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Did the 2008 rebate fail? a response to Taylor and Feldstein

Abstract: *Did the 2008 rebate fail to stimulate consumer spending? In their influential* American Economic Review *articles, John Taylor and Martin Feldstein each claim that BEA (Bureau of Economic Analysis) aggregate time series data show that the 2008 rebate failed. Reexamining the BEA data, we find that the data instead show there is a high probability that the rebate stimulated consumption. Moreover, the hypothesis that a rebate has half the impact of ordinary disposable income cannot be rejected. Thus, we find that analysis of the BEA aggregate time series data is consistent with the conclusion from the micro-data studies that the 2008 rebate stimulated consumer spending.*

Key words: *fiscal policy, fiscal stimulus, tax rebates.*

Did the 2008 rebate fail to stimulate consumer spending? In their influential *American Economic Review (AER)* articles, John Taylor (2009) and Martin Feldstein (2009) each analyzed U.S. Bureau of Economic Analysis's (BEA) National Income and Product Accounts (NIPA) aggregate time series data and concluded that the rebate failed. In this paper we reexamine their analyses of the BEA data.

In February 2008, Congress enacted an economic stimulus package that included a tax rebate for households. The U.S. Treasury mailed checks to households mainly in May, June, and July. Most single individuals received $300 plus $300 per dependent child and most married couples received $600 plus $300 per dependent child. For example, a family of two parents and three children received $1,500. The rebate amount phased in for low-income households and phased out for high-income households.[1]

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[1] Although called a "tax rebate," the payment was technically a credit for tax year 2008 and the phase-in and phase-out were based on reported income for tax year 2007.

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Taylor's and Feldstein's conclusions from the BEA aggregate data about the 2008 rebate contrast with two studies that use individual household micro-data to study the impact of the 2008 rebate (Parker et al., 2011; Sahm et al., 2010). Both studies conclude that the rebate had a significant effect on the spending of a typical household receiving the rebate.

Parker et al. (2011) report on their study of the effect of the 2008 rebate on consumer expenditure using micro-data consisting of the reports of individuals of the dollar amounts of their recent consumer expenditures from the Consumer Expenditure Survey conducted by the U.S. Bureau of Labor Statistics. They write in their abstract:

> We measure the response of household spending to the economic stimulus payments (ESPs) disbursed by mid-2008, using special questions added to the Consumer Expenditure Survey and variation arising from the randomized timing of when the payments were disbursed. We find that, on average, households spent about 12–30% (depending on the specification) of their stimulus payments on non-durable expenditures during the three-month period in which the payments were received. Further, there was also a significant increase in spending on durable goods, in particular vehicles, bringing the average total spending response to about 50–90% of the payments. (ibid.)

Parker et al.'s conclusion for the 2008 rebate is similar to the conclusion from Johnson et al.'s *AER* article on the effect of the 2001 tax rebate using the same kind of micro-data:

> Using questions expressly added to the Consumer Expenditure Survey, we estimate the change in consumption expenditures caused by the 2001 federal income tax rebates and test the permanent income hypothesis. We exploit the unique, randomized timing of the rebate receipt across households. Households spent between 20 to 40 percent of their rebates on nondurable goods during the three-month period in which their rebates arrived, and roughly two-thirds of their rebates cumulatively during this period and the subsequent three-month period. The implied effects on aggregate consumption demand are substantial. Consistent with liquidity constraints, responses are larger for households with low liquid wealth or low income. (2006, p. 1589)

Sahm et al. (2010) evaluate the impact of the 2008 tax rebates based on a survey of individual households about what they say they "mostly did" with their 2008 rebate—the Reuters/University of Michigan Survey of Consumers which is a nationally representative monthly survey based on about 500 telephone interviews. They write:

> In summary, the survey results suggest that roughly one-third of the rebate income was spent in 2008 and that the spending response was concentrated in the first few months after receipt. (ibid., p. 71)

Sahm et al.'s result is consistent with what was found in Shapiro and Slemrod's (2009) study a few months earlier in which they asked households what they *intended* to "mostly do" with their 2008 rebate. Based on the answers about intent, Shapiro and Slemrod estimated that the typical rebate recipient intended to spend about one-third of the rebate in the near future.

To summarize, according to the micro-data studies of the 2008 rebate, the typical rebate recipient spent between one-third and two-thirds of the rebate within six months of receiving the rebate.

In this paper we reexamine the BEA aggregate time series data used by both Feldstein and Taylor. We consider two alternative hypotheses: (1) the Taylor/Feldstein hypothesis that the rebate had little or no effect and (2) the hypothesis that the rebate had half the effect of ordinary disposable income.

After analyzing the same data used by Feldstein and Taylor, we come to the following conclusions. First, we do *not* go to the other extreme and claim that the data show that the rebate definitely worked. We find that the data *do not show* that the rebate failed and instead show there is a high probability that the true rebate coefficient is positive. Moreover, the hypothesis that the rebate had half the impact of ordinary disposable income cannot be rejected. Thus, we find that analysis of the BEA aggregate time series data is consistent with the conclusion from the micro-data studies that the 2008 rebate stimulated consumer spending.

The op-ed columns of Feldstein and Taylor

In their *AER* articles, Feldstein (2009) and Taylor (2009) each refer to their op-ed articles in the *Wall Street Journal (WSJ)* (Feldstein, 2008; Taylor, 2008) on the impact of the 2008 rebate. In this section we review their op-ed articles.

Before discussing Feldstein and Taylor, it is useful to state our own hypothesis about how a rebate works. In our view, when households receive a tax rebate, they deposit the additional cash and their saving initially increases by the amount of the rebate. Gradually, the household spends more than it otherwise would have. Thus, immediately after a household receives a rebate check, we expect a spike in saving, but *not* a spike in spending, relative to what it would have been without the rebate. The key

issue is the time path of consumption spending following receipt of the rebate compared to what it would have been—in particular, the spending differential over the year following the receipt of the rebate.

