Measuring Wealth Effects Using U.S. State Data

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Abstract

This paper describes a new panel dataset of financial wealth for U.S. states constructed from anonymous proprietary account-level records on geographic wealth holdings. The new data set is more comprehensive and representative than existing alternative measures. The paper also constructs significantly improved state-level consumption data, then combines these datasets to provide new estimates of effects on consumption from changes in stock and housing wealth. I find large but sluggish housing wealth effects. The estimated response of consumption to a one dollar change in housing wealth that happened two years ago is in the neighborhood of 6.5 cents. Surprisingly, the data show no evidence for significant stock wealth effects, although large standard errors mean that the differences from housing wealth effects are statistically insignificant.

Keywords: consumption, wealth effect, endogeneity, panel data
JEL Classification: E21, C22, C33

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†I am also very appreciative of IXI Corporation for permitting me to create and publish the stock wealth data at the state level. The data will be available upon request at www.ixicorp.com/academicdata. All other data used in the empirical work in this paper will be made publicly available at www.econ.jhu.edu/people/zhoux when the final version of the paper is completed.

1 Introduction

The skyrocketing stock market boosted the wealth holdings of American households during the second half of the 1990s. Over that period, the personal saving rate dropped from about 8 to 2 percent. This so-called savings rate puzzle has sparked renewed policy and research interests in wealth effects on consumption. Figure 1 shows a relatively stylized negative correlation between saving rate and net worth to income ratio, implying a positive correlation between wealth and consumption after controlling for the income effect.

If it is the rise in wealth that drives down the personal saving rate, then we should expect that any future variations in wealth will have an impact on consumption. Therefore, wealth effects should be taken into consideration when monetary policy is to be implemented. However, we should be skeptical about the seemingly obvious relationship between consumption and wealth for a variety of reasons. First, the association we observe in Figure 1 could be mainly a result of simultaneity. For instance, a shock to consumers' optimism or pessimism could have an impact on housing price, stock price, and consumption growth in the same direction. Second, endogeneity could also be triggered by reverse causality from consumption on wealth. Aggregation is another problem in the presence of heterogeneity since distribution matters and summing over individuals might not lead to a representative consumer. In addition, measurement error could lead to unreliable associations. To give an example, assume income Y is measured with error. Then by construction, personal saving rate $s = 1 - C/Y$ is mismeasured in the same direction of Y, yet the measured wealth-income ratio W/Y is biased in the opposite direction. The measurement error in income therefore induces a negative correlation between saving rate and worth-income ratio.

A majority of the current literature on wealth effects employs either aggregate or household level data. Studies using aggregate data are subject to endogeneity and aggregation problems. On the other hand, studies using household-level data suffer from serious measurement error problems. There is in fact a very limited choice of micro data available for study. *The Panel Study on Income Dynamics* (PSID) only measures food consumption.

The Consumer Expenditure Survey (CEX) has detailed but noisy data on household expenditures, and poor financial information. *The Survey of Consumer Finances* (SCF) has no measure of consumption at all.

An alternative approach that potentially avoids some of the problems of both micro and macro data is to utilize regional variations. First, aggregation likely to be less of a problem when less aggregated data is used. Second, if there is enough variation across regions, the endogeneity problem could be better controlled. For instance, let us assume a region specific shock to consumers' confidence. It might still have a large impact on consumption behavior of households residing in the region. However, given a well integrated stock market, this region specific shock might not have as great impact on regional stock price as an aggregate shock could. Therefore, the endogeneity problem is alleviated to some extent. On the other hand, it can be argued that regional data has more comprehensive and better measures of variables important to the study than household-level data. Furthermore, regional data is more likely to cover a longer time period and therefore allows a study of richer dynamics.

Case, Quigley, and Shiller (2005) did pioneering work using U.S. state-level data to estimate and compare housing wealth effects and stock wealth effects. This paper extends their work in several aspects. A new panel dataset of financial wealth for U.S. states is constructed from anonymous proprietary account-level records on geographic wealth holdings. The new dataset is more comprehensive and representative than existing alternative measures. This paper also improves upon Case, Quigley, and Shiller (2005) by constructing significantly improved state-level proxy for consumption data. These datasets are then combined to provide new estimates of wealth effects on consumption from changes in stock and housing wealth. The paper is then organized as follows: Section 2 reviews the related literature; Section 3 discusses the limitations with currently available state-level consumption and stock wealth datasets; Section 4 describes the newly constructed data and compares it with U.S. aggregate series and other state data applied to the wealth effect studies; Section 5 presents the model specification and regression results and Section 6 contains my conclusions.

2 Recent Evidence

The current literature on marginal propensities to consume out of different components of wealth is limited. Davis and Palumbo (2001) compares the stock market wealth effect with the non-stock market wealth effect using U.S. aggregate data. The results from cointegration analysis are however sensitive to the model specification. Specifically, the long run effects of both wealth forms are about the same size (i.e., 0.06 for stocks, and 0.08 for non-stocks) when the level of variables is used. But after taking logarithms, the results show a non-stock wealth elasticity 4 times larger than the stock wealth elasticity, implying an MPC out of non-stock wealth at least twice as large as the MPC out of stock wealth. Also using aggregate data but applying a different method, Carroll, Otsuka, and Slacalek (2006) reports an immediate MPC out of housing wealth about 1.5 cents and an immediate stock wealth MPC of 0.75. But the difference is found statistically insignificant from zero.

To the best of my knowledge, Levin (1998) is the first study that uses micro data in the U.S. to estimate a differential effect for housing and stock wealth. That paper found essentially no effect of housing wealth on consumption using the Retirement History Survey. Out of eight spending categories, only three of them report a statistically significant difference between the coefficients of liquid and housing wealth. This finding contradicts those studies using macro data as summarized above. A possible reason could be the fact that every interviewee in the survey is at least 65 years old. If elder people tend to view housing wealth more as a consumption item instead of an investment item, then their housing wealth effect would be lower than otherwise. Using CEX and SCF, Bostic, Gabriel, and Painter (2005) find no evidence for important housing wealth effect in the all household sample. But among home owners, the housing wealth elasticity is consistently significant and larger than the stock wealth elasticity. The paper also suggests different consumption behaviors across credit-constrained and non credit-constrained samples.

Case, Quigley, and Shiller (2005) is probably the most cited paper using panel data in the current literature. By using quarterly U.S. state level data from 1982 to 1999, Case and his colleagues found a significant housing wealth elasticity of about 5 percent but an economically negligible stock wealth elasticity under most model specifications. When using a panel of annual data of 14 developed countries, they found a even larger housing wealth elasticity in the range of 11-15 percent. But there was still no evidence for important stock wealth effect under all cases. Bayoumi and Edison (2003) used data for 16 industrial countries and found significant wealth effects for most samples and periods. Their estimated housing wealth effect is consistently larger than the estimated equity wealth effect. Ludwig and Sløk. (2002) found evidence against the studies cited above. Using annual data from 16 OECD countries and taking housing prices and stock market prices as proxies for the wealth components respectively, the authors reported an estimate of stock wealth elasticity twice the size of housing wealth elasticity. In addition, both estimates were found positive and statistically significant. Girouard and Blöndal (2001) on the other hand, also used OECD data but could not get consistent results while comparing housing wealth vs. financial wealth. Dvornak and Kohler (2003) using Australian state-level data found a larger stock wealth effect than housing wealth effect.

