

The 2011 Payroll Tax Cut and Household Spending - Evidence from a Quasi-Natural Experiment

By NAVEEN SINGHAL*

University of Illinois at Chicago (UIC)

Job Market Paper

This draft: November 7, 2014

Abstract

In 2011, the federal government reduced the payroll tax rate from 6.2 to 4.2 percent, while at the same time Illinois increased its state income tax rate from 3 to 5 percent. Consequently, Illinois workers were largely unaffected by these tax changes, but workers elsewhere experienced an increase in their take-home income. Using this variation in tax liability, I estimate that for every dollar of tax decrease, household spending increased by about 89 cents, especially on recreation, dining, vacations, clothing, and personal care. Additional analysis indicates that the estimates are unlikely to be biased from Illinois-specific shocks and may therefore be interpreted causally.

(JEL D12 E21 E62 H24 H31 H71 H55 J63)

* University of Illinois at Chicago, Department of Economics, 601 South Morgan Street, 725 University Hall (MC 144), Chicago, IL 60607 (e-mail: navsinghal@hotmail.com). I thank Professors Darren Lubotsky, Robert Kaestner, David Merriman, Ben Ost and Joseph Persky for guidance on this research. Nathan Anderson, Marcus Casey, George Karras, Derek Laing, Paul Pieper, Javaeria Qureshi, Steven Rivkin, Houston Stokes, and participants in several UIC seminars provided very useful feedback. Daniel Feenberg at the National Bureau of Economic Research (NBER) helped with the Taxsim program. Lorenz Kueng wrote the Taxsim code to be used for the Consumer Expenditure Survey data which I adapted to my needs. Anuj Gangopadhyaya helped with Current Population Survey data. Vera Crain and Scott Curtin at the Bureau of Labor Statistics patiently answered numerous queries related to the Consumer Expenditure Survey data. Donald Jones' keen editorial eyes improved my drafts multiple times. All of this help is gratefully acknowledged.

Do tax cuts stimulate spending? Although research in the past decade has greatly improved our understanding of the effects of tax cuts, a definite answer to this question still eludes us. A key obstacle in this regard is that both in levying taxes in general and in foregoing tax revenues during recessions, governments are committed to the principle of horizontal equity. They purposely design tax regimes to treat similar people similarly. This fact makes it rare to find the exogenous variation in tax liability of otherwise similar individuals that may enable a researcher to identify the causal effect of tax cuts on spending. This paper aims to take advantage of one such exogenous variation arising from mutually offsetting tax policy decisions of the US federal government and the state government of Illinois.

In 2011, as part of a broader package of programs to boost the economy, the federal government reduced employees' share of the payroll tax from 6.2 percent to 4.2 percent of wage income. Concurrently, the state of Illinois increased its flat income tax rate from 3 percent to 5 percent. These two tax changes were mutually offsetting for Illinois workers, while workers elsewhere benefited from the payroll tax cut. I use this exogenous variation in direct tax liability of arguably similar households to identify the effect of the payroll tax cut on household spending. To do this, I first estimate the change in households' tax liability between 2010 and 2011 using Consumer Expenditure Interview Survey data and NBER's Taxsim program. I then estimate the impact of a change in tax liability on households' consumption spending in a 2SLS model using residence in Illinois as an instrument for change in tax liability.

The estimated coefficients indicate that the 2011 payroll tax cut gave a considerable boost to consumption expenditures. With every dollar of decrease in tax liability, household spending increased by about 89 cents. All of the increase in household spending came from an increase in discretionary spending, which is easier to change in the short term. Within discretionary spending, spending

increased the most on recreation, dining, vacations, clothing and personal care. Somewhat surprisingly, discretionary medical expenditures also seemed to respond to the relatively small increase in disposable income.

The 2011 payroll tax cut was but one among the several tax rebates and credits provided by the federal government in the 2000s. In fact, as Figure 1 shows, tax relief has been the dominant policy response of the federal government to the Great Recession.¹ Tax cuts can, however, be efficacious in increasing aggregate demand only if taxpayers spend a large fraction of the additional disposable income. Studying previous tax rebates, several papers in the last decade have contributed to our understanding of whether this is true. A series of papers by Matthew Shapiro, Joel Slemrod and others have used survey responses in which the households were asked about their intention to save or spend any tax rebates to estimate the effect of tax rebates on consumer spending and to analyze possible sources of variations in consumers' response.² A second strand of literature pioneered by David Johnson, Jonathan Parker, Nicholas Souleles and others has relied on randomization of tax rebates by the Internal Revenue Service (IRS) in 2001 and in 2008 to identify the effect of tax rebates using micro-data on household expenditures.³ Most of these papers find moderate to large effects of the tax cuts, with the studies relying on incurred expenditures typically finding a

¹ Faced with a recession, governments have a choice to either directly spend on public goods and services, or spur private demand through tax cuts. Public spending has the potential to increase aggregate demand dollar for dollar or even more due to a multiplier effect. However, due to a number of problems associated with public spending - for instance, administrative delays, allocative inefficiency, and structural rigidities in the economy - many economists prefer that governments instead reduce personal taxes during recessions. See Alesina and Ardagna (2009) for a comparative study.

² Shapiro and Slemrod (2003a, 2003b, 2009); Sahm, Shapiro and Slemrod (2010, 2012); Souleles (2004). Also see Graziani, Klaauw and Zafar (2013); Coronado, Lupton and Sheiner (2005).

³ Johnson, Parker and Souleles (2006, 2009); Agarwal, Liu and Souleles (2007); Parker, Souleles, Johnson and McClelland (2013); Broda and Parker (2008); Misra and Surico (2011); Hamilton (2008); Bertrand and Morse (2009).

somewhat larger effect than those studying stated intentions of surveyed households.

The present paper contributes to the current literature on the effect of tax cuts in several ways. First, the tax cut studied in this paper was implemented by lowering withholdings on employees' paychecks as opposed to one-time tax rebates. To my knowledge, my paper is the first to study the consumption response to a broad-based change in marginal tax rates using survey data on incurred expenditures.

Second, I make a distinction between discretionary expenditures and non-discretionary expenditures. The former are the expenditures that a household is likely to find feasible to change soon after an increase in disposable income. The latter are the expenditures that are contractually fixed or are otherwise costly to change at short notice. The effect of a temporary tax cut measured with data generated soon after the tax cut should largely be seen in the discretionary expenditures. By demonstrating that the 2011 payroll tax cut indeed had a large effect on the discretionary expenditures but none on the non-discretionary expenditures, I bring additional credibility to the recent literature that indicates that tax cuts are effective in stimulating demand (Parker, Souleles, Johnson and McClelland, 2013).

Last, the quasi-natural experiment that I use allows me to address methodological concerns in the use of randomization of rebate checks to identify the effect of tax cuts. The IRS mailed the rebate checks in 2001 and in 2008 only to those households which had filed a tax return in the previous year. In the studies relying on randomization, the effect of tax rebates is estimated by comparing expenditures of the households which received the rebate check to those of the households which did not receive the check during the comparison period. However, a subset of the latter households did not receive the check not because of random chance but because they had not filed a tax return in the previous year. Such households are likely to be different – often younger or

poorer - than the households who did receive the rebate. In other words, randomization offered by the rebate checks is not perfect unless we exclude the households which did not file a tax return in the previous year.

Parker et al. (2013) address this concern by restricting the sample to only those households which received the 2008 rebate at some point or other. The resulting research design is impeccable but, unfortunately, it leads to another concern. When the effect on spending is estimated using only those households which received the rebate check sooner or later, strictly speaking, the estimated coefficient measures not the effect of a dollar reduction in taxes but of early receipt of a sure cash flow (Kaplan and Violante, 2011).

Using the quasi-natural experiment, I am able to avoid the concerns stated above. I compare the spending of households whose annual disposable incomes increased with the tax cut to the spending of those households whose annual incomes were unaffected by the tax changes. Moreover, since the payroll tax cut affected marginal tax rates, it potentially also affected work incentives. Consequently, the results in this paper are useful in extending our understanding of the likely effect of a tax cut in the presence of labor market adjustments.

Identification in this paper is not free from the risk of bias. The variation in tax liability is due to Illinois and US workers being exposed to differential policy shocks in 2011. This ought to affect their spending. However, Illinois workers might also have been subject to other economic shocks in 2011 unrelated to the tax change that might have affected them differently compared to workers elsewhere in the US. If true, this could bias the estimates. I address this concern with two additional types of analysis. First, I take advantage of the fact that the non-worker households in my data, most of whom are retirees, did not owe state income taxes in Illinois, and were therefore unaffected by the tax increase in 2011. I use these households as another comparison group to control for any uniform Illinois-wide shocks to consumption. I show that relative to Illinois

households, spending in the rest of the US increased only among the worker households and not among the non-worker households. As a further robustness check to my baseline results, I estimate an alternative 2SLS model in which worker households and non-worker households are pooled together, and an interaction dummy indicating a worker-resident-in-Illinois is used to instrument for change in tax liability. The results from this augmented model are almost identical with the key results in my main model, which indicates that there were no significant Illinois-wide shocks in 2011 that uniformly affected workers and non-workers in Illinois but not elsewhere.

This still leaves open the possibility that the estimates in this paper may be biased due to the shocks in 2011 that might have specifically affected only the Illinois workers. I address this concern with a second analysis. Using Current Population Survey data, I show that for Illinois workers, the probability of job separation did not increase in 2011 relative to workers in other states. Similarly, Illinois workers had no reason to expect a greater stagnation in their wages. This lack of other adverse Illinois labor market shocks retains the payroll tax cut as the most credible explanation for the observed spending increase. Taken together, these additional analyses indicate that the estimates in this paper may be interpreted causally.

The rest of the paper is organized as follows. Section I describes the tax changes in 2011. Section II reviews relevant economic theory and empirical literature. Section III describes the Consumer Expenditure Interview Survey data that I use for estimation in this paper. Section IV discusses my main empirical results. In Section V, I present additional analyses to support a causal interpretation of the estimated effects. Section VI concludes.

I. Payroll Taxes in the United States and Tax Changes in 2011

Most wage-earners in the US are required to participate in the Old Age, Survivors and Disability Insurance (OASDI) program of the federal government.⁴ Among other benefits, the OASDI program provides a retirement pension to the covered worker and his/her surviving spouse. The program is primarily funded by payroll taxes levied on wage income below an inflation-indexed ceiling which was \$106,800 in 2011. Since 1991, the FICA tax rate has been 6.2 percent for employees and employers alike for a total of 12.4 percent.⁵

With the intent to support a still weak economy after the Great Recession, the federal government enacted on December 17, 2010, the Tax Relief, Unemployment Insurance Reauthorization, and Job Creation Act of 2010 (TRUIRJC). Along with other measures largely aimed at extending the Bush era tax cuts and the duration for unemployment eligibility, TRUIRJC temporarily reduced the employees' share of the FICA tax rate from 6.2 to 4.2 percent. The reduction in the payroll tax did not impact the Social Security Trust Fund since the shortfall in FICA contributions was made good out of general revenues of the federal government. Consequently, workers had no reason to anticipate a lower retirement income due to the tax cut. The fiscal cost of the payroll tax cut was estimated to be about \$112 billion per year. At the same time, the government saved about \$60 billion dollars per year by not renewing the Making Work Pay

⁴ Around 3 percent of workers in the US are exempt from social security. These include federal workers hired earlier than 1984; some state and local government employees in the following ten states – Alaska, California, Colorado, Illinois, Louisiana, Maine, Massachusetts, Nevada, Ohio, and Texas; foreign workers in certain visa categories; certain religious groups like the Amish; and children under 18 working for money in family businesses. Exempt state government employees number about 6 million (30 percent of all state and local government employees). These workers are exempt only from Social Security tax but pay the Medicare tax.

⁵ Self-employed employees pay both the employer's and employee's share for a total of 12.4 percent. Also, in addition to the OASDI tax of 12.4 percent, employees and employers pay 1.45 percent each as Medicare tax with no ceiling on the tax base.

credit of 2009 and 2010. This credit reduced the FICA tax liability of wage-earners by 6.2 percent of earned income up to a maximum of \$400.⁶ Therefore, the net benefit of these tax changes to most workers was 2 percent of their wage income minus the loss of \$400 on account of discontinuation of Making Work Pay credit.

Concomitant with the 2 percentage point decrease in the FICA tax rate, the state income tax rate in Illinois moved in the opposite direction in January 2011, increasing from a flat rate of 3 percent to a flat rate of 5 percent for a projected revenue gain of \$6.5 billion in 2011.⁷ Besides being equal in magnitude, the changes in the payroll tax and in the Illinois state income tax also paralleled each other in several other respects. First, both the taxes were deducted from employees' paychecks in each pay period. Second, both the tax changes were temporary.⁸ Last, both the tax changes - and extensions and eventual discontinuation in the case of FICA tax - were quite uncertain almost until they were actually enacted.

