since the same information is not available for all plants in our sample, different variables are taken into account for each plant when constructing the individual-level weights. This type of nonresponse adjustment is most effective when the available variables used to construct the subgroups (e.g. sex, age, nationality, occupation) are correlated to the variable of interest in the study (e.g. reemployment prospects). The literature on job displacement suggests that this is the case: sex, occupation, age and nationality seem to affect reemployment chances (e.g. Farber 1997; Chan and Stevens 2001; Kletzer 2001; Jolkkonen et al. 2012). Our method to construct weights thus appears relevant.

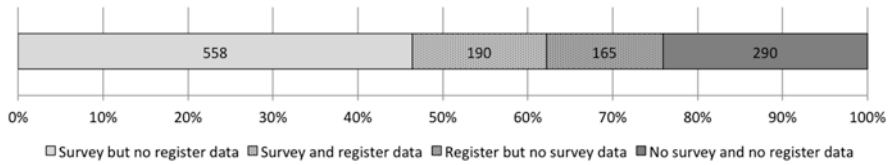


Fig. 2.2 Proportions of available survey and register data. Note: The numbers in the figure indicate the size (n) of each group of workers. Total N = 1203

register data contains numerous variables on the workers' unemployment history. However, it is only available for a limited number of workers since access to this information depends on several preconditions. First, a worker must have registered with the unemployment insurance, secondly access was possible only if there was no explicit refusal by the workers¹⁰ and thirdly the workers had to be identifiable in the unemployment insurance database on the basis of ambiguous indicators, that is to say name and address. Workers who did not apply for unemployment benefits because they found a job right away, went into retirement or preferred to avoid the stigma of living on benefits are not covered by this data source.

In total, we gained access to the unemployment register data of 355 workers. 190 of these 355 workers also participated in the survey; for these 190 workers, we have information from two sources on certain measures such as pre-displacement income. The other 165 (of the 355) did not participate in the survey; the data available on these nonparticipants increased the number of workers for whom we have relevant information to 913 workers or 76% of the total survey population (Fig. 2.2). For the post-displacement labor market status – one of our main outcome variables – we have information for 884 workers, which is equal to 74% of the total survey population.

Finally, our database also includes basic information such as birth date, occupation or nationality that we received from the workers' former employers. Plant register data is available for all displaced workers (N = 1203), but the amount and type of information vary across plants: while we received important information such as occupation or age from some plants, we obtained information only on the displacement date from others.

In Table 2.2 we present the descriptive statistics of our study. We distinguish between plant, survey and register data. In addition, we created variables that contain the maximum available information by combining the three data sources.¹¹ The combined dataset reveals that about 16% of the workers in our sample are female and 84% male. 8% worked before the displacement as managers, 5% as

¹⁰In order to receive the workers' agreement we included a question in our survey that was formulated in such a way that the respondents had to inform us if they did not wish us to access their data. 144 respondents – about 20% of the respondents – refused access.

¹¹We prioritize register data before survey data and survey data before plant data whenever more than one data source was available. For the construction of the wage variable, we prioritized survey data for workers with monthly wages over CHF 10,500 because for administrative reasons wages above this amount are not assessed.

Table 2.2 Descriptive statistics for different types of data (in %)

		Plant data	Survey data	Register data	Combined data
Sex	Female	17.1	17.2	18.0	15.7
	Male	82.9	82.8	82.0	84.3
ISCO 1-digit occupation (before displacement)	Managers	10.9	4.9	2.8	8.1
	Professionals	5.5	9.9	6.7	5.2
	Technicians	21.7	17.9	13.7	19.4
	Clerks	5.0	11.5	10.9	8.3
	Craft workers	25.0	25.6	27.1	25.9
	Machine operators	29.9	24.3	32.4	29.6
	Elementary occupations	2.0	6.0	6.4	3.5
Age (at displacement)	<25	14	7.1	5.1	8.2
	25–29	6.3	3.5	6.8	5.4
	30–34	6.8	6.3	8.5	6.7
	35–39	9.2	7.7	7.9	8.2
	40–44	13.4	11.7	11.6	11.9
	45–49	14.3	17.3	18.6	16.5
	50–54	13.4	14.7	17.5	15.0
	55–59	10.2	14.8	11.8	11.9
>59	12.3	17.0	12.1	16.3	
Education	Does not know/refusal	–	4.0	5.1	4.5
	Mandatory education or less	–	9.5	18.3	13.3
	Pre-apprenticeship	–	3.6	3.7	3.4
	Upper secondary education	–	53.4	59.2	54.1
	Higher vocational education	–	17.3	7.1	14.7
	University of applied sciences or university	–	12.2	6.6	10.1
Nationality	Switzerland	76.4	74.4	69.4	71.6
	Germany	6.5	3.9	3.4	4.2 ^a
	France	0.6	7.6	0.1	5.3
	Italy	5.4	4.6	5.3	8.0
	Portugal	1.0	1.3	1.1	
	Spain	2.2	1.3	3.1	
	Other EU countries	1.0	–	0.6	
	Kosovo and Albania	1.3	0.6	1.0	10.9 ^b
	Ex-Yugoslavia	3.7	3.1	9.55	
	Turkey	1.4	2.6	4.5	
	Asia	0.6	–	1.7	
<i>N max</i>	1203	748	355	1203	