3 Limitations With Existing State-Level Consumption and Stock Wealth Data

Case, Quigley, and Shiller (2005) constructed the only measure of quarterly state-level stock wealth for the U.S. from 1982 to 1999. They obtained annual information on mutual funds holdings at the state level which is only available for the years 1986, 1987, 1989, 1991 and 1993. In order to construct stock wealth, the authors had to make two very restrictive assumptions. First, they assume a constant proportion of mutual funds out of financial assets. Yet Figure 2 plots the proportion of mutual funds out of total stock wealth and shows an evident increase of the proportion. Second, they assume constant asset distribution across states for years in which mutual fund data were not available. Therefore during those years, the stock wealth of each state should, by construction, mimic the movement of aggregate stock wealth. In the absence of real wealth distribution across states, the data set they created is not "state-level" data at all.

To the best of my knowledge, there are three distinctive state-level consumption datasets used by Asdrubali, Sorensen, and Yosha (1996), Case, Quigley, and Shiller (2005), and Garrett, Hernàndez-Murillo, and Owyang (2004) respectively. Among them, only Case, Quigley, and Shiller (2005) apply the data to wealth effect examinations. The consumption data used by Asdrubali, Sorensen, and Yosha (1996) and Case, Quigley, and Shiller (2005) were retail sales constructed by different private sector sources. However, the data quality from both private sources is questionable for a variety of reasons. First, the methodology of the data construction was never explicitly revealed by either private source. Second, retail sales were presented for states that do not implement sales tax that is perhaps the single most important source of calculating state retail sales after the Census Bureau ceased its report of monthly retail sales by state since 1997. Last but not least, both sources vaguely mentioned that important state variables like wage and employment are incorporated into the estimation of retail sales. As a result, it will induce unreliable estimations of the relation between consumption and any variable that is correlated with wage or employment.

Garrett, Hernàndez-Murillo, and Owyang (2004) compute quarterly retail sales by dividing sales tax revenue by sales tax rate. The data is potentially a good measure of state retail sales and thus will be generally adopted in this paper. One problem with it, however, is the sales tax revenues are measured with serious errors, which results in unreasonable large consumption variations and apparent outliers. Therefore, this paper improves upon the data used in Garrett Et. Al.(2004) by constructing more accurate measures of state retail sales and by explicitly taking care of the outliers.

4 Data Description

This paper uses a panel dataset of 44 U.S. state data at a semiannual frequency from 2001 through 2005. The newly constructed datasets are stock wealth and consumption at the state level. Other important variables include after-tax labor income and housing wealth. All are expressed in real per capita terms. I make extensive efforts to compare the newly constructed series with those state-level data applied by other researchers in addition to the U.S. aggregate series. There is evidence that my data is more comprehensive and accurate than other existing alternatives. Some important findings will be discussed in the rest of this section.

4.1 Stock Wealth Data

I obtained anonymous account-level records on financial wealth holdings at the ZIP+4 Code level from IXI Corporation. At the end of each semiannual cycle, IXI collects data from more than 85 leading financial institutions in its network, IXI▶Net™. The reporting institutions include major banks, brokerage firms, insurance companies, and mutual funds dealers. More information can be found in the Appendix.

Stock market wealth is defined as the sum of directly and indirectly held (those invested in the form of IRA and Keogh accounts) stocks and mutual funds. The coverage of the IXI data on the national level is estimated in Table 1 where the Survey of Consumer Finances (SCF) of 2001[1] and the Flow of Funds Accounts (FFA)[2] at the end of 2001 are considered as benchmarks. The table indicates that the IXI data is estimated to cover more than 30 percent of the U.S. aggregate stock holdings and more than 50 percent of the aggregate mutual funds holdings. Along with my definition of stock market wealth, IXI is estimated to aggregate data of about 40 percent of U.S. total stock market assets held by U.S. households.

In order to answer the question of whether the IXI data constitutes a representative sample of the U.S. households, Figure 3 plots the growth rates of stock wealth as measured by IXI and FFA between 2000h2 and 2005h2. These two series move closely together with each other, giving reason to believe that the IXI data is representative of the nation as a

[1]SCF 2004 is the most recent version of the Survey of Consumer Finances. However, I could not compare it with IXI Corporation's data since starting with 2004, the SCF no longer reports money invested in retirement accounts by where it is invested. As a result, indirectly held stocks and mutual funds cannot be separated from other asset categories in the form of retirement accounts

[2]Thanks for Dr. Michael G. Palumbo who kindly provides me with detailed information about the components of each asset category in the FFA.

whole.

Stock wealth growth was constructed in a consistent method for all 50 states plus the District of Columbia.[3] The geographic distribution of stock wealth growth is plotted in Figure 4. The similar patterns across states as presented in the figure is expected because of a well integrated US stock market. However, whether the state heterogeneity manifested in the figure is a result of reality could not be readily answered since there exists no alternative state level wealth data that can be used to compare.

Nevertheless, there are some stylized facts about the U.S. that could help make the judgment. Florida and Arizona are the two states that have the highest percentage of retired people. As reflected in Figure 4, their seasonal patterns also distinguish themselves from other states. To better present the differences, Figure 6(a) and 6(b) compare the stock wealth growth of Florida and Arizona with the average of other states. Both figures indicate that both Florida and Arizona have a much higher stock wealth growth rate than other states at the second half of each year, and vice versa at the first half of each year. This phenomenon might seem strange at the first glance, but it is actually an outcome of the "snow-bird effect". In the U.S., retired people tend to move to Florida and Arizona in the winter and then move back to their permanent residence after the winter is over. By doing so, these old people also bring their assets around if they update their physical mailing addresses with their financial institutions.[4] Together with a single measure of population within a year, the "snow bird" effect should be fully captured by stock wealth growth at semiannual frequencies. Therefore, Figure 6(a) and 6(b) provide another piece of evidence that the heterogeneity as reflected by my data accords with the truth.

I made substantial efforts to find other potential financial resources at the state level to compare with my data. Bloomberg reports local stock indices for 22 states, the growth of which is expected to be positively but not perfectly correlated with local stock wealth growth. Figure 6 presents the correlation between local stock index and local stock wealth

[3]Details of the construction are included in the Appendix.

[4]Per IXI practice, the assets are now considered as belonging to the Zip Code +4 of the updated new address.

graphically. Out of the 22 calculated correlations, only 2 negative numbers are found. When looking at the idiosyncratic growth level, which is defined as the state growth subtracting off the U.S. national component, I still obtained 16 positive correlations. These facts further provide supporting evidence that my data gives the true distribution of stock market wealth across states .

4.2 Consumption Data

Since measures of personal consumption expenditures (PCE) at the state level are not available in the U.S., I use retail sales as the proxy for consumption. For the U.S., national retail sales accounts for roughly half of PCE, and *the Retail Trade Survey* is probably the single most important source for the national PCE estimation by the Bureau of Economic Analysis (BEA).[5] These considerations provide us with a rationale for the use of retail sales in place of consumption.

Unfortunately, even retail sales data is not directly available at the state level for the U.S. Following Garrett, Hernàndez-Murillo, and Owyang (2004), I obtained quarterly state-level general sales tax revenues from the *Quarterly Summary of State and Local Government Tax Revenue* published by the U.S. Census Bureau. Together with general sales tax rates collected from various sources,[6] state level retail sales were computed by dividing the state general sales tax revenue by the general sales tax rate. One limitation of this method is it can only be applied to 44 states plus District of Columbia.

