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## **FLAWED STUDY SHOULD BE GIVEN NO CREDENCE IN EVALUATING JOBS AND REVENUE IMPACT OF CALIFORNIA CORPORATE TAX BREAK**

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### **Summary**

A coalition of California corporations has released an economic analysis of the job and revenue gains the state supposedly can expect to see if a corporate tax break is allowed to go into effect next year. The study is so flawed, however, that it should not be given any credence in evaluating the potential impact of the tax break on California employment and tax receipts.

The study is based on a statistical analysis of job changes in five other states. It asserts that California will experience job gains and no state tax revenue loss if it implements a “single sales factor apportionment formula” (SSFF) for the state corporate income tax in 2011, as scheduled under 2009 legislation. The formula, a part of the state’s tax code, determines the share of a multistate corporation’s nationwide profit that the state will tax. Proposition 24, on the November 2, 2010 ballot, would block implementation of the new formula along with two other corporate tax breaks if the voters approve the measure; the older, less generous formula, would be retained.

The study, by university professor Charles W. Swenson, makes its predictions based on changes in employment patterns in Georgia, Louisiana, New York, Oregon, and Wisconsin over the 2003-2008 period, a period in which each of those states implemented SSFF. Swenson compares the rate of job creation at each employer, in each state, during each of two periods – 2003-05 and 2006-08. He finds that employers were more likely to add more jobs in the later period if they had a set of characteristics that, in his opinion, are likely associated with benefitting from this particular tax break. He concludes from this that benefitting from the tax break caused employers to accelerate job creation, and estimates a statistical relationship between the jobs and the tax-break-affected characteristics. He then applies this conclusion to California, and predicts modest job growth. Finally, using that estimate of job growth and a related estimate of increased corporate profits, he predicts very large tax revenue growth.

Flaws in this study include the following:

- **Jobs data that diverge dramatically from official government statistics.** Official U.S. government data, as well as a broad consensus of economists, holds that 2003-2008 was a period of employment growth across the country. In fact, the U.S. Bureau of Labor Statistics finds that in both the 2003-2005 period and the 2006-2008 period, in all five states studied, there was *job growth*. But Swenson is not using publicly available, official data, but rather a proprietary dataset. And according to the figures he reports from his dataset, in all five of those states, in both periods, there was a large *decline* in employment. There are other elements in Swenson's reported data that also suggest it may contain errors, such as huge, erratic, and inexplicable swings in calculations of total sales. Unfortunately, because it is a private database available only to those able to pay relatively large fees, it is not possible to determine whether the source of these apparent errors is the database itself or the way Swenson has manipulated it. But they might well render this analysis worthless.
- **Misstatement of time periods.** Swenson writes on p. 4 of his study that all five states "switched" to SSFF "in 2006," which if true would validate his approach of contrasting job creation in the 2003-05 and 2006-08 periods in those states. He calls these "natural experiments." In fact, four of the five states implemented SSFF gradually over this period, and the implementation periods vary greatly. For example, Oregon was well on its way to full implementation of SSFF in 2003; Georgia by contrast did not start it until 2006 and did not fully implement SSFF until 2008. As a result, Swenson's differentiation of the two periods as a clearcut pre-SSFF period and a post-SSFF period is wrong.
- **Misidentification of which firms benefited from the tax break.** Swenson's conclusions are entirely dependent on the accuracy with which he categorizes firms as being likely to benefit from SSFF or not. But his categorization is extremely rough; his data do not actually allow him to identify employers that have out-of-state sales, employees, or property, so he uses a series of rough proxies. One of his proxies, for instance, is simply whether the employer has subsidiaries, even though SSFF is not limited to corporations with subsidiaries. Another proxy is whether the employer is a publicly traded corporation, again legally unrelated to SSFF eligibility. Yet another is the industrial sector of the business; Swenson contends that the single sales factor formula is inapplicable to retail store chains in "most of the five states examined," when in fact it applies to such businesses in all of them.

As it turns out, his strategy for identifying businesses that are likely to be eligible for SSFF is demonstrably incorrect, and not even close. Swenson concludes that there were 3,109 firms in Georgia that benefited from the state's adoption of SSFF, but the state's own study found that the actual number was 12,426, almost four times as high. Swenson classified 1,796 Wisconsin firms as likely to be affected by the SSFF, but a state study put the number at 6,404, more than three times as many. In two other states as well, the state estimate of SSFF-affected firms is several times larger than Swenson's. In other words, Swenson's technique for measuring the impact of the tax break excludes most of the businesses that were actually eligible for it. This proves that the methodology is well off the mark.

- **What about the other states that didn't enact SSFF in this time period?** Normally, one would expect a study of the connection between state tax policy and economic growth to look at all 50 states if at all possible. But this study completely ignores all the other 45 states,

including the 23 states with corporate income taxes that have never implemented the SSFF. Without such a “control group,” we cannot know whether the changing employment patterns that Swenson claims to have discovered were specific to those five states, or whether they were in fact widespread across the U.S. economy — and therefore entirely independent of SSFF implementation. It is totally possible, for instance, that what Swenson’s data actually show is that the 2006-08 period was much better for big, multistate corporations in every state — for some reason unrelated to state tax treatment — than for smaller employers. In fact, there are at least some national data that suggest this is the case.

