Objects in the mirror are closer than they appear: Unemployment, retrospective error, and life satisfaction

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Abstract (100 words): I compare current and one-year retrospective data on unemployment in the GSOEP. 17 percent of all unemployment spells are not reported one year later, and another 8 percent are misreported. The ratio of retrospective to current unemployment (as a measure of unemployment salience) has increased in recent years and it is related to the loss in life satisfaction associated with unemployment. Individuals with weak labour force attachment, e.g. women with children or individuals close to retirement, have the largest propensity to underreport unemployment retrospectively. The data are consistent with evidence on retrospective bias found by cognitive psychologists and survey methodologists.

Keyword: Unemployment, Retrospective Bias, Salience, Life Satisfaction

JEL-Codes: C81, J64

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1. Introduction

In order to study the dynamics of labour force participation empirically one needs detailed information on an individual's labour force status over time, ideally on a monthly or even weekly basis. Since a monthly or weekly collection of data is too costly under most circumstances, survey researchers often rely on the collection of retrospective data. For example, respondents are asked to report transitions between labour market states that happened during a specific reference period. Another option is to use calendars and ask respondents to report labour market states retrospectively for each sub-period (e.g. month) within a specific reference period.

Retrospective data, however, is likely to be inaccurate in several ways. Respondents might simply forget events that researchers are interested in but that are not important to the respondent, or not important anymore at the time of recall. For example, a respondent may have been unemployed for a short period between two jobs and does not remember that short period one year later. Respondents might also consciously or unconsciously re-define their past. It is surprisingly common for women, for example, to claim they have been housekeepers although at the time they said they were unemployed.

Most studies that have been conducted so far on the reliability of retrospective data have analysed differences in reports of unemployment between the U.S. Current Population Survey (CPS) and its annual supplement on work experience, the Work Experience Survey – WES (Akerlof and Yellen 1985, Horvath 1982, Morgenstern and Barrett 1974). The CPS is a monthly sample survey conducted by the U.S. Census Bureau for the Bureau of Labor Statistics. Data from the CPS are used to obtain monthly estimates of US unemployment levels. The annual work experience is asked of the March sample only. The supplement includes questions about work activity during the prior calendar year. As the CPS is a repeated cross-section, comparison of retrospective and current data for the same individual is

not possible. Moreover, the WES only asks respondents to report the number of weeks that they were unemployed, not in which months they were unemployed. Comparisons are mostly done on an aggregate level for specific socio-economic groups, that is by creating a quasi-panel. The results suggest that unemployment is underreported by some 20 percent and that retrospective bias is larger for spells in the first six months of the year than for the last six months, lending support to the claim that the length of the recall period is important for recall accuracy (Horvath 1982).

Mathiowetz and Duncan (1988) use PSID validation data and compare individual respondent reports with company records. They find that a stunning two thirds of spells remain unreported. A strong relationship exists between spell length and the degree of underreporting.. Long-term unemployment is much more easily remembered than short spells. In contrast to the CPS-WES comparison studies they find that the length of the recall period is of minor importance.

Paull (2002) uses overlaps in retrospective information in the British Household Panel Survey (BHPS). Each year, respondents are asked to report changes in labour market states since September 1st of the previous year. Depending on the interview date, this results in overlapping report periods of up to nine months. Paull finds a considerable degree of inconsistency in the reporting of unemployment spell starts and ends between the two reports. The pattern of recall errors in the BHPS is similar to results using different data and different methods: for instance, fewer spells are reported as the recall period lengthens and women tend to re-define unemployment as time out of the labour force.

The studies mentioned so far deal with one year recall. Longer recall periods are analysed in Elias (1997). He compares unemployment rates calculated from nine year employment biographies reported in the second wave of the BHPS to corresponding (current) unemployment rates in the British Labour Force Survey. Underreporting begins to become a

serious problem if a spell dates back more than three years. Again, this holds in particular for females.

In this paper I will study retrospective bias regarding unemployment in a large-scale German panel survey (GSOEP). As described below in more detail, the GSOEP uses monthly calendars to elicit retrospective data on labour force participation. These data are often used to generate spell data for event history or duration analyses. While many microeconometric studies of labour market behaviour in Germany rely on this data (e.g. Hunt 1995, Hujer and Schneider 1989), the quality of this retrospective data and its implications for the analyses is yet unclear. Potential problems are often ignored or dealt with in a rather ad hoc manner. For example, retrospective data that is collected repeatedly in the form of calendars, often suffers from a particular "seam problem", that is one finds spurious transitions between calendars collected in subsequent years (Kraus and Steiner 1998, Wolff and Augustin 2003). Compared to official (unemployment register) data of unemployment, the GSOEP overstates entries into unemployment in January and overstates exits in December. This seam problem, also known as “heaping”, is an artefact of using a calendar to aid recall.

Considering the fact that many applications in labour economics rely on non-linear methods, measurement error of the dependent variable (e.g. the length of an unemployment spell) can potentially bias the results. Although there may be no alternative to retrospective information, it is still useful to know which factors influence retrospective error and how bias due to retrospective error can be minimised.[1]

In the following I will study retrospective bias by comparing reports on being currently unemployed in a specific month with the retrospective calendar data on unemployment in that same month – reported one year later. The analysis is thus similar to

[1] Kraus and Steiner (1998) suggest to use external validation information to deal econometrically with retrospective errors in the GSOEP calendars.

the CPS-WES comparisons. The GSOEP also contains another source of information on labour market status. Respondents are asked each year if their employment status has changed since January 1st of the previous year and if so, how has it changed and for which reasons. Although it would thus be possible to study retrospective errors by exploiting overlaps in recall periods – as Paull (2002) does with BHPS data – I ignore this type of retrospective information and focus on the calendar data.