To investigate whether the computed retail sales is a valid proxy for consumption, I construct my measure of national retail sales by summing over all the individual states and then compare it with the aggregate PCE recorded by the BEA. Table 2 shows that at the growth level, the correlation between my aggregate retail sales per capita and the national PCE per capita is positive and of moderate size.[7] More importantly, a regression of my national retail sales on national PCE indicates that a one unit increase in the growth

[5] See Wilcox (1992).

[6] State general sales tax rate could be found from various sources such as the State Government Tax Collections, and the Tax Foundation's Facts and Figures on Government Finances.

[7] The correlation at the levels of the two series is as high as 0.96.

of PCE is accompanied by approximately one unit increase in my constructed aggregate retail sales growth. This result implies that the movements of real consumption have been reflected by my retail sales data and thus justifies the use of my computed retail sales.

Strictly speaking, the computed retail sales are only a component of real retail sales since they exclude items that are either not subject to sales tax or in special tax programs like consumption of liquor and cigarettes. Furthermore, there is serious measurement error problem with the computed retail sales. Fortunately, I found state-level government-reported taxable retail sales for 10 states and government-reported retail sales for another two states.[8] These measures are more comprehensive than the computed retail sales because they either include all consumption items (as government-reported retail sales do) or at least include consumption items in special tax programs which altogether constitute about 25 percent of total sales tax revenue. Furthermore, these government-reported measures should be more accurate and reliable than the computed ones since the local governments have access to more information regarding their own sales tax system and tax collection practices than I do.[9]

Ideally, I should use government-reported (taxable) retail sales as the measure of consumption. However, since they are only available for limited number of states, I compile three sets of consumption data according to the quality of the retail sales data. The first one includes those 12 states that have government reported retail sales or taxable retail sales, and is considered the "Best Data". The second one is called the "All Data" which is the "Best Data" plus computed retail sales for other states. Finally, the "Good Data" is the "All Data" with outliers taken care of.[10]

4.3 Data from Other Sources

Other important variables used in this paper include quarterly after-tax labor income and housing wealth. After-tax labor income is calculated following Lettau and Ludvigson (2001).

[8]Data are obtained from the web site of corresponding state tax administrations.

[9]They are are either calculated by the local governments (like Virginia), or come directly from the reports on dealers' return (like Iowa).

[10]The method is described in the Appendix.

The formula to construct state level housing wealth is similar to the one adopted in Case, Quigley, and Shiller (2005) and is given as follows:

$$w^h_{i,t} = (HO_{i,t} * HH_{i,t}) * HPI_{i,t} * HV_i$$

where w^h_i is the value of owner occupied housing wealth for state i, HO is home ownership rate from the Census Bureau, HPI is the weighted repeat sales housing price index from the Office of Federal Housing Enterprise Oversight (OFHEO), and HV is the average home price in 1999 from Census 2000.

4.4 Data Issues

One important data issue arises here. As mentioned above, all variables except stock market wealth are all available on quarterly frequencies. To make them analogous to stock market wealth, I take their means over quarters throughout each half year and thus convert them into semiannual frequencies.

There are, however, evident and sizable seasonal patterns in the data set at the semiannual frequency, especially in the constructed consumption data. I made considerable efforts to attempt to remove them in a consistent fashion but could not achieve so at the semiannual frequency. It is largely because of the heterogeneity of seasonal patterns across states and the relatively short time horizon. Nevertheless, many state governments recommend using longer time spans for more reliable trends. One is warned that measures of taxable sales (or revenue) at higher frequencies may be misrepresentative for the purpose of comparison because of timing errors within a year. The above considerations suggest using annual growth rates to eliminate seasonal effects at cost of fewer observations and thus a reduced regression power.

Furthermore, to avoid the time aggregation problem, I did not use annual averages to calculate growth rates. Instead, I computed $\Delta c_{i,t}$ as the log difference between consumptions at the first half of year t and year $t-1$. The first half was chosen due to the consideration that state fiscal years end with June 30^{th}. It is arguable that data collected towards the end of a fiscal year would be more accurate than at any other time.

4.5 Another Look at My Data

Since this paper relies heavily on the two newly constructed data sets, before examining the wealth effects, I took another close look at my data by estimating the following equation.

$$\Delta c_{i,t} = \alpha_t + \beta_1 \Delta y_{i,t} + \beta_2 \Delta w^f_{i,t} + \beta_3 \Delta w^h_{i,t} + \varepsilon_{i,t} \tag{1}$$

where Δ denotes for growth rate of a variable, i.e., the log difference of the variable in real per capita terms. Equation 1 is a simple description of the data without taking simultaneity and aggregation problems into consideration. Table 3 reports the results for all three data sets. It shows that income growth is the one variable that consistently has the largest and most significant coefficient. Perhaps the most interesting finding is there is evidence that consumption is positively correlated with growth rates of housing wealth and stock wealth when they are regressed separately, yet whenever income growth is included, their coefficients become much less significant with reduced size.

5 The Regressions

5.1 Wealth Effect Estimations

Most work in the current literature adopts regressions similar to Equation 1 to study the immediate response of consumption to wealth.[11] Such regressions, however, do not yield straightforward wealth effects since they only report the contemporaneous percentage correlation between consumption and wealth. Worse, the test of equal stock and housing wealth

[11]Cointegration analysis is another standard method adopted in the current literature to study long-term MPCs. But it is not allowed by my data due to the relatively short time horizon. In addition, cointegration analysis is intrinsically problematic. The most relevant problem in the context of income and wealth effect analysis is the requirement of the stability of cointegrating vectors, which in turn requires a stable saving rate. Yet this requirement is obviously opposite to what the data tells us as illustrated in Figure 1.

effects is not transparent.[12] In order to solve this problem, I adopt an approach similar to the one employed by Carroll, Otsuka, and Slacalek (2006) where the ratio of change in each variable to an initial level of after-tax labor income is used. That is, if we define

$$\Delta\tilde{c}_{i,t} = \frac{C_{i,t} - C_{i,t-1}}{Y_{i,0}}$$
$$\Delta\tilde{y}_{i,t} = \frac{Y_{i,t} - Y_{i,t-1}}{Y_{i,0}}$$
$$\Delta\tilde{w}^h_{i,t} = \frac{W^h_{i,t} - W^h_{i,t-1}}{Y_{i,0}}$$
$$\Delta\tilde{w}^f_{i,t} = \frac{(W^f_{i,t} - W^f_{i,t-1})}{Y_{i,0}}$$

where $Y_{i,0}$ is the state after-tax labor income at 2000h1, then the regression of

$$\Delta\tilde{c}_{i,t} = \alpha_t + \beta_1\Delta\tilde{y}_{i,t-1} + \beta_2\Delta\tilde{w}^f_{i,t-1} + \beta_3\Delta\tilde{w}^h_{i,t-1} + \Delta\tilde{\varepsilon}_t \tag{2}$$

potentially produce direct measures of MPC out of changes in housing wealth and stock wealth.

Similar to Equation 1, Equation 2 is subject to serious endogeneity problem and thus is considered as another simple data description. Table 4 indicates that under this model specification, income change is still the most correlated variable with consumption. But unlike what Table 3 presents, there is robust evidence for a significant association between changes in housing wealth and consumption.