We accept the view associated with the permanent income hypothesis and the life cycle hypothesis that there is consumption spreading—consumption does not spike whenever disposable income spikes. We are, however, skeptical of the extreme version of either the permanent income hypothesis or the life cycle hypothesis that holds that a rebate would be spread evenly over the remainder of a person's life, in which case its impact on spending in the following year would be virtually zero.

Two aspects of the Feldstein and Taylor op-ed articles require reexamination. First, to assess the impact of any policy, a comparison is required between what actually happened after the policy was implemented and what *would have happened if* the policy had not been implemented. But what would have happened can only be *estimated*—it cannot be known with certainty. Yet both claim that by looking only at actual data—what actually happened—it is possible to assess the impact of a policy.

Second, Feldstein and Taylor focus primarily on the immediate impact—the impact in the month following the household's receipt of the rebate. They note that there is no spike in consumer spending in the month after the rebate and conclude that the rebate did not work. They do not study whether the rebate raised spending *gradually* over the following year (relative to what it otherwise would have been).

Table 1 shows the rebates paid in each month in 2008. The total rebate actually paid out in the two quarters was $92.6 billion: $77.9 billion in 2008Q2 and $14.7 billion in 2008Q3.

Figure 1 shows personal consumption outlays (seasonally adjusted *quarterly amount* in current dollars—the *annual rate* is four times the *quarterly amount* shown in the figure) for each quarter from 2007Q1 through 2009Q4 *but omits the two quarters when the rebates were received,* 2008Q2 and 2008Q3. We cannot know for certain what personal consumption outlays would have been in 2008Q2 and 2008Q3 had there been no rebates. Figure 2 shows *one* possible path for personal consumption outlays in 2008Q2 and 2008Q3 if there had been no rebates.

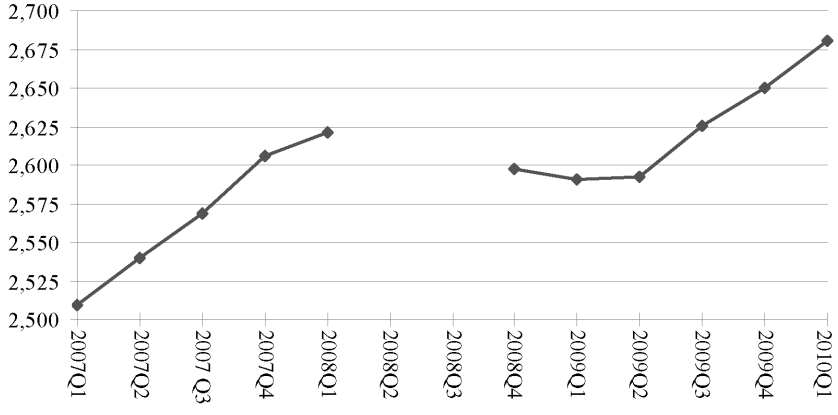
Figure 3 gives the actual values for personal consumption outlays in 2008Q2 and 2008Q3. Compared to the possible path (had there been no rebates) shown in Figure 2 (and also shown in Figure 3), actual personal consumption outlays were $18 billion higher in 2008Q2 and $18 billion higher in 2008Q3. If this possible path would have occurred had there been no rebates, then the $92.6 billion rebates raised personal consumption outlays $36 billion above what they would otherwise have been, so

Table 1
2008 rebate

2008Q2	April		$1.9 billion
	May		$48.1 billion
	June		$27.9 billion
		Subtotal	$77.9 billion
2008Q3	July		$13.7 billion
	August		$1.0 billion
	September		$0.0 billion
		Subtotal	$14.7 billion
		Total	$92.6 billion

Source: Bureau of Economic Analysis (2008a).

Figure 1 Personal outlays (billions of dollars)



Source: Bureau of Economic Analysis (2009).

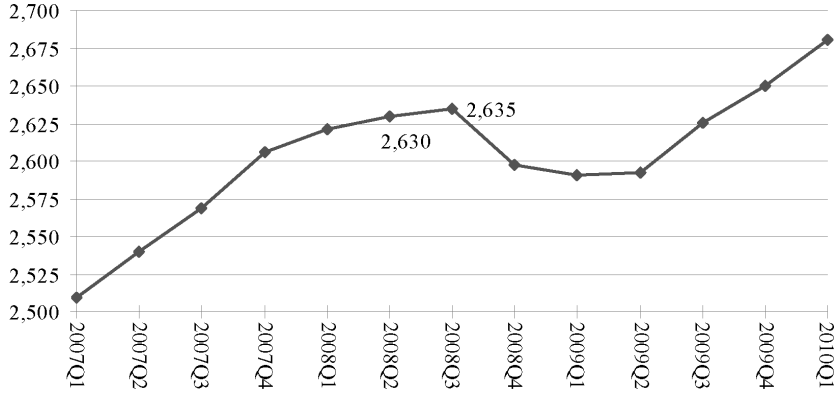
the increase in outlays due to the rebates would have been 39 percent of the rebates ($36/$92.6 = 39 percent).

Of course, we cannot be sure that consumption outlays would have followed the possible path shown in Figure 2. Thus, we cannot be sure that the rebates raised personal consumption outlays by 39 percent of the rebates.

