Are Resource Booms a Blessing or a Curse? Evidence from People (not Places)

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Abstract

We provide the first estimates of the long-run income effects of temporary resource booms on people, rather than places, focusing on the U.S. oil boom and bust of the 1980s. Using household-level longitudinal data, we find positive effects during the boom period and negative effects during the bust period. The cumulative effect through 2012 was arguably negative when restricting the sample to prime working years (<55) and unambiguously positive otherwise because the boom delayed retirement. The evidence suggests the boom was ultimately a curse for the average household. It failed to generate net income gains during prime age and its volatility caused costly income-smoothing later in life.

Keywords: resource curse, resource boom, oil, volatility, retirement
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1 Introduction

Will temporary resource booms benefit residents of resource-endowed communities in the long run? This question is of long-standing interest to social scientists who have sought to determine if natural resources are a blessing or a curse (van der Ploeg, 2011), yet the literature has only partially addressed it by studying data aggregated for places rather than following data on individuals over time.[1] U.S. studies, for example, have tracked county economies before, during, and after oil, gas, and coal booms. Findings from a large literature indicate that economies have benefited during the boom period, experiencing short-run surges in employment, wages, and earnings despite uneven responses across sectors.[2] Findings from a smaller literature suggest that short-run gains have not, in general, been exceeded by losses from resource busts although recession depths and durations have varied across regions.[3]

The literature's focus on places (rather than people), while useful,[4] can potentially mislead policy decisions for two reasons. First, place-based analyses can confound the effects of booms on residents with compositional changes driven by migrants who moved for boomtown employment. If migrants earn higher (lower) incomes than residents, then place-level measures will overstate (understate) the benefits to local residents. Migration is espe-

[1]With respect to the vastness of the literature, the phrase "resource curse" appeared in about 1,500 published articles in 2018 (see https://app.dimensions.ai/discover/publication). Recent reviews of the literature are provided by Deacon (2011), van der Ploeg (2011), Aragón et al. (2015), Mason et al. (2015), Venables (2016), Marchand and Weber (2017), van der Ploeg and Poelhekke (2017), and Jacobsen (2019a).

[2]For example, there is almost unanimous agreement that the recent hydraulic fracturing boom has boosted overall short-run employment and earnings in counties near shale plays (Weber, 2012; Hausman and Kellogg, 2015; Bartik et al., 2017; Brown et al. 2016; Feyrer et al., 2017; Maniloff and Mastromonaco, 2017; Jacobsen, 2019b; James and Smith, 2019). However, there is some evidence that manufacturing and agriculture have been harmed (Feyrer et al., 2017, Farah 2019). Studies examining booms other than the fracking boom document positive or neutral short-run effects in other settings (Carrington, 1996; Marchand, 2012; Caselli and Michaels, 2013; and Aragón and Rud, 2013).

[3]For coal-mining Appalachian counties during 1970-1989, Black et al. (2005b) conclude that employment conditions were worse after the boom-and-bust cycle than before the boom. Similarly, for oil-rich counties in the western U.S. during 1969-1998, Jacobsen and Parker (2016) estimate that employment conditions and income per capita were below what they would have been if the boom had not occurred. James and Aadland (2011) find persistent yet waning negative effects from 1980-2005 in case studies of resource booms in Maine and Wyoming counties. However, other studies over longer time periods have not found evidence of lasting negative effects (Michaels, 2011; Allcott and Keniston, 2017; Clay and Portnkyh, 2018).

[4]Place-level analyses are especially useful for evaluations of outcomes that are not sensible to measure at the individual or household-level, such as analyses of the effect of natural resources on government spending and revenue (e.g., James, 2015).

cially relevant for resource booms because booming areas often experience rapid population changes.[5] Second, place-level analyses have not accounted for the actions individuals take to smooth income under volatile and uncertain boom-and-bust conditions. For example, if some boomtown residents can maintain consumption during and after the bust only by delaying retirement, then place-level income measures may falsely exaggerate the long-run net benefits of the boom-and-bust cycle by masking these costly coping mechanisms.[6] Examining individual-level responses to boom-induced volatility is important because place-based evidence has indicated that, "volatility is a quintessential feature of the resource curse" (van der Ploeg and Poelhekke, 2009, 727).

We address these shortcomings in the literature by using the 1970s/1980s U.S. oil boom to i) estimate the effects of temporary resource booms on household incomes over most of a life-cycle and ii) examine how experiencing a resource boom during one's prime working years affects retirement timing later in life.[7] We do so by studying restricted data on households from the Panel Study of Income Dynamics (PSID). Using the PSID allows us to conduct a longitudinal, national study of households in and outside of boom locations and follow their outcomes through 2012, long after the bust of the late 1980s. We focus on this boom-and-bust cycle because of its exogeneity - neither its onset nor its collapse was anticipated (Baumeister and Kilian, 2016) - and because it is recent enough to have been documented by a high-quality data series yet old enough to allow for an examination of its long-run effects. To our knowledge, this study is the first to estimate the short and long-term

[5]Wilson (2018), for example, documents the large effects of the fracking boom on U.S. migration. A resource boom might also drive changes in fertility and mortality patterns, which can also lead to compositional changes, especially over the long-term (see, e.g., Kearney and Wilson, 2018).

[6]Existing research indicates that sharp economic downturns, such as those that occur during a resource bust, have delayed retirement in other cases. While macroeconomic recessions have theoretically ambiguous effects on retirement timing (see Coile and Levine, 2007), empirical evidence indicates that individuals who stayed employed during the Great Recession (beginning in 2008) adjusted upwards their planned retirement ages, presumably to offset stock and housing value losses and to make up for increased consumption occurring prior to 2008 (Goda et al., 2011, McFall, 2011, Begley and Chan, 2018). This is relevant because exposure to energy booms increases consumption and debt during the boom years, leaving individuals exposed to the uncertain timing of resource busts (Brown, 2018), and because the 1980s oil bust we study sharply decreased housing values. Newspaper accounts, for example, show steep declines in housing prices in oil-rich Texas and Oklahoma during the 1980s and 1990s (see Klein, 2016).

[7]Throughout this paper we sometimes refer to the long run or cumulative effects of "booms" as short-hand for the effects of the full boom-and-bust cycle. To avoid confusion, we explicitly write "boom years" or "boom period" when we are referring to the period of elevated extraction.

effects of resource booms using data on people, not places.[8]

Our empirical approach is as follows. We divide the available time periods in the PSID into different groupings based on trends in oil prices and sectoral employment: pre-boom (1969-1974), early boom (1975-1979), late boom (1980-1984), early bust (1985-1988), late-bust (1989-1992), post-bust (1993-1999), and millennium (2000-2012). Next, we create a "boom household head" indicator variable for household heads present in a county with a large share of oil sector employment during 1980, the height of the boom. We interact this boom indicator with the time periods and, using household fixed effects, measure the differential responses of households who experienced the boom over time relative to counterfactual households who did not. Following precedent in the resource curse literature, the key outcome that we focus on is household income.[9]

To evaluate the boom-and-bust based on exogenous changes in economic opportunities, rather than endogenous household-level choices related to retirement, we focus on instrumental variables (IV) estimates from a sample that excludes households once the head becomes 55 years old, which corresponds to the end of their "prime" working years.[10] The IV estimates instrument for 1980 location with a household head's location in 1973, which pre-dates the beginning of the boom. This accounts for endogenous in-migration, allowing us to estimate the effect of the boom on households whose residency pre-dated the beginning of the boom.

The key results from our preferred specifications indicate the boom-and-bust was arguably a curse for the average resident household. The boom period increased annual

[8]Winters et al. (2019) estimate positive short-term effects from the fracking boom using individual-level data, but do not look at long-term effects because the boom happened too recently to allow for such an examination. With respect to the long-term effects of other types of economic booms, related studies have looked at some of the long-term effects of a housing boom and bust (Charles et al., 2018) and a technology boom and bust (Hombert and Matray, 2019).

[9]While effects on income may not translate directly into effects on welfare, the resource curse literature has historically evaluated per capita income as a bottom line measure of whether a region suffers from the "resource curse." Seminal work by Sachs and Warner (1995) focuses on income per capita, as do most country-level studies that followed thereafter (as discussed in Deacon, 2011; van der Ploeg, 2011). Within-country studies have also assessed the curse using income per capita and this is the convention used in papers that study the curse within the United States (e.g., Papyrakis and Gerlagh, 2007; James and Aadland, 2011; Clay and Portnykh, 2018). Booms may have impacts on welfare through non-income related channels. For example, drilling can lead to substantial environmental degradation (Boomhower, 2019).

[10]The Bureau of Labor Statistics typically describes age 55 as the end of prime working years (USBLS, 2015; USBLS, 2018).

income flows by approximately $5,000 during the early boom and $6,900 during the late boom.[11,12] The bust decreased annual incomes by about $8,000 during the early bust and $8,700 during the late bust, all relative to a no-boom-and-bust counterfactual. Incomes were not statistically different from counterfactual levels over 1993-2012. Aggregating across years using a zero discount rate (based on real interest rates in the early 1970s), the average effect on cumulative income gains and losses over 1975-1992 was negative, at about -$7,600 per household.

Estimates from non-preferred specifications highlight why researchers may be apt to conclude the episode was a blessing if they use placed-based data that does not distinguish between residents and migrants or between prime-aged and older workers. First, the cumulative effects of the boom-and-bust on income implied by our OLS estimates of prime-aged households, which do not instrument for pre-boom residency, are positive at about $13,400. The OLS estimates overstate the benefits to pre-existing local residents, apparently because migrants to boom counties experienced larger income gains.[13] Secondly, and more importantly (with respect to the magnitude of the difference in the estimates), the bust and post-bust period IV estimates are very different in the Under 55 sample when compared to the All Ages sample: estimates from the All Ages sample reveal no evidence of negative effects on income flows during or after the bust period. The estimates from the All Ages sample imply the boom-and-bust cycle raised cumulative income for the average household by about $65,300.

We reconcile the stark difference in prime-aged versus all-age results by examining retirement. Consistent with households endogenously adjusting to the boom-and-bust to smooth income volatility, the results indicate that household heads older than 54 were about 15 percentage points less likely to retire during the late bust period. We further validate the role of retirement in our income estimates by estimating the boom-and-bust effects using a sample that excludes observations with retired household heads, regardless of their age.

[11] In our analysis, income is measured as both the household head's income plus, if applicable, the spouse's income.

[12] All dollar amounts presented in this paper are indexed to the year 2018.

[13] This finding is related to Guettabi and James (2017), who study the effects of a recent oil boom on individuals in Alaska's North Slope. Using data that differentiates between place of work and place of residence, the authors find evidence that employment and wage gains were disproportionately captured by migrant workers, rather than by local residents.

Results mirror the results from the Under 55 sample. Excluding retirees, the cumulative effect of the boom-and-bust on average household income was even more negative than the cumulative effect implied by estimates from the Under 55 sample.

Overall, our evaluation of the household-level effects of the boom-and-bust leans toward concluding it was a curse for local residents. Although boom period income gains were large for the average working age household, these gains were offset by bust period losses such that the cumulative effect was close to zero, if not negative. Additionally, the boom-and-bust created income volatility that caused some households to undertake costly smoothing behavior in the form of delayed retirements.[14] The results highlight the importance of considering individual-level effects in research examining the economic implications of natural resource development because some costs to people, such as delayed retirement, can be "hidden" in analyses of aggregate data.

2 Data and Empirical Setting

To execute the analysis, we combine restricted data from the Panel Study of Income Dynamics (PSID) with county-level data on oil and gas employment. We use the PSID data to track outcomes for households and the county-level data to define households who did and did not experience the boom. In order to control for pre-boom differences across households, we limit the analysis to household heads who were in the PSID dataset during the pre-boom period. This restriction means that our analysis focuses on household heads who were typically in their mid-20s or older when the boom began.

2.1 Panel Study of Income Dynamics

The Panel Study of Income Dynamics (PSID) is a longitudinal survey that has been studied in economics, sociology, and other fields.[15] The survey began collecting data on the outcomes of approximately 4,500 families and their members in the late 1960s. Over time,

[14]Delayed retirement can be considered costly because retirement is associated with improved health and life satisfaction (Gorry et al., 2019).

