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Summary (100 words): I compare current and one-year retrospective data on unemployment in the German SOEP. 13 percent of all unemployment spells are not reported one year later, and another 7 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 labor 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.

Keywords: Unemployment, Retrospective Bias, Salience, Life Satisfaction

1.   Introduction

In order to study the dynamics of labor force participation empirically one needs detailed information on an individual's labor 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 labor market states that happened during a specific reference period. Another option is to use calendars and ask respondents to report labor 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 as "unemployment" when interviewed 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 U.S. 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 between CPS and WES are mostly made 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 Panel Study of Income Dynamics (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 labor 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 labor 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

unemployment rates derived from the British Labour Force Survey. Underreporting becomes 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 (SOEP). As described below in more detail, the SOEP uses monthly calendars to elicit retrospective data on labor force participation. These data are often used to generate spell data for event history or duration analyses. While many microeconometric studies of labor market behavior 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 SOEP overstates entries into unemployment in January and overstates exits in December. Baseline hazard rates derived from SOEP data for exit from unemployment look very different from those derived from administrative unemployment records (Biewen and Wilke 2004). For instance, in the SOEP, they peak at about 12 months, compared to about 20 months in the administrative records. It is very likely that this difference is an artefact of using retrospective data, specifically from calendars.

Considering the fact that many applications in labor 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 often 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 minimized.[1]

In the following, I compare 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 the CPS-WES comparisons. The SOEP also contains another source of information on labor 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 CPS-WES comparisons. 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 labor

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

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 organized as follows: Section II describes the data and how current and retrospective data are compared. Section III contains the results of the empirical analysis. Section IV concludes.

2.   Data and descriptive results

The data used in this study are derived from the German Socio-Economic Panel Study SOEP and cover the period from 1985 to 2003 (for a description of the data see SOEP Group 2001. Extensive documentation is provided at www.diw.de/english/sop/index.html). I restricted the sample to respondents aged 20 to 59 in both East and West Germany. After age 59, workers in Germany 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 person-year observations is approximately 115,000.

A simple yes/no-question asks for current unemployment: "*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

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[2] Data in 1984 – the first year of the SOEP – had to be dropped because unemployment was measured differently. There was no separate question on unemployment. Rather, "being unemployed" was listed among other labor 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.

who fulfil certain criteria (looking for employment, available to work). This definition of unemployment differs from the ILO definition (not working, available to work, actively seeking employment) used in many labor force surveys, because it includes only those officially registered at a government agency. Data on current unemployment is only available for interview months. The SOEP 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 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. Note that unemployment is explicitly referred to as "registered unemployment", thus using the same concept as the question for current unemployment.

Table 1.   Labor force states coded in the SOEP calendars

Employed	*Unemployed*	*Out of labor 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)

Most labor market analyses using the SOEP use the calendar to construct labor 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 another advantage over comparisons of CPS and WES, which rely on self-perceptions of the respondents.
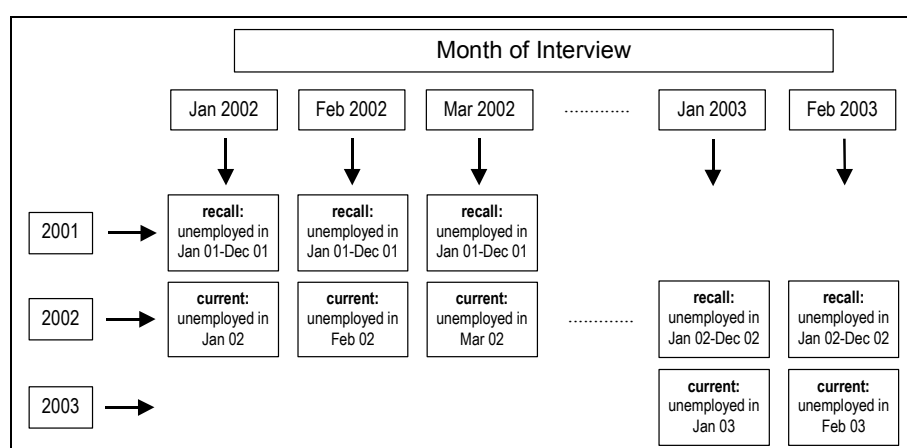
Fig. 1: The structure of unemployment data in the SOEP

The basic 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 2003. 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 labor market status.

