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Do Monthly Labor Force Surveys Affect Interviewees' Labor Market Behavior? Evidence from Israel's Transition from Quarterly to Monthly Surveys

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Abstract

This study provides evidence for the impact of monthly interviews conducted for the Israeli Labor Force Surveys (LFSs) on estimated flows between Labor Force (LF) statuses and on coefficients in fixed-effects estimations. The study uses the natural experiment of parallel interviews for *the quarterly* and *the monthly* LFSs in Israel in 2011 for demonstrating that the Labor Force Participation (LFP) rate of Jewish persons who participated in the *monthly* LFS increased between interviews, while in the *quarterly* LFS it decreased. Interestingly, the estimated impact on the LFP rate of self-reporting individuals is 2.6–3.5 percentage points while the impact on the LFP rate of individuals whose data was reported by another member of their household (a proxy), is lower and statistically insignificant. The relative increase of the LFP rate in the monthly survey is a result of a lower rate of exit from the LF and a somewhat higher rate of entry into the LF relative to these flows in the quarterly survey. These differing flows have a bearing on labor search models as the monthly survey portrays a labor market with less friction and a "steady state" LFP rate that is 5.9 percentage points higher than the quarterly survey. The study also demonstrates that monthly interviews affect a specific group (45–64 year-olds); thus the sign of coefficient of age as an explanatory variable in fixed-effects regressions on LFP is negative in the monthly survey and positive in the quarterly survey.

האם תדירות הראיונות לסקר כוח האדם משפיעה על התנהגות המרואיינים בשוק העבודה? עדויות מהמעבר של הלמ״ס מסקר רבעוני לסקר חודשי

חגי אטקס

תמצית

מחקר זה מציג עדויות לכך שתדירות הראיונות לסקר כוח האדם (סכ״א) משפיעה על המעבר בין מצבים בשוק העבודה (משתתף בכוח העבודה, מועסק, ומובטל) ועל אומדים ברגרסיות אפקטים קבועים (fixed effects). המחקר בוחן ניסוי טבעי שהתרחש בישראל בשלהי 2011 ובמסגרתו נערכו במקביל ראיונות לסכ״א הרבעוני ולסכ״א החודשי. עולה ממנו כי שיעור ההשתתפות בכוח העבודה בקרב יהודים עלה בין ראיונות בסקר החודשי, בשעה שבסקר הרבעוני הוא ירד. אומדן ההשפעה של הראיונות החודשיים על המרואיינים עצמם עמד על 2.6—3.5 נקודות אחוז, ואילו אומדן ההשפעה של הראיונות על בני משפחה שהמרואיין דיווח עליהם (פרוקסי) קטן יותר ואינו מובהק מבחינה סטטיסטית. עלייתו היחסית של שיעור ההשתתפות בסקר החודשי נובעת מכך ששיעור היציאה מכוח העבודה (שיעור ההצטרפות לכוח העבודה) נמוך יותר (מעט גבוה יותר) מהזרמים המקבילים בסקר הרבעוני. לפערים בין הזרמים יש השלכה על ניתוח של שוק העבודה שמבוסס על מודל חיפוש, משום שהסקר החודשי מתאר שוק עבודה שיש בו פחות חיכוכים ושיעור ההשתתפות בו ב״מצב עמיד״ (steady state) גבוה ב-5.9 נקודות אחוז מהנתון בסקר הרבעוני. עוד מעלה המחקר שהראיונות החודשיים השפיעו בעיקר על מרואיינים בני 45—64; על כן מקדם הגיל ברגרסיית אפקטים קבועים על שיעור ההשתתפות הוא שלילי בסקר החודשי וחיובי בסקר הרבעוני.

I. Introduction

Longitudinal surveys, also known as panel surveys, are widely used in the social sciences as a prime statistical source. These surveys play a key role in empirical analyses of changes over time, such as flows into and out of the labor force (LF). In addition, panel surveys are frequently analyzed with fixed-effects (FE) to control for unobserved time-invariant personal characteristics. The present study highlights a potential *panel-conditioning* bias in such analyses originated by interview frequency (monthly vs. quarterly).

Specifically, we examine a natural experiment of parallel quarterly and monthly interviews for the Israeli Labor Force Surveys (LFS) in late 2011, during the transition from the former survey to the latter. In the monthly LFS a household is interviewed during four successive months as in the US-CPS, while in the quarterly survey a household was interviewed in the parallel first and fourth months as common in the EU-LFS. This natural experiment allows us to examine the impact of the additional interviews for the monthly LFS, during the second and third months after a household entered the sample, on outcomes reported in the fourth month in both surveys.

We document: (i) The Labor Force Participation (LFP) rate among self-reporting persons in the monthly survey increased between interviews by 2.6–3.5 percentage points relative to the quarterly survey. The impact on other individuals—whose details were provided by another member of the household (a proxy)—is lower and statistically insignificant; (ii) The LFP rate of self-reporting persons increased between interviews in the monthly survey due to the smaller flows out from the LF and the somewhat larger flows into the LF in the monthly LFS relative to the quarterly LFS. The monthly LFS therefore portrays a labor market with less friction in comparison to the market as portrayed by the quarterly survey. Specifically, the computed "steady state" LFP rate in the monthly LFS is approximately 5.9 percentage points higher than in the quarterly survey; (iii) The above findings are pronounced mainly among 45–64 year-old self-reporting persons. As a result, the estimated coefficient of age as an explanatory variable in a FE regression on LFP is negative in the monthly survey but positive in the quarterly survey. The above findings are presumably the result of panel conditioning, that is the effect of being interviewed on subsequent reported results, among mature self-reporting persons.

The rest of the paper is structured as follows: The next section reviews literature on panel conditioning in social sciences. Section III describes the structure of the monthly and quarterly surveys, and focuses on the different sampling frequencies of households in the surveys. Section IV presents the empirical strategy. Section V provides empirical evidence for the impact of sampling frequency on LF outcomes in a cross-sectional sub-sample (Section V-I) and in a panel sub-sample (Section V-II). Section V-II also documents the differing flows between LF statuses in the quarterly and monthly surveys, which have bearing for search models. Section VI documents the heterogeneity of the effect by age, which apparently biases coefficients of age in

FE regressions on the LFP rate and regressions of flows between labor market statuses. Section VII concludes the study.

II. Panel Conditioning in Social Sciences

Concerns regarding the impact of interviews in panel surveys on subsequent reported or actual results—also known in various fields of social sciences as *panel conditioning, time in sample effect,* and *mere measurement effect*[1]—are not new in the social sciences: Seven decades ago, Lazarfeld (1940:128) hypothesized in the context of public opinion polls that repeated interviews are likely to influence respondents' opinions by increasing interviewees' awareness of the issues examined in the surveys. Increased awareness due to surveys may also affect *actual* economic behavior, as Morwitz and Fitzsimons (2004) and Stango and Zimnan (2011) documented regarding consumers (see below).[2] Finally, repeated interviews can also affect *reported* behavior: veteran interviewees may alter their answers in order to reduce the burden of the interview. In addition, familiarity with the survey questionnaire may help interviewees avoid reporting self-stigmatizing information (such as being unemployed). The last two mechanisms were suggested in Halpern-Manners and Warre's (2011) study of *panel conditioning* in the CPS, which is the US counterpart of the Israeli LFS analyzed here.

Some social scientists tested the effects of being surveyed on subsequent actual and reported behavior: Political scientists provided some evidence that being surveyed regarding the intention to participate in elections increased the interviewees' actual voting rate (Clausen, 1968; Kraut and McConahay, 1973; and Traugott and Katosh, 1979). In the context of education, Murry et al. (1988) provide evidence that students whose schools were surveyed for 5 subsequent years regarding smoking habits had lower smoking rates than students in similar—as yet un-surveyed—schools. On the other hand, O'Sullivan et al. (2004) demonstrated that patients given a questionnaire regarding health on a random basis resorted to health-care screening slightly faster than "control" patients. Nevertheless, the authors did not find any effect on absolute service uptake.

Surveys may also affect subsequent estimated economic activity. In this respect, marketing literature acknowledges the *mere measurement effect*, that is, the effect of intention questions (how likely are you to buy X) on subsequent consumer behavior (Morwitz et al. 1993). Morwitz and Fitzsimons (2004) provided evidence that general intent questions ("how likely are you to buy a car", without mentioning a specific brand) influence consumer behavior by means of a

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[1] The impact of surveys that is examined here is related to *self-prophecy* and *question behavior effects* in which the questions about their future behavior induce changes in subsequent behavior. Yet, in this study the content of the questionnaire is similar in the quarterly and in the monthly surveys and it does not derive the results, only the frequency of the interviews.
[2] Empirical evidence on limited attention effect, that is people's tendency to overlook some consequences of their behavior, is surveyed in Della Vigna (2009).

psychological mechanism of changing the accessibility of attitudes towards specific options.[3] Surveys may affect households' financial decisions: A recent economic study provides evidence that households that responded to an Internet-based survey on expenditures and needs in retirement reduced their *actual* non-housing saving rate by 3.5 percentage points (Crossley et al., 2014). Stango and Zinma (2011) provide evidence that individuals who face questions on overdraft fees are less likely to incur such a fee in the following two years. In any event, this literature should make economists aware of the plausibility of the effect of surveys on economic behavior, even though few economists have examined this issue.