[15]Recent PSID studies in economics include (Kniesner et al., 2012; Altonji et al., 2013; Hoynes et al., 2016; and Jackson et al., 2016).

the panel expanded because members of the original families married and gave birth and contracted because people died or were no longer tracked for idiosyncratic reasons.

PSID surveys were conducted annually through 1997, and biennially thereafter. The initial samples drew from the Survey Research Center (2930 interviews) and the Survey of Economic Opportunities (1872 interviews). The goal was to recruit participants from these samples to create a nationally representative sample, although there was a slight oversampling of poorer households in the earliest survey waves (Hill, 1991). Empirical studies have generally verified that the PSID sample has remained nationally representative over time. For example, Gousaka and Schoeni (2007) show the sample is comparable to the Current Population Survey and Lilliard and Panis (1998) find that bias due to attrition is minor.

During each survey wave, household heads were interviewed about the social and economic characteristics of their family unit. The data collection for household heads is by far the most comprehensive.[16] Key variables included in the PSID include total income, taxable income, wage rates, unemployment, and retirement.[17] We measure household income as the amount received by the household head and, if the household head is married, the income of the household head and spouse. Taxable income includes income from labor (wages and bonuses) and non-labor income from household-owned businesses and assets. Total income also includes retirement income (i.e., pensions and social security income) plus miscellaneous transfer income, such as unemployment compensation. Because social security income is not reported in PSID after 1992, our analysis of income over the full time period (through 2012) focuses on taxable income. We measure wage rates as the weighted average of the head and spouse's wage rate, where the weights account for hours worked. We measure unemployment and retirement status based on the household head's reported employment status.[18]

So that the sample is compatible with our difference-in-differences empirical framework, we limit the PSID sample to household heads for whom we can observe five or more

[16] Household heads are defined by the PSID as a male in the family unit above the age of 16, and there is exactly one per family per year. If a female is unmarried, or in the rare occasion when the husband is not available (such as in an institution or in the military), she can be defined as a household head.

[17] We arrange the data such that the key variables are all linked to the year of activity as opposed to the year of reporting.

[18] Most household heads retired a single time, but 26.4% of retirement observations come from those who retired multiple times.

annual observations, and at least one observation prior to 1974. We adjust all monetary variables to 2018 dollars using the national CPI.[19] In order to avoid undue influence from outliers, households with high taxable incomes (above $500,000) or wages (above $200/hour) are excluded. The final dataset spans 3,811 different households spanning a 43-year period.

2.2 Defining Boom Households

Figure 1 depicts the U.S. oil boom and bust that we study.[20] Annual oil prices more than quadrupled from 1974 to 1980 before crashing to pre-boom levels in 1985 and remaining at those levels for 1985-1998. Both the price spike and its collapse were unanticipated (Baumeister and Kilian, 2016). A National Geographic report in February 1981, for example, stated that "conservative estimates" predicted that oil prices would rise from a nominal price of about $35 per barrel ($100 in 2018 dollars) to a nominal price of $80 by 1985 ($230 in 2018 dollars) (Weaver 1981, 2). As it turned out, oil prices collapsed to $27 per barrel in 1985 and $14 in 1986 (respectively $58 and $33 in 2018 dollars). The price volatility translated, with a lag, into volatility in the number of wells drilled annually and in the share of employees in the oil and gas industry. By 1992, the country's share of oil workers was similar to the share in 1973 at about two per 1,000 workers.

To differentiate households who experienced the boom from those who did not, we make use of the 1980 U.S. Census Integrated Public Use Microdata Sample (IPUMS) (Ruggles et al., 2019). It reports employment information on the industry in which an individual works, as well as the county group where the individual is located.[21] Using this information, we aggregate up to the county group level to calculate the share of employed individuals who work in the oil and gas industry. Following an approach used elsewhere in the literature (as described below), we label "boom households (Boom HH)" as those for whom the household head lived in a county during 1980 that, at that time, had an oil employment share exceeding 2.5 percent. The year 1980 was approximately the height of the oil boom, based

[19] We discuss the issue of differential regional inflation in Section 4.3 and Section 4.4.

[20] Throughout the paper we refer to the "oil" boom and bust for ease of exposition, but readers should recognize that, as is common in oil booms, natural gas production also increased and declined during this period. Our choice to focus on oil rather than oil and gas follows convention from the literature, such as Michaels (2011).

[21] A county group is a group of one or several counties in which the population is roughly 100,000 people.

on global oil prices and U.S. drilling activity (see Figure 1).

Figure 2 is a map of boom counties. There are 489 counties spanning 15 states. The counties are concentrated in the Southwest and the Rocky Mountain regions (e.g., Texas, Louisiana, Oklahoma, Texas, Wyoming, Utah) and portions of Kansas, North Dakota, and Utah. Because IPUMS reports data for county-group aggregates for sparsely populated counties, some of the boom counties are clustered with neighboring counties. We do not consider the somewhat coarse measurement problematic because research suggests the economic impacts of oil booms spill into neighboring areas (Feyrer et. al, 2017; James and Smith, 2019).

Using 1980 oil industry employment shares to measure variation in the intensity of a household's experience with the boom merits further discussion. The precedents for this approach are Kumar (2017) and Black et al. (2005a), who define treated counties as those with greater than 2% and 3% of employment in the booming sector, respectively. We set our boom threshold at 2.5% because it is the midpoint of these two cut-points.[22] To verify that the 489 counties were in fact "booming" during 1980, we also assess their employment shares in 1980 relative to 1970 and 1990. Across the 489 boom counties, the mean oil and gas employment share was 6.2 percent in 1980 compared to 3.8 percent in 1990. We cannot run a complete comparison with 1970 census data because 1970 employment shares were reported for only 293 of the 489 boom counties. Among this subset, the mean oil and gas employment share was 4.0 percent in 1970, compared to 6.2 percent in 1980 and 4.1 percent in 1990.

2.3 Summary Statistics

Table 1 shows summary statistics for our estimating samples. Across samples, 75 to 80 percent of household heads are male and about two-thirds are married during a given year. The average household head has about twelve years of education and is around forty years old. There are roughly three people per household. Slightly over six percent of household heads were living in a boom county at the peak of the boom in 1980 and a bit fewer were

[22] As we discuss more in Section 4.3, we examine how results change when we vary the boom threshold to 2.0 or 3.0.

living in a boom county in 1973.[23] The mean total income across samples is between $65,000 and $70,000 and is comprised primarily of labor income. The hourly wage rate is around $25 and the probability of being unemployed is between three and five percent. Retirement is low (less than one percent) in the Under 55 sample but increases to nearly 20 percent of observations in the All Ages sample.

3 Empirical Framework

The foundation of our empirical strategy is a difference-in-differences (DiD) framework that compares differences in outcomes for households that experienced the boom ("boom households") to those that did not ("non-boom households"). As discussed above, we define households as experiencing the boom if the household head lived in a boom county during 1980. The empirical estimates focus on how differences in outcomes across these two household types varied over 1975-2012 relative to pre-boom differences. Relative increases in income for boom households indicate positive boom effects while relative decreases indicate negative effects.

3.1 Empirical Specification

To evaluate the effects of the boom and bust, we employ a series of regression models. Our baseline regression model takes the following form,

$$y_{it} = \alpha_i + \tau_t + \mu_s + \beta \text{Boom HH}_i \times \text{Period}_t + \gamma X_{it} + \epsilon_{it}.$$

In this model, y_{it} represents income for household i in year t, α_i represents a vector of household head fixed effects, τ_t represents a vector of year fixed effects, μ_s is a state fixed effect,[24] X_{it} is a vector of time-varying controls (family size and a marriage indicator), and $\text{Boom HH}_i \times \text{Period}_t$ is a vector of interaction terms (where Boom HH is interacted with

[23]The relatively low number of oil county observations is not surprising because the 489 oil dependent counties accounted for only 6.81 percent of the total U.S. population in 1980.

[24]State fixed effects are not collinear with household fixed effects because people move across state borders.

temporal indicators for different time periods).[25] The coefficients of primary interest are those on the interaction terms. We omit the pre-boom period in all models, so that the coefficients on the interaction terms indicate the change in income for boom households across time relative to non-boom households. Because we measure income in levels, the coefficients can be interpreted as the effect on income as measured directly in dollars.[26]

Because the sample sizes in boom counties are small in the PSID, we group years rather than trying to separately estimate annual effects. We group years into seven time periods. These are the "pre-boom" (1969-1974), the "early boom" (1975-1979), the "late boom" (1980-1984), the "early bust" (1985-1988), the "late bust" (1989-1992), the "post-bust" (1993-1999), and the "millennium" (2000-2012). These definitions correspond to changes in oil prices, drilling, and oil sector employment as displayed in Figure 1. The end of the "late bust" is assumed to be 1992 primarily because of data availability. The PSID measure of transfer and retirement income includes social security through 1992 but not after. This means we cannot compare "total" incomes after 1992. We can, however, evaluate taxable income over the entire time period.

The "millennium" period is of less focus in our analysis because, by this point in time, the sample thins substantially due to deaths and survey attrition. By 2000, the number of remaining PSID households who resided in boom counties in 1980 is only 59 (as opposed to 231 in 1973). Part of the reason for the drop-off in 2000 is that the original PSID core sample was reduced from 8,500 to 6,168 in 1997 to enable the inclusion of more international immigrants in the data.

We initially focus our analysis on the Under 55 sample in order to estimate the economic effects on households during their prime working years. The key benefit of estimating boom-and-bust effects from a sample with this age cutoff is that the coefficients are likely to be driven by exogenous variation in labor market and business earnings conditions (i.e. changes in demand for labor and local business output) as opposed to endogenous decisions

[25]We control for family size and marriage because more earners in the household could mechanically increase household income. However, we recognize these variables could be endogenous to the boom and bust (see Kearney and Wilson, 2018). We have also run the model without these controls and the results are very similar. Marriage does not appear to statistically relate to the boom, and there is some weak evidence that family size grows with the boom, as found in Kearney and Wilson (2018).

[26]We assess functional form assumptions in Section A.5 in the Appendix.

about whether to remain in the work force or retire.[27] We then replicate the analysis using the All Ages sample in order to incorporate endogenous decisions made toward the end of a career, such as delays in retirement, which may also affect long-run incomes. We also present results from a sample that simply drops all observations from retired household heads (in tables, these are described as the "Non-Retired" sample).

3.2 Identification and Interpretation

There are two key identification concerns. The first is that boom and non-boom households may have been on different economic trajectories prior to the boom. If this were the case, we could erroneously attribute differential outcomes across households to the boom and bust when in fact the differences simply reflect a projection of pre-existing trends into later time periods. We address this issue by examining trends in outcomes during the period prior to the beginning of the boom. The second concern is that, even if pre-trends were similar, the boom households may have endogenously selected into booming counties because they forecasted that doing so would improve their future incomes. To address this self-selection problem, we instrument for the boom household indicator. Our instrument is an indicator for the household's location in 1973, before the boom began. This instrument is strongly correlated with 1980 location and using it helps to purge the effects of selective migration between 1973 and 1980 into boom counties.[28]

The IV estimates capture the local average treatment effect of the boom for pre-existing residents. We focus on these estimates because local governments are primarily concerned about the welfare of pre-existing residents when deciding on policies that could promote or restrict a local resource boom. Our OLS estimates, in contrast, provide an estimate of the average effect of the boom when pooling across both pre-existing residents and households that migrated into a booming area. Ideally, we could also estimate models that

[27] In our sample, the average age of first retirement was 62.77. Only 10.7% of sample household heads who we observe retiring do so prior to age 55 whereas 77.0% retired between the ages of 55 and 70.