According to the permanent income hypothesis (PIH), changes in consumption only respond to unexpected information. Thus, using once lagged variables could potentially solve the simultaneity problem to some extent. Nevertheless, time aggregation and measurement error could cause current consumption changes to be correlated with once lagged income and wealth changes even if the PIH holds true. Aggregation matters when the PIH holds in

[12] Yet one benefit of such estimations is to produce some results comparable to those in the current literature. For the purpose of comparisons, results of similar estimations are included in the Appendix of this paper.

continuous time and measures of consumption are time averages. In this situation, change in time averaged consumption will have a nonzero first order serial correlation which leads to nonzero correlations between changes in consumption and once lagged variables. It is also easy to prove that measurement errors in the level of consumption could cause measured consumption changes correlated with once lagged explanatory variables.[13] Because of above considerations, I employ the following equation to address the question of wealth effects[14]:

$$\Delta\tilde{c}_{i,t} = \alpha_t + \beta_1\Delta\tilde{y}_{i,t-2} + \beta_2\Delta\tilde{w}^f_{i,t-2} + \beta_3\Delta\tilde{w}^h_{i,t-2} + \Delta\tilde{\varepsilon}_t \tag{3}$$

Equation 3 employs twice-lagged independent variables and thus report MPCs out of changes in housing wealth and stock wealth occurred two periods ago.

There are, however, two minor modifications need to be done. First, what $C_{i,t}$ captures here is the taxable retail sales instead of real personal consumption for state i. Thus, the estimation of Equation 3 using my data actually yields the response of taxable retail sales to changes in wealth. To gauge the approximate change in real consumption, I assume the initial state consumption can be determined by $C^*_{i,0} = Y_{i,0} * \frac{C^*_0}{Y_0}$, where C^*_0 and Y_0 are respectively aggregate personal consumption expenditure and after-tax labor income. In addition, I assume the proportion of my measure of retail sales out of consumption is constant over time, i.e., $\frac{C_{i,t}}{C^*_{i,t}} = \frac{C_{i,0}}{C^*_{i,0}}$. Therefore, the change in state consumptions can be roughly measured by

$$\begin{aligned}(C^*_{i,t} - C^*_{i,t-1}) &= (C_{i,t} - C_{i,t-1}) * (\frac{C^*_{i,0}}{C_{i,0}}) \\ &= (C_{i,t} - C_{i,t-1}) * (\frac{C^*_0}{Y_0}\frac{Y_{i,0}}{C_{i,0}})\end{aligned}$$

[13]Assume $c_t = c_{t-1} + \varepsilon_t$ and $c_t = c^*_t + \upsilon_t$, where c_t is real consumption, c^*_t is the measured consumption and υ_t is the measurement error. Although the real consumption growth follows a random walk, the measured consumption growth $\Delta c^*_t = \varepsilon_t - (\upsilon_t - \upsilon_{t-1})$ which is correlated with once lagged information.

[14]IV regression is another commonly used method to solve endogeneity problem. However, variables lagged by two years would have weak explanatory power, especially for income and stock wealth growth. Thus it will lead us to another econometric issue: weak instruments.

The same problem arises for my measure of stock wealth. Thus, it is assumed that $\frac{W^f_{i,t}}{W^{f*}_{i,t}} = \frac{W^f_{IXI,0}}{W^f_{FFA,0}}$ for all states and time periods where $W^{f*}_{i,t}$ denotes for the real state stock wealth at time t.

Therefore, if we redefine

$$\Delta\tilde{c}_{i,t} = \frac{C_{i,t} - C_{i,t-1}}{Y_{i,0}} * (\frac{C^*_0}{Y_0}\frac{Y_{i,0}}{C_{i,0}}) \tag{4}$$

$$\Delta\tilde{w}^f_{i,t} = \frac{(W^f_{i,t} - W^f_{i,t-1})}{Y_{i,0}} * \frac{W^f_{FFA,0}}{W^f_{IXI,0}} \tag{5}$$

the regression of 3 would report approximate estimates of marginal propensity to consumption out of changes in housing wealth and stock wealth.

Table 5 summarizes the results from estimating Equation 3. It indicates that the "Best Data" produces relatively different results from the "All Data" and "Good Data". Both wealth effects estimated using the "Best Data" are found to be statistically insignificant from zero despite their respective non-negligible size.[15] Additionally, large standard errors mean that the difference between the housing wealth and stock wealth effect is statistically insignificant, though a much larger point estimate of housing wealth effect is presented.

In contrast, the estimations using the "All Data" and the "Good Data" find a highly significant housing wealth effect of about 6.5 cents. It is surprising that we have such sluggish yet sizable housing wealth effects on consumption. Possible explanations could be sticky information or adjustment costs of housing wealth. There is still no evidence for significant MPCs out of stock wealth occurred 2 years ago. This finding is consistent with Dynan and Maki (2001) who find the impact of stock wealth on consumption shows up very quickly, and any lagged changes in stock wealth beyond 9 months could not have significant effect on consumption. The difference between housing and stock wealth effects is found to be significant by using the "Good Data", and on the verge of significancy (0.102) by using the "All Data".

[15]To the contrary, as Table A-1 in the Appendix indicates, a customary estimation of wealth effects actually implies a large and highly statistically significant housing wealth effect.

It is unfortunate that the overall evidence on a housing wealth effect that is larger than the stock wealth effect is mixed. But in the presence of the consistently larger point estimates of housing wealth effect, we should still be alert to the potentially different impacts on consumptions from movements in the housing and stock markets. Monetary policy makers should also take this possibility into consideration whenever a policy is to be implemented and a macroeconomic forecast is to be made.

5.2 A Habit Formation Test

The above estimations only report the relatively immediate responses of consumption to wealth changes. In the absence of cointegration analysis, I intend to apply a different method proposed by Carroll, Otsuka, and Slacalek (2006) to derive "long term" wealth effects. The basic idea is if there is evidence for habit formation, then consumption growth will be serially correlated. Thus, any impact on consumption from wealth changes could be delivered in the very long run through the serial correlation of consumption growth. Then the long run wealth effect could be derived by the short run wealth effect divided by one minus the habit formation coefficient. Following this literature, I employ the following equation as a habit formation test.

$$\Delta\tilde{c}_{i,t} = \alpha_t + \lambda E_{t-2}\Delta\tilde{c}_{i,t-1} \tag{6}$$

Table 6 reports the estimations from Equation 6. The results do not produce evidence for the existence of habit formation using currently available state-level instruments.[16] This could be because of the short time horizon of my data. Therefore the estimation of habit formation remains as an interesting topic for my future research.

[16] I tried many other IV sets but also failed to get positive and significant habit formation coefficient with reasonable first stage results.

6 Conclusion

This paper describes a new panel dataset of financial wealth for U.S. states constructed from anonymous proprietary account-level records on geographic wealth holdings. The new data set is more comprehensive and representative than existing alternative measures. The paper also constructs significantly improved state-level consumption data, then combines these data sets to provide new estimates of effects on consumption from changes in stock and housing wealth. Two approaches are applied in the study to address the question. Consistent and strong evidence is provided for large but sluggish housing wealth effects. Taking results from the new approach as the benchmark, two out of three data sets indicate the MPC out of a one dollar change in two-year lagged housing wealth is about 6.5 cents. Surprisingly, a statistically insignificant and economically small stock wealth effect is found under almost all specifications. Nevertheless, the evidence on whether the housing wealth effect is significantly larger than the stock wealth effect is a bit mixed. The "Best Data" easily accepts the hypothesis of equal wealth effects, yet the "All Data" and the "Good Data" samples tend to give supporting evidence for a housing wealth effect that is larger than the stock wealth effect. These results, however, could still help explain the strength of consumption after the stock market bubble burst at the end of 1990s. Therefore, with respect to monetary policies, these results suggest that it is necessary to take into consideration the potentially substantial difference between consumers' reaction to fluctuations in housing markets and stock markets.

