Reversal of Fortunes: The Rise and Fall of Lifetime Earnings of Iranian Men

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I. Introduction

Iran has experienced huge shocks to its economy in the last 3 decades. The Islamic Revolution of 1979 was followed by an 8-year bloody war with Iraq and the collapse of oil prices in 1986. In this article, we ask how individuals and families have fared through these tumultuous times. We compare individuals' lifetime earnings and consumption before and after the revolution by following cohorts of individuals over time. Looking at the well-being of cohorts offers a deeper understanding of changes in welfare than the common reference to average incomes. Average economic indicators tell us how things are at any point in time but do not reflect well what individuals and families experience over a lifetime. As Deaton (1997, 117) has remarked, "Questions about gainers and losers from economic development can be conveniently addressed by following cohorts over time."

We are particularly interested in the fate of the younger cohorts who reached the critical age of entry into the labor market and family formation at the time of the revolution in 1979 and the start of the war with Iraq in 1980. The entire first decade of the revolution is marked by strong ideological fervor and economic decline. Cultural, social, and economic factors at critical ages influence cohorts for a lifetime. "Traumatic episodes like war and revolution," writes Ryder (1965, 851), "may become the foci of crystallization of the mentality of a cohort." The experience of the cohorts of Iranians who suffered

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the dislocations of the war and the revolution in the 1980s was compounded by the negative labor market effects of a growing cohort size. As Easterlin (1987) has shown, demographic shifts can appreciably affect a cohort's fortune, let alone facing war and revolution as one prepares for adult life. In this article, we attempt to measure the combined effect of these events on these cohorts' lifetime earnings and consumption.

We focus on men because women's share of the labor force is low, and their labor market behavior has changed over time. Women are about one-fourth as likely to report earnings as men, which means fewer observations and lowers the precision of the estimates. Furthermore, women's participation rate has increased over time, from about 10% for cohorts born in the 1930s to 20% for cohorts born in the 1970s, which probably causes some selection and estimate bias. Finally, because social norms in Iran still conform to the one-breadwinner model, we believe that changes in men's earnings reflect better the changing fortunes of generations through the upheavals of revolution and war. We show that the cohorts of men born in the 1970s fared poorly in terms of earnings, relative to their parents who—although they, too, experienced the economic decline of the 1980s—had lived in better times and were in their midcareers when the economy collapsed. Our findings also show that, in contrast to earnings, in terms of per capita expenditures these younger cohorts did not do as badly, a phenomenon that we attribute to transfers from the older generations and from the government.

Like other people, Iranians compare their well-being to that of generations before them, especially their parents. Tainted with memories of the revolution and the war, these comparisons are often very gloomy and at odds with the fact that, thanks to steady economic growth in the last decade, the economy has nearly fully recovered. Average real incomes in 2007 were about the level they were during the 1970s oil boom, a period that for most Iranians is the golden age.[1] Furthermore, in recent years poverty has declined substantially, and inequality, while it has remained high, is lower compared to the 1970s (Salehi-Isfahani 2009). The frustrations Iranians express regarding their present economic conditions and the nostalgia they feel for the golden age can be reconciled with macroeconomic facts if the comparison is based on lifetime earnings of cohorts instead of period averages.

Considering lifetime experiences is especially important for a case such as Iran's in which the economy has experienced large booms and busts. Com-

[1] For example, a reporter for the *Washington Post*, stating his impressions from talking to ordinary people, wrote, "In real terms, Iranians earn one-fourth of what they did earn [before the 1979 revolution]" (Molavi 2003). For more examples of reports of dire economic conditions, see Salehi-Isfahani (2006).

parisons based on period averages tell only a partial story because most individuals live through both good and bad times, and changes in overall average earnings do not accurately describe any particular cohort's life experience. For example, the effect of the 1974–77 boom, which seems to have made a lasting impression on Iranians of all ages, on any specific cohort's lifetime earnings or consumption is limited because 4 years is only a fraction of any cohort's lifetime. Similarly, the impression left by the worst years of the war (1985–88), as bad as it has been for the life experience of most Iranians, was still short compared to a lifetime. So, to the extent that all cohorts have experienced both good and bad times, the comparisons between successive cohorts' lifecycle earnings may tell a story that is different from the ups and downs of average earnings.

Since the seminal article by Ryder (1965), sociologists and demographers have been aware of the conceptually separate cohort, age, and period effects on a variety of indicators such as fertility, mortality, and labor force participation. A growing literature in economics also recognizes the importance of distinguishing empirically between cohort, age, and period effects on consumption and earnings of individuals and families (Shorrocks 1975; Johnson 1980; Deaton 1985, 1997; Heckman and Robb 1985; Deaton and Paxson 1994, 2000; Attanasio and Davis 1996; Blundell and Preston 1998; Beaudry and Green 2000; Gosling, Machin, and Meghir 2000; Skoufias and Suryahadi 2002; Fukuda 2006; McKenzie 2006a, 2006b) and in labor supply (Farkas 1977; Clogg 1982; Beaudry and Lemieux 1999; Attanasio, Low, and Sanchez-Marcos 2004).

Browning, Deaton, and Irish (1985) and Deaton (1985) pioneered the use of annual surveys to track cohorts over time and attempted to decompose changes in cohort averages over time into age, period, and cohort effects. The main difficulty with such a decomposition is that age, year of birth, and the year of observation are linearly dependent—knowing any two determines the third. All three effects cannot be identified, unless one is ready to make some strong assumptions. Many researchers have adopted restrictions based on prior information that break this identity. Heckman and Robb (1985) question the very idea of decomposition into linearly dependent variables. They propose that the search for prior information should be about the variables that underlie the age, cohort, and period effects, rather than restrictions that break the identity. They advocate using proxy variables, such as the rate of unemployment for the year effects, to substitute for one of the three effects. Despite this objection, many researchers have found it useful to use nonparametric methods with restrictions on the coefficients of the regression equation to estimate age, cohort, and period effects. In the context of earnings and consumption, Deaton

and Paxson (1994), Paxson (1996), Attanasio (1998), and Parker (1999), among others, have used restrictions on the year effects that require them to be orthogonal to a time trend and sum to zero. The appeal of this method is that time is given a neutral role, allowed only to affect the variable being decomposed via cyclical variation. Any trend in the underlying data is thus attributed to age and cohort effects. Another strand of the literature, which attempts to explain technical change from price data on used capital goods, offers a more defensible and straightforward method of identification (Cagan 1965; Hall 1971; Berndt, Griliches, and Rappaport 1995). This method assumes that the cohort effects for neighboring cohorts are equal. Fukuda (2006) imposes similar prior information but uses Bayesian procedures. In this article, we assume explicitly that cohorts born in 5-year intervals have the same cohort effects.[2]

Our main results are robust to the choice of methodology and plausible in view of the known facts of economic fluctuations and the relationship between age and earnings in Iran. The estimated year effects pick up the large downturn in the economy in the 1980s and the recovery after 1989, and the age profiles indicate sharply rising earnings until age 30, which peak at the mid-40s. The cohort effects, which indicate how the age-earnings profiles of different cohorts stack up relative to one another and are the object of this study, show a rising trend for cohorts born before the mid-1950s, who were in their mid-20s and older at the start of the revolution. This trend is reversed for cohorts born later, who reached adult life after the revolution. According to our estimates, the youngest cohorts we study, born during 1975–79, experienced declines ranging from 20% to 30% in their lifetime earnings.

The validity of any decomposition of cohort data depends crucially on the length of time for which each cohort is observed. We are fortunate to have access to an unusually long series of household expenditures and income surveys, taken between 1984 and 2007, which allow us to track cohort earnings and expenditures for 24 years, encompassing the economic decline of the 1980s as well as the boom of 2000–2007.[3] We use data from these surveys to construct profiles of income and expenditures for individual cohorts of men as they age. This enables us to follow cohorts born as early as 1924 and those born as late as 1979. While no cohort is observed for the entire life cycle, the method we employ allows us to estimate shifts in the position of the life-cycle income and consumption profile of each cohort over time.