- **Faulty tax assumptions.** Even if Swenson’s conclusions about job creation were right, which for the reasons described above they probably are not, it would not necessarily prove that SSFF implementation is a good idea. Other analyses of the impact of corporate tax cuts on state economic growth — including one based on the state’s own model of the California economy — have found that it would cost California state government a huge amount of tax revenue that it can ill afford to lose. But Swenson finds that, in fact, the new jobs (and associated boost in corporate profits) would produce so much tax revenue that it would offset the cost of the tax break. To make this calculation, he assumes that every dollar of new corporate profits would increase California tax revenue by 19 cents, and the average new worker would pay an additional \$5,167 in California personal income and sales taxes. But these are unrealistic assumptions. California business taxes as a share of California corporate profits are probably half as much as Swenson assumes. And a worker earning the average annual wage in California of about \$50,000 generally will pay significantly less than \$5,167 in combined income and sales taxes.
- **Complete lack of transparency and peer review.** Standard practice in economic analysis is to present, for a reader’s inspection, the calculations and data on which the analysis was based. The Swenson report does not make even the most minimal attempts to show how the results of its statistical analyses translate into actual predictions of job and revenue growth in California; policymakers and potential expert reviewers are supposed to take it on faith that these calculations were done properly. Nor is there any evidence that it was submitted to any independent economists for review before publication. Nor has Swenson responded to repeated attempts by one of the authors of this report to contact him for his methodology and results.
- **Math and typographical errors.** Finally, it is worth noting that the Swenson report is rife with careless arithmetic and presentation errors; even simple percentage changes between two numbers are repeatedly miscalculated. This further complicates any attempt to understand and critique his methodology, since many of his results cannot be replicated.

These are just a few of the flaws in the Swenson study; they are explained further, and other flaws are described, in the remainder of this report.

## The Swenson Study

The study that is the subject of this analysis was prepared by Charles W. Swenson, a professor at the Marshall School of Business at the University of Southern California.<sup>1</sup> “California Competes, a group of California-based biotech, media, and high-tech companies,” announced its release on June 24.<sup>2</sup> Some press reports at the time describe the group as having “commissioned” the study,<sup>3</sup> but the report itself contains no acknowledgement of that, and whether the group paid for it cannot be determined since its members were not identified and it has no website.

The Swenson study attempts to estimate the job gains and tax revenue impacts that California can expect to realize as a result of allowing SSFF apportionment for the state corporate income tax to go into effect in 2011, as scheduled under 2009 legislation. The formula, embedded in the state’s tax code, determines the share of a multistate corporation’s nationwide profit that the state will tax. The *current* formula determines that share with reference to the shares of a corporation’s nationwide property, payroll, and sales that are located in California. Under the “single sales factor formula” (SSFF), the share of a corporation’s profit taxable by California will be determined solely with reference to its sales in the state. Thus, for example, a corporation that has all of its property and employees in California but makes all of its sales outside the state would no longer owe any state corporate income tax under the SSFF. The 2009 legislation allows corporations, beginning in 2011, to choose between the SSFF and the current formula. Proposition 24, on the November 2, 2010 ballot, would repeal the SSFF along with two other corporate tax breaks if the voters approve it.

## Overview of the Swenson Methodology and Conclusions

The Swenson study relies on the supposed fact that five other states — Georgia, Louisiana, New York, Oregon, and Wisconsin — enacted SSFF in the same year, 2005. For each of the five states separately, the study compares the rate of job creation/retention in the two years immediately prior to the enactment of SSFF (January 2003-January 2005) with a two-year period following its implementation (January 2006-January 2008) for two groups of firms.

The first group of firms includes those that Swenson believes are unlikely to have been affected by the adoption of the SSFF in their state, that is, not incentivized to create in-state jobs as a result of the formula’s enactment. The second group of firms is those that *were* likely incentivized by the adoption of the SSFF to create in-state jobs. Swenson uses a statistical technique referred to as regression analysis to compare the two groups of firms within each state, correcting for the fact that the two groups might be concentrated in different industries with different underlying growth rates during the time periods examined. Swenson assumes that if the rate of job growth between the two, two-year intervals accelerated more for the SSFF-affected firms than it did for the SSFF-non-affected firms in each state, it was attributable to the implementation of SSFF in the intervening year.

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<sup>1</sup> The Swenson study is available on the website of the business coalition opposing repeal of the tax break, at [www.stopprop24.com/wp-content/uploads/SSF-Study-\\_6-2-10\\_.pdf](http://www.stopprop24.com/wp-content/uploads/SSF-Study-_6-2-10_.pdf). Downloaded July 15, 2010.

<sup>2</sup> “California’s Single Sales Factor Formula Will Create 144,000 New Jobs,” BusinessWire.com, June 24, 2010.

<sup>3</sup> See, for example, Greg Lucas, “Weighing the Economic Benefits of the Single Sales Factor,” California’s Capitol, <http://californiascapitol.com/blog/?p=3433>, July 16, 2010.

Swenson claims that the statistical analysis demonstrates that the adoption of the SSFF in each of these five states did indeed create jobs that otherwise would not have been created, boosting employment in those states by an average of about one percent in just two years relative to what it otherwise would have been. He then applies that growth rate to the current level of employment in California, asserting that the adoption of the SSFF would lead to the creation of 144,000 jobs within a two-year period. Finally, he makes very rough calculations of both the California personal income taxes that would be paid by these employees and additional taxes that would be paid by the expanding corporations. He claims that these new taxes would generate annual revenue far in excess of the \$800 million direct cost of SSFF itself, resulting in a *net* increase in state revenue of more than \$400 million each year.

The remainder of this paper will describe a host of problems with the study, ranging from numerous errors in the presentation of its underlying data and its results, to gross inconsistencies with other published data on job growth and the number of SSFF-affected firms within the five states during the period studied, to highly problematic methodological choices and assumptions.

## **Problems with the Underlying Data**

As will be discussed at greater length below, the data upon which Professor Swenson's statistical analysis relies derive from a relatively new, private proprietary database, the National Employment Time Series — NETS. There appear to be some significant problem with the data; they simply do not make sense in some significant ways. Whether those problems are inherent in the data or result from Professor Swenson's manipulation of it cannot be determined without access to it. In either case, if the data are indeed flawed, then the conclusions drawn from it are suspect.