As illustrated in Table 1, changes in a person's tax liability due to the payroll tax cut and the increase in Illinois state income taxes almost cancelled out each other. For an Illinois worker making \$50,000 a year, personal tax liability increased by \$266 between 2010 and 2011. In contrast, a simple average of the

⁶ Therefore, workers making more than \$6,451 in the year availed the maximum reduction of \$400. This rebate was implemented through changes in tax withholdings.

⁷ The Illinois Legislature approved this increase on January 12, but applied it retroactively from January 1. The income tax rate is scheduled to drop to 3.75 percent in 2015 and to 3.5 percent in 2025. The corporate income tax rate also increased from 7.3 to 9.5 percent, and is scheduled to drop to 7.75 percent in 2015 and then to the original 7.3 percent in 2025. Illinois increased these tax rates to finance a fiscal deficit of \$15 billion (NCSL, 2011, page 2).

⁸ Initially, the reduction in the FICA tax rate was to apply only during 2011. However, in December 2011, the reduced rate was extended for the first two months of 2012, and then later in February 2012, it was extended for the whole of 2012. There were legislative plans towards the end of 2012 to renew the tax cut for another year, however these plans did not progress and the FICA tax rate reverted back to 6.2 percent in 2013. The Illinois state income tax is scheduled to drop to 3.75 percent in 2015, and then further to 3.5 percent in 2025.

increase in tax liability for a workers making \$50,000 a year and living in one of the other 50 jurisdictions was -\$711. The difference between these two changes is \$977 which almost equals workers' savings due to the 2 percentage point payroll tax cut.

Table 1 also suggests that there were possibly no substantial changes in income tax rates in the other states. To further investigate if this was true, I once again calculate change in tax liability for worker households in each of the 50 states in the US, as well as in Washington D.C., but this time, in the interest of realism, the change in tax liability is calculated using income and other characteristics of actual worker households from the Consumer Expenditure Interview Survey. Figure 2 shows the mean dollar change and the mean percentage change in state income tax liability in the six states with the largest changes. Illinois is clearly the outlier state. The state income tax liability increased by more than \$1,500 on average in Illinois. In percentage terms, reflecting the rate change from 3 to 5 percent, state tax liability increased by about 68 percent between 2010 and 2011. On the other hand, income tax rates were essentially flat in all the states except Connecticut, North Dakota, Rhode Island, California, and Ohio. Even in these five states, changes were rather modest.⁹ Tax changes in the other states were even more inconsequential, mostly amounting to tinkering with tax brackets, credits and deductions (NCSL, 2011). In fact, for the sample underlying Figure 2 and for all non-Illinois states, state-tax-to-income ratio changed from an average of 3.54 percent in 2010 only to 3.51 percent in 2011. Correspondingly, the

⁹ Connecticut increased marginal tax rates and adjusted brackets for an expected revenue gain of \$893 million. The other four of these five states marginally reduced the tax rates. North Dakota reduced income taxes across the board, with the top rate declining from 4.86 percent to 3.99 percent. Rhode Island reduced the number of brackets along with a modest decrease in the top marginal rate. In California, an earlier temporary 0.25 percentage point increase was allowed to expire in 2011, for a projected revenue loss of \$1.3 billion. Ohio marginally reduced its income tax rates across the board, bearing a projected revenue loss of \$519 million (NCSL, 2011).

marginal tax rate changed from an average of 4.81 percent only to an average of 4.73 percent.

Given the modest changes in income tax rates in other states, households that had at least one adult working in 2011 may be divided into two groups with respect to the tax changes in that year. The first group comprises Illinois workers, for whom the increase in state income tax cancelled out the payroll tax cut. The second group comprises workers in the rest of the US whose disposable income grew due to the payroll tax cut. Figure 3 illustrates how the net dollar impact of the tax changes in 2011 varied with wage income for both these groups. It is clear from this figure that for most workers there was a penalty associated with residence in Illinois in 2011 which equaled about 2 percent of their total income. This was, however, not true of the workers at the very low end of the wage distribution because Illinois income tax allowed an exemption of \$2,000 per person. On the other hand, Illinois penalty exceeded 2 percent of income for workers at the high end of the wage distribution as well as for those with significant non-wage income.

II. Economic Theory and Empirical Literature

In this section, I briefly review the theoretical reasons which might lead us to expect household spending to respond to tax cuts. I also review the empirical evidence on the subject, most of which comes from the papers studying the impact of the rebates offered by the federal government between 2000 and 2010.

A. Theory

According to the Permanent Income Hypothesis, the relevant budget for consumers is their permanent income (Friedman 1957). Since an avowedly

temporary tax cut is unlikely to change this budget by much, consumption should not respond to small idiosyncratic changes like a temporary tax cut (Aiyagari, 1994). Nevertheless, dating back at least to Hall (1982), excess sensitivity of current consumption to current income has long been recorded in the empirical literature. Various hypotheses have been advanced to explain this sensitivity.

If some consumers are liquidity constrained, their propensity to consume out of any additional disposable income may be quite high. Using macroeconomic evidence from several industrialized countries, Bacchetta and Gerlach (1997) find that aggregate consumption in the economy does respond to credit conditions in the economy. This suggests that some households are indeed liquidity constrained and recessions exacerbate this constraint by making it difficult to obtain credit just when current income is below anticipated future income. Also, it is not necessarily the poor who are credit constrained. Several recent papers have developed models in which even the wealthier households choose to be endogenously liquidity constrained in the presence of transaction costs and a higher return on the illiquid asset; see for instance, Kaplan and Violante (2011), and Huntley and Michelangeli (2011). Taking their model to the 2001 Survey of Consumer Finance data, Kaplan and Violante (2011) find that between 1/4 and 1/3 of US households fit the profile of being liquidity constrained while holding substantial wealth.

Another reason for excess sensitivity of current consumption is loss aversion. If consumers' preferences are characterized by loss aversion, then even if they are not objectively credit constrained, their consumption may respond to a tax reduction, as argued by Shea (1995) to explain consumption growth with wage growth for union workers whose wages were determined by long-term union contracts. Rule-of-thumb behavior, habit-persistence and non-separable preferences over consumption and leisure are yet other explanations for predictable growth in consumption expenditures (Kiley, 2010). Calculations by

Cochrane (1989) and Browning and Crossley (2001) show that with reasonable risk aversion, loss of utility from failing to smooth consumption is quite small, which helps explain how such non-optimizing behavior might endure. Finally, the excessive-sensitivity-of-consumption puzzle is partly solved once it is recognized that expenditures on durable and semi-durable goods provide a flow of utility well into the future. Hayashi (1982) showed that broadening the consumption measure to include service flows from consumer durables eliminated excess sensitivity of consumption in his data.

It is clear from the foregoing discussion that because of liquidity constraints, transaction costs, behavioral rules, and definitional issues, measured household spending may respond to a temporary spike in disposable income even if such a spike leaves permanent income unchanged. Besides, the payroll tax cut of 2011 did not leave the permanent income of wage-earners unchanged. Wage-earners benefited from the tax cut at the expense of non-wage-earners because (a) employers' contributions into Social Security Trust Fund remained unchanged, (b) the 2 percentage point shortfall in employees' contributions into the Fund was made good by the federal government out of general tax revenues. Therefore the net effect of the payroll tax cut was to increase the permanent income of wage-earner households at the expense of other households. For both these reasons – sensitivity of current consumption to current income and a modest increase in permanent income with the payroll tax cut - the expected effect of the 2011 payroll tax cut is to induce more consumption spending by the wage-earner households. Whether the magnitude of the effect is large or small is an empirical question.

B. Empirical Literature

The 2011 payroll tax cut is but one among the several tax rebates and credits provided by the federal government since 2000. In 2001, the IRS mailed \$300 rebate checks to tax-payers. In 2008, most households received rebate checks varying between \$600 and \$1,200, plus an additional \$300 for each dependent child. In 2009 and 2010, the federal government provided a Making Work Pay credit of \$400 to most wage-earners. Table 1 provides more details regarding these tax rebates. Below, I focus on the empirical literature using these tax rebates to identify the effect of tax cuts on current consumption. These studies either use data on incurred household expenditures, or use the survey responses that directly ask the rebate recipients what they intend to do (or did) with the rebates.

Johnson et al. (2006) used the randomization feature¹⁰ of the 2001 tax rebates to find that, cumulatively, consumption of non-durables increased by 69 percent of the amount of the tax rebate (spending increased by 37 percent in the quarter in which the rebate was received; the remainder of the increase was in the following quarters). The effect on strictly non-durables was moderate, and the effect on food expenditures was negligible. The 2001 tax rebates were also studied by Agarwal, Liu, and Souleles (2007) who tracked the trajectory of debt, spending, and repayments by households following the receipt of rebate checks. They found that at first households used the rebate payments to pay down debt, but after a while they borrowed again such that the debt level was about the same after 9 months. Hamilton (2008) and Misra and Surico (2011) also studied the 2001 tax rebate, finding a somewhat smaller effect than Johnson et al. (2006).

In 2008, IRS once again mailed rebate checks ranging from \$600 to \$1,200, averaging \$950 per family. These checks were similarly randomized as in 2001.

¹⁰ To ease the administrative burden, IRS had mailed the rebates over a ten week period and used social security numbers of the rebate recipients (which are random) to prioritize mailings.

Parker, Souleles, Johnson and McClelland (2013) used Consumer Expenditure Interview Survey data and exploited variation in the timing of stimulus payments to estimate that 50 to 90 percent of the rebate payments were spent in the quarter in which they were received. In contrast to Johnson et al. (2006), who did not find any effect of the 2001 rebate on spending on durable goods and services, Parket et al. (2013) found large increases in spending on consumer durables (including vehicles) as a result of the 2008 rebate checks. They hypothesized that this might be due to the large size and lump sum nature of the tax rebate in 2008. Studying the same 2008 rebate and using data from the AC Nielsen HomeScan survey, Broda and Parker (2014) found that the households receiving the rebate check in a week increased their expenditures on food, mass-merchandise and grocery products by 3.5 percent in that week. Bertrand and Morse (2009) found that receipt of the rebate check in 2008 reduced the probability of taking a payday loan, causing approximately a 12 percent reduction in outstanding debt. Nevertheless, the dollar reduction in debt was moderate, which is consistent with most of the rebate check being spent or used in retiring past bills. It would be fair to summarize that most of the empirical studies of tax rebates using micro-data on expenditures have found moderate to large effects of tax rebates on household spending; although the fact that these studies only measure the effect of variation in timing of the rebate receipt over a short period (as opposed to an increase in disposable income of the households over the year) makes it makes it difficult to interpret the estimates as the effect of a tax cut.

A second strand of the literature directly asks households what they plan to do or did with the rebate. Studying the effect of the 2001 rebate checks using the Michigan Survey of Consumers, Shapiro and Slemrod (2003a) found that only 22 percent of the rebate recipients reported that they would mostly spend the rebate. Most of the respondents claimed intent to save it. From these responses, the authors deduced a marginal propensity to consume of about 1/3. Studying the

2008 rebate with data from the Michigan Survey of Consumers, Shapiro and Slemrod (2009) found results that were very similar to those of their 2003 paper. Studying the 2011 Payroll tax cut which is also the focus of my paper, and using the American Life Panel's online survey, Graziani, Klaauw and Zafar (2013) probe survey respondents regarding their plans for additional disposable income. Ex-ante, workers in the survey intended to increase spending by 10 to 18 percent of the tax cut but, ex-post, report having spent 28 to 43 percent of the additional income.

In general, as compared to the studies relying on micro-data on expenditures, survey based studies that directly query households regarding their intent about spending or saving the rebates have tended to find a lower propensity to consume out of tax savings. This may be due to the usual bias in survey responses to give the “correct” answer. It is also possible that the survey respondents more accurately report the true effect of the tax cut taking a broader and longer term view of their finances, whereas a part of the effect of tax rebates in the studies relying on expenditures micro-data is due to the households finding it convenient to match their cash inflows with cash outflows (Gelman et al., 2014).

III. Data – The Consumer Expenditure Interview Survey

Estimation in this paper is based on the Consumer Expenditure Interview Survey (CE) data. CE is a short panel of rotating households. After an initial induction interview, households in the CE sample are interviewed consecutively every three months for 4 quarters with a focus on getting data on household expenditures.¹¹ The survey collects the month-of-purchase as well as the value-of-

¹¹ New households are recruited into the survey every month so as to yield approximately 7,200 completed interviews every quarter. Completion rate is quite good with 63 percent of the households completing all the four interviews, and 80 percent completing at least 3 interviews.

purchase information for relatively high-value purchases. For small-value, high-frequency purchases such as groceries, the survey asks only for ballpark estimates with questions such as, “What was your usual weekly expense?”¹² According to BLS, the larger purchases constitute about 60 to 70 percent of total expenses, and the smaller purchases constitute another 20 to 25 percent of household expenditures. Therefore, in all, the CE Interview Survey covers as much as 80 to 95 percent of all household expenditures.¹³

Although the CE survey collects some income and tax information as well, it is quite limited in these respects. Household members’ income and occupation information is collected only twice - during the first and the fourth interviews.¹⁴ Much of the tax information is either missing or is imputed by BLS. For this reason, I do not make use of tax reports from the survey. Instead, I estimate taxes using NBER’s Taxsim program (see <http://users.nber.org/~taxsim/>). For a given household, Taxsim takes as input income and other information about the

The interviews are conducted in-person using the Computer Assisted Personal Interview (CAPI) format which ensures data quality through on-the-spot verification and correction.