[2] Two- and 3-year cohorts produce similar results.

[3] The longest pseudopanel of which we are aware, McKenzie's (2006b) study of savings and consumption in Taiwan, is 21 years.

We also consider changes in individual welfare based on cohort effects of consumption (measured by per-adult-equivalent household expenditures), which include transfers within the household and from the government. In contrast to individual earnings, we find that the estimates of cohort effects are much more sensitive to our decomposition assumptions. Cohort per capita household expenditures have a stronger positive time trend than earnings, so in their case the assumption to make the period effects orthogonal to the time trend becomes critical. Without the orthogonality restriction, the year effects absorb the rising trend in consumption, and the cohort effects are rather flat for younger cohorts, in contrast to earnings. With the orthogonality restriction, the cohort effects rise continuously for all cohorts, similar to what we reported in Marku and Salehi-Isfahani (2006).[4] Both sets of assumptions lead to the same conclusion that younger cohorts did better in terms of lifetime consumption than earnings. We attribute this to compensatory transfers from the older generation to these cohorts as well as rising transfers from the government that have boosted personal consumption.

The plan of this article is as follows. Section II provides a brief overview of the macroeconomic shocks of the last 3 decades. Section III describes the microdata, and Section IV discusses the method of decomposition. Section V presents the empirical results for individual earnings, and Section VI presents those for per capita household expenditures. Section VII concludes by discussing the implications of our findings for Iran's political economy.

II. The Iranian Context

The Islamic Revolution interrupted the longest period of economic growth in Iran's history, which began with Iran's brief growth miracle in the 1960s—when the economy grew at nearly 9% per year, unaided by large inflows of oil money—and ended with the oil boom of 1973–77 (fig. 1).[5] The economic collapse of the 1980s, coming as it did after 25 years of steady improvements in the standard of living, is one reason why today there exists a widespread feeling of lost fortunes as a result of the revolution, even though, as measured by average indicators, economic growth since the end of the war with Iraq in 1988 has erased most of those losses. Figure 1 depicts the wide fluctuations in GDP and private consumption per capita (PrivateCPC) taken from the World Bank (2007) and the Central Bank of Iran. All three series show, in addition to the long period of economic growth in the 1960s and 1970s, the

[4] Our previous study was based on 21 surveys from 1984 to 2004 and followed closely the method suggested by Deaton and Paxson (1994) and Deaton (1997).

[5] For surveys of Iran's macroeconomic conditions, see Amuzegar (1993), Pesaran (2000), and Jalali-Naini (2005).

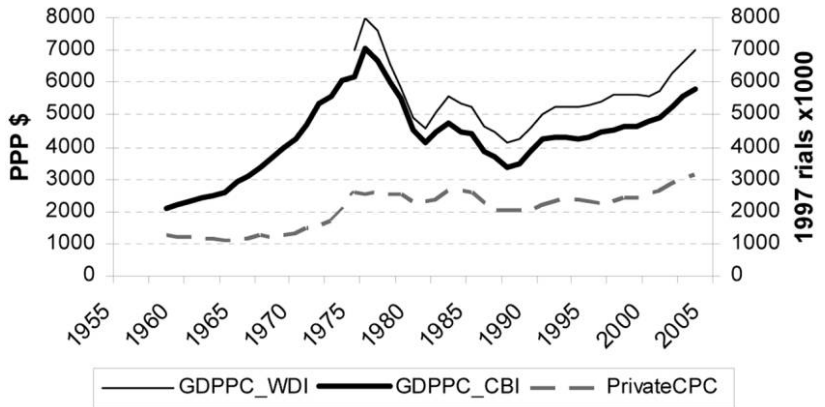

Figure 1. Rise and fall of GDP per capita in Iran, 1955–2005. GDPPC_WDI (*left axis*) is GDP per capita in 2000 purchasing-power-parity dollars from World Development Indicators, and GDPPC_CBI (*right axis*) is from the Central Bank of Iran and is measured in thousands of 1997 rials. PrivateCPC is private consumption per capita from the national accounts. Sources: World Bank (2007) and the Central Bank of Iran, annual report, various years.

economic collapse after the revolution and how the recession deepened during the war with Iraq (1980–88) and after the collapse of oil prices in 1986. By 1987, GDP per capita was down by about 50%, compared to its peak in 1975. They also show how the pace of economic recovery since 1989 depended on the rise of oil prices. Growth was robust when oil prices spiked as a result of the first Persian Gulf war in 1990–92, was very slow in the mid-1990s when oil prices hit a 30-year low (in 1998), and has picked up pace with rising oil prices since 2000. From the macro perspective, the best of times were in 1973–77, before the start of revolutionary disruptions in 1978, and the worst of times were the mid-1980s.[6] According to the national accounts data in figure 1, fluctuations in average private consumption per capita were softened considerably as investment took the brunt of the macroeconomic shocks.

The survey data that we use in this article also form the basis for the calculation of personal consumption in the national accounts produced by the Central Bank of Iran. Naturally, the two series (i.e., the one that we compute and that provided by the Central Bank) track each other well, as seen in figure 2. This figure also shows that earnings have fluctuated more widely than consumption and that rural households have been, for the most part, shielded

[6] According to the GDP series published by the World Bank and Central Bank of Iran, even in the 1990s per capita output was 30% lower.

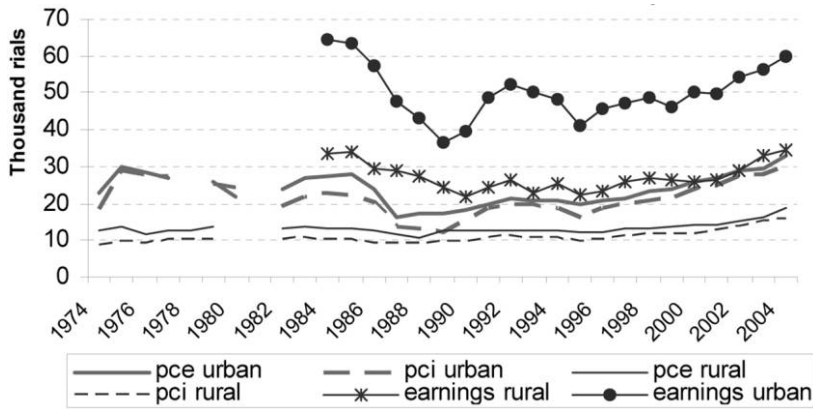

Figure 2. Fluctuations in individual earnings and average per capita household income (pci) and expenditures (pce), 1984–2004. Sources: Before 1984, Statistical Center of Iran; 1984–2004, authors' calculations from Household Expenditures and Income Survey data.

from these fluctuations. All series show declining real individual earnings and household expenditures until 1988 and slow recovery thereafter.

III. Survey Data

We use 24 rounds of the Household Expenditures and Income Surveys (HEIS) conducted annually by the Statistical Center of Iran to track cohort earnings and consumption. The surveys have been conducted since 1963, but only those for 1984–2007 are available to us in unit records. New samples are drawn each year for these surveys, so they do not form a panel and it is not possible to track households over time. Because these surveys are used to construct national macroeconomic statistics, they are carefully scrutinized and are therefore reliable and consistent over the years. The focus of the surveys is on expenditures, which are collected in impressive detail, but they also contain information on demographics, labor market status, and incomes of individuals.

Expenditures consistently exceed income (see fig. 2) because of underreporting of incomes, which is not unusual for survey data from developing countries (Deaton 1997, 29), and inclusion of implicit rent for owner-occupied housing. Survey respondents may try to hide nonwage and nonsalary income for reasons of tax evasion. The gap between incomes and expenditures has remained relatively constant over time (less than 10% in urban areas and 15%–20% in rural areas), and the two series track each other closely.

