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DISCUSSION PAPER SERIES

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Ragui Assaad
Caroline Krafft
Shaimaa Yassin

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Ragui Assaad
University of Minnesota and IZA

Caroline Krafft
University of Minnesota and St. Catherine University

Shaimaa Yassin
University of Paris 1 Panthéon-Sorbonne and University of Le Mans

ABSTRACT

Comparing Retrospective and Panel Data Collection Methods to Assess Labor Market Dynamics

There is potential for measurement problems in both retrospective and panel microdata. In this paper we compare results on basic indicators related to labor markets and their dynamics from retrospective and panel survey data in Egypt, in order to determine the conditions under which results are similar or different. Specifically, we (1) assess the consistency of reporting of time-invariant characteristics in different waves of the panel, (2) compare the retrospective and panel data results on past labor market statuses, (3) assess the consistency of estimates of labor market transition rates across two specific dates by comparing panel and retrospective data, (4) assess the consistency of estimates of the level and trends of annual labor market transition rates across retrospective data from different waves of the survey, and (5) assess whether retrospective data can provide accurate trends of labor market aggregates, such as unemployment rates. We find that it is possible to garner useful information on labor market dynamics from retrospective data, but one must be cautious about which information to trust and at what level of detail. We conclude with a discussion of implications for future research as well as future survey design.

Corresponding author:
Ragui Assaad
Humphrey School of Public Affairs
University of Minnesota
301 19th Avenue South
Minneapolis, MN 55455
USA

E-mail: assaad@umn.edu

1 Introduction

The analysis of labor market dynamics requires the availability of data about the same individuals at multiple points in time. This kind of data allows for the examination of flows between different labor market states rather than simply assessing labor market stocks over time, which is what is usually possible with cross-sectional data. Data about the same individuals over time can either be in the form of panel data, where individuals are visited and interviewed multiple times over the course of several months or years, or retrospective data, where individuals are asked about their past labor market trajectories at one point in time. Although both methods of data collection suffer from different kinds of measurement errors, panel data are often deemed superior because they minimize recall error, which could be substantial in retrospective data (Artola & Bell, 2001; Bound, Brown, & Mathiowetz, 2001; Magnac & Visser, 1999; Mathiowetz & Duncan, 1988; Pina-Sánchez, Koskinen, & Plewis, 2014). Panel data, however, are expensive and difficult to collect and are, therefore, rarely available to researchers in developing countries. If available, they are generally not collected frequently enough to observe complete labor market trajectories and transitions (Blossfeld, Golsch, & Rohwer, 2007) and suffer from sample attrition and a variety of measurement errors (Artola & Bell, 2001; Feng & Hu, 2013). It is therefore useful to examine how well retrospective data perform in assessing labor market dynamics and the extent to which analyses that depend on them conform to results obtained from panel data.

Due to potential problems with both retrospective and panel data, it is worthwhile to compare results on basic indicators related to labor market dynamics from retrospective and panel data on the same sample of individuals, in order to determine the conditions under which they provide similar or substantially different results. This paper takes advantage of a unique

opportunity to undertake such a comparison, where both panel and retrospective data are available for the same individuals in the 1998, 2006 and 2012 Egypt Labor Market Panel Surveys (ELMPSs). Not only do the reference periods of the retrospective data overlap with the dates of the previous waves of the survey, but the retrospective periods from different waves of the survey overlap with each other as well. This allows for comparisons of retrospective and panel data at the same point in time, as well as comparisons of recalled events from one wave with the same events recalled in another wave.

In this paper, we (i) assess the consistency of reporting of time-invariant characteristics in different waves of the panel, (ii) compare the retrospective and panel data results on past labor market statuses, including the estimation of multivariate models of the determinants of alignment between the two data sources, (iii) assess the consistency of estimates of labor market transition rates across two specific dates by comparing panel and retrospective data, (iv) assess the consistency of estimates of the level and trends of annual labor market transition rates across retrospective data from two different waves of the survey, and (v) assess whether retrospective data can provide accurate trends of labor market aggregates, such as employment-to-population ratios and unemployment rates.

The rest of the paper is organized as follows. Section 2 reviews the theory and past evidence on measurement error in contemporaneous and recalled data. Section 3 includes a discussion of our data source and methods of analysis. Section 4 lays out all our findings on the various comparisons we make and section 5 concludes with recommendations as to what kinds of information can be reliably collected using retrospective questions, how to improve retrospective data collection strategies to obtain more reliable information, and potential methods for correcting measurement errors.

2 Theories and Past Evidence on Measurement Problems in Current and Recalled Data

The literature on measurement error suggests a wide variety of issues that might contribute to measurement errors in both current (contemporaneous panel) and recalled (retrospective) data. The implications of measurement error depend substantially on the nature of the problems. Truly random errors in continuous variables will not bias estimates of key statistics such as means or estimates of linear regression models when serving as the dependent variable (Bound, Brown, & Mathiowetz, 2001). Random errors in an explanatory variable, x, will downward-bias or attenuate the estimated coefficient. Random errors in categorical or binary variables are more problematic as they bias model estimates and descriptive statistics.

When measurement errors are systematic, they will bias both basic statistics and regression coefficients in complex ways (Bound, Brown, & Mathiowetz, 2001). For instance, when studying the incomes of the self-employed, individuals with more education may keep accounting books and be able to more accurately report their incomes. If less educated individuals systematically under-report their incomes, this will systematically bias a regression estimating the relationship between years of education and income. Particularly for topics that relate to behaviors or states that have strong connotations of social (un)desirability, such as the intention to send children to school or the receipt of charity, respondents may intentionally misreport. Under-reporting will occur for socially undesirable phenomena, and over-reporting for desirable phenomena, generating "social desirability bias" (Bound, Brown, & Mathiowetz, 2001).

A large body of literature focuses on the recall or retrieval process and the nature of errors in recall. These are particularly likely to be affected by the recall period. That is, the longer the recall period, the more likely that respondents will report with error, although the

extent to which this is a problem varies substantially over studies of different outcomes (Bound, Brown, & Mathiowetz, 2001). Studies of panel data on dates have identified what is commonly referred to as a "seam effect," i.e. excessive numbers of changes at the "seam" between one study period and the next (Bound, Brown, & Mathiowetz, 2001).

The "salience" or importance of events may affect the accuracy with which they are reported (Bound, Brown, & Mathiowetz, 2001; Judge & Schechter, 2009). For instance, unemployment spells of only a few weeks may be of lower salience than unemployment spells that last a year and therefore be more likely to be forgotten. If individuals do remember events, they may not readily remember the exact timing of events. This leads to measurement errors such as "heaping," where individuals tend to report certain numbers as responses (Roberts & Brewer, 2001). For instance, respondents often report adult ages in years in multiples of 5 or child ages in months rounded to the nearest year or half year (Heitjan & Rubin, 1990; Roberts & Brewer, 2001). Question and questionnaire design can play an important role in whether respondent errors occur. Identifying the best respondent within a household, deciding on the level of aggregation for data, and asking for information in the most appropriate units and for the most appropriate reference period are important elements of design that will affect the accuracy of measurement (Grosh & Glewwe, 2000; Puetz, 1993).

A study on measurement error from the Malaysian Family Life Survey (MFLS) panel illustrates some of the issues that may occur in developing country data (Beckett, Da Vanzo, Sastry, Panis, & Peterson, 2001). The findings demonstrate that substantial errors can occur, but also that reporting of retrospective events can be quite accurate. For instance, while 95% of the ever-married sample reported being currently married in the first wave of the survey in 1976, twelve years later, only 84% of panel respondents reported in 1988 that they had been married in

1976. However, the same rate of mortality for children born prior to 1976 results from both the 1976 and 1988 interviews (Beckett, Da Vanzo, Sastry, Panis, & Peterson, 2001). The level of detail in the question affected the accuracy of reporting as well; for instance, agreement was much higher in reporting whether a child was ever breastfed than the duration of breastfeeding. The salience of events also mattered; women reported inter-district moves (a more substantial move) more consistently in 1976 compared to 1988 than intra-district moves. Different reporting errors with the MFLS were related to respondent characteristics.

A number of studies have also been conducted on measurement of income, assets, and consumption. Respondents tend to resort to inference, i.e. reporting mean income, as recall periods lengthen (de Nicola & Giné, 2014). Agricultural data show relatively little recall bias, although more salient events may be reported more accurately (Beegle, Carletto, & Himelein, 2012). Asking about the dates of major purchases directly elicits responses of similar quality to asking in relation to time cues (anchoring) important to the respondent; using unimportant time cues generates substantially worse results (de Nicola & Giné, 2014). Compared to a benchmark of personal diary use other diary and retrospective approaches have lower recall, with particularly acute problems for poorer, larger, and less educated households (Beegle, De Weerdt, Friedman, & Gibson, 2012). Questions on total rather than categorical expenditure suffer less recall bias (Hiroyuki, Sawada, & Mari, 2010). On the dynamic side, poverty mobility may suffer from: (1) inaccurate measures of income or consumption (2) price deflation and (3) mismatching of households over survey waves (Dercon & Shapiro, 2007).

3   Data and Methods

3.1   Data Sources

The Egypt Labor Market Panel Survey (ELMPS) provides a unique opportunity to assess data issues. With waves in 1998, 2006, and 2012, it is possible to use the ELMPS to compare retrospective and panel data over multiple periods. The ELMPS is a nationally representative household survey with detailed modules on current and past labor market statuses. Of the original 23,997 individuals interviewed in 1998, 13,218 (55.1%) were re-interviewed in both 2006 and 2012. Of the 37,140 individuals interviewed in 2006, 18,770 (77.5%) were re-interviewed in 2012.[1] A retrospective panel of annual statuses is constructed from retrospective data in each wave and compared to panel and retrospective data from previous waves. Reporting of time invariant information, such as parent's education, is also compared based on reports in different waves of the survey.

A particularly important element of our analyses relies on the labor market history section of the ELMPS surveys, which is administered to all individuals 15 and older who ever worked. In 2012, this section asks chronologically for the start dates (year, month) and characteristics of the first four labor market statuses (be it employment, unemployment or out of the labor force) lasting six months or more[2] from the time the individual exited school.[3] If the individual is employed in that status, she or he is asked about the details of such employment. Moreover, the total number of employment spells and their start and end dates can be obtained from the life events calendar section of the questionnaire.

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[1] See Assaad and Roushdy (2009) and Assaad and Krafft (2013) for a discussion of attrition from the various waves of the ELMPS.

[2] Statuses of less than six months are dropped and if four statuses are not enough to reach the current status, the fifth and later statuses are also dropped.

[3] For individuals who never went to school, the retrospective period starts at age 6.

It is important to note that in the preceding waves of the ELMPS survey (1998 and 2006), the labor market history questions were sequenced differently. Specifically, these waves of the survey used a reverse chronological order (starting with the current status and moving backward) in eliciting labor market trajectories as compared to the chronological method (starting with the first status and moving forward) used in 2012. In addition in 1998 and 2006, information was collected in a separate part of the questionnaire about the first job in which the individual was engaged for a period of more than six months. Unlike the 2012 wave, the 1998 and 2006 waves did not contain a life events calendar and therefore no information on the total number of primary jobs the individual engaged over his/her lifetime. This questionnaire design implies that initial unemployment and out of labor force states could be missed, as well as employment states between the first job and the pre-previous status.