[28] An alternative to this approach is to re-code our Boom HH based on the household's location in 1973. The problem with this approach is that it may underestimate the effect of the boom on local residents that experienced the boom because some residents that lived in a booming region in 1973 moved away before the height of the boom. Regardless, the empirical results are very similar in reduced-form regressions based on 1973 location rather than using the IV approach.

isolate the effect of the boom on migrants. Unfortunately, this is not possible because the PSID dataset is small with too few sample households who migrated into a booming area to estimate this effect with enough precision. While we can infer, to a certain extent, how the effects on pre-existing residents compares to the effects on migrant households by examining how the IV estimates differ from the OLS estimates, we emphasize the empirical setting is better suited for evaluating how the boom affected pre-existing residents. For this reason, the forthcoming sections predominantly focus on the IV estimates.

3.3 Trends Prior to the Boom

We evaluate the identification assumptions by examining pre-boom trends visually and statistically. Figure 3 plots total and taxable income for the Under 55 and All Ages samples. The solid line shows the mean for boom observations, based on 1973 location, and the dashed line shows it for non-boom observations. The dots plot the difference in means between the two groups, all relative to 1969.

Focusing for now on the pre-boom patterns, we note there is not a discernible, systematic difference in the time trends across boom and non-boom households until about 1974, when the means for boom households start to increase relative to the means of non-boom households. The stability of pre-boom differences in means provides support for the "common trends" assumption that is required for identification in our difference-in-differences empirical framework (Meyer, 1995). However, the visualization here may fail to detect pre-trends because the graphics do not control for household fixed effects, so it is possible that changes in the sample composition between 1969-1974 are obscuring trends based on within-household variation.

To examine trends based on within-household variation, we employ regression models. Tables 2 and A.1 show the results of statistical tests for differential pre-trends. In Table 2, we regress annual household income over 1969-1973 and interact year indicators with an indicator for a household residing in a boom location in 1973. The year omitted from the estimates is 1969. A pattern of statistically significant coefficients of the same sign would indicate differential pre-trends. A pattern of coefficients with idiosyncratic signs (and lacking statistical significance) would demonstrate no evidence of differential pre-trends. We find no evidence of differential pre-trends: none of the coefficients are statistically significant, and patterns of positive and negative coefficients are sporadic and intermittent. The results suggest that pre-trends were similar across household types and this lends credibility to our DiD estimating framework. An additional test, which interacts 1973 boom indicator with a continuous year variable, is displayed in Table A.1 in the Appendix and demonstrates no evidence of a differential, linear time trend across boom and non-boom households over 1969-1973.

4 Evaluating the Effects of the Boom-and-Bust Cycle

In this section we evaluate the effects of the boom and bust on income and retirement. We begin by discussing graphical evidence of income effects based on Figure 3. We next present regression estimates of annual effects on income, evaluate the robustness of our estimates to alternative models, and compute the cumulative effects on income by aggregating the annual estimates. We then examine the effect of the boom on the decision to retire and discuss how our estimates compare to estimates from the place-based literature.[29]

4.1 Graphical Evidence Based on Trends in Means

Figure 3 shows graphical evidence of boom-and-bust effects on total and taxable income for the Under 55 and All Ages sample. Focusing first on the Under 55 sample, Panels A and B show that, beginning in around 1974 or 1975, the mean income of boom households starts to increase relative to the mean for non-boom households. Beginning in around 1983, the mean income of boom households begins to fall and reaches a low, relative to non-boom households, in 1988. There remains some evidence of a negative effect on income through 2000. Mean taxable income of boom households becomes erratic during the 2000s. This is largely due to the thinning sample over time as discussed earlier.[30]

Turning to means based on the All Ages sample, which are displayed in Panels C and

[29] We also conduct an analysis of the channels through which the boom affected incomes by estimating the effects on wages, unemployment, and other income sources. These results and an accompanying discussion appear in the Appendix.

[30] As we will discuss more later, we are hesitant to conclude that boom households experienced positive boom effects during the 2000s, despite the relative increase in means, because standard errors become larger during these periods, which is a result of small sample size.

D, the trends indicate a clearly positive effect during the boom years because boom households experienced a relative increase in incomes. These boom-period patterns mirror the patterns seen in the Under 55 sample. During the bust, however, the trends in mean incomes look different in the All Ages sample. The bust-period patterns provide some evidence of a modest positive effect during certain years and we do not observe a substantial decrease in relative income during any sample year.

Overall, Figure 3 provides visual evidence of boom period gains and bust period losses in the Under 55 sample and boom period gains and minimal bust period effects in the All Ages sample. The figures also highlight the income volatility experienced by boom households over time, in the sense that households experienced positive, yet fleeting income gains during the boom years in all cases. The volatility is more extreme in the Under 55 sample suggesting that older households had more options to smooth income (e.g., via endogenous retirement timing). We next formalize and expand the analysis using regression models. In addition to producing estimates of standard errors, an additional advantage of the regression analysis relative to the graphical analysis is that the estimates from regression analysis are driven exclusively by within-household variation. By contrast, compositional changes in the unbalanced panel of households can affect the graphical analysis of means.

4.2 Regression Estimates

Table 3 shows coefficient estimates for the Under 55, All Ages, and Non-Retired samples for total income, which is consistently measured in PSID through 1992. The odd-numbered columns show OLS estimates and the even-numbered columns show IV estimates. For the IV results, the instrument is an indicator for whether the household lived in a boom county during 1973, prior to the start of the boom, as discussed above. The first-stage results indicate a strong positive relationship between living in a boom county in 1980 and living in that same county during 1973.[31]

The pattern of estimates in Table 3, Columns 1-4 are generally consistent with the

[31] We report estimates from the first stage for a representative regression (the total income model using the Under 55 sample) in Table A.2. For each of the instrumented interaction terms, the first stage instrument is significant at the .001 level. We additionally report unconditional and conditional first-stage F-statistics for equations with multiple endogenous variables (Sanderson and Windmeijer, 2016). The F-statistics are large for each endogenous variable.

graphical patterns in Panels A and C of Figure 3. In both the Under 55 and All Ages samples, the coefficients suggest the boom significantly increased average household incomes during 1975-1984. The estimated effect of the bust, however, is different in the Under 55 versus All Ages samples. Estimates from the Under 55 sample suggest the bust significantly depressed boom household incomes by an average annual amount roughly similar in magnitude to the average annual boom period gains. Estimates from the All Ages sample suggest the bust did not affect the average incomes of boom households relative to the incomes of counterfactual households during 1985-1992. The estimates in Columns 5 and 6 of Table 3 are from the sample of households with non-retired heads and more closely resemble the estimates from the Under 55 sample. Hence, the coefficients in Columns 5 and 6 suggest the differences in the Under 55 versus All Ages sample estimates may be explained by retirement timing: we examine this possibility in Section 4.5.

A comparison of the OLS and IV estimates is helpful for evaluating how selective migration to boom counties may distort measurement of the effect of the boom and bust on local residents. Based on the OLS estimates in the Under 55 sample, average annual household income increased by approximately \$6,307 and \$7,382 during the early and late boom and decreased by \$7,128 and \$6,628 during the early and late bust. Based on the IV estimates, which we prefer because they purge the endogenous migration decision, average annual household income increased by \$5,053 and \$6,851 during the early and late boom and decreased by \$8,054 and \$8,737 during the early and late bust.[32] These results suggest that OLS estimates – which pool residents with migrants who moved to boom areas between 1974 and 1980 – overstate the income gains to residents during the boom and understate the income losses during the bust. This finding is consistent with Guettabi and James (2017), who find that estimates of the effect of an oil boom on the North Slope of Alaska on economic outcomes is larger when the treatment variable is coded based on place of work rather than place of residence.

Table 4 shows the results when the outcome variable is taxable income, rather than total income. The advantages of using taxable income are that it is i) consistently mea-

[32]Out-migration during the bust is also endogenous but we do not account for it because we are interested in the long-run effect of the boom, inclusive of any defensive measures taken to minimize the impacts of the bust (such as out-migration).

sured in PSID through 2012 enabling a longer-run evaluation and ii) it excludes pension and retirement income allowing us to compare sample estimates on income earned prior to retirement. We find that the estimates for taxable income closely mimic those for total income over 1975-1992. In terms of the 1993-1999 and 2000-2012 time-period results in Table 4, we find that the coefficients are imprecisely estimated. Because of the imprecision, we fail to reject the null hypothesis that the boom-and-bust cycle had no effect on taxable income after 1992.

4.3 Robustness Checks

The Appendix shows several robustness checks for the main income estimates. First, rather than using a 2.5% employment share threshold to define booming area, we use thresholds of 2% and 3% (results reported in Tables A.3 and A.4). These thresholds were employed by Kumar (2017) and Black et al. (2005a), respectively, in earlier studies of resource booms. Second, we drop households in counties that had shares of 1980 oil employment between 1% and 2.5% to eliminate borderline boom households from the analysis (Table A.5). Third, we include separate year effects for rural and urban areas to account for the possibility that oil boom regions, which tend to be rural, experienced differential time shocks that may have been unrelated to the oil boom and bust (Table A.6). Fourth, we show the results with and without state fixed effects (Table A.7). Fifth, we restrict the sample to household heads who never changed their county location across the entire time period (Table A.8).[33] The last robustness check is designed to address the concern that households residing in a boom county were less mobile than households living in non-boom counties and therefore less able to pursue economic opportunities throughout their lives. By limiting the sample to non-movers, we investigate whether our results hold even in a setting where differential

[33]There were 88 (36%) treated households that moved at some point during the sample. Among those, 46 (52%) moved to a new state whereas 42 (48%) remained in the same state. With respect to boom versus non-boom status, 58 (66%) of the treated movers moved to a non-boom region at some point whereas 30 (34%) of the treated movers remained in boom county for the duration of the analysis.

mobility choices cannot operate as a mechanism for income effects.[34] In general, the main patterns of coefficient estimates found in Tables 3 and 4 hold across the various scenarios just described.

As mentioned above, our main analysis adjusts for inflation using the national CPI, which is the conventional approach in the literature on booms and busts within the United States (e.g., Allcott and Keniston, 2017; Jacobsen and Parker, 2016). We would ideally prefer to deflate income with a county-level CPI, but such a geographically precise index does not exist for our time window of study. Instead, as an additional robustness check, we adjust the data using the four regional CPIs, as opposed to a single national CPI. As Appendix Table A.9 shows, adjusting by the regional CPI diminishes the positive boom period income effects. This makes sense because i) the boom counties likely experienced higher price levels during the boom period due to elevated economic activity and short-run supply inelasticities, and ii) the regional CPI picks up this difference because the boom counties were concentrated in the South and West (rather than the Midwest and East).[35] Overall, as we describe in more detail below, adjusting for the regional CPIs makes the cumulative effects of the boom on income less positive and thus makes our primary estimates based on the national CPI conservative with respect to concluding the boom-and-bust cycle was more of a curse than a blessing.

4.4 Effects on Cumulative Income

In order to assess the cumulative effects of the boom-and-bust cycle on average household income, we sum the annual income increases and decreases indicated by the regression

[34]We also investigated a placebo analysis in which we coded households that lived in a boom area just prior to the start of the boom period (1969 to 1973) but moved to a non-boom area by 1974 as the treatment group. Unfortunately, there are only 16 household heads that met such criteria, so the estimates from the model were very imprecise, with standard errors in the tens of thousands of dollars. However, the point estimates from the model were positive during every post-1973 era and there is no evidence that these positive relationships diminish during the bust. Although statistically imprecise, the estimates help cast doubt on an alternative explanation that boom county residents were predisposed to lower cumulative earnings irrespective of experiencing the boom-and-bust cycle.