The number of households in these surveys ranges from a low of 5,700 in 1986–87 (the worst years of the war) to 36,500 in 1995 (just before the 1996 census). Despite the variation in sample size, the surveys have remained na-

TABLE 1
CELL SIZES FOR SELECTED COHORT-YEAR PAIRS OF MEN

	Cohort					
Year	1930	1940	1950	1960	1970	1979
1984	361	367	499	696		
1985	402	547	656	826		
1986	76	70	105	140		
1987	58	71	111	147		
1988	103	108	169	229		
1989	134	150	187	234		
1990	336	345	474	554		
1991	200	234	275	409		
1992	242	280	337	506	545	
1993	133	144	199	276	348	
1994	179	248	285	510	550	
1995		494	838	1,085	1,015	
1996		230	342	569	633	
1997		248	336	587	630	
1998		174	266	427	533	
1999		257	369	643	700	
2000		338	444	747	839	
2001		200	316	565	672	1,003
2002		294	462	815	882	1,161
2003		184	280	536	594	791
2004		146	252	454	629	889
2005			350	737	835	870
2006			277	570	804	1,041
2007			294	541	748	914

tionally representative. This is the case even for the war years during which war-ravaged provinces were also sampled proportionately. The number of individuals covered varies from about 37,000 in 1986 to over 193,000 in 1995 (see table A1 for sample sizes). Altogether, there are approximately 2.6 million individuals in the 24 surveys.

We use probability sampling weights in all estimations in this article. The HEIS does not identify households by their census block, so we are not able to estimate robust standard errors that correct for intrablock correlations between households. We do not think that this is a serious problem in our case because our estimation technique mainly exploits the variation between cohorts rather than between individuals and households.

To construct our pseudopanel, we define cohorts by their birth year and extract their earnings from consecutive surveys. Table 1 shows, for selected cohorts, the number of men in each cohort-year cell. Cell sizes range from a low of 58 for the cohort born in 1930 and observed in 1986, when the survey was collected in a much smaller scale, to a high of 1,161 for the cohort born in 1979 and observed in 2002. For earnings, we use net annual wage and salary income and income from self-employment. Table 2 presents the sample

TABLE 2
MEDIAN REAL EARNINGS AND PER CAPITA EXPENDITURES (2007 RIALS)

	Total Earnings		
Year	Men	Women	Expenditures per Capita, Men
1984	27,428,572	16,842,106	20,785,070
1985	27,118,644	18,743,730	20,562,418
1986	24,063,926	12,054,794	17,244,546
1987	20,855,614	13,096,086	16,272,514
1988	19,809,972	10,574,959	15,550,159
1989	18,347,416	9,154,929	15,833,840
1990	19,218,142	7,559,395	17,708,666
1991	20,925,554	8,781,362	18,682,986
1992	22,014,390	10,359,713	18,833,522
1993	22,611,242	8,430,913	18,564,556
1994	20,962,706	9,922,012	18,313,732
1995	18,224,028	6,699,752	17,770,526
1996	19,227,144	7,639,257	17,905,462
1997	21,012,714	7,405,576	18,508,112
1998	21,085,142	8,504,607	19,766,666
1999	20,390,824	6,880,734	20,606,566
2000	21,612,522	7,141,061	21,412,388
2001	22,302,484	7,449,210	21,876,754
2002	24,851,276	8,635,578	23,417,958
2003	26,673,410	8,794,430	24,760,016
2004	29,685,444	8,646,672	27,783,108
2005	31,074,554	11,127,187	28,526,982
2006	31,713,202	13,109,283	27,932,328
2007	33,311,600	13,900,000	29,703,320

Sources. Authors' calculations and Household Expenditures and Income Survey data files.
Note. Sample includes individuals age 20–64 with nonzero earnings (except for the unemployed).

medians for earnings of men and per-adult-equivalent expenditures for men.[7] The survey averages show that over the entire period, men's earnings have been rising, but they have declined for women. Per-adult-equivalent expenditures (defined by dividing total expenditures by the square root of household size) have been smoother over time, although they, too, were below their 1984 value in the 1980s and 1990s.

We do not have information on hours worked, so we cannot measure hourly wages. We include in the sample all active individuals: those employed who report positive income from wages and self-employment and the unemployed. Including the unemployed is important because the unemployment rate for the young has increased substantially over time, and it is important that the average cohort incomes reflect the fact that they are unable to work. Young people are also staying in school longer, some of whom report earnings. Because many who work while studying are part-time workers, their annual earnings

[7] We prefer to work with the median in this article because it reduces the influence of outliers. In Marku and Salehi-Isfahani (2006), we used the means of variables.

TABLE 3
PROPORTION OF MEN AND WOMEN WITH REPORTED EARNINGS, BY COHORT AND AGE

Age	1939–43	1944–48	1949–53	1954–58	1959–63	1964–68	1969–73	1974–78
Men:								
20–24					.671	.589	.663	.661
25–29				.912	.862	.888	.880	.853
30–34			.957	.951	.961	.956	.950	.958
35–39		.965	.962	.972	.967	.961	.966	
40–44	.959	.968	.967	.966	.961	.944		
45–49	.952	.957	.950	.951	.968			
50–54	.921	.901	.878	.848				
55–59	.846	.802	.800					
60–64	.703	.709						
Women:								
20–24					.083	.092	.167	.159
25–29				.132	.121	.180	.204	.187
30–34			.125	.139	.192	.201	.202	.183
35–39		.087	.130	.208	.202	.196	.171	
40–44	.084	.124	.199	.213	.197	.209		
45–49	.089	.177	.190	.183	.196			
50–54	.138	.160	.161	.153				
55–59	.137	.130	.124					
60–64	.097	.116						

Sources. Authors' calculations and Household Expenditures and Income Surveys data files.

do not reflect their true earning power; we try to minimize this problem by focusing on postschool ages, 22–64, and excluding students. The results are not too sensitive to the choice of age limits or exclusion of students.

Excluding nonearners does not introduce selection into the sample for men because, unlike women, their participation in the labor market has remained stable over time and between cohorts. Table 3 shows that the proportion of earners for 5-year cohorts has remained quite stable for all cohorts at the same age, except for the 50–54-year-olds for whom the proportion of earners has declined across cohorts.[8]

Not all cohorts are observed for the same length of time. The youngest cohort to be observed for the entire 24 years of surveys was born in 1964; it was first observed in 1984 and last observed in 2007. The youngest cohort in our sample was born in 1979 and was only observed for 8 years, 1999–2007. The earnings profiles are easily constructed by using the median earnings of 20-year-olds in 1984, 21-year-olds in 1985, and so on. Figure 3 summarizes cohort earnings profiles for cohorts of men born since 1935, in 5-year intervals.

[8] The younger cohorts of women start their careers earlier, but all cohorts behave the same when they are in their 30s and 40s. For cohorts born during 1969–78, this proportion is about 16%–17% when they are 20–24 years old, up from 8%–9% for cohorts born during 1959–68. This is understandable because younger cohorts of women stay in school longer and marry later, both of which reduce labor force participation.