The comparison of current and retrospective information on unemployment results in four possible cases with two types of misclassification: *false negatives* and *false positives*. 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, 19.8 percent of all respondents who say they are registered unemployed fail to report unemployment in that month when interviewed one year later (see the last column of Table 2). One can further identify varying degrees of failure. First, there are 12.4 percent *hard errors*, meaning that respondents do not report a single month of unemployment in the calendar. Then there are *soft errors*, meaning that respondents do report spells of unemployment in the preceding year, but not in the month in which last year's interview took place. In 4.1 percent of the cases, the difference between retrospective and current unemployment is only one month. In another 3.4 percent, the difference is larger than one month.

Table 2 also reveals interesting similarities and differences in reporting behavior between respondent groups. First, there is a large 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. More than 20 percent of the women who report to be currently unemployed do not report any unemployment when asked one year later. The percentage of hard errors is thus more than twice as large among women than among men. Note, however, that there is no such sex difference in East Germany. East German men and women have the same propensity to report unemployment retrospectively. The proportion of hard errors is about one percentage point smaller than among West German men. Hence the group that under-reports unemployment most are West German women.. Given earlier results from the U.S. it is actually more surprising – at first sight – that East German women are as likely to report unemployment retrospectively as men (in East and West). However, this observation fits nicely with the fact that the labor force attachment of East German women is stronger than the

labor force attachment of West German women. In the former GDR, women were strongly encouraged, if not to say expected, to work in the labor market. Finally, with respect to soft errors, differences between respondent groups are only minor.

Table 2. Prevalence of false negatives and false positives, by sub-sample, respondent sex, and type or error (column percentages in parentheses)

| | West | | East | | |
Error Type	Men	Women	Men	Women	Total
Currently unemployed	2,225	2,176	1,986	2,951	9,338
No error	1,844	1,576	1,638	2,432	7,490
	(82.88)	(72.43)	(82.48)	(82.41)	(80.21)
Hard error	230	463	188	272	1,153
	(10.34)	(21.28)	(9.47)	(9.22)	(12.35)
Soft error: 1 month difference	93	76	93	120	382
	(4.18)	(3.49)	(4.68)	(4.07)	(4.09)
Soft error: >1 month difference	58	61	67	127	313
	(2.61)	(2.80)	(3.37)	(4.30)	(3.35)
Currently employed	37,389	26,955	22,062	18,870	105,276
False positives	119	70	103	69	361
	(0.32)	(0.26)	(0.47)	(0.37)	(0.34)

Source: SOEP 1985-2003

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 in fact correct. Of course, if one takes into account the possibility that current unemployment is measured with error – which I do not – there are even more types of error. Table 2 shows that false positives are rare in absolute as well as in relative terms. Of more than 105,000 observations who said they were employed in the preceding interview, only 361 retrospectively report having been unemployment in the interview month. Moreover, there are no systematic differences between groups. Preliminary

analyses shown in an earlier version of the present paper suggest that false positives are most likely 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 at the time). In the rest of the paper, I will thus focus on false negatives.

3. Results

A. Retrospective bias and unemployment salience: a replication of Akerlof-Yellen

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 *ceteris paribus* 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 public health, hospitalization tends to be less underreported the longer the stay, or own diseases tend to be remembered more than family members' diseases. In expenditure surveys, women are more likely to remember purchases of clothing while men remember

purchases of tires. In political science, voters are more likely to remember whether and how they have voted in presidential elections than in local elections, etc.


Fig. 2: Self-reported current and retrospective unemployment, by sex and region

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 does measure 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 individuals younger than 25 years and individuals older than 54, 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.