¹² BLS’ approach is to collect details about the high-frequency smaller purchases through a complementary diary survey. The diary survey has an independent sample which is not a panel. Each household in the diary sample completes the diary for two weeks for *all* purchases, although in its published reports, BLS uses the diary data only to estimate the high-frequency, low-value expenditures such as groceries.

¹³ Garner, McClelland, and Passero (2009) evaluate strengths and weaknesses of the CE survey in comparison with other surveys such as Panel Study of Income Dynamics. Li et al. (2010) conclude that the Panel Study of Income Dynamics (PSID) and CE expenditure estimates are very close (PSID contains only a few expenditures like on groceries). Garner et al. (2006) note that CE expenditure aggregates may be as much as 80 percent lower than the corresponding PCE estimates from National Income Accounting. Sabelhaus et al. (2013) attribute this shortfall to a lower-than-average propensity to consume of the richer households which also have a poorer response rate to the survey.

¹⁴ For the in-between interviews, income related questions are asked again only if a family member not previously working starts working or turns 14 after the previous interview. Also, the income information and the expenditure information in the CE data do not cover the same time period. Expenditure data are collected for the three months ending in the most recently completed month prior to the interview. For example, for a household interviewed on February 27, that period would be November 1 of the previous year through January 31 of the current year. Income, however, is queried as of the date of the interview for the previous 12 months with a question like “During the past twelve months.... How much did you receive as wages?”

household and provides as output estimates of personal federal, state and FICA taxes payable by the household. It was possible to obtain from the CE data all the 22 input variables needed by Taxsim to estimate tax liability, except the short-term and the long-term capital gains, which I assumed to be zero.

IV. Estimates

Consistent with the specification in Johnson et al. (2006), Parker et al. (2013) and other papers, I estimate the following model.

$$(1) \quad \Delta E_i = \beta_0 + \beta_1 \Delta T_i + \beta_2 \Delta A_i + \beta_3 \Delta K_i + \sum_{m=1}^{24} \gamma_{mi} + \mu_i$$

where the operator Δ with a variable denotes change in that variable between 2010 and 2011. E_i is a measure of consumption expenditure for household i . Total expenditures include, besides regular household and personal expenses, cash payments towards durable goods; transfer payments such as charity, alimony and child support; and indirect taxes such as property taxes. Savings and investments, including contributions to retirement/pension plans/social security or repayment of principal on mortgages and other loans are excluded.¹⁵ Payments of direct taxes such as income tax are also excluded. Expenditures in the survey are reported inclusive of sales and excise taxes, and no attempt is made to separate these indirect tax payments, as from consumers' point of view there is not much of a difference between these indirect tax payments and the base prices of goods.

On the right hand side, the main coefficient of interest is β_1 , which measures the responsiveness of households' expenditures to changes in direct tax liability, T_i , which is the sum of federal income tax, state income tax, payroll tax and the

¹⁵ In its published reports, BLS includes contributions to retirement and pension plans in the definition of Total Expenditures. It would however be inappropriate to regard these contributions as 'consumption' in this paper.

Making Work Pay credit. Additional covariates include the number of adults, A_i , and the number of kids, K_i . Consistently with the previous literature, I include these covariates to account for expenditure changes merely due to changes in family size and composition. γ_{mi} is a collection of 24 dummy variables – one for each month of the calendar years 2010 and 2011 – to completely absorb any seasonal effects on spending. γ_{mi} takes a value of 1 for household i if the household reported expenditures in that month, and 0 otherwise.¹⁶ Finally, μ_i is the error term.¹⁷

Estimation of equation (1) requires that households report their expenditures for at least one month in each of the two years 2010 and 2011. The households which do not meet this condition are excluded from the sample. Since households report data for unequal number of months, I scale taxes and expenditures to annualized basis before estimating equation (1). For the baseline estimates, I restrict the sample to wage-earner households because only such households were impacted by the payroll tax cut. I further exclude the households with very low total income, as these households were not impacted by the tax changes (see Figure 2).¹⁸ Finally, the households for which BLS suppressed the state of residence

¹⁶ No household reported expenditure for the same month in both the years. This is by design since households in the CE sample are interviewed consecutively every 3 months for up to 4 times. For example, if we have expenditure data for a household pertaining to the last four months of 2010, then 2011 data for the same household would pertain to the first eight months of 2011. No household reported expenditure for the same month in both the years. The total number of months for which data is available for a household is used as frequency weight for that household. In unreported results, I have confirmed that choosing to weight the households has no appreciable effect on the results.

¹⁷ The right hand side omits change in income between 2010 and 2011. This is done as change in income is highly correlated with change in taxes, and also because a separate cross-sectional regression analysis of the CE data shows that transitory changes in income are unrelated to changes in consumption. Including change in income on the right hand side preserves statistical significance, and the measured effect of tax change on spending is even larger.

¹⁸ The chosen criterion was that per head income in the family should exceed \$2,000 which is the personal exemption allowed in Illinois.

information are excluded from the sample as I rely on 2SLS estimation of equation (1) with residency in Illinois instrumenting for change in taxes.¹⁹

Table 3 presents summary statistics of the variables used in estimation for both Illinois and the rest of the US sub-samples. The last two columns of the table present a t-test on the difference in means between these sub-samples. A significant increase in the tax burden on Illinois workers is evident in the summary table itself. Table 3 also shows that despite the larger increase in income among the Illinois households in the sample,²⁰ their consumption expenditures declined sharply in 2011.

The OLS estimate of 0.12 in column 1 of Table 4 illustrates the difficulty of recovering the parameter of interest without exogenous variation in taxes. The coefficient on change in taxes has the wrong sign, is small in magnitude, and is statistically insignificant. The key problem here is the bias from omitted variables that are correlated with changes in tax liability as well as with changes in consumption expenditures. For instance, a household that received a positive labor market shock in 2011, such as a wage increase, is likely to increase its spending even as its tax liability goes up, leading to a spurious result of the type found in column 1.

The omitted variable problem may be overcome by using residency in Illinois as an instrument for changes in tax liability. The first stage of the model is the average change in taxes in Illinois relative to the rest of the United States. Using these predicted tax changes in the second stage to estimate equation (1) will lead to an unbiased estimate of β_1 as long as Illinois residency affected the

¹⁹ Around 13.72 percent of the quarterly interviews had the state information suppressed, including some from Illinois.

²⁰ Mean household income in both the sub-samples is rather high as most households in the data have two working partners (mean of 1.68 workers per household), and also because, as explained earlier, I exclude the very low end of the wage distribution as these households might not be impacted by changes in state income taxes.

consumption expenditures only through the taxes. This exogeneity assumption almost certainly does not hold in *levels* because different states have different costs of living, income levels, state laws and other factors directly relevant for consumption choices. However, the difference model in (1) should eliminate all persistent influences of Illinois residency on consumption. It is still a concern that besides the state tax increase, Illinois workers might have suffered other Illinois- and-2011-specific shocks that affected their consumption choices. I address this concern in the next section with additional analyses.

Column 3 of Table 4 reports the first stage for the 2SLS estimates in column 2. Due to the policy changes in 2011 that increased the tax burden of Illinois workers but decreased it for workers elsewhere, the first-stage regression yields a large and statistically significant estimate of the coefficient on Illinois residency. Residency in Illinois was associated with a penalty of \$2,445. This penalty is in line with what one would expect given the policy changes in 2011 and the reported changes in income in the CE sample. As is apparent from the F-statistic, the first stage is sufficiently strong.

In column (2), the predicted changes in taxes from the first-stage regression are used to instrument actual changes in tax liability. The resulting 2SLS estimate is large in magnitude and statistically significant at the 1 percent level. Column 2 indicates that for every dollar of decrease in tax liability, total expenditures increase by about 89 cents.²¹ This effect is very close to the estimate of 91 cents in Parker et al. (2013, Table 3). Ex-ante, I expected my coefficients to be larger than those in Parker et al. since those estimates measure the effect of early receipt of a cash payment as opposed to a clear increase in annual disposable income.

²¹ In unreported results, I have confirmed that including additional covariates such as age and family size does not much affect either the magnitude or standard error of the estimate. It would have been of interest to study heterogeneity in the effect of the tax cut among sub-populations. However, due to insufficient Illinois observations, my sample lacks the power to detect any effects on sub-populations.

The similarity in the magnitude of the two estimates despite the difference in the design of respective tax cuts gives us greater confidence in the efficacy of rebates in stimulating demand during a recessionary period. However, this conclusion does not appear to be definitive in the light of Sahm et al. (2012). That paper estimated the spending response to be only half as large when the tax cut was implemented through withholdings on pay checks in the case of Making Work Pay Credit of 2009, as compared to the lump sum rebate checks of 2008. Further research is warranted on this aspect.

A. Discretionary Expenditures

It is of interest to identify the types of expenditures that were most responsive to the tax cut. The CE micro-data include the total expenditure aggregate as well as its decomposition into several functional categories such as food, housing, apparel, transport health, entertainment, personal care, education, etc. which have been created by BLS to help the researchers. However, a perusal of the individual line items that constitute these aggregates indicates that within every sub-category, there are expenditures that a household may increase or decrease at will, and others – often contractually fixed – which either cannot be changed at all or would be inconvenient to change at short notice. For instance, within the category of *Education*, a household looking to adjust its expenses following a temporary tax decrease may find it easy to spend more on supplies and tutoring services, but not worthwhile to increase spending on tuition or day care expenses by changing providers of these services. Similarly, within *Transport*, gasoline and cab fares may be adjustable immediately, but not lease payments. Most transfers in the category *Cash Contributions* are discretionary, however alimony expenses are not so. Even in the category of *Recreation*, there are expenses such as club membership fees that may not be amenable to adjustments in the short run.

Therefore, the functional classification offered by BLS, while useful for other research questions, creates a limitation for the researcher investigating the impact of a temporary tax cut. Fortunately, the micro-data also include approximately 540 line items directly corresponding to the survey questions. I studied these individual line items and classified them into discretionary expenditures and non-discretionary expenditures on the basis of whether a household is likely to find it easy and worthwhile to change the outlay in question soon after an increase in disposable income. I further decomposed discretionary expenditures into the seven sub-categories described in Table 6. I then re-estimated equation (1) with discretionary expenditures and with the sub-categories of discretionary expenditures. Tables 5 and 6 report the resulting estimates.

It is clear from Table 5 that virtually the entire effect of the tax decrease on consumption is due to the effect on discretionary spending (94 cents). As one would expect, the effect on non-discretionary expenditures is indistinguishable from zero. Compared to the estimate of the effect on total expenditures, the effect on discretionary spending is also more precisely measured, with a standard error of 0.15. This implies that the lower bound of the 95 percent confidence interval is about 65 cents. This is strong evidence in support of a large effect of the tax cut on spending. Since the effect of a temporary tax cut as measured with a short panel data should theoretically be entirely on discretionary spending, and it is seen to be so in the data, I focus on discretionary spending for additional analysis and tests in Section V.

The results in Table 6 suggest that within discretionary spending, the following sub-categories were especially responsive to the tax cut – *Recreation, Dining & Vacation* (54 cents); *Clothing & Personal Care* (24 cents); and *Medical Expenses* (16 cents). The last of these effects is somewhat surprising as one would not ordinarily expect discretionary medical spending to be responsive to small changes in disposable income.

Estimates of the coefficients on the control variables are also of interest and along expected lines. Pair formation – as evidenced by change in the number of adults – leads to a substantial increase in all expenses, but especially on *Recreation, Dining & Vacation*. On the other hand, arrival of a kid in the family – often a newborn – only modestly increases most expenses, and has no effect on *Recreation, Dining & Vacation*, which is not surprising given that time is required as a concomitant input to increase monetary expenses on recreation, dining and vacation – and as an input time becomes especially scarce with the arrival of a newborn.

B. Replicating Johnson et al. (2006) and Parker et al. (2013)

In order to be able to compare the effect of the 2011 payroll tax cut with those of 2001 and 2008 rebate checks, I re-estimated equation (1) with the three sub-categories previously used by Johnson et al. (2006) and Parker et al. (2013) - *food expenditures; strictly non-durable goods and services; and broad non-durable goods and services*. Table 7 reports these estimates.

Consistent with Johnson et al. (2006), I find negligible change in food spending. However, my estimate for non-durables is larger than in Johnson et al. as well as in Parker et al. (2013). I find an effect on non-durables spending as large as 96 cents, whereas Johnson et al. found the corresponding effect to be 69 cents (or 37 cents excluding lags), and Parker et al. only 31 cents. My estimate is statistically different from the latter estimate of 31 cents but not from the Johnson et al. estimate of 69 cents.