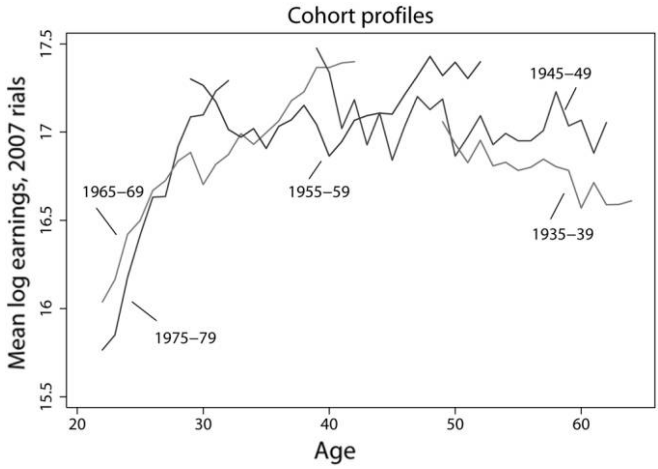

Figure 3. Average real earnings of men by age for selected 5-year cohorts, 2007 prices

Each line in this graph represents the median earnings of a particular cohort as it ages during the period in which it is observed in successive surveys. The first line on the left shows median earnings of all individuals born during 1975–79, whom we can observe from age 15 (in 1984) to age 32 (in 2007) but who appear in our empirical sample during 1997–2007, since we limit the age range to 22–64. The second line from the left shows the earnings for the cohort of men born in 1965 and observed between age 22 and 40, and so on. The last line to the right belongs to the 1935–39 cohort, which we observe from age 50 to 64 (between 1984 and 1998). If cohorts had lived through a long period of consistent economic growth, as they did in Taiwan (Deaton 1997, 118), the lines representing cohort earnings would not cross one another; the profiles for younger cohorts would lie to the left and above the profiles for older cohorts. In Iran, the large economic decline of the 1980s appears to have hurt nearly all cohorts' earnings but at different points in their lives, as demonstrated by the intersecting lines representing earning profiles in figure 3.

IV. Methodology

Our objective in this article is essentially to estimate the relative position of the age-earnings profiles of different cohorts from their actual age-earnings profiles, by purging from the latter the effect of the year of observation. Figure 3 depicts these profiles for selected cohorts. Each line describes the median earnings of a cohort as it has aged over the period covered by our surveys. Since these cohorts are observed during different periods of their lives, the

lines in figure 3 reflect the effect of age (inverted U shape) as well as how these cohorts have fared relative to one another. Without knowing the shape of the age-earnings profile, it seems impossible to say whether the younger cohorts are better off than the older cohorts. Thus, a natural step in comparing lifetime earnings of these cohorts is to assume a uniform age-earnings profile for all cohorts. Our semiparametric regressions let the data determine the shape of this common profile.

Following the literature, we express the log median earnings of each cohort in a given year as a linear, additive function of its age a, birth year c, and year of observation t:

$$\ln y_{ct} = \beta + \alpha_a + \gamma_c + \psi_t + u_{ct}. \tag{1}$$

The age effects, α_a, pick up changes in earnings associated with age and experience; the cohort effects, γ_c, stand for factors that affect cohorts because of the time they were born, when they entered the labor market, and the like. The year effects, ψ_t, stand for the conditions of the economy at the time of observation. While these factors can be understood as having independent effects on a person's earnings in any given year, as written they are linearly independent ($c = t - a$). As Deaton (1997) has noted, this functional form is useful for thinking about changes in the standard of living of cohorts, which are measured by their life-cycle earnings or consumption. Any difference in the life-cycle earnings of two cohorts is captured by the cohort effects, while keeping age and year effects constant.

As Deaton (1997) has noted, when we lack a priori information on the shape of the cohort, age, and year effects, and the pseudopanel spans a long period, the best way to estimate this relationship is by semiparametrically using cohort, age, and year dummies. This suggests an estimation equation such as

$$y = \beta + A\alpha + C\gamma + Y\psi + u, \tag{2}$$

where A is the matrix of age dummies, C the matrix of cohort dummies, and Y the matrix of year dummies, and y is the stacked vector of cohort-year observations.

Equation (2) cannot be estimated as it is because of a linear relationship between A, C, and Y:

$$As_a = Ys_y - Cs_c, \tag{3}$$

where the s vectors are arithmetic sequences $\{0, 1, 2, 3, \ldots\}$ of the length given by the number of columns of the matrix that premultiplies them. The identity in equation (3) simply states that if we know the birth year of a person and the year of the survey, we know his or her age. This is the well-known problem

of identification in any analysis of cohort behavior (Glenn 2005).[9] We drop one column from each of the dummy matrices but need one more restriction on the coefficients to break the identity. We follow Hall (1971) and assume that the variation in cohort effects is smooth enough that the coefficients of adjacent cohorts in equation (2) are equal. We experimented with 2-, 3-, and 5-year cohorts and settled on the last, which gave the most precisely estimated coefficients (the results for other groupings were similar and are available on request). Hall (1971) employed used prices for trucks of different vintages to separate the age and year effects on these prices from embodied technical change or vintage effects.

An alternative method that is popular in longitudinal studies of savings and consumption is to restrict the year effects to cyclical fluctuations only, by restricting them to average to zero over all periods and be orthogonal to a time trend (the orthogonality restriction, for short).[10] The imposition of cyclicality on the year effects is not really prior information, but it acts as such; it is in fact a decision by the researcher to attribute the variation in savings or earnings to cohorts and age instead of time, which is fine when the objective is a decomposition that reveals how consumption and savings change over the life cycle and between generations (Deaton and Paxson 1994; Paxson 1996; Deaton 1997; Attanasio 1998; Parker 1999). As Deaton (1997, 126) warns, this procedure requires a sufficient number of surveys (long enough time series) for the separation of the trend from the transitory shocks to be carried out with confidence. In addition, identification of the year effects requires sufficient variation in the variable of interest over time to make the estimation possible (Glenn 2005). Both conditions are met in our case, especially with earnings that fluctuated more than expenditures, which is why in Marku and Salehi-Isfahani (2006), which used the Deaton method, we were able to obtain stable results. Here, we use orthogonality not for identification but to see how the cohort and age effects are affected if the trend is not absorbed by the year

[9] In the context of decomposition, which is our intent here, identification is a rule for attribution of change to age, cohort, and year effects, rather than to help isolate a behavior relation.

[10] This results in the following normalization: $s_y'\psi = 0$. Subject to this normalization, we can estimate eq. (2) by regressing y on dummies for each cohort excluding the first, dummies for each age excluding the first, and a set of 19 year dummies defined as follows for $t = 3, \ldots, 21$ (the first- and second-year dummies are dropped to achieve identification). This restriction is implemented via the following relation among the year dummies (Deaton 1997): $d_t^* = d_t - (t - 1)d_2 + (t - 2)d_1$, where d_t is the usual year dummy equal to 1 if the year is t and 0 otherwise. This procedure enforces the restriction in the equation, as well as the restriction that dummies must add up to zero. Through the regression, we obtain estimates of the year effects for 1986–2004. The year effects for 1984 and 1985 can be recovered by the fact that all year effects add to zero and satisfy the equation. Additionally, to achieve identification, a second year dummy is set to zero.

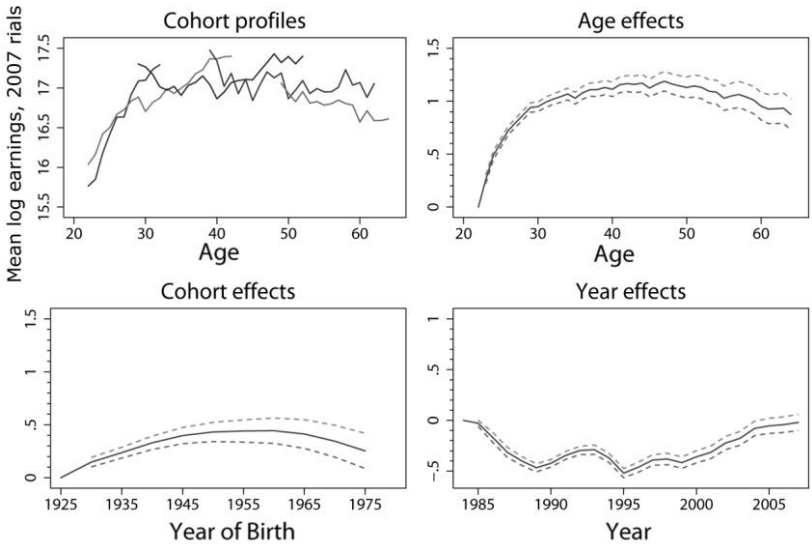

Figure 4. Decomposition of real individual earnings of men. Source: Authors' calculations based on Household Expenditures and Income Surveys, 1984–2007.

effects. The results show that, in the case of earnings, we do not observe much change from imposing orthogonality, but, in the case of per capita expenditures, the results do change significantly.