Most significantly, there is virtually no correspondence between the changes in employment for all firms combined in each of the five states reported in Tables 1-6 and the changes for the same periods in total state payroll employment reported by the federal Bureau of Labor Statistics. Indeed, the year-to-year changes in the BLS data go in the *opposite direction* of the data used by Swenson in every time interval in all five states. Table 1 below shows how divergent the two sources of data are. For each of the five states, the study reports the change in total state employment in "all firms" in the state between January 2003 and January 2005 and between January 2006<sup>4</sup> and January 2008 as taken from the NETS database. For all 10 of those data points, Swenson reports a *drop* in total in-state employment; for all 10, the Bureau of Labor Statistics reports *gains* in total in-state (non-farm) employment.<sup>5</sup> Before the results of the report can be considered credible, Professor Swenson

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<sup>4</sup> As discussed below, this second interval is actually labeled as January 2005, but the description of the study methodology seems to indicate that is in fact January 2006 and the percentage changes reported correspond to percentage changes between values reported in the table for January 2006.

<sup>5</sup> As will be discussed below, it appears that the data being reported for "all firms" by Swenson includes non-profit and governmental organizations in addition to profit-making businesses; that is why the BLS data for all non-farm employment is being used for comparison purposes in Table 1. However, if BLS data for just private firms were used, the picture would be the same — employment gains in both time intervals in all five states.

<b>Table 1: Comparison of Bureau of Labor Statistics and Swenson Study Employment Growth Rates (Swenson: All establishments in State BLS: Payroll employment, all non-farm establishments)</b>				
	<b>January 2003-January 2005</b>		<b>January 2006-January 2008</b>	
	Swenson	BLS	Swenson	BLS
Georgia	-7.8%	+2.9%	-10.8%	+2.4%
Louisiana	-4.7%	+1.2%	-9.4%	+6.7%
New York	-0.6%	+0.8%	-7.3%	+2.7%
Oregon	-7.8%	+3.0%	-10.8%	+3.3%
Wisconsin	-6.8%	+1.7%	-9.4%	+1.4%

should explain why the critical state-by-state employment data upon which his study relies appear to diverge so sharply from the official federal government employment statistics.<sup>6</sup>

There is another, completely separate reason to have grave suspicions about the dataset. In three of the five states examined — Georgia, Louisiana, and Oregon — Swenson reports there were very dramatic jumps between 2005 and 2006 in reported business *sales* in the study’s data. In a single year, the reported sales of the *average* SSFF-affected firm in Georgia jumped 57 percent, in Louisiana 80 percent, and in Oregon 75 percent. For the largest non-SSFF-affected firms, the increases were sometimes even larger.

These increases in sales are completely implausible. Swenson, in the study, describes them as “curious.”<sup>7</sup> But the study offers no explanation and provides no indication that Swenson took any steps to determine whether there was some problem with his data. The fact that in all three states the sales figures then dropped dramatically in 2007 should have been a further tip-off that there is something wrong with the data.

In short, there appear to be some serious anomalies in both the business employment and sales data upon which the Swenson report relies. Until these can be explained, the study’s results cannot be trusted.

### **Egregious Lack of Transparency and Dubious Assumptions in Deriving Employment and Revenue Estimates for California**

In presenting the key aspects of his report — the translation of the five state regression results into specific predictions of job and revenue gains for the state of California resulting from the

<sup>6</sup> The NETS data include self-employed individuals who are not covered by the unemployment compensation system while BLS data do not. During economic downturns, therefore, the NETS data would tend to show slower employment declines than the BLS data, since someone laid-off from a job covered by unemployment compensation who goes into business for herself would be counted as a lost job by the BLS but not by NETS. (Mazerov conversation with Don Walls, Walls & Associates, September 23, 2009.) But this would hardly seem to explain the pattern in Table 1, since the job trends shown go in sharply opposite directions, and all but the final month of the January 2003 through January 2008 period was one of positive national economic growth. (The arbiter of national business cycles, the National Bureau of Economic Research, says that the economy reached its last peak in December 2007.)

<sup>7</sup> One conceivable explanation for such a sharp one-year increase in sales is that one or more major corporations headquartered in a state sold a large subsidiary, realized a multi-billion-dollar capital gain, and NETS assigned the gain to the corporate headquarters. But if that is the case, such an “outlier” observation sharply distorts the regression results and should be removed or adjusted.

enactment of the SSFF — Swenson did not make even the most minimal attempt to “show his work.” The results of these critical calculations simply should not be given any credence until they are disclosed completely and thoroughly. Moreover, as described below, most intermediate steps in these calculations have significant flaws. Finally, it should be noted that Professor Swenson’s predictions of both the job gains California can expect from enactment of the SSFF and the revenue impact of the change diverge sharply from the conclusions of most other economic studies of the impact of corporate tax breaks, including some specific to California. The problems with the study’s predictions for California include the following:

- The report asserts that the regression results mean that the enactment of SSFF resulted in the creation of a specific number of jobs in each state.<sup>8</sup> The report offers no explanation of how those numbers were derived from the regression results.
- The report asserts that those specific job numbers translate to an average 0.83 percent increase in employment in the five states. The report provides no derivation of that figure. Moreover, the report says that the denominator used to calculate the percentage change is the NETS database figure for total employment for all firms in the state; as discussed above on pages 5-6, the NETS data figures diverge sharply from Bureau of Labor Statistics data for reasons Swenson has not explained.
- Swenson then proceeds to predict the additional California *revenue* that will be generated as a result of adopting the SSFF. That calculation starts with the predicted sales growth in the SSFF-affected firms in the five states, which are used to create industry averages. (Here again, Swenson makes no effort to show any of the steps in this calculation.) As discussed above, those sales data evidence abrupt and highly-implausible year-to-year changes in three of the five states Swenson studied. Moreover, there is a serious question as to whether the sales data in the NETS database are sufficiently reliable to be used in this way at all. As will be discussed below, the NETS database is not a database of firms; it is actually a database of individual locations within firms. The sales figures assigned to these individual locations are not reported by the companies themselves; they are assigned by taking the national level of sales of the parent company — or even the average number of sales per employee in the industry— and then apportioning them to the individual establishment based on the reported number of employees.<sup>9</sup> This is a very crude methodology, and while it may provide a reasonable “order of magnitude” estimate, it is highly questionable that it is sufficiently accurate to be the base upon which to build a state revenue forecast.
- The average rates of growth in industry-by-industry sales are apparently then used to create industry-by-industry growth in California profits by multiplying them by average industry rates