In the context of Labor Force Surveys, Kruger et al. (2014) examined *group rotation bias*—the tendency for LF statistics to vary systematically by month in sample—in the American CPS. They documented that since the redesign of the CPS in 1994 unemployment rates of veteran panels are lower than the rates of fresh panels due to exits from the LF and not due to an increase in employment. This bias became stronger in the last two decades as the non-response rate to the CPS declined.[4] Kruger et al. (2014) claim that there is no evidence that the change in the group-rotation bias is driven by the "Heisenberg Principle", the effect of being interviewed in the past on subsequent results. Yet, they do document that self-reported responses had a larger bias than proxy-reported responses both before the redesign of survey in 1994 and afterwards (Table A3). We interpret the different patterns of *group rotation bias* between self-reported and proxy reported responses both in the US-CPS and in the Israeli LFS as evidence for such "Heisenberg Principle" that affects only the interviewees.

The impact of surveys on *actual behavior* is tested by Zwane et al. (2011) in a series of experiments regarding health and micro-credit in developing economies. The most relevant experiment for our study examines the impact of the frequency of interviewing a household regarding the source of domestic water supply on water treatment and child diarrhea in Kenya. In this experiment, the "treated" households were surveyed in 18 bi-weekly rounds, while the "control" group was surveyed 3 times every 6 months. The surveys included questions about the households' water sources and health status, and an analysis of a sample from the households' water tanks. The "treated" households *reported* fewer cases of child diarrhea, and *actually* used chlorine to disinfect drinking water more often than the less frequently surveyed "control" group. The latter result suggests that the act of being frequently surveyed sometimes affects *actual behavior*. Zwane et al. (2011) report on two other health-related studies which indicate that a survey could affect *actual* behavior. On the other hand, two micro-credit studies failed to detect

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[3] More recently Sprott et al. (2006) chose to unify this marketing literature on "mere measurement effect" with similar literature in psychology on the "self-prophecy effect", under a new term, "question-behavior effect".

[4] Halpern-Manners and Warren (2011) compared observations in the first and the second waves, and documented that the LFP rate in the CPS decreases in the second wave due to unemployed persons leaving the labor force and becoming non-participants. They suggested that this pattern is driven by interviewees' preference for avoiding the stigmatized status of unemployment.

such an impact on behavior. The authors concluded that the impact of surveys on subsequent behavior is context dependent.

One of the main results of this study—namely the evidence for a significant panel conditioning effect on estimates of labor market flows—may cast doubt on the validity of estimated flows used in macro-economic analyses of the labor market (e.g. Blanchard and Diamond, 1990; Hall, 2005; Shimer, 2007). Similar doubts were raised by Abowd and Zellner (1985) as well as by Poterba and Summers (1986)[5], who used re-interviews in the American CPS, which were conducted a week after the original interview, for correcting initial "misclassification" in the labor force status.[6] Our results cast doubt on the validity of this method as it finds that re-interviews are likely to be biased by the panel conditioning.[7]

In short, the social sciences are familiar with the concept that interviews may affect interviewees' subsequent *actual* and *reported* behavior. The literature suggests several plausible reasons for such an effect in the economic context: making the interviewees aware of certain options, incentives to minimize the duration of the interview, and a preference for giving non-stigmatizing answers. Some studies provide suggestive evidence for such an effect, while few studies provide solid evidence. The firmest evidence is provided by Zwane et al. (2011) in the context of health in developing societies. On the other hand, evidence for *panel conditioning* in regular surveys such as the LFS/CPS in developed societies is weak. The present study opts to fill this gap by demonstrating that the frequency of interviews—a specific type of panel conditioning—affects subsequent reported outcomes by using the rare period, in which the Israeli CBS conducted both the quarterly and the monthly LFS.

III. Data: Structure of the Israeli Quarterly and Monthly Labor Force Surveys

The Israeli Labor Force Surveys have been conducted regularly by the Israeli Central Bureau of Statistics (CBS) since 1954. The surveys estimate the LF characteristics of the civilian[8] labor force aged 15 and above, including: employment, unemployment and being outside the labor force. The quarterly LFS was published until 2011 (inclusive) while the official publication of the monthly survey begun in 2012.

This study uses the rare episode: the parallel operation of the quarterly and the monthly surveys in late 2011 along the transition from the former to the latter. The quarterly and the

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[5] Bound et al. (2001) survey the literature on measurement errors in household surveys including LFS.
[6] Presumably, the misclassification errors inflated the transitions in and out of unemployment and portrayed a dynamic US labor market.
[7] Harley et al. (2005) also criticized the use of re-interviews as a bench-mark for "true" data because the re-interview sample may not be representative of the CPS as a whole due to a lower response rate.
[8] Excluding military personnel, who are defined as being outside of the labor force. The official monthly labor force survey starting in 2012 includes military personnel as employed persons.

monthly surveys share many basic features: They were conducted by the same governmental agency, the CBS, using the same legal powers[9], and the interviews were based on the same questionnaire. Both surveys are household surveys, with one person providing the information on himself and acting as a proxy who provides information on other members of the household. The empirical analysis here uses the distinction between self-reported data and proxy-reported data in order to differentiate between the aforementioned (p. 4) mechanisms that may generate panel conditioning.

Differences do, however, exist between these two surveys: The main difference, on which this study is based, is the differing frequency at which households were interviewed in the quarterly and monthly surveys. In the 2-2-2 rotational structure of the quarterly survey, a household was interviewed in two successive quarters, was left for two successive quarters, and then interviewed again for two successive quarters before being dropped from the sample. Typically, a household was interviewed in the same ordinal month in the quarter (for example, July and October 2009 and 2010–the first month in the third and the fourth quarters). In the 4-8-4 rotational structure of the monthly survey, a household was interviewed during four successive months, was left for eight months, and interviewed again for 4 months before being dropped from the sample. In fact, the Israeli quarterly LFS is similar to the rotational structure in some countries in Europe while monthly LFS has the same rotational structure as the American CPS.

Figure 1: Rotational Structure of Quarterly and Monthly LFSs and Relevant Period of Employment and Unemployment

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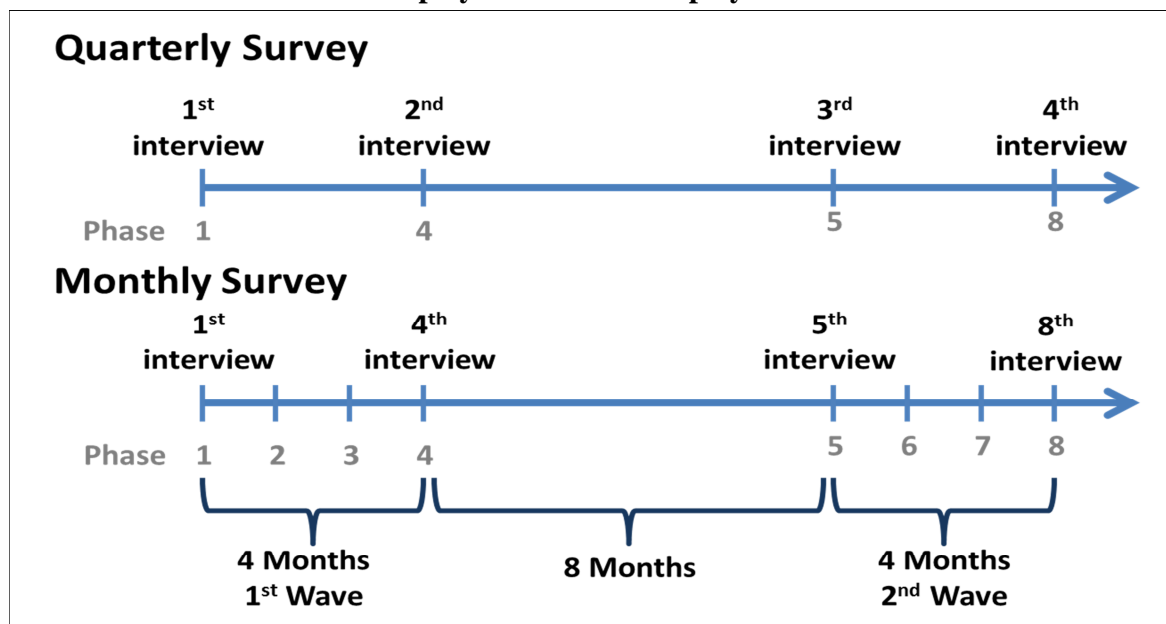
The CBS is the official statistical agency in Israel, whose mission and powers are defined in the Statistics Order, 1972. One of the legal powers of the CBS is to demand that interviewees give truthful answers to the interviewers.