V. Decomposition of Individual Earnings of Men

We define earnings as net wage and salary income plus net income from self-employment, as do Beaudry and Green (2000). We use the consumer price index published by the Central Bank of Iran for urban areas and by the Statistical Center of Iran for rural areas to measure all earnings in 2007 prices.

We present the decomposition results for men's earnings in a set of four graphs in figure 4 and in table 4, column 1. The coefficients are generally significant at 1% and 5% levels.[11] The graphs show the 95% confidence bands for the estimated coefficients. In the top-left-corner graph, we reproduce figure 3, which shows the actual cohort profiles that are being decomposed into age, year, and cohort effects. The fluctuations in earnings due to macroeconomic shocks, especially the economic collapse of the 1980s, are quite visible in these earning profiles. These fluctuations, which affected nearly all cohorts but at different ages, are picked up by the year effects. The cohort and age effects are more difficult to see in the raw data, which is why we need the decomposed results depicted in the other graphs. The three other graphs depict the co-

[11] The detailed estimation results for these graphs are in table A2.

efficients of the regression equation (2) and measure percent change relative to a reference group. For example, the age effects in figure 4 show the increase in earnings by age relative to 22-year-olds, which is the reference category. The reference group for the cohort effects is the 1925–29 cohort, and for the year effects it is 1984.

The age effects indicate that men's earnings rise sharply until age 30 and peak at about age 47 before declining gently. This is in contrast to the age profiles estimated for Taiwan (Deaton 1997, 118) and Indonesia (Skoufias and Suryahadi 2002), wherein incomes rise continuously even past age 50. According to our estimates, in Iran between age 22 and 30 median earnings rise by about 110% but are relatively flat thereafter. This pattern is consistent with the two cohorts that we observe in the surveys before age 30, the 1970–74 and 1975–79 cohorts; their actual earnings in figure 3 are seen to rise sharply before age 30. It is also consistent with the general facts about the labor market experiences of young and older workers in Iran. Workers under 30 face high rates and long durations of unemployment, whereas above age 30 unemployment is low (Salehi-Isfahani and Egel 2009). Flat earnings beyond age 30 are consistent with the fact that in Iran compensation is more closely linked to degrees, which do not change with age after some point, rather than actual skills, which can rise with work experience.

The year effects shown in figure 4 reproduce closely Iran's macroeconomic fluctuations, which we saw in figure 1: the sharp drop in incomes during 1984–89, the rise in oil prices in 1990–91, the imports compression shock of the mid-1990s, and the oil boom of 2000–2007 are all reproduced here. The estimated cohort effects, which are of greatest interest to us, are also shown in figure 4. The cohort effects are increasing for all generations born before 1950, who were mature adults and well into their careers (30 years and older) at the time of the revolution in 1979. For the cohorts born around 1950, lifetime earnings were about 44% higher than the generation born during 1925–29. The cohort effects are flat for the 1950–64 cohorts, who were young at the time of the 1979 revolution (15–29 years old). The most striking result is the decline in lifetime earnings of the youngest cohorts, who were born after the mid-1960s and reached labor market entry age after the revolution. According to these results, the cohorts born in the early 1970s experienced a decline in lifetime earnings of about 10% (significant at the 5% level) relative to those born a decade earlier, and the cohorts born in the late 1970s suffered a 19% drop. The latter had the same lifetime earnings as those born 40 years earlier (see table 4). Thus, in terms of earnings, the revolution generation appears to have fared even worse than its parents born 20–30 years earlier.

TABLE 4

REGRESSION RESULTS: DEPENDENT VARIABLE = LOG COHORT EARNINGS

	Men's Earnings		Expenditures	
	Unrestricted	Orthogonal	Unrestricted	Orthogonal
	(1)	(2)	(3)	(4)
Age:				
23	.262***	.266***	.003	.018
24	.488***	.495***	.026*	.052***
25	.604***	.617***	−.008	.038**
26	.722***	.741***	.014	.083***
27	.797***	.821***	−.004	.082***
28	.868***	.897***	−.014	.087***
29	.940***	.972***	−.004	.109***
30	.948***	.984***	−.052***	.080***
31	.989***	1.031***	−.036*	.117***
32	1.018***	1.065***	−.033*	.138***
33	1.041***	1.094***	−.036*	.153***
34	1.068***	1.124***	−.004	.196***
35	1.028***	1.088***	−.073***	.145***
36	1.079***	1.145***	−.027	.213***
37	1.109***	1.180***	−.007	.251***
38	1.109***	1.186***	−.005	.272***
39	1.129***	1.209***	.023	.311***
40	1.112***	1.197***	−.006	.299***
41	1.155***	1.246***	.056*	.385***
42	1.166***	1.262***	.075**	.421***
43	1.159***	1.260***	.076**	.441***
44	1.171***	1.275***	.089***	.464***
45	1.124***	1.232***	.048	.441***
46	1.161***	1.276***	.111***	.526***
47	1.189***	1.308***	.118***	.551***
48	1.163***	1.287***	.124***	.575***
49	1.145***	1.272***	.135***	.596***
50	1.129***	1.262***	.112***	.592***
51	1.143***	1.282***	.139***	.642***
52	1.131***	1.274***	.135***	.654***
53	1.094***	1.242***	.149***	.686***
54	1.085***	1.236***	.144***	.692***
55	1.028***	1.184***	.091**	.657***
56	1.049***	1.212***	.120**	.710***
57	1.063***	1.231***	.153***	.760***
58	1.040***	1.212***	.104**	.728***
59	1.011***	1.186***	.106**	.741***
60	.953***	1.133***	.108**	.761***
61	.926***	1.113***	.086	.762***
62	.928***	1.120***	.085	.780***
63	.934***	1.131***	.105*	.820***
64	.875***	1.076***	.110*	.836***
Cohort:				
1930	.147***	.172***	.090***	.180***
1935	.236***	.285***	.131***	.308***
1940	.330***	.403***	.210***	.477***
1945	.398***	.495***	.284***	.638***
1950	.432***	.554***	.322***	.764***
1955	.441***	.586***	.310***	.837***

TABLE 4 (*Continued*)

	Men's Earnings		Expenditures	
	Unrestricted	Orthogonal	Unrestricted	Orthogonal
	(1)	(2)	(3)	(4)
1960	.444***	.613***	.297***	.910***
1965	.413***	.607***	.301***	1.005***
1970	.345***	.563***	.280***	1.069***
1975	.253***	.495***	.272***	1.149***
Year:				
1984	.000	.330	.000	.195
1985	−.028*	.295	−.017	.295
1986	−.168***	.158***	−.188***	.061***
1987	−.314***	.006	−.249***	−.022
1988	−.400***	−.088***	−.297***	−.096***
1989	−.467***	−.161***	−.288***	−.109***
1990	−.424***	−.124***	−.182***	−.028***
1991	−.347***	−.052***	−.131***	.008
1992	−.297***	−.007	−.133***	−.014
1993	−.289***	−.005	−.140***	−.039***
1994	−.371***	−.091***	−.167***	−.084***
1995	−.520***	−.246***	−.204***	−.141***
1996	−.462***	−.193***	−.197***	−.149***
1997	−.391***	−.126***	−.167***	−.140***
1998	−.381***	−.122***	−.101***	−.092***
1999	−.417***	−.162***	−.063***	−.071***
2000	−.360***	−.110***	−.025	−.052***
2001	−.315***	−.069***	−.008	−.049***
2002	−.224***	.017*	.061**	.002
2003	−.177***	.058***	.111***	.032***
2004	−.079**	.151***	.226***	.131***
2005	−.054	.170***	.252***	.135***
2006	−.042	.178***	.230***	.098***
2007	−.021	.195***	.282***	.131***
Constant	15.882***	15.386***	16.591***	15.726***
N	996	996	996	996
Adjusted R^2	.956	.956	.919	.902