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<sup>8</sup> The alleged job gains range from 12,203 new jobs in Wisconsin to 104,158 jobs in New York. See Table 1 of the Swenson report, p. 4.

<sup>9</sup> Documentation provided by the database’s owner states: “Sales are reported at the firm level; so a standalone establishment would generally report its sales. For other establishments, firm-level or, when available, industry sales per employee are used to estimate the establishment sales. In many cases, there are no sales at the establishment because it is an intermediate production or distribution facility. However, the estimated sales are reported to indicate the level of economic activity at that facility.” In other words, at some establishments the sales figures used by Swenson are completely fictitious; the establishment is a division of a larger corporation and makes no sales. Walls & Associates, “National Establishment Time-Series (NETS) Database: 2008 Database Description.”

of profit in California taken from California corporate tax returns. Once again, Swenson makes no effort to show any of the steps in this calculation. It is also conceptually flawed. The fact that the predicted sales growth in each of the five states for all SSFF-affected firms combined is statistically significant does not mean that the predictions for individual industries are statistically significant, and Swenson provides no evidence that they are.

- Swenson then combines the separate industry profit totals into one for all industries in the state, asserting that California firms will realize an additional \$2.4 billion in profits annually as a result of SSFF-induced growth. He multiplies that figure by an alleged ratio of total California business taxes paid to California business profits of 19.45 percent to predict that firms would pay an additional \$467 million in taxes to California state and local governments annually as a result of the growth in their activity that enactment of the SSFF would stimulate. As the source of that 19.45 percent figure, he cites to an earlier paper he wrote. That citation is useless, since the 2005 paper contains no explanation of the data Swenson used to derive it.<sup>10</sup> Reading between the lines, however, it appears that he is dividing some estimate of the amount of taxes California businesses pay by an estimate of the total profits of California businesses taken from the Commerce Department's "State Gross Domestic Product Accounts," likely a line-item referred to as "Gross Operating Surplus." The most recent Gross Operating Surplus figure for California firms is for calendar year 2007 and is \$655 billion. Economists with the Ernst & Young accounting firm estimate that the total amount of state and local taxes paid by California businesses in calendar 2007 was approximately \$73 billion.<sup>11</sup> That results in an estimated California business tax burden as a share of California business profits of approximately 11 percent, *only slightly more than half as large as Swenson's 19.45 percent estimate.*

After estimating the taxes that businesses would allegedly pay on their additional, SSFF-stimulated profits, Swenson turns to estimating the additional California personal income and sales taxes that the 144,000 newly-employed workers would pay. Again, without showing any derivation of the number, he asserts that to be \$744 million annually, which works out to \$5,167 per employee. According to the Bureau of Labor Statistics, the average annual wage in California in 2009 was \$49,550.<sup>12</sup> The personal income tax due on a wage of that amount ranged between approximately \$800 (for a married couple) and \$2,000 (for a single person). The amount of sales tax paid per employed Californian is approximately \$1,400.<sup>13</sup> Thus, if one

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<sup>10</sup> In the 2005 paper, Swenson reported that total California business taxes as a share of California business profits were 19.34 percent in 2002. In an earlier paper, he reported that figure to be 5.3 percent for 1999. (Charles W. Swenson, "Does Your State Overtax Business Income?" *State Tax Notes*, October 22, 2001.) In a still earlier paper, he reported the overall California business tax burden to be 15.97 percent in 1993. (Charles W. Swenson, "Does Your State Overtax Business Income?" *State Tax Notes*, November 3, 1997.) None of the papers give any indication that the methodology had been changed; the published 1999 figure clearly was erroneous.

<sup>11</sup> Andrew Phillips, Robert Cline, and Thomas Neubig (Ernst & Young), "Total State and Local Business Taxes, 50-State Estimates for Fiscal Year 2007," Council on State Taxation (publisher), 2008. Since California has a July 1 to June 30 fiscal year, the calendar year 2007 estimate was derived by averaging the figure for California in the foregoing report with the comparable figure taken from the comparable FY 2008 COST report.

<sup>12</sup> Bureau of Labor Statistics' "Occupational Employment Statistics" series.

<sup>13</sup> In FY08, total state and local general sales tax collections in California were \$41.1 billion, according to the Census Bureau. According to the Ernst & Young accounting firm, \$19.7 billion of that amount — 48 percent — was paid by businesses, with 52 percent — \$21.4 billion — paid by individuals. With 15.15 million employed Californians in an

assumed that all of the new jobs he predicted were filled by single people, Swenson is exaggerating the amount of associated personal and income and sales taxes by approximately fifty percent. If one assumed that some would be filled by the single earner in a married couple or a single head of household with children — who pay lower personal income taxes — the exaggeration would be even greater. Even if one assumed that all the jobs would be filled by the second earner in a married couple in which the previously-employed spouse already was earning the average state wage, Swenson still would be exaggerating the additional revenue by at least 10 percent.<sup>14</sup> In other words, Swenson’s prediction of the amount of personal income and sales tax revenue that would be generated by new SSFF-stimulated jobs is — like his prediction for business taxes — substantially exaggerated.