We map the quarterly rotation into the monthly rotation, referring in both surveys to the timing of the eight planned interviews in the monthly survey as "phases" (see Figure 1). The mapping shows that in both surveys, households were interviewed in the first and fourth phases/months after entering the survey; but only in the monthly survey were households sampled in the second and third phases as well. Households in both surveys then had a break of eight months. Finally, in both surveys households were interviewed in the fifth and eighth phases, but only those in the monthly survey were interviewed in the sixth and the seventh phases.

These rotational structures enable us to test the impact of frequent interviewing on LF characteristics. Specifically, we compare households interviewed in four successive months in the monthly LFS with households sampled only in the parallel first and fourth months in the quarterly LFS. For example, we can test the impact of the additional interviews in phases 2 and 3 on LF outcomes in phase 4. The main analysis here focuses on the first wave of interviews (phases 1–4) and only reports basic statistics for the second wave (phases 5–8).[10] Notably, the status of employment (unemployment) is defined for the last week (4 weeks) before the interview, and it is therefore possible that monthly interviews could affect these outcomes.

Table 1 demonstrates the rotational process during the first wave (phases 1–4). Row A depicts the interviews of the panel whose first interview was in July 2011: households in the quarterly survey (left side in Table 1) were interviewed in July and in October 2011 for the second time, while households in the monthly survey (right side in Table 1) were interviewed in four successive months, in July, August, September and October 2011. Each month a fresh panel entered the sample and veteran panels were sampled again, left out, or dropped from the sample. Every month the sample included fresh panels and veteran panels. For example, in October 2011 households in the quarterly panels of July and October were interviewed for the first and second time (column iv in Table 1), while households in the monthly panels of July-August-September-October 2011 were interviewed for the first/fourth (respective) time (column xiii in Table 1).

These rotational structures enable us to carry out two sets of exercises:
- A cross-sectional comparison of observations from different panels at 2011:Q4 (columns iv–vi and x–xii). For example, we can compare the difference between the LF outcomes of panels of July–September 2011—which were interviewed for the second time in the quarterly survey and for the fourth time in the monthly survey—with the panels of October–December 2011—which were interviewed for the first time in the quarter in both surveys.
- A panel analysis which examines the change in LF outcomes of the *same individuals* over time (rows A–C). An example is estimation of the change in employment rate of the panel of

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[10] Comparison of the second wave of interviews (phases 5–8) between the quarterly and the monthly surveys does not identify the impact of frequent sampling because the household participating in the monthly survey in the transition period in late 2011 were not interviewed in 2010, while those participating in the quarterly survey were interviewed back in 2010.

September 2011, between September and December 2011 in the two surveys. In December, households in the monthly survey were interviewed for the fourth time, while households in the quarterly survey were interviewed for the second time. By its very nature, the panel analysis—unlike the cross-sectional analysis—follows the same individuals and enables us to examine the impact of frequent sampling on the flows between LF statuses (for example, the flow from ot into LF).

Table 1: Investigation of Panels in the First Wave of Interviews, July–December 2011

		Quarterly LFS							Monthly LFS				
		Month of survey											
	Panel – "First Interview"*	Jul	Aug	Sep	Oct	Nov	Dec		Jul	Aug	Sep	Oct	Nov	Dec
		i	ii	iii	iv	v	vi		x	xi	xii	xiii	xiv	xv
A	7-2011	1ᴴ			2				1ᴴ	2	3	4		
B	8-2011		1ᴴ			2				1ᴴ	2	3	4	
C	9-2011			1ᴴ			2				1ᴴ	2	3	4
D	10-2011				1ᴴ							1ᴴ	2	3
E	11-2011					1ᴴ							1ᴴ	2
F	12-2011						1ᴴ							1ᴴ

Notes: The numbers in the table cells designate the ordinal number of the interview held in that month. For example, the monthly panel of September 2011 was interviewed for the fourth time in December 2011.
* The panels in the monthly LFS, whose "date of first investigation" preceded July 2011.
H Home visit; undesignated interviews were conducted over the phone.

There are additional differences which may affect our analysis of the impact of interview frequency on LF outcomes. Firstly, the quarterly LFS covered a representative sample of the Israeli adult (15+) population every quarter (columns iv–vi in Table 1), while the monthly survey covered the Israeli population every month (column ix). This difference is expressed in the size of the samples: the sample of the quarterly survey includes 22,500 people *per quarter* and the sample of the monthly survey is 21,500 people *per month*.

The surveys also differed in the investigation period during which those conducting the CBS surveys sought out a household in order to interview its members. The investigation period of a household in the quarterly survey was the determinant week (the week in which the household was originally scheduled to be interviewed) and an additional three weeks. The investigation period in the monthly survey was the determinant week and an additional week. As a result, the non-response rate and the attrition rate in the monthly survey are higher in the monthly data,[11] which may result in a selection bias. We treat this potential bias by dropping the

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[11] Data on response rate in the sub-sample of the quarterly and monthly surveys used in this study are unavailable. Yet, we can compare the response rate in the quarterly survey in 2011 (87.1 percent) was higher than the response rate in the monthly survey in 2012 (82.1 percent) as expected because of the loner interview period in the quarterly survey (CBS, 2013, p. Table 12.1; CBS, 2014, p. 7).

observations from the third and fourth weeks after the determinant week in the quarterly survey, and thus making the investigation period in our sample equal.[12]

Two additional differences in the design and implementation of the surveys are not likely to have a differential impact on the LF outcomes by phases; hence it is plausible that these differences have no effect on our main results. The first, presumably, phase-neutral difference is the sampling frame: The quarterly survey was based on municipal tax files, while the monthly survey is based on the Building and Dwelling Registry after anchoring dwellings to statistical areas. The latter database includes municipal tax files as well as additional data and the sampling units in the monthly survey are therefore likely to be more homogeneous. The sampling frame is used only in the first phase and accordingly, the differences between the sampling frames in the two surveys are likely to be phase-neutral.

The second presumed phase-neutral difference is operational: The CBS recruited a fresh team of survey takers for the monthly survey in 2011, and in January 2012 it laid-off or assigned to different positions the veteran surveyors employed in the quarterly survey. This may have generated a learning-by-doing effect in the monthly survey, and lax efforts to elicit and record data by the veteran staff of the quarterly survey.

Finally, the LF outcomes of Arabs[13] in the monthly and quarterly LFS are very different. For example, the unemployment rate of Arabs in the monthly survey is double the corresponding rate in the quarterly survey. Cohen et. al (forthcoming), used LFS administrative data in order to examine the reasons for these differences. They ascribe the different outcomes to differences in the interview methods employed in the Arab sector in the two surveys. We exclude the Arab population from this study in order to avoid such interview-method differences.

IV. Empirical strategy and Balancing Tests

The following empirical analyses compare labor force outcomes of observations from the quarterly and the monthly LFS, which were sampled at phases 1 and 4, as defined above. We present two sets of exercises: The first compares different individuals at different phases (repeated cross-section), while the second follows the same individuals over phases (panel). Both the cross-sectional and the panel analyses enable us to examine the impact of the monthly interviews in phases 2 and 3 on the levels of LFP, employment and unemployment rates, but only the panel analysis examines flows of transitions between LF statuses.

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[12] The observations from the third and fourth weeks are about 12 percent of the observations in the monthly LFS.

[13] The Israeli LFS samples Arab citizens and Arab residents who live in the State of Israel, but not in the West Bank or Gaza.

The estimation of the impact of the monthly interviews on the levels of the variables that are of interest, such as LFP rate, is based on a Difference in Difference (DID) estimation. It compares the changes in the level of the variables that are of interest in the quarterly and monthly surveys. Some specifications include controls for personal characteristics, and in some panel estimations they also include fixed-effects for individuals. Formally we estimate the following DID equation:

(*) $\quad y_{i,t} = \beta_1 \cdot Monthly_{i,t} + \beta_2 phase4_{i,t} + \beta_3 \cdot Monthly_{i,t} \cdot phase4_{i,t} + \gamma X_{i,t} + \epsilon_{i,t}$

Where $y_{i,t}$ is the variable that is of interest, $Monthly_i$ is a dummy variable designating the monthly survey; $Phase4_i$ is a dummy variable for data collected in phase 4; X_i is a vector of personal characteristics such as education, age, and home district; and ε_i is an error term. The coefficient of interest is β_3, which estimates the change in outcomes in the monthly survey relative to the change in outcomes in the quarterly survey. A positive and significant β_3 suggest that monthly interviews had a positive impact on the level of $y_{i,t.}$

In addition, in the panel analysis we estimate regressions of flows such as exit from LF and entry into LF between phase 1 in 2011:Q3 and phase 4 in 2011:Q4.