[35]The regional CPIs for the South and West regions increased relative to the Northeast and Midwest during the boom period and, in the longer-run, the relative price ratio across regions settled close to pre-boom differences. This is consistent with Allcott and Keniston (2017) who find that differences in housing rents (which account for a large proportion of the CPI) between boom and non-boom counties returned to pre-boom levels after the bust.

models, as reported in Table 3, and calculate the net effect.[36] We focus on total income, rather than taxable income, thereby omitting estimates from the post-bust and millennium periods because there is little evidence that the boom-and-bust cycle affected income during these years. We also omit from the calculations the bust period coefficient estimates from the All Ages sample because these estimates are not statistically significant. Using estimates based on total income allows us to capture income effects that operate through any channel, including through pensions payments, which are excluded from our the PSID's taxable income variable.[37]

To account for the differential timing of the effects, we discount all income effects using 1975 as a base year. We consider 0%, 1%, 2%, and 3% discount rates because these rates cover the range of real interest rates observed over our sample (Yi and Zhang, 2016). A discount rate of 0% is arguably preferable because it corresponds to real interest rates in the early 1970s. While zero percent may appear surprising, recall that the data have already been adjusted for inflation, which was 11 percent in 1975 and remained high throughout the early boom.

Table 5 reports the cumulative effects on income from the different samples. The calculations discount over 18 years, from 1975 to 1992, using the following procedure,

$$\text{Cumul. Eff.} = \sum_{1975}^{1979} \frac{\beta_{\text{Early Boom}}}{(1+r)^{t-1975}} + \sum_{1980}^{1984} \frac{\beta_{\text{Late Boom}}}{(1+r)^{t-1975}} + \sum_{1985}^{1988} \frac{\beta_{\text{Early Bust}}}{(1+r)^{t-1975}} + \sum_{1989}^{1992} \frac{\beta_{\text{Late Bust}}}{(1+r)^{t-1975}},$$

where r represents the annual discount rate, the β's represent the coefficient estimates from Table 3, and t represents year.

Focusing on the zero discount rate scenario, we note the OLS results in both the Under 55 and Non-Retired sample imply modest but positive cumulative income effects at $13,420

[36]An alternative approach would be to aggregate the data into a cumulative income measure prior to running regressions and then estimate the effect of exposure to the boom using this aggregated outcome variable. This is an inferior approach in our empirical settings for two reasons. First, there are some "holes" in the PSID data due to households not completing the PSID survey in certain years. These missing observations are much more problematic for an aggregated analysis—where the holes are implicitly assumed to be zero—than they are for our unbalanced panel data analysis of annual incomes. Secondly, because the PSID data include many different cohorts, households are observed over different time spans. In our analysis of annual income, we can control for this using year effects. In an analysis of aggregated income, the different cohorts would add noise and potentially bias to the estimates.

[37]In reality, many forms of pension payments are taxable, but the PSID data do not include them in the taxable income variable and notes that "some classification names do not accurately reflect their components."

and \$9,390 respectively. When we use the IV model to capture effects on pre-existing residents, the effects turn negative at -\$7,645 and -\$11,147, indicating that not accounting for migration overstates the within-household net gains for pre-existing residents.

The results change dramatically when we employ the coefficient estimates from the All Ages sample. The cumulative effect of the boom is \$65,284 based on OLS estimates and \$56,146 based on IV estimates. The difference between our least preferred estimate, which is \$65,284, and our most preferred estimate, which is -\$7,645, is large at \$72,929. The least preferred estimate ignores endogenous selection into location and retirement whereas the most preferred estimate is based on exogenous age and pre-boom location.

A caveat about the cumulative income calculations just described is that they are generated from sample estimates of a panel of households that is unbalanced due to attrition. In the Under 55 sample, attrition is due to sample household heads reaching age 55. In the Non-Retired sample, attrition is due to retirement. In the All Ages sample, attrition is due to death of the household head and other factors. As a robustness check, we limit the sample to a "young" cohort of household heads who were 35 years or younger in 1970 to reduce attrition from the sample over 1975-2012. The results from this sample are shown in the Appendix Tables A.10 and A.11. The findings indicate that, with a zero percent discount rate, the OLS coefficient estimates imply that the average cumulative effect of the boom and bust on young households was positive at \$9,798. The IV estimates suggest the cumulative effect was negative at -\$18,042. Of the discounting scenarios, the IV estimates generate a positive cumulative effect (of only \$42) only when we assume a 3% discount rate.

As discussed earlier, our primary analysis is conducted based on data that have been adjusted for inflation using the national CPI. However, it is worth highlighting that, as shown in Appendix Table A.12, the cumulative effects of the boom-and-bust cycle are appreciably more negative when we adjust income to the regional (rather than national) CPI. With regional CPI adjustments, the IV estimates of the Under 55 sample are always negative, ranging from -\$21,527 (0% discount rate) to -\$4,941 (3% discount rate). Accounting for the regional CPIs makes the boom-and-bust effect more negative effect because the regional CPI accounts for the elevated price levels in booming regions during the boom period and hence the lower levels of real income for residents of those counties.

The comparisons in Table 5, which are complemented by those in Appendix Tables A.11 and A.12, suggest the boom-and-bust cycle was a resource curse for households during their prime working age because, under reasonable discounting assumptions, it caused cumulative income losses.[38] At minimum the effects on income were not overwhelmingly positive, as they correspond to substantially less than a year of mean household income under all scenarios.

4.5 Retirement

The estimated effects on income reported above show that the boom and bust created volatility in the form of a rapid increase and then decrease in income. We now examine how household heads responded to this volatility later in life. We focus on delayed retirement partly due to data availability, but also because it is one of the most prominent actions a household can take to smooth fluctuations in cumulative lifetime income.

We cannot assess the effects on retirement probabilities using the same DiD empirical model used to assess income because of PSID sample limitations. The DiD model relies on pre-boom observations as a benchmark for baseline outcomes and the PSID sample of retired household heads during the pre-boom period is thin, particularly for the sample of boom households. The sample contains only 27 oil-boom retirement observations during 1969-1974 spanning 11 households. The thin data for this period is not surprising because the PSID did not recruit household heads into the sample who were already retired in 1968, or on the cusp of retiring.

In lieu of using a DiD approach that relies heavily on pre-boom means, we instead estimate the probabilities of retirement separately for each period, for household heads 55 years or older.[39] Retirement probabilities during the non-bust years serve as a type of placebo test. They indicate whether boom households simply had systematically different probabilities of retirement across all time periods. All regression models include state and year fixed effects. Because these models do not include a household fixed effect, we also

[38]These results are related to those in Hombert and Matray (2019), who show that cumulative earnings over the information technology boom and bust of the 1990s were negative.

[39]An alternative approach is to employ a survival model where survival is akin to remaining in the workforce. We do not employ a survival model because many household heads in the PSID retire multiple times as discussed above.

include a male indicator, years of education, a male-by-education interaction, a quadratic in age, a married indicator, and family size as control variables.[40]

Table 6 shows the results. The statistically insignificant coefficients in most columns demonstrate that boom household heads were not generally more or less likely to retire than non-boom household heads. The major exception is that boom household heads had a much higher probability of retiring during 1989-1992, after experiencing the boom era and the early bust. As shown in the IV results in Column 5, household heads over 55 who lived in a boom county during 1980 were 15 percentage points less likely to retire during the late bust when compared to household heads who did not experience the boom. The Column 5 estimate represents a 30 percent decrease in retirement probability relative to the 0.50 sample mean for retirement in the 55+ sample during 1985-1992.

The finding that household heads who experienced the boom were more likely to delay retirement during the end of the bust reconciles the large discrepancy in the estimated bust period effects on total and taxable income across the Under 55 and All Ages samples. The All Ages sample results overstate the benefits from the boom-and-bust cycle if delayed retirement is undesirable as discussed below. This finding matters because place-based estimates of aggregate income, or of income per capita, implicitly employ an all-age sample and, in doing so, will also be inflated by "hidden" income smoothing behavior (i.e. delayed retirement) during bust years.

Although data limitations in the PSID prevent us from examining mechanisms through which the boom and bust caused delayed retirement, the literature on retirement determinants offers potential explanations. Economic theories of retirement, as well as empirical tests, generally infer a positive relationship between cumulative earnings and the probability of retirement, holding constant an individual's age (see Coile, 2015). The timing of retirement is more often aimed at achieving a target pension or annuity flow rather than being determined by age. In our setting, the fact that retirement was not delayed until the late bust, after the early bust had caused a decrease in income, is consistent with delayed retirement occurring as a response to an unexpected decrease in cumulative income. It is also

[40]The results are similar when we drop the marriage and family size variables that are possible endogenous to the boom.

possible that delayed retirement during the late bust was a response to a decrease in wealth that is not observable in the PSID data.[41] Exposure to energy booms may reduce wealth by increasing household consumption and debt (Brown, 2018), and because local resource busts may sharply reduce home equity.

We interpret delayed retirement as further evidence that the boom-and-bust cycle negatively affected households. Our interpretation assumes that delayed retirement was an undesirable income smoothing response to the uncertainty and volatility of the boom-and-bust cycle. This is consistent with research elsewhere suggesting individuals would give up significant amounts of lifetime consumption simply to know their actual retirement date at age 23 (Caliendo et al., 2016).

Retirement decisions are complex and multifaceted and we cannot rule out other explanations for delayed retirement. Based on our review of the literature, however, there is one primary scenario in which the delayed retirement of boom households could be interpreted as positive for welfare. The scenario has two components: i) the boom years improved individual health outcomes, and ii) households chose to allocate the health windfall to further work rather than to leisure. The literature on resource booms suggests (i) is possible but probably unlikely in our setting. On one hand, boom periods can improve health by providing people with more income to spend on health care (Acemoglu et al. 2013; Cotet and Tsui 2013). On the other hand, resource booms often create unhealthy local conditions and several studies argue that community health suffered in boomtowns created by the oil boom we study.[42] The literature on retirement timing similarly suggests that condition (ii) is possible but not necessarily likely. Studies identify cases in which healthier individuals have chosen to work longer, however the relationship is hard to pin down due to the endogeneity of retirement and health and the most credible evidence comes from major recent negative health shocks, such as heart attack, that may not be relevant to our evaluation because it focuses on long-run relationships (Coile, 2015). In summary, there is not much evidence in

[41]The literature on retirement timing demonstrates that it is a function not only of cumulative earnings, but also of wealth, which depends on consumption prior to retirement and asset values at the time of retirement (see, e.g., Coile and Levine, 2007; Goda et al., 2011; McFall, 2011; Begley and Chan, 2018).

[42]There is a large literature in sociology on the boomtowns of the 1970s and 1980s. Several authors reported that boom growth harmed mental health among both new in-migrants and long-term residents (e.g., Gilmore, 1976). Smith et al. (2001) provide a review and critique of this literature.

support of the conditions that would be necessary to interpret delayed retirement as positive in our setting. Consequently, we think the best explanation is that delayed retirement was a negative consequence of economic uncertainty and unexpected cumulative income losses resulting from the boom-and-bust cycle.

4.6 Comparison with Place-Based Literature

How do the results here differ from findings from the placed-based literature? Consider first our short-run finding, that the boom period increased annual income for the average resident household. This finding aligns with place-based estimates, which indicate that a locale's average earnings, earnings per capita, and earnings per worker almost always increase when extractive activity in the locale is elevated (see, e.g., Aragón et al., 2015; Mason et al., 2015; Venables, 2016; Marchand and Weber, 2017; van der Ploeg and Poelhekke, 2017; and Jacobsen, 2019a). The main qualification is that placed-based studies often pool existing residents with migrants, and our results suggest that pooling may exaggerate the gains accrued by residents. Setting that aside, the similarity of our findings to the findings from the place-based literature points to the conclusion that, in the short run, booms that are good for places are also likely to be good for people.

By contrast, our study demonstrates that place-based estimates are less useful approximations for the long-run and cumulative effects of resource booms on households. On the one hand, our finding from the All Ages sample that households who experienced the 1980s oil boom and bust earned a bit more cumulative lifetime income than households who did not is consistent with another recent study of nationwide data, by Allcott and Keniston (2017), which shows that U.S. oil booms have had positive or neutral long-run effects on earnings per worker in oil-producing counties. On the other hand, the effects in the All Ages sample are positive only because households in oil-producing counties worked longer, by delaying retirement. Once we adjust for this endogenous response–which would be impossible to do with place-level data—we find that the boom-and-bust episode reduced long-run cumulative income during prime working years for the average resident household.[43] In general, place-based studies may lead to misleading or incomplete conclusions about the long-run labor market and income-generating consequences of booms on the people who experience them.