^aGermany and Austria

^bNon-EU countries. Although Croatia has been a member of the European Union since 2013, it is included in this category

professionals, 19 % as technicians or associate professionals, 8 % as clerks or sales workers, 26 % as craft workers, 29 % as machine operators or assemblers and 4 % in elementary occupations. Regarding the age structure, 14 % were aged under 30, 43 % were between 30 and 50, and 43 % over 50. With respect to education, 17 % have less than upper secondary education, 69 % have upper secondary education, 10 % have a tertiary degree and about 4 % refused to answer. Finally, the large majority of the workers are Swiss citizens (72 %), 4 % are German and 5 % French citizens, 8 % come from other European Union countries and 11 % from non-EU countries. Overall, the median worker in our sample is a prime-aged male production worker.¹²

In addition to the survey and register data we collected some qualitative data. We interviewed the head of the human resource department of Plants 3 (NWS 1), 4 (Bern) and 5 (NWS 2), the chairperson of the works council of Plant 1 (Geneva) and the trade unionist who represented workers' interests in the insolvency process of Plant 2 (Biel). The conversations covered the closure procedure and the details of the redundancy plan (if one was negotiated). Over the period of our study, we had regular exchanges with some of the displaced workers about their experience when we tested the questionnaire and by telephone when they had questions about it. Moreover, in June 2013 we presented and discussed the results of the study to the survey participants in Bern and Geneva. These events not only provided a useful reality check of the results but also gave us another valuable opportunity to learn how workers lived through the displacement.

2.5 Identifying the Presence of Bias in Our Data

The crucial issue is whether our survey design and the use of register data helped us to avoid the main biases outlined above. Nonresponse bias is present when individual characteristics relevant for the outcome variables of our study – for example reemployment – also determine the likelihood of participating in the survey. A proper nonresponse analysis requires the availability of characteristics for all workers – participants and nonparticipants. Since our dataset only partially fulfills this requirement, we first analyze nonresponse with the available characteristics and then analyze the propensity to participate using variables that are available for all respondents plus the nonrespondents for whom we have register data.

Figure 2.3 presents a logistic regression analysis predicting the likelihood of participating in the survey. For this analysis we use only data that is available for both survey participants and nonparticipants. As independent variables we use nationality, sex, age and occupation. The variable “nationality” is a proxy that we constructed on the basis of the workers' surnames. The other variables were provided

¹²The dataset is publicly available from FORSbase service at FORS Center: <https://forsbase.unil.ch/project/study-public-overview/13181/0/>The dataset is called “Situation professionnelle deux ans après des licenciements collectifs dans le secteur industriel – 2011”.