3.2 *Methods*

To compare retrospective and panel data, the retrospective data were mapped on to panel data from previous waves in such a way that retrospective and current information is available for the same individual at the same point in time. We then draw on the econometric literature on measurement error to assess and compare the data sources and suggest possible corrections to account for measurement error (Black, Berger, & Scott, 2000; Bound, Brown, & Mathiowetz, 2001; Carroll, Ruppert, Stefanski, & Crainiceanu, 2012; Fuller, 1987; Magnac & Visser, 1999).

As a first check on the accuracy of the panel data, we begin by comparing the consistency of time invariant information across different waves of the panel. We do this for own education for adults 30-54 in 1998, father's sector of work when the individual was 15 years of age, and recalled costs of marriage. We then compare labor market statuses at a given point in time (1998

and 2006) across retrospective and panel data to assess the accuracy of recall and identify statuses that are particularly prone to erroneous recall. We subsequently estimate a multivariate model of the probability of alignment in labor market status between the two kinds of data as a function of individual characteristics, whether the information was elicited from the individual him/herself or a proxy respondent, the nature of the past employment status itself, and the contemporaneous employment status in 2012.

The next step is to assess the consistency of reporting of labor market transitions in retrospective and panel data. To do this, we convert the retrospective data into an annual retrospective panel, which contains information about the main labor market variables every year since the individual exited school. Using this retrospective panel we calculate the rate of change in employment status from 1998 to 2006 using the respective waves of the panel for those dates to the rates of change over the same period as reported by the 2012 retrospective data. We then move to comparing annual transition rates derived from the retrospective data in different waves of the survey.

In examining labor market transitions, we examine two types of transitions of particular interest to the study of labor market dynamics: transitions among employment, unemployment and out-of-labor-force states, and job-to-job transitions among the employed. Within the first type, we include job-finding rates for the unemployed and those out of the labor force, and separation rates from employment to either unemployment or out of the labor force. The second type includes two-way transitions across different types of jobs, such as public and private employment and wage and non-wage work. We examine how different waves of the retrospective data generate transition rates, by type of transition. Finally, we revisit the question of whether the levels and trends in important labor market measures, such as the employment-to-

population ratio and the unemployment rate, can be accurately assessed using the retrospective data, by comparing different waves of retrospective data and the retrospective and contemporaneous sources of these data, such as the official labor force survey.

4   Findings

4.1   *Consistency of reporting of time-invariant information across different waves of a panel survey*

4.1.1   *Own Education for Adults*

The accuracy of the characteristics individuals report in any survey, such as their age, education, or labor market characteristics, plays an important role in researchers' ability to accurately describe economies and labor markets. Because the ELMPS is a panel, we can compare characteristics that should remain largely unchanged, such as education (for adults) as reported in different waves of the survey in order to assess their accuracy. Figure 1 compares the reporting of education in 1998 with that in 2006 for individuals aged 30-54 in 1998. It is important to keep in mind that either the 1998 or the 2006 response could be inaccurate when they disagree, or both could be consistent (but still inaccurate) over time. Overall, 79% of responses are the same over time, but there is substantial variation in terms of which education categories are reported consistently. For instance, 90% of those who reported being illiterate in 1998 report being illiterate again in 2006. The remainder primarily reported being able to read and write but having no education certificate (7%), which could be a genuine change in literacy, or having primary education (3%). The ability to read and write appears to be the most poorly reported, with only 34% of those reporting they could read and write but having no education certificate in 1998 reporting the same status in 2006. The most common response in 2006 for this

group is being illiterate, which may represent a genuine decay in reading and writing ability. In general, when reporting is different, the reported alternative is usually a proximate level of education. For instance, 20% of those who reported general secondary in 1998 then report they attended vocational secondary in 2006.[4] Creating less finely aggregated categories, such as only four education levels, leads to more consistency in reporting. When the respondent is the individual in question in both rounds as opposed to a proxy respondent, there is a slight (but not dramatic) improvement in the consistency of data. [5]

Figure 1. Education (8 categories) as reported in 1998 vs. 2006, ages 30-54 in 1998 (percentage)
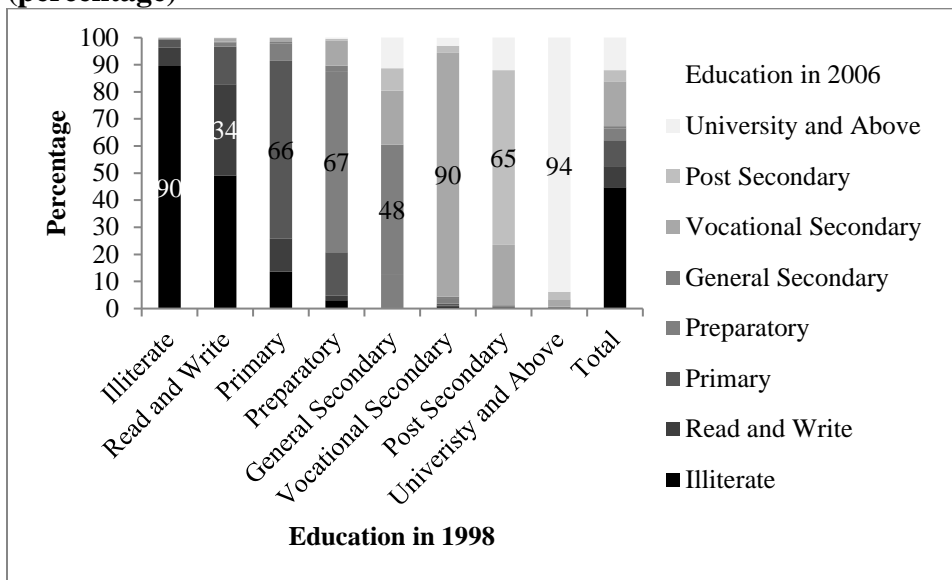
Source: Authors' calculations based on ELMPS 1998 and ELMPS 2006

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[4] Smaller categories, such as general secondary, may suffer from more mis-reporting due to their tiny size. For instance, if random typos are uniformly distributed across the categories, more responses in the smallest categories are likely to be errors.
[5] When individuals were not consistently responding for themselves, it is possible that the same individual was responding in their place in both waves (e.g. a spouse) but the data does not allow us to determine whether this is so.

4.1.2 Father's Sector of Work

The ELMPS collects information on father's characteristics when the respondent's age was 15 when the father is not present in the household. Figure 2 shows the consistency of responses over time (2006 versus 2012) among respondents aged 30-54 whose father was not present in 2006 or 2012. Approximately 71% of respondents who reported their father worked in government in 2006 then report that their father worked in government in 2012. Private sector work is relatively consistently reported (91% the same from 2006 to 2012). Consistency is most problematic in terms of identifying work in public enterprises. Only 35% of individuals who reported that their father worked in public enterprise in 2006 reported the same status in 2012. Instead, 40% reported their father worked in government and 25% reported their father worked in the private sector. The results suggest that respondents are sometimes inferring or reconstructing their father's sector of work. For instance, individuals may know that their father works in a utility but not know the sector of employment and reconstruct it. Analysis according to the identity of the respondent shows that only in regards to public enterprise work does having the individual in question be the respondent appreciably improve consistency.

Figure 2. Father's sector of work when age 15, as reported in 2006 versus 2012, father not in household in 2006 or 2012, age 30-54 in 2006 (percentage)
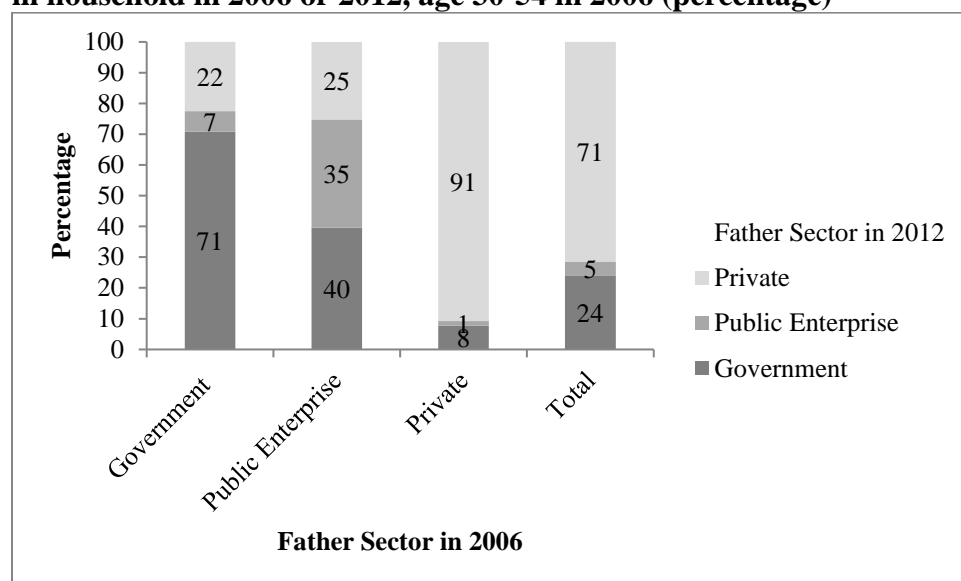
Source: Authors' calculations based on ELMPS 2006 and ELMPS 2012

*4.1.3   Recalled Costs of Marriage*

Understanding the investments individuals have made over time often requires asking about past outlays of expenditure. Individuals are expected to report the value of expenditures and investments at the time these outlays occurred. However, especially when individuals are inferring or reconstructing the value of an investment, for instance inferring the cost of their housing at the time of marriage based on their current cost or value of housing, this can cause problems in assessing trends over time. Essentially, individuals may (fully or partially) update past expenditures from nominal to real terms. Figure 3 shows the trends in the total costs of marriage over time for individuals who were married in 2012 and present in both the 2006 and 2012 waves (they may not have yet been married in 2006). Marriage is an enormous investment for young people and their families, and the cost of marriage and its trends in Egypt and in the MENA region are the subject of substantial concern and discussion (Assaad & Krafft, 2015a, 2015b; Assaad & Ramadan, 2008; Dhillon & Yousef, 2009; Salem, 2014, 2015, 2016;

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Singerman & Ibrahim, 2003; Singerman, 2007). The figure shows both the nominal (reported) costs and the real (inflated to 2012 LE) costs by year, as reported in both 2006 and 2012. Nominal marriage costs are clearly rising over time using both the 2006 and 2012 data, and the difference between those reports (when they overlap in time) may be due to inflation. Using the 2006 data and 2012 prices, it appears that from 2000 to 2006, marriage costs were flat or slightly declining, and averaged around 60,000 to 70,000 LE. Using the 2012 data and 2012 prices, looking at the same respondents' reported marriage costs from 2000 to 2006, it appears marriage costs were flat or slightly declining, but averaged around 90,000 LE. This is clearly inconsistent with what was reported in 2006, even when updated to 2012 prices. It appears individuals are partially (but not fully) updating nominal expenditures into real terms. Additionally, further investigation suggests different elements of marriage costs are updated differentially, likely related to how easy they are to recall or reconstruct.