APPENDIX

A Stock Wealth Data

The data used in this paper were constructed during my part-time employment at IXI Corporation over the past two years. IXI informally permitted me to construct the stock wealth data at the U.S. state level. All IXI financial data feeds, through its network known as IXI▶Net^{TM}, are provided as anonymous, ZIP+4 Code data by product type. IXI receives absolutely no non-public, personally identifiable information on U.S. households. Once the ZIP+4 Code level data is received it is joined and averaged with other ZIP+4 Codes. The average value is then imputed back down to the ZIP+4 Codes participating in this specific averaging. The rules utilized to perform the joining and averaging are complex and based to some extent on geographical proximity. The most important factor is the requirement that there should be at least 7 households among the ZIP+4 Codes. Therefore, a ZIP+4 Code with less than 7 households will be joined with its neighbors to achieve the requirement.

Names of the financial institutions reporting to IXI are suppressed and cannot be revealed. More information on IXI can be found on their web site: www.ixicorp.com.

The number of institutions in IXI's network changes over time. Hence, my biggest concern with constructing the stock wealth data is the possibility that the variations in wealth are caused by reasons other than the wealth holding variations of the state residents. To minimize this problem, I expended a great deal of effort keeping track of all mergers and institutions' membership in the network. The formation of a consistent source of financial data over time was performed at the state level. Thus, I could construct a common group of reporting institutions for every TWO ADJACENT CYCLES. So the growth rates could be calculated as the log difference of two adjacent values on a rolling basis. Specifically speaking, for growth rate of stock wealth for state i at time t, I summed up the assets by those institutions reporting to IXI at both t and $t-1$ for state i, and then I took the log difference.

B Consumption Data

States that have no sales tax are Delaware, Montana, Oregon, New Hampshire, and Alaska. Nevada was excluded due to incomplete reporting of sales tax collections. Although the tax revenue data for Minnesota is complete, it is so noisy that the absolute value of its growth rate goes beyond 1 for several periods. I believe this high volatility is not reflecting the consumption behavior of Minnesota households. Rather, it is caused by measurement errors. Considering that these unreasonable growth rates are present for many periods, I dropped off Minnesota because I believe that the entire series of tax revenue data from Minnesota is sabotaged by measurement errors. As a result, my constructed consumption data set only includes 44 states.

The main difference between taxable retail sales and real retail sales is the exclusion of food (including alcoholic beverage) and tobacco consumptions. According to the BEA, they constitute about 15 percent of total personal consumption. There is no empirical evidence that food consumptions differ very much from other types of consumptions.

Basing on the characteristics of the "best" data, I implemented various algorithms to define and take care of outliers in the "all" data set. The results are all qualitatively and quantitatively similar. Here I apply the simplest one for the annual growth rates: outliers are those with absolute value being higher than 0.1, and they were replaced by the state average annual growth over the sample period.

C Results from Customary Estimations

I address the problem by estimating the following two equations:

$$\Delta c_{i,t} = \alpha_t + \beta_1 \Delta y_{i,t-2} + \beta_2 \Delta w^f_{i,t-2} + \beta_3 \Delta w^h_{i,t-2} + \varepsilon_{i,t} \tag{7}$$

Table A-1 reports results from Equation 7 respectively. The findings are roughly consistent across the three data sets.

The most outstanding and robust finding is the sizable and statistically significant coefficient of lagged housing wealth. Using the average ratio of aggregate consumption to housing wealth between 2001 through 2005, the estimations from the best data imply a rather large two-year lagged housing wealth effect of 12.72 cents. The "All Data" and the "Good Data" produced similar but smaller housing wealth effects about 7 cents. All these values are reasonable in the context of recent wealth effect literature.

Stock wealth effects reported in Table A-1 are all statistically insignificant. Furthermore, in 2 out of the 3 estimations, the size of stock wealth effect is economically small. The tests of whether we have equal housing wealth and stock wealth coefficients are however all accepted by relatively large margins except for one case.

Table A-1: $\Delta c_{i,t} = \alpha_t + \beta_1 \Delta y_{i,t-2} + \beta_2 \Delta w^f_{i,t-2} + \beta_3 \Delta w^h_{i,t-2} + \varepsilon_t$

	Best Data	All Data	Good Data
$\Delta y_{i,t-2}$	$.444^{***}$ (.280)	671^{**} (.312)	$.552^{**}$ (.225)
$\Delta w^f_{i,t-2}$	.309 (.275)	-.005 (.096)	-.031 (.075)
$\Delta w^h_{i,t-2}$	$.451^{**}$ (.189)	$.267^{**}$ (.116)	$.246^{***}$ (.091)
Test of β_2=β_3	0.685 (Accepted)	0.110 (Accepted)	0.038 (Rejected)
obs	24	88	88
$\bar{R}^2$	.385	.024	.099
Partial $\bar{R}^2$	.248	.032	.097

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Figure 1: Saving Rate vs. Net Worth - Income Ratio