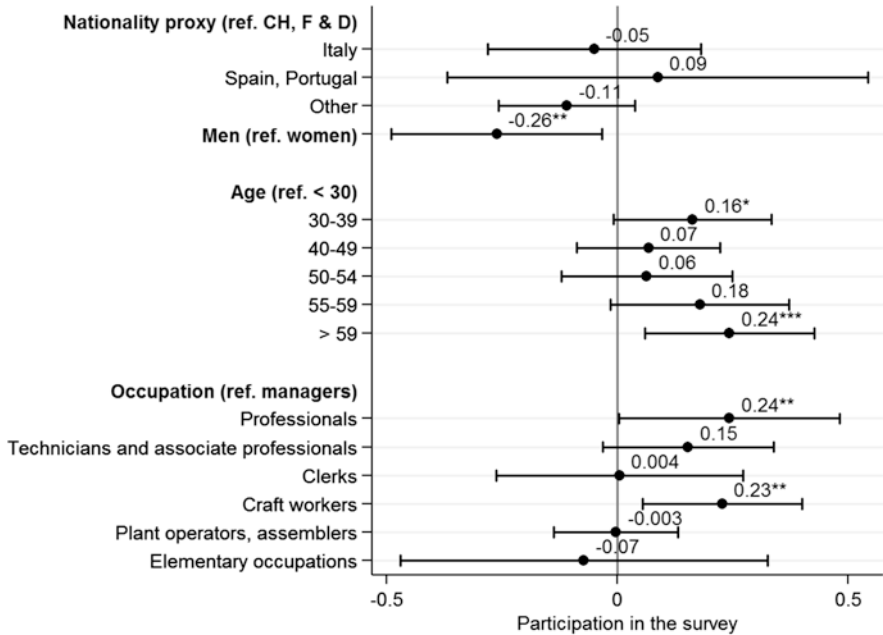


Fig. 2.3 Average Marginal Effects (AME) for a binomial logistic regression for participation in the survey. N=350. Note: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This analysis is based on data from only one plant (Plant 3) because the same variables were not available for the other plants. Reading example: As compared to workers with Swiss, French or German nationality, workers with Italian nationality are 5 percentage points less, workers with Spanish and Portuguese nationality 9 percentage points more and workers with other nationality 11 percentage points less likely to participate in the survey

by the plants. However, not all plants provided us with complete information: Only Plant 3 (NWS 1) provided us with the variables sex, age and occupation. Since the nationality proxy is based on workers’ names, this information is available for workers from all plants. Accordingly, we base Fig. 2.3 solely on data from Plant 3. Since logistic regression estimates cannot be interpreted as relative risks, we indicate the average marginal effects which specify the effect size (Mood 2010: 80).

Our analysis shows that there are no statistically significant differences in participation in the survey with respect to nationality. In contrast, we observe that men are 26 percentage points likely than women to answer the survey questionnaire – a finding that confirms earlier results on nonresponse in surveys in Switzerland (Joye and Bergman 2004: 79). We also find significant differences with respect to age, workers aged 30–39 or over 55 being 16–24 percentage points more likely to participate in the survey than workers in their twenties. With respect to occupation, the analysis shows that that professionals and craft workers are 23–24 percentage points more likely to participate in the survey than managers.

In order to evaluate the effect of our strategies to circumvent nonresponse bias – repeated contact attempts, mixed methods, weighting and using register data – we

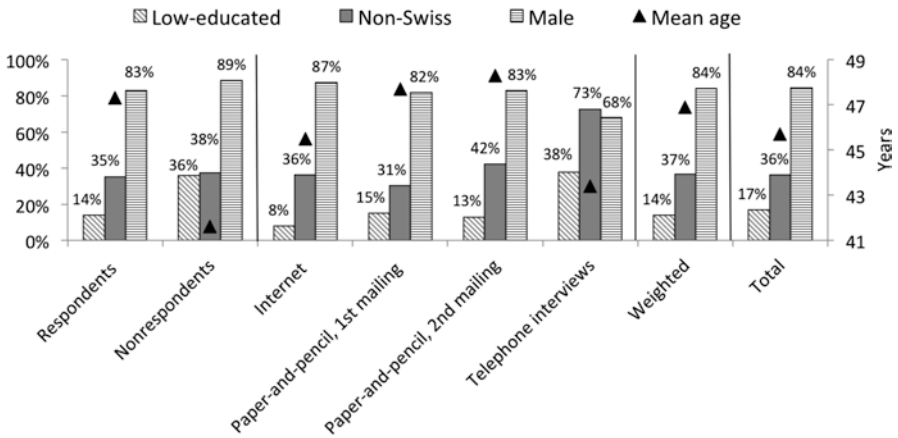


Fig. 2.4 Respondents’ characteristics by survey mode, weight and response. Note: The low-educated include individuals with less than upper secondary education. N Respondents=748, N Nonrespondents=165, N Internet=157, N Paper-and-pencil 1st mailing=398, N Paper-and-pencil 2nd mailing=165, N Telephone interviews=22, N Weighted=748, N Total=1203. Z-tests reveal that respondents and nonrespondents are significantly different for the following characteristics: Male ($p < 0.03$), Non-Swiss ($p < 0.02$), Low-educated ($p < 0.00$). A t-test reveals that respondents and nonrespondents differ according to age ($p < 0.00$)

would ideally compute a similar model as presented in Fig. 2.3 for different worker subgroups. But since these worker subgroups are small, an analysis by means of a logistic regression is difficult. For this reason, we proceed with a descriptive analysis, comparing the socio-demographic characteristics of different worker subgroups. The analysis of the nonrespondents relies on the register data available for 165 nonrespondents.

Accordingly, Fig. 2.4 illustrates the characteristics – the proportion of low-educated, non-Swiss and male workers and the mean age – of the nonrespondents and subgroups of respondents according to different survey modes, weighted data and the total (respondents and nonrespondents combined). Among the respondents 14% are low-educated, 35% are non-Swiss and 83% are male, and they have a mean age of 47.3 years. In contrast, among the nonrespondents there are more than twice as many low-educated workers (36%), and slightly more workers have a foreign nationality (38%) or are male (89%). In addition, nonrespondents have a significantly lower mean age (41.6) than respondents. Thus, there seem to be substantial differences between respondents and nonrespondents, regarding their age and their level of education.