Continuing to examine the 2012 data out to 2012 in real terms, it appears that marriage costs have fallen substantially over time, from around 90,000 in 2000-2006 to around 60,000-70,000 by 2012. This implies the cost of marriage over the 2000-2012 period decreased almost a third. However, looking back at marriage costs as reported in 2001-2006, and updated to 2012 terms, marriage costs have remained essentially constant, in the 60,000-70,000 range (in 2012 LE). This is evidence that, particularly when asked about events that are a number of years in the past, individuals may be inferring their value or inflating into current terms. This suggests that retrospective data should not be used to assess time trends for financial outlays; repeated cross sections or panel data are required. Comparing investments in the few years preceding a survey wave to investments in the few years preceding different survey waves will be more accurate for such data.

Figure 3. Total marriage costs (in LE), as reported in 2006 versus 2012, individuals in both waves, answering marriage section in 2012 (may not yet have been married in 2006)
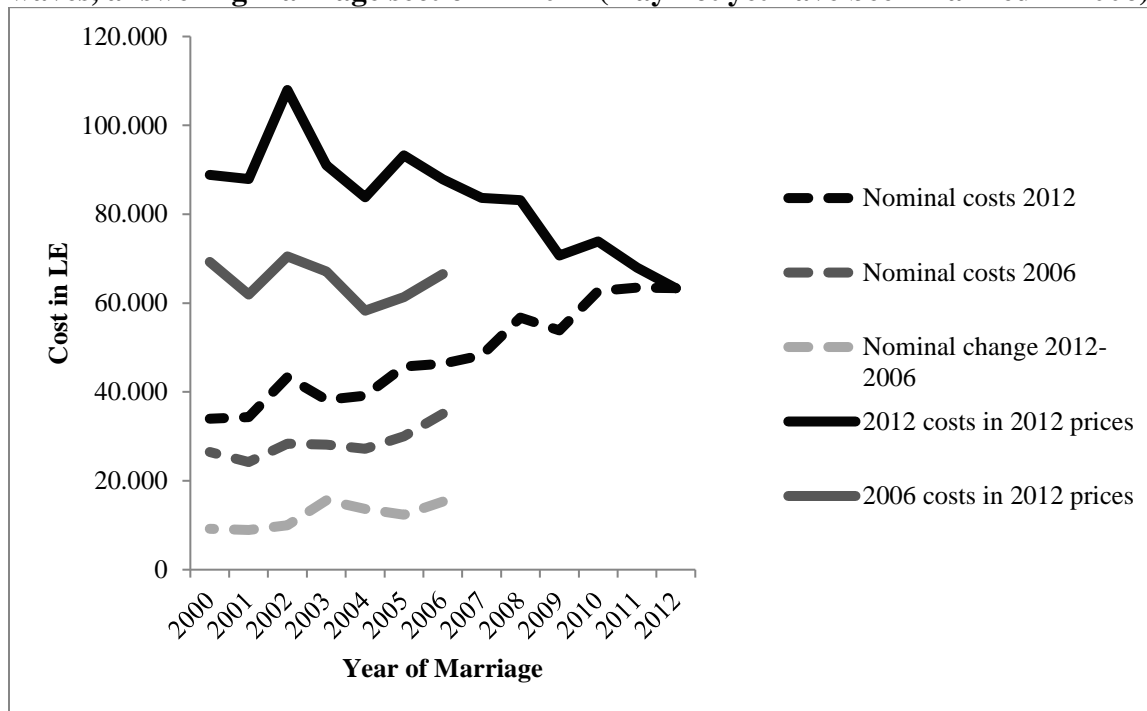
Source: Authors' calculations based on ELMPS 2006 and ELMPS 2012

4.2 Comparing labor market statuses across retrospective versus panel data

4.2.1 Alignment of labor market statuses in general

Individuals' labor market statuses, namely whether they are out of the labor force, unemployed, or employed, and if employed, their employment status, are at the heart of labor market surveys. This section first assesses whether aggregate statistics vary across retrospective and panel data and then whether the same individuals report consistently across contemporaneous and retrospective data. Figure 4 presents aggregate labor market statistics by gender for 1998 and 2006 using both contemporaneous statistics from the waves of the panel and retrospective reports from 2012 for those years. Notably, for males, the aggregates from both the retrospective and contemporaneous data are quite similar, with a few exceptions. Reporting of

public sector work, private formal and informal regular wage work, and self-employment are fairly similar. Irregular wage work is differentially reported in the retrospective data, which is likely because hours of work fluctuate over time; individuals may remain at the same job over time, but report that it is irregular in 2012 and map that back onto their status in previous years. Changes in regularity within the same job are also not captured within the labor market history and are difficult to detect. Regularity of employment should therefore not be assessed from retrospective data. Being an employer was more likely to be reported in the contemporaneous than in retrospective data. This is again because whether a self-employed worker hires other workers or not is a varying, time-bound status that is not easily recalled.

For individuals 30-54 in 2012, their ages would have been approximately 24-48 in 2006 and approximately 16-40 in 1998. Thus, it is only in 1998 that many would have been out of or transitioning into the labor force (unemployed). These statuses appear to be under-reported in the retrospective data, when comparing 1998 contemporaneous data to the retrospective data for 1998 from 2012. For instance, while 7% of males were unemployed in 1998 contemporaneously, in the retrospective data only 2% of males report being unemployed at that date. Likewise in the 1998 contemporaneous data, more individuals report being out of the labor force.

There are so few females in a number of labor market statuses that our assessment for women focuses primarily on the public sector, unpaid family work, unemployment, and being out of the labor force. Public sector work is quite consistently reported in the aggregates, which may be due in part to the stability of this employment status. Unpaid family work, which includes subsistence work, is much more frequently reported in the contemporaneous data (6-10% across years) than in retrospective data (3-4%). This may be in part because individuals are only asked the labor market history in 2012 if they report having ever worked in market work,

and unpaid family workers may frequently switch into and out of market work, sometimes

producing agricultural goods for their own subsistence and sometimes selling them on the market.

Unemployment is also more frequently reported in the contemporaneous data than in the

retrospective data. This is likely due to the fact that many women who search for work never end

up working (Assaad & Krafft, 2014) and thus are not asked the questions in the labor market

history. As a result of these patterns in reporting employment, being out of the labor force is

higher in the retrospective than contemporaneous data for women.

Figure 4. Labor market status, as reported contemporaneously for 1998 and 2006 and as reported retrospectively for those years from 2012 data, by sex, individuals ages 30-54 in 2012 present in both waves (percentage)
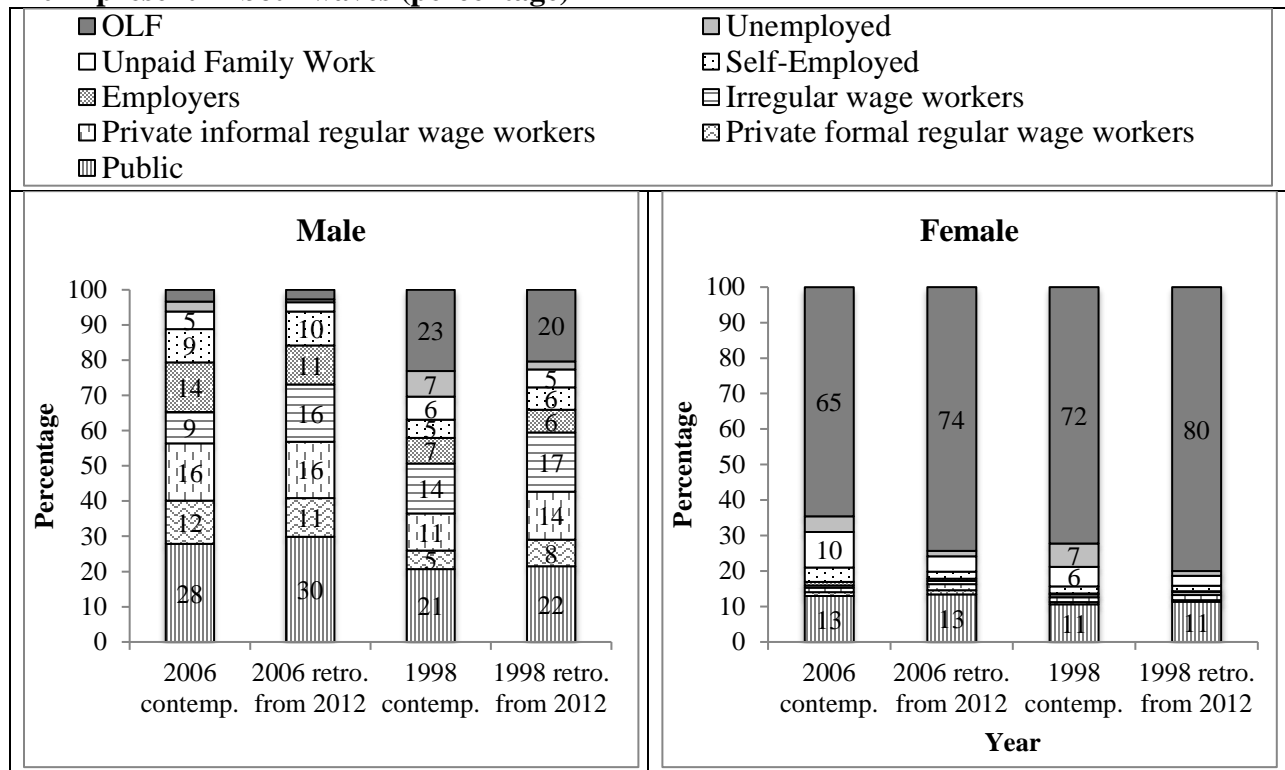
Source: Authors' calculations based on ELMPS 1998, ELMPS 2006 and ELMPS 2012

A number of labor market statuses are particularly prone to misreporting over time,

comparing retrospective and contemporaneous data. Figure 5 presents the distribution of

retrospective statuses going back from 2012 to various years by the status reported contemporaneously from 2006 or 1998 for individuals 30-54 in 2012, by sex. This essentially compares the aggregates of Figure 4. There is somewhat greater inconsistency comparing 1998 statuses than 2006 statuses, which is likely due to recall deteriorating over time. While public sector employment tends to be reported quite consistently, other labor market statuses are frequently not reported consistently. Formal private sector work tends to be more consistently reported than informal work. Most reporting of wage work is consistent, but the type of wage work is not consistently reported. Distinctions between self-employment and being employers are likewise blurred. A higher degree of inconsistency is also apparent for those who were unemployed or out of the labor force. Some of this may be because the duration of these statuses is shorter, so the contemporaneous status may be off relative to the status that is measured as the predominant status for the year in the retrospective data. Females have a much higher probability than males of reporting that they are out of the labor force (which is their predominant status). Less formal forms of employment in the contemporaneous data for women, such as being an employer, self-employment, and unpaid family work are particularly likely to be reported as being out of the labor force. Being unemployed suffers from a similar problem, likely, as mentioned earlier, due to the large share of women who are unemployed but ultimately never find work.

The problems associated with detecting employment even contemporaneously among marginally employed women in agriculture and animal husbandry in Egypt are well known (Anker & Anker, 1995; Assaad, 1997; Langsten & Salem, 2008). These problems are compounded when the question refers to a reference period well in the past. Further examination of the data demonstrated that a key problem is detection of whether women ever worked at all.

Among the women examined, just two-thirds (67%) of those who were identified in 2006 as engaging in market work reported that they ever worked in 2012. This was not a problem for men (<1%). The problem is primarily driven by women who were no longer working in 2012; all of those working in 2012 were, of course, identified as having ever worked. However, among women who were not working in 2012 but were working in 2006, only 16% reported ever working in 2012. Only those who report ever working are asked the labor market history, and thus these women are considered to never have worked and no labor market history data is collected.