- Finally, Swenson combines his predicted increase in California business taxes of \$467 million annually with his predicted increase in California personal income taxes of \$744 million annually, for a total of \$1.211 billion. He subtracts from it an estimated direct cost in forgone state corporate income tax revenue of \$800 million annually from the enactment of the elective SSFF to conclude that, on balance, enactment of the formula would result in a net revenue *gain* for the state of \$411 million annually. Quite apart from all the flawed methodologies and assumptions that led to this prediction, it is well outside the bounds of conventional economic analysis. No reputable mainstream economist would assert that a business tax cut provided through a mechanism that does not in any way *condition* the tax cut on the creation of additional jobs would generate so much additional economic activity that it would pay for itself — let alone generate additional revenue equal to 150 percent of the initial revenue loss as Swenson asserts. For example, a 2001 run of California’s official “dynamic” economic model concluded that only 18 percent of a \$1 billion across-the-board corporate income tax cut would be recouped through increased economic activity after five years.<sup>15</sup> (The model also predicted a potential long-term employment gain of approximately 12,000 jobs — less than one-tenth the number Swenson asserts will materialize within two years.) An analogous 2001 study using the “Oregon Tax Incidence Model” predicted that a \$100 million across-the-board cut in state corporate income taxes would recoup only 16 percent of the original revenue loss through increased economic growth.<sup>16</sup> In other words, Swenson’s prediction of a net California revenue gain from the enactment of the elective SSFF is not supported by any objective analyses.

In sum, at virtually every step along the way, Swenson’s derivations of his job and revenue predictions for California’s adoption of an elective SSFF are characterized by opacity, questionable methodological choices, and unsupported assumptions.

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average month in FY08, according to the Bureau of Labor Statistics, that means that the average employed Californian paid approximately \$1,400 in sales taxes in that year.

<sup>14</sup> The California personal income tax calculations that underlie this paragraph are available from author Mazerov on request.

<sup>15</sup> P. Berck, E. Golan, and B. Smith, “Dynamic Revenue Analysis for California,” California Department of Finance, summer 1996, [www.dof.ca.gov/HTML/FS\\_DATA/DYNA-REV/DYNREV.HTM](http://www.dof.ca.gov/HTML/FS_DATA/DYNA-REV/DYNREV.HTM).

<sup>16</sup> Oregon Legislative Revenue Office, “The Oregon Tax Incidence Model,” March 16, 2001, p. 71.

## Study Is Inconsistent in Its Treatment of Single Sales Factor Formulas that Are Phased In

As explained above, the basic methodology of the Swenson report relies on his assertion that the five states he studied all enacted the SSFF at various times during the same year, 2005. It compares the rate of acceleration or deceleration in job growth in the five states in the two-year interval leading up to 2005 — January 2003 to January 2005 — with the rate of acceleration/deceleration in job growth in the two years after 2005 — January 2006 to January 2008 — for two different groups of companies. The two groups are single-state corporations allegedly not affected by the enactment of the SSFF and multistate corporations allegedly affected. Calendar 2005 is treated as the year in which the SSFF went into effect, and job growth in that year is not counted.

In actuality, the SSFF was *not* enacted in 2005 in all five states, nor was it in effect in all five states as of January 2006. Indeed, Louisiana was the only one of the five states in which that was true. The SSFF was enacted in 2005 in both Georgia and New York, but it did not go into effect in either state immediately. Rather, it was phased in by increasing the “weight” that sales plays in the overall apportionment formula from its prior weight of 50 percent (property and payroll each representing equal shares of the remaining 50 percent) to the 100 percent weight that characterizes the SSFF. Indeed, in Georgia, the SSFF did not even fully go into effect until January 2008, the final month in Swenson’s analysis.

Now, it could be argued that the key date in assessing the potential impact of the SSFF on employment growth is the date on which it is enacted, not the date on which it actually goes fully into effect. From the enactment date forward, even if it is being phased in, a corporation deciding where to locate a new facility, or whether to move an existing facility into a SSFF state, can be reasonably certain that any potential tax savings from enactment of the formula will in fact be available for most of the expected economic life of the facility. Therefore, it is conceivable that the enactment of a SSFF could have an impact on the location of jobs before it is actually in effect.<sup>17</sup> Accordingly, it arguably was not unreasonable for Swenson to effectively treat New York and Georgia as SSFF states from the moment the enactment occurred.

The problem, though, is that if Swenson is going to treat New York and Georgia as SSFF states from the moment the formula began phasing in under this rationale, he has to do the same with Oregon and Wisconsin — the remaining two states in his study not yet discussed in this section. But Wisconsin enacted the SSFF in 2003, with the actual phase-in beginning in January 2006. Oregon enacted a law increasing the weight of its sales factor from 50 percent to 80 percent all the

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<sup>17</sup> It is highly unlikely, however, that the enactment of the SSFF would have a measurable positive impact on employment in the *first two years* after enactment in a state in which it is being phased in — in contradiction to Swenson’s conclusions. It often takes several years for a business to plan an entirely new facility or to relocate an existing operation. Even if one conceded that the enactment of a SSFF formula could provide such a large tax savings to a firm that it could drive such a location decision, the jobs would likely show up only after a significant lag.

The other mechanism by which the enactment of a SSFF could stimulate job creation in a state adopting it would be by encouraging a corporation to reallocate its production from an *existing* facility in a non-SSFF state to an *existing* facility producing the exact same item(s) located in the state switching to a SSFF. In theory, that could happen relatively quickly. In reality, it assumes both that there is excess production capacity in the facility in the SSFF state and that the corporation produces identical item(s) in multiple plants. Both conditions may be rare in the real world. In any case, however, there is *no* incentive to make such a production shift until the SSFF is actually legally in effect. Accordingly, it is highly unlikely that a *phased-in* SSFF is going to have any significant impact on employment in the first couple years of the phase-in.

way back in 2001, effective in May 2003. It then began phasing up from the 80 percent weight to a SSFF in legislation enacted in 2003 (the phase-in was subsequently accelerated.)