While we believe our study demonstrates the importance of household-level panel data for understanding how temporary booms affect the long-run welfare of residents in resource-endowed locales, we emphasize that place-based studies are critical for understanding other dimensions of the resource curse hypothesis. In particular, place-based studies are better suited for examining policy and fiscal responses to resource booms (e.g., James, 2015), and for understanding how economic sectors differentially react to booms and busts (e.g., Black et al., 2005a; Marchand, 2012; Jacobsen and Parker, 2016; Allcott and Keniston, 2017; Feyrer et al, 2017; Maniloff and Mastromonaco, 2017; Clay and Portnkyh, 2018).

5 Conclusion

Although the literature on resource booms and the resource curse is vast, ours is the first study that we are aware of to evaluate the short and long-run effects of resource booms and busts on people, rather than places. We do so by tracking longitudinal data on households before, during, and after the U.S. oil boom and bust of the 1970s and 1980s. Our goal is to extend the important debate about when, if, and how resource booms benefit local economies to include an assessment of when, if, and how resource booms benefit individual residents of booming economies.

Was the resource boom a curse or a blessing for the individuals who experienced it? The results of our evaluation lean toward concluding it was more of a curse. Why? We find unambiguously positive effects on cumulative household incomes only when we allow boom household heads more time to earn income by delaying their retirement after the boom has ended. By contrast, when we restrict the sample to prime-age households or to non-retired households, the effect of the boom and bust on cumulative income is negative under

[43]More obviously, placed-based studies do not allow researchers to discount and net out the positive and negative effects of booms and busts on households over time as we have done. Doing so highlights how long-run comparisons of boom and non-boom counties can mask the cumulative effects on households resulting from asymmetries in the duration or depth of boom periods vs. bust periods.

reasonable discounting assumptions.

In addition to finding that the boom-and-bust cycle had negative or at most slightly beneficial effects on income for working age individuals, our results also suggest the economic volatility of resource booms has negative effects on boom-area residents that may not be detectable in place-based income aggregates. This volatility can complicate planning decisions related to savings, consumption, and investment and compel households into employing costly smoothing behavior. We find evidence of the negative effects of volatility in the form of delayed retirements during the bust.

Though our study has limitations, we hope that it will stimulate further research on the resource curse that is focused on people, rather than places. With respect to limitations, we have studied a single resource boom from one country using a longitudinal data set with a somewhat small sample size. Boom-and-bust cycles in different regions, and from different eras, will likely have different cumulative effects. Continued research of resource booms and busts that vary in length and intensity will remain valuable for academic understanding and policy. We hope our study of PSID data will provoke future research that employs detailed individual or household-level data in new empirical settings with data sets that are large enough to study heterogeneous effects on households based on age, gender, and economic status. Larger sample sizes of households would also enable study of how mobility helps households cope (or not) with the volatility of resource booms and, especially, resource busts.

We also hope that our study stimulates more research on how economic volatility differs for booms from exhaustible natural resources versus resource booms from renewable energy sources. While volatility is a defining feature of exhaustive energy sources such as fossil fuels (Baumeister and Kilian, 2016), it may be a less defining feature of alternative forms of energy whose prices are less dependent on world events (e.g., solar power). Understanding the volatility-related effects of different sources of energy may be important for informing public policy as alternative sources of energy become increasingly available.

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Figures and Tables

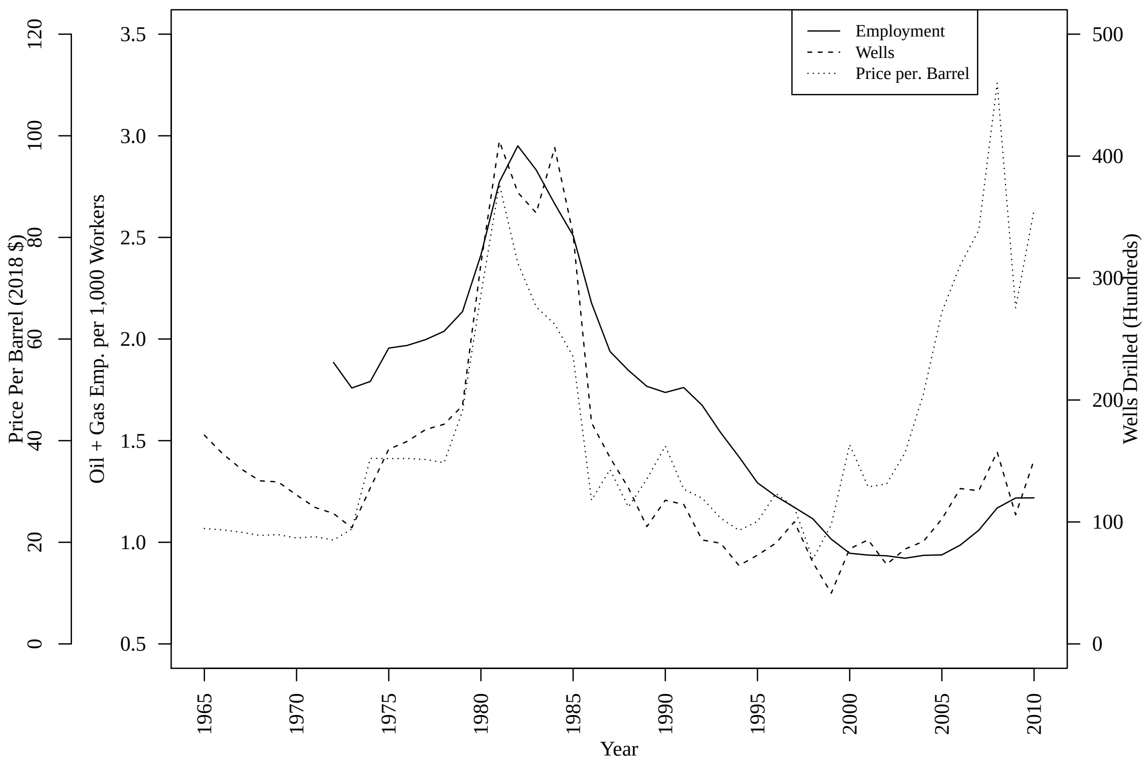


Figure 1: **U.S. Trends in Oil Prices, Drilling, and Employment.** Oil and gas extraction employees per 1,000 workers, exploratory and developmental oil wells drilled, and end of year oil price per barrel. *Data Sources: USEIA (2019), USBLS (2019a), USBLS (2019b), and USEIA (2018).*

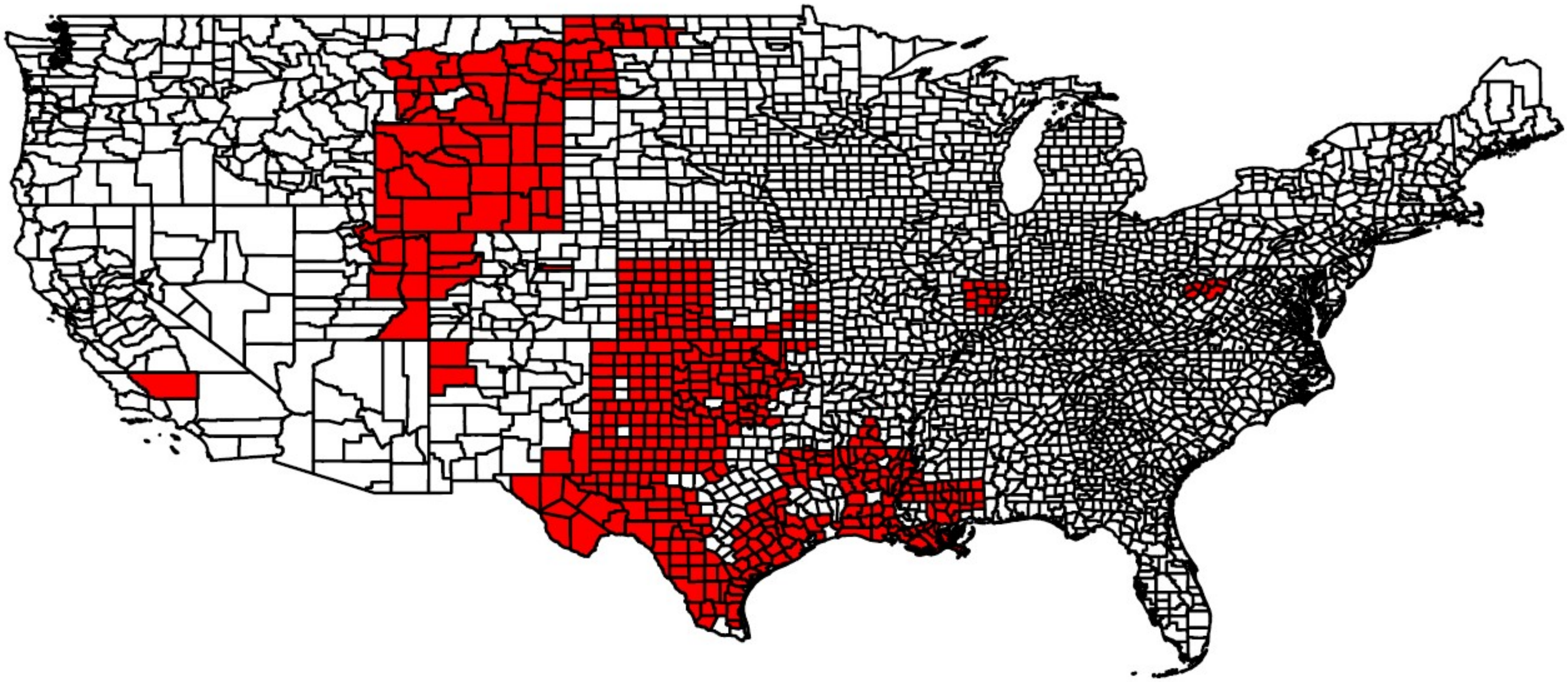
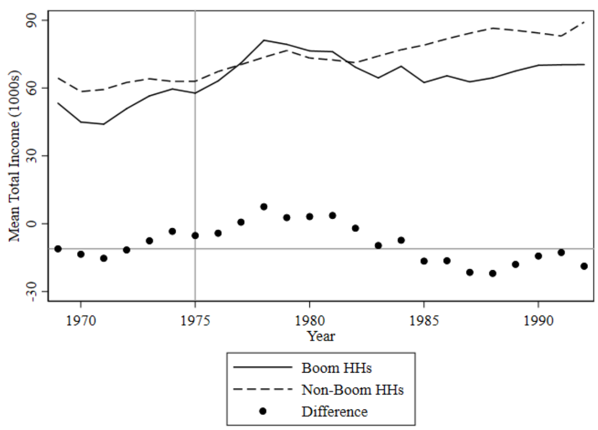


Figure 2: **Map of Boom Counties.** Counties in red are defined as “oil boom counties," those in which more than 2.5% of the county group’s employment in 1980 was in the oil and gas extraction sector. *Data Source: IPUMS (Ruggles, 2019).*