Note. Expenditures are per adult equivalent. The year effects are restricted to be orthogonal to a time trend and sum to zero. For standard errors of the earnings equation, see table A2.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

Next, we impose the orthogonality restrictions on the year effects to see the extent to which the results are sensitive to how the time trend is treated in the decomposition. We continue to use 5-year cohorts to keep the two sets of results comparable but require the coefficients of the year dummies to be orthogonal to a time trend and to sum to zero (see n. 10 for the details of how this is done). As discussed in Section IV, imposing the orthogonality constraint requires dropping one more year dummy. Which other dummy is dropped should matter because it in fact sets the year effect equal to the reference year. This is a main problem with the method proposed by Deaton

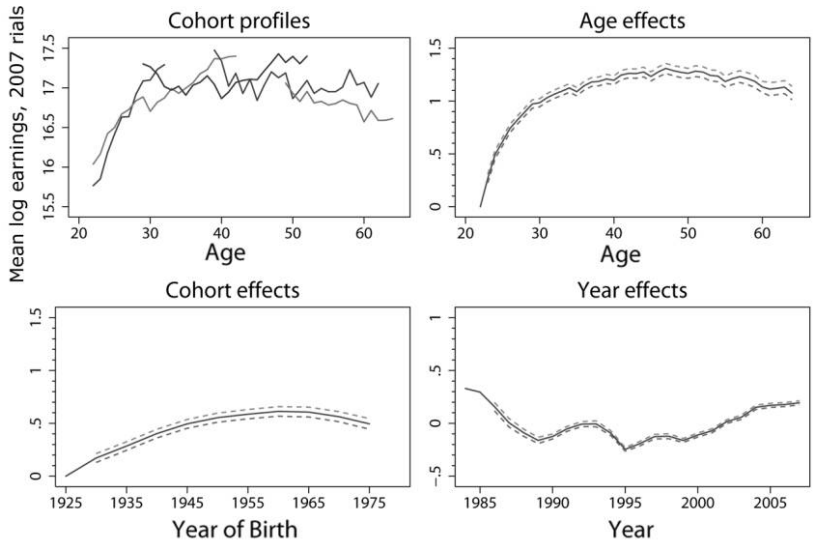

Figure 5. Decomposition of real individual earnings of men, orthogonal year effects. Source: Authors' calculations based on Household Expenditures and Income Surveys, 1984–2007.

and Paxson (1994), so we tried dropping different year dummies in turn and, not surprisingly, noticed that results are sensitive to the choice of normalization. In figure 5 and table 4, column 2, we report the results when we drop the second year dummy, as recommended by Deaton (1997). The estimated cohort and year effects are similar but not the same: the decline in the cohort effects between those born in the 1960s and 1970s is now 13% instead of 19% in the unrestricted case, and the age effects, which are very similar in shape, are 5%–10% higher for middle-age men relative to young men. A representative result of the sensitivity analysis on year dummies is dropping the last year dummy, 2007. This reduces the estimated difference between the cohort effects of the 1960–64 and 1975–79 cohorts to 12% instead of 19% and is closer to the unrestricted case.

Similar declines in cohort earnings have been documented for developed countries, but mostly for a particular skill group and in relation to shifts in demand for certain types of skills. For example, Beaudry and Green (2000) show that cohort-specific age-earnings profiles for Canadian men have shifted down for successive cohorts of high-school-educated men since the 1978 entry cohort and from even an earlier point for university-educated men. They report a 20% decline in earnings for those entering the labor market in 1992 compared to cohorts who entered 20 years earlier, which is similar in magnitude to what we find here. For the United Kingdom, Gosling et al. (2000) find a

rising pattern, as do Deaton and Paxson (1994) and Skoufias and Suryahadi (2002) for a few developing countries.

VI. Decomposition of Household Expenditures

Changes in individual earnings are important to understand, but they do not fully reflect changes in welfare for at least two reasons. Individuals may benefit from being part of a household and share in its resources, and they may benefit from transfers from the government to households in the form of subsidies and in-kind payments. In Iran, there is evidence of significant consumption smoothing through intergenerational transfers within households. A high and rising proportion of young people live with their parents well into their 20s. This is probably related to their lower earnings as well as their inability to find a steady job, both of which seem preconditions for forming a household (Salehi-Isfahani and Egel 2009). During 2000–2007, about 40% of men age 25–29 lived with their parents, and 23% were heads of households. These numbers are in stark contrast to numbers from the 1980s, when less than 20% of the age group lived with their parents and 33% were household heads (Salehi-Isfahani and Egel 2009). In 2007, unemployment rates for individuals 30 years and older were less than 5%, compared to 25% for those age 20–24. Because individuals must work before they can collect unemployment benefits, first-time job seekers are not eligible for unemployment insurance, and their families are their only source of support. Government transfers also play a significant role in boosting individual welfare in Iran. The government spends a hefty 10% of the GDP on subsidies for food, fuel, and medicine. Our calculations from the HEIS data files reveal that, in 2004, nonmonetary transfers boosted incomes by about 31%, and commodity subsidies boosted private expenditures by about 22%.

To account for the effect of resource pooling within the household, as well as transfers from the government, in this section we use household-level per capita expenditures instead of individual earnings to measure changes in lifetime welfare. For comparability, we restrict the sample to men 22–64 years old with earnings, exactly as in the earnings decomposition. They can be heads of households or live in the household. We use per-adult-equivalent expenditures of the household, which is obtained by dividing household expenditures by the square root of the household size. Previous works in the literature have defined cohorts on the basis of the age of the household head (see Deaton and Paxson 1994 and McKenzie 2006b). However, such a choice introduces a potential bias if, plausibly, the young adults who live with their parents and are therefore assigned the age of the household head in the decomposition

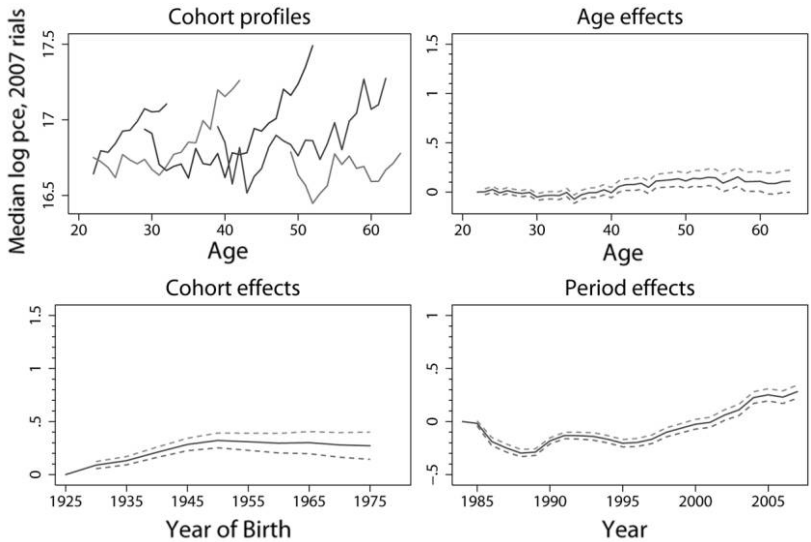

Figure 6. Decomposition of real per-adult-equivalent expenditures of men. Source: Authors' calculations based on Household Expenditures and Income Surveys, 1984–2004.

analysis happen to be less well off. The results of the decompositions using either basis for defining age are remarkably the same.