Further analysis of the data demonstrated that reporting whether women ever worked at all varied substantially by the respondent. Among the women examined where a proxy respondent was providing data, just 55% of those who were identified in 2006 as engaging in market work reported that they ever worked in 2012, compared to 71% when the respondent was consistently the individual herself. Among women who were not working in 2012 but were working in 2006, only 11% reported ever working in 2012 when it was not consistently the individual in question reporting, and only 19% when it was the individual reporting. While both illustrate extremely low rates of reporting work, having the individual in question as the respondent did lead to increased accuracy in regards to ever working.

These patterns, as with education, suggest a number of issues for analyzing labor market statuses and dynamics. For instance, a category of private wage work, incorporating regular formal and informal and irregular workers would be more consistently reported than the disaggregated categories, and transitions between regular/irregular and, to a lesser extent, formal/informal may be poorly reported over time. Self-employment and being an employer also often are mixed up, and might be better combined into a single category. For females,

retrospective data should be treated with particular caution, as women may not report ever working when they have done so, or report being out of the labor force when they were in fact working in the private sector.

Figure 5. Labor market status, as reported in 1998 or 2006 versus 2012 retrospective data for 1998 or 2006, by sex, individuals ages 30-54 in 2012 present in both waves (percentage)
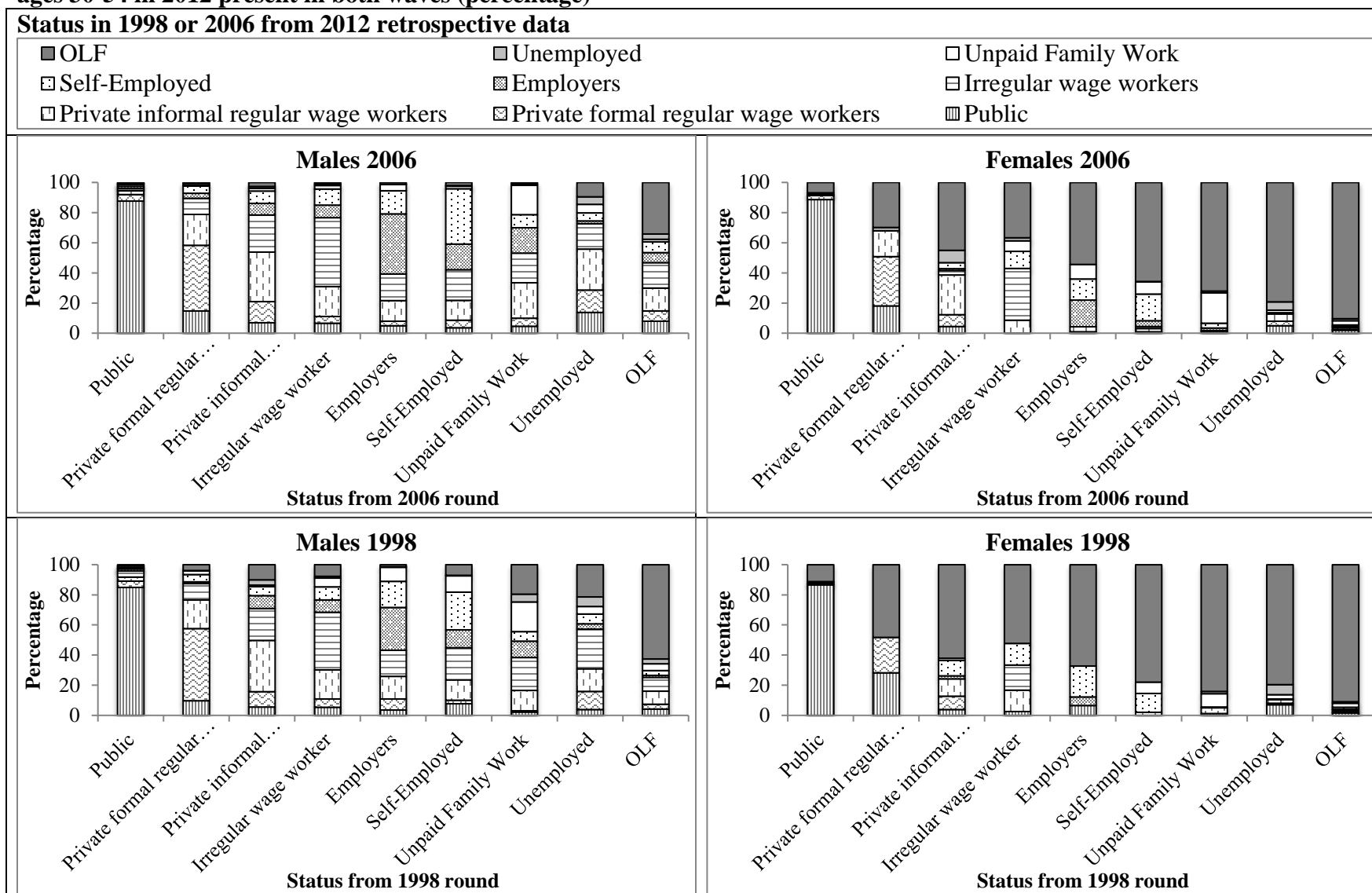
Source: Authors' calculations based on ELMPS 1998, ELMPS 2006 and ELMPS 2012

We had initially expected substantially more consistent reporting of labor market statuses when the individual was responding for him/herself. However, that was not the case. The lack of higher consistency when the individual is reporting for him or herself could be due to a variety of reasons. It may be that individuals are more accurate reporters, but not necessarily more consistent reporters, in that when others are reporting on behalf of an individual they provide consistent but potentially inaccurate responses, or increase consistency by simplifying the labor market history.

4.2.2 Recalling past unemployment spells

While the aggregate labor market statistics are not substantially different, the inconsistency of individuals' responses over time is troubling. This section attempts to analyze some of the patterns and sources of disagreement in the data sources, focusing on the case of unemployment, the occurrence and duration of which is of particular interest within the Egyptian and MENA labor markets (Assaad & Krafft, 2014; Kherfi, 2015). The inconsistencies between contemporaneous unemployment and retrospective unemployment reporting could be occurring for a variety of reasons. Because only individuals who ever worked are asked the retrospective questions, excluding women who sought but never began work, this section focuses solely on the unemployment dynamics of individuals who ever worked and examines several different questions essentially revolving around the issue of why there are inconsistencies across the data sources. Do individuals report unemployment in their retrospective histories, but just during a different year? Are shorter spells of unemployment more likely to be forgotten with time?

Since the primary concern is that unemployment is under-reported in the retrospective data, in Table 1, for those who reported unemployment in the 2006 or 1998 waves,[6] we examine the reports of unemployment in the retrospective data and a number of characteristics, including the mean current unemployment duration at the time of survey, and the percentage of individuals who experienced short (less than six month) current unemployment durations as of the time when they were surveyed. Notably, for those unemployed in the contemporaneous data for 1998, just 9% of unemployment statuses in the 2012 retrospective data for 1998 included a report of unemployment. The alignment was slightly better in 2006, when 13% were aligned. Individuals who were unemployed in 2006 were more likely to report unemployment within one year (5%) or two to five years (12%) than those unemployed in 1998 (1% reported unemployment within one year and 7% within two to five years). More individuals reported being unemployed at some point more than five years out in 1998 (11%) than in 2006 (7%). Reporting of unemployment is less accurate, both in terms of reporting at all and the timing of unemployment, going further back in time.

Notably, 71% of individuals who were contemporaneously unemployed in 1998 did not ever report being unemployed in the labor market histories. Because the labor market histories in 2012 go forward in time, it is possible that unemployment occurred after the fourth status (the last status asked in the labor market history). Therefore, those with a fourth status are separated out, and comprise a small share of the distributions (6% for those unemployed contemporaneously in 1998 and 4% of those in 2006).

The characteristics of unemployment, specifically its duration to date as of the contemporaneous status reported in 1998 or 2006, are related to the probability of accurately

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[6] Data is not separated by gender or restricted by age so as to ensure an adequate sample size.

reporting. Those whose reporting aligned had, on average, long durations of unemployment to

date, 23 months in 1998 and 16 months in 2006. Those who reported their unemployment, but

with imprecise timing, tended to have shorter durations of unemployment than the average, a

year or less. Those who never reported being unemployed in the retrospective data had slightly

longer than average unemployment durations. Overall, it appears that gathering data on historical

patterns of unemployment, even among those who ever worked, is likely to produce substantially

different results than using contemporaneous data. It seems likely that retrospective data will

both under-report past unemployment and distort its characteristics. Having the individual in

question report for him or her self does not substantially improve the reporting of unemployment.

Table 1. Patterns of unemployment reporting as reported in 1998 or 2006 versus 2012 retrospective data for 1998 or 2006, individuals reporting contemporaneous unemployment in 2006 or 1998 and present in 2012

Comparison to retrospective data	Dist. (%) 1998	Mean current unemp. dur. mos. 1998	% less than six months 1998	Dist. (%) 2006	Mean current unemp. dur. mos. 2006	% less than six months 2006
Aligned	9	23	26	13	16	31
Unemployed within one year +/-	1	0	22	5	5	24
Unemployed within two-five years +/-	7	12	33	12	9	38
Unemployed more than five years +/-	11	8	31	7	7	46
Never unemployed but have a fourth status	6	15	35	4	27	31
Never unemployed no fourth status	65	22	41	59	22	29
Total	100	19	37	100	18	32
N	261	261	261	443	443	443

Source: Authors' calculations based on ELMPS 1998, ELMPS 2006 and ELMPS 2012

4.2.3 Multivariate models of alignment between retrospective and panel data

Particularly concerning in assessing measurement error is whether errors are systematic (related to covariates). Such relationships will bias any attempts to examine the relationship between covariates and mis-measured outcomes. To assess whether there are systematic patterns of misreporting, in Table 2 we run probit models for whether individuals' responses about their contemporaneous (panel) data in 1998 and 2006 were consistent with their (2012) retrospective data for those years. Models are restricted to those who were 30-54 in 2012 and run separately for males and females and therefore allow for a comparison of how characteristics are related to reporting both by gender and over varying retrospective spans from 2012.

The probability of alignment in reporting is high for the reference case, a 30-34 year-old university educated individual living in Greater Cairo, who did not consistently respond for him or her self, was a public wage worker in the 1998/2006 contemporaneous (panel) data, was employed in 2012, was a regular worker in 2012, and was a formal worker in 2012. For retrospective data referring to 1998, the reference case has a probability of alignment between retrospective and panel data of more than 0.9. This is actually lower, around 0.8, for retrospective data referring to 2006. Those 30-54 in 2012 would have been 24-48 in 2006 and 16-40 in 1998. Individuals may have better retrospective recall about first statuses than subsequent non-current statuses, such that the 1998 data, with more first statuses, is more consistent. Compared to the university educated, for males recalling distant (1998) statuses, all other education levels perform significantly worse, but this pattern does not hold for females or males recalling less distant (2006) statuses.

For males, compared to those 30-34 in 2012, those 35-44 in 2012, but not those 45-50 had significantly less alignment. For women there was at most a small increase in consistency in

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reporting among older females (45-50) for more recent (2006) statuses. Few regional differences occurred, with only slightly better alignment in Upper Egypt for male's more recent (2006) statuses. After controlling for other characteristics, there were no significant differences in consistency dependent on whether or not the respondent was consistently the individual in question.