In sum, in two of the five states in Swenson’s analysis, by his own implicit logic, the phasing in of a single sales factor formula was already significantly affecting employment growth between January 2003 and January 2005 — supposedly the pre-SSFF “control” period that is a critical component of his entire methodology. Accordingly, these two states should not have been included in his analysis at the very least. Ideally, they would have been subjected to the same type of analysis, looking at earlier two-year periods prior to the enactment of SSFF. Swenson’s decision to include them in the study in a manner inconsistent with the treatment of other states is an additional indication that the study methodology was not well thought-through and that the results are unreliable.

### **Database Is Ill-Suited to the Methodology Chosen**

The adoption of the single sales factor formula affects the tax liabilities of specific corporate *taxpayers*. Thus, any analysis of the economic incentive effects allegedly created by the adoption of the formula that is based on an examination of individual corporations — as the Swenson report is — must use a data source that corresponds closely to the actual corporate tax filing units that are affected by the formula.

Moreover, the Swenson study compares the job-creation rates of multistate firms that are purportedly incentivized by the adoption of the SSFF to create in-state jobs to the job-creation rates of non-multistate firms that purportedly receive no such incentives. Accordingly, the underlying data source must also permit an accurate identification of multistate versus non-multistate firms.

In fact, the database upon which the Swenson study relied was deficient in both of these areas. It did not allow him to accurately identify actual tax-filing units, nor to accurately distinguish between single-state and multistate firms.

### **Cannot Accurately Replicate Tax Filing Units**

As its name suggests, the National Establishment Time Series (NETS) database that Swenson used is not a database of corporations or firms; it is a database of business *establishments*, that is, individual business *locations*. There is no necessary correspondence between a business establishment and a tax-filing unit. A single corporate tax return can aggregate the economic activity of numerous establishments, and these establishments can be located in the same state or in multiple states. Moreover, states differ in how they tax multi-establishment firms. If each establishment is separately incorporated, some states will treat each establishment as a separate taxpayer. Other states, those that mandate “combined reporting,” effectively treat even separately-incorporated establishments as a single tax filing unit if one owns the other(s) or if they have a common parent.<sup>18</sup>

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<sup>18</sup> To be included in a combined report, the related corporations must exhibit significant economic interdependence in addition to common ownership.

The “standard” or “off-the-shelf” version of the NETS database Swenson used does not contain sufficient data to accurately mirror tax-filing units in many instances. That is true for both non-combined reporting and combined reporting states.

In states that do not require combined reporting, the tax filing unit is an individual corporation, and an individual corporation can have multiple establishments. When the corporation of interest is a subsidiary of another corporation, there is no data element in the standard version of the NETS database that allows a user to link that corporation to the establishments it owns when the latter are (non-separately-incorporated) branches.<sup>19</sup> In other words, in non-combined reporting states (which included Georgia, Louisiana, and Wisconsin in the time periods in the study), the version of the NETS data to which Swenson had access often cannot replicate the actual tax filing unit in a multi-establishment firm.

The standard version of NETS *does* include an identifier for the ultimate parent corporation. With respect to combined reporting states (a category that included New York for part of the time period in Swenson’s study and Oregon for the entire period), it therefore *would* be possible to treat all individual establishments owned by a common parent, including the parent itself, as perhaps a reasonable approximation of the tax-filing unit — the “unitary group.”<sup>20</sup> There is only one problem: the unitary group includes all commonly-owned corporations, *including those located out of state*. Because of the cost of obtaining the full national NETS database, Swenson only had access to information for the establishments that were actually located within the five states he examined. Without that information, he cannot approximate the actual tax filing units affected by the SSFF in combined reporting states, either.

By treating every individual establishment as a separate firm rather than grouping them into entities reasonably approximating actual tax filing units and treating the latter as the “firms” of interest, Swenson may well be drawing conclusions about the efficacy of the SSFF completely at odds with the facts. It would be quite possible, for example, for an establishment whose growth accelerated between 2003-2005 and 2006-2008 in one of Swenson’s five states to be part of a multistate firm (comprising a single taxable entity) whose growth in non-SSFF states accelerated even *more* rapidly. Such a scenario would hardly be evidence that SSFF was an effective job creation incentive for that firm, yet Swenson’s methodology would treat it as such because it would only be

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<sup>19</sup> The underlying data in the NETS database are generated by the Dun & Bradstreet business credit information firm. These data are coded to permit an accurate tracing of the ownership chain of a corporation with a multi-tiered legal structure, that is, a parent corporation that owns a subsidiary, which in turn owns other subsidiaries and/or non-separately-incorporated establishments (“branches,” in D&B terminology). But the standard version of the NETS database generally only includes the code for the ultimate parent corporation. Thus, in an example from actual sample data provided by the firm that has created NETS, US Airways owns Piedmont Airlines, which is a separately-incorporated subsidiary; Piedmont in turn has 14 establishments. In a non-combined reporting state, those 14 establishments and the Piedmont headquarters would constitute a tax filing unit if the establishments were divisions of Piedmont rather than being separately incorporated themselves. Author Mazerov asked whether the “parent” code for the 14 establishments shown in their individual records in the standard version of NETS would be that of Piedmont or that of US Airways, and was told that it would mostly likely be that of US Airways. (Author conversation with Don Walls, Walls & Associates, September 15, 2010.) Thus, there would be no way to identify the 14 establishments as members of the same tax filing unit with the Piedmont headquarters.