A comparison of my results with Parker et al. is especially interesting. Parker et al. estimated the effect on total expenditures to be 91 cents²² which is very close to my estimate of 89 cents in Table 4. However, following the receipt of the 2008

²² Parker et al. (2013), Table 3 on Page 37.

rebate checks, most of the increase in spending in their sample was due to the purchase of durable goods, especially vehicles, whereas, following the 2011 payroll tax cut, most of the increase in spending in my sample comes from non-durables goods and services. This comparison suggests that policy makers can not only spur aggregate demand through tax rebates, they can also influence the type of aggregate demand that is incentivized through tax design. Perhaps when tax rebates are offered incrementally, most of the additional spending is on non-durables; when households receive a large lump sum payment, they are induced to use it as down payment in the purchase of consumer durables.

V. Threats to Identification

In the preceding section, isolating the policy-induced tax change by using an appropriate instrument, the effect of the tax cut is identified in a model that controls for household fixed effects. The coefficient estimates on both total spending and discretionary spending are significant at the 1 percent level. On decomposing the total effect into the sub-categories of total expenditures, it is seen that the sub-categories that one would expect to be most elastic to a temporary tax cut do indeed show greater responsiveness. All these findings lend credibility to the methodology and data used in this study.

Nevertheless, a causal interpretation of the estimates is justified only if residency in Illinois in 2011 was uncorrelated with any of the relevant variables omitted from equation (1). This exogeneity assumption would be violated if, other than the tax change, any economic or other shocks affected the wage-earner households differently in Illinois in 2011 than how those shocks affected the wage-earner households in the rest of the US, *and* how those shocks affected Illinois wage-earners in 2010 (shocks that affected workers in both the years are differenced out). I distinguish between two different types of such Illinois-and-

2011 specific shocks that Illinois wage-earner households might have suffered in 2011 – the shocks that they endured along with non-wage-earner households in Illinois, and the shocks that specifically affected Illinois wage-earners (labor market shocks). In the remainder of this section, I present additional evidence and analyses to sequentially address concern on account of both of these types of shocks. In the end, I use a permutations test to assess how likely it is to estimate a consumption response in one state, even if there were no actual tax changes there.

A. Shocks Uniformly Affecting Worker and Non-Worker Households in Illinois

Since the CE survey data include both wage-earner and other households, I am able to compare how these two subsets of households fared in Illinois relative to the rest of the US. Table 8 reports the result of such a comparison.

Estimates in columns 1 and 2 of Table 8 are obtained by estimating the following reduced-form model using change in discretionary spending as the dependent variable.

$$(2) \quad \Delta E_i = \beta_0 + \beta_1 R_i + \beta_2 \Delta A_i + \beta_3 \Delta K_i + \sum_{m=1}^{24} \gamma_{mi} + \mu_i$$

In the above equation, R_i is a dummy variable indicating residency in Illinois. Other variables have the same interpretation as in equation (1). Column 1 reports the OLS estimate when the sample is restricted to wage-earner households. In this sample, the coefficient on Illinois Residency is estimated to be large and significantly different from zero. However, as reported in column 2, the same coefficient estimate is indistinguishable from zero when the sample is restricted to non-wage-earners. This shows that only the wage-earner households in Illinois faced the negative shock in 2011.

The same conclusion can again be drawn via 2SLS estimation of the model given in equation (3). In this model, wage-earners and non-wage-earners are

pooled together, and an interaction dummy between Illinois residency and wage-earner status is used to instrument for the change in taxes. This instrument is appropriate because a majority of non-wage-earners in the sample comprise retirees, and retirement income is untaxed in Illinois. Other non-wage-earners also had negligible taxable income.

$$(3) \quad \Delta E_i = \beta_0 + \beta_1 \Delta T_i + \beta_2 \Delta A_i + \beta_3 \Delta K_i + \sum_{m=1}^{12} \gamma_{mi} + R_i + W_i + \mu_i$$

In the above equation, W_i is a dummy for wage-earner status. All other terms have the same interpretation as before. The dependent variable is change in discretionary expenditures. Since this model provides an intercept for Illinois residency, the estimate of β_1 should be unbiased from any 2011 shocks that uniformly affected workers and non-workers in Illinois. Such shocks include changing housing values,²³ some state government policies and taxes, weather changes, local price changes etc. The model also provides an intercept for wage-earner status; therefore, the estimate of β_1 should also be unbiased from any factors affecting workers everywhere such as federal fiscal policies affecting aggregate demand, and monetary policies affecting credit availability to businesses. The estimate of β_1 can only be biased from the shocks specific to the 2011 Illinois labor market, which I will consider in the next sub-section.

Column 4 in Table 8 reports the first stage of estimating (3) using Illinois Residency x Wage-Earner to instrument for the change in taxes. The first-stage is sufficiently strong, and the estimated coefficient on the instrument is as expected in size. Column 3 of Table 8 reports the impact of the tax change on discretionary spending obtained by estimating equation (3) with predicted changes in tax

²³ The Case-Shiller price index of housing values for Chicago declined by 5 percent over 2010-2011. The decline of the broad 20 metros index was more moderate at 3.9 percent (S&P, 2011). In my sample, the proportion of homeowners is about the same among workers (64 percent are homeowners) as among non-workers (60 percent are homeowners).

liability from the first-stage regression. The resulting point estimate of 96 cents has adequate statistical power, and is very close to my baseline estimate of 94 cents from Table 5.

Based on the results presented in Table 8, it would be fair to conclude that there were no significant Illinois-wide shocks in 2011 that could have biased the baseline results in this paper. Bias may still result from the shocks specific to Illinois labor market, which I study next.

B. Shocks Specific to the Illinois Labor Market

With regards to Illinois-and-2011 specific labor market shocks, I consider three possible factors that, if true, could alternatively account for a relative decline between 2010 and 2011 in the consumption expenditures of Illinois workers. These are:

- (i) A long-term negative time trend in consumption expenditures of Illinois workers relative to the rest of the US.
- (ii) A greater rise in the risk of unemployment for Illinois workers in 2011.
- (iii) Illinois workers anticipating more sluggish growth in their wages than the national average.

To evaluate the first factor, I estimate the following reduced-form model.

$$(4) \Delta E_{it} = \beta_0 + \sum_{t=2007}^{2012} \beta_t R_{it} + \beta_2 \Delta A_i + \beta_3 \Delta K_i + \sum_{m=1}^{72} \gamma_{mi} + \sum_{t=2008}^{2012} Y_{it} + \mu_i$$

Compared to the previous models, this model has an intercept for each year, Y_t , and interaction terms between Illinois residency and each year, R_{it} . The sample consists of wage-earner households in the CE data between 2007 and 2012 who

reported expenditures in two consecutive calendar years. Table 9 provides the results from estimating this model.

Table 9 shows that Illinois workers reduced their 2011 expenditures much more than they did in any year previous or since. Among other years, only in 2009, residence in Illinois adequately predicted a large change in expenditures, and for broad non-durables, not even in that year. Overall, Table 9 seems to rule out any consistent time trend in year-to-year expenditure changes of Illinois workers, relative to the rest of the US.

The same conclusion is also supported by the time series data on personal disposable income shown in Figure 4. Growth in disposable income in 2011 was in fact stronger in Illinois than in the rest of the country.

I next focus on factors 2 and 3, which describe two possible negative shocks of relevance to employed workers – probability of losing one’s job²⁴ and the rate of growth in wages. If Illinois workers began to anticipate in 2011 a heightened risk of job separation, they might have depressed their current consumption relative to 2010. The same holds true in the case of anticipated wage-stagnation. In either of these cases, the effect of the tax cut would be overestimated in my model. In this sub-section, I investigate these possibilities using Current Population Survey data, which is one of the key resources for studying labor markets in the US.

Panel A in Figure 5 charts the probability that a currently employed worker would be observed to be without work in the same month the next year. While the job-separation probability was indeed higher in 2011 in Illinois, the change between 2010 and 2011 was the same for workers in Illinois as for workers elsewhere. Therefore, assuming correct expectations on average, there was no more reason for Illinois workers to grow more pessimistic in 2011 than there was for workers elsewhere.

²⁴ My baseline sample comprises only employed workers, for whom the probability of losing job is likely to be more relevant than the aggregate unemployment rate.

Consider now the relative growth in wage rates. Panel B of Figure 5 indicates that between 2010 and 2011, expected future wages for employed workers grew more robustly in Illinois than in the US. This finding is corroborated by Figure 4, presented earlier, and also by the summary statistics in Table 3, which showed that wages grew more sharply in the Illinois sub-sample.

From the results of the analyses in this sub-section, it is reasonable to conclude that there were no significant Illinois-and-2011 specific shocks that could account for the relative increase in household spending among US workers in 2011, as an alternative to attributing this increase to the payroll tax cut.

C. A Placebo Test

The estimates above rely on a tax change in Illinois. In this subsection I perform a placebo test in which I re-estimate the reduced-form equation (2) multiple times – substituting Illinois with another state in each case. Since measured consumption changes from year to year in all states due to actual shocks and due to sampling variability in the data, I expect to find non-zero “effects” even in states that didn’t actually change their tax rate. The test of whether my estimates from Illinois are credible is whether I estimate an unusually large effect there compared to what I find in models for other states. In particular, the distribution of estimates from the other states can be interpreted as the distribution of the estimated treatment effect under the null hypothesis that the true effect is zero. (I used reduced-form estimation for this exercise instead of 2SLS, because residency in other states does not provide a valid instrument for change in tax liability, and as is well known, if the first stage is weak, 2SLS results can be very biased.)

Restricting the exercise to the states which contributed at least 50 observations to the sample, I get a total of 20 regressions. Table 10 summarizes the results of this experiment, with each row reporting a separate regression. Table 10 shows

that the impact of Illinois residency was more negative than that of residency in any other state by a wide margin. Out of 20 other estimates, only the estimate for Massachusetts is as large in absolute value as that for Illinois. Under the null hypothesis that there is no true effect in any other state, the distribution of effects in across other states indicates that there is a 5 percent chance of finding the \$2,302 effect in Illinois when the true effect is zero.

It may seem a cause of concern that residency in several of the other states should have estimated effects that differ from zero and are also statistically significant. Notice, however, that all the five states as large as Illinois - Pennsylvania, Florida, New York, California and Texas - have estimated effects of less than \$1,000 in size, and in three of those cases, the effect is even smaller than \$250. This suggests that the non-zero effects in the case of other states in Table 10 is due to sampling error. Such an interpretation of the results in Table 10 is further supported by a simulation experiment I undertook, which I describe below.

In the simulation experiment, I randomly re-distributed states (without replacement) among the households in the non-Illinois sub-sample, and then re-estimated equation (2) for each of the 20 “states” listed in rows 2 through 21 of Table 10. I repeated this exercise 250 times, giving me a total of 5,000 estimates. By construction, any non-zero estimate in this experiment can only be due to sampling error. In Table 11, I compare the results from this simulation exercise to what is observed in the placebo test reported in Table 10. It is evident from this comparison that the observed dispersion of the estimates, as well as the frequency of statistically significant estimates in the placebo test, are both consistent with the hypothesis that there was no effect of residency on change in consumption expenditures in any state other than Illinois.

In light of the results from the placebo test and the simulation experiment, it is unlikely that the results in this paper are driven by sampling error or idiosyncratic changes in Illinois.

VI. Conclusion

Using a fortuitous quasi-natural experiment, this paper estimates that the 2011 payroll tax cut led to an economically significant increase in consumption expenditures in the economy. The point estimate indicates that for each dollar of reduction in tax liability, households increased their spending by 89 cents. All of the increase in spending was from discretionary spending which is easy to change soon after an increase in disposable income. This finding is consistent with what one would expect to see when measuring the effect of a temporary tax cut with a short panel data. Within discretionary spending, large effects were seen in the categories of recreation, dining, vacation, clothing, personal care, and somewhat surprisingly, medical care. A comparison with previous empirical research suggests that the design of a tax stimulus may affect the types of expenditures that are most affected, with expenditures on durables responding more to lump sum tax rebates, and expenditures on non-durables responding more to a smaller but regular increase in monthly disposable income.

In theory, the estimates presented in this paper may be biased due to Illinois-specific shocks. However, further analysis of the data reveals that as compared to their Illinois counterparts, household spending increased only among the US households affected by the payroll tax cut (i.e. wage-earner households), and not among the households not affected by the payroll tax cut (retiree households and others). Further, a supplementary analysis of the Current Population Survey data indicates that there were no differential trends between the labor markets of Illinois and the US at large which could explain this relative increase in spending

of only the wage-earner households, as an alternative to attributing it to the 2011 payroll tax cut. These findings indicate that the estimates in this paper can be interpreted causally.