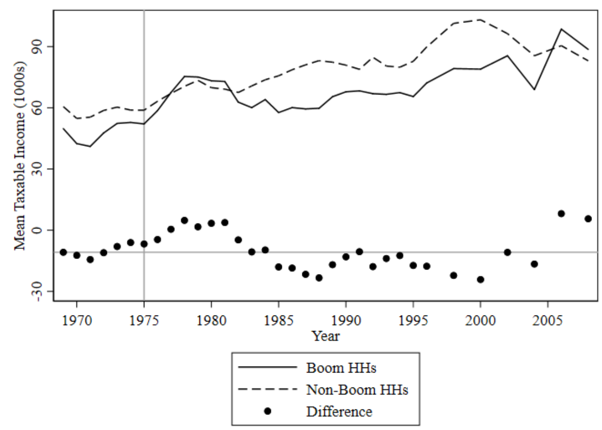
Table 1: Summary Statistics

	Under 55	All Ages	Non-Retired
Gender (=1 if Male) +	0.78	0.75	0.75
	(0.41)	(0.43)	(0.43)
Years of Education +	12.90	12.32	12.55
	(2.72)	(3.16)	(3.04)
Married (=1 if Yes) +	0.67	0.64	0.65
	(0.47)	(0.48)	(0.48)
Family Size +	3.50	2.99	3.16
	(1.87)	(1.80)	(1.79)
Age +	38.67	49.15	44.77
	(9.46)	(16.25)	(13.52)
Share of Household Head's County Employment in Oil, 1980 (%) +	0.46	0.47	0.48
	(1.35)	(1.41)	(1.40)
Boom Household (Boom HH) (=1 if Yes) +	0.062	0.062	0.064
	(0.242)	(0.242)	(0.245)
Boom HH in 1973 (=1 if Yes) +	0.061	0.061	0.062
	(0.239)	(0.240)	(0.242)
Total Income^	71,830	65,025	69,793
	(53,674)	(54,086)	(55,684)
Taxable Income^	70,171	57,675	67,569
	(57,222)	(59,505)	(60,369)
Labor Income^	65,371	51,090	61,683
	(50,600)	(53,052)	(53,182)
Non-Labor Income^	4,619	6,458	5,729
	(18,436)	(22,052)	(21,090)
Transfer and Retirement Income^	3,418	7,273	4,309
	(7,836)	(12,707)	(8,642)
Hourly Wage Rate #	25.33	25.19	25.59
	(16.80)	(20.37)	(19.10)
Unemployed Indicator (=1 if Yes) +	0.048	0.034	0.042
	(0.214)	(0.181)	(0.200)
Retirement Indicator (=1 if Yes) +	0.008	0.186	0.000
	(0.087)	(0.389)	(0.000)
Number of Observations	55,227	88,726	69,568
Number of Household Heads	3,197	3,811	3,710

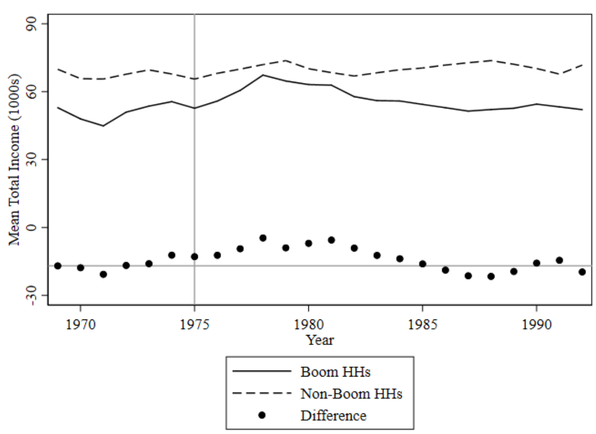
Notes: The temporal unit of analysis is always annual. The cross-sectional unit of analysis is as follows: + household head, ^ household head plus spouse, # weighted average of household head and spouse where the weights represent hours worked. Sample is limited to household heads with five or more observations in total, and at least one income observation prior to 1975. "Boom Household (Boom HH)" is an indicator for whether the household head was in a boom county in 1980. "Boom HH in 1973" is an indicator for whether the household head was in a boom county in 1973. Observations with unusually high incomes (above $500,000) or wages (above $200/hour) are excluded. Income and wage data are in 2018 dollars, adjusted by the national CPI. The share of the county's employment in the oil sector is technically the share in the oil and gas sector, which was dominated by oil employment in 1980.



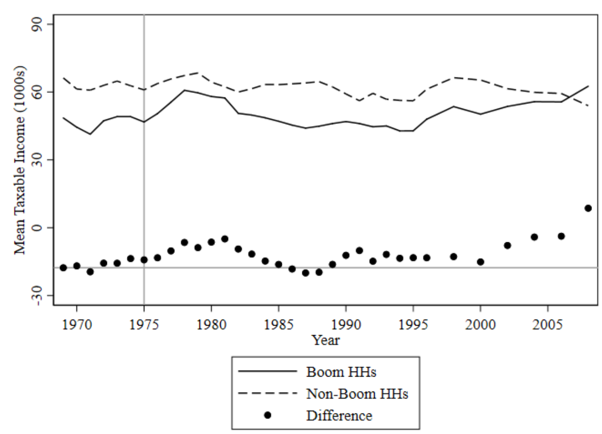
(a) Total Income, Under 55



(b) Taxable Income, Under 55



(c) Total Income, All Ages



(d) Taxable Income, All Ages

Figure 3: **Trends in Mean Income Based on 1980 Location.** The vertical line corresponds to the beginning of the energy boom and the dots corresponds to differences in mean incomes relative to the first year of the sample, which is 1969. The total income data series stops in 1992. The second phrase in each label describes the sample.

Table 2: Analysis of Pre-trends - Discrete

	Y= Total Income		Y= Taxable Income	
	Under 55	All Ages	Under 55	All Ages
	(1)	(2)	(3)	(4)
Boom HH in 1973 * 1970	1,609	1,208	2,565	2,026
	(1,781)	(1,454)	(1,818)	(1,470)
Boom HH in 1973 * 1971	-1,633	-2,356	-593	-1,428
	(2,102)	(2,805)	(2,438)	(2,907)
Boom HH in 1973 * 1972	2,215	-131	4,492	1,498
	(3,449)	(3,263)	(4,059)	(3,600)
Boom HH in 1973 * 1973	-1,069	-2,762	303	-1,243
	(2,986)	(3,145)	(4,005)	(3,628)
Boom HH in 1973 * 1974	260	-990	240	-859
	(2,812)	(3,016)	(4,316)	(3,691)
Observations	8,399	11,732	8,425	11,759
R-Squared	0.872	0.881	0.879	0.879

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The sample includes observations from 1969-1974. The omitted time period is 1969.

Table 3: Regression Estimates of Effects on Total Income

	Under 55		All Ages		Non-Retired	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Boom HH * 1975-1979	6,307***	5,053**	5,398***	4,457**	5,318***	4,446**
	(1,894)	(2,093)	(1,646)	(1,748)	(1,612)	(1,734)
Boom HH * 1980-1984	7,382***	6,851**	7,659***	6,773***	7,192***	6,692***
	(2,593)	(2,954)	(2,109)	(2,284)	(2,175)	(2,404)
Boom HH * 1985-1988	-7,128**	-8,054**	-1,713	-2,862	-6,711***	-8,103***
	(2,929)	(3,682)	(2,278)	(2,664)	(2,461)	(2,996)
Boom HH * 1989-1992	-6,628*	-8,737**	-344	-1,718	-6,579**	-8,607**
	(3,421)	(4,060)	(2,611)	(2,962)	(2,915)	(3,386)
Observations	49,501	49,501	72,664	72,664	59,487	59,487
R-Squared	0.768	0.768	0.761	0.761	0.784	0.784

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The even number columns show IV results where the instrument for "Boom HH" is "Boom HH in 1973." The omitted period is 1969-1974.

Table 4: Regression Estimates of Effects on Taxable Income

	Under 55		All Ages		Non-Retired	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Boom HH * 1975-1979	6,236***	4,425**	5,765***	4,357**	5,192***	3,636*
	(1,959)	(2,182)	(1,851)	(1,955)	(1,732)	(1,874)
Boom HH * 1980-1984	7,795***	6,825**	8,824***	7,245***	7,525***	6,469**
	(2,738)	(3,110)	(2,445)	(2,640)	(2,402)	(2,652)
Boom HH * 1985-1988	-6,808**	-7,325*	352	-1,088	-6,600**	-8,154**
	(3,128)	(3,800)	(2,657)	(3,047)	(2,716)	(3,232)
Boom HH * 1989-1992	-5,476	-7,470*	3,390	2,079	-5,379*	-7,775**
	(3,559)	(4,184)	(3,085)	(3,473)	(3,134)	(3,586)
Boom HH * 1993-1999	-790	-6,060	5,941	3,699	-702	-5,772
	(5,873)	(6,006)	(3,888)	(4,005)	(4,475)	(4,662)
Boom HH * 2000-2012	-3,753	-9,278	12,974*	12,170	-2,399	-3,801
	(10,424)	(11,493)	(7,877)	(8,710)	(10,136)	(10,957)
Observations	55,227	55,227	88,726	88,726	69,568	69,568
R-Squared	0.746	0.746	0.661	0.661	0.729	0.729

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The even number columns show IV results where the instrument for "Boom HH" is "Boom HH in 1973." The omitted period is 1969-1974.

Table 5: Cumulative Effects on Total Income

	Under 55		All Ages		Non-Retired	
Discount Rate	OLS	IV	OLS	IV	OLS	IV
0	**13,420**	**-7,645**	**65,284**	**56,146**	**9,390**	**-11,147**
1	17,191	-1,967	62,182	53,434	13,114	-5,410
2	20,247	2,747	59,302	50,917	16,138	-634
3	22,705	6,652	56,627	48,580	18,579	3,334

Notes: Cumulative effects are based on annual income effects reported in Table 3. We do not include estimates for the post-bust and millennium periods because there is little evidence that the boom affected income during these years (see Table 4). Bust period coefficient estimates from the All Ages sample are omitted because these estimates are not statistically significant (see Table 3). Discounting is annual, beginning in 1975 which represents t = 0.

Table 6: Regression Estimates of Effects on Retirement for 55+ Sample

	Pre-Boom (70-74) (1)	Early Boom (75-79) (2)	Late Boom (80-84) (3)	Early Bust (85-88) (4)	Late Bust (89-92) (5)	Post-Bust (93-99) (6)	Millennium (00-12) (7)
OLS Results							
Boom HH	-0.034	-0.005	-0.011	-0.079	-0.133**	-0.036	-0.041
	(0.068)	(0.057)	(0.060)	(0.057)	(0.066)	(0.067)	(0.062)
Observations	3,265	4,830	5,586	4,470	4,452	4,768	5,447
R-Squared	0.274	0.342	0.357	0.381	0.361	0.361	0.353
IV Results							
Boom HH	-0.064	-0.010	0.040	-0.035	-0.150*	-0.065	-0.008
	(0.075)	(0.066)	(0.068)	(0.068)	(0.080)	(0.081)	(0.097)
Observations	3,265	4,830	5,586	4,470	4,452	4,768	5,447
R-Squared	0.274	0.342	0.357	0.381	0.361	0.361	0.353
First Stage F Stat.	706.839	396.651	420.440	209.139	165.387	120.270	70.906

Notes: All models include a male indicator, years of education, a male-by-education interaction, a quadratic in age, a married indicator, family size, state effects, and year effects. The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. The instrument for "Boom HH" is "Boom HH in 1973." Kleibergen and Papp (2006) first stage F statistics are reported.

A Appendix

1. Analysis of Linear Pre-Trends
2. First-Stage Estimates
3. Robustness Checks
4. Cumulative Effects on Total Income Under Alternative Scenarios
5. Functional Form
6. Channels and Auxiliary Evidence

A.1 Analysis of Linear Pre-Trends

Table A.1: Analysis of Pre-trends - Linear

	Y= Total Income		Y=Taxable Income	
	Under 55	All Ages	Under 55	All Ages
	(1)	(2)	(3)	(4)
Boom HH in 1973 * Year	-85	-423	6	-317
	(643)	(658)	(927)	(796)
Observations	8,399	11,732	8,425	11,759
R-Squared	0.872	0.881	0.879	0.879

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The sample includes observations from 1969-1974. The omitted time period is 1969.

A.2 First-Stage Estimates

Table A.2: First-Stage Estimates

	Boom HH * 1975-1979 (1)	Boom HH * 1980-1984 (2)	Boom HH * 1985-1988 (3)	Boom HH * 1989-1992 (4)
Boom HH in 1973 * 1975-1979	0.895***	0.007**	0.001	0.000
	(0.022)	(0.004)	(0.002)	(0.002)
Boom HH in 1973 * 1980-1984	0.013***	0.884***	0.003	-0.001
	(0.005)	(0.025)	(0.002)	(0.002)
Boom HH in 1973 * 1985-1988	0.011*	0.011*	0.877***	-0.002
	(0.006)	(0.006)	(0.030)	(0.003)
Boom HH in 1973 * 1989-1992	0.012	0.009	0.000	0.876***
	(0.008)	(0.007)	(0.004)	(0.034)
Observations	49,501	49,501	49,501	49,501
First Stage F-Stat.	597.928	384.293	253.520	170.243
Conditional F-Stat.	2,094.985	2,227.044	2,115.117	1,916.598

Notes: First stage IV coefficients from column (2) of Table 3. Each column reports the results from the first stage for the corresponding endogenous variable, as indicated by the column headings. The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The omitted period is 1969-1974. "First Stage F-Statistic" reports the reduced-form F-statistic from the first stage while the "Conditional F-Statistic" accounts for the multiple endogenous variables (Sanderson and Windmeijer, 2016).