Feldstein

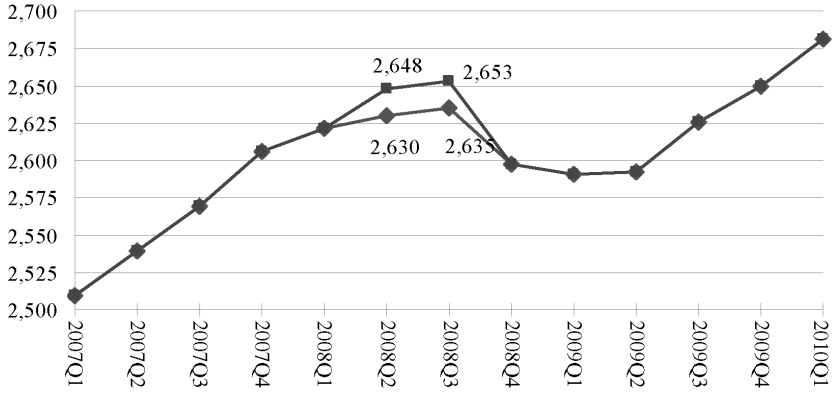
In his 2008 *WSJ* article, entitled "The Tax Rebate Was a Flop. Obama's Stimulus Plan Won't Work Either," Feldstein asserts that he can tell the

Figure 2 Possible personal outlays (billions of dollars) in 2008Q2 and 2008Q3



Source: Bureau of Economic Analysis (2009).

Figure 3 Possible and actual personal outlays (billions of dollars) in 2008Q2 and 2008Q3



Source: Bureau of Economic Analysis (2009).

rebate did not work simply by examining how actual data changed over time. He says:

> Recent government statistics show that only between 10% and 20% of the rebate dollars were spent. The rebates added nearly $80 billion to the permanent national debt but less than $20 billion to consumer spending. This experience confirms earlier studies showing that one-time tax rebates are not a cost-effective way to increase economic activity. (ibid., A15)

Table 2
Quarterly changes in personal outlays

Personal outlays	2007Q4	2008Q1	2008Q2
Change	(+23.9)		(+35.9)
Change of the change		[+12.0]	

He continues:

> Here are the facts. Tax rebates of $78 billion arrived in the second quarter of the year. The government's recent GDP [gross domestic product] figures show that the level of consumer outlays only rose by an extra $12 billion, or 15% of the lost revenue. The rest went into savings, including the paydown of debt. (ibid., A15)

Table 2 presents the BEA data that Feldstein uses in his article (Bureau of Economic Analysis 2008b). Personal outlays increased $23.9 billion from 2007Q4 to 2008Q1, and increased $35.9 billion—$12 billion more than $23.9 billion—from 2008Q1 to 2008Q2. He says this "extra" $12 billion increase is only 15 percent of the $78 billion rebate received in 2008Q2 and claims this shows that only 15 percent of the rebate was spent.

Feldstein implicitly assumes that had there been no rebate, the actual quarterly amount of personal outlays would have increased $23.9 billion in 2008Q2 simply because it increased $23.9 billion in 2008Q1. He then implicitly assumes that it increased $12 billion more than $23.9 billion—$35.9 billion—solely because of the $78 billion rebate in 2008Q2. With these assumptions, he concludes that the $78 billion rebate caused only a $12 billion increase in outlays ($12/$78 = 15 percent).

But just because outlays increased $23.9 billion in 2008Q1 does not mean outlays would have increased $23.9 billion in 2008Q2 had there been no rebate. There is no reason to expect personal outlays to increase the same amount every quarter. For example, the BEA reports that from 2007Q3 to 2007Q4, personal outlays increased $31.8 billion, not $23.9 billion.

What would have happened had there been no rebate in 2008Q2 depends on what else was occurring in the economy. House prices, the Dow Jones average, and the University of Michigan's index of consumer sentiment all fell significantly (Bear Stearns nearly failed in March). These declines might have caused personal outlays to increase less than $23.9 billion had there been no rebate. Suppose that without the rebate other factors would

have caused outlays to increase only $11.9 billion ($12 billion less than $23.9 billion), not $23.9 billion. Then the $78 billion rebate would have caused an "extra" $24 billion, not an "extra" $12 billion; $24 billion is about 30 percent, not 15 percent, of the $78 billion rebate.

Taylor

In his 2008 *WSJ* article, entitled "Why Permanent Tax Cuts Are the Best Stimulus," Taylor gives this account of the mid-2008 rebate to households:

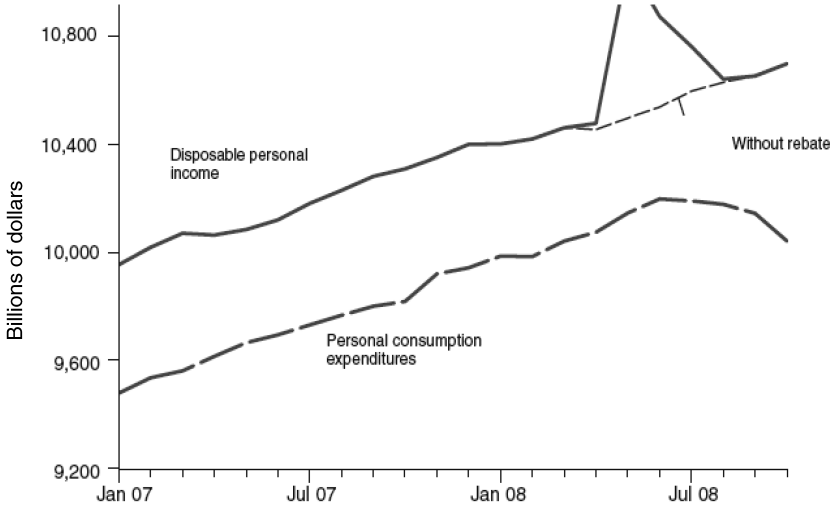
> The major part of the first stimulus package was the $115 billion, temporary rebate payment program targeted to individuals and families that phased out as incomes rose. Most of the rebate checks were mailed or directly deposited during May, June, and July. The argument in favor of these temporary rebate payments is that they would increase consumption, stimulate aggregate demand, and thereby get the economy growing again. What were the results? The chart nearby reveals the answer [see Figure 4, which is a copy of Taylor's *WSJ* chart included in his *AER* article; note that Taylor's chart shows *annual rates,* not *quarterly amounts,* so his numbers are roughly four times the numbers in our Figures 1, 2, and 3, which show *quarterly amounts*]. The upper line shows disposable personal income through September. Disposable personal income is what households have left after paying taxes and receiving transfers from the government. The big blip is due to the rebate payments in May through July. The lower line shows personal consumption expenditures by households. Observe that consumption shows no noticeable increase at the time of the rebate. Hence, by this simple measure, the rebate did little or nothing to stimulate consumption, overall aggregate demand, or the economy. (ibid., p. A15)