Evaluating the results in this paper from the perspective of public policy, the point estimate of 0.89 implies that the payroll tax cut increased aggregate household spending in 2011 by about \$46 billion.²⁵ In its annual reports based on the Consumer Expenditure Survey data, BLS estimates that between 2010 and 2011, household spending in the US increased by \$1,596 per household, or about \$193 billion in the aggregate (Bureau of Labor Statistics, 2013, Table 1). Thus, even ignoring any multiplier effects, the 2011 payroll tax cut may be credited with contributing about a quarter of the total recovery in household spending following the end of the Great Recession.

REFERENCES

- Agarwal, Sumit, Chunlin Liu, and Nicholas S. Souleles.** 2007. "The Reaction of Consumer Spending and Debt to Tax Rebates—Evidence from Consumer Credit Data." *Journal of Political Economy* 115(6): 986-1019.
- Aiyagari, S. Rao.** 1994. "Uninsured idiosyncratic risk and aggregate saving." *Quarterly Journal of Economics* 109 (3): 659–684.
- Alesina, Alberto F. and Silvia Ardagna.** 2009. "Large Changes in Fiscal Policy: Taxes Versus Spending," National Bureau of Economic Research Working Paper 15438.
- Bacchetta, Philippe, and Stefan Gerlach.** 1997. "Consumption and credit constraints: International evidence." *Journal of Monetary Economics* 40(2): 207-238.
- Barrow, Lisa, and Leslie McGranahan.** 2000. "The Effects of Earned Income Credit on the Seasonality of Household Expenditures." *National Tax Journal* 53: 1211-44

²⁵ From Table 2, fiscal cost of the payroll tax cut was about \$52 billion net of savings due to discontinuation of Making Work Pay Credit of 2009 and 2010. Applying the point estimate of 0.89, I get \$46 billion.

- Bertrand, Marianne, and Adair Morse.** 2009. "What do High-Interest Borrowers do with their Tax Rebate?" *American Economic Review* 99(2): 418-429.
- Broda, Christian, and Jonathan A. Parker.** 2014. "The Economic Stimulus Payments of 2008 and the Aggregate Demand for Consumption." National Bureau of Economic Research Working Paper 20122.
- Bureau of Labor Statistics.** 2013. "Consumer Expenditures in 2011." United States Department of Labor Report 1042.
- Browning, Martin, and Thomas F. Crossley.** 2001. "The life-cycle model of consumption and saving." *Journal of Economic Perspectives* 15(3): 3–22.
- Campbell, Jeffrey R., and Zvi Hercowitz.** 2009. "Liquidity Constraints of the Middle Class." Chicago Fed Working Paper 2009-20.
- Campbell, Jeffrey R., and N. Gregory Mankiw.** 1989. "Consumption, income, and interest rates: reinterpreting the time series evidence," *NBER Macroeconomics Annual* 4: 185–216.
- Cochrane, John H.** 1989. "The Sensitivity of Tests of the Intertemporal Allocation of Consumption to Near-Rational Alternatives." *The American Economic Review* 79(3): 319–337.
- Coronado, Julia L., Joseph P. Lupton, and Louise M. Sheiner.** 2005. "The Household Spending Response to the 2003 Tax Cut: Evidence from Survey Data." Federal Reserve Board. Finance and Economics Discussion Series Working Paper 2005-32.
- Elmendorf, Douglas W., and Jason Furman.** 2008. "If, When, How: A Primer on Fiscal Stimulus." Brookings Institution. The Hamilton Project Strategy Paper. <http://www.brookings.edu/research/papers/2008/01/10-fiscal-stimulus-elmendorf-furman> (accessed July 15, 2014).
- Friedman, Milton.** *A Theory of the Consumption Function*. Princeton: Princeton University Press, 2006.
- Garner, Thesia I., George Janini, William Passero, Laura Paszkiewicz and Mark Vendemia.** 2006. "The CE and the PCE: a comparison." *Monthly Labor Review* 129(9): 20-46.
- Garner, Thesia I., Robert McClelland, and William Passero.** 2009. "Strengths and Weaknesses of the Consumer Expenditure Survey from a BLS Perspective." Paper Presented at the NBER Summer Institute, July 2009.
- Gelman, Michael, Shachar Kariv, Matthew D. Shapiro, Dan Silverman, and Steven Tadelis.** 2014. "Harnessing Naturally Occurring Data to Measure the Response of Spending to Income." *Science* 345: 212-215.
- Graziani, Grant, Wilbert van der Klaauw, and Basit Zafar.** 2013. "A Boost in the Paycheck: Survey Evidence on Workers' Response to the 2011 Payroll Tax Cuts." Federal Reserve Bank of New York Staff Report No. 592.

- Hall, Robert E. and Frederic S. Mishkin.** 1982. "The Sensitivity of Consumption to Transitory Income: Estimates from Panel Data on Households." *Econometrica* 50(2): 461-481.
- Hamilton, Douglas.** 2008. "A reexamination of Johnson, Parker, and Souleles 2001 tax rebate estimate." Mimeo, Congressional Budget Office.
- Hayashi, Fumio.** 1982. "The Permanent Income Hypothesis: Estimation and Testing by Instrumental Variables." *Journal of Political Economy* 90(5): 895-916.
- Hsieh, Chang-Tai.** 2003. "Do Consumers React to Anticipated Income Changes? Evidence from the Alaska Permanent Fund." *American Economic Review* 93(1): 397-405.
- Huntley, Jonathan, and Valentina Michelangeli.** 2014. "Can Tax Rebates Stimulate Consumption Spending in a Life-Cycle Model?" *American Economic Journal: Macroeconomics* 6(1): 162-89
- Jappelli, Tullio and Pistaferri Luigi.** 2010. "The Consumption Response to Income Changes." *Annual Review of Economics* 2: 479-506.
- Johnson, David, Jonathan A. Parker, and Nicholas S. Souleles.** 2006. "Household Expenditure and the Income Tax Rebates of 2001." *American Economic Review* 96(5): 1589-1610.
- Johnson, David, Jonathan A. Parker, and Nicholas S. Souleles.** 2009. "The Response of Consumer Spending to Rebates During an Expansion: Evidence from the 2003 Child Tax Credit." Chicago Fed Working Paper.
- Kaplan, Greg and Giovanni L. Violante.** 2011. "A Model of the Consumption Response to Fiscal Stimulus Payments." National Bureau of Economic Research Working Paper 17338.
- Kiley, Michael T.** 2010. "Habit Persistence, Non-separability Between Consumption and Leisure, or Rule-of-Thumb Consumers: Which Accounts for the Predictability of Consumption Growth?" Federal Reserve Board. Finance and Economics Discussion Series Working Paper 2005-32.
- Li, Geng, Robert F. Schoeni, Sheldon H. Danziger, and Kerwin Charles.** 2010. "New Expenditure Data in the Panel Study of Income Dynamics: Comparisons with the Consumer Expenditure Survey Data." *Monthly Labor Review* 133(2): 29-39.
- Lusardi, Annamaria.** 1996. "Permanent Income, Current Income, and Consumption: Evidence from Two Panel Data Sets." *Journal of Business Economics and Statistics* 14(1): 81-90.
- Mankiw, N Gregory.** 2000. "The Savers-Spenders Theory of Fiscal Policy." *American Economic Review* 90(2): 120-125.
- Mian, Atif and Amir Sufi.** 2012. "The Effects of Fiscal Stimulus: Evidence from the 2009 'Cash for Clunkers' Program." *The Quarterly Journal of Economics* 127(3): 1107-1142

- Misra, Kanishka., and Paolo Surico.** 2011. "Heterogeneous Responses and Aggregate Impact of the 2001 Income Tax Rebates." Center for Economic Policy Research Discussion Paper 8306.
- NCSL.** 2011. "State Tax Update: August 2011" Preliminary Report. National Conference of State Legislatures.
http://www.ncsl.org/documents/fiscal/STU2011Prelim_final.pdf (accessed July 15, 2014).
- Oh, Hyunseung, and Ricardo Reis.** 2011. "Targeted Transfers and the Fiscal Response to the Great Recession." National Bureau of Economic Research Working Paper 16775.
- Ohlemacher, Stephen.** 2007. "Analysis ranks Illinois most average state." *The Southern Illinoisan*, May 17. http://thesouthern.com/news/analysis-ranks-illinois-most-average-state/article_a697be25-baf2-56c6-87c4-5428d10590de.html (accessed July 15, 2014).
- Parker, Jonathan, Nicholas S. Souleles, David S. Johnson, and Robert McClelland.** 2013. "Consumer Spending and the Economic Stimulus Payments of 2008." *American Economic Review* 103(6): 2530-53.
- Romer, Christina D., and David H. Romer.** 2010. "The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks." *American Economic Review* 100(3): 763-801
- S&P Indices.** 2011. "Home Prices Weaken as the Third Quarter of 2011 Ends." Press Release, November 29.
http://www.housingviews.com/wpcontent/uploads/2011/11/CSHomePrice_Release-21.pdf (accessed July 15, 2014).
- Sabelhaus, John, David Johnson, Stephen Ash, David Swanson, Thesia Garner, John Greenlees and Steve Henderson.** 2013. "Is the Consumer Expenditure Survey Representative by Income?" *NBER Working Paper* 19589.
- Sahm, Claudia R., Matthew D. Shapiro, and Joel B. Slemrod.** ed. 2010. "Household Response to the 2008 Tax Rebates: Survey Evidence and Aggregate Implications." *Tax Policy and The Economy*, ed. Jeffrey R. Brown, Cambridge: MIT Press.
- Sahm, Claudia R., Matthew D. Shapiro, and Joel B. Slemrod.** 2012. "Check in the Mail or More in the Paycheck: Does the Effectiveness of Fiscal Stimulus Depend on How It Is Delivered?" *American Economic Journal: Economic Policy* 4(3): 216–250.
- Shapiro, Matthew D., and Joel B. Slemrod.** 1995. "Consumer Response to the Timing of Income: Evidence from a Change in Tax Withholding." *American Economic Review* 85(1): 274-83.
- Shapiro, Matthew D., and Joel B. Slemrod.** 2003a. "Consumer Response to Tax Rebates." *American Economic Review* 93(1): 381–96.

- Shapiro, Matthew D., and Joel B. Slemrod.** 2003b. "Did the 2001 Tax Rebate Stimulate Spending? Evidence from Taxpayer Surveys." *Tax Policy and the Economy* 17: 83–109.
- Shapiro, Matthew D., and Joel B. Slemrod.** 2009. "Did the 2008 Tax Rebates Stimulate Spending?" *American Economic Review Papers and Proceedings* 99(2): 374-79.
- Shea, John.** 1995. "Union Contracts and the Life-Cycle/Permanent-Income Hypothesis." *American Economic Review*, Vol. 85(1): 186-200.
- Souleles, Nicholas S.** 1999. "The Response of Household Consumption to Income Tax Refunds." *American Economic Review* 89(4): 947-958.
- Souleles, Nicholas S.** 2004. "Expectations, Heterogeneous Forecast Errors, and Consumption: Micro Evidence from the Michigan Consumer Sentiment Surveys." *Journal of Money, Credit, and Banking* 36 (1): 39-72.
- Stephens, Melvin Jr.** 2003. "3rd of the Month: Do Social Security Recipients Smooth Consumption Between Checks?" *American Economic Review* 93(1): 406-422.
- Taylor, John B.** 2011. "An Empirical Analysis of the Revival of Fiscal Activism in the 2000s." *Journal of Economic Literature* 49(3): 686-702.
- Telyukova, Irina A.** 2013. "Household Need for Liquidity and the Credit Card Debt Puzzle." *Review of Economic Studies* 80(3): 1148-1177.
- Thaler, Richard H.** (1990): "Anomalies: Saving, Fungibility, and Mental Accounts." *The Journal of Economic Perspectives* 4(1): 193–205.
- Wilcox, David W.** 1989. "Social Security Benefits, Consumption Expenditures, and the Life Cycle Hypothesis." *Journal of Political Economy* 97(2): 288-304.
- Zeldes, Stephen P.** 1989. "Consumption and Liquidity Constraints: An Empirical Investigation." *Journal of Political Economy* 97(2): 305-346.