A.3 Robustness Checks

Table A.3: Regression Estimates of Effects on Total Income: Boom Definition Based on Threshold of 2.0% Oil Employment Share in 1980

	Under 55		All Ages		Non-Retired	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Boom HH * 1975-1979	4,876***	3,580*	4,498***	3,587**	3,928***	2,863*
	(1,744)	(1,890)	(1,491)	(1,565)	(1,490)	(1,585)
Boom HH * 1980-1984	5,080**	4,669*	5,590***	5,036**	4,578**	3,891*
	(2,387)	(2,672)	(1,955)	(2,087)	(2,068)	(2,257)
Boom HH * 1985-1988	-8,830***	-8,523**	-3,343	-3,663	-8,989***	-9,901***
	(2,766)	(3,417)	(2,145)	(2,475)	(2,343)	(2,785)
Boom HH * 1989-1992	-8,570***	-8,693**	-2,019	-2,149	-8,484***	-9,484***
	(3,230)	(3,896)	(2,456)	(2,804)	(2,787)	(3,237)
Observations	49,501	49,501	72,664	72,664	59,487	59,487
R-Squared	0.768	0.768	0.761	0.761	0.784	0.784

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The even number columns show IV results where the instrument for “Boom HH” is “Boom HH in 1973.” The omitted period is 1969-1974.

Table A.4: Regression Estimates of Effects on Total Income: Boom Definition Based on Threshold of 3.0% Oil Employment Share in 1980

	Under 55		All Ages		Non-Retired	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Boom HH * 1975-1979	6,267***	5,133**	5,336***	4,473**	5,468***	4,585**
	(1,962)	(2,153)	(1,710)	(1,801)	(1,680)	(1,789)
Boom HH * 1980-1984	7,540***	6,997**	7,712***	6,847***	7,495***	6,879***
	(2,679)	(3,033)	(2,190)	(2,353)	(2,263)	(2,481)
Boom HH * 1985-1988	-7,093**	-7,965**	-1,777	-2,822	-6,530**	-7,931***
	(3,011)	(3,753)	(2,358)	(2,733)	(2,553)	(3,071)
Boom HH * 1989-1992	-6,549*	-8,631**	-465	-1,838	-6,376**	-8,574**
	(3,481)	(4,131)	(2,696)	(3,030)	(3,019)	(3,466)
Observations	49,501	49,501	72,664	72,664	59,487	59,487
R-Squared	0.768	0.768	0.761	0.761	0.784	0.784

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The even number columns show IV results where the instrument for "Boom HH" is "Boom HH in 1973." The omitted period is 1969-1974.

Table A.5: Regression Estimates of Effects on Total Income: Excludes Observations in Counties with 1% to 2.5% Oil Employment Share in 1980

	Under 55		All Ages		Non-Retired	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Boom HH * 1975-1979	6,393***	5,209**	5,509***	4,494**	5,383***	4,499***
	(1,893)	(2,086)	(1,648)	(1,744)	(1,614)	(1,728)
Boom HH * 1980-1984	7,528***	7,863***	7,801***	7,448***	7,285***	7,299***
	(2,597)	(2,865)	(2,114)	(2,241)	(2,179)	(2,343)
Boom HH * 1985-1988	-7,251**	-6,017*	-1,784	-1,664	-6,935***	-6,769**
	(2,936)	(3,333)	(2,285)	(2,513)	(2,467)	(2,769)
Boom HH * 1989-1992	-6,582*	-7,723**	-284	-1,203	-6,658**	-7,963**
	(3,421)	(3,912)	(2,615)	(2,896)	(2,919)	(3,294)
Observations	47,851	47,851	70,111	70,111	57,396	57,396
R-Squared	0.771	0.771	0.763	0.763	0.787	0.787

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The even number columns show IV results where the instrument for "Boom HH" is "Boom HH in 1973." The omitted period is 1969-1974.

Table A.6: Regression Estimates of Effects on Total Income: Includes Separate Year Effects for Urban and Rural Areas

	Under 55		All Ages		Non-Retired	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Boom HH * 1975-1979	6,095***	4,757**	5,407***	4,355**	5,267***	4,334**
	(1,912)	(2,123)	(1,651)	(1,759)	(1,635)	(1,764)
Boom HH * 1980-1984	8,007***	7,464**	8,376***	7,372***	8,029***	7,530***
	(2,604)	(2,975)	(2,121)	(2,312)	(2,204)	(2,441)
Boom HH * 1985-1988	-6,234**	-6,912*	-417	-1,421	-5,347**	-6,467**
	(2,953)	(3,660)	(2,311)	(2,684)	(2,514)	(3,015)
Boom HH * 1989-1992	-5,916*	-7,893*	460	-936	-5,562*	-7,420**
	(3,452)	(4,064)	(2,629)	(2,976)	(2,958)	(3,414)
Observations	49,501	49,501	72,664	72,664	59,487	59,487
R-Squared	0.769	0.769	0.762	0.762	0.785	0.785

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head and state fixed effects, separate year fixed effects for urban and rural areas, and controls for family size and marriage. The even number columns show IV results where the instrument for “Boom HH” is “Boom HH in 1973.” The omitted period is 1969-1974.

Table A.7: Regression Estimates of Effects on Total Income: Excludes State Fixed Effects

	Under 55		All Ages		Non-Retired	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Boom HH * 1975-1979	5,908***	4,939**	5,047***	4,441**	5,050***	4,384**
	(1,905)	(2,104)	(1,641)	(1,754)	(1,620)	(1,747)
Boom HH * 1980-1984	6,958***	6,826**	7,347***	6,842***	7,000***	6,727***
	(2,585)	(2,988)	(2,098)	(2,298)	(2,175)	(2,425)
Boom HH * 1985-1988	-7,699***	-8,234**	-2,043	-2,796	-7,102***	-8,211***
	(2,912)	(3,734)	(2,270)	(2,692)	(2,462)	(3,041)
Boom HH * 1989-1992	-6,721**	-8,909**	-300	-1,557	-6,569**	-8,669**
	(3,422)	(4,178)	(2,616)	(3,015)	(2,923)	(3,474)
Observations	49,501	49,501	72,664	72,664	59,487	59,487
R-Squared	0.766	0.766	0.758	0.758	0.782	0.782

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head and year fixed effects and controls for family size and marriage. The even number columns show IV results where the instrument for "Boom HH" is "Boom HH in 1973." The omitted period is 1969-1974.

Table A.8: Regression Estimates of Effects on Total Income: Excludes Observations from Households that Changed Counties During the Sample

	Under 55 (1)	All Ages (2)	Non-Retired (3)
Boom HH * 1975-1979	4,413*	3,735*	4,082**
	(2,264)	(1,913)	(1,775)
Boom HH * 1980-1984	5,067*	5,203**	5,268**
	(2,994)	(2,292)	(2,344)
Boom HH * 1985-1988	-9,017**	-2,806	-7,714***
	(3,794)	(2,627)	(2,918)
Boom HH * 1989-1992	-12,444***	-3,468	-10,335***
	(4,187)	(2,988)	(3,431)
Observations	28,305	43,692	36,595
R-Squared	0.804	0.786	0.812

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. All models show OLS results (in this sample, the OLS estimates are identical to the IV estimates). The omitted period is 1969-1974.

Table A.9: Regression Estimates of Effects on Total Income: Adjusts for Inflation Using Regional CPIs

	Under 55		All Ages		Non-Retired	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Boom HH * 1975-1979	5,538***	4,121**	4,780***	3,732**	4,628***	3,634**
	(1,817)	(1,998)	(1,583)	(1,674)	(1,548)	(1,657)
Boom HH * 1980-1984	6,267**	5,612**	6,686***	5,740***	6,102***	5,539**
	(2,486)	(2,833)	(2,028)	(2,196)	(2,090)	(2,308)
Boom HH * 1985-1988	-7,911***	-8,805**	-2,475	-3,620	-7,449***	-8,824***
	(2,879)	(3,633)	(2,237)	(2,623)	(2,417)	(2,946)
Boom HH * 1989-1992	-6,649*	-8,743**	-509	-1,855	-6,666**	-8,595**
	(3,402)	(4,030)	(2,582)	(2,925)	(2,889)	(3,347)
Observations	49,501	49,501	72,664	72,664	59,487	59,487
R-Squared	0.769	0.057	0.762	0.046	0.785	0.052

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The second column shows IV results where the instrument for “Boom HH” is “Boom HH in 1973.” The omitted period is 1969-1974.

A.4 Cumulative Effects on Total Income Under Alternative Scenarios

Table A.10: Regression Estimates of Effects on Total Income: Limits Sample to Young Cohort of Household Heads 35 Years or Younger in 1970

	OLS	IV
	(1)	(2)
Boom HH * 1975-1979	6,965***	5,893**
	(2,287)	(2,689)
Boom HH * 1980-1984	7,552**	6,258*
	(3,096)	(3,633)
Boom HH * 1985-1988	-9,012***	-10,571**
	(3,252)	(4,173)
Boom HH * 1989-1992	-6,685*	-9,128**
	(3,693)	(4,437)
Observations	34,957	34,957
R-Squared	0.736	0.736

Notes: The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The second column shows IV results where the instrument for "Boom HH" is "Boom HH in 1973." The omitted period is 1969-1974.

Table A.11: Cumulative Effects on Total Income - Young Cohort

Discount Rate	OLS	IV
0	**9,798**	**-18,042**
1	14,294	-10,935
2	17,981	-4,967
3	20,990	42

Notes: Cumulative effects are based on annual income effects reported in Table A.10. Discounting is annual, beginning in 1975 which represents t = 0.

Table A.12: Cumulative Effects on Total Income - Inflation Adjusted Using Regional CPIs

	Under 55		All Ages		Non-Retired	
Discount Rate	OLS	IV	OLS	IV	OLS	IV
0	**789**	**-21,527**	**57,329**	**47,358**	**-2,806**	**-23,813**
1	5,360	-15,014	54,614	45,064	1,720	-17,303
2	9,142	-9,540	52,094	42,936	5,470	-11,825
3	12,260	-4,941	49,752	40,959	8,570	-7,215

Notes: Cumulative effects are based on annual income effects reported in Table A.9. Discounting is annual, beginning in 1975 which represents t = 0.

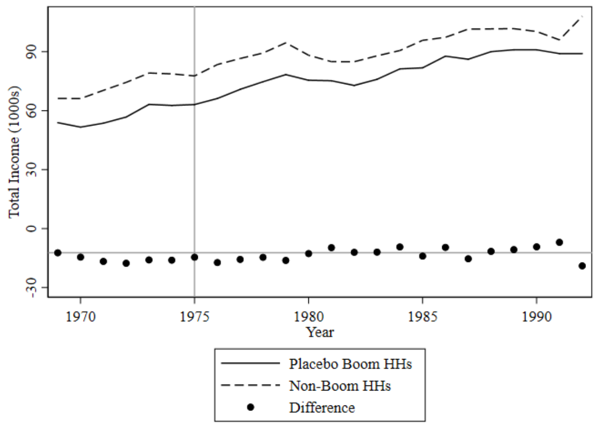
A.5 Functional Form

The analysis requires choosing whether to model boom-and-bust effects using a level scale (i.e. measuring income in dollar amounts) or logarithmic scale (i.e. measure the natural log on income). A key benefit of modeling income in levels, rather than log points, is that it is compatible with using a dependent variable that includes zero values, which is the case for income due to unemployment, retirement, and unprofitable household businesses. It is possible, however, that measuring income effects in levels may not be the appropriate functional form. The choice to model income changes in level versus percentage terms could be consequential in our setting because the average boom household had lower income at the beginning of the sample relative to non-boom households. If temporal shocks to within-household income variation operate on a percentage scale , and we model them using a level scale (or vice versa), then our regression models would be specified incorrectly.