The main hypothesis tested in this paper is that the degree of under-reporting in retrospective data is inversely related to the salience, importance or "painfulness" of the unemployment experience to the unemployed. Changing degrees of underreporting can then be interpreted as a changing salience of unemployment This hypothesis was first formulated by Akerlof and Yellen (1985) and demonstrated using a CPS-WES comparison. The two innovations of the current paper compared to Akerlof and Yellen are (1) the use of true panel data, which enables us to study recall at the individual level, and (2) the possibility to provide a more direct test of their hypothesis by looking at the relationship between general well-being at the time of unemployment and recall of unemployment one year later. It can be shown that lower life satisfaction levels while being unemployed are related to better individual recall, and that the ratio of retrospective to current unemployment is larger when the difference in life satisfaction between the unemployed and others (i.e. the loss in well-being from unemployment) is larger. Such information might also be of interest to labour market politics, because it suggests to use rates of "recalled unemployed" as an additional measure of unemployment that takes into account the subjective importance of the experience.

The paper is organised as follows: Section 2 describes the data and how current and retrospective data are compared. Section 3 contains the results of the empirical analysis. Section 4 concludes.

2. Data

The data used in this study are derived from the German Socio-Economic Panel Study GSOEP and cover the period from 1985 to 2001 (for a description of the data see SOEP Group 2001). The sample is restricted to respondents aged 20 to 59 in both East and West Germany. After age 59, Germans are usually not unemployed merely by definition. At age 60, unemployed men are eligible for early retirement and unemployed women are eligible for regular old-age retirement. The total number of observations is approximately 140,000.

Current unemployment is asked for with a simple yes/no-question: "*Are you officially registered as unemployed at the Employment Office (Arbeitsamt)*?".[2] Registration at the Employment Office is a necessary condition to receive unemployment benefits. The officially published unemployment rates in Germany are based on the number of registered individuals who fulfil certain criteria (looking for employment, available to work). Note that this definition of unemployment differs from the ILO definition (not working, available to work, actively seeking employment) used in many labour force surveys. Data on current unemployment is only available for interview months. The GSOEP field period usually covers January to October, with more than 80 percent of the interviews being conducted until the end of April.

Retrospective data on unemployment is derived from the employment calendars: "*And now think back on all of <preceding year>. We have drawn up a type of calendar below. Listed on the left are various employment characteristics that may have applied to you last*

[2] Data from 1984 – the first year of the GSOEP – had to be dropped because unemployment was measured differently. There was no separate question on unemployment. Rather, "being unemployed" was listed among other labour market states, with no opportunity of giving multiple responses. Since it is possible to work for less than 15 hours per week and be registered unemployed, the data in 1984 is likely to be inaccurate.

year. Please go through the various months and check all the months in which you were employed, unemployed, etc. Please note that one must be checked for each month! ***Even if you were unemployed for less than one month, please check off that month***." [bold letters not in original]. The employment characteristics listed in the calendar are shown in Table 1.

<Table 1 about here>

Most labour market analyses using the GSOEP use calendar data to construct labour market spells. In principle, retrospective error should be low. First, the recall period is rather short (on average one year) and the question format (calendar) is an established way to aid memory (Eisenhower et al. 1991). Even the shortest spell of unemployment can be reported. Second, being registered as unemployed is a legal status, not a subjective state of which the perception can change over time. In principle, there should not be much scope for retrospective bias due to re-interpretation of the past (e.g. "I was not *really* looking for work"). The possibility to study how well respondents recall "objective", i.e. legal, employment states is an advantage over comparisons of CPS and WES, which relies on self-perceptions of the respondents.

<Figure 1 about here>

The fundamental comparison in this paper is between the current data and the calendar data collected one year after. Figure 1 describes the structure of the data. For example, a respondent might have been interviewed in February 2002 and January 2003. In February 2002, two types of information on unemployment were collected: (1) retrospective data on unemployment in all of 2001 and (2) current data on unemployment in February 2002 (strictly speaking on the day of the interview in 2002). In January 2003, the same type information was collected but for one year later: (1) retrospective data on unemployment in all of 2002 and (2) current data on unemployment in January 2002. The information overlap that allows a comparison between current and retrospective data is thus between current unemployment in

February 2002 and recalled unemployment in February 2002 (reported in January 2003). Throughout this paper, I will assume that current data reflects an individual's true labour market status.

<Figure 2 about here>

How does retrospective information on unemployment compare to reports of current unemployment one year earlier? Figure 2 shows the development of current and retrospective unemployment rates over time, by sex and region.[3] Retrospectively reported unemployment follows current unemployment closely over time but it is always below current unemployment. The gap between current and recalled unemployed seems to get smaller towards the end of the observation period for all types of respondents, in particular among East Germans. Following Akerlof and Yellen (1985), this could be interpreted as evidence for an increasing salience or "painfulness" of unemployment, which could for example be caused by a larger proportion of long-term unemployed or reductions in unemployment benefits.

The comparison of current and retrospective information on unemployment results in four possible cases with two types of misclassification: *false negatives* and *false positives*. (see Table 2).