Taylor therefore claims that by looking only at how actual data changes from May through July, he can infer that the rebate did not work. He continues:

> These results may seem surprising, but they are not. They correspond very closely to what basic economic theory tells us. According to the permanent-income theory of Milton Friedman, or the life-cycle theory of Franco Modigliani, temporary increases in income will *not* lead to significant increases in consumption. (ibid., p. A15)

Taylor's chart shows a spike in disposable income but no spike in consumption spending. But what Taylor's chart of actual data does not show, and cannot show, is what would have happened to spending from May through the next 12 months had there been no rebate. In mid-2008,

Figure 4 Income, consumption, and the 2008 rebate payments



Source: Taylor (2009, p. 551).

several other influences—the fall in house and stock prices and the unprecedented high level of consumer debt—would likely have reduced spending. Yet actual spending did not fall until September. It is therefore possible that the rebate kept spending steady when it otherwise would have fallen. Of course, no one can know with certainty what would have happened to spending had there been no rebate, but Taylor does not acknowledge this point.

Taylor's and Feldstein's *AER* regressions

In their *AER* articles, after a brief review of their own *WSJ* columns, Taylor and Feldstein each turn to their regression analysis of the BEA data. Before looking at their regressions in detail, it is must be emphasized that a rebate was paid out in *only 6 months*—3 months in 2001 and 3 months in 2008 (a tiny amount of rebate was paid out in the month before and the month after the three months in 2001 and in 2008). Taylor's sample runs 106 months (from January 2000 to October 2008), and Feldstein's sample runs a much larger number of months (from January 1980 to November 2008), but there are only 6 rebate months. Thus, both Taylor and Feldstein have only 6 data points with which to analyze the effect of a rebate.

Table 3
Monthly personal consumption expenditure regressions with rebate payments

	1 Taylor 2009, *AER*	2 Taylor 2009, our replication	3	4	5
Lagged PCE	0.794 (0.057) [13.93]	0.794 (0.058) [13.69]	0.823 (0.060) [13.65]	0.795 (0.057) [13.86]	0.821 (0.060) [13.73]
Rebate payments	0.048 (0.055) [0.87]	0.048 (0.056) [0.86]	0.081 (0.059) [1.36]	0.072 (0.057) [1.26]	0.099 (0.060) [1.65]
DPY (without rebate)	0.206 (0.056) [3.68]	0.206 (0.058) [3.55]	0.182 (0.059) [3.07]	0.205 (0.057) [3.58]	0.184 (0.059) [3.13]
Consumer sentiment			0.833 (0.514) [1.62]		0.742 (0.512) [1.45]
Change in Dow				0.016 (0.009) [1.84]	0.015 (0.009) [1.69]
R^2	0.999	0.999	0.999	0.999	0.999

Notes: Sample period is January 2000–October 2008. Standard errors are shown in parentheses; *t*-values are shown in brackets.

Taylor

Column 1 of Table 3 shows the regression results using monthly BEA data that appear in Taylor's 2009 *AER* article. In his regression, the dependent variable is personal consumption expenditures (PCE), and there are only three right-hand variables: lagged PCE, rebate payments (which occurred only in three months in 2001 and three months in 2008), and disposable personal income excluding any rebate payments (DPY). Taylor finds that the estimated DPY coefficient is 0.206 with a standard error of 0.056, therefore implying a *t*-value of 3.68 (0.206 / 0.056 = 3.68) and that the estimated rebate coefficient 0.048 is roughly a fourth of the estimated disposable income coefficient. The estimated rebate coefficient has a standard error of 0.055, therefore implying a *t*-value of 0.87 (0.048 / 0.055 = 0.87).

Taylor states that "the impact of the rebate is statistically insignificant and much smaller than the significant impact of disposable personal income excluding the rebate" (ibid., p. 551). But this is hardly surprising

because, as emphasized above, there are 106 months of data on disposable income but only 6 months of data on a tax rebate—too little tax rebate data to estimate its impact with much precision.

What conclusion should be drawn from these regression results? The point estimate of the rebate coefficient is 0.048 and the estimated standard error is 0.055, so a 95 percent confidence interval centered on 0.048 for the true rebate coefficient β is (–0.061, +0.157).[2] It is true that this 95 percent confidence interval includes 0. On the other hand, using the same point estimate and estimated standard error, the interval centered on 0.048 with a lower endpoint of 0 and a higher endpoint of $2 \times 0.048 = 0.096$, (0.00, +0.096), is a 61.4 percent confidence interval for the true rebate coefficient β.[3] This means that the probability that β is within this range is 61.4 percent; the probability that β is below this range is 19.3 percent and above this range is 19.3 percent.[4] Hence, the probability that $\beta > 0$ is 80.7 percent.