Large differences did occur by both the retrospective status and 2012 employment characteristics. Compared to public wage workers in the contemporaneous data (1998/2006), private wage workers were significantly less likely to have consistent reports, non-wage workers even more so. For men, those not working in the contemporaneous data were also significantly less likely to report consistently, but there were no such differences for women. The magnitude of the differences is substantial; non-wage males had around a 30 percentage point higher probability of disagreement, and non-wage women 66-72 percentage points. For males, there are mixed differences comparing the effect sizes back to 2006 versus 2012. For females, more recent reporting is consistently more aligned, although not by large margins. In terms of 2012 employment characteristics, females not employed in 2012 are significantly more likely to consistently report their 1998 status, but not their 2006 status, while for males those not employed in 2012 are significantly less likely to report their 2006 status but not their 1998 status. Both males and females who were irregular in 2012 were significantly less likely to report their 2006 statuses consistently but not their 1998 ones, possibly due to the rising volatility of their employment being relatively recent (Assaad & Krafft, 2015c). Those males who were informal in 2012 were significantly less likely to report their 1998 status consistently. Overall, there are mixed relationships between 2012 status and recall of past statuses, but definite disagreements related to the contemporaneous (panel) employment type in the preceding 1998 or 2006 wave.

Table 2. Probit model marginal effects for the probability of alignment of reporting between contemporaneous 1998 or 2006 and 2012 retrospective data by sex, individuals in 2006 or 1998 and present in 2012, ages 30-54 in 2012

	1998 Male	1998 Female	2006 Male	2006 Female
Reference Case Probability:	0.927	0.918	0.811	0.796
Own Education (Univ. omitted)				
Illit. or R&W	-0.185***	0.017	-0.011	0.051*
	(0.042)	(0.028)	(0.028)	(0.022)
Basic	-0.225***	0.021	-0.021	0.049
	(0.040)	(0.032)	(0.028)	(0.027)
Secondary	-0.196***	-0.047	-0.026	0.028
	(0.030)	(0.026)	(0.023)	(0.021)
Age group in 2012 (30-34 omit.)				
35-39	-0.132***	-0.042*	-0.012	-0.002
	(0.033)	(0.018)	(0.021)	(0.017)
40-44	-0.136***	-0.067**	-0.008	0.017
	(0.041)	(0.022)	(0.024)	(0.017)
45-49	-0.049	-0.041	0.000	0.033*
	(0.042)	(0.022)	(0.026)	(0.016)
50	-0.021	-0.037	0.031	0.042*
	(0.044)	(0.021)	(0.027)	(0.018)
Region (Gr. Cairo omitted)				
Alex. and Suez Canal	0.027	0.032	-0.003	0.022
	(0.043)	(0.027)	(0.032)	(0.024)
Urban Lower	0.067	0.052	0.046	0.037
	(0.040)	(0.027)	(0.029)	(0.022)
Urban Upper	0.057	0.030	0.071*	0.028
	(0.038)	(0.024)	(0.029)	(0.021)
Rural Lower	0.008	0.030	0.025	0.007
	(0.039)	(0.026)	(0.026)	(0.020)
Rural Upper	0.066	0.038	0.058*	0.012
	(0.041)	(0.025)	(0.028)	(0.021)
Consist. Respondent (Not consist. omit.)				
Consist. resp.	-0.011	0.001	0.005	0.018
	(0.027)	(0.018)	(0.016)	(0.012)
Panel (1998 or 2006) employment status (public wage omit.)				
Private wage	-0.105**	-0.604***	-0.137***	-0.459***
	(0.037)	(0.088)	(0.025)	(0.052)
Non-wage	-0.305***	-0.718***	-0.290***	-0.663***
	(0.042)	(0.052)	(0.025)	(0.033)
Not working	-0.276***	0.005	-0.547***	-0.017
	(0.041)	(0.036)	(0.037)	(0.026)
2012 Employment chars.				
Not employed in 2012	-0.054	0.224***	-0.202***	0.005
	(0.061)	(0.041)	(0.044)	(0.024)
Irregular in 2012	-0.070	-0.127	-0.082***	-0.269**
	(0.036)	(0.100)	(0.021)	(0.084)
Informal in 2012	-0.010	-0.111***	0.008	0.052
	(0.029)	(0.030)	(0.021)	(0.043)
N(Obs.)	2408	2465	4540	4656

4.3   *Comparing labor market transition rates across retrospective versus panel data*

An important application of retrospective and panel data on labor market statuses is measuring transition rates between different labor market statuses in order to assess labor market dynamics. We have demonstrated that there could be substantial misalignment between contemporaneously measured statuses and ones measured by means of retrospective questions, but also that the overall distribution of statuses is fairly similar (Figure 4). If the measurement errors are primarily an issue of random errors in the reporting of the timing of statuses, measures of labor market transition rates could still be fairly sound. However, if entire statuses are lost (as appears to be the case for unemployment), then measures of labor market dynamics will be understated and will point to a more rigid labor market than is actually the case. Because the ELMPS contains three panel waves, it is actually possible to assess labor market transition rates by using either purely retrospective or purely panel data. This section specifically compares transition rates, by status, from 1998 to 2006, based at first on the 1998 and 2006 panel data, and second, on the retrospective data collected in 2012 for 1998 and 2006. This analysis is performed only for individuals who appear in all three waves and who were 30-54 in 2012. The status used for classification purposes comes from either the retrospective or the panel data, depending on which data are being used to calculate the transition rates.

There are some key points to keep in mind when considering this comparison. The contemporaneous status is (as is the case throughout this paper) the "usual" status in the 3-month period preceding the survey, if an individual is employed. In the retrospective data coming from the labor market history module of the survey, statuses have to be at least six months long to be reported. It is therefore likely that in the panel data some of the transitions that are detected relate

to statuses that lasted less than six months and that would not be observed by definition in the retrospective data. This would tend to inflate panel data transition rates upward, but probably not by much. We know from the 2012 contemporaneous data that only 1.6% of employed individuals have a different primary job in the reference week than in the reference three months, suggesting that short-term transitions are rare. Transition rates in the panel data are therefore only likely to be inflated by a few percentage points at most. Although the probability of reporting statuses across panel and retrospective data is fairly similar (Figure 4), the differences that do exist are going to affect the measurement of transition rates as well.

In Figure 6, the rates of change in the various labor market statuses are assessed using panel and retrospective data sources, according to the 1998 status. Notably, transition rates for males are understated by about half in the retrospective data relative to the panel data (35% versus 59%) and by about two-thirds for females (9% versus 33%). Looking across statuses, every employment status in 1998 suffers from under-reporting problems in the retrospective data, but to varying degrees. For males, transitions out of unemployment and OLF statuses are fairly comparable, but this is not the case for females.

Figure 6. Rates of status change (percentage) in panel data for 1998 to 2006 versus rates of status change in retrospective data from 2012 for changes from 1998 to 2006 by sex and status in 1998
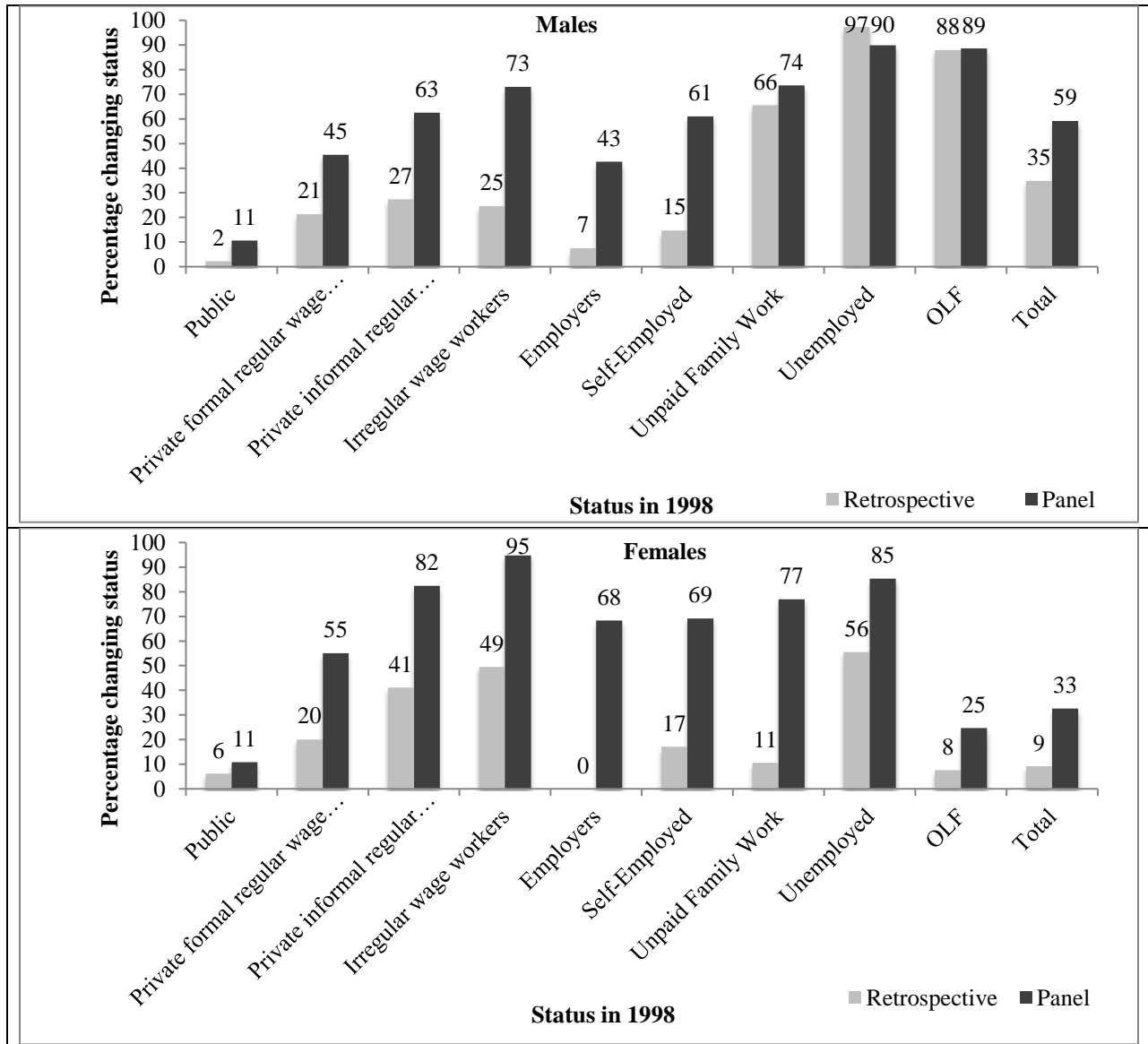
Source: Authors' calculations based on ELMPS 1998, ELMPS 2006 and ELMPS 2012
Notes: Based only on individuals in all three waves. Status in 1998 is from either retrospective or panel data depending on whether transition rates are being examined for retrospective or panel data.