<Table 2 about here>

A *false negative* occurs when a respondent fails to report having been unemployed in a month although he reported being unemployed when interviewed in that month. Overall, some 25% of all reports of current unemployment have no match in the following wave's

[3] Note that the unemployment rates in Figure 2 are not adjusted for seasonal effects. This might affect comparisons over time and between regions. Between 1985 and 2001, the median interview day shifted from March 26th to February 19th in West Germany, and from April 3rd to February 11th in East Germany.

calendar. A *false positive* occurs when a respondent retrospectively reports to have been registered unemployed in a specific month, although he did not report being currently registered unemployed in that month. Unsurprisingly, the proportion of false positives is much lower than the proportion of false negatives (less than 1 percent). In the following section, I will examine which type of respondent is likely to be a false positive or negative.

3. Results

3.1. Retrospective bias as a measure of salience

According to cognitive psychology, the accuracy of recall, not only in surveys, depends on three major factors, which are not necessarily independent of each other: interference, length of recall period, and salience (or importance) of the event to be reported (Eisenhower et al. 1991). Interference means the occurrence of many similar or related events that reduce the memorability of each single of these events. Stated differently, rare events are more easily remembered than frequent events. As memory decays over time, the probability of accurate recall generally decreases with the length of the recall period. However, decreasing the recall period will not always reduce recall bias. If the recall period becomes too short, respondents have a tendency to "telescope" rare events into that period, which gives rise to over-reporting. Salience basically means how important an event is to the respondent. Salient events are usually rare, have large economic or social costs or benefits and they have continuing consequences. More salient events are remembered more easily than less salient events, with the exception of traumatic or threatening events. The literature surveyed in Akerlof and Yellen (1985) provides ample evidence for this relationship. For example, in the area of health, hospitalisation tends to be less underreported the longer the stay, or own diseases tend to be remembered more than family members' diseases. Women are more likely

to remember purchases of clothing while men remember purchases of tires. Voters are more likely to remember how they have voted in presidential elections than in local elections, etc.

Based on this psychological evidence, Akerlof and Yellen (1985) suggest that spells of unemployment that are remembered must have been more salient or painful than spells of unemployment that are forgotten or that remain unreported. The difference between (aggregate) retrospective and current data on unemployment can thus be used as a measure of the salience or painfulness of unemployment. Akerlof and Yellen compare unemployment rates between the CPS and the WES over time and across groups of individuals. Since the WES (retrospective) unemployment rate has decreased steadily relative to the CPS (current) unemployed rate between 1960 and 1981, particularly for younger and older persons, Akerlof and Yellen conclude that unemployment in the U.S. has become less salient during that period. Another finding is that unemployment is less salient for women than for men.

<Figure 3 about here>

In this subsection I present similar evidence from the GSOEP. Figure 3 shows the development of the ratio of recalled to current unemployment, separately for males and females in East and West Germany. The higher this ratio, the smaller the amount of underreporting and the more serious or salient is the experience of unemployment. Several observations are worth noting. First, there is an obvious sex difference in salience in West Germany. As in the U.S., men are considerably more likely than women to report unemployment in the previous year. Note, however, that there is no such sex difference in East Germany. This observation fits nicely with the fact that the labour force attachment of East German women is stronger than the labour force attachment of West German women. Unemployment is thus a more serious problem for women in the East. Second, unemployment salience seems to be on the increase, slowly in West Germany, much faster in East Germany, leading to an overall convergence between West and East. The increase in salience suggests

that unemployment might have become more costly in economic and social terms, or that the effects of unemployment are felt longer than in previous years.[4]

If, following Akerlof and Yellen's interpretation, unemployment has become a more painful experience, at least psychologically, during the past one or two decades. Other, more direct measures of well-being, such as life satisfaction should show a similar trend. Fortunately, this kind of data is available in all years of the GSOEP. Putting together data on retrospective bias and psychological well-being over time enables us to test the Akerlof and Yellen-hypothesis.

Unemployment has a strong and lasting negative relationship to well-being (see e.g. Winkelmann and Winkelmann, 1998, Clark et al. 2001). An aggregate measure of this relationship is the difference in average "happiness" between the unemployed and those who are not unemployed. Figure 4 shows the proportion of "happy" respondents (defined as respondents with a general life satisfaction index higher than 7 on the GSOEP's 0-to-10 scale) by employment status. The figure contains several interesting findings. First, East Germans report much lower levels of life satisfaction across all sub-groups. Second, for non-unemployed East German respondents, the life satisfaction gap to West Germans is gradually closing. Third, the unemployed are less happy than others in across all sub-groups. The percentage difference is smaller for West German women than for West German men, and seems to be about the same for East German men and women.

< Figure 4 about here >

[4] See the literature on unemployment scarring (e.g. Arulampalam 2001, Gregory and Jukes 2001, Clark et al. 1999). An alternative explanation for the secular increase in salience are panel effects: Respondents might become more familiar with the instrument and try to answer more accurately.

How does the difference in happiness between the unemployed and other respondents relate to the prevalence of retrospective error (or salience)? In order to analyse this relationship in more detail, I have stratified the sample by eight age groups (20-24, 25-29, ..., 55-59) in addition to sex and region, which results in a total of 32 different groups.[5] For these groups I have calculated the average retrospective error and the average life satisfaction differential in each year.