<sup>20</sup> This would require assuming that the economic interdependence criterion discussed in Note 18 had been satisfied, a questionable assumption in many cases. That is, there are many corporations that are not in the same tax filing unit as their parent corporation even in combined reporting states.

interested in what occurred in the SSFF state and not what the firm's job creation rate was in other states in which it was taxable. Likewise, a multistate firm might close a plant in one single sales factor state not among Swenson's five and consolidate that production at a plant in one of the five states. In the real world, that would not be evidence of the efficacy of the SSFF, because the firm received the same tax benefit in both locations. But it would be counted as evidence in Swenson's study, because it only measures job growth in establishments located in one of the five states.

### **Cannot Accurately Distinguish Single-State from Multistate Firms**

The same problems carry over into identifying multistate versus single-state firms — again, the central component of Swenson's methodology. One obvious definition of a multistate firm is a firm owning establishments and/or subsidiary corporations in multiple states. However, as just explained, Swenson didn't have access to information on commonly-owned establishments or subsidiaries in states other than the five he studied. Another way to define a multistate corporation is as a corporation that has *sales* in multiple states. The NETS database *does* report estimated sales information for each establishment, but it *does not* contain any information about where those sales occurred.

### **Proxies for Classifying “Firms” Do Not Work**

Swenson attempts to deal with some of these constraints by using five NETS data elements that he asserts are reasonable proxies for a multistate firm incentivized by the SSFF to create jobs. The study asserts that an establishment will be classified as a firm not affected by the SSFF if it meets any one of the five criteria.<sup>21</sup> However, Swenson's choices are highly debatable and demonstrably result in the mis-classification of many “firms”:

- Swenson classifies a corporation as not being affected by the SSFF if it consists of a single location. But it is quite possible for a single-location corporation to have customers in nearby states and to send employees into those states to service those customers. In that event, it is a multistate corporation and its tax liability will be affected by its home state's adoption of a single sales factor formula.
- Swenson classifies a corporation as not being affected by the SSFF if its stock is not publicly traded. But there are many large multistate corporations in these five states that are privately held.<sup>22</sup> Such corporations are potentially affected by the adoption of the SSFF but are classified as not being affected.

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<sup>21</sup> In fact, the actual regression “runs” do not correspond with this definition. Swenson states: “I define SSFA [Single Sales Factor-Affected] firms as those which meet ALL of the following criteria: public[ly traded], corporate, have subsidiaries, belong to SIC<5200 [i.e., are not in retailing or service industries], and are not single locations.” This would imply that he would create an explanatory variable in his regressions that would equal “one” if all five criteria were satisfied and “zero” otherwise. Instead, however, he included all five criteria separately in the regression, which means that no particular establishment was definitively classified as a SSFF-affected firm or a SSFF-non-affected firm. The two different approaches are not equivalent, and Swenson did not explain why the text and actual statistical methodology do not align.

<sup>22</sup> Each year, *Forbes* magazine publishes a list of the largest privately-held companies in the United States. The list includes many companies with a presence in numerous states, for example, hospital, hotel, and grocery chains. For the 2009 list, see: Andrea D. Murphy and John J. Ray, “America's Largest Private Companies,” *Forbes*, October 28, 2009; [www.forbes.com/2009/10/28/largest-private-companies-business-private-companies-09\\_land.html](http://www.forbes.com/2009/10/28/largest-private-companies-business-private-companies-09_land.html). Some of these

- Swenson classifies a corporation as being more likely to be a multistate corporation affected by the SSFF the more subsidiary corporations it owns. But this is irrelevant in three of the five states in the study. In Georgia, Louisiana, and Wisconsin a parent corporation's tax calculation is not affected at all by that of its subsidiaries if they are legally-separate corporations.
- Finally, Swenson automatically classifies an establishment as not potentially affected by adoption of the SSFF if its industry code classifies it as “retail, services, nonprofits, etc., which tend to serve local markets.”<sup>23</sup> Swenson also states that “There are a number of large, multi-state retailers which operate within these states which would be affected by SSF, if not for the fact that the SSF does not apply to these industries in most of the five states examined.” The latter is an incorrect statement; all five states in Swenson's study apply the SSFF to retailers. Swenson's classification of service businesses as not being potentially affected by the SSFF is also arbitrary and unjustified. There are *numerous* types of service businesses that are multistate corporations, including virtually all large banks, for-profit hospital and nursing home chains, hotel chains, nationwide temporary employment agencies, advertising agencies, movie theatre chains, health club chains, and many others.

In sum, the version of the NETS database upon which the Swenson report relies does not in many circumstances permit the grouping of individual business establishments into the actual corporate income tax filing units whose liability is potentially affected by the adoption of the SSFF. Nor does it permit an accurate classification of a particular establishment as being part of — or not part of — a multistate corporation. These limitations were substantially compounded by Swenson's inability to afford the entire 50-state NETS database. The proxy measures Swenson used instead are inadequate and likely result in the misclassification of many establishments as not potentially being affected by the SSFF when they are, in fact, potentially affected — and vice versa.

### **Actual State Data Demonstrate That Sorting Methodology Is Flawed**

Four of the five states that Swenson examined actually did studies of their own of the number of multistate corporations that would be affected by the adoption of the single sales factor apportionment formula — that is, whose tax liability would go up or down. The results of these studies diverge significantly from the results of Swenson's sorting exercise. These state-specific studies are also far more reliable, because they take actual filed corporate tax returns and recalculate the corporations' tax liability altering only the apportionment formula from prior law to the SSFF. The differences between the findings of these studies and Swenson's calculations are dramatic:

- Swenson concludes that there were 3,109 firms in Georgia that were affected by the state's adoption of SSFF, but the state's own study found that the actual number was 12,426, almost

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companies may be organized as partnerships or limited liability companies, but the majority undoubtedly are incorporated.