Did the use of a mixed-mode approach and multiple contact attempts reduce the differences between respondents and nonrespondents? Participants who responded on the Internet were somewhat younger (45.5), less likely to be low-educated (8%), and more likely to be male (87%) as compared to participants who answered the questionnaire on paper. The workers who responded after the first or the second mailing by means of the paper-and-pencil questionnaire are similar: they have a

mean age of 47.7 and 48.3, 15 % and 13 % respectively are low-educated, and 82 % and 83 % respectively are male. They differ only regarding the proportion of workers with foreign nationality, 31 % and 42 % respectively being non-Swiss. In contrast, differences are noteworthy with respect to respondents who answered the questionnaire by telephone: this specifically targeted group is younger (43.4), more likely to be low-educated (38 %), much more likely to have a foreign nationality (73 %) and much less likely to be male (68 %).

If we examine the weighted survey data we find that it is very similar to the unweighted survey data (respondents): the mean age is 46.9, 14 % are low-educated, 37 % are non-Swiss and 84 % are male. Accordingly, the weighted sample strongly differs from the group of the nonrespondents, most of all in terms of age and education. This suggests that the nonrespondents' weights did not strongly adjust for nonresponse and thus failed in their purpose. The quality of the nonresponse weights depends on the available data (Groves and Peytcheva 2008). In our case, the socio-demographic variables most strongly affecting nonresponse such as age, education, nationality and sex were not available for all workers and therefore we could not appropriately correct for nonresponse. Finally, if we collapse the respondents and the nonrespondents into one group (total), the mean age is 45.7, 17 % are low-educated, 36 % have a foreign nationality and 84 % are male.

Our results suggest that the use of telephone interviews and the register data provided the highest contribution to the nonresponse adjustment. In contrast, the other two survey modes of Internet and paper-and-pencil, as well as the weighting did not contribute, even though the former two helped to substantially increase the response rate.

We now turn to the evaluation of the measurement error by comparing the survey data with the register data. We use the register data as "true" values since they rely on official documents and recordings. The difference between these two types of data and the survey data is thus considered as the measurement error. For all the analysis we only use those cases in our database for which both register and survey data are available. Following Bound et al. (1994), we report the mean pre-displacement wage¹³ for both datasets and the difference between the survey and the register mean, which is defined as mean measurement error. However, since in the register data the workers' wage is top-censored at CHF 10,500 for policy reasons, we limit the analysis of the measurement error to wages up to this amount in both databases.

The mean wage of the survey data is CHF 6283 with a standard deviation of CHF 1580. For the register data we observe a mean wage of CHF 6268 with a standard deviation of CHF 1593. The box-and-whisker plot in Fig. 2.5 complements these results by indicating the distribution of the data. The horizontal line in the middle of the gray box represents the median (50 % percentile), the lower hinge of the box the 25th percentile and the higher hinge the 75th percentile. The two boxes are almost identical, suggesting that the distribution of the wages is highly similar. The single

¹³We use a wage measure that includes a 13th monthly salary for both survey and register data.

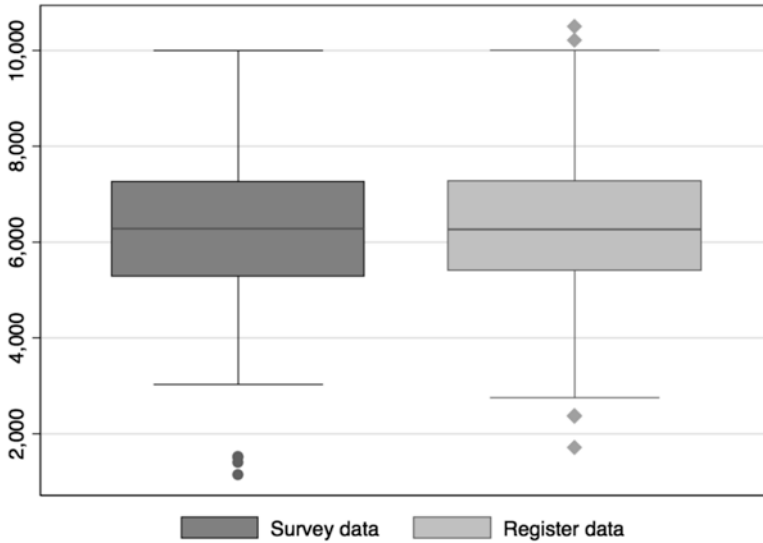


Fig. 2.5 Box-and-whisker plot for pre-displacement wages according to survey and register data. N = 150

exceptions are some outliers which are represented by the dots located outside of the whiskers¹⁴ in Fig. 2.5.