As well as differential rates of change, there are differential patterns of change in terms of

which transitions are detected or not detected (not shown). More subtle transitions, such as

transitions from informal to formal private wage work or from employer to self-employed and

vice versa, are more likely to be missed in the retrospective data. More distinctive transitions—

such as those between public and private sector jobs and between wage and non-wage work—are

also somewhat under-reported in the retrospective data, but to a lesser extent. Particularly for

women, the retrospective data is less able to detect transitions into and out of the out of the labor

force, a problem related to the issue we discussed earlier about the difficulty in detecting

women's self-employment and unpaid family labor in the Egyptian context. Women in the public

sector are much more likely to report being employed in the past. Since they typically have low

transition rates, this tends to understate overall transition rates for women.

4.4   Comparing the levels and trends of annualized labor market transition rates across retrospective data from different waves of the survey

4.4.1 Measuring annualized transition rates from retrospective data

To further investigate the extent to which the ELMPS retrospective data suffer from

measurement problems, we compare the transition probabilities obtained from the retrospective

data for the same time period as assessed by different waves of the survey.[7] The dynamics we

focus on are primarily the job finding (*f*) and separation rates (*s*), which can be defined as the

share of employed, *E*, and non-employed, *NE,* changing states over time, *t*;

$$f = \frac{NE_{t-1} \rightarrow E_t}{NE_{t-1}}$$

$$s = \frac{E_{t-1} \rightarrow N\square_t}{E_{t-1}}$$

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[7] See *[Working paper version, citation removed for anonymous peer review]* for a discussion of how different approaches to data inclusion affect estimation of dynamics.

4.4.1 Separation rates

Using the data from three waves of the survey, in Figure 7 we overlap the separation rates calculated over the years.[8] This is done for male workers between 15 and 54 years of age in year *t*. A remarkable increase in the separation rates in the most recent year of each survey is observed. This tends to be true for both men and women (not shown) but is much more pronounced in the male workers' trends. The ELMPS 2012, most likely due to the chronological design of the retrospective accounts, is doing a better job than the ELMPS 2006 in capturing the transitions of individuals that are further back in time. Still, if we take more recent rates as true, both are underestimating the employment to non-employment transitions.

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[8] Given that the survey was designed to capture only retrospective labor market statuses that last for more than 6 months, we reclassify, for comparability, those short unemployment spells as employment lags.

Figure 7. Employment to non-employment separation rate (percentage) by wave, males ages 15-54, 1990-2011
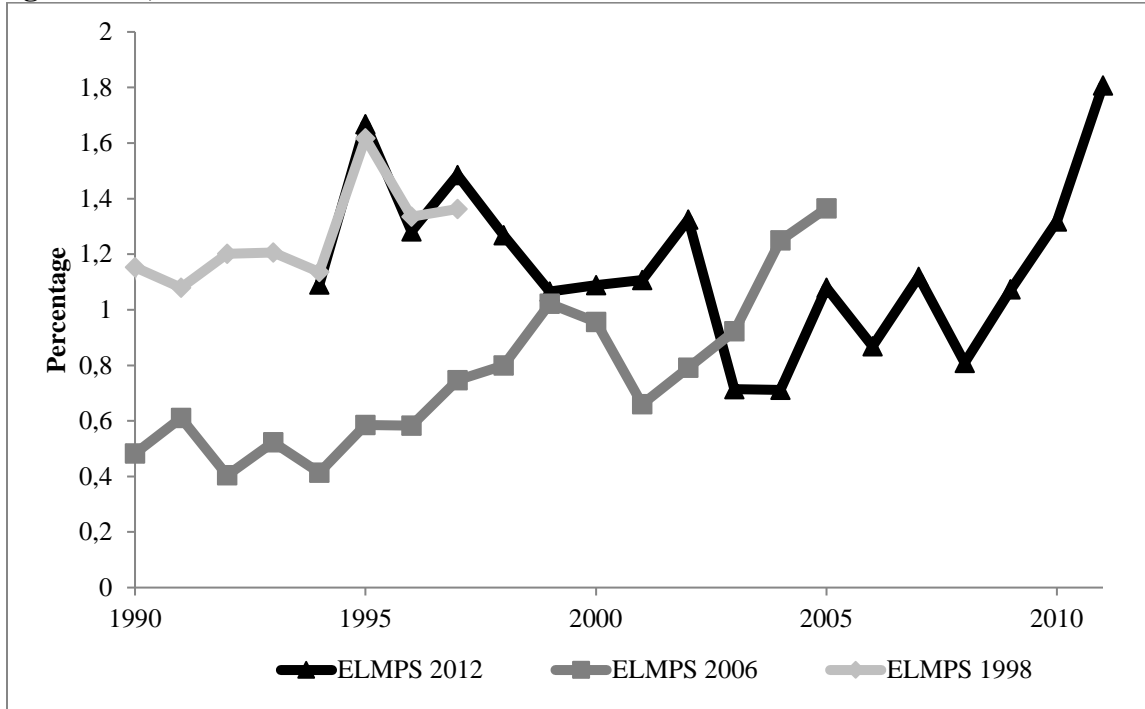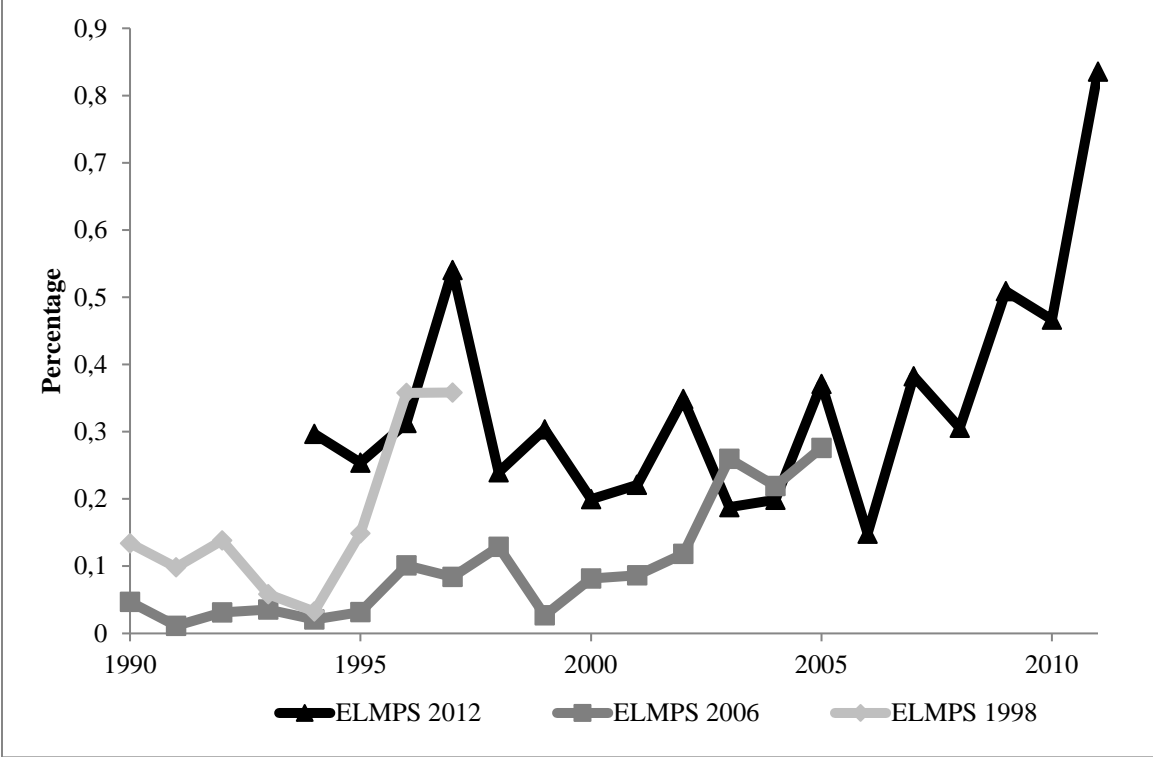

Source: Authors' calculations based on ELMPS 2006 and ELMPS 2012

As a further investigation into how employment to non-employment transitions are under-estimated using retrospective panels, we repeat the above exercise making a distinction between unemployment and inactivity states. We replot the three separation rate time series, first using employment to unemployment transtions then second using employment to inactivity transitions. Figure 8 and Figure 9 show that the increase in the separation rates continues to appear for both types of separations. While the underestimation is much attenuated for the employment to unemployment transitions by excluding from the contemporaneous reports the unemployment statuses that lasted less than six months, this cannot be done for the employment to inactivity transitions, where the date of start of the status is not accessible and hence assumed to be at the begining of the year. In fact the retrospective accounts of the ELMPS 2012 seem to capture well the employment to unemployment separation rates. The underestimation of the

separation rates observed in Figure 14 for the ELMPS 2006 and 2012 seem to result mainly from

including inactivity statuses lasting less than six months as shown in Figure 16. The increase in

the employment-to-inactivity separations seem to occur gradually over the most recent two years.

Figure 8. Employment to unemployment separation rate (percentage) by wave, males ages 15-54, 1990-2011

Source: Author's calculations based on ELMS 1998, ELMPS 2006, and ELMPS 2012

**Figure 9. Employment to inactivity separation rate (percentage) by wave, males ages 15-54, 1990-2011**
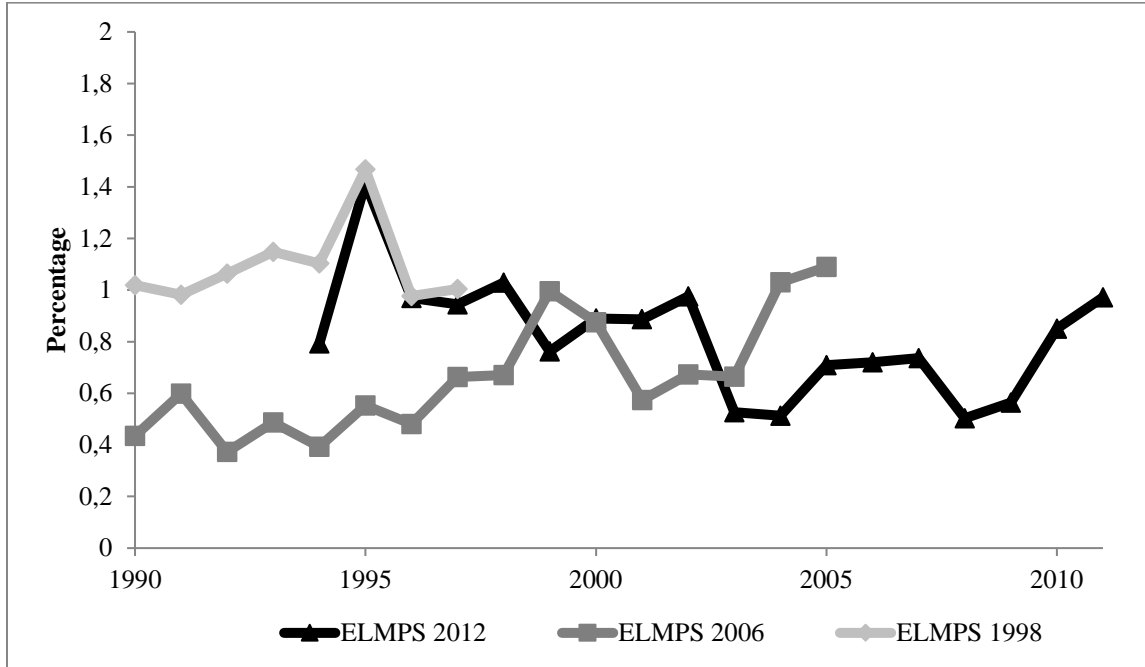
Source: Author's calculations based on ELMS 1998, ELMPS 2006, and ELMPS 2012

4.4.2 *Job-finding rates*

Moving now to the dynamics of the job finding process, recall that these rates are the

flow of workers from non-employment to employment relative to the stock of non-employed. As

was true for the job separation rates time series, we again suspect an underestimation of the non-

employed as we go back in time. This time, the job-finding rates calculated from our

retrospective panels are over-estimated. The most reliable point, in terms of the level of the job-

finding rate, is likely to be the most recent point. Figure 10 shows the overlap of the the three

finding rates time series. We note likely over-estimation in the job finding rates mirroring the

under-estimation we noted above in the separation rates. If we set aside the levels issue, and

focus of the trend of both finding and separation rates over time, we note that there has been a

very slight increase in the job finding rates over time for males and almost no substantial change

for the females. A relatively higher increase in the separation rates over time is observed for both

male and female workers.