In this subsection I present similar evidence from the SOEP. Figure 2 shows the development of current and retrospective unemployment rates over time, by sex and region. 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.
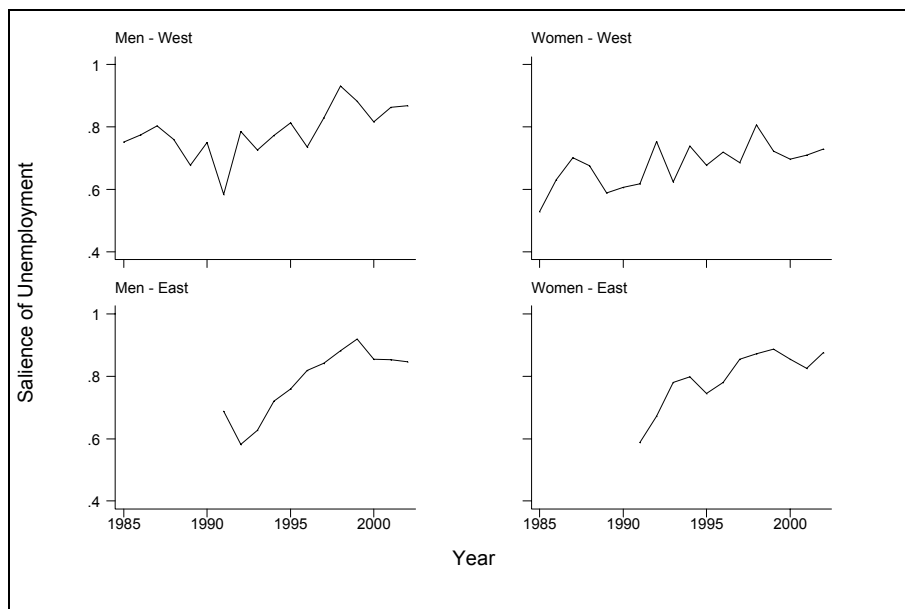
Fig. 3: The ratio of retrospective to current unemployment ("unemployment salience"), by sex and region

This in confirmed in Figure 3, which shows the development of the ratio of recalled to current unemployment, separately for men and women in East and West Germany. The larger this ratio, the smaller is the amount of underreporting and the more serious or salient is the experience of unemployment. Unemployment salience seems to be on the increase, slowly in West Germany, much faster in East Germany, leading to an overall convergence between West German men and East German men and women. The increase could be caused for example by a larger proportion of long-term unemployed or reductions in unemployment benefits. Unemployment might have become more costly in economic and social terms, or the effects of unemployment are felt longer than in previous years.[3] If the Akerlof-Yellen-

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[3] 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

hypothesis is correct, other, more direct measures of the effect of unemployment on well-being, such as the loss in life satisfaction should show a similar trend. Fortunately, well-being data is available in all years of the SOEP. Putting together data on retrospective bias and psychological well-being over time enables us to test the Akerlof-Yellen-hypothesis directly.

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 well-being or 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 6 on the SOEP'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 across all sub-groups. Fourth, the percentage difference is smaller for West German women than for West German men. West German men and women who are not unemployed are equally happy, but unemployed women report to be happier than do unemployed men. When combined with the earlier result that West German women are more likely to under-report unemployment retrospectively, this is direct evidence in favor of the Akerlof/Yellen-hypothesis. In East Germany, the life satisfaction difference seems to be a bit larger among men in some years (around 1995) but a systematic difference between men and women can be hardly seen.

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panel effects: Respondents might become more familiar with the instrument and try to answer more accurately. This explanation will be examined in the regression analysis in section III.B.
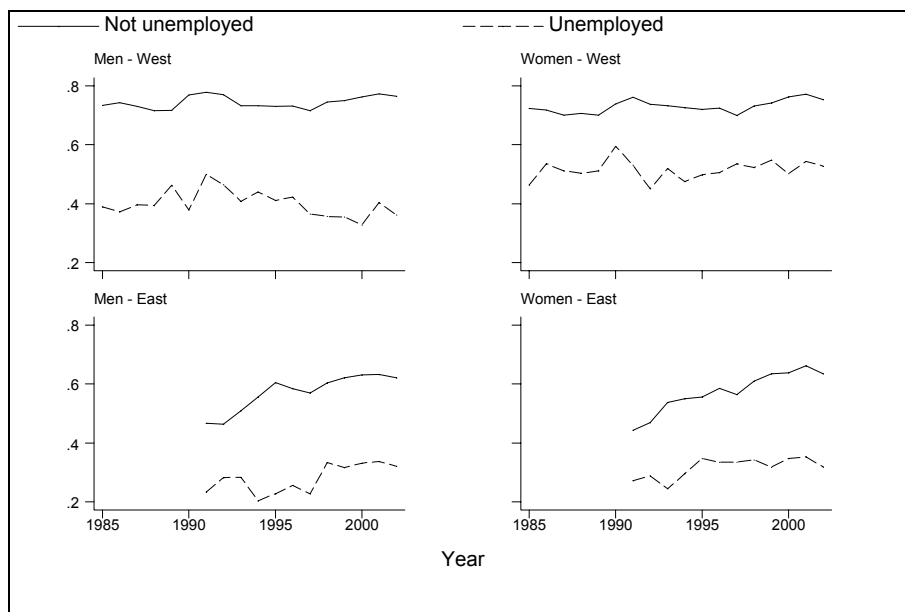
Fig. 4: Proportion of "happy" respondents, by sex, region and employment status

Let us continue with a more systematic "test" of the Akerlof/Yellen-hypothesis. How does the difference in happiness between the unemployed and other respondents relate to the prevalence of retrospective error (or salience)? In order to analyze this relationship in more detail, I have stratified the sample by four age groups (20-29, 30-39, 40-49, 50-59), sex and region of residence (federal states), which results in a total of 112 different groups. For each of these groups I have calculated the average retrospective error and the average life satisfaction differential in each year.