(**) $\quad Flow_i = \beta_1 Monthly_i + \gamma X_i + \epsilon_i$

Where $Flow_i$ is, for example, the share of entrants into the LF in phase 4 out of those who were outside the LF in phase 1, or the share of dropouts from the LF of those who were active in phase 1. The other variables are as defined above. The variable of interest is the coefficient of the Monthly dummy variable (β_1), which denotes the difference between the flows in the quarterly survey and in the monthly survey.

The cross-sectional and panel analyses complement each other: The cross-sectional data resemble the data used for the estimation of official LF statistics, but they may suffer from attrition. In contrast, by its very nature the panel analysis monitors individuals over time and can therefore provide clues about the impact of frequent sampling on flows between labor force statuses and on attrition, which are not observed in the cross-sectional analysis.

Balancing Tests

There are few reasons to test whether the quarterly and monthly surveys are balanced: First, the process of sampling the data for quarterly and monthly surveys was based on a similar but somewhat different sampling frame. In addition, it is unclear whether the sub-samples in phases 1 and 4—which we compare separately in the cross-sectional analysis—are balanced.

The balancing tests between the quarterly and monthly surveys in the *panel* sub-samples are presented in the left section of Table 2. We compare the personal characteristics of these individuals as recorded in the first interview.[14] Typically, the monthly and quarterly sub-samples in the panel dataset are balanced. The main exceptions are the number of years of schooling, which is higher by approximately 0.4 years in the monthly data, and the higher rate of currently studying individuals.

The balancing tests in the cross-sectional sub-sample (right section in Table 2) include separate balancing tests for phase 1 and phase 4, and a difference in differences (DID) estimator of the personal characteristics. The separate balancing tests may expose trends that are observable over the phases, and the DID estimates test whether the hypothetical differing trends add up to significant differences in these observables. Such differing trends could explain patterns which might otherwise be attributed to the frequent interviewing in the monthly survey. As previously mentioned, most variables are balanced, but "schooling" and "currently studying" are not balanced. Moreover, the difference between the quarterly and monthly samples is smaller in phase 4, which suggests that the relative schooling of individuals in the quarterly survey increased. This relative increase in schooling is expected to increase the LFP and employment rates in the quarterly sample, and to bias the results against the main result of this study: monthly interviews increased the LFP and employment rates. In any case, the DID estimates suggest that different trends in schooling between the monthly and quarterly surveys are statistically insignificant.

Another imbalance in the cross-section data is the statistically significant larger share of married individuals in the monthly survey in phase 1 than in the quarterly survey. However, the magnitude of the difference is small (2 percent) and this imbalance is not likely to have a major impact on our uncontrolled results. In the empirical analysis below, we chose to correct for the above-mentioned imbalances by controlling for this variable.

We also note that the share of self-reporting individuals is balanced both in the panel and in the cross-sectional sub-samples. This balance is important for the empirical analysis here, which differentiates between the impact of interview frequency on self-reported and proxy-reported data.

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[14] Comparison of the data supplied at the first interview avoids a potential bias resulting from verifications with past information used in the monthly survey, but not in the quarterly survey. Such verifications affected data collected in the second-eights interviews for the monthly survey.

Table 2: Tests for Balancing between Quarterly and Monthly Surveys in the Panel and the Cross-section Sub-samples

| | Balanced Panel Sub-sample | | | Cross Section Sub-sample | | | | | | DID |
| | | | | Phase 1 | | | Phase 4 | | | Phase 4-1 |
	Quarterly	Monthly	Difference	Quarterly	Monthly	Difference	Quarterly	Monthly	Difference	
Female	0.532	0.531	0.001	0.520	0.524	-0.004	0.530	0.529	0.001	-0.003
	(0.009)	(0.008)	(0.012)	(0.008)	(0.007)	(0.010)	(0.008)	(0.006)	(0.010)	(0.007)
Age	5.651	5.628	0.023	5.600	5.507	0.090	5.557	5.519	0.037	0.053
(Grouped) [1]	(0.052)	(0.044)	(0.068)	(0.048)	(0.039)	(0.062)	(0.045)	(0.037)	(0.058)	(0.106)
Immigrants	0.392	0.371	0.019	0.381	0.371	0.010	0.388	0.368	0.020	-0.010
(Olim) [2]	(0.009)	(0.007)	(0.011)*	(0.083)	(0.064)	(0.010)	(0.077)	(0.062)	(0.010)**	(0.021)
Married	0.604	0.622	-0.017	0.601	0.576	0.025	0.595	0.592	0.002	0.023
	(0.009)	(0.007)	(0.011)	(0.084)	(0.065)	(0.011)**	(0.077)	(0.064)	(0.010)	(0.017)
Schooling [3]	13.181	13.621	-0.360	13.202	13.707	-0.505	13.242	13.633	-0.391	-0.127
	(0.066)	(0.053)	(0.084)***	(0.068)	(0.046)	(0.077)***	(0.058)	(0.045)	(0.071)***	(0.131)
Currently	0.124	0.136	-0.013	0.128	0.170	-0.042	0.141	0.161	-0.021	-0.021
Studying	(0.006)	(0.005)	(0.008)*	(0.006)	(0.005)	(0.008)***	(0.005)	(0.005)	(0.007)***	(0.014)
Residents of Tel- Aviv &	0.500	0.491	0.009	0.473	0.487	-0.014	0.501	0.498	0.003	-0.018
Central Districts	(0.009)	(0.008)	(0.012)	(0.085)	(0.066)	(0.011)	(0.008)	(0.065)	(0.010)	(0.028)
Self-Reported	0.434	0.430	0.005	0.430	0.417	0.012	0.429	0.417	0.012	0.000
Information	(0.008)	(0.007)	(0.012)	(0.008)	(0.01)	(0.011)	(0.007)	(0.006)	(0.010)	(0.011)
Obs.	**3,058**	**4,190**		**3,423**	**5,667**		**4,389**	**5,907**		

Notes:

1. The micro data available do not list the age in specific years. The data in the table are the average and SD of the following age groups are: 1: 15–17; 2: 18–24; 3: 25–29; 4: 30–34; 5: 35–44; 6: 45–54; 7: 55–59; 8: 60–64; 9: 65–69; 10: 70–74; 11: 75+.
2. Immigrants who arrived after 1990.
3. The panel dataset includes individuals who entered the sample in 2011:Q3 and were interviewed in the fourth phase as well. The data in the balancing tests for the panel data refer to the first phase. Excluding mis-reported observations with more than 50 years of schooling. Schooling as reported in September for the purpose of measuring schooling in repeated interviews differed in the quarterly and monthly surveys.

V. Empirical Analysis

V-A. Cross-sectional analysis

The cross-sectional analysis examines a dataset of Jewish households sampled in either the quarterly or the monthly LFS in 2011:Q4. The average LFP rate of males in the monthly survey is significantly higher than in the quarterly survey (63.0 percent and 61.1 percent, respectively; p-value ~0.001). There is also a similar but smaller and statistically insignificant difference in the female LFP rate between the two surveys (59.4 percent and 58.6 percent, respectively). Similarly, among males the employment rate in the monthly survey is higher than in the quarterly survey (58.9 percent and 57.7 percent, respectively), but this difference among females is much smaller (55.5 percent and 55.2 percent, respectively) and statistically insignificant. The following analysis pools together the data on both males and females.

Figure 2: Labor Force Activity by Survey Frequency and Gender
(October–December 2011)
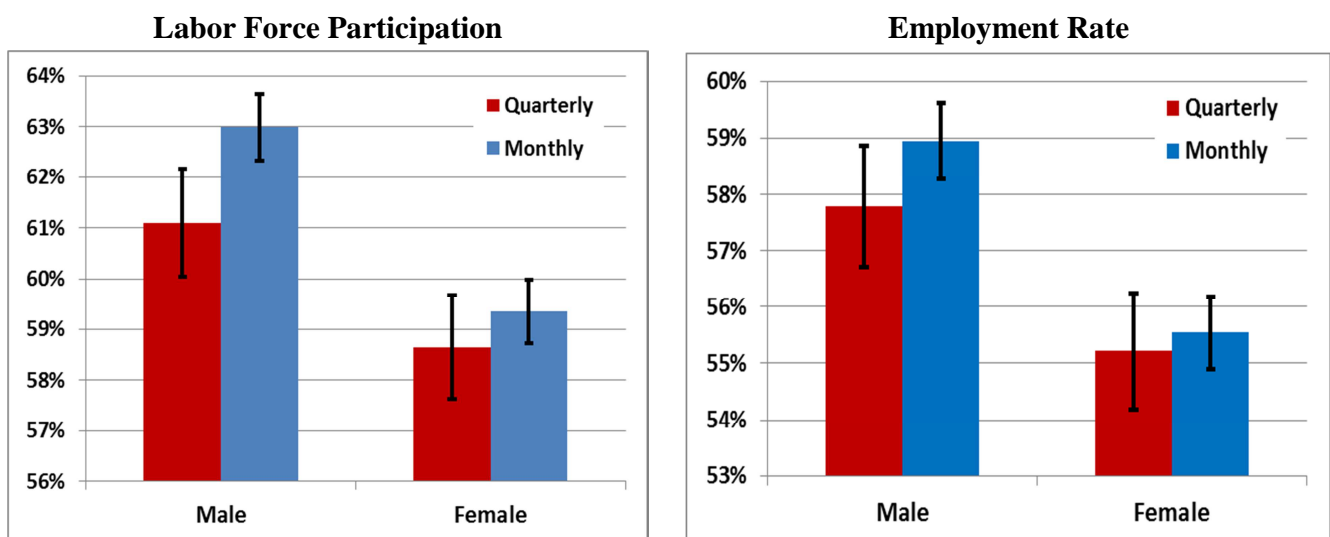
Source: Israeli CBS, LFS.