< Table 3 about here>

Table 3 shows within group (fixed effects), between group, and random effects estimates of the relationship between the two variables. All estimates are positive and significant, indicating that the positive relationship is driven by cross-sectional as well as time-series variation. A Hausman test rejects the random effects specification at the 10 percent level ($\chi^2(1) = 2.75$). The fixed effects parameter indicates that, on average, the gap between recalled and current unemployment decreases by 1.8 percentage points when the life satisfaction difference between unemployed and other respondents increases by 10 percentage points. This supports to the idea that retrospective bias in unemployment data is related to the salience or importance of unemployment for the respondent.

3.2. False negatives

False negatives occur when respondents fail to report unemployment spells retrospectively. Since this can only happen if someone was unemployed, we confine the analysis to those respondents who – in the previous year – reported to be currently unemployed. As already noted, roughly 25% of all respondents who say the are currently

[5] The average group size is 332, the smallest group consists of 118 observations, the largest group has 695 observations.

unemployed "forget" to report unemployment in that month when interviewed in the next year. The question addressed in this subsection is: who forgets to report unemployment and why?

First of all, we can distinguish different types of false negatives. 68 percent of all false negatives do not report a single month of unemployment in the calendar. I will call this a *hard error* because it has a different quality than *soft errors*, where respondents report to have been unemployment in the previous year but not in the month in which last year's interview took place. In 17 percent of all false negative cases, there is only a one month difference between reported and current unemployment. The remaining 15 percent have reported unemployment in the last year, but that was more than one month away from the last year's interview date.

<Table 4 about here>

The following analysis will concentrate on hard errors. Table 4 shows the results of probit estimates for the probability to recall having been registered unemployed in the previous year. The coefficients are marginal effects, for dummy variables they reflect the effect of a discrete change of the variable from 0 to 1. The six different columns show different specifications, for West and East Germans separately.

Let us begin with the first two column, which contain the basic specification. West German females are less likely to report unemployment retrospectively than West German men. The difference of 3.5 percentage points may seem rather small, but note that the presence of children (who are younger than 16) in the household is controlled for, and how this variable interacts with the respondent's sex. West German females with children are again much more likely to underreport unemployment. This pattern is well in line with the salience interpretation of retrospective bias. In fact more than two thirds (70.7 percent) of all West German women with children who fail to report unemployment say they have been a housewife. Interestingly, this pattern is much weaker for East German women. The effect of

sex is insignificant and the interaction of sex with the presence of children is small and only marginally significant. Only 9.1 percent of the East German women with children who fail to report unemployment say they have been a housewife. This suggests that unemployment is a much more painful experience for East German than for West German women, an observation that can be easily explained by the significance of female employment in the former GDR.

The effect of age on recalling unemployment is rather small. The oldest group (55-59) is most likely to underreport. However, this finding is hardly due to deteriorating memory. In fact, the oldest group is close to retirement, and 52.9 percent of all false negatives in that age group say they have been a pensioner rather than unemployed. East Germans who are not so close to retirement (aged 45-54) seem to suffer most from unemployment as they are least likely to underreport. This could be explained by low re-employment probabilities of this group.

Education, measured in years of education, has a negative – but not always significant – effect on recall, suggesting that the experience of unemployment is less salient for well educated respondents. It is a priori unclear which sign the coefficient should have. One the one hand, better educated respondents might be more frustrated when unemployed because they have invested more in human capital without currently yielding a return. On the other hand, they may be less frustrated because they tend to have better re-employment opportunities. Log per capita household income has no significant effect on recall.

One of the most important determinants of recall is unemployment at the time of recall. Unsurprisingly, respondents who are unemployed at the time of recall have a much higher propensity to remember that they were also unemployed last year. The estimated marginal effects are nearly 19 percentage points in West Germany and about 7 percentage points in East Germany. Many of those who are unemployed in the previous and in the current year may not have worked at all in-between. The regional unemployment rate (at the

time of unemployment) has no effect on recall, i.e. I do not find any effects of reference group unemployment (see Clark 2003).

The recall periods in our sample range from 5 to 20 months, and the modal recall period is 12 months, i.e. most respondents' interviews are exactly one year apart. Given the presumed salience of unemployment, a year seems to be a relatively short recall period. Memory problems are not likely. Still, there is a marginally significant negative effect on recall in West Germany of about 6 percentage points. Extrapolation of this effect to four or five year recall periods (although actually not permissible given the range used to estimate the effect) suggest that recall problems might in fact become serious after several years. In East Germany, the estimated effect is smaller than in the West and insignificant. However, the variance in recall period length is smaller in the East, which might be the simple reason for a less precise estimate.

Let us now turn to two subjective indicators of unemployment salience, measured *at the time of unemployment*. The first indicator is overall life satisfaction (see columns 3 and 4 in Table 4). The results are consistent with the salience interpretation of recall. Those who are less satisfied with life while unemployed, i.e. whose spells appear to be more painful, do remember these spells better. For example, an "unhappy" West German unemployed (with a value of below 5 on the 0-to-10 life satisfaction scale) has a 3.8 percent higher probability of recalling unemployment than a "happy" unemployed (with value greater than 7). In East Germany the difference is 3.0 percent.

The second subjective indicator of unemployment salience is derived from the answers to the question whether a respondent "*intends to engage in paid employment (again) in the future?*" and if yes, "*when, approximately, would you like to start with paid employment?*" Possible answers to the second question were "as soon as possible", "next year", "in the next two to five years", and "in more than five years". I combined the answers to the two questions

to one variable that contains the information whether a respondent does wants to take up an employment immediately, in the next year, in more than a year or not at all. This variable captures the strength of an unemployed's labour force attachment. Weaker labour force attachment means that unemployment is a less salient event, and the smaller the probability of recall.