Columns 2 to 4 of Table 3 show pooled, random effects and fixed effects tobit estimates of the relationship between the two variables, controlling for a linear time trend. The fixed effects models are estimated by including within-group averages of all explanatory variables on the right hand side of the equation. Tobit models are more appropriate than simple linear models because salience can be interpreted as a latent variable of which observations are censored at zero and at one. Considering false negatives only, the number of respondents who report unemployment retrospectively cannot be larger than the number of the currently unemployed or smaller than zero.

Table 3.   Regressions of Salience on the Unemployment – Well-Being Differential

	Mean (1)	Tobit (2)	RE Tobit (3)	FE Tobit (4)	Tobit (5)	RE Tobit (6)	FE Tobit (7)
%-Diff. in Life Satisfaction	0.2751	0.1517	0.1369	0.1243	0.1537	0.1373	0.1187
		(0.0340)	(0.0341)	(0.0363)	(0.0337)	(0.0338)	(0.0359)
Time trend (1990 = 0)	4.7038	0.0129	0.0124	0.0120	0.0091	0.0087	0.0069
		(0.0019)	(0.0019)	(0.0020)	(0.0021)	(0.0021)	(0.0023)
Unemployment rate	0.1145				0.9183	1.0546	3.2361
					(0.2356)	(0.3020)	(0.7490)
Prop. long-term unemployed	0.3079				0.1632	0.2474	0.0397
					(0.1609)	(0.1892)	(0.3087)
Constant		0.7066	0.7165	0.6087	0.5686	0.5341	0.6075
		(0.0155)	(0.0180)	(0.0387)	(0.0499)	(0.0590)	(0.0774)
Observations		1479	1479	1479	1479	1479	1479
Number of groups			104	104		104	104
Left censored at y = 0		72	72	72	72	72	72
Right censored at y = 1		393	393	393	393	393	393
Hausman Chi-Squared (df)			1.79 (2)			14.82 (4)	

Note – FE = Fixed Effects; RE = Random Effects; Standard errors in parentheses; Source: SOEP 1985-2003.

All three specifications show a positive and significant relationship between the life-satisfaction differential due to unemployment and the Akerlof/Yellen-measure of salience. They pooled tobit specification suggests a 1.5 percentage point increase in salience (i.e. a 1.5 percentage point decrease in the proportion of unreported unemployment spells) when the life satisfaction difference between unemployed and other respondents increases by 10 percentage points. Controlling for group heterogeneity by estimating panel models reduces the size of the effect to about 1.3 percentage points. Although the effect seems to be rather small in size, this supports to the idea that retrospective bias in unemployment data is related to the salience or importance of unemployment for the respondent.

The estimates also show a significant linear time trend which is quite stable across specifications. The coefficient suggest about a 1.2 percentage point increase in salience per year. Above, I was speculating that an increase in long-term unemployment might be the driving force behind this time trend. In Columns 5 to 7 of Table 3, I thus also control for regional unemployment rates and the regional proportion of long-term (one year or longer) unemployed (again, measured at the time of "current" unemployment). These numbers are

taken from official labor force statistics and they are available by year, sex, and federal state. There are several notable results. First, the relationship between the life satisfaction loss from unemployment and salience is not affected. Second, the time trend becomes smaller (down to .7 percentage points per year in the fixed effects specification) but it remains statistically significant. Third, the regional unemployment rate and the proportion of long-term unemployed in a region have positive effects on the salience of unemployment. However, their coefficients are greatly affected by the specification choice. The difference between the random and the fixed effects coefficients is so large that the Hausman test statistic rejects the random effects model at the 1% significance level. This suggests that one should consider the fixed effects models as the most appropriate. According to this model, a percentage point increase in the regional unemployment rate raises the Akerlof/Yellen-measure of salience by 3 percentage points. In contrast to this rather large effect, the proportion of long-term unemployed has an only moderate (and insignificant) relationship to salience.