TABLE 1 — ILLUSTRATING INCREASE IN TAX LIABILITY OF A WORKER EARNING \$50,000 AS WAGES IN 2010 AND 2011 AND NO OTHER INCOME OR TAX COMPLICATIONS

| | 2010 | 2011 | Change |
|---|----------|----------|----------|
| Wage Income | \$50,000 | \$50,000 | \$0 |
| Other Income | \$0 | \$0 | \$0 |
| <i>Panel A. Taxes for an Illinois Worker</i> | | | |
| Making Work Pay Credit | -\$400 | 0 | +\$400 |
| Payroll Tax (OASDI plus Medicare) | \$3,825 | \$2,825 | -\$1,000 |
| Federal Income Tax | \$6,344 | \$6,250 | -\$94 |
| State Income Tax (personal exemption = \$2000) | \$1,440 | \$2,400 | +\$960 |
| Total | \$11,209 | \$11,475 | +\$266 |
| <i>Panel B. Taxes for a Non-Illinois Worker</i> | | | |
| Making Work Pay Credit | -\$400 | 0 | +\$400 |
| Payroll Tax (OASDI plus Medicare) | \$3,825 | \$2,825 | -\$1,000 |
| Federal Income Tax | \$6,344 | \$6,250 | -\$94 |
| Average State Income Tax (see notes) | \$1,770 | \$1,753 | -\$17 |
| Total | \$11,539 | \$10,828 | -\$711 |
| Increase in Illinois minus Increase elsewhere. | | | \$977 |

Notes: State income tax for the non-Illinois worker is simple average of the tax payable in other 50 states by a worker making \$50,000 a year, all in wages. OASDI rate was 6.2 percent of wages in 2010 and 4.2 percent of wages in 2011. Medicare was 1.45 percent of wage in both the years. There were no changes between 2010 and 2011 to federal income tax rates. The decrease in federal income tax in this illustration is due to bracket creep between the two years, and an increase in standard deduction and personal exemption allowances.

Source: Author calculations using NBER's Taxsim program.

TABLE 2—FEDERAL TAX RELIEF SINCE 2000

| Tax Relief | Bush Tax Cuts of 2001 and 2003 | Economic Stimulus Payments in 2008 | “Making Work Pay” Credit in 2009 and 2010 | Social Security Tax Relief, 2011 and 2012 |
|-----------------------------|---|--|---|--|
| How Much? | <p>\$300 per tax payer in 2001.</p> <p>In 2001, tax rate on the first \$6000 of a tax payer’s income reduced from 15 percent to 10 percent for 10 years. However, the Act was passed in May. Accordingly, \$300 was intended to be an advance refund for 2001 tax return while the lowest tax rate was kept at 15 percent for 2001.</p> <p>Growth and Tax Relief Act of 2003 further lowered taxes on capital, and accelerated tax changes envisaged in the 2001 Act.</p> | <p>\$600 per tax payer plus \$300 per dependent kid (age < 17), if earned income including pension benefits > \$3000. Five cents of reduction for every dollar earned over \$75000.</p> <p>Out of \$600, \$300 was refundable credit not limited by tax paid. Rebate was based on 2007 tax return with upward only adjustment on 2008 return.</p> <p>Formula: $\text{Rebate} = [\text{Max}(0, \text{Max}(\\$300, \text{Min}(\\$600, \text{Tax paid})) + \\$300 * \text{kids} - 0.05 * (\text{Income over } \\$75000))] * (\text{Income} > 3,000)$</p> | <p>6.2 percent of <i>earned</i> income up to a maximum of \$400 per tax payer. [6.2 percent is employee’s social security contribution].</p> <p>Two cents of reduction for every dollar earned over \$75000.</p> <p>\$250 reduction in 2010 for the social security recipients/retired railroad workers/disabled veterans to avoid double dipping with Economic Recovery Payment.</p> <p>Refundable credit not limited by tax paid.</p> | <p>Employee’s share decreased from 6.2 percent to 4.2 percent of social security wages up to \$106,800 in 2011. Employer’s share remains the same.</p> <p>Workers under alternative retirement systems e.g. CSRS, SURS etc. do not see the benefit.</p> <p>To make up for the shortfall, funds transferred from the General Account into Social Security Trust Fund.</p> |
| How long? | 10 years, even though rebate checks were only for 2001. | Only a one-time rebate. | Intended as a one-time rebate, but renewed in 2010. | Only for 2011, but later extended to 2012. |
| When? | The rebate checks were mailed between June and December, 2001. | Most payments were disbursed between May and June of 2008. | No rebate checks. Employers required to lower income tax withholdings by April 1, 2009. | No rebate checks. Employers required to lower withholdings by January 31, 2011. |
| Fiscal Cost? | \$38 billion in 2001 | \$96 billion in 2008 | \$60 billion in 2009 and 2010 each. | \$112 billion in 2011. |
| Impact on Household budgets | About 2/3 rd of households received the rebate amounting to \$450 on average. | About 70 million households received a rebate of \$950 on average. | 75 percent of the households are expected to receive full credit and another 10 percent partial credit. | The average size of the tax relief is likely to be more than \$1000 per household. |
| Selected Literature | <ul style="list-style-type: none"> • Agarwal, Liu, Souleles (2007) • Coronado et al. (2005) • Hamilton (2008) • Johnson, Parker and Souleles (2006) • Misra and Surico (2011) • Shapiro and Slemrod (2003) | <ul style="list-style-type: none"> • Bertrand and Morse (2009) • Broda and Parker (2014) • Parker et al., (2013) • Sahm, Shapiro and Slemrod (2010) • Shapiro and Slemrod (2009) | <ul style="list-style-type: none"> • Sahm, Shapiro and Slemrod (2012) • Taylor (2011) | <ul style="list-style-type: none"> • Graziani, Klaauw and Zafar (2013) |

TABLE 3—WAGE-EARNER HOUSEHOLDS IN 2010 AND 2011 - SUMMARY STATISTICS AND T-TEST

| | Illinois | | Rest of the US | | T-Test | |
|---|----------|-------|----------------|-------|---------------------|-------|
| | Mean | SE | Mean | SE | Difference in Means | SE |
| Number of Households | 147 | | 2,685 | | | |
| Interviews per Household | 3.67 | | 3.52 | | | |
| <i>Panel A. Levels in 2011</i> | | | | | | |
| Total Income | 96,807 | 7,253 | 80,971 | 1,389 | 15,836** | 7,385 |
| Wage Income | 90,337 | 6,836 | 73,170 | 1,216 | 17,167** | 6,943 |
| Tax Liability | 23,033 | 2,521 | 16,968 | 489 | 6,065** | 2,568 |
| Total Expenditures | 51,430 | 2,759 | 50,126 | 787 | 1,304 | 2,869 |
| Discretionary Expenditures | 28,187 | 1,421 | 26,852 | 409 | 1,335 | 1,479 |
| Non-Discretionary Expenditures | 23,243 | 1,577 | 23,274 | 543 | -31 | 1,668 |
| <i>Discretionary Expenditures</i> | | | | | | |
| Housing and Commute | 12,289 | 691 | 11,475 | 165 | 814 | 710 |
| Grocery, Alcohol and Tobacco | 5,958 | 232 | 6,016 | 71 | -57 | 243 |
| Recreation, Dining and Vacation | 5,587 | 497 | 5,052 | 143 | 536 | 517 |
| Clothing and Personal Care | 1,713 | 174 | 1,413 | 80 | 300 | 191 |
| Medical Expenses | 1,394 | 185 | 1,384 | 50 | 11 | 192 |
| Transfers | 1,069 | 224 | 1,338 | 149 | -269 | 269 |
| Other Expenditures | 176 | 53 | 176 | 13 | 0 | 55 |
| Number of Adults | 1.98 | 0.074 | 1.99 | 0.017 | -0.01 | 0.08 |
| Number of Kids <=17 years | 0.80 | 0.094 | 0.74 | 0.020 | 0.06 | 0.10 |
| <i>Panel B. Changes from 2010</i> | | | | | | |
| Change in Total Income | 3,261 | 3,006 | 1,855 | 653 | 1,405 | 3,076 |
| Change in Wage Income | 3,446 | 2,549 | 1,153 | 395 | 2,293 | 2,579 |
| Change in Tax | 1,793 | 1,150 | -313 | 253 | 2,106* | 1,177 |
| Change in Total Expenditures | -3,106 | 1,761 | -996 | 716 | -2,111 | 1,901 |
| Change in Discretionary Expenditures | -3,341 | 1,030 | -1,080 | 308 | -2,262** | 1,075 |
| Change in Non-Discretionary Expenses | 235 | 1,374 | 84 | 662 | 151 | 1,525 |
| <i>Change in Discretionary Expenses on:</i> | | | | | | |
| Housing and Commute | -133 | 522 | -254 | 165 | 121 | 547 |
| Grocery, Alcohol and Tobacco | 160 | 187 | 149 | 47 | 11 | 193 |
| Recreation, Dining and Vacation | -1,464 | 502 | -143 | 132 | -1,320** | 519 |
| Clothing and Personal Care | -1,118 | 279 | -633 | 70 | -485* | 287 |
| Medical Expenses | -367 | 224 | 95 | 62 | -462** | 232 |
| Transfers | -432 | 163 | -269 | 167 | -163 | 234 |
| Other Expenditures | -13 | 44 | -15 | 18 | 1 | 48 |
| Change in Number of Adults | -0.034 | 0.027 | 0.006 | 0.005 | -0.040 | 0.026 |
| Change in Number of Kids | 0.018 | 0.024 | 0.010 | 0.004 | 0.008 | 0.027 |

Notes: Sample comprises wage-earner households who reported expenditures in both 2010 and 2011. See notes to Tables 4-6 for definition of variables. All values in current dollars.

Source: Consumer Expenditure Interview Surveys, 2010 and 2011 waves. Taxes are calculated using NBER's TAXSIM program and include personal income taxes at federal and state level, any tax rebates, and payroll taxes (employees' share only).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 4 —DOLLAR CHANGE IN TOTAL EXPENDITURE BETWEEN 2010 AND 2011

| | (1) | (2) | (3) |
|----------------------------|------------------------|------------------------|-------------------------|
| | OLS | 2SLS | <i>First Stage</i> |
| Change in Taxes | 0.12 (0.11) | -0.89*** (0.27) | |
| Change in Number of Adults | 4,145.59 (4,288.80) | 6,440.27 (4,224.20) | 2,334.96*** (773.25) |
| Change in Number of Kids | 5,869.11 (4,523.81) | 6,275.59 (4,504.88) | 375.77 (878.0) |
| Illinois Resident | | | 2,445.87*** (286.87) |
| Seasonal controls | Yes | Yes | Yes |
| Observations | 2,832 | 2,832 | 2,832 |
| R ² | 0.02 | . | 0.01 |
| RMSE | 34,033 | 36,534 | 13,250 |
| F-statistic | 5.93 | | 29.48 ^a |
| Chi2 | | 485.62 | |

Notes: Dependent variable in columns 1 and 2 is change in total expenditures between 2010 and 2011. Total expenditures are measured inclusive of sales taxes but exclude contributions to retirement plans and other savings and investment by the household. Column 1 reports OLS estimation. Column 2 reports 2SLS estimation with change in tax liability instrumented by Illinois Residency. Households' tax liability is estimated with NBER's Taxsim program using income and other data available in the Consumer Expenditure Interview Survey. Column 3 reports the first stage of 2SLS estimation with change in tax liability as the dependent variable.

Sample is restricted to the wage-earner households defined as households with positive wage income and a total income that was sufficiently high to owe taxes in Illinois in 2011 (typically, this implied that income in the household exceeded \$2,000 per head). Households for which expenditure data is not available for either of the two years are excluded, as are households for whom BLS chose to suppress the state of residence information. Number of months for which expenditure data is available for a household is used as frequency weight for that household. Sample size is 2,832 out of which 147 observations came from Illinois and 2685 from other states.

All regressions included a full set of month dummies following equation (1). Kids are 0 to 17 years old. Adults are 18 years and older.

^aAngrist-Pischke F-statistic for excluded instrument is 86.89

Standard errors reported in parentheses are clustered by state.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 5—CHANGES IN DISCRETIONARY AND NON-DISCRETIONARY EXPENDITURES

| | Discretionary Expenditures | Non-Discretionary Expenditures |
|----------------------------|----------------------------|--------------------------------|
| Change in Taxes | -0.94*** (0.15) | 0.05 (0.25) |
| Change in Number of Adults | 4,047.03*** (1,081.49) | 2,393.24 (3,964.31) |
| Change in Number of Kids | 899.24 (1,120.27) | 5,376.35 (4,257.5) |
| Seasonal controls | Yes | Yes |
| Observations | 2,832 | 2,832 |
| RMSE | 20,776 | 30,817 |
| Chi2 | 287 | 165 |

Notes: Estimation method is 2SLS in both the columns with change in taxes instrumented by Illinois Residency. See notes to Table 4 for the first-stage, description of the sample used in estimation and other details. Non-Discretionary expenditures are expenditures that a household may find difficult to alter in the short run. All other expenses are discretionary expenses. A list of these expenditures is provided below.

| Discretionary Expenses | Non-Discretionary Expenses |
|---|--|
| <ul style="list-style-type: none"> • Lotteries, cigarettes, tobacco • Groceries other than meals received as pay • Dining out, clothing, saloon & personal care • Gifts and charity contributions • Home furnishings, laundry, and utility bills • Purchase of small and large appliances • Physician visits • Medical supplies^a (Prescription drugs, medical equipment for general use, pet services and medicines) • Commuting^a (gasoline, diesel and motor oil, vehicle rentals, cab fares) • Education^a (books, supplies and tutoring) • Recreation^a • Repairs^a (roof, painting, construction, floor replacement, upholstery, vehicle service and repair) • Rent^a (rent of televisions, furniture, non-camper trailer parking other than at residence, vacation clubs) • Vacation^a • Vehicles^a | <ul style="list-style-type: none"> • Alimony and child support payments • Support for college going students • Insurance and interest payments • Legal fee, accounting fees and occupational expenses • Funeral expenses, domestic service • Property taxes • Vehicle registration, driving license and vehicle inspection • Food or board while at school, school meals, meal as pay • Tuition and day care expenses • Hospital expenses • Commuting^a (Tolls, GPS services, towing charges, school bus) • Medical supplies^a (eyeglasses and contact lenses, hearing aids, rental of medical equipment for convalescence, adult diapers) • Membership fees^a (credit cards, shopping clubs safe deposit boxes) • Recreation^a (club membership, computer info services) • Rental payments^a and Repairs^a • Vacations^a (housing for someone at school, out of town tolls) • Vehicle expenses^a (cash down, lease payments and finance charges) |

^aThese expenditure categories contain discretionary as well as non-discretionary subcategories. Relevant subcategories are listed in parentheses unless too numerous.