As can be seen in columns 5 and 6 of Table 4, this variable turns out to have a massive effect on recall. Respondents who claim they want to start employment within the next year have recall probability that is 4.9 percentage points lower in West Germany and 8.8 percentage points lower in East Germany than those who seek work as soon as possible. If employment is envisaged in more than a year, the difference to those who want to take-up work immediately rises to 10.2 and 22.5 percentage points, respectively. Finally, for those who claim they do not intend to get back to work the difference rises to 15.7 and 28.6 percentage points, respectively.

Although the results presented so far are mostly according to expectations and in accordance with psychological explanations of recall, the analysis might still suffer from one problem: the data does not allow to ascertain the true length of the unemployment spell that is to be remembered. Certainly, longer spells are more easily remembered than shorter spells: They are more painful and it is also less likely to just report the wrong month if the reported spell is long rather than short. The problem is that the available information on spell length is in some sense endogenous, because it is usually derived from the employment calendars. For example, a respondent who reports unemployment for the entire 12 months of the preceding year cannot be a false negative by definition. A respondent who reports no unemployment spell at all in the employment calendar will be a false negative with probability one. One possibility to deal with this shortcoming would be an instrumental variable-type approach in

which one uses some estimate of spell length as an explanatory variable. This extension is yet beyond the scope of the current paper.

3.3 False positives

False positives occur when respondents report being unemployed in a specific month although they did not report this when asked in this month. False positives are far less common than false negatives. In theory, false positives would come in three different forms: (1) soft errors, that is wrong spell begins and spell ends, (2) hard errors, that is spells are reported that actually never happened, and (3) pseudo errors. These happen if a respondent was unemployed in a specific month, but not on the day of the interview, so that both the retrospective information gathered in the calendars and the contemporaneous information are correct. Of course, if one takes into account the possibility that current unemployment is measured with errors – which I do not – there are even more types of error.

<Table 5 about here>

Table 5 shows probit regression results for the probability of being a false positive. The explanatory variables are the same as above, except of course future employment plans. Although there are no theoretical arguments concerning the relationship of the explanatory variables to erroneous recall, many variables have surprisingly strong and significant parameters. For example, being unemployed at the time of recall is the strongest predictor for recalling unemployment, education and income (in the last year) are negatively correlated to recall, and low life satisfaction in the previous year is associated with higher probabilities of being a false positive. All these results are actually compatible with the assertion that most of false positives are in fact pseudo errors. Respondents have been unemployed in the survey month last year, but probably not on the day of the interview (or they failed to report it). That would explain the low income and low life satisfaction of those who recall unemployment. It

would also be consistent with less education years of those who recall unemployment, because the less educated are more likely to be unemployed and of course, probably the best predictor of past unemployment is present unemployment.

4. Summary and conclusion

In this paper, I compare current and one-year retrospective data on unemployment derived from 18 years of the German Socio-Economic Panel. Assuming that reports of current unemployment reflect the true labour market status, the data suggests that monthly retrospective data in the GSOEP employment calendars suffers from serious underreporting. About 25 percent of all reports of being unemployed have no match in the calendar completed in the following year. 17 percent of all respondents who said they were unemployed when interviewed in the previous year fail to mention a single month unemployment when asked one year later.

In the first part of the analysis, I study long-term trends in underreporting. Following Akerlof and Yellen (1985), I interpret the ratio between the retrospective and the current unemployment rate as an indicator of the psychological seriousness or "salience" of unemployment. I find that the salience of unemployment has increased for both men and women in East and West Germany. In support of Akerlof and Yellen's salience interpretation, I find that the ratio of retrospective to current unemployment is correlated to the life satisfaction differential between unemployed and non-unemployed respondents, presumably a more direct measure of unemployment salience.

Next, I study retrospective error in the GSOEP employment calendar at the individual level. The analysis of so-called false negatives suggests that respondents with weak labour force attachment are most likely to underreport unemployment. For example, West German women, particularly when they have children, are most likely to not report unemployment

retrospectively. Rather, they have a strong tendency to interpret periods of unemployment as having been a homemaker. This does not hold for East German women. Unemployed respondents who said they want to start employment as soon as possible are much more likely to recall unemployment than others. The unemployed (at the time of recall) remember much more easily that they were unemployed. More painful spells (in the sense that life satisfaction at the time of unemployment was low relative to other unemployed individuals) are less easily forgotten.