The mean of the measurement error – the difference between the survey data wage mean and the register data wage mean – is CHF 103 with a standard deviation of CHF 1054. Thus, on average the measurement error is less than 2%, which indicates that our survey measured the pre-displacement wage accurately. The distribution of the measurement error is presented graphically in Fig. A.1 in the Annex. The illustration shows that most errors lie close to zero, but that we are confronted with a small number of large errors. This finding is expressed by the large standard deviation of the measurement error.

These findings lead us to the question whether the error in measurement substantially influences the outcomes of our study. In order to test this question we run two OLS-regressions where we measure the effect of age, sex, nationality and education on workers' pre-displacement wage, first based on survey data and then based on register data. Again, we only use those cases in our database for which we have both survey and register data at hand. The results are presented in Table A.1 in the Annex. We find statistically significant effects for exactly the same characteristics, in particular an advanced age, sex and tertiary education. Regarding the size of the effect we find differences that are mostly small except for some characteristics such as tertiary education, where they are of the order of about 15% and thus noteworthy. Overall, Table A.1 suggests, however, that measurement error does not constitute a major problem of our survey.

¹⁴The whiskers represent the short horizontal lines above and below the colored box.

It is worthwhile to improve the quality of the data within the bounds of possibility. One approach is to drop outliers, another to replace survey data with register data whenever both data sources are available (what we call “combined data”). Both approaches involve some problems. Dropping outliers relies on having a precise definition of when a value is an outlier. It also reduces the sample size and thus the statistical power of the analysis. Likewise, by replacing survey data with register data, we assume that register data is more reliable than survey data, which may not always be true. In the case of our study, we have the additional problem that we have two data sources for a maximum of only 190 workers and thus cannot assess measurement error for all workers who responded to the survey.

2.6 Constructing a Non-experimental Control Group

Scholars studying job displacement have pointed out that the causal effects of job loss can only be fully understood if displaced workers are compared with non-displaced workers (see e.g. Jacobson et al. 1993). The analysis of wage losses requires information about the counterfactual earning path since the workers’ earnings would probably have increased if they had not been displaced (Fallick 1996: 9). Likewise, the reemployment prospects of displaced workers must be compared with those of non-displaced workers among whom – about 2 years later – some may have lost their job or gone into early retirement. Inclusion of a control group in a job displacement study thus allows us not only to compare outcomes before and after displacement, but also to compare the outcomes of displaced workers with the hypothetical situation in which they had remained in their former job.

The study of causal effects ideally builds on data from randomized experiments.¹⁵ Since randomized experiments often cannot be implemented in the social sciences, quasi-experimental techniques have been developed (Dehejia and Wahba 1999: 1053). One of these is difference-in-difference, an approach that aims at comparing the evolution of outcomes between two groups, one of which has undergone a particular treatment while the other has not (Angrist and Pischke 2010: 14). The idea behind this technique is that a potential difference in the outcomes can be attributed to the treatment – the so-called treatment effect (Caliendo and Kopeinig 2008: 32–4).¹⁶

¹⁵Random attribution to the treatment or the control group theoretically provides researchers with two identical groups regarding the individuals’ observed and unobserved characteristics. This setting allows comparison between the outcome of the treated and the non-treated individuals. Thereby the outcome of the control group simulates the counterfactual – the outcomes that hypothetically would have been observed if the treated individuals had remained untreated. In the absence of the treatment it is assumed that the outcome would be the same for both groups.

¹⁶The parameter that is estimated is the average treatment effect on the treated (ATT). It is defined as the difference between expected outcome values with and without treatment for those who actually received a treatment. It is given by $\beta = E[Y(1) - Y(0) | Z = 1]$, $Y(0)$ standing for the outcome without, $Y(1)$ for the outcome with treatment, and Z for the treatment.

The post-hoc construction of a control group proceeds by pairing the individuals in the sample – the treated individuals – with untreated individuals who are similar on observable characteristics (Brand 2006: 277). This procedure is called matching and simulates a random attribution of individuals to either the treatment or the control group. Accordingly, this technique is based on the assumption that the control group is different from the treated group only with respect to the treatment.