Figure 3 documents the pattern underlying the higher LFP rate in the monthly survey: At phase 1, the LFP rate in the monthly survey is actually one percentage point less than the rate in the quarterly survey. The LFP rate in the quarterly survey then has a negative trend over the phases, while it displays a positive trend in the monthly survey, at least until phase 7. In short, the LFP rate in the monthly survey increased over the phases *relative* to the same rate in the quarterly survey.

Figure 3: Labor Force Participation by Survey Frequency and Phase
(October–December 2011)
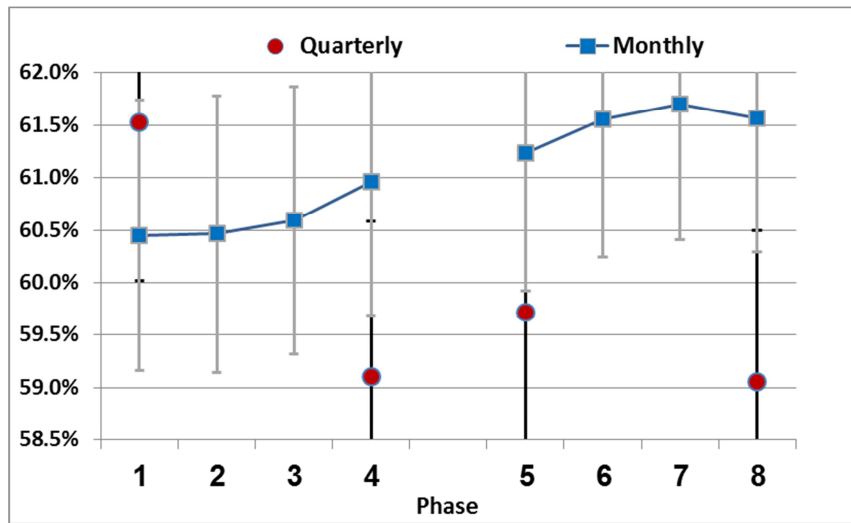
The upward trend of the LFP in the monthly survey and its downward trend in the quarterly survey result mainly from parallel trends in the employment rate (employees/population, see Figure 4), rather than from the unemployed over population ratio. While the employment rate in the quarterly survey decreases in most of the phases, the monthly employment rate increases throughout the first 7 phases, and declines between phase 7 and phase 8. The overall decline between phases 5 and 8 in the monthly survey is more moderate than the parallel decline in the quarterly survey. Even in the second wave of interviews (phases 5–8) therefore, the employment rate in the monthly LFS increased relative to the quarterly LFS.

Figure 4: Employment Rate by Survey Frequency and Phase
(October–December 2011)

15
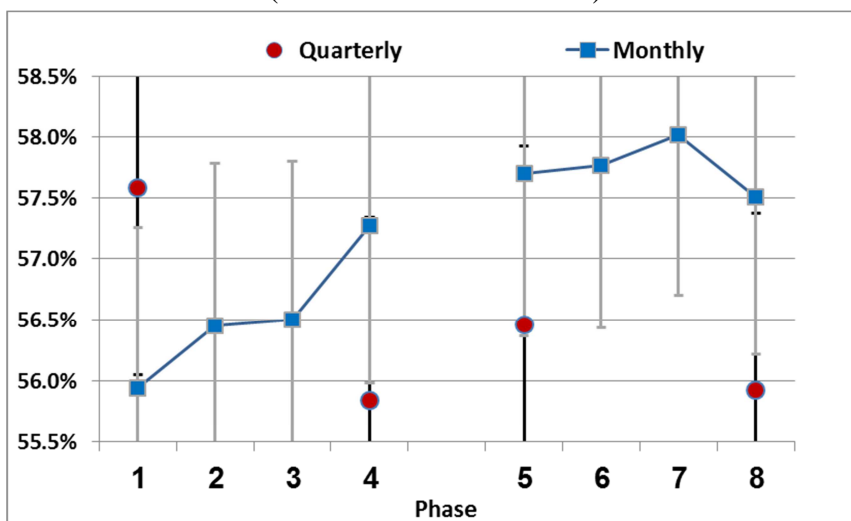
The crude relative increase of LFP and the employment rates may be the result of changing personal characteristics between the phases compared. In order to account for observable characteristics, we estimated the impact of the additional interviews in the monthly survey with a DID regression (specification * in p. 11) using data from the first wave of interviews (phases 1–4).[15]

Table 3 presents the DID regression on the LFP rate (left side) and employment rate (right side) without (columns i and iv) and with controls (columns ii, iii, v and vi). The crude DID estimates are barely statistically significant. Adding the controls for personal characteristics and adding district FE makes the estimated impact more significant, while hardly affecting its magnitude. Notably, the estimated effect on the employment rate is somewhat larger than on the LFP rate.

Table 3: Labor Force Participation and Employment Rates
(Phases 1 and 4; October–December 2011)

	Labor Force Participation			Employment rate		
	i	ii	iii	iv	v	vi
Monthly LFS*100	-0.91	-2.52	-0.32	-1.41	-2.79	-1.03
	(1.13)	(1.11)*	(0.92)	(1.12)	(1.05)**	(0.96)
Phase 4*100	-2.40	-2.34	-2.30	-1.49	-1.47	-1.46
	(1.04)**	(1.08)**	(0.97)**	(1.16)	(1.12)	(1.10)
Monthly LFS * Phase 4 *100	2.84	3.06	2.45	2.77	3.05	2.53
	(1.70)*	(1.71)*	(1.14)**	(1.63)	(1.61)*	(1.32)*
Personal Characteristics	N	Y	Y	N	N	Y
Month of Survey FE	N	N	Y	N	Y	Y
Sub-district FE	N	N	Y	N	N	Y
R^2	0.0003	0.084	0.413	0.0002	0.081	0.387
N	19,044	18,760	18,760	19,044	18,760	18,760

Note: Robust SE clustered by sub-district.

We differentiate between data on a self-reported individual and data reported by a proxy (another member of the household) in order to gain a deeper understanding of the potential mechanisms underlying the above-mentioned patterns. A larger impact on self-reporting individuals compared with proxy-reported individuals conforms to an increase of actual LFP due to increased awareness or a higher reported LFP rate resulting from a preference for not giving self-stigmatizing information (by not admitting to unemployment). On the other hand, a similar impact of monthly interviews on self-reporting and proxy-reported individuals conforms to a preference for reducing the length of the interview. As Table 4 suggests, the impact of the

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[15] The regressions analysis excludes data from the second wave (phases 5–8), for the reasons explained above (Footnote 10).

additional interviews on the LFP rate among self-reporting individuals (columns i–iii) is almost double the impact among proxy-reported individuals (columns iv–vi). Similar results are reported for the employment rate in the appendix (Table 4A in the appendix). This result suggests that increased awareness and/or refraining from giving self-stigmatizing information are the potential mechanisms underlying the estimated effect of the frequent interviews.

The cross-sectional analysis does not inform us of the dynamics, that is, the flows between LF statuses which underlie the main result of this paper: the relative increase of the LFP and the employment rates with the frequency of interviews. Neither can it be taken as ruling out the possibility that this pattern is the result of increased attrition caused either by the frequent interviews themselves or by the shorter period of investigation in the monthly survey (see p. 9). These issues are addressed in the following panel analysis.

Table 4: Cross-sectional DID Estimates of Labor Force Participation by Reporter
(Phases 1 and 4; October–December 2011)

	Self-Reporting Individuals			Proxy Reported Individuals		
	i	ii	iii	iv	v	vi
Monthly LFS*100	-0.65	-2.58	-0.59	-0.94	-1.69	-0.47
	(1.47)	(1.36)*	(1.47)	(1.35)	(1.42)	(1.04)
Phase 4*100	-2.27	-2.00	-0.44	-2.49	-2.56	-3.76
	(1.38)	(1.31)	(1.60)	(1.35)*	(1.37)*	(1.04)***
Monthly LFS * Phase 4 *100	4.10	4.11	3.00	1.95	2.23	1.89
	(1.77)**	(1.87)**	(1.60)*	(2.28)	(2.37)	(1.96)
Personal Characteristics	N	Y	Y	N	Y	Y
Month of Survey FE	N	N	Y	N	N	Y
Sub-district FE	N	N	Y	N	N	Y
R^2	0.001	0.117	0.431	0.0003	0.084	0.412
N	8,035	7,949	8,431	11,009	10,811	10,811

Note: Robust SE clustered by sub-district.