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Table 1: Labour force states coded in the GSOEP calendars

Employed	Unemployed	Out of labour force
• full-time employed (including state employment programs) • part-time employed • in occupational training / apprenticeship, retraining, further professional education • in compulsory military / community service	• registered unemployed	• in retirement or early retirement • on maternity leave • in school or university • homemaker • other (specify)

Table 2: Types of retrospective error

	Currently unemployed	
Recalled unemployed	Yes	No
Yes	Correct Positives: (N=7,954)	False Positives (N=745)
No	False Negatives (N=2,615)	Correct Negatives (N=125,648)

Table 3: Regressions of Salience on the Well-Being Differential

	Within Groups	Between Groups	Random Effects
%-Diff. in Life Satisfaction	0.181	0.464	0.237
	(2.34)*	(2.88)**	(3.39)**
Constant	0.793	0.739	0.782
	(46.00)**	(22.67)**	(45.06)**
Observations	416	416	416
Groups	32	32	32

Absolute value of t-statistics in parentheses; * significant at 5% level; ** significant at 1% level

Table 4: Probit estimates of correctly recalling unemployment

	(1)	(2)	(3)	(4)	(5)	(6)
	West	East	West	East	West	East
Female	-0.040	0.003	-0.038	0.002	-0.036	0.015
	(2.63)**	(0.21)	(2.51)*	(0.15)	(2.49)*	(1.15)
Kids < 16 yrs	0.019	0.028	0.022	0.030	0.016	0.020
	(1.08)	(1.39)	(1.22)	(1.47)	(0.96)	(1.09)
Female * Kids	-0.140	-0.042	-0.135	-0.039	-0.100	-0.019
	(5.45)**	(1.74)+	(5.27)**	(1.64)	(4.06)**	(0.89)
Age 20-24	-0.013	-0.025	-0.011	-0.014	-0.019	-0.014
	(0.86)	(1.13)	(0.72)	(0.69)	(1.31)	(0.74)
Age 45-54	-0.025	0.017	-0.027	0.016	-0.019	0.018
	(1.61)	(1.11)	(1.68)+	(1.04)	(1.23)	(1.31)
Age 55-59	-0.104	-0.180	-0.101	-0.172	-0.044	-0.075
	(5.38)**	(7.68)**	(5.27)**	(7.50)**	(2.25)*	(3.75)**
Years of education	-0.004	-0.005	-0.004	-0.005	-0.006	-0.004
	(1.70)+	(1.47)	(1.75)+	(1.73)+	(2.26)*	(1.47)
Log per capita hh income	-0.006	0.008	-0.002	0.011	0.001	0.000
	(0.54)	(0.47)	(0.20)	(0.68)	(0.13)	(0.03)
Unemployed *at recall*	0.188	0.073	0.186	0.071	0.186	0.074
	(16.97)**	(6.32)**	(16.74)**	(6.18)**	(16.68)**	(7.26)**
Regional unempl. rate	0.002	-0.002	0.002	-0.002	0.002	-0.002
	(0.85)	(0.90)	(0.76)	(0.85)	(0.86)	(0.73)
Iviewer not present *at recall*	0.036	-0.001	0.037	-0.003	0.045	0.002
	(2.79)**	(0.09)	(2.83)**	(0.26)	(3.55)**	(0.20)
Recall period (in years)	-0.055	-0.035	-0.068	-0.033	-0.066	0.004
	(1.61)	(0.61)	(1.99)*	(0.58)	(1.92)+	(0.07)
Year	0.009	0.014	0.009	0.014	0.010	0.012
	(8.83)**	(4.38)**	(8.73)**	(4.45)**	(9.35)**	(4.20)**
Life Sat. medium (5-7)			0.037	-0.001		
			(3.45)**	(0.04)		
Life Sat. low (0-4)			0.038	0.030		
			(2.84)**	(1.97)*		
Take up empl.: no					-0.157	-0.286
					(7.57)**	(10.48)**
Take up empl.: > 1 year					-0.102	-0.225
					(4.93)**	(7.47)**
Take up empl.: next year					-0.049	-0.088
					(3.10)**	(5.14)**
Observations	5986	3795	5971	3780	5889	3741
Mean dep. var.	.836	.898	.836	.899	.838	.899
Model Chi-Sq	515.1	173.8	519.3	180.7	571.1	340.4
Model df	13	13	15	15	16	16
Pseudo-R2	.146	.103	.149	.105	.167	.181

Robust z statistics in parentheses; + p < 0.10; * p < 0.05; ** p < 0.01

Table 5: Probit estimates of erroneously recalling unemployment

	(1)	(2)	(3)	(4)
	West	East	West	East
Female	-0.00078	-0.00222	-0.00073	-0.00209
	(1.65)+	(2.27)*	(1.65)+	(2.22)*
Kids < 16 yrs	-0.00240	-0.00357	-0.00207	-0.00325
	(4.63)**	(3.24)**	(4.19)**	(3.02)**
Female * Kids	0.00189	0.00446	0.00179	0.00440
	(2.29)*	(2.32)*	(2.30)*	(2.35)*
Age 20-24	0.00251	-0.00013	0.00264	0.00014
	(4.18)**	(0.11)	(4.51)**	(0.13)
Age 45-54	-0.00080	-0.00218	-0.00078	-0.00219
	(1.81)+	(2.42)*	(1.88)+	(2.50)*
Age 55-59	-0.00008	-0.00093	-0.00005	-0.00087
	(0.12)	(0.81)	(0.09)	(0.78)
Years of education	-0.00045	-0.00042	-0.00042	-0.00041
	(4.66)**	(1.71)+	(4.72)**	(1.75)+
Log per capita hh income	-0.00398	-0.00312	-0.00337	-0.00265
	(10.56)**	(3.19)**	(9.63)**	(2.79)**
Unemployed at recall	0.07526	0.02737	0.06841	0.02564
	(35.98)**	(12.55)**	(34.97)**	(12.23)**
Regional Unemployment rate	0.00017	0.00027	0.00015	0.00025
	(2.37)*	(1.66)+	(2.14)*	(1.58)
Iviewer not present at recall	-0.00005	-0.00125	-0.00032	-0.00129
	(0.11)	(1.67)+	(0.82)	(1.77)+
Recall period (# of months)	-0.00029	-0.00442	-0.00034	-0.00453
	(0.23)	(1.27)	(0.29)	(1.32)
Year	-0.00009	0.00006	-0.00008	0.00009
	(2.59)**	(0.34)	(2.50)*	(0.55)
Life Sat. medium (5-7)			0.00135	0.00121
			(3.81)**	(1.40)
Life Sat. low (0-4)			0.00820	0.00507
			(8.69)**	(2.91)**
Observations	95022	22874	94836	22813
Mean dep. var.	.008	.007	.008	.007
Model Chi-Sq	1743.4	270.4	1752.7	302.7
Model df	13	13	15	15
Pseudo-R2	.223	.14	.234	.146