Thus, based on the regression results from Taylor's sample of data, it is wrong to conclude that the rebate did not work. There is an 80.7 percent probability that $\beta > 0$. An "insignificant" *t*-value does not mean the rebate had no effect.

[2] Following Pindyck and Rubinfeld (1998, p. 68), let $t_{0.025}$ be the *t*-value that leaves 2.5 percent of the distribution in the upper tail. Then $\text{prob}(-t_{0.025} < t < +t_{0.025}) = 0.95$, where $t = [(\hat{\beta} - \beta)/s_{\hat{\beta}}]$ and $s_{\hat{\beta}}$ is the estimated standard error of $\hat{\beta}$. This implies that $\text{prob}\{[\hat{\beta} - t_{0.025}(s_{\hat{\beta}})] < \beta < [\hat{\beta} + t_{0.025}(s_{\hat{\beta}})]\} = 0.95$. Since Taylor's sample size is 106, with 4 regressors including the constant term, there are 102 degrees of freedom (df). Using a *t*-distribution calculator, we find that $t_{0.025}$ with df = 102 equals 1.983. Since $s_{\hat{\beta}} = 0.055$ and $\hat{\beta} = 0.048$, the lower endpoint of the 95 percent confidence interval centered on 0.048 is $0.048 - 1.983 \times 0.055 = -0.061$ and the higher endpoint is $0.048 + 1.983 \times 0.055 = +0.157$.

[3] We obtain the 61.4 percent confidence interval as follows. Assuming $\hat{\beta} > 0$, we consider the interval $(0, 2\hat{\beta})$. Consider the generic confidence interval, $\text{prob}\{[\hat{\beta} - t_c(s_{\hat{\beta}})] < \beta < [\hat{\beta} + t_c(s_{\hat{\beta}})]\} = 1 - 2c$, where c is the area under the *t*-distribution to the right of t_c. We solve this expression for t_c such that the lower bound of the confidence interval is 0: $t_c = \hat{\beta}/s_{\hat{\beta}}$. Given t_c and the degrees of freedom, the *t*-distribution is used to calculate c. In this sample, $\hat{\beta} = 0.048$ and $s_{\hat{\beta}} = 0.055$, so $t_c = 0.048/0.055 = 0.87$. Using a *t*-distribution calculator, we find that with df = 102, the probability that $t > 0.87$ equals 0.193, so $c = 0.193$. Thus, (0, 0.096) is a $1 - 2c = 61.4$ percent confidence interval.

[4] Pindyck and Rubinfeld write: "Assume, for example, that $\hat{\beta}$ is .9. If we choose a level of significance of 10 percent, the 90 percent confidence interval for β might be $.6 < \beta < 1.2$. This means that the probability that β is within the range .6 to 1.2 is .90" (1998, p. 67). This is the sense in which we use the phrase "the probability that β" throughout the paper. Stock and Watson write: "A 95% confidence interval for β . . . is an interval [before actual numbers are assigned] that has a 95% probability of containing the true value of β" (2007, p. 156).

Column 2 of Table 3 reports our replication of Taylor's regression using his data and sample. Our replication is virtually identical—the numbers in column 2 are almost the same as column 1. We find that the estimated rebate coefficient is roughly a fourth of the estimated disposable income coefficient and its *t*-statistic is 0.86.

However, in mid-2008, housing prices were falling, home foreclosures were rising, Bear Stearns had been barely rescued in March, and reflecting these events, the index of consumer sentiment was collapsing and the stock market was plunging. Table 4 shows that in June 2008 (as rebate checks were being received), the University of Michigan's consumer sentiment index fell to a low point of 56.4 (in contrast to its January value of 78.4) and the Dow Jones average plunged 1,288.31 points. All of these downward currents together might have pulled down personal consumption expenditures. Yet Taylor apparently did not try to control for these downward currents.

Inclusion of the consumer sentiment index in column 3 of Table 3 has a dramatic effect on the estimated rebate coefficient. The estimated coefficient nearly doubles so that it is nearly half the value of the estimated disposable income coefficient (0.081 versus 0.182) and its *t*-statistic nearly doubles to 1.36.

The inclusion instead of the change in the Dow Jones average in column 4 has an effect that is similar to the inclusion of the consumer sentiment index: it raises the estimated rebate coefficient and *t*-statistic.

Finally, the inclusion of both the consumer sentiment index and the change in the Dow Jones average in column 5 of Table 3 has a stronger effect on the rebate coefficient than either one alone. The estimated rebate coefficient is now slightly greater than half of the estimated disposable income coefficient (0.099 versus 0.184).

The point estimate of the rebate coefficient is 0.099 and the estimated standard error is 0.060, so a 95 percent confidence interval centered on 0.099 for the true rebate coefficient β is (–0.020, +0.218).[5] It is true that this 95 percent confidence interval includes 0. On the other hand, using the same point estimate and estimated standard error, the interval centered on 0.099 with a lower endpoint of 0 and a higher endpoint of $2 \times 0.099 = 0.198$, (0.00, +0.198), is an 89.8 percent confidence interval for the true rebate coefficient β.[6] This means that the probability that β

[5] With 6 regressors including the constant term there are 100 degrees of freedom, and $t_{0.025} = 1.984$.

[6] We obtain the 89.8 percent confidence interval the same way we obtained the 61.4 percent confidence interval above. In this sample, $\beta\hat{} = 0.099$ and $s_{\beta\hat{}} = 0.060$, so $t_c = 0.099/0.060 = 1.65$. Using a *t*-distribution calculator, we find that with

Table 4
Data for 2008

	Consumer sentiment	Change in Dow
January	78.4	–614.46
February	70.8	–383.97
March	69.5	–3.50
April	62.6	557.24
May	59.8	–181.81
June	56.4	–1,288.31
July	61.2	28.01
August	63.0	165.53
September	70.3	–692.89
October	57.6	–1,525.65
November	55.3	–495.97
December	60.1	–52.65

is within this range is 89.8 percent; the probability that β is below this range is 5.1 percent and above this range is 5.1 percent. Hence, the probability that $\beta > 0$ is 94.9 percent.