B. Who reports unemployment retrospectively? Evidence from panel data

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. The following analysis will concentrate on hard errors. Soft errors are included in the regressions as correctly recalled unemployment. Leaving such cases out of the analysis does not change the main results presented below. Table 4 shows the results of probit estimates for the probability to recall having been registered unemployed in the previous year, separately by sex and region. The coefficients are marginal effects, for dummy variables they reflect the effect of a discrete change of the variable from 0 to 1. In order to account for potential attrition bias, all estimates use longitudinal weights (inverse attrition probabilities)

provided with the data. Sample means and standard deviations of all explanatory variables can be found in Table A1 in the Appendix.

Table 4.   Probit estimates of correctly recalling unemployment, marginal effects

| | West | | East | |
	Men	*Women*	*Men*	*Women*
Age 20-24	-0.0201	-0.0232	0.0214	-0.0307
	(0.0208)	(0.0300)	(0.0173)	(0.0284)
Age 25-44 (baseline)	0.0000	0.0000	0.0000	0.0000
Age 45-54	0.0160	-0.0394	0.0150	0.0201
	(0.0163)	(0.0305)	(0.0152)	(0.0165)
Age 55-59	-0.0659	-0.1263	-0.1422	-0.1452
	(0.0246)	(0.0459)	(0.0322)	(0.0363)
Years of education	-0.0073	-0.0067	-0.0019	-0.0060
	(0.0028)	(0.0047)	(0.0035)	(0.0035)
Kids < 16 yrs	-0.0125	-0.1556	-0.0017	-0.0078
	(0.0168)	(0.0255)	(0.0165)	(0.0179)
Log per capita hh income	-0.0284	0.0230	-0.0276	0.0216
	(0.0163)	(0.0252)	(0.0190)	(0.0184)
Unemployed at recall	0.1304	0.2146	0.0741	0.0782
	(0.0146)	(0.0181)	(0.0123)	(0.0120)
Regional unemployment rate	0.0033	0.0055	-0.0002	0.0014
	(0.0024)	(0.0036)	(0.0024)	(0.0023)
Recall period (# of months)	0.0161	0.0511	-0.0202	0.0023
	(0.0151)	(0.0208)	(0.0133)	(0.0116)
Face-to-face interview	-0.0018	-0.0103	-0.0151	0.0003
	(0.0032)	(0.0047)	(0.0044)	(0.0043)
Calendar Year	0.0026	0.0047	0.0057	0.0023
	(0.0011)	(0.0017)	(0.0025)	(0.0023)
Completed Interviews	0.0011	0.0072	0.0099	0.0055
	(0.0016)	(0.0028)	(0.0026)	(0.0026)
Observations	2,082	2,044	1,898	2,817
Mean of dependent variable	.8986	.7880	.9047	.9063

Note – Standard errors corrected for repeated observations are in parentheses; Source: SOEP 1985-2003.

Let us begin with age and education. For the regression analysis, I have constructed four age groups: 20-24, 25-44, 45-54, and 55-59. Prime age respondents (25-44) are the reference group. Overall, the effect of age on reporting unemployment retrospectively is rather small, except for the oldest age group (55-59), which is most likely to underreport. However, this finding is hardly due to deteriorating memory. In fact, the oldest age group is close to retirement, and more than half of all false negatives in that age group say they have been a pensioner rather than unemployed. West German men and East German men and

women who are not so close to retirement (aged 45-54) seem to suffer most from unemployment as they are least likely to underreport, which could be explained by low re-employment probabilities of this group. However, the difference to prime aged respondents is not statistically significant.

Years of education have 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.

The presence of children in the household has a large negative effect (more than 15 percentage points) on retrospective unemployment reports among West German women. The presence of children thus accounts for about two thirds of the difference between West German women and the other three groups (43.2 percent of the female sample have children younger than 16 in the household). In fact, of those West German women with children who fail to report unemployment, more than two thirds (70.7 percent) say they have been a housewife. Interestingly, the effect of children is virtually zero among East German women. Of all East German women who fail to report unemployment, only 9.1 percent say they have been a housewife. This striking difference suggests that unemployment is a much more painful experience for East German than for West German women. Given the significance of female employment in the former GDR, it appears to be much harder for East German women to "re-interpret" past unemployment spells as being a housewife.