Since exact matches on relevant characteristics such as age, education or occupation are hard to find, Rosenbaum and Rubin (1985: 34) introduced the propensity score matching method. The propensity score is a function that describes the individuals' propensity to experience the treatment event given their characteristics (Rosenbaum and Rubin 1983: 43). Propensity score matching is used in particular when the matching is based on multiple covariates and when the sample is small. Since the estimation of the propensity score relies on *observable* covariates, this technique involves the strong assumption that the attribution to the treatment was based on observables (Dehejia and Wahba 1999: 1053).¹⁷

For our study, we construct a control group of non-displaced workers. The control group provides us with counterfactual information and thus with more precise estimations of the change in wages and the employment rate which workers experience as a consequence of plant closure. We construct the control group on the basis of data from the Swiss Household Panel (SHP). This database contains information on almost 10,000 individuals from about 4000 households and offers a large range of variables relevant for the study of the labor market. We use two waves of data from 2009 – the year when (most of) the workers in our sample were displaced – and 2011 – the year when the treated individuals were surveyed.

Using data from the SHP may raise methodological issues. First, the sampling is done at the level of the household and not the individuals. Second, attrition in the SHP does not occur randomly (Voorpostel 2010: 372). Individuals who remain longer in the sample, and thus are less prone to attrition, are more likely to be female, married, older and higher-educated individuals. Accordingly, we need to keep in mind that the SHP consists of a selected group of people.

We chose a binary model distinguishing between displacement by plant closure (treatment) and no displacement (no treatment). Our specification includes workers who were employed in 2009, which results in a number of observations of 4601. A more in-depth description of the control groups can be found in the dissertation which is the basis for this book (Baumann 2015).

¹⁷Formally, the matching estimator can be described as $\Pr(Z=1 | X)$, rather than x as is the case in other matching techniques. X is the vector of covariates for a particular individual and Z indicates whether the individual was exposed ($z=1$) or unexposed ($z=0$) to the treatment. The treated and the controls are selected in such a way that they have the same distribution of x . The matching process relies on two further assumptions (Caliendo and Kopeinig 2008: 35). First, the covariates that are included in the model are chosen based on the conditional independence assumption (CIA). Second, the common support assumption affirms that the individuals' (pre-treatment) characteristics do not perfectly predict attribution to the treatment. This condition guarantees that individuals with the same characteristics can be in both the treatment and the control group.

2.7 Limits

Our sampling strategy is convenience sampling, an approach that has its limitations (Lohr 1999: 5). In particular, the data are not generated by a known probability mechanism such as random sampling and therefore do not allow inferences from the sample to the entire population (Western and Jackman 1994: 412; Berk 2004: 51). Focusing on the manufacturing sector, our results are probably not generalizable to other sectors. For instance, workers in this sector are on average somewhat more likely to have completed an apprenticeship than in other sectors.

However, within the manufacturing sector the composition of the workforce of the plants in our sample is similar to that of other firms. This suggests that our results can be generalized for manufacturing workers. Nevertheless, since we sampled only from closed plants in the manufacturing sector, inference on the entire manufacturing sector has to be made with caution and accordingly the significance level should be read with reservations. A more conservative interpretation of our findings would be that they are the results of a case study. However, since no database was available from which we could have drawn a random sample, a procedure corresponding to the standards of the art would have been very costly, both in terms of time and money. In addition, our survey follows an established tradition of plant closure studies which analyze single firms (Kriechel and Pfann 2005; Trotzier 2005; Jolkkonen et al. 2012).

A problem that is linked to the sampling method and the incomplete randomness of experiencing a plant closure is treatment effect heterogeneity (Cha and Morgan 2010: 1141; Burda and Mertens 2001: 22–24). Treatment effect heterogeneity describes the problem that individuals differ not only in terms of socio-demographic characteristics and therefore in their propensity to experience a treatment (pre-treatment heterogeneity), but also in how they are affected by a particular treatment (Brand and Simon Thomas 2014). If treatment effect heterogeneity is at work, average treatment effects can vary widely depending on the socio-demographic composition of the treated and simple averages do not have a straightforward interpretation. Solutions to this problem require data on treated and non-treated individuals which can be linked by means of propensity score matching. However, since it is impossible in the large Swiss surveys to identify workers who have experienced plant closure, we were not able to assess potential treatment effect heterogeneity.

A third limit of this study is the absence of proper longitudinal data. Our use of retrospective measures to assess workers' occupational situation and life satisfaction before they lost their job is definitely valuable in order to examine within-individual changes. However, it is clearly only a second-best solution (Hardt and Rutter 2004). It is widely accepted in the literature that retrospective measures are biased. Some measures can be assessed more correctly than others by retrospective assessment. Unfortunately, accuracy seems to be comparatively low for psychosocial indicators such as subjective well-being – an important measure in our study (Henry et al. 1994). Accordingly, data reliability could be significantly enhanced if repeated survey waves and thus panel data on displaced workers in Switzerland were available.