In his *AER* article, Taylor also reports the results when he includes the price of oil (lagged three months) in his equation. The column 1 of Table 5 shows the regression results with the price of oil that appear in Taylor's 2009 *AER* article. Taylor finds that the estimated DPY coefficient is statistically significant with a *t*-value of 3.42 (0.188 / 0.055 = 3.42) and that the estimated rebate coefficient is roughly half of the estimated disposable income coefficient with a *t*-statistic of 1.50 (0.081 / 0.054 = 1.50).

The point estimate of the rebate coefficient is 0.081 and the estimated standard error is 0.054, so a 95 percent confidence interval centered on 0.081 for the true rebate coefficient β is (–0.026, +0.188).[7] It is true that this 95 percent confidence interval includes 0. On the other hand, using the same point estimate and estimated standard error, the interval centered on 0.081 with a lower endpoint of 0 and a higher endpoint of $2 \times 0.081 = 0.162$, (0.00, +0.162), is an 86.3 percent confidence interval

df = 100, the probability that $t > 1.65$ equals 0.051, so $c = 0.051$. Thus, (0, 0.198) is a $1 - 2c = 89.8$ percent confidence interval.

[7] With 5 regressors including the constant term there are 101 degrees of freedom, and $t_{0.025} = 1.984$.

Table 5
Monthly personal consumption expenditure regressions with rebate payments with oil price

	1	2	3	4	5
	Taylor 2009, *AER*	Taylor 2009, our replication			
Lagged PCE	0.832	0.832	0.844	0.829	0.840
	(0.056)	(0.056)	(0.058)	(0.056)	(0.058)
	[14.86]	[14.78]	[14.47]	[14.72]	[14.40]
Rebate payments	0.081	0.086	0.100	0.096	0.109
	(0.054)	(0.055)	(0.057)	(0.056)	(0.058)
	[1.50]	[1.57]	[1.74]	[1.73]	[1.88]
DPY (without rebate)	0.188	0.189	0.179	0.190	0.180
	(0.055)	(0.055)	(0.057)	(0.055)	(0.057)
	[3.42]	[3.43]	[3.14]	[3.45]	[3.16]
Oil price (lagged three months)	–1.007	–1.100	–1.028	–1.008	–0.942
	(0.325)	(0.322)	(0.334)	(0.333)	(0.345)
	[–3.10]	[–3.42]	[–3.08]	[–3.02]	[–2.73]
Consumer sentiment			0.416		0.396
			(0.512)		(0.513)
			[0.81]		[0.77]
Change in Dow				0.009	0.009
				(0.009)	(0.009)
				[1.05]	[1.01]
R^2	0.999	0.999	0.999	0.999	0.999

Notes: Sample period is January 2000–October 2008. Standard errors are shown in parentheses; *t*-values are shown in brackets.

for the true rebate coefficient β.[8] This means that the probability that β is within this range is 86.3 percent; the probability that β is below this range is 6.8 percent which is equal to the probability that it is above this range. Hence, the probability that $\beta > 0$ is 93.2 percent.

Column 2 of Table 5 reports our replication of Taylor's regression using his data and sample. Our replication is virtually identical—the numbers in column 2 are almost the same as in column 1. We find that the estimated rebate coefficient is roughly half of the estimated disposable income coefficient and its *t*-statistic is 1.57.

[8] We obtain the 86.3 percent confidence interval the same way we obtained the 61.4 percent confidence interval above. In this sample, $\hat{\beta} = 0.081$ and $s_{\hat{\beta}} = 0.054$, so $t_c = 0.081/0.054 = 1.5$. Using a *t*-distribution calculator, we find that with df = 101, the probability that $t > 1.5$ equals 0.068, so $c = 0.068$. Thus, (0, 0.162) is a $1 - 2c = 86.3$ percent confidence interval.

The inclusion of the consumer sentiment index in column 3 of Table 5 increases the *t*-statistic of the rebate to 1.74. The inclusion of the change in the Dow Jones average in column 4 increases the *t*-statistic of the rebate to 1.73.

Both the consumer sentiment index and the change in the Dow Jones average are included in column 5 of Table 5. The estimated rebate coefficient is slightly greater than half of the estimated disposable income coefficient (0.109 versus 0.180) and its *t*-statistic is 1.88.

The point estimate of the rebate coefficient is 0.109 and the estimated standard error is 0.058, so a 95 percent confidence interval centered on 0.109 for the true rebate coefficient β is (–0.006, +0.224).[9] It is true that this 95 percent confidence interval includes 0. On the other hand, using the same point estimate and estimated standard error, the interval centered on 0.109 with a lower endpoint of 0 and a higher endpoint of $2 \times 0.109 = 0.218$, (0.00, +0.218), is a 93.7 percent confidence interval for the true rebate coefficient β.[10] This means that the probability that β is within this range is 93.7 percent; the probability that β is below this range is 3.2 percent and above this range is 3.2 percent. Hence, the probability that $\beta > 0$ is 96.9 percent.

Testing an alternative hypothesis

A plausible alternative hypothesis is that a rebate payment has roughly half the impact of ordinary disposable income on consumption expenditure. Blinder (1981, p. 47) summarizes his empirical results studying the effect of temporary tax changes and transfers on consumption using aggregate time series data this way:

> Though the standard error is unavoidably large, the point estimate suggests that a temporary tax change is treated as a 50–50 blend of a normal income tax change and a pure windfall. Over a 1-year planning horizon, a temporary tax change is estimated to have only a little more than half as much impact as a permanent change of equal magnitude, and a rebate is estimated to have only about 38 percent as much impact. (ibid., p. 47)

We consider the null hypothesis that the true rebate coefficient is half the value of the true disposable income coefficient—equivalently, the

[9] With 7 regressors including the constant term there are 99 degrees of freedom, and $t_{0.025} = 1.984$.