Figure 10. Non-employment to employment job finding rate (percentage) by sex and wave, ages 15-54, 1990-2011
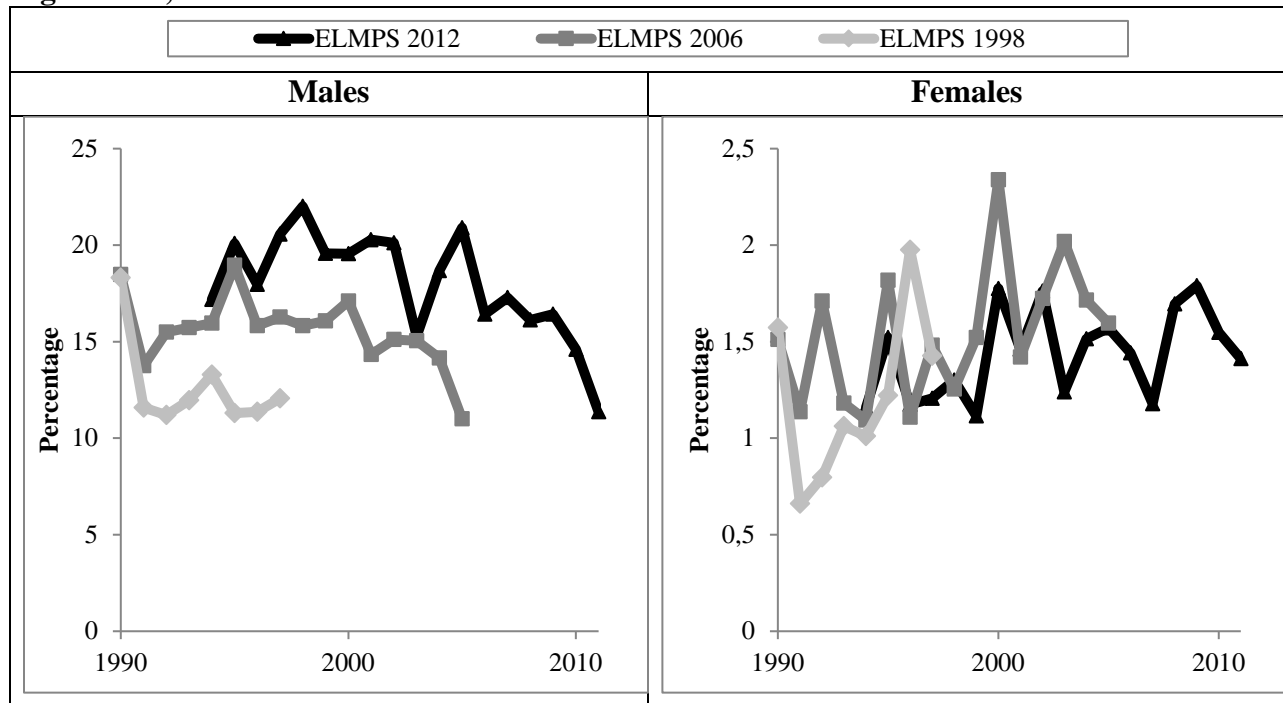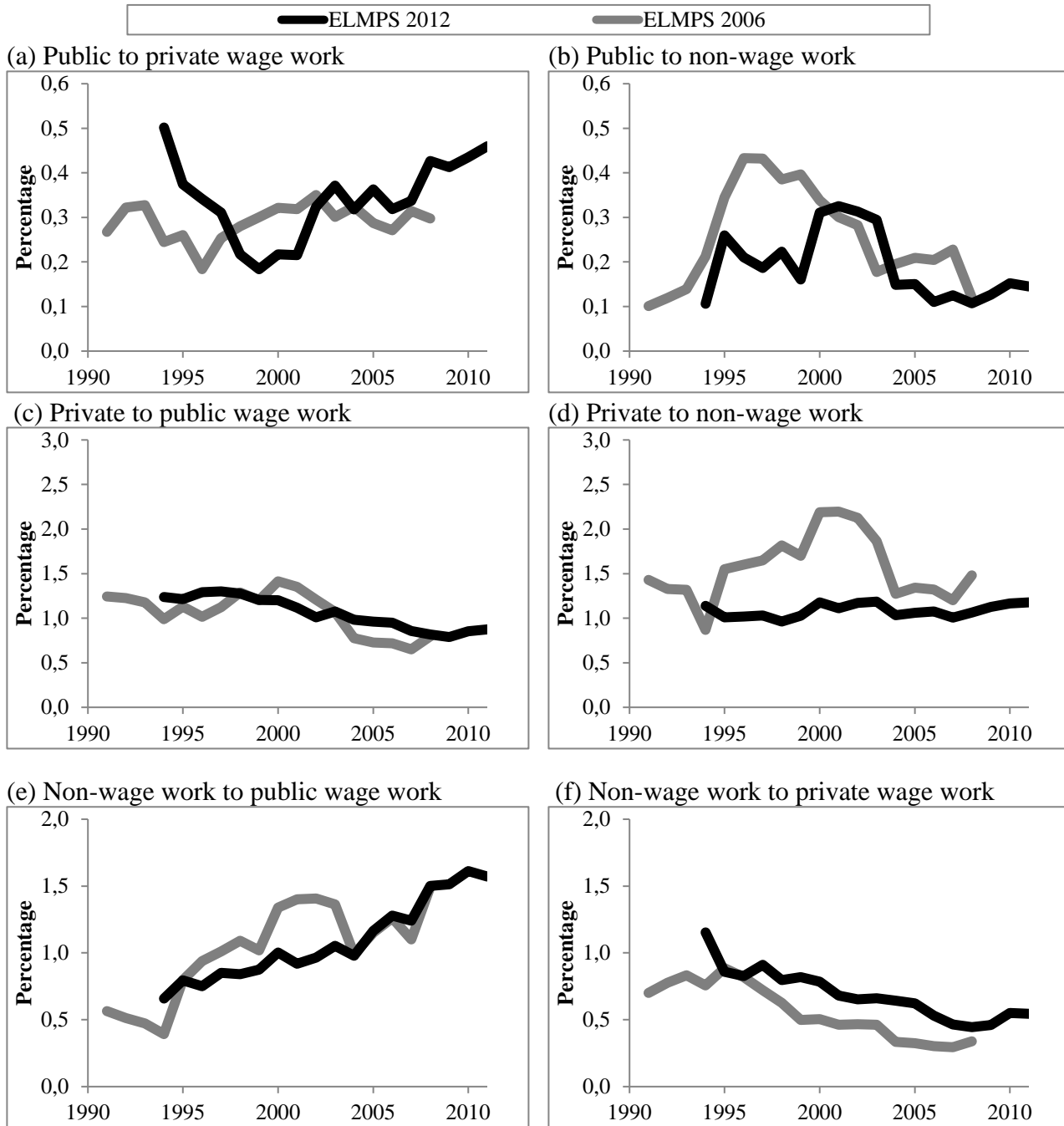

Source: Author's calculations based on ELMS 1998, ELMPS 2006, and ELMPS 2012

4.4.3   *Job-to-job transitions*

Having examined states and transitions between employment and non-employment, we

now examine job-to-job transitions among the employed. The comparisons of retrospective and

panel data show that more aggregated employment statuses are likely to be more consistently

reported. For instance, it appears that respondents have difficulty distinguishing between

informal and formal employment states as well as regular and irregular work. Therefore, we limit

the analysis of the retrospective transitions rates to three broad employment sectors, namely

public wage work, private wage work and non-wage work.

In Figure 11, we overlap the job-to-job transition rates calculated using the ELMPS 2006 and ELMPS 2012 retrospective panels. These rates are obtained by dividing the number of workers transitioning from one sector to another between years *t* and *t+1* by the number of workers employed in the origin sector in year *t*. Generally, we observe a close overlap of the job-to-job transition rates obtained using the two different retrospective panel data sets. This finding suggests that using retrospective accounts give consistent conclusions about the trends of job-to-job transition rates over time, especially when these trends are smoothed. However, it's crucial to note that the levels of these transition rates are under-estimated given what we saw earlier in Figure 6.

Figure 11. Job-to-job transitions by wave for 2006 and 2012, male workers, ages 15-54, 4 period moving averages, 1990-2011
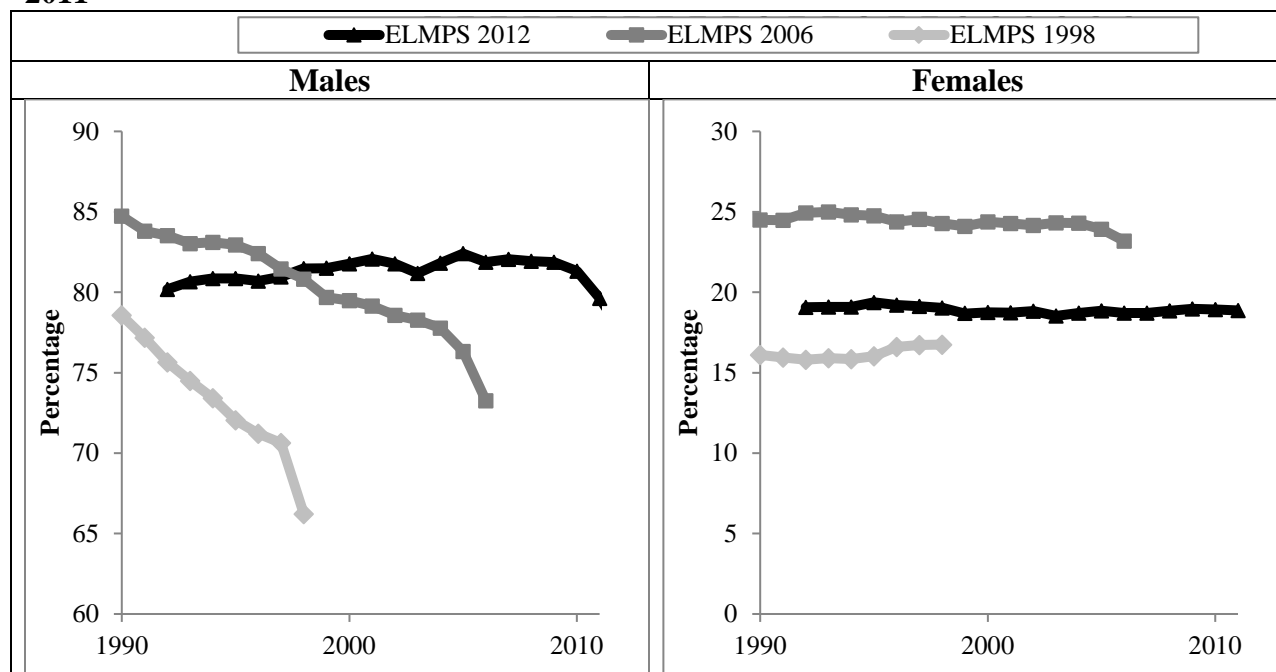