2.8 The Institutional Context of the Swiss Labor Market

Several institutions affect how displaced workers experience the occupational transition after job loss: the employment protection legislation shaping the procedure of dismissal, the unemployment insurance, the retirement regulations, the skill regime and the overall labor market situation. Although the employment legislation regarding individual dismissals in Switzerland is comparatively weak and termination pays are not required,¹⁸ plant closures are regulated more strictly (OECD 2013: 78, 85). Plant closures are a form of collective dismissal that is legally defined as a displacement of more than 10% of the workforce for reasons not related to individual workers.¹⁹ Firms that undertake a collective dismissal are obliged to announce the layoff at least 1 month in advance to their personnel and to the cantonal employment office. At the same time, the company has to inform its workforce about the number of displaced workers, as well as the reason and the date of displacement. Furthermore, the company has to offer the workers the opportunity to negotiate a potential redundancy plan and a strategy to avoid displacements. Consequently, plant closures in Switzerland are usually accompanied by collective negotiations over redundancy plans. However, in 2009 and 2010 there was not yet a legal – although there was an informal – obligation for the companies to offer a redundancy plan.²⁰

While employment protection in Switzerland is low, the unemployment insurance is comparatively generous (Schwab and Weber 2010). All employed workers are compulsorily enrolled in the unemployment insurance. Workers who contributed at least 12 (18) months within the preceding 24 months are entitled to a benefit period of 12 (18) months. The replacement rate is 70% of the last six salaries (80% for workers with low income and job seekers with children). For workers aged over 54, a contribution period of 24 months provides them with access to unemployment benefits for 24 months. Workers who become unemployed 4 years or less before their regular pension age are entitled to a total benefit period of two and a half years. Workers under the age of 25 receive unemployment benefits for a maximum of 10 months. Additionally, all workers who receive unemployed benefits are monitored and have access to a range of active labor market measures.

In Switzerland the legal retirement age is 65 for men and 64 for women. A full pension requires a contribution of at least 44 years for men and 43 for women (OECD 2011: 310–1). The pension system has a redistribution part, a savings part – both mandatory –, and a voluntary provision. Early retirement is possible 2 years before the official retirement age but implies a reduction of 7% of pension benefits for each year of early retirement. Thus the governmental pension system offers little

¹⁸During the first year of employment, the notice period of dismissal is 1 month, during the second to the ninth year 2 months and thereafter 3 months.

¹⁹See Swiss Code of Obligations Art. 335d-k.

²⁰This legislation changed in June 2013: Art. 335i in the Code of Obligation now requires that plants employing more than 250 and displacing more than 30 workers negotiate a redundancy plan.

incentive to retire early. However, if redundancy plans with early retirement schemes are available, this option may be convenient.

In 2011, 35 % of the residents of Switzerland aged 25–64 held a tertiary degree, 50 % an upper secondary or post-secondary non-tertiary degree, and 15 % had less than upper secondary education (OECD 2014). As compared to other countries, the share of individuals with tertiary education is higher than in Germany (28 %) or Austria (19 %) but lower than in the UK (39 %) or the US (42 %). The share of individuals with upper secondary education is higher in Germany (58 %) and Austria (63 %) and lower in the US (46 %) and the UK (37 %) than in Switzerland.

Among the young people finishing compulsory school, about two-thirds enroll in vocational education and training (VET) (SERI 2015: 4). Most VET programs consist in a dual vocational education combining workplace-based training with school-based general education; a minority of young people attend exclusively school-based VET programs (Fuentes 2011). After 3 or 4 years, students graduate from their VET program with a standardized certificate. Students who have additionally completed a Federal Vocational Baccalaureate may enroll in tertiary-level professional education and training (PET) which prepares them for specialized technical and managerial positions. The VET and PET training systems are strongly oriented towards the demand for skills in the labor market. Both systems are collectively organized by the state, the cantons and employers' organizations and trade unions; vocational schools and host companies monitor the quality of the training. Switzerland's educational system is also standardized with respect to tertiary education. About a quarter of all young people graduating from compulsory school enroll in such training (Helbling and Sacchi 2014: 2). A school-leaving exam (Matura – comparable to the Abitur in Germany) provides access to tertiary education institutions for all students.

Referring to the classification of educational systems developed by Allmendinger (1989), Switzerland's vocational training system is highly standardized on a national level. It comprises both training protocols that set the quality standards of the educational system and a procedure of skill certification in a similar way as in Germany (see Dieckhoff 2008: 94). Allmendinger (1989: 240) argued that standardized certificates serve employers as screening devices for workers' skills before they hire them, in contrast to non-standardized educational systems where workers have to be screened on-the-job. As a consequence, in less standardized educational systems newly hired workers are more likely to lose their job again. In Switzerland in contrast new job matches are likely to be relatively stable and occupational transitions comparatively smooth.