[10] We obtain the 93.7 percent confidence interval the same way we obtained the 61.4 percent confidence interval above. In this sample, $\hat{\beta} = 0.109$ and $s_{\hat{\beta}} = 0.058$, so $t_c = 0.109/0.058 = 1.88$. Using a *t*-distribution calculator, we find that with df = 99, the probability that $t > 1.88$ equals 0.032, so $c = 0.032$. Thus, (0, 0.218) is a $1 - 2c = 93.7$ percent confidence interval.

rebate coefficient minus half the disposable income coefficient is equal to zero. In the Table 3 regressions where an oil price is not included in any equation, the ratio of the estimated rebate coefficient to the estimated disposable income coefficient in each column is as follows: column 1, 0.048/0.206 = 0.23; column 2, 0.048/0.206 = 0.23; column 3, 0.081/0.182 = 0.45; column 4, 0.072/0.205 = 0.35; and column 5, 0.099/0.184 = 0.54.

We perform a *t*-test of the hypothesis that the rebate coefficient minus half the disposable income coefficient is equal to zero. In column 2 of Table 3, which has Taylor's variables, the point estimate of the rebate coefficient minus half the disposable income coefficient is –0.054 and the estimated standard error of the "difference" is 0.058, so a 95 percent confidence interval for the true difference is (–0.169, +0.061), which comfortably includes the value of 0. Thus, we cannot reject the null hypothesis that the rebate coefficient equals half the disposable income coefficient—that is, the difference is 0. It can be shown that the interval centered on –0.054 (–0.108, 0.00) is a 17.7 percent confidence interval so that the probability that the true value of the difference is positive is 41.2 percent.

In column 5 of Table 3, where the regression includes Taylor's variables plus the consumer sentiment index and the change in the Dow Jones average, the point estimate of the rebate coefficient minus half the disposable income coefficient is +0.007 and the estimated standard error of the "difference" is 0.064, so a 95 percent confidence interval for the true difference is (–0.120, +0.134), which comfortably includes the value of 0. Once again, we cannot reject the null hypothesis that the rebate coefficient equals half the disposable income coefficient—that is, the difference is 0. It can be shown that the interval centered on +0.007 (0.00, 0.014) is a 45.7 percent confidence interval so that the probability that the true value of the difference is positive is 72.8 percent.

We also test the null hypothesis using the regressions in Table 5 where an oil price is included in each equation. In the Table 5 regressions, the ratio of the estimated rebate coefficient to the estimated disposable income coefficient in each column is as follows: column 1, 0.081/0.188 = 0.43; column 2, 0.086/0.189 = 0.46; column 3, 0.100/0.179 = 0.56; column 4, 0.096/0.190 = 0.51; and column 5, 0.109/0.180 = 0.61.

Again we perform a *t*-test of the hypothesis that the rebate coefficient minus half the disposable income coefficient is equal to zero. In column 2 of Table 5, which has Taylor's variables, the point estimate of the rebate coefficient minus half the disposable income coefficient is –0.009 and the estimated standard error of the "difference" is 0.057, so a 95 percent

confidence interval for the true difference is (–0.122, +0.104), which comfortably includes the value of 0. Thus, we cannot reject the null hypothesis that the rebate coefficient equals half the disposable income coefficient—that is, the difference is 0. It can be shown that the interval centered on –0.009 (–0.018, 0.00) is a 43.7 percent confidence interval so that the probability that the true value of the difference is positive is 28.1 percent.

In column 5 of Table 5, where the regression includes Taylor's variables plus the consumer sentiment index and the change in the Dow Jones average, the point estimate of the rebate coefficient minus half the disposable income coefficient is +0.019 and the estimated standard error of the "difference" is 0.062 so a 95 percent confidence interval for the true difference is (–0.104, +0.142), which comfortably includes the value of 0. Once again, we cannot reject the null hypothesis that the rebate coefficient equals half the disposable income coefficient—that is, the difference is 0. It can be shown that the interval centered on +0.019 (0.00, 0.038) is a 38 percent confidence interval so that the probability that the true value of the difference is positive is 69 percent.

Feldstein

In contrast to Taylor, who presents his regression results with details in a table, Feldstein provides only this paragraph:

> More recently, Stephen Miran and I estimated a consumer expenditure equation using monthly data from January 1980 through November 2008. While the marginal propensity to consume (MPC) out of real per capita disposable income is estimated to be 0.70, the estimated MPC from the corresponding rebate variable is only 0.13 (standard error 0.05). (The other variables in the equation are the unemployment rate, the ten-year interest rate, and a quadratic time trend.) A variety of short distributed lag specifications confirms this result and indicates that there is no delayed impact of the rebate; all of the monthly lag coefficients are completely insignificant and their sum is negative. (2009, p. 557)

Note that the implied *t*-statistic of his rebate variable is 0.13 / 0.05 = 2.6 so that even his regression makes it likely that the rebate had a positive effect on consumption. Like Taylor, Feldstein did not include the consumer sentiment index or the change in the Dow Jones average to capture consumer anxiety. The unemployment rate variable is not a satisfactory substitute because it lags behind the economy. For example, in May 2008 the unemployment rate was still only 5.4 percent. By contrast, the consumer sentiment index had already plunged.