Source: Authors' calculations based on ELMPS 2006 and ELMPS 2012

4.5   *Do retrospective data provide accurate trends of past labor market aggregates?*

The problems we observe in assessing labor market dynamics using retrospective data also present challenges to assessing stocks over time. This section examines the stocks derived from the retrospective data for two specific statistics: the unemployment rate and the employment to population ratio (employment rate). Figure 12 illustrates the evolution of employment to population ratio. The pattern suggests that as we go back in time, we only retain the employment states of our sample and lose track of their non-employment history. We obtain as a result a decreasing employment-to-population ratio, which is not consistent with patterns observed contemporaneously in the panel (Assaad & Krafft, 2015c). The magnitude of the decrease differs from one survey to the other. The ELMPS 2012 seems to have less of a decreasing trend than the ELMPS 2006. The most likely explanation for this observation is the different structure of the ELMPS 2012 questionnaire. Asking individuals about their past statuses in a chronological order, starting with the first status, rather than backward in time as was the case in 1998 and 2006, may have increased consistency of employment trends. However, none of these approaches recovers the pattern of employment observed in the panel contemporaneous statuses, which is an increase in employment rates over time for men and a rise and fall in employment rates for women.

Figure 12. Employment-to-population ratio (percentage) by sex and wave, ages 15-54, 1990-2011

Source: Authors' calculations based on ELMS 1998, ELMPS 2006, and ELMPS 2012

Superimposing the retrospective data and the unemployment rates from the official Labor Force Sample Survey (LFSS) further illustrates how the proportions of different labor market states and consequently labor market transitions get distorted if one uses retrospective data. Figure 13 shows that the retrospective data does not align with the evolution of Egypt's unemployment rate over the past two decades as reported in official statistics.

Figure 13. Unemployment rate (percentage), males, ages 15-64, by wave and compared to those reported in the LFSS, 1990-2011
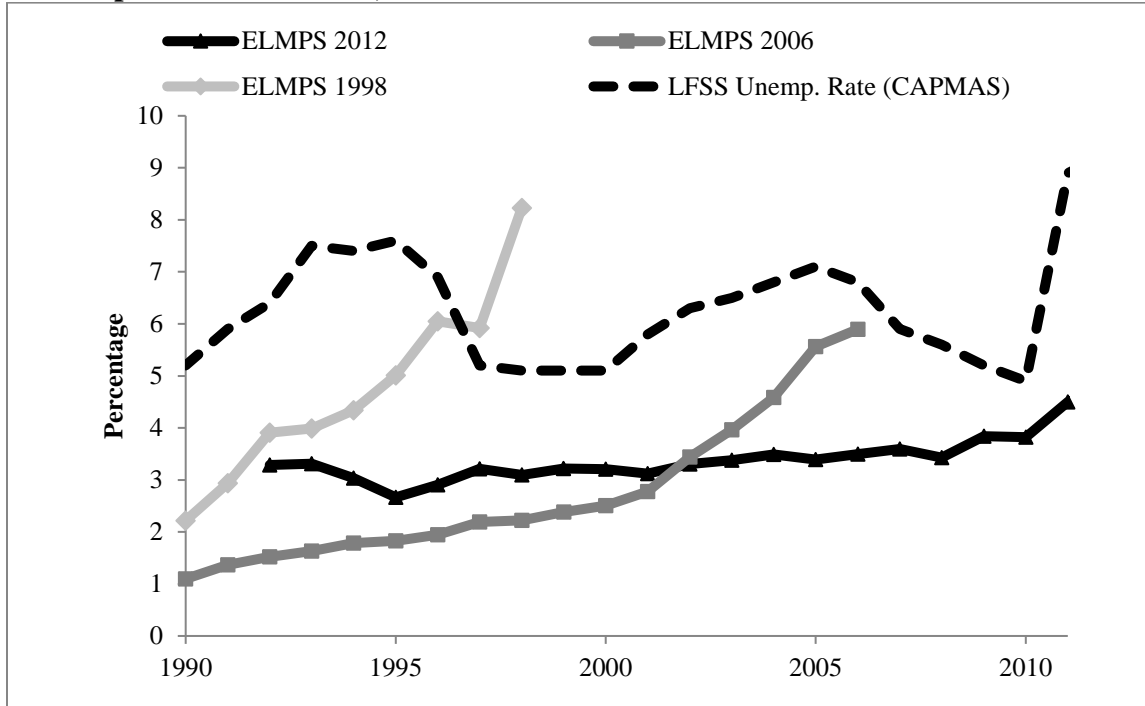
Source: Authors' calculations based on ELMS 1998, ELMPS 2006, ELMPS 2012, and LFSS data[9]

5   Conclusions

The primary objective of this paper is to assess the accuracy of labor market dynamics using retrospective data. We conclude that it is possible to garner useful information on labor market dynamics from retrospective data, but one must be cautious about which information to trust and at what level of detail. One of our most basic conclusions is that information on past employment collected using retrospective data can be fairly reliable, so long as fine distinctions between employment states are not made. For instance, the distinctions between employer and self-employed, between formal and informal wage work, or regular and irregular wage work are not easily made using retrospective data.

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[9] Based on CAPMAS's bulletin of the Labor Force Sample Survey for 1989-2011

In the case of women engaged in self-employment, whether in agriculture or outside agriculture, the distinction between being employed and not employed is hard enough to make in contemporaneous data, let alone in retrospective data. In Egypt, women in this kind of employment typically do not consider themselves to be employed and may move frequently between employment and non-employment states, as defined by international labor statisticians. To assess their current status accurately, researchers must use complex keyword-based questions that inquire about a large number of activities, and even this detailed approach often fails to elicit reliable estimates of female participation in home-based self-employment and unpaid family labor (Anker & Anker, 1995; Assaad & El-Hamidi, 2009; Assaad, 1997; Langsten & Salem, 2008). It is impossible to ask questions at this level of detail about a retrospective period, casting doubt on the employment transitions obtained from retrospective data for women in self-employment. Conversely, transitions across well-defined employment states, such as between public and private wage work, or between public wage work and non-wage work can be captured fairly reliably using retrospective data.

Spells of non-employment interspersed between employment spells are usually hard to recall, whether they are unemployment spells or spells outside the labor force altogether. For instance, 71% of those observed as unemployed in the 1998 never reported any unemployment at any time in the past in the retrospective data obtained from them in the 2012 wave. Thus transitions from non-employment to employment and vice-versa will be understated in retrospective data, with important implications for the accurate reporting of separation rates for the employed and job-finding rates for either the unemployed or those outside the labor force, and the stock of unemployed in past dates. Generally, these rates will be understated, and possibly increasingly so as we go back in time, confounding any measurement of trends. In

contrast, trends describing job-to-job transitions can be captured more reliably using retrospective data.

Another conclusion we derived from analyzing the reporting of recalled marriage costs is that retrospective questions eliciting monetary amounts are unreliable at best. Even when asked to report the nominal amount paid at the time, at least some respondents tend to inflate the amount to their equivalent value at the time of the survey. It thus becomes impossible to ascertain monetary trends over time when some of the data is inflated and some of it is not.

Finally, this experience has allowed us to derive some important lessons on how to improve questionnaire design to collect more accurate retrospective data. First, in comparing the retrospective data from 2012 to the data from previous rounds, we determined it is preferable to ask questions about the individual's labor market trajectory in chronological rather than in reverse chronological order. It elicits better information about labor market entry and in particular about any initial unemployment spells prior to first employment. Second, we suspect that many respondents (and possibly interviewers) interpreted status to mean job, contributing to the underreporting of non-employment spells. In future versions of these labor market panel surveys we will test using separate questions for non-employment spells and for employment spells. The questionnaire should elicit first information about the non-employment spell just after exit from school, if any, and determine whether it was an unemployment spell or an out of the labor force spell. This would be followed by questions about the first employment spell and its characteristics, the next non-employment spell, if any, and so on. Another improvement to the questionnaire will be to ask those who have never worked for a period of more than six months prior to the interview and are currently inactive about whether they have ever sought employment and about the timing and length of the spell in which they were seeking

employment, at least for the first time. Even though these changes will not eliminate recall bias, they could potentially reduce bias that results from questionnaire design.

Given budgetary and data availability constraints, the retrospective panels are currently the primary source of data in the MENA region that allow researchers study labor market dynamics. Having discussed the errors encountered in retrospective data, it is important to note that it is possible to use some remedies to attenuate these measurement errors and eventually produce less biased (or possibly unbiased) results. A possible solution would be to match biased moments obtained from retrospective data with more accurate moments obtained from auxiliary contemporaneous cross-sectional data. Of course, this could be obtained from the same dataset or an external data source, so long as comparability between the data sets is verified. In this case, one assumes that the information obtained from the contemporaneous data is the most accurate. Assumptions about the (functional) form of the "forgetting rate" or information loss in the retrospective data would also be required. Langot and Yassin (2017) correct the ELMPS aggregate labor market transition rates between employment, unemployment and inactivity states, obtained from the retrospective panels, using this methodology. This approach can even be extended to make use of the micro-level information available about the labor market transitions. Using the aggregate measurement errors estimated for the different types of transitions, one could distribute these errors in the form of weights to the individuals in the survey (Yassin, 2016). Again, assumptions need to be made on how to attribute weights to the individuals. Another possible solution, with a different assumption, would be to estimate the alignment rate, possibly the rate of telling the truth, and eventually creating a weight such that individuals who report the truth have higher weights. This requires however the availability of both micro-level contemporaneous and retrospective information for the same individuals. In our case, it could be

applied to the ELMPS but not to other datasets, for instance the Jordan Labor Market Panel Survey (JLMPS) and the Tunisia Labor Market Panel Survey (TLMPS), where for the time being only one wave of the panel is available. Drawbacks of how representative the sample becomes after the creation of such weights need to be also discussed.

To conclude, we believe that panel data with retrospective modules to fill in the gaps between waves of the panel are the best data we can realistically hope for to study labor market dynamics in developing country contexts. Some advanced countries have moved to continuous administrative data to study such phenomena. However, given the low administrative capacity of most developing countries and the high rates of labor market informality, such methods are unlikely to become practical in developing country contexts very soon. In the absence of such panel data, a great deal can be learned from properly designed retrospective questions, so long as researchers are aware of the limitations of these data. As a general rule, distinctions that are hard to make in contemporaneous data, like differences between regular and irregular employment, formality and informality, illiteracy and literacy, and non-employment and home-based self-employment for women, are going to be even harder to make retrospectively. Shorter spells and more frequent events in an individual's labor market trajectory are more likely to be forgotten. We have attempted in this work to highlight some of these problems, but we are in no way suggesting that analyses based on retrospective data are worthless. We are simply advising that proper caution needs to be exercised in interpretation and have provided some pointers as to where the potential pitfalls might lie.

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