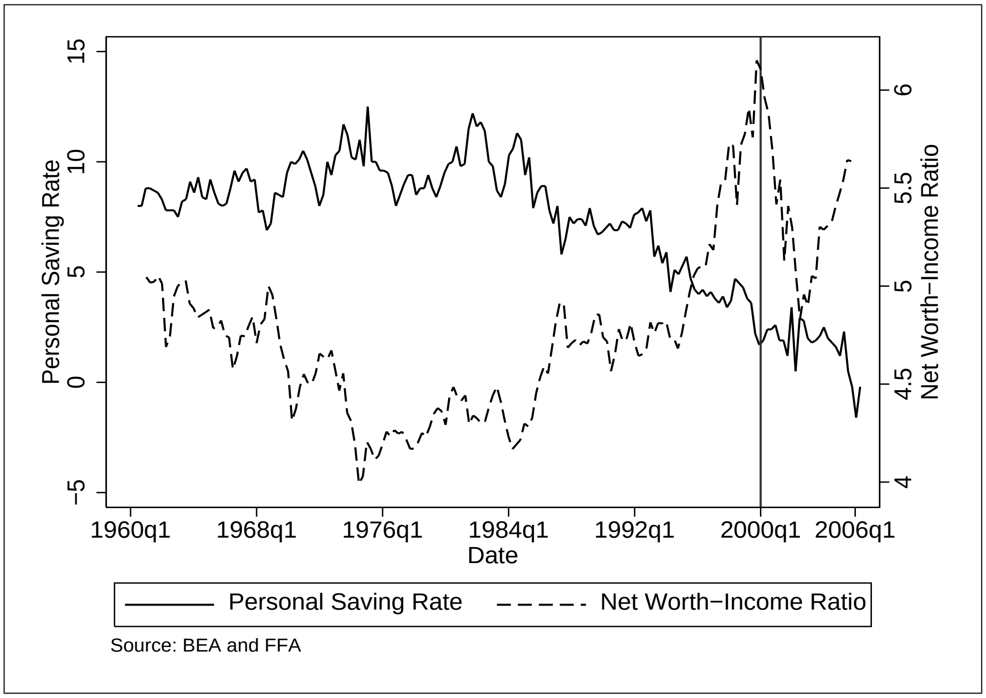


Figure 2: Mutual Funds vs. Total Stock Wealth

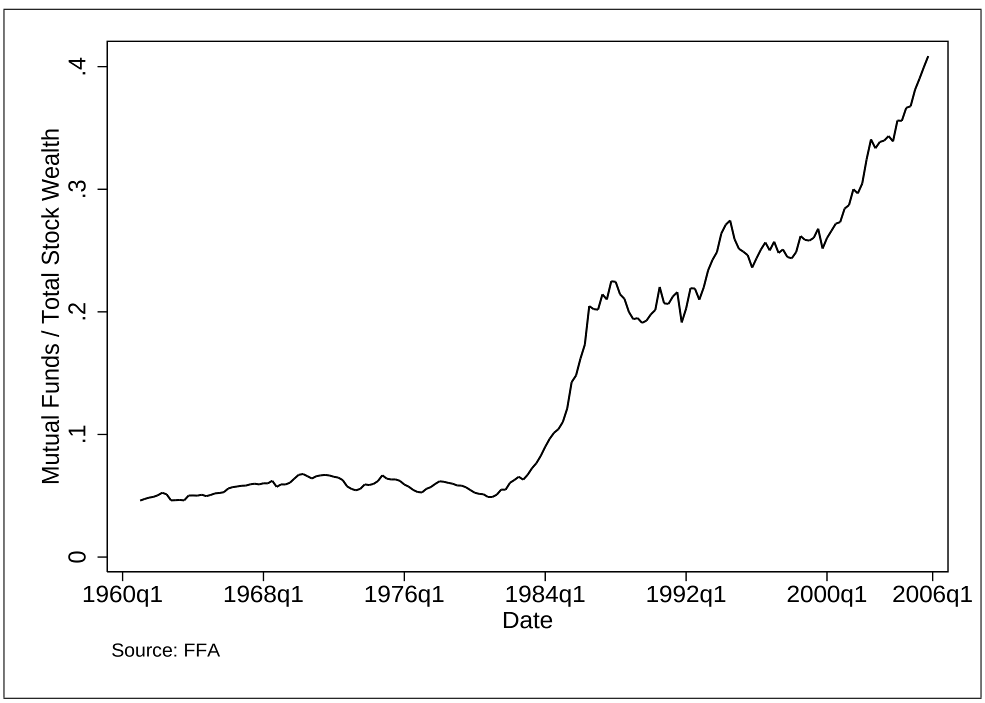


Figure 3: IXI vs. FFA

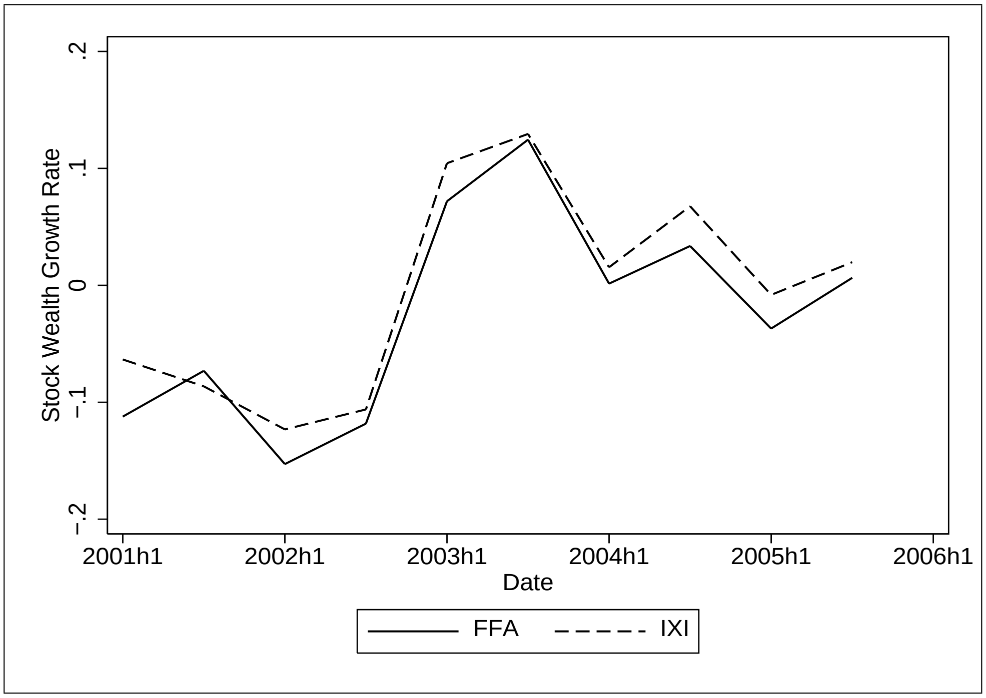