V-B. Panel analysis

The panel analysis follows individuals who were sampled in 2011:Q3 for the first time (phase 1), and who were interviewed again in Phase 4 during 2011:Q4. The interviews in Phase 4 were the second interviews in the quarterly survey and typically, the fourth interviews in the monthly survey. We use a balanced panel which by construction does not suffer from attrition. As noted above, the data on individuals whose first interview was in 2011:Q3 are not designed to be similar to those who were sampled in 2011:Q4. For these two reasons, we do not expect the estimates of the panel sample to be equal to the aforementioned estimates in the cross-sectional analysis.

Table 5 presents indications for the LF activity of individuals who were interviewed personally and who reported their own employment details in both phase 1 *and* phase 4 (columns i–iv), and for individuals whose information was reported by a proxy in phases 1 *and/or* phase 4 (columns v–viii). The table presents the crude averages and SE of the LFP and the employment rates, the phase 4–phase 1 differences for the quarterly and the monthly surveys, as well as the Difference-In-Differences (DID) estimate.

Interestingly, among self-reporting individuals, the LFP rate in the quarterly survey declined by 1.8 percentage points while in the monthly survey it increased by 0.8 percentage points. The resulting simple DID estimate for the impact of the additional interviews in phases 2 and 3 on the LFP rate in phase 4 of self-reported individuals is 2.6 percentage points (Table 5, columns i-ii). This crude *un-clustered DID* estimate is statistically insignificant plausibly because of serial correlation. As with the cross-sectional analysis, most of the effect derives from changes in the reported employment rate (columns iii–iv), and the apparent impact on unemployment is minor and statistically insignificant (not reported). By contrast, among individuals whose details were reported by a proxy (another household member), the trends of the LFP and the employment rates in the quarterly and monthly surveys were almost identical, and the resulting DID is low and even negative (columns v–viii).

Table 5: Labor Force Participation and Employment Rates by Survey Frequency and Phase
(Percent, Persons who entered the sample in 2011:Q3)

	Self-Reported in Phases 1 and 4				Reported by A Proxy in Phase 1 or Phase 4			
	Labor Force Participation		Employment Rate		Labor Force Participation		Employment Rate	
	Q	M	Q	M	Q	M	Q	M
	i	ii	iii	iv	v	vi	vii	viii
Phase 1	63.1	64.9	59.6	61.1	57.2	59.3	53.9	55.4
	(1.5)	(1.5)	(1.6)	(1.3)	(1.1)	(0.9)	(1.1)	(0.9)
Phase 2		65.1		60.9		59.7		55.1
		(1.4)		(1.4)		(1.0)		(1.0)
Phase 3		66.3		61.7		59.7		55.7
		(1.4)		(1.4)		(1.0)		(1.0)
Phase 4	61.6	66.0	58.9	61.6	57.1	59.0	54.7	56.6
	(1.5)	(1.3)	(1.4)	(1.3)	(1.1)	(0.9)	(1.1)	(0.9)
Difference Phase 1–4	-1.4	1.1	-0.7	0.4	-0.1	-0.3	0.8	0.2
	(2.2)	(1.8)	(2.2)	(1.9)	(1.6)	(1.3)	(1.4)	(1.3)
DID Q-M†	2.5		1.3		-0.2		-0.6	
	(1.1)**		(1.0)		(0.8)		(0.9)	
N	**979**	**1,335**	**979**	**1,335**	**1,959**	**2,854**	**1,959**	**2,854**

Notes:
Q – Quarterly Survey; M – Monthly survey.
† SE clustered by individuals.

We tested the impact of the monthly interviews on the LFP rate with DID estimates, which account for serial correlation by clustering the SE for individuals. In these estimates, the DID estimator for self-reporting individuals becomes statistically significant (Table 6, columns i–iii). Controlling for observable characteristics or for personal FE (column iii) does not alter the estimate, suggesting that the correlation between the imbalances in the panel sub-sample and the impact of the monthly interviews on the LFP rate is negligible. As previously, there was no evidence for monthly interviews impacting the recorded behavior of individuals whose data were provided by a proxy (columns iv–vi).

Table 6: Difference in Differences Estimates of the Effect of Monthly Interviews on LFP by the Identity of the Information Provider

(Persons who entered the sample in 2011:Q3, balanced panel)

	Self-Reporting Individuals in Phases 1 and 4			Individuals Reported by Proxy in Phase 1 or Phase 4		
	i	ii	iii	iv	v	vi
Monthly LFS*100	1.83 (2.00)	-0.11 (1.51)		1.79 (1.42)	2.06 (1.17)*	
Q4*100	-1.44 (0.85)*	-1.20 (0.86)	-1.43 (0.82)	0.04 (0.69)	-0.41 (0.69)	0.10 (0.62)
Monthly LFS* Q4*100	2.47 (1.09)**	2.33 (1.11)**	2.48 (1.06)**	-0.14 (0.90)	-0.22 (1.05)	-0.21 (0.89)
Controlling For Personal Characteristics	N	Y	N	N	Y	N
Controlling For Personal FE	N	N	Y	N	N	Y
R2	0.001	0.428	0.001	0.0004	0.350	0.0000
N	4,662	4,623	4,662	9,713	9,564	9,713

Notes:
1. SE are clustered by individuals.
2. Personal Characteristics include age, schooling, gender, district of residence.
3. R^2 in the FE specifications (columns iii & vi) are overall R^2.

Table 7 decomposes the impact of monthly interviews on the level of LFP among self-reporting individuals into flows from and to the LF by regressions (specification **, p. 11). The sub-sample in the regressions of LF exit includes self-reporting individuals who were in LF in phase 1. The dependent variable is 1 for those who left the LF in phase 4 and 0 for those who stayed in the LF. The sub-sample in the regressions of LF entry includes self-reporting individuals who were outside the LF in phase 1. The dependent variable is 1 for those who entered the LF, and 0 for those who stayed outside the LF. Columns i and iii present the crude regressions, and columns ii and iv present regressions with controls for such observations as age, schooling, gender and district of residence.

The data in Table 7 show that monthly interviews reduce reported exit from LF and have an insignificant impact on entry to the LF. The magnitude is large considering the size of the

flows in the quarterly surveys: the exit rate in the monthly surveys (6.31 percent, the sum of the coefficients in column i) is approximately a quarter less than the exit rate in the quarterly survey (8.58 percent), and the entry flow in the monthly survey (6.92 percent) is double that in the quarterly survey (3.38). It is possible that monthly interviews induced self-reporting individuals who were active in the LF in the past, to report that they are still active. Indeed, it is plausible that people white-lying by providing information which was true until recently. We cannot rule out the possibility that monthly interviews provide interviewees with an incentive to stay in their present job or to seek a job.

Table 7: Regressions of Flows Out of and Into the Labor Force on LFS Frequency,
Self-Reporting Individuals (Persons who entered the sample in 2011:Q3)

Dep. Variable:	Exit Labor Force		Enter Labor Force	
	i	ii	iii	iv
Monthly LFS*100	-2.27 (1.18)*	-2.25 (1.15)***	3.54 (1.94)*	1.92 (1.92)
Constant	8.58 (2.07)***	N.A	3.38 (3.02)	N.A
Personal Characteristics	N	Y	N	Y
R^2	0.003	0.025	0.004	0.117
N	1,485	1,481	829	815

Notes:
1. SE are clustered by individuals.
2. Personal Characteristics include age, schooling, gender, district of residence.

As documented above (Table 7), the moderate impact of monthly interviews on the LFP rate results from a somewhat larger inflow of people entering the LF and of a smaller outflow from the LF following the month-after-month interviews. These LF flows play a fundamental role in search models, including in the estimation of employment and unemployment in steady state equilibrium (a large literature developed following Blanchard and Diamond, 1990). It is possible to approximate a naïve "steady state" LFP rate under the assumption that the flows are constant [LFPss=inflow/(inflow+ outflow)]. The results of this study–namely that the frequency of interviews affect the flows between labor force statuses–therefore reveal a potential bias in such macro-labor analyses.

Table 8 documents the LF inflow (outflow) as a proportion of the non-participant (participant) population in the sample of both self-reported and proxy-reported responses, as is usual in the search literature. The base LFP rates of the quarterly and monthly surveys– which define the denominators of the proportions–were roughly similar. The sample in Table 8 includes all Jewish individuals in the balanced panel, and not only the self-reporting individuals as in Table 7, with the result that the flows here are smaller than those above. The inflow to LF in the monthly survey was higher by 1.7 percentage point than in the quarterly survey. The outflow

from the LF in the monthly survey was 0.5 percentage points less than in the quarterly survey. Both differences are statistically insignificant.