<sup>23</sup> It is unfathomable that Swenson apparently did not exclude either non-profit organizations or governmental establishments from his study sample. Rather, he effectively classified them as for-profit businesses not potentially affected by SSFF — which biases the results in unknown ways.

four times as high.<sup>24</sup> Swenson’s methodology effectively would have inaccurately classified the more than 9,000 additional affected firms as not being affected.

- Swenson classified 4,860 New York firms as affected by the SSFF, but a 2001 study estimated that 15,218 corporations would be affected — more than three times as many.<sup>25</sup>
- Swenson classified 1,474 Oregon corporations as affected by the adoption of SSFF, but a 2204 study by the state placed the number at 4,161, almost three times as many.<sup>26</sup>
- Swenson classified 1,796 Wisconsin firms as affected by the SSFF, but a state study put the number at 6,404, more than three times as many.<sup>27</sup>

In short, data from the states themselves indicate that the central element of Swenson’s study — using “proxy” characteristics of firms to sort them into SSFF-affected and SSFF-non-affected firms — simply did not achieve accurate results. The actual numbers of SSFF-affected firms identified by the states were anywhere from 180 percent to 300 percent larger than Swenson’s estimates.

### **Swenson’s Own Results Confirm that Methodology Is Flawed**

Swenson’s own regression results also provide evidence that his approach to classifying establishments as either potentially affected by the enactment of a SSFF or not affected was, in fact, flawed. Table 2 sets forth the signs of the five classification criteria used in the regressions for each of the five states. By his own expressed logic, there should be “+” signs in the first four rows for all five states – indicating that being an establishment that was a corporation, that was owned by a *publicly-traded* corporation, that owned a large number of subsidiaries, and that was in a manufacturing, extractive, or similar industry and not in a retailing or service industry was positively correlated with accelerating job growth between the two time intervals in the study. Conversely, the sign of the “single location” criterion in the last row should have been *negative* for all five states; that is, status as a single-location firm allegedly not-affected by SSFF should have been negatively correlated with an acceleration in employment growth between the two periods.

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<sup>24</sup> Laura Wheeler, “Effect of Change in Apportionment Formula on Georgia Corporate Tax Liability,” Georgia State University Fiscal Research Center Policy Brief Number 206, December 2009; [aysps.gsu.edu/frc/files/Brief\\_206.pdf](http://aysps.gsu.edu/frc/files/Brief_206.pdf). The number of affected firms was calculated by multiplying the shares of firms with a change in their apportionment factor *and* a change in tax liability shown in Table 6 by the number of firms with a change in their apportionment factor in Table 2.

<sup>25</sup> Table dated April 11, 2001 faxed to author Mazerov from Robert Megna, New York Division of the Budget, same date.

<sup>26</sup> Michael Kennedy, Oregon Office of Economic Analysis, “Corporate Income Tax Apportionment in Oregon,” Presentation to the Federation of Tax Administrators Revenue Estimating and Tax Research Conference, Fall 2004; [www.taxadmin.org/fta/meet/re\\_pres04/kennedy.pdf](http://www.taxadmin.org/fta/meet/re_pres04/kennedy.pdf).

<sup>27</sup> Pamela Walgren, Division of Research and Policy, Wisconsin Department of Revenue, “Single Sales Factor Apportionment Distributional Data,” presentation to the Federation of Tax Administrators Revenue Estimating and Tax Research Conference, September 25, 2001.

**Table 2: Signs on Coefficients of SSFF-Affected and SSFF –Non-Affected Classification Measures in Swenson Study, from Tables 11 – 15 (variable being predicted is change in rate of employment growth; results contrary to prediction are shaded)**

	Georgia	Louisiana	New York	Oregon	Wisconsin
Corporation	-	-	+	-	-
Publicly-traded	+	+	-	+	+
No. of subsidiaries	+	+	-	+	+
SIC Code < 5200 (excluding retail and service businesses)	+	+	+	+	-
Single location firm	-	-	+	-	-

As Table 2 reveals, however, Swenson’s results were not always consistent with his hypothesis. In every state, in fact, there is at least one variable whose sign is not consistent with Swenson’s theory, and for every variable there is at least one state whose sign is inconsistent. Overall, of the 25 measurements of correlation between Swenson’s theory and his results, almost one-third (that is, eight of the 25) go in the opposite direction from his prediction. This indicates that Swenson’s classification scheme for SSFF-affected versus SSFF-non-affected “firms” is very weak. In the body of the report Swenson completely glosses over the fact that some of the signs on the variables are inconsistent with his theory. For example, with respect to the New York results, he writes: “Coefficients for the variables support the prediction that SSFA [Single Sales Factor-Affected] firms enjoy increased. . . employment after SSF designation.” *But three of the five variables in New York had the wrong signs.* The pattern shown in Table 2 should have led Swenson to reconsider the validity of his model. But it did not.

### **Methodology Fundamentally Flawed By Lack of Proper “Control” Group of States**

Finally — and quite apart from all the shortcomings discussed up to this point — the *fundamental* methodology of the report is seriously flawed. It cannot prove its assertion that enactment of the SSFF incentivizes job creation because it does not examine changes in the rate of job creation in a “control group” of states that did not have the SSFF in effect during the time period it examined.

Even if the methodology a) had accurately replicated tax filing units; b) properly sorted them into two groups of firms — one supposedly incentivized to create in-state jobs by the enactment of the SSFF and the other one not so-incentivized; and c) shown that the rate of job growth in the incentivized firms in the two-year period following the 1985 adoption of the SSFF accelerated more than it did for the non-incentivized firms (or decelerated less), Swenson still would *not* have proved his assertion that the adoption of the SSFF is an effective job-creation incentive. The reason is that the study contains no “control group” of states that did *not* implement the SSFF at that time. It is entirely