Standard errors reported in parentheses are clustered by state.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 6—CHANGES IN SUBCATEGORIES OF DISCRETIONARY EXPENDITURES

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|----------------------|---------------------|---------------------------------|------------------------------------|-------------------------------|----------------------|--------------------|-------------------|
| | Housing, Commute | Grocery, Alcohol, Tobacco | Recreation, Dining, Vacation | Clothing, Personal Care | Medical Expenses | Transfers | Other Expenses |
| Change in Taxes | -0.06 (0.05) | 0.01 (0.02) | -0.54*** (0.08) | -0.24*** (0.04) | -0.16*** (0.04) | -0.08 (0.06) | 0.01** (0.01) |
| Change in #Adults | 197.78 (451.09) | 552.82** (223.45) | 1,613.57*** (709.07) | 762.55*** (256.30) | 594.62** (237.47) | 330.98 (309.78) | -5.28 (67.17) |
| Change in # Kids | -425.08 (533.17) | 574.36*** (164.67) | 7.63 (573.16) | 375.08 (288.37) | 287.05 (277.51) | 67.76 (123.29) | 12.44 (50.12) |
| Seasonal controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observation | 2,832 | 2,832 | 2,832 | 2,832 | 2,832 | 2,832 | 2,832 |
| RMSE | 8,217 | 2,434 | 9,916 | 4,745 | 4,057 | 7,895 | 1,022 |
| Chi2 | 54 | 128 | 1,538 | 798 | 171 | 201 | 98 |

Notes: Estimation method is 2SLS in all the columns with change in taxes instrumented by Illinois Residency. See notes to Table 4 for the first-stage, description of the sample used in estimation and other details. Sub-categories of discretionary expenditures are described below.

Housing and Commute expenses include expenses relating to maintenance of house and daily life such as laundry, utility, hired help, commuting, decoration, purchase and repair of appliances, linen, drapery, carpeting, china.

Grocery, Alcohol and Tobacco includes food, beverages and alcohol purchased in super markets, and any tobacco purchases.

Recreation expenses include magazines, audio-video CDs, sports equipment and fee, hobbies, entertainment expenses, dating services, rentals of boats and vehicles in home town. Short term membership fee for clubs etc. is included. Dining expenses include food and alcohol purchased at restaurants. Vacation expenses are expenses on travel, food and lodging during out-of-town trips.

Clothing expenses include all expenses on clothing and their upkeep and repair. Personal care expenses are saloon and other grooming expenses.

Medical expenses include expenses/copays on physicians' visit and medical supplies including drugs. Hospital expenses /copays are deemed non-discretionary and hence excluded.

Transfers include gifts and charitable giving. Non-discretionary transfer payments like child alimony are excluded.

Other discretionary expenses include discretionary educational supplies and miscellaneous expenses.

Standard errors reported in parentheses are clustered by state.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 7 — REPLICATION EXERCISE WITH RESPECT TO JOHNSON ET AL. (2006) AND PARKER ET AL. (2013) -
CHANGES IN EXPENDITURES ON FOOD, STRICTLY NON-DURABLES, AND BROAD NON-DURABLES

| | (1) Food | (2) Strictly Non- Durables | (3) Broad Non- Durables |
|----------------------------|-----------------------|-------------------------------------|-------------------------------|
| Change in Taxes | -0.06** (0.03) | -0.50*** (0.06) | -0.96*** (0.11) |
| Change in Number of Adults | 786.95*** (269.35) | 2,604.56*** (1,090.94) | 3,985.84*** (1,327.45) |
| Change in Number of Kids | 458.64** (220.31) | 941.36 (777.07) | 1,110.06 (1,146.92) |
| Seasonal controls | Yes | Yes | Yes |
| Observations | 2,832 | 2,832 | 2,832 |
| RMSE | 3,755 | 10,153 | 16,453 |
| Chi2 | 238 | 538 | 736 |

Notes: Estimation method is 2SLS in all the columns with change in taxes instrumented by Illinois Residency. See notes to Table 4 for the first-stage, description of the sample used in estimation and other details. Food expenses are a subset of expenses on Strictly Non-Durable goods and services, which in turn are a subset of Non-Durable Expenses. Scope of these categories is provided below and is identical with Johnson et al. (2006) and Parker et al. (2006).

Food expenses include grocery purchases and food outside home except meals received as pay.

Strictly non-durable goods and services include expenses on food, utility bills, housing operations, public transport, gas and motor oil, personal care expenses, tobacco and miscellaneous.

Broad Non-durable expenses include expenses on strictly defined non-durable goods and services, plus apparel, health expenses, and reading materials.

Standard errors reported in parentheses are clustered by state.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 8—CHANGES IN DISCRETIONARY EXPENDITURES WITH ALTERNATIVE SAMPLES

| | (1) | (2) | (3) | (4) |
|----------------------------|--------------------------|-------------------------|-------------------------|-------------------------|
| | OLS | OLS | 2SLS | <i>First Stage</i> |
| Sample | Wage-Earners | Non-Wage-Earners | All Households | |
| Change in Taxes | | | -0.96*** (0.18) | |
| Illinois Resident | -2,302.06*** (221.31) | -230.25 (247.81) | 12.73 (244.3) | 107.01 (127.37) |
| Wage-Earner | | | -299.42 (307.41) | -351.50 (245.09) |
| Illinois x Wage-Earner | | | | 2,296.82*** (294.88) |
| Change in Number of Adults | 1,849.36** (840.88) | 1,953.42** (798.962) | 3,313.22*** (810.82) | 1,590.61*** (484.21) |
| Change in Number of Kids | 545.56 (856.75) | -741.85 (1,817.83) | 375.68 (1006.84) | 59.98 (698.21) |
| Seasonal controls | Yes | Yes | Yes | Yes |
| Sample | Wage-Earners | Non-Wage-Earners | Both | Both |
| Observations | 2,832 | 1,754 | 4,585 | 4,585 |
| RMSE | 15,208 | 12,065 | 18,267 | 10,592 |
| F-statistic | | | | 114.43 ^a |

Notes: Dependent variable in columns 1-3 is change in discretionary expenditures from between 2010 and 2011. Column 1 reports reduced-form estimate based on wage-earner households. Column 2 reports the same model based on non-wage-earners. Column 3 reports 2SLS estimation with change in taxes instrumented by Illinois Residency x Wage-Earner status. Column 4 reports the first stage of the 2SLS regression in column 3.

^a Angrist-Pischke F-statistic for excluded instrument is 60.67

Standard errors reported in parentheses are clustered by state.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 9— REDUCED-FORM ESTIMATES INCLUDING PREVIOUS YEARS

| | (1) | (2) |
|-------------------|-------------------------------|------------------------------------|
| | Discretionary Expenditures | Broad Non-Durables Expenditures |
| Illinois x 2008 | -536.40 (485.90) | -322.77** (150.55) |
| Illinois x 2009 | 1,337.38*** (298.36) | 401.42** (182.84) |
| Illinois x 2010 | -194.74 (353.49) | -755.86*** (149.96) |
| Illinois x 2011 | -2,221.00*** (214.16) | -2,313.92*** (196.38) |
| Illinois x 2012 | -758.99* (404.47) | -346.50 (210.75) |
| Year dummies | Yes | Yes |
| Seasonal controls | Yes | Yes |
| Observations | 14,592 | 14,592 |
| RMSE | 17,960 | 9,462 |

Notes: Estimation method is 2SLS. Dependent variable in column 1 is change in discretionary expenditures. Dependent variable in column 2 is change in broad non-durables. See notes to Table 5 and Table 7 for a description of these expenditures, respectively. Sample includes all wage-earner households who reported expenditures in two consecutive calendar years between 2007 and 2012. All the regressions included year and monthly dummies.

Standard errors reported in parentheses are clustered by state.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 10—PLACEBO TEST - CHANGE IN DISCRETIONARY EXPENDITURES

| State | Coefficient on Residency in State | Standard Error | Number of Households in State | Sample Size |
|----------------|-----------------------------------|----------------|-------------------------------|-------------|
| ILLINOIS | -2,302.06*** | 221.31 | 147 | 2,832 |
| Virginia | -1,365.01*** | 213.97 | 112 | 2,685 |
| Ohio | -1,347.65*** | 289.97 | 74 | 2,685 |
| PENNSYLVANIA | -965.51*** | 228.44 | 143 | 2,685 |
| Washington | -897.45*** | 204.04 | 56 | 2,685 |
| Michigan | -839.65*** | 191.98 | 63 | 2,685 |
| Missouri | -820.70*** | 301.18 | 56 | 2,685 |
| New Jersey | -500.18* | 252.48 | 98 | 2,685 |
| FLORIDA | -220.36 | 235.55 | 163 | 2,685 |
| Minnesota | -207.36 | 234.06 | 55 | 2,685 |
| NEW YORK | -75.81 | 182.01 | 190 | 2,685 |
| CALIFORNIA | 41.14 | 233.91 | 353 | 2,685 |
| Georgia | 333.24 | 225.56 | 85 | 2,685 |
| Oregon | 475.42* | 246.09 | 59 | 2,685 |
| South Carolina | 567.42** | 215.71 | 57 | 2,685 |
| TEXAS | 695.64** | 295.68 | 249 | 2,685 |
| Maryland | 1,559.60*** | 288.55 | 59 | 2,685 |
| Louisiana | 1,674.88*** | 256.62 | 57 | 2,685 |
| Arizona | 2,034.24*** | 251.26 | 71 | 2,685 |
| Wisconsin | 2,056.44*** | 317.89 | 66 | 2,685 |
| Massachusetts | 4,237.40*** | 335.08 | 83 | 2,685 |

Notes This table reports regressions for all the states which contributed at least 50 observations to data. Each row represents a separate OLS regression. Regressions are arranged in the increasing order of the magnitude of the coefficient on state dummy. Five states that are comparable or larger than Illinois are shown in the upper case. Except in the first row, all the regressions excluded Illinois households.

Standard errors reported in parentheses are clustered by state.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 11—COMPARISON OF THE RESULTS IN TABLE 10 WITH THE SIMULATION EXPERIMENT

| | Simulation Experiment | Table 10 |
|--|-----------------------|------------|
| Number of Regressions | 5,000 | 20 |
| Mean of the point estimates | \$47 | \$322 |
| Mean standard error | \$338 | \$250 |
| Proportion of estimates in the range [-\$1,500, \$1,500] | 66 percent | 75 percent |
| Proportion of estimates in the range [-\$1,000, \$1,000] | 48 percent | 65 percent |
| Proportion of estimates in the range [-\$500, \$500] | 26 percent | 24 percent |
| Proportion of estimates significant at 1 percent level | 55 percent | 55 percent |
| Proportion of estimates significant at 5 percent level | 10 percent | 10 percent |
| Proportion of estimates significant at 10 percent level | 6 percent | 10 percent |
| Proportion of statistically insignificant estimates | 29 percent | 25 percent |
| Mean absolute size of the point estimates: | | |
| conditional on significance at 1 percent level | \$2,034 | \$1,618 |
| conditional on significance at 5 percent level | \$825 | \$632 |
| conditional on significance at 10 percent level | \$648 | \$488 |
| conditional on no statistical significance | \$293 | \$176 |

Notes: In the simulation experiment, states were randomly redistributed among the households in the sample (N=2,685). This random redistribution was repeated 250 times. After each redistribution, the model in equation (2) was re-estimated for each of the 20 non-Illinois states in Table 10, yielding a total of 5,000 estimates.