These differences in the flows imply markedly different levels of LFP rates in "steady state" (the last row in Table 8). While the imputed steady state LFP rate in the quarterly survey is 55.9 percent, the imputed rate in the monthly survey is 61.8 percent, a difference of 5.9 percentage points. These results suggest that although the LFP rate could be expected to converge downwards according to the quarterly survey, the monthly data suggest that the rate is close to the LF in "steady state". Indeed, the quarterly survey "reflects" a labor market with more frictions than that "reflected" by the monthly survey. As the surveys were conducted in the same period in the same economy, it is plausible that these different expressions of the situation in the labor market resulted from the differing frequency of interviews.

Table 8: Flows From and Into the Labor Force by Survey Frequency, Phases 1 and 4

(Percent, Persons who entered the sample in 2011:Q3, balanced panel)

	Quarterly	Monthly	Difference
	i	ii	iii
LFP rates in 2011:Q3	59.1 (0.82) [3,568]	61.1 (0.75) [4,265]	-2.0 (1.17)* [7,833]
Entering Labor Force	9.3 (0.84) [1,200]	11.0 (0.77) [1,628]	-1.7 (1.16) [2,828]
Leaving Labor Force	7.4 (0.62) [1,738]	6.8 (0.49) [2,608]	0.5 (0.8) [4,744]
LFP in "Steady State"	55.9	61.8	5.9

Entering LF – the proportion of individuals who entered the LF to non-participants.
Leaving LF – the proportion of individuals who left the LF to participants.
LFP_{ss}- Labor Force Participation rate in steady state in a search model;
$LFP_{ss} \equiv$ Entering LF/(Entering LF +Leaving LF)
SE in parentheses; number of observations in brackets.

We can conclude that the first wave of the cross-sectional analysis and the panel analysis provides evidence that monthly interviews increase the reported LFP rate and the employment rate. The magnitude of the impact on the level of LFP rate of self-reporting individuals is moderate: an increase of 2.5–3.5 percentage points in phase 4 relative to an LFP rate of 66 percent in the quarterly survey. However, its impact on the estimated flows of self-reporting individuals between LF statuses is considerable relative to the flows themselves. The differing flows out from and into LF result in a markedly different "steady state" LFP rate in the monthly and quarterly surveys.

VI. Implications for Fixed Effects Estimations of Heterogeneous Effects

The psychological mechanisms, such as increased awareness or avoidance of self-stigmatizing replies, may have a heterogeneous impact on different groups. The left side of Table 9 presents DID estimates of the impact of monthly surveys on LFP rate in the panel dataset stratified by age groups. It documents that most of the effect of monthly interviews in the pooled estimations comes from self-reporting persons aged 45–64: In all specifications (cross section, panel, and exit from LF) this group exhibits a statistically significant and economically high tendency to be in the LF following monthly interviews. In all specifications, the estimated impact is about 5.5–6.25 percentage points. In contrast, the effect on persons aged 25–44 is essentially zero, and the effect on persons outside the prime-age LF (25–64 years old) is not precisely estimated. The right side of Table 9 documents similar yet smaller and less statistically significant patterns regarding the impact of monthly surveys on employment.

**Table 9: Estimates of the Effect of Monthly Interviews on
LFP and Exit from LF / Employment and Exit from Employment**
(Self-reporting persons who entered the sample in 2011:Q3, balanced panel)

	Labor Force Participation			Employment		
	Dependent Variable: LFP		Dependent Variable: Exit from LF	Dependent Variable: Employment		Dependent Variable: Exit from Employ-ment
	Cross Sectional Estimator	FE Panel Estimator		Cross Sectional Estimator	FE Panel Estimator	
	i	ii	iii	iv	v	vi
All Ages (pooled)	4.46 (1.77)**	2.48 (1.07)**	-2.02 (1.18)*	4.49 (1.69)**	1.37 (1.01)	-0.57 (1.12)
15–24 years old	12.59 (10.87)	-2.49 (6.97)	6.57` (6.67)	8.78 (14.16)	-4.54 (5.61)	12.09 (8.08)
25–44 years old	0.53 (2.11)	-0.10 (1.81)	0.13 (1.51)	1.50 (2.29)	0.00 (1.81)	0.57 (1.49)
45–64 years old	5.52 (3.16)*	5.97 (1.94)***	-6.25 (1.90)***	6.00 (3.67)*	3.88 (1.75)**	-3.97 (1.71)**
64+ years old	1.02 (3.37)	0.75 (1.63)	4.54 (7.07)	0.58 (3.12)	-0.08 (1.51)	5.25 (7.19)

Notes:
1. Regression coefficients are multiplied by 100
2. The cross-section and exit-flow regressions include controls for personal characteristics, excluding age, and FE for subdistrict of residence. The panel regressions include FE for individuals.
3. SE are clustered by subdistrict in the cross-sectional regressions and by individuals in the panel regressions.

At this stage we can only speculate regarding the reasons for the pronounced impact among 45–64 year-old interviewees. Perhaps, older interviewees are more affected by the self-stigmatizing mechanisms than younger interviewees. We find no reason to assume that the increased awareness mechanism affects older interviewees more than younger ones.

The heterogeneous effect of the frequent interview by age group suggests that interview frequency may bias regression estimates of the association between LFP rate and age. We test this suggestion by regressing LFP on the interaction of age (grouped) with dummy variables for the monthly and quarterly surveys.

$$y_{i,t} = \beta_1 \cdot Quarterly_{i,t} \cdot age_{i,t} + \beta_2 \cdot Monthly_{i,t} \cdot age_{i,t} + \beta_3 \cdot Monthly_{i,t} + \gamma X_{i,t} + \epsilon_{i,t}$$

Where $Quarterly_{i,t}$ and $Monthly_{i,t}$ are dummy variables designating the frequency of the interviews, $age_{i,t}$ is the individual's age (grouped), and $X_{i,t}$ *is* a vector of other personal characteristics. This regression allows us to test the difference between the coefficients of age in regressions based on the two surveys by a simple F-test ($Quarterly_{i,t} \cdot age_{i,t} = Monthly_{i,t} \cdot age_{i,t}$). We sharpen the results by focusing on prime-age individuals to capture the different effects between younger adults (25–44 years old) and older adults (45–64 years old).

Table 10 presents the coefficients of the above regression, *which do not include FE,* and demonstrates that there is no significant difference between the association of LFP and age in the quarterly and monthly surveys. In contrast, the regression coefficients of estimations of the association between LFP and age in *regressions with FE* and the association between age and exit from LF are markedly different: In the FE specification, the coefficient of age is positive in the quarterly sample (6.73 percentage-point increase in LFP for each age group) and negative in the monthly sample (8 percentage-point decrease in LFP for each age group) Similarly, the

Table 10: Association between Age Group and LF Outcomes by Survey Frequency

(Self-reporting 25–64 year-old persons who entered the sample in 2011:Q3, balanced panel)

Dependent Variable:	Labor Force Participation			Exit from LF	
	i	ii	iii	iv	v
Age (grouped)* Quarterly Survey*100	-7.11 (1.32)***	-6.83 (1.31)***	10.68 (8.05)	1.99 (1.06)*	1.90 (1.07)*
Age (grouped)* Monthly Survey*100	-5.76 (0.88)***	-5.44 (1.12)***	-15.41 (7.08)	-1.08 (0.65)*	-0.99 (0.64)
Monthly LFS*100	-9.50 (9.31)	-9.62 (9.24)		14.06 (6.79)**	13.15 (6.81)*
Q4*100	-2.47 (1.08)**	-2.48 (1.08)**	-2.35 (1.06)**		
Monthly LFS*Q4*100	3.53 (1.36)***	3.21 (1.38)**	3.43 (1.34)**		
Controlling For Personal Characteristics	N	Y	N	N	Y
Controlling For Personal FE	N	N	Y	N	N
R²	0.0342	0.054	0.010	0.010	0.021
N	3,316	3,303	3,316	1,359	1,356
F-test: Age Q=Age M [p-value]	0.60 [0.394]	0.65 [0.418]	5.92 [0.015]**	6.10 [0.031]**	5.32 [0.021]**

Notes:

Age groups are: 1: 25-34; 2: 35-44; 3: 45-54; 4: 55-64.

SE are clustered by individuals.

R^2 in the FE specifications (columns iii & vi) are overall R^2.

likelihood of exit from LF is *positively* associated with the age variable in the quarterly survey, but *negatively* associated with age in the monthly survey. An F-test of the difference between the coefficients of age in these samples (last row in Table 10) suggests that the difference is marginally significant in the FE specification, and highly significant in the estimations of the flows. Table 10-A documents a similar pattern when the dependent variable was employment, rather than LFP; yet the estimated impact of monthly interviews on the regression coefficients is smaller and its statistical significance is weaker.