Robust z statistics in parentheses; + $p < 0.10$; * $p < 0.05$; ** $p < 0.01$

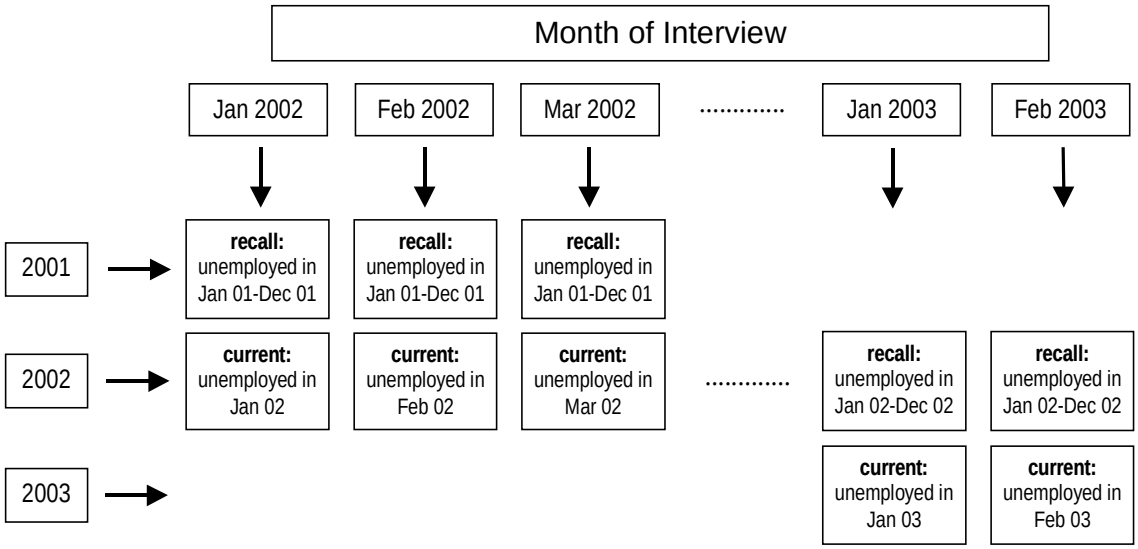

Figure 1: The structure of unemployment data in the GSOEP

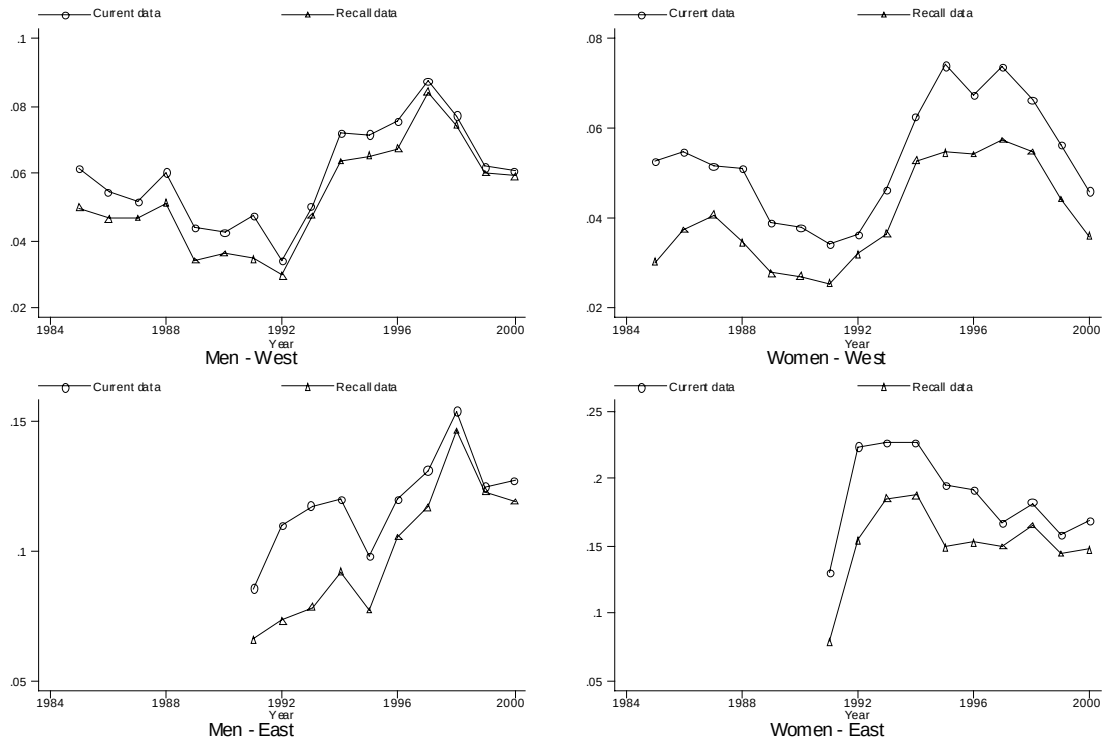

Figure 2: Self-reported current and retrospective unemployment, by Sex and Region (not adjusted for interview month)