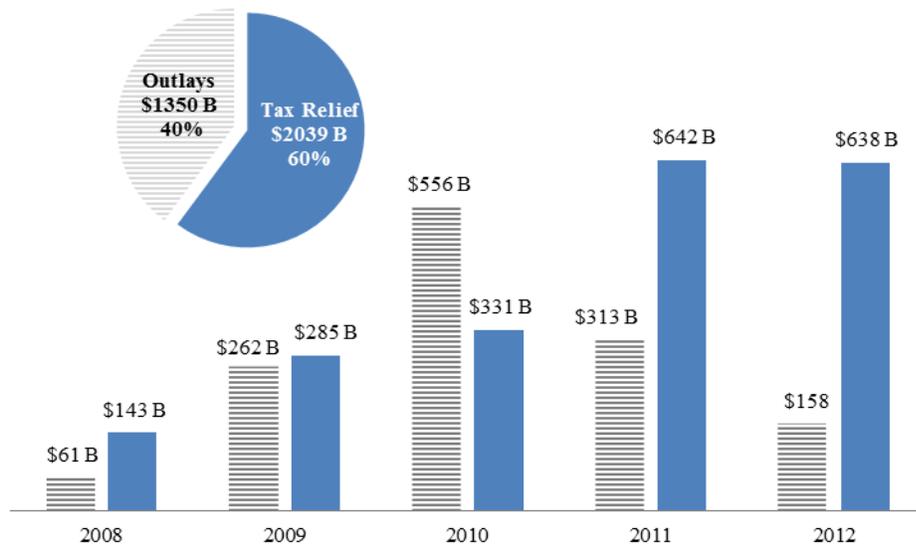


FIGURE 1. US FEDERAL GOVERNMENT'S RESPONSE TO THE GREAT RECESSION-
NEW OUTLAYS VS. TAX RELIEF IN BILLIONS OF DOLLARS

Source: Anderson (2011), Table 4, page 13.

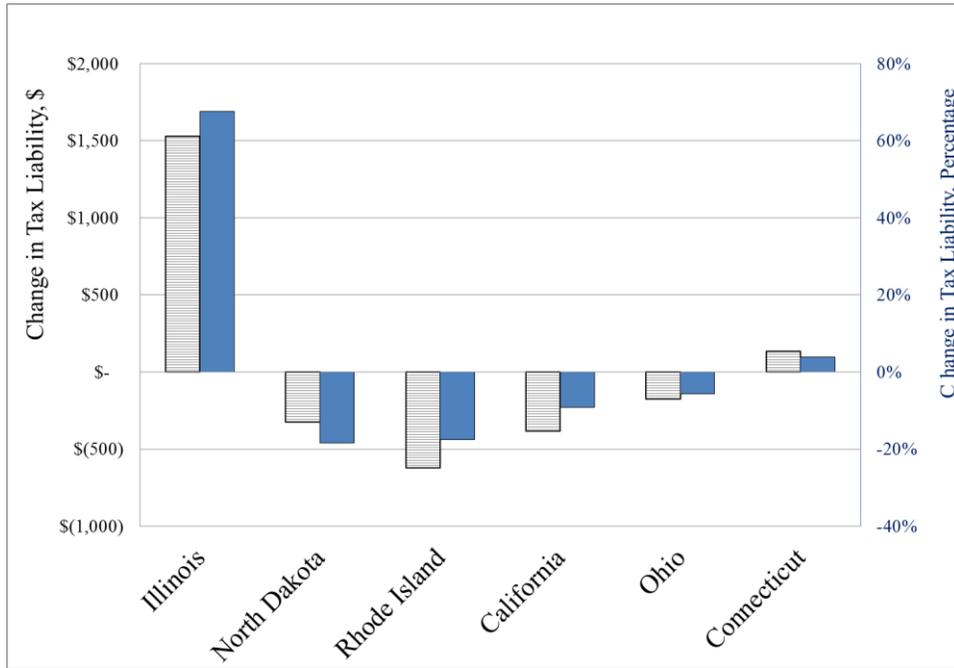


FIGURE 2. EFFECT OF CHANGES IN STATE INCOME TAX RATES IN 2011

Notes: Figure shows mean change in state income tax liability in 2011 as compared to 2010. Mean is calculated using the Illinois cohort of wage-earner households in the 2011 Consumer Expenditure Interview Survey. For any state, mean tax increase is the increase that these households would have seen in their state income tax liability in 2011 if they lived in that state ($n = 757$). To isolate tax changes driven by policy alone, households are assumed to have same income and other characteristics in 2010 as they did in 2011. States in which change in tax liability was less than 2.5 percent are not shown (changes in the range of 2.5 percent may be expected on account bracket creep alone).

Source: Author's calculations using data from the Consumer Expenditure Interview Survey and NBER's Taxsim Program.

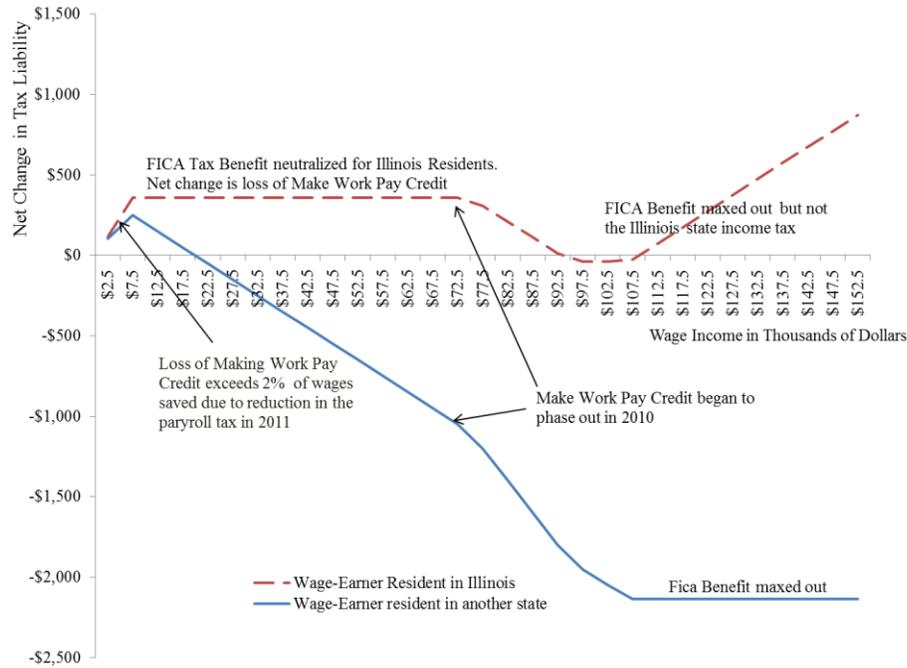


FIGURE 3. NET EFFECT OF CHANGES IN FICA, ILLINOIS STATE INCOME TAX, AND MAKING WORK PAY CREDIT

Notes: Figure illustrates how the net effect of the key tax changes in 2011 diverged between wage-earners residents in Illinois and the wage-earners resident in other states. Over a broad range of income, Illinois penalty is about 2 percentage of wage income.

Source: Author's calculations assuming single marital status at tax filing, no non-wage income, and no itemized deductions or credits. To isolate the effect of policy changes, income is assumed to remain the same before and after the tax changes.

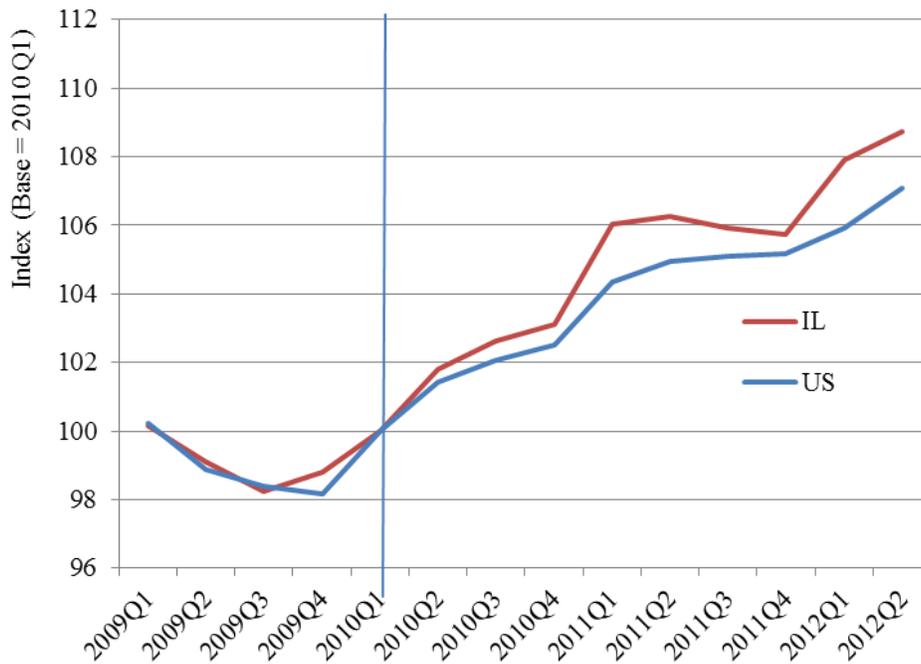
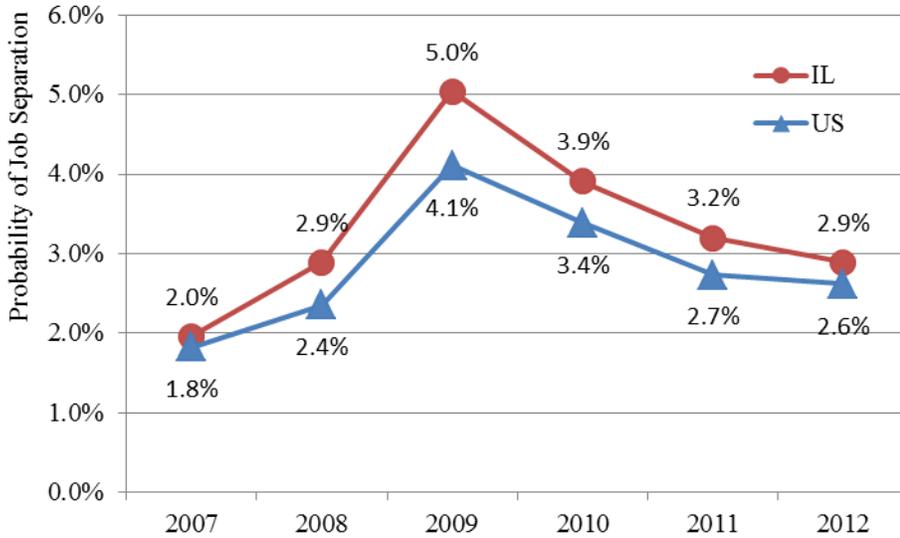


FIGURE 4. INDEX OF DISPOSABLE PER CAPITA INCOME IN ILLINOIS AND US

Notes: Base for the index is 2010Q1. In this quarter, per capita personal disposable income was \$41,249 for Illinois, and \$35,285 for US.

Source: Author's calculations based on data series obtained from the Bureau of Economic Analysis. Data series is A229R0 for US, and ILOPCI for Illinois. The series are seasonally adjusted for the US but not for Illinois.

Panel A: Probability of Job Separation



Panel B: Growth in Wages of Employed Workers

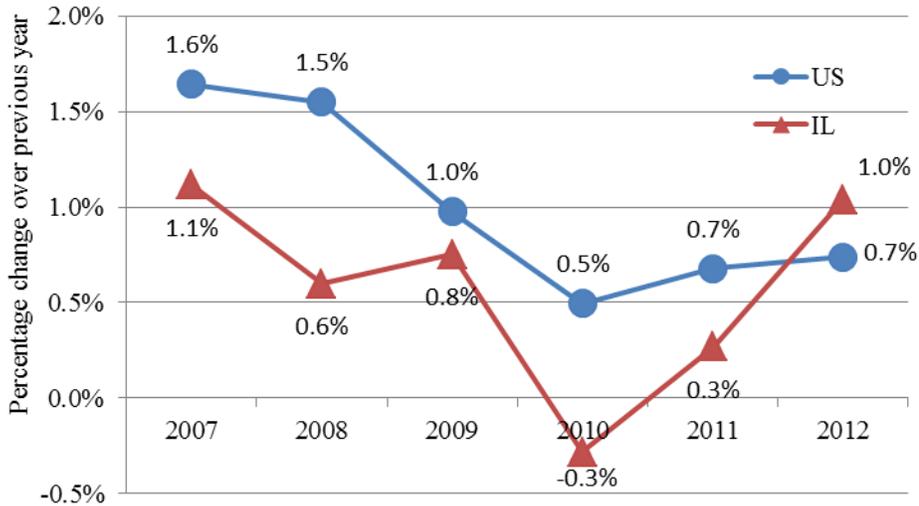


FIGURE 5. PROBABILITY OF JOB SEPARATION AND GROWTH IN THE WAGES OF EMPLOYED WORKERS

Notes: In Panel A, figures corresponding to a given year show the probability that a worker employed in that year would be either unemployed or laid-off in the next year. In Panel B, figures corresponding to a given year show growth in the wages of employed workers between that year and the next year.

Source: Current Population Survey, Monthly Outgoing Rotation Groups, 2007-2012.