These findings suggest that frequency of interviews for a survey might influence estimations of determinants of levels of LFP using FE regressions, as well as the estimations of flows. The apparent impact of panel conditioning in FE regressions and regressions on flows—but not other regressions on levels—are explained by Bound et al.'s conclusion in their survey of measurement errors in survey data: Some variables which "… seem to be reported with reasonable accuracy become candidates for concern when panel data are used in ways that *effectively difference out* much of the true variation while increasing the noise." Indeed, both FE regressions and regressions of flows difference out much of the variation between individuals, yet keep statistical noise including panel-conditioning-induced biases.

VII. Concluding Remarks

This study uses a rare episode: a large scale survey, the Israeli LFS, was fully conducted concurrently at two frequencies: monthly and quarterly. The monthly survey included four month-after-month phases of interviews, while the quarterly survey included two interviews in the parallel first and fourth months. We can therefore estimate the impact of additional interviews in the second and third months on reported LF outcomes in the fourth month (phase 4).

The study provided evidence that among self-reporting individuals, monthly interviews increased the LFP rate in phase 4 relative to the quarterly survey by 2.6–3.4 percentage points. Specifically, monthly interviews seem to decrease the rate of LF exit and somewhat increase the rate of LF entry among self-reporting individuals. In contrast, the effect on proxy reported individuals, that is, those whose data was supplied by another household member, is smaller and statistically insignificant in the cross-sectional specification and practically zero in the panel specification. This contrast supports the credibility of the results, by suggesting that it is not merely the differences in the design of the quarterly and the monthly surveys that should have affected both self-reporting and proxy-reported individuals.

The estimated impact of monthly interviews on self-reporting individuals could be explained by means of two mechanisms: Monthly interviews increase the interviewee's preference or familiarity with the questionnaire and his ability not to provide self-stigmatizing information. Alternatively, monthly interviews increase the interviewee's awareness and thereby

induce him to remain at his job or keep searching for a job. As the data in the LFS are based on interviews rather than on administrative data, we are unable to determine whether the estimated effect reflects merely miss-reported data by the interviewee as the first mechanism suggests, or an actual behavioral change as the second mechanism implies. Such interview-induced change in *actual* behavior was demonstrated in the context of public health by Zwane et al. (2010). In any event, the minor or non-existent effect of monthly interviews on proxy-reported individuals suggests that interviewees' preference for reducing the length of the interviews–which also applies to proxy-reported individuals–is not the main mechanism at work in our case.

This case study provides some general lessons regarding panel conditioning in household surveys. First, frequent interviews are likely to affect mainly self-reporting individuals and to lesser extent proxy-reported individuals. In other words, this specific panel conditioning is likely to be more pronounced in surveys in which all of the data are self-reported, or the share of proxy-reported data is small, that is, when the number of adults in the households are small.

Second, panel conditioning is likely to have a relatively large impact on flows between labor market statuses, while its impact on levels is moderate. In the context of the LFS, this result implies that LFS-based dynamic analyses are likely to be biased and portray labor markets covered by monthly surveys (such as those in the USA, Canada and Australia) as having fewer frictions than labor markets covered by quarterly surveys. Indeed, to the extent that the panel conditioning effect on the interviewees fades over time, our results cast doubts on the credibility of estimates of changes over time which are based on frequent panel surveys such as the monthly US-CPS. Notably, measuring changes over time is one of the motivations for conducting panel surveys.

Third, we demonstrated that interview frequency can affect coefficients of explanatory variables in fixed effect regressions on levels of outcomes, and parallel coefficients in regressions of flows. In the context of our study, the coefficient of age was positive in the quarterly survey but negative in the monthly survey. Presumably, this difference is a result of the impact of the monthly interviews on older (45–64 year-old) interviewees but not on younger ones. Notably, Zwane et al. (2011) provide a similar result.[16] In contrast, the effect of interview frequency on coefficients of regressions (without fixed effects) on levels of outcomes is likely to be small as the levels of the variables are roughly similar in both surveys.

Finally, the differing impact on self-reporting and proxy-reported individuals highlights the need to include administrative data generated by the survey itself, such as the identity of the interviewee, in standard micro-datasets. Such administrative data may enable scholars to test the sensitivity of their results to the survey design. In other cases, the administrative data may allow

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[16] The health of households that used chlorine because they were frequently interviewed on sources and treatment of water was not correlated with alternative water treatments. In contrast, the health of infrequently-interviewed-households improved following the use of alternative water treatment.

researchers to estimate the impact of changes in the survey design itself on outcomes, as this study estimated the impact of varying the interview frequency.

This study documented the impact of increasing the frequency of interviews from two to four interviews within four months a short panel. One may wonder whether surveys of higher frequency, such as Princeton's weekly survey of unemployed in New Jersey[17], suffer from a larger bias. Similarly, it is plausible that longitudinal surveys in which the same individuals are interviewed over many years, such as the NLY, also suffer from a larger panel-conditioning bias.

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[17] http://opr.princeton.edu/archive/njui/

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Appendix – Additional Tables

The tables below present estimation results on the effect of interview frequency on employment, while similar results on LFP were presented in corresponding tables in main part of the article. Table 4-A corresponds to Table 4, Table 6-A corresponds to Table 6, and so on.

Table 4-A: Cross Sectional DID Estimates of Employment by Reporter
(Pooled sample of persons in phases 1 and 4; October–December 2011)

	Self-Reporting Individuals			Proxy-Reported Individuals		
	i	ii	iii	iv	v	vi
Monthly LFS*100	-2.56 (1.41)*	-4.53 (1.28)***	-1.78 (1.35)	-0.80 (1.53)	-2.19 (1.46)	-0.92 (1.18)
Phase 4*100	-2.61 (1.32)*	-2.61 (1.21)**	-0.19 (1.18)	-1.07 (1.51)	-1.40 (1.45)	-2.93 (1.13)**
Monthly LFS * Phase 4 *100	4.77 (1.58)***	4.93 (1.62)**	3.52 (1.46)**	1.81 (2.24)	2.33 (2.23)	2.31 (1.78)**
Personal Characteristics	N	Y	Y	N	Y	Y
Month of Survey FE	N	N	Y	N	N	Y
Subdistrict FE	N	N	Y	N	N	Y
R²	0.043	0.107	0.400	0.0001	0.085	0.392
N	8,522	8,431	8,431	11,677	11,464	11,464

Note: Robust SE clustered by subdistrict.

Table 6-A: Difference in Differences Estimates of the Effect of Monthly Interviews on Employment by the Identity of the Information Provider
(Persons who entered the sample in 2011:Q3, balanced panel)

Dependent Variable:	Self Reported in Phases 1 and 4			Reported by Other Household Member in Phase 1 or phase 4		
	i	ii	iii	iv	v	vi
Monthly LFS*100	-2.15 (3.14)	-1.04 (2.27)		1.13 (1.95)	1.81 (1.58)	
Q4*100	-1.90 (1.18)	-1.46 (1.22)	-2.05 (1.18)	-0.04 (0.84)	-0.10 (0.86)	0.08 (0.80)
Monthly LFS*Q4*100	3.82 (2.08)*	2.76 (1.89)	2.51 (1.44)*	0.28 (1.39)	-1.00 (1.29)	-0.04 (0.10)
Controlling For Personal Characteristics	N	Y	N	N	Y	N
Controlling For Personal FE	N	N	Y	N	N	Y
R²	0.0004	0.354	0.0002	0.0001	0.347	0.0001
N	2,946	2,905	2,946	7,890	7,804	7,890

Notes:
SE are clustered by individuals.
R² in the FE specifications (columns iii and vi) are overall R².

Table 10-A: Association Between Age Group and LF Outcomes by Survey Frequency
(Self-reporting 25–64 year-old persons who entered the sample in 2011:Q3, balanced panel)

Dependent Variable:	Employment			Exit from Emp.	
	i	ii	iii	iv	v
Age (grouped)* Quarterly Survey*100	-7.36 (1.42)***	-7.00 (1.40)***	1.32 (0.98)	0.99 (10.25)	0.85 (10.46)
Age (grouped)* Monthly Survey*100	-5.32 (1.22)***	-5.01 (1.21)***	-11.61 (7.45)	-1.40 (0.72)*	-1.32 (0.73)*
Monthly LFS*100	-13.54 (10.20)	-13.24 (10.11)		11.41 (6.97)	10.33 (7.03)
Q4*100	-1.59 (1.00)	-1.58 (1.01)	-1.32 (0.98)		
Monthly LFS*Q4*100	2.50 (1.29)*	2.16 (1.31)*	2.23 (1.29)*		
Controlling For Personal Characteristics	N	Y	N	N	Y
Controlling For Personal FE	N	N	Y	N	N
R^2	0.029	0.051	0.001	0.006	0.015
N	3316	3303	3,316	1,282	1,279
F-test: Age Q=Age M [p-value]	1.18 [0.27]	1.14 [0.28]	2.97 [0.08]*	3.63 [0.06]*	2.90 [0.09]*

Notes:

SE are clustered by individuals.

R^2 in the FE specifications (columns iii & vi) are overall R^2.