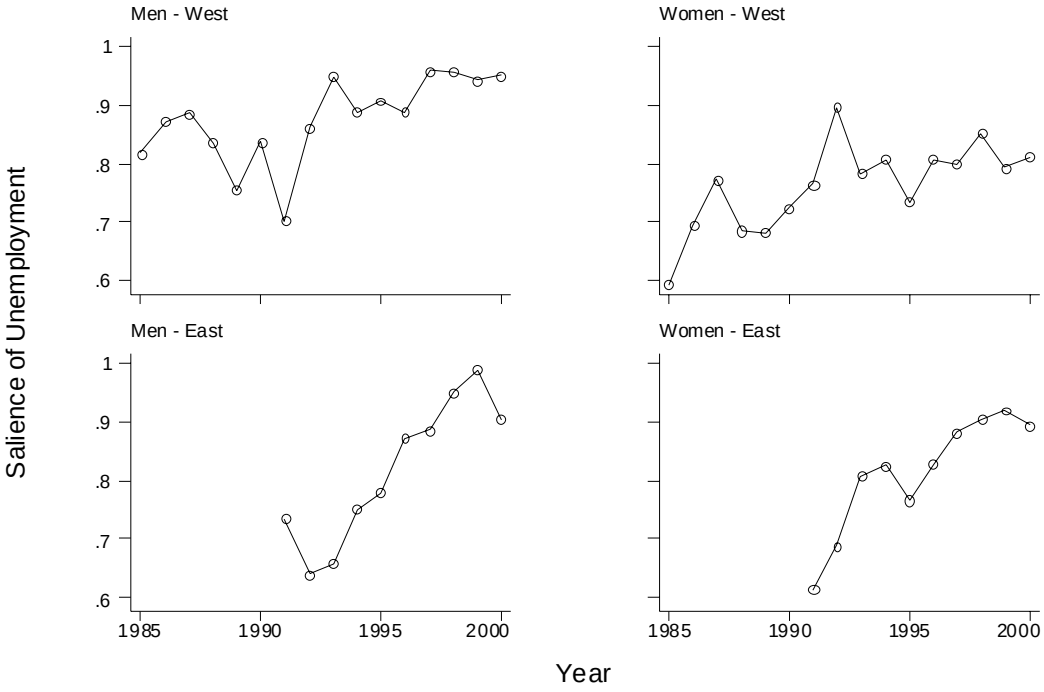

Figure 3: "Salience" of Unemployment, by Sex and Region

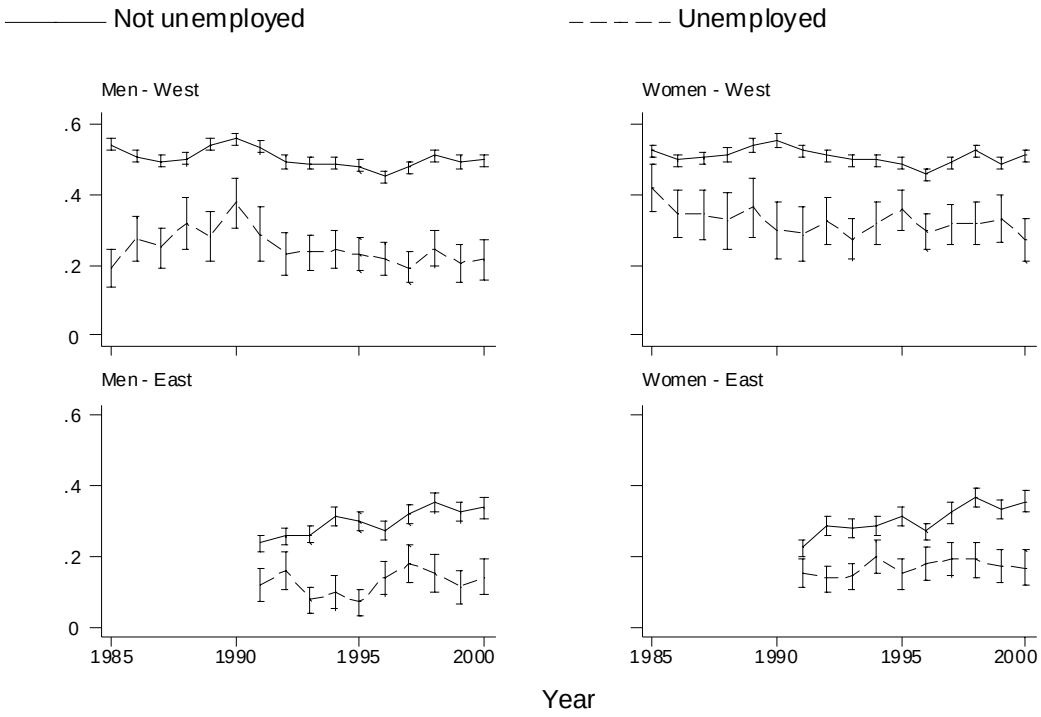

Figure 4: Proportion of "very happy" respondents, by sex, region and employment status