Log per capita household income (at the time of unemployment) has no significant effect on recall, but there are some regularities in the data in the sense that the estimated coefficients are negative for men and positive for women, independent of the region. It is

unclear how to interpret the effect found for women because a higher income should dampen the negative consequences of unemployment independent of the respondents' sex.

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 approximately 13 percentage points for West German men, 21 percentage points for West German women, and 7 to 8 percentage points for East German men and women. 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 a positive but insignificant effect on recall in West Germany and virtually no effect in East Germany, i.e. I do not find systematic effects of reference group unemployment (see Clark 2003).

The recall periods in our sample range from 3 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. I find significant negative effects on recall among West German women and East German men but practically no effects in the other two groups. These mixed results raise doubts about the presence of pure memory effects.

The indicator variable for face-to-face interviews measures a possible survey mode effect. Respondents who are interviewed in person often answer questions somewhat differently than those who fill out a self-completion questionnaire. For instance, they might be less willing to answer questions on sensitive issues such as income or wealth. If recent unemployment is a sensitive issue for some respondents, face-to-face interviews might result in more false negatives than self-completion questionnaires. But the presence of interviewers can also substantially increase the quality of survey data because interviewers can clarify

questions, explain unknown concepts, etc. Yet, as far as the accuracy of retrospective unemployment information is concerned, face-to-face interviewing seems to have more of a negative effect, especially among East German men.

The results presented in the section 3.A. have shown a significant time trend towards more accurate reports of unemployment, also after controlling for regional unemployment rates. It remained unclear how much of the remaining trend was due to a panel rather than a pure time effect. A panel effect could be explained by the fact that respondents learn about the survey instrument when they are repeatedly interviewed so that their answers become more accurate. The large SOEP refreshment sample that was started in 2000 and the continuous inflow of new (mainly young) respondents from existing households provides enough independent variation of calendar year and individual survey year to identify pure time and panel effects separately. The positive coefficients of "calendar year" imply that, between 1985 and 2003, the proportion of unreported unemployment spells has decreased in all three groups (although among East German women, the effect is not significant). The completed number of interviews also has a significant positive effect on recall (except among West German men). This is good news for the survey methodologist, because it suggests that data quality increases in the course of a panel survey, for instance because respondents become familiar with the survey instrument and become more likely to give accurate answers. An alternative interpretation is that the sample becomes more selective because unmotivated and hence unreliable respondents tend to drop out of the panel earlier. However, in additional analyses not shown in this paper, future panel attrition does not help to predict retrospective bias in the SOEP calendar.

C. Life satisfaction, labor force attachment and retrospective error

Let us finally turn to two direct indicators of individual unemployment salience, measured *at the same time* as current unemployment. The first indicator is overall life satisfaction. I use three different specifications of this variable: (a) the *level* of life satisfaction at the time of unemployment. Due to different reference levels of life satisfaction, levels might not be directly comparable across individuals. To avoid such problems, I also compute relative satisfaction levels: (b) the instant individual loss of life satisfaction due to unemployment – measured as the difference between life satisfaction at the time of unemployment and the average life satisfaction index in non-unemployment years, and (c) the average individual loss in life satisfaction – measured as the difference between the life satisfaction index averaged across all unemployment years and the life satisfaction index averaged across all non-unemployment years. The two latter specifications measure the individual loss in life satisfaction from unemployment and account for possible differences in individual reference levels of reported life satisfaction.

Table 5. Marginal effects of unemployment salience measures on retrospective unemployment reports

	West		East	
	Men	*Women*	*Men*	*Women*
Life satisfaction				
(a) Instant level (at unemployment)	-0.0037	-0.0095	-0.0081	-0.0043
	(0.0028)	(0.0044)	(0.0030)	(0.0028)
(b) Instant life satisfaction loss	-0.0016	-0.0109	-0.0052	-0.0132
	(0.0040)	(0.0066)	(0.0042)	(0.0054)
(c) Average life satisfaction loss	-0.0037	-0.0059	-0.0030	-0.0088
	(0.0030)	(0.0051)	(0.0031)	(0.0039)
Employment plans				
Take up empl.: at some time	-0.0853	-0.0813	-0.1250	-0.0967
	(0.0235)	(0.0217)	(0.0294)	(0.0178)
Take up empl.: does not want to	-0.2062	-0.1141	-0.3056	-0.2686
	(0.0528)	(0.0355)	(0.0525)	(0.0478)

Note – Standard errors corrected for repeated observations in parentheses. All regressions include Table 4 control variables; Source: SOEP 1985-2003.