In terms of the skill production regime typology developed by Estevez-Abe et al. (2001), Switzerland corresponds approximately to the firm- and industry-specific type, characterized by a large proportion of workers with vocational training. The classification of Switzerland in the specific-skill regime contradicts Estevez-Abe et al. (2001: 8) insofar that those authors expect the workers to only invest in firm-specific skills if employment protection is high – which is not the case in Switzerland. Nevertheless, since low employment protection is cushioned by relatively generous unemployment benefits, a high proportion of workers in Switzerland

seem to be willing to invest in specific skills. In line with the argument by those authors, high unemployment benefit replacement rates and long benefit entitlement durations may provide displaced workers with the conditions to find a new job that matches their skills.

2.9 Aggregate Unemployment

The plant closures that we sampled occurred in the context of a rather low but rising unemployment rate in Switzerland. At the beginning of the financial crisis, in 2008, the unemployment rate was 3.4%.²¹ It then increased to 4.3% in 2009 and to 4.5% in 2010 and fell to 4.0% in 2011. Four of the plants in our sample were located in German-speaking regions where the unemployment rates were close to the national average. One plant in contrast was located close to Geneva in French-speaking Switzerland where unemployment rates were higher in this period at about 7%.

Regional differences in aggregate unemployment – which seem to be stable over time – are a much-debated issue among Swiss scholars (Flückiger et al. 2007: 57). First, it has been observed that unemployment levels are generally higher in French- or Italian-speaking cantons than in German-speaking cantons (Flückiger et al. 2007: 60; Brügger et al. 2007). This result is due to both higher inflows and lower outflows of unemployed workers in the Latin (French and Italian-speaking) regions. A possible explanation for this pattern may be cultural differences in attitudes toward work as residents of the Latin-speaking regions consider work less important and are more favorable in votes to restrict working time than those of German-speaking regions (Brügger et al. 2009). Second, the demographic structure of the active population varies by canton (de Coulon 1999). Third, cantonal tax policies and the presence of foreign workers seem to contribute to the differences (Feld and Savioz 2000).

These factors may explain one of the main findings of this study, namely that workers from Plant 1, located in French-speaking Geneva, were substantially less likely to return to employment than workers in Plants 2–5, located in German-speaking Switzerland. More precisely, due to its proximity to the French border – and thus the greater competition among job seekers – and the higher prevalence of older workers, workers from Plant 1 may have experienced much more difficulty in finding a new job than workers from other plants (Flückiger and Vasiliev 2002: 407).

It should also be noted that deindustrialization progressed slowly over the period of our study: in relative terms the share of manufacturing employment decreased from 19.3% in 2008 to 18.2% in 2012. In absolute terms manufacturing employment was rather stable after 2009. Measured in full-time equivalents, manufacturing employment decreased from 661,000 in 2008 to 629,000 in 2009 and 626,000 in

²¹ Rates according to the ILO definition of unemployment and based on the Swiss Labour Force Survey.

2010, before recovering in 2011 and 2012, when the figure increased again to 633,000 and 636,000.²²

Although Switzerland does not belong to the European Union, its economy and labor market do not constitute a case *sui generis* as is often assumed. In fact, the Swiss economy shares many common features with Austria and Southern Germany, in particular a strong reliance on vocational education, a resilient manufacturing sector and low levels of unemployment. As an illustration, in 2011 the unemployment rates in the adjacent *Bundesländer* of Austria and Germany were lower than in Switzerland – with below 3.5 % in Western Austria (comprising Oberösterreich, Salzburg and Tirol), 3.3 % in Bavaria and 3.6 % in Baden-Württemberg.²³ It may thus be expected that a survey on plant closure in Salzburg, Stuttgart or Munich would produce comparable results to the ones presented here.

In this chapter we have discussed how we collected our data and the extent to which we are confronted with survey bias. In a nutshell, our analysis led to two main findings: first, the use of a mixed-mode approach – and in particular telephone interviews – helped to decrease nonresponse bias. Second, although our data does not seem immune to measurement error, the use of different sources, i.e., register data, improves the quality of the data. Overall, applying a tailor-made survey design substantially contributed to addressing survey bias.

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²² Source: Swiss Federal Office of Statistic, BESTA/STATEM statistics. Data for third semester.

²³ Source: Eurostat (accessed on May 5, 2015): <http://appsso.eurostat.ec.europa.eu/nui/show.do>

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