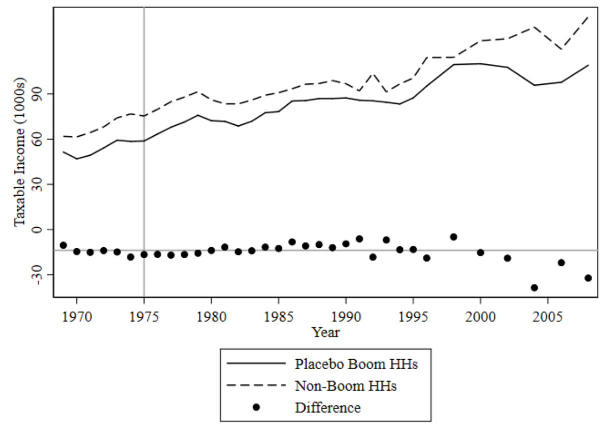
To determine appropriate functional form given the average income differences, we conduct a placebo analysis that splits *non-boom* households into two groups: those with above-average incomes and those with below-average incomes. Next, we identify a subsample of the households for whom the pre-boom difference in means for the above-average and below-average groups are equivalent to the pre-boom difference in means between boom and non-boom households.[44] We then code the below-average households as "boom households" and above-average households as "non-boom households." Using this sample, we produce placebo graphs of means and estimates of changes in income across years. As shown in Figure A.1, the placebo treatment and control groups experience very similar changes in means, when measured as levels, over time. Similarly, as shown in Table A.13, these two groups experience similar changes over the course of the sample when income is measured in level terms, as can be inferred by the insignificant coefficients on all of the interaction terms. These placebo results indicate that within-household changes in income across time primarily operate in level terms in our sample. For this reason, combined with the fact that many income observations have a zero value, we conduct the analysis modeling income in levels.

While we believe modeling income in levels is appropriate in our setting, we also investigate the sensitivity of our model to the inverse hyperbolic since (IHS). Unlike using the log of income, the IHS transformation is defined at zero values and thus allows us to include all observations. Table A.14 reports the estimation results. While these IHS results are a bit noisier, they are qualitatively similar to our primary income estimates reported in Table 3. In the All Ages sample, we document positive effects during the boom period and small insignificant effects during the bust and post-bust periods. In the Under 55 and Non-Retired samples, we estimate sharp positive boom effects yet negative bust and post-bust effects that are larger in absolute value than the positive boom-era estimates. While the negative effects in the bust and post-bust effects are only on the cusp of statistical significance in the Under 55 sample, they are significant in the Non-Retired sample.

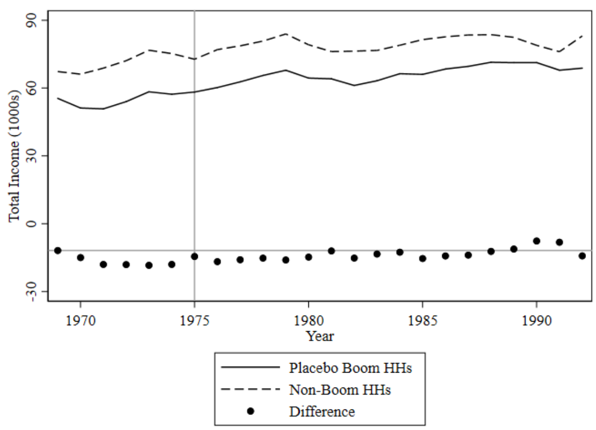
[44]To select the subsample, we first require non-boom households to have a mean pre-boom income that is within an arbitrarily small, symmetric bandwidth of the overall pre-boom sample mean (the pre-boom sample mean is about $60,000) in order to be included in the subsample. We then expand the bandwidth symmetrically by $100 until the difference within the subsample between mean incomes for above-average households and mean incomes for below-average households reaches the pre-boom difference in mean incomes for boom and non-boom households. This occurs at a bandwidth of about plus-or-minus $15,000 and leaves between 400 and 600 households on either side of the sample mean, depending on the outcome (total vs. taxable income) and age restriction (Under 55 vs. All Ages).



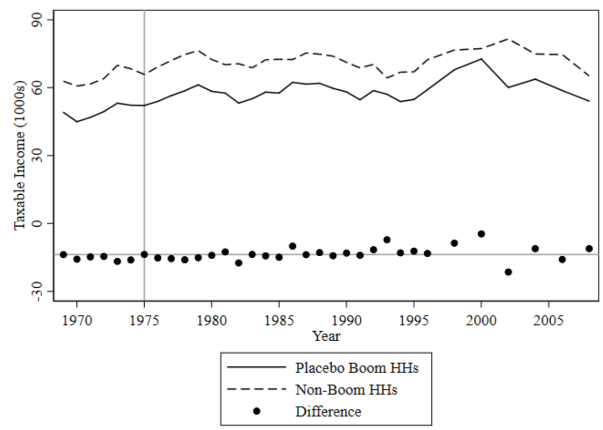
(a) Total Income, Under 55



(b) Taxable Income, Under 55



(c) Total Income, All Ages



(d) Taxable Income, All Ages

Figure A.1: **Investigating Functional Form: Placebo Trends in Mean Income.** Sample limited to a subsample of control households (see text earlier in this subsection for details on sample selection); no treatment households are included. Households with below-average pre-boom incomes are falsely coded as boom households. The vertical line corresponds to the beginning of the energy boom and the dots corresponds to differences in mean incomes relative to the first year of the sample, which is 1969. The second phrase in each label describes the sample.

Table A.13: Investigating Functional Form: Placebo Estimates of Effects on Total Income

	Total Income		Taxable Income	
	Under 55	All	Under 55	All
	(1)	(2)	(3)	(4)
Placebo Boom HH * 1975-1979	-341	-1,054	-1,387	878
	(1,583)	(1,338)	(1,675)	(1,665)
Placebo Boom HH * 1980-1984	1,273	1,359	1,514	2,043
	(2,372)	(1,849)	(2,505)	(2,327)
Placebo Boom HH * 1985-1988	-684	-30	2,514	3,226
	(3,387)	(2,501)	(3,505)	(3,044)
Placebo Boom HH * 1989-1992	548	3,732	1,696	4,295
	(3,527)	(2,689)	(3,759)	(3,249)
Placebo Boom HH * 1993-1999			1,544	5,171
			(4,953)	(3,813)
Placebo Boom HH * 2000-2012			-8,388	7,926
			(9,190)	(5,238)
Observations	15,037	21,831	15,829	23,479
R-Squared	0.534	0.518	0.526	0.526

Notes: Sample limited to a subsample of control households (see text earlier in this subsection for details on sample selection); no treatment households are included. Households with below-average pre-boom incomes are falsely coded as boom households. The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The omitted period is 1969-1974.

Table A.14: Investigating Functional Form: Inverse Hyperbolic Sine

	Under 55		All Ages		Non-Retired	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Boom HH * 1975-1979	0.128***	0.140***	0.078**	0.103***	0.081**	0.109***
	(0.037)	(0.040)	(0.035)	(0.034)	(0.036)	(0.034)
Boom HH * 1980-1984	0.165***	0.198***	0.110**	0.119**	0.121***	0.130***
	(0.056)	(0.059)	(0.045)	(0.047)	(0.045)	(0.050)
Boom HH * 1985-1988	-0.127	-0.143	-0.059	-0.079	-0.122*	-0.150*
	(0.093)	(0.110)	(0.061)	(0.069)	(0.071)	(0.083)
Boom HH * 1989-1992	-0.208	-0.224	-0.117	-0.143	-0.197*	-0.230*
	(0.138)	(0.162)	(0.085)	(0.096)	(0.106)	(0.122)
Observations	49,501	49,501	72,664	72,664	59487	59,487
R-Squared	0.641	0.028	0.657	0.024	0.668	0.024

Notes: The models are analagous to our primary models reported in Table 3 except the dependent variable is transformed using the inverse hyperbolic sine. The standard errors (in parentheses) are clustered at the household head level. * p<0.1 ** p<0.05 *** p<0.01. All models include household head, state, and year fixed effects and controls for family size and marriage. The omitted period is 1969-1974.

A.6 Channels and Auxiliary Evidence

Through which channels did the boom and bust affect income? In Table A.15, we evaluate boom and bust effects in separate categories dominated by labor income, business income, and retirement income. We also examine unemployment and wage rates to investigate the effects on labor income in detail. To summarize, we find that strong gains in labor income during the boom period (via higher wage rates) easily offset smaller boom-period losses in household business and investment income.[45] During the early bust, gains in labor income turned negative (due to higher unemployment) and were compounded by losses in business and investment income for the average household that persisted during the late bust. We also find evidence that retirement and transfer income significantly decreased for

[45]The negative boom period effect on business and investment income is potentially explained by 1) rising input costs for small business owners (which is evidenced by the rise in wage rates), 2) household heads being drawn out of proprietorship into employment based on attractive boom period wage rates, and 3) an over-exuberant increase in business investment during the boom period that outpaced revenue gains.

boom households during the late bust (1989-1992), relative to non-boom households, in the All Ages sample. By contrast, there was no effect in the Under 55 sample. In combination with our earlier findings related to delayed retirement, this pattern likely reflects older individuals extending their working years, thereby delaying receipt of retirement income.

Table A.15: Regression Estimates of Effects on Different Channels

	Labor Income		Wage Rate		Unemployment		Trans. & Retrmnt Inc.		Non-Labor Income	
	Under 55	All Ages	Under 55	All Ages	Under 55	All Ages	Under 55	All Ages	Under 55	All Ages
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Boom HH * 1975-1979	5,680***	5,603***	1.84***	1.95***	-0.011	-0.007	264	124	-1,147	-1,019
	(2,078)	(1,860)	(0.68)	(0.67)	(0.009)	(0.006)	(378)	(470)	(846)	(697)
Boom HH * 1980-1984	9,479***	8,874***	4.03***	3.58***	0.004	0.001	327	-103	-2,940***	-1,735**
	(2,781)	(2,494)	(1.08)	(0.92)	(0.015)	(0.009)	(480)	(707)	(918)	(861)
Boom HH * 1985-1988	-6,618*	655	0.00	-0.26	0.039*	0.026**	493	-966	-829	-1,826
	(3,524)	(2,959)	(1.30)	(1.10)	(0.020)	(0.012)	(618)	(868)	(1,888)	(1,417)
Boom HH * 1989-1992	-4,991	4,508	0.41	0.26	-0.000	-0.001	-377	-3,187***	-1,985*	-2,249**
	(4,070)	(3,487)	(1.52)	(1.45)	(0.018)	(0.011)	(721)	(1,039)	(1,196)	(991)
Boom HH * 1993-1999	-2,577	6,069	0.67	0.06	0.009	-0.000			-3,309	-2,361*
	(5,346)	(3,965)	(1.78)	(1.53)	(0.019)	(0.009)			(2,154)	(1,343)
Boom HH * 2000-2012	-7,553	16,209**	3.47	4.06	0.018	0.007			-2,049	-4,146
	(8,792)	(8,136)	(3.90)	(3.97)	(0.029)	(0.014)			(5,014)	(2,716)
Observations	55,265	88,833	49,173	65,303	52,565	85,448	49,758	72,989	55,198	88,694
R-Squared	0.750	0.645	0.650	0.587	0.220	0.200	0.473	0.536	0.423	0.466

Notes: The standard errors (in parentheses) are clustered at the household head level. * $p<0.1$ ** $p<0.05$ *** $p<0.01$. All models include household head, state, and year fixed effects and controls for family size and marriage. All columns show IV results where the instrument for "Boom HH" is "Boom HH in 1973." The omitted period is 1969-1974.