Figure 4: Wealth Growth Across States

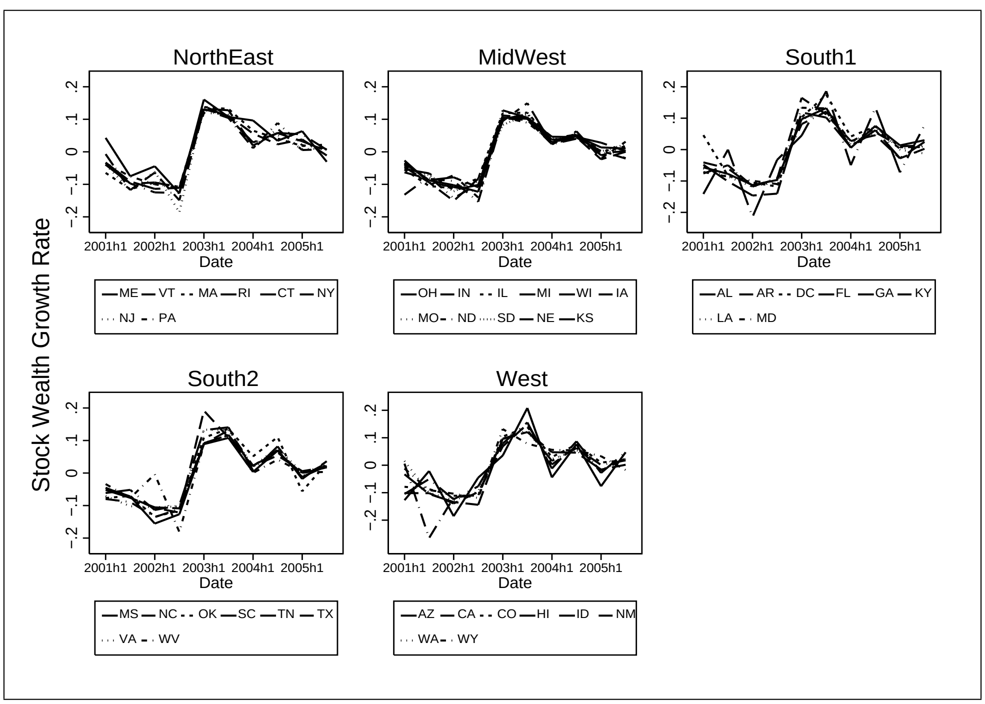
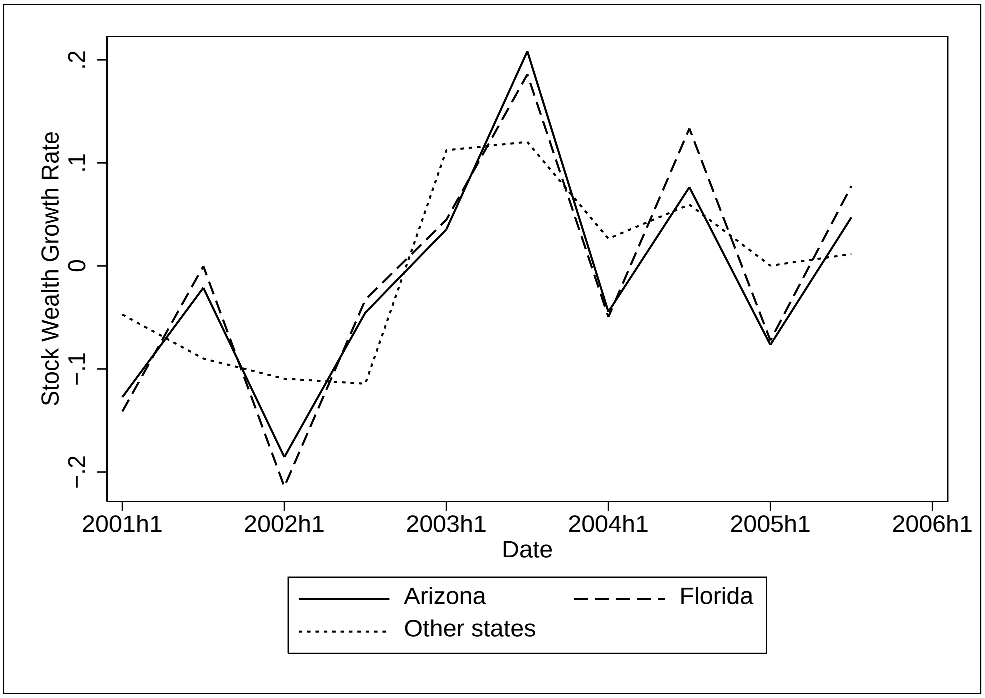
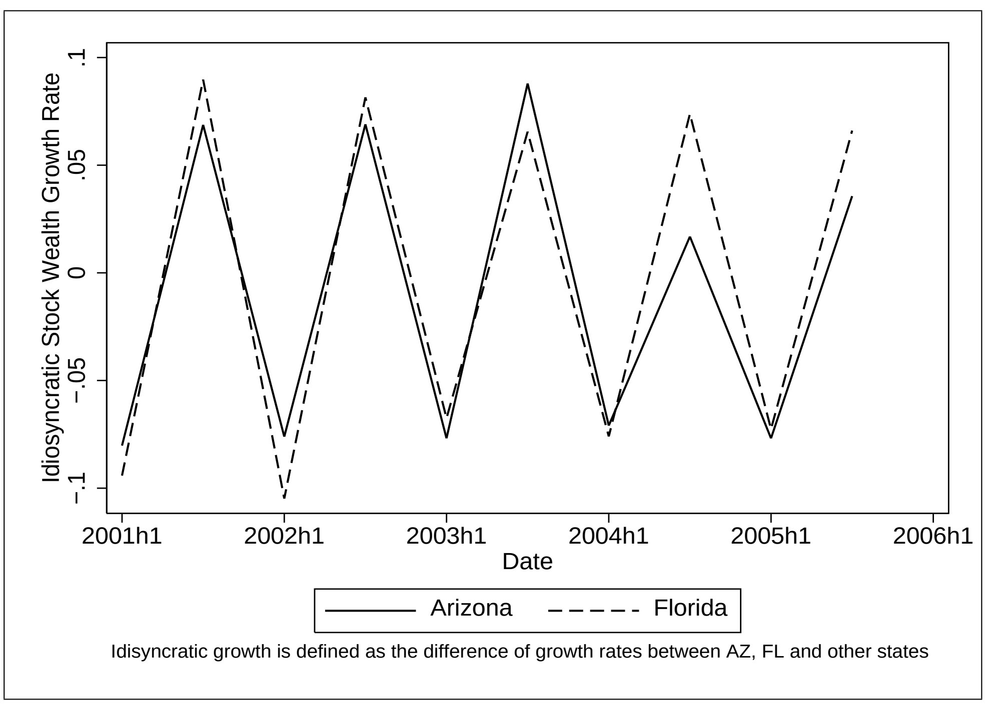