Table 5 shows the result of different probit regressions of recall on the three life satisfaction measures (in addition to the set of control variables listed in Table 4). The

relationship between recall and life satisfaction level or life satisfaction loss, respectively, is negative in all sub-groups and specifications. The relationship seems to be stronger among women than among men. It is also more often statistically significant. Among women, two of the three specification yield parameters that are significant at the 10 percent level. Absolute t-values are 2.2 (life satisfaction level) and 1.7 (instant loss) in the West German sample and 2.5 (instant loss) and 2.3 (average loss) in the East German sample. Among men, the only coefficient that is significant at the 10 percent level is that of life satisfaction level in the East German sample (absolute t-value 2.7)

Overall, these findings are consistent with the Akerlof/Yellen-hypothesis that more painful episodes of unemployment are more likely to be reported retrospectively. However, the evidence is somewhat weak. One possible reason for the weakness of the results is that the measures of individual losses in life satisfaction derived from the SOEP data are inherently imprecise. The SOEP measures life satisfaction (i.e. experienced utility) at a few distinct points in time. This will only by chance measure remembered utility, which is conceptually more relevant for reporting behavior. Psychological evidence suggests that remembered (dis-) utility is rather determined by the so-called peak-end rule (Kahneman et al. 1997): the remembered disutility of an unpleasant episode equals the average of the peak (instant) disutility and the end (instant) disutility of that episode. It is likely that data on the peak loss in life satisfaction from an unemployment spell and the loss measured towards the end of the spell would predict false negatives better than the single measurement per year that is available in the data. In fact, recent evidence shows that application of the peak-end rule to job satisfaction data yields better predictions for labor market behavior (job quits) than e.g. current job satisfaction levels (Clark and Georgellis, 2004).

Another indicator of unemployment salience is the strength of an unemployed's labor force attachment. Weaker labor force attachment means that unemployment is a less salient

event, which reduces the probability of recall. My indicator for labor force attachment 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 latter 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 both questions into one variable with three categories: respondent wants to re-enter employment "as soon as possible", "at some time", or "never".

The results are shown at the at the bottom of Table 5: future employment plans or labor force attachment have a massive effect on recall (all parameters are statistically different from zero at the 1 percent level). Compared to the reference category of respondents who say they want take up employment immediately, men who claim they would rather like to re-enter employment at some time in the future have an estimated recall probability that is 8.5 percentage points lower in West Germany and 12.5 percentage points lower in East Germany. Among women, the respective effects are 8.1 percentage points in the West and 9.7 percentage points in the East. Male respondents in West Germany who are registered unemployed and who say they do not intend to re-enter employment are 20.6 percentage points less likely to report unemployment retrospectively. In East Germany, the negative effects are even stronger: 30.6 percentage points among men and 26.9 percentage points among women. These results show that retrospective information on unemployment spells given by those who do not care much about being unemployed because they do not want to return to work anyway contains a large "downward" bias.

For the sake of brevity, I do not show what happens to the estimated effects of the control variables in Table 4 when life satisfaction or labor force attachment are included in the regression analysis. Still, a few remarks are in order. The most important findings discussed in section III.B. do not change – with one exception. Quite plausibly, much of the effects of

age, i.e. the relatively low recall probability of the age group that is close to retirement is soaked up by including future employment plans in the regressions. Many of those who are unemployed and close to retirement do not want to go back to work and rather wait to become eligible for old-age pensions.

4.   Summary and discussion

In this paper, I have compared current and one-year retrospective survey data on unemployment. The data was derived from 19 years of the German Socio-Economic Panel, covering the years 1985 until 2003. Assuming that reports of current unemployment reflect the true labor market state, the data suggested that monthly retrospective data in employment calendars suffers from a sizeable amount of underreporting. About 20 percent of all reports of being currently unemployed had no match in the calendar completed in the next year. 13 percent of all respondents who said they were unemployed when interviewed in the previous year failed to mention a single month unemployment when asked one year later.

The first part of the substantive analysis examined aggregate long-term trends in underreporting. Following Akerlof and Yellen (1985), the ratio between the retrospective and the current unemployment rate was interpreted as an indicator of the psychological seriousness or "salience" of unemployment. I found that, during the observation period, the salience of unemployment has increased for both men and women in East and West Germany. Further, in support of Akerlof and Yellen's salience interpretation, I found that the ratio of retrospective to current unemployment was positively related to the life satisfaction differential between unemployed and non-unemployed respondents, presumably a more direct measure of unemployment salience.