As we did with earnings, we present the results of the decomposition of per-adult-equivalent expenditures using 5-year cohorts with and without the orthogonality assumption (see figs. 6 and 7 and table 4, cols. 3–4). The contrast between the two sets of results is quite sharp. The results in the unrestricted case are remarkable in their relatively flat age and cohort effects and rising year effects. The unrealistically positive rising year effects naturally affect the trends in the age and cohort effects and make them more flat than they would be otherwise. The results in figure 7, in which the year effects are restricted to be orthogonal to a time trend, seem more realistic in the pattern of the year effects. They are more flat, giving rise to monotonically increasing age and cohort effects.

The age profiles in the restricted case are similar to the age profiles of consumption reported in Deaton (1997, 118) for Taiwan, in that they do not display the inverted U shape we observed for individual earnings. The rising age profiles observed in these graphs may reflect the fact that household resources increase with age, perhaps because later in life children leave the household, thus raising per capita expenditures, and some who stay work and add to household resources. In 2004, the smallest households with 2.45 mem-

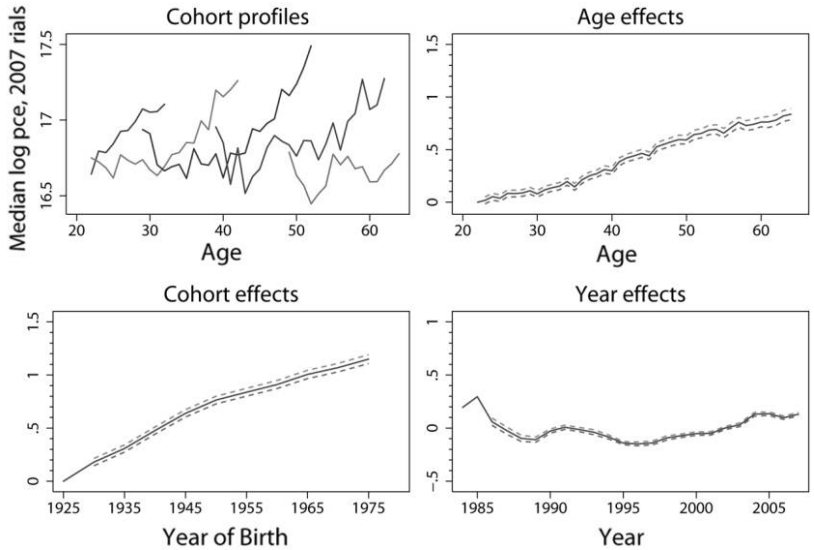

Figure 7. Decomposition of real per-adult-equivalent expenditures of men, orthogonal year effects. Source: Authors' calculations based on Household Expenditures and Income Surveys, 1984–2007.

bers were those headed by 20-year-olds, the largest with 5.61 members were headed by 49-year-olds, and the oldest households had 4.4 members.

The key difference here is obviously in the restrictions on the year effects. When the year effects are allowed to have a trend, they tend to absorb the trend in the original individual cohort data. The reason why the restricted and unrestricted results are similar in the case of earnings may be due to the fact that earnings data do not have a strong trend between cohorts, whereas expenditures data do (seen in how the lines stack against one another in the upper-left graph of the respective figures). Thus, restricting the year effects may not have a big influence in the case of earnings, but it has in the case of expenditures.

Which set of results is to be believed depends in part on the question being asked. If the objective is to compare the relative position of the age-earnings or age-consumption profiles of different cohorts, then restricting the year effects offers some insight. This is because the year effects do not have a welfare interpretation on their own. If we want "good years" to mean that the people who lived in them are better off, then it makes sense to restrict the year effects to cyclical variation only and let the age and cohort effects absorb all the improvement. In the case of savings and consumption, where the interpretation of the year effects as cyclical change is more appealing, imposing the no-trend

restriction seems warranted. However, one should bear in mind that this insight comes at the cost of having to choose without any theoretical guidance which other year dummy to drop. But if the information we elicit from the data about the year effects is important in and of itself, the unrestricted regression offers results that are more transparent. This is the case with earnings because the year effects have a useful interpretation, for example, the labor market conditions, and the cohort effects would then measure the change in the human capital endowments of various cohorts. We believe presenting the results in both cases helps the reader make a choice as to what the data are actually saying.

One thing that both sets of results share is the larger decline in the fortunes of the younger cohorts in earnings compared to consumption. The difference is much more pronounced when the year effects in both decompositions are restricted, but the fact that there is a difference irrespective of method of estimation prompts the question of why the younger cohorts are doing better in consumption than earnings. One plausible explanation, as noted earlier in this section, is that the lost ground in terms of earnings has been partly recovered by increased transfers from the government to families and increased transfers from the older to the younger generation within the household. Interestingly, starting with cohorts born in the early 1950s, we notice a change in the slope of the linear trend in the cohort effects for both income and expenditures, which corresponds to the time when cohort effects of earnings stop rising. This may be an indication that transfers did not fully compensate for the falling earnings.

We do not have information on the level of subsidies that may have influenced how the real value of household expenditures per person has changed over time. But we can learn something about income pooling within the family from our data. Are individual earnings being increasingly supplemented by other sources of income? To answer this question, we compare the individual earnings and per-adult-equivalent income of two 5-year cohorts in 2004, those 40–44 years old (born 1960–64) and those 30–34 years old (born 1970–74), for whom we estimate lower lifetime earnings but constant or higher per-adult-equivalent household expenditures. What we find corroborates our conjecture that income pooling within the household may be responsible for the divergence between the two sets of results. Average individual earnings for the older cohorts is 32.7% higher than the younger cohort, while their per-adult-equivalent household income is only 6.5% higher. On the basis of the decomposition framework that we have presented in this article, the difference in earnings and per-adult-equivalent income between these two cohorts can be explained by the period effect and the cohort effect (the year effect in this

example is zero because we are examining two cohorts in the same year). Thus, to compare the two cohorts, we must first make an adjustment for the age effects by raising the average earnings and per-adult-equivalent income for the younger cohort by their respective estimated average age effects from the decompositions (about 16%). With this adjustment, in 2004 the younger cohort earned 16% less than the older cohort but lived in households with about 9.5% more per-adult-equivalent income. The reason for this difference is, at least in part, due to the contribution of younger cohorts to lower-income households. On average, in 2004, earnings of those in their 20s contributed 10.8% to total household income, compared to 17.3% for those in their 30s. In other words, the younger cohorts benefited more from income pooling inside the household.

VII. Conclusion

In this article, we examine changes in lifetime earnings and consumption for Iranian men who have lived through the last 3 tumultuous decades. We find evidence that all cohorts born before the mid-1950s, who were at least in their mid-20s at the time of the revolution in 1979, experienced gains in lifetime earnings relative to older cohorts. But the younger cohorts born after 1960, who were in their late teens or early 20s in 1979—the revolution generation—appear to have lost ground in terms of lifetime earnings relative to their predecessors.

Swings in cohort earnings, even cohort losses, are not unusual, although it is rare to find a reversal of fortunes as dramatic as what we observe in the case of Iran. Several explanations are encountered in the literature on determinants of earnings. Easterlin (1987) emphasizes cohort size, but in the case of Iran this factor is probably dwarfed by other large shocks. Moreover, growing cohort size has been a factor for cohorts born before 1950 whose lifetime earnings rose. Another explanation emphasizes the conditions of the labor market at the time of a cohort's entry. Behrman and Birdsall (1988) identify such a cohort effect for returns to education for Brazil. The idea that labor market conditions during the early years of a person's career are critical for his or her human capital accumulation is very intuitive. Much is learned on the job, and long periods of unemployment can quickly depreciate a young person's skills. This explanation fits better with the facts concerning Iran. The revolution and the war that ensued were very costly not only in terms of lives but also in human capital. Iranian universities, which provide the main incentive for schooling, were closed for 2 full academic years during 1980–82, while the academy was being purged of non-Islamic elements. In addition, the war diverted millions of young people away from attending high schools

and universities to the war front. Finally, disruptions in employment relations after the revolution (Bayat 1987; Nomani and Behdad 2006) reduced the incentives and the effectiveness of on-the-job learning. Significantly, average years of schooling for successive cohorts has been rising consistently during the entire period under consideration (Salehi-Isfahani 2005), which indicates that rising education was not sufficient to compensate for declining earnings.