Figure 5: Snow Bird Effect

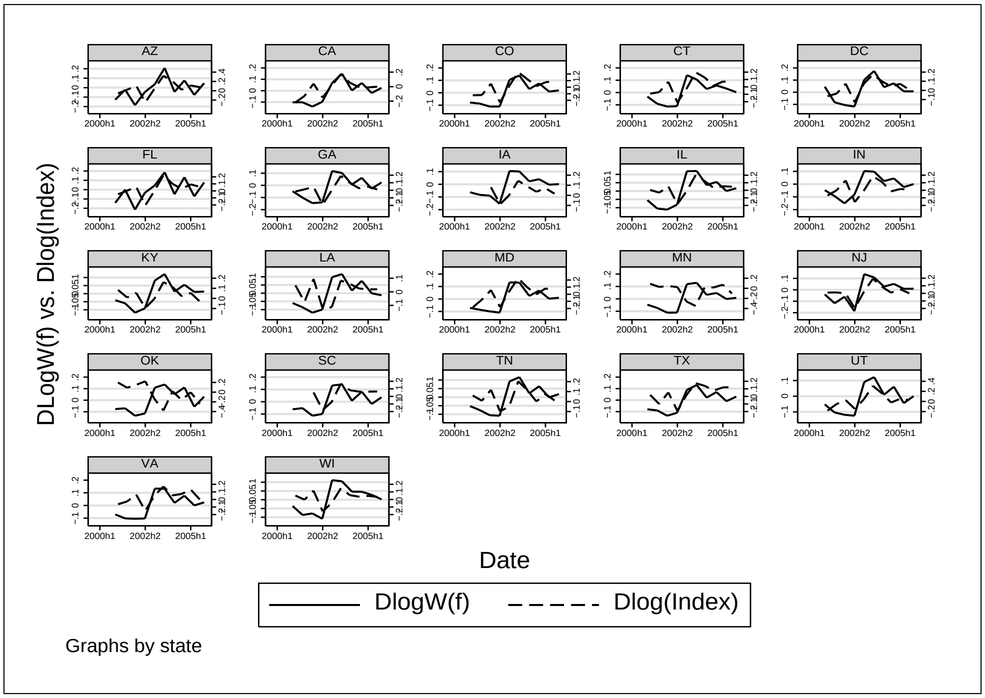


(a) Florida and Arizona vs. Average of Other States



(b) The Differences of Florida and Arizona from Other States

Figure 6: Local Stock Index vs. State Wealth



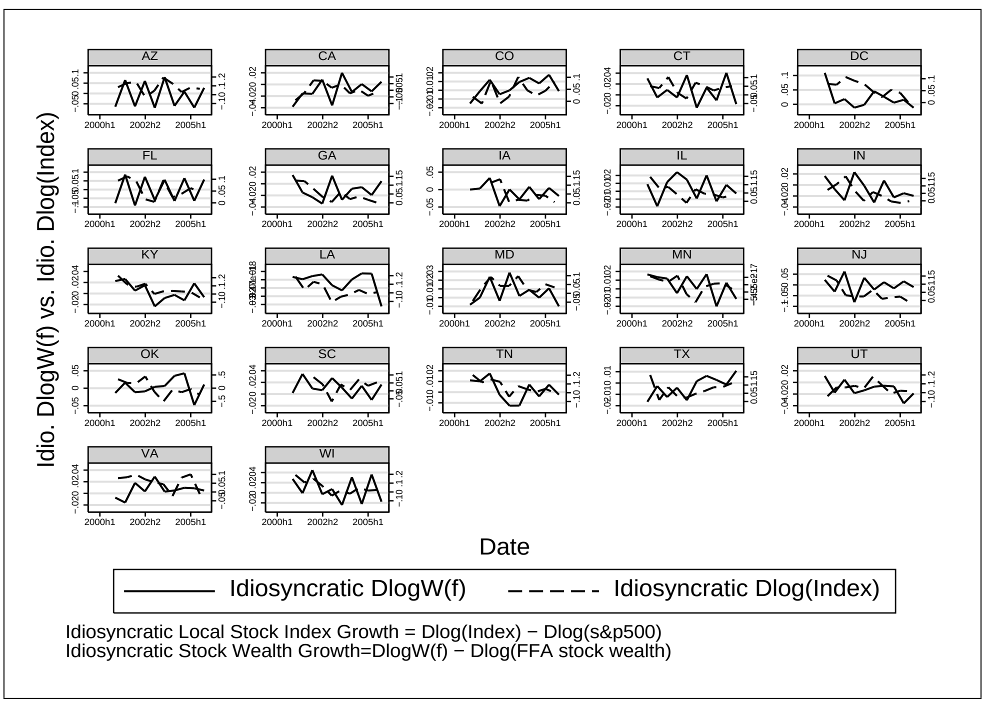


Table 1: IXI vs. SCF 2001 and FFA (trillions of dollars)

	SCF 2001	FFA 2001	IXI 2001h2[a]	IXI/SCF	IXI/FFA
Stocks	6.56	6.72	2.15	32.8%	31.9%
Mutual Funds	3.10	2.83	1.59	51.2%	56.2%
Total	9.66	9.56	3.74	38.7%	39.1%

[a]Sum of assets values reported by all institutions in the network at the end of 2001h2

Table 2: $\triangle$retail_sum vs. $\triangle$pce_us and $\triangle$retail_us

$$(1)\ \triangle\text{retail\_sum} = \alpha_1 + \beta_1 \triangle\text{pce\_us} + \varepsilon$$
$$(2)\ \triangle\text{retail\_sum} = \alpha_2 + \beta_2 \triangle\text{retail\_us} + \varepsilon$$

	Coef	Std. Err	t	$P > \lvert t \rvert$	[95% Conf.	Interval]
(1)	0.923	0.154	5.98	0.000	0.618	1.228
(2)	0.790	0.180	4.39	0.000	0.433	1.146

Table 3: Data Description: $\Delta c_{i,t} = \alpha_t + \beta_1 \Delta y_{i,t} + \beta_2 \Delta w^f_{i,t} + \beta_3 \Delta w^h_{i,t}$

Best Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta y_{i,t}$	.832***			.769***	.861***		.795***
	(.198)			(.197)	(.180)		(.173)
$\Delta w^f_{i,t}$		.348*		.259		.368**	.277
		(.179)		(.181)		(.173)	(.170)
$\Delta w^h_{i,t}$			.137**		.148**	.144**	.152**
			(.065)		(.059)	(.066)	(.061)
obs	48	48	48	48	48	48	48
$\bar{R}^2$	.713	.666	.670	.721	.743	.693	.755
Partial $\bar{R}^2$ [a]	.190	.056	.067	.215	.277	.135	.311

All Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta y_{i,t}$	1.816***			1.703**	1.770**		1.675**
	(.684)			(.704)	(.717)		(.731)
$\Delta w^f_{i,t}$		.419**		.328		.396**	.322
		(.193)		(.247)		(.193)	(.246)
$\Delta w^h_{i,t}$			.144**		.055	.114*	.035
			(.069)		(.076)	(.065)	(.070)
obs	176	176	176	176	176	176	176
$\overline{R}^2$	.204	.129	.101	.224	.201	.131	.22
Partial R^2	.126	.044	.013	.153	.128	.052	.153
Partial $\overline{R}^2$	.121	.038	.008	.143	.118	.041	.139

Good Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta y_{i,t}$	1.048***			1.025***	.977***		.961***
	(.217)			(.217)	(.216)		(.216)
$\Delta w^f_{i,t}$		.122		.066		.096	.054
		(.081)		(.078)		(.080)	(.078)
$\Delta w^h_{i,t}$			.133**		.084*	.126**	.080
			(.052)		(.050)	(.053)	(.050)
obs	176	176	176	176	176	176	176
$\overline{R}^2$	.328	.241	.259	.326	.334	.26	.331
Partial R^2	.124	.011	.034	.127	.136	.04	.138
Partial $\overline{R}^2$	.119	.005	.028	.117	.126	.029	.123

[a] The proportion of variance explained by all variables other than the year dummies.

Table 4: $\Delta\tilde{c}_{i,t} = \alpha_t + \beta_1\Delta\tilde{y}_{i,t} + \beta_2\Delta\tilde{w}^f_{i,t} + \beta_3\Delta\tilde{w}^h_{i,t}$

	Best Data	All Data	Good Data
$\Delta y_{i,t}$	.959*** (.249)	2.028** (.865)	1.293*** (.286)
$\Delta w^f_{i,t}$	.039 (.030)	.029 (.057)	−.009 (.020)
$\Delta w^h_{i,t}$	.019* (.010)	.027** (.012)	.019** (.009)
$\beta_2 = \beta_3$	0.494 (Accepted)	0.774 (Accepted)	0.262 (Accepted)
OBS	48	176	176
$\bar{R}^2$	.732	.214	.276
Partial $\bar{R}^2$	.124	.058	.120

Table 5: $\Delta\tilde{c}_{i,t} = \alpha_t + \beta_1\Delta\tilde{y}_{i,t-2} + \beta_2\Delta\tilde{w}^f_{i,t-2} + \beta_3\Delta\tilde{w}^h_{i,t-2}$

	Best Data	All Data	Good Data
$\Delta y_{i,t-2}$	.704* (.399)	1.027*** (..386)	.833*** (.298)
$\Delta w^f_{i,t-2}$	.019 (.046)	-0.009 (.034)	.002 (.021)
$\Delta w^h_{i,t-2}$	.084 (.051)	.064** (.026)	.065*** (.022)
$\beta_2 = \beta_3$	0.277 (Accepted)	0.102 (Accepted)	0.036 (Rejected)
OBS	24	88	88
$\bar{R}^2$	.244	.036	.091
Partial $\bar{R}^2$	.095	.042	.101

Table 6: Habit formation: $\Delta\tilde{c}_{i,t} = \alpha_t + \lambda E_{t-2}\Delta\tilde{c}_{i,t-1} + \varepsilon_t$

	Best Data	All Data	Good Data
$E_{t-2}\Delta\tilde{c}_{i,t-1}$ [a]	.656* (.379)	.079 (.283)	.318 (.201)
obs	24	88	88
$\bar{R}^2$	-.099	-.054	-.164
First Stage:			
Partial R^2	.383	.154	.151
$P - val$	.060	.002	.017

[a] IV: $\Delta\tilde{c}_{i,t-2}$, $\Delta\tilde{y}_{i,t-2}$, $\Delta\tilde{w}^f_{i,t-2}$, $\Delta\tilde{w}^h_{i,t-2}$.