The second part of the analysis examined individual-level determinants of retrospective error in the SOEP employment calendar. The analysis of so-called false

negatives suggested that respondents with weak labor force attachment were most likely to under-report unemployment. For example, West German women, particularly when they had children, were most likely to under-report. Instead, they showed a strong tendency to interpret periods of unemployment as having been a homemaker. This did not hold for East German women, who are traditionally attached more closely to the labor market. Unemployed respondents who said they wanted to start employment as soon as possible were much more likely to recall unemployment than others. The unemployed (at the time of recall) remembered much more easily that they were unemployed in the preceding year. Further, more painful spells (in the sense that life satisfaction at the time of unemployment was low relative to other unemployed individuals or relative to own levels of life satisfaction in times on non-unemployment) went less often unreported.

Although the results presented in this paper are mostly according to expectations and in accordance with psychological explanations of recall, the analysis is limited by an unavoidable problem: the data did not allow to ascertain the true length of the unemployment spell that was to be remembered. Certainly, longer spells are more easy to remember than shorter spells: they are presumably more painful and it is also less likely to just report the wrong month if the reported spell is long rather than short. The general problem is that the available information on spell length is endogenous in a specific sense, because it can only be derived from error-prone retrospective data. 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. Such extension is yet beyond the scope of the current paper.

The present analysis implies that research on unemployment that draws on retrospective data should always try to account for the shortcomings of such data. Recent comparisons of transition rates based on German administrative data with transition rates based on SOEP calendars suggest that SOEP calendars underestimate unemployment spell

length by a considerable margin, especially for West German women. (Biewen and Wilke 2004). This finding can be readily explained by the reporting behavior of SOEP respondents.

A further suggestion for future research is to augment the analysis of psychological effects of unemployment (scarring as well as recall) by giving special attention to Kahneman's peak-end rule and to study which are the specific conditions that determine the salience of unemployment.

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Appendix

Table A1.   Sample description

	West								East							
	Men				*Women*				*Men*				*Women*			
	Mean	*SD*	*Min*	*Max*	*Mean*	*SD*	*Min*	*Max*	*Mean*	*SD*	*Min*	*Max*	*Mean*	*SD*	*Min*	*Max*
Age 20-24	.119				.142	.349			.101				.068			
Age 25-44	.514				.517	.5			.474				.531			
Age 45-54	.206				.196	.397			.238				.244			
Age 55-59	.162				.146	.353			.187				.157			
Years of education	10.9	2.14	7	18	11	2.15	7	18	11.5	1.86	7	18	11.5	1.74	7	18
Kids < 16	.373				.432				.363				.52			
Ln hh income	6.30	.497	4.45	8.15	6.43	.534	3.58	8.61	6.26	.453	3.35	8.57	6.18	.422	4.02	8.57
Unemployed at recall	.556				.469	.499			.522				.607			
Unemployment rate	9.70	2.58	3.7	20.8	9.59	2.69	3.7	19.3	16.6	3.23	6.3	21.7	16.2	3.2	4.9	21.7
Face-to-face interview	.248				.295	.456			.406				.383			
Recall period (months)	11.8	1.86	3	19	11.7	1.97	5	20	11.8	1.41	4	18	11.7	1.35	4	18
Calendar year	10.6	5.89	0	18	10.5	5.82	0	18	13.3	3.45	7	18	12.5	3.47	7	18
Survey year	4.62	4.73	0	18	4.37	4.47	0	18	4.05	3.26	0	11	3.76	3.14	0	11
Life satisfaction level	5.6	2.33	0	10	6.28	2.1	0	10	5.21	2.02	0	10	5.41	2.02	0	10
Inst. life sat. loss	-1.1	1.66	-8.4	4.6	-.458	1.5	-8.21	5.5	-.818	1.34	-6.8	5.67	-.621	1.27	-6.59	3.56
Average life sat. loss	-1.08	2.12	-9	5.83	-.45	1.87	-9.17	6.56	-.828	1.8	-7.5	5	-.654	1.76	-7.2	8.67
Take up empl.: immediately	.762				.537				.827				.703			
Take up empl.: at some time	.145				.348				.11				.224			
Take up empl.: never	.093				.116				.064				.073			
N	2,082				2,044				1,898				2,817			

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