Regressions with quarterly data

Neither Taylor nor Feldstein report regressions with quarterly data, which are commonly used in macro-econometric models. Moreover, quarterly data may be preferable for testing the impact of a rebate on consumption because, as we emphasized earlier, when a household receives a rebate check it usually deposits the check, initially raising its saving, and only gradually raises its spending over the next year, so one month may not be enough time to detect the impact of the rebate on spending.

Table 6 presents the regression results over Taylor's sample period using the BEA quarterly data that corresponds to Taylor's BEA monthly data. The regressions include the same variables used in the monthly regressions reported in Tables 3 and 5. In Table 6, columns 1 through 4 report the same regressions as Table 3 where the oil price is omitted, and columns 5 through 8 report the same regressions as Table 5 where the oil price is included. In column 1 of Table 6, the rebate coefficient is 0.170 with a *t*-value of 2.10. By contrast, in column 1 of Table 3 with monthly data, Taylor's regression equation had a rebate coefficient of only 0.048 with a *t*-value of only 0.87. Thus, if Taylor had used quarterly data instead of monthly data, he would have found a statistically significant impact of a rebate on consumer spending.

Columns 2 through 4 of Table 6 include first the consumer sentiment index, then the change in the Dow Jones average, and then both. Columns 5 through 8 repeat columns 1 through 4 except they include the oil price. In all eight equations (columns 1 through 8), the rebate coefficient stays about 0.16 with a *t*-statistic of about 2.

When a rebate is received, the coefficient of the rebate variable shows the marginal propensity to consume in the same quarter. But the rebate continues to affect spending in the next quarter through the lagged PCE variable. In column 1 of Table 6, the coefficient of lagged PCE is 0.728 and the rebate coefficient is 0.170. This implies that in the second quarter, the lagged effect of the rebate on consumption would be $0.728 \times 0.170 = 0.124$, so the impact of the rebate on spending over two quarters equals $(0.170 + 0.124) = 0.294$, as indicated in the table—the two-quarter MPC is 0.294. Similarly, the impact over four quarters equals $(0.170 + 0.124 + 0.090 + 0.066) = 0.450$.[11]

Looking across the columns of Table 6, the two-quarter MPC is fairly stable at roughly 0.28 and the four-quarter MPC is fairly stable at roughly 0.44.

[11] The same calculation gives the lagged effects of ordinary disposable income (DPY) shown in Table 6.

Table 6
Quarterly PCE regressions with rebate payments

	1	2	3	4	5	6	7	8
Lagged PCE	0.728	0.807	0.728	0.806	0.708	0.787	0.708	0.787
	(0.089)	(0.096)	(0.091)	(0.098)	(0.099)	(0.105)	(0.101)	(0.107)
	[8.15]	[8.40]	[7.99]	[8.23]	[7.12]	[7.47]	[6.99]	[7.33]
Rebate payments	0.170	0.149	0.169	0.148	0.170	0.149	0.171	0.149
	(0.081)	(0.079)	(0.083)	(0.081)	(0.082)	(0.080)	(0.084)	(0.082)
	[2.10]	[1.89]	[2.05]	[1.84]	[2.07]	[1.86]	[2.03]	[1.83]
DPY (without rebate)	0.272	0.205	0.273	0.206	0.286	0.218	0.286	0.218
	(0.087)	(0.091)	(0.089)	(0.093)	(0.092)	(0.097)	(0.094)	(0.098)
	[3.13]	[2.24]	[3.08]	[2.20]	[3.10]	[2.26]	[3.05]	[2.22]
Oil price					0.309	0.294	0.319	0.298
					(0.624)	(0.601)	(0.664)	(0.640)
					[0.50]	[0.49]	[0.48]	[0.47]

(continues)

Table 6
(continued)

	1	2	3	4	5	6	7	8
Consumer sentiment		1.647		1.648		1.640		1.64
		(0.893)		(0.908)		(0.905)		(0.921)
		[1.84]		[1.81]		[1.81]		[1.78]
Change in Dow			–0.001	–0.001			0.000	0.000
			(0.008)	(0.008)			(0.009)	(0.008)
			[–0.09]	[–0.12]			[0.05]	[0.02]
R^2	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.999
Two quarters								
Rebate	0.294	0.269	0.292	0.267	0.290	0.266	0.292	0.269
DPY	0.470	0.370	0.472	0.372	0.488	0.390	0.488	0.390
Four quarters								
Rebate	0.449	0.445	0.447	0.441	0.436	0.431	0.438	0.431
DPY	0.719	0.612	0.722	0.614	0.733	0.631	0.733	0.631

Notes: Sample period is January 2000–March 2008. Standard errors are shown in parentheses; *t*-values are shown in brackets.

Conclusion

Did the 2008 rebate fail to stimulate consumer spending? In their influential *AER* articles, Taylor (2009) and Feldstein (2009) claim that the BEA's NIPA aggregate time series data show that the 2008 rebate failed. They both conclude that policymakers should therefore not repeat a failed policy.

In this paper we reexamined the BEA aggregate time series data used by Feldstein and Taylor. We found that the aggregate time series data *do not show* that the rebate failed. We considered two alternative hypotheses: (1) the Taylor/Feldstein hypothesis that the rebate had little or no effect and (2) the hypothesis that the rebate had half the effect on consumption of ordinary disposable income.

After analyzing the same data used by Feldstein and Taylor, we came to these conclusions. First, we did *not* go to the other extreme and claim that the data show that the rebate definitely worked. We found that the data *do not show* that the rebate failed and instead show there is a high probability that the true rebate coefficient is positive. Moreover, the hypothesis that the rebate has half the impact of ordinary disposable income cannot be rejected. Thus, we found that analysis of the BEA aggregate time series data is consistent with the conclusion from the micro-data studies that the 2008 rebate stimulated consumer spending.

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