We also analyze changes in household resources over time to learn how cohort welfare changed as cohort earnings declined. We find that, assuming equal access to family resources, contrary to the results for earnings, the cohort effects of per-adult-equivalent household expenditures of the younger cohorts stayed the same or increased. We offer an explanation to reconcile the two sets of results by noting that transfers from the older generation to the new, and from government to households, have helped to not only maintain lifetime consumption but also increase it. In light of this finding, and the fact that average cross-section incomes in Iran have recovered from their postrevolution collapse, the nostalgia that many Iranians feel for prerevolution times and the strength of the widely held view of general economic decline in Iran may seem surprising (Salehi-Isfahani 2009). One possible explanation for the persistence of economic discontent among the generation of Iranians who came of age at the time of the revolution—the youngest cohorts in our study, now in their 50s—is that despite lower lifetime earnings, many have to support their own children, who are unable to find a job and therefore have to depend on their parents for support (these cohorts are just starting to enter the pseudopanel so they are not included in this study). Another explanation is that, to the extent that a person's self-worth is measured by his or her own earnings, the revolution generation may still feel worse off compared to their parents.

Furthermore, as we have shown, for a long succession of Iranian cohorts, lifetime earnings were on the increase, generating the sort of optimism that comes from seeing each generation do better than the one before, leading them to believe that the same will happen to them and to their children. The revolutionary upheavals, the war, and the largest oil price collapse in history appear to have reversed this trend and dashed many hopes. The cohort evidence shows that the young revolutionaries in their late teens and early 20s, who were full of revolutionary optimism around 1979, were precisely those who were hit the hardest by the reversal of fortunes. For most people in growing economies who find themselves in the losing end of comparisons with peers, a consoling fact might be their advantage over their parents. This is not true for Iran's revolution generation. To gauge the extent of their disappointment, imagine a trend line that projects forward the rising cohort effects of the early decades, placing the lifetime earnings of this generation about 30% higher

than their parents. Instead, because of the falling cohort effects, their actual lifetime incomes have turned out to be some 20% lower, which is about half of what they might have expected. Viewed in this light, it may be easier to understand why, despite the oil boom, many Iranians continue to express pessimism about their economy.

Appendix

Description of the Surveys

The HEIS has maintained a consistent structure since its inception in 1963, although it has changed in the amount of detailed information it collects. The HEIS is the largest survey conducted by the Statistical Center of Iran. The questionnaires consist of more than 1,000 items organized into modules. The first is the demographic module, which reports on age, sex, marital status, relationship to the head of the household, education, and employment status. Module 2 contains information on household ownership of assets and amenities. Module 3 records very detailed information on food expenditures, which can be aggregated into broader groups such as grains, meats, dairy, and so on. Module 4 reports on nonfood expenditures, including nondurables and semidurables such as clothing, household items, rent, and utilities. The recall period for these expenditures is the last month. Module 5 records expenditures on durables, which include appliances, furniture, vehicles, bikes, as well as expenditures on vacation travel, school tuition, or housing extension. Modules 6, 7, and 8 record information on individual wage and salary income, self-employment income, and other income from retirement, rent, or other sources.

The HEIS reports expenditures rather than consumption. All goods acquired through purchase, home production, or transfers are included (except for in-kind transfers between households, which are not reported for most years). All expenditures data are collected on recall. For 1984–89, the recall period for food expenditures was the last 48 hours in urban areas and the last 24 hours or last month (depending on the item) for rural areas. Starting in 1990, the recall period on food expenditures has been the last month for both urban and rural areas. The recall period for nonfood expenditures has remained the same throughout the time period under consideration.

The HEIS follows a two-stage stratified sampling method that has remained the same over time. The most recent census of the population serves as the frame from which, in the first stage, the requisite number of blocks is randomly selected, and, in the second stage, five households are selected from each block. The sample is stratified by urban and rural locations, as well as by province. The number of blocks (or observations) for each geographic unit (rural or urban areas of each province) is determined by taking into account the precision

requirements for estimation of certain indexes (such as food expenditures). The number of blocks in each unit is simply the total number of households divided by five.

TABLE A1
SAMPLE SIZES FOR HOUSEHOLDS AND INDIVIDUALS

Year	Households	Individuals (20–64)	Earners
1984	27,148	28,130	25,169
1985	27,260	27,955	25,079
1986	5,760	5,990	5,284
1987	5,766	5,986	5,327
1988	8,318	8,535	7,690
1989	11,520	12,276	10,641
1990	18,430	19,955	19,331
1991	18,661	19,900	19,392
1992	18,653	20,328	19,779
1993	12,763	14,142	13,808
1994	19,904	22,036	21,480
1995	36,572	41,308	39,449
1996	21,963	25,150	24,508
1997	21,949	25,270	24,698
1998	17,477	20,060	19,676
1999	27,464	31,735	31,089
2000	26,941	30,724	30,178
2001	26,961	30,650	30,177
2002	32,152	36,302	35,813
2003	23,134	27,427	27,082
2004	24,534	29,460	29,046
2005	26,895	32,340	31,869
2006	30,910	36,898	36,323
2007	31,283	36,732	36,142
Total	522,418	589,289	569,030

TABLE A2

DECOMPOSITION REGRESSION OF EARNINGS OF MEN: DEPENDENT VARIABLE = LOG COHORT EARNINGS

Age Dummies			Cohort Dummies			Year Dummies		
Age	Coefficient	SE	Cohort	Coefficient	SE	Year	Coefficient	SE
23	.262	.021	1930	.147	.023	1985	−.028	.015
24	.488	.021	1935	.236	.026	1986	−.168	.025
25	.604	.020	1940	.330	.032	1987	−.314	.025
26	.722	.021	1945	.398	.039	1988	−.400	.022
27	.797	.021	1950	.432	.046	1989	−.467	.021
28	.868	.022	1955	.441	.054	1990	−.424	.019
29	.940	.023	1960	.444	.061	1991	−.347	.020
30	.948	.023	1965	.413	.069	1992	−.297	.021
31	.989	.025	1970	.345	.077	1993	−.289	.024
32	1.018	.025	1975	.253	.085	1994	−.371	.023
33	1.041	.027				1995	−.520	.023
34	1.068	.028				1996	−.462	.025
35	1.028	.028				1997	−.391	.027
36	1.079	.030				1998	−.381	.028
37	1.109	.032				1999	−.417	.029
38	1.109	.033				2000	−.360	.030
39	1.129	.035				2001	−.315	.032
40	1.112	.035				2002	−.224	.033
41	1.155	.038				2003	−.177	.035
42	1.166	.039				2004	−.079	.036
43	1.159	.040				2005	−.054	.038
44	1.171	.042				2006	−.042	.039
45	1.124	.042				2007	−.021	.041
46	1.161	.045						
47	1.189	.047						
48	1.163	.048						
49	1.145	.050						
50	1.129	.050						
51	1.143	.053						
52	1.131	.054						
53	1.094	.056						
54	1.085	.057						
55	1.028	.058						
56	1.049	.061						
57	1.063	.063						
58	1.040	.064						
59	1.011	.066						
60	.953	.066						
61	.926	.070						
62	.928	.071						
63	.934	.073						
64	.875	.074						

Note. $N = 996$; $R^2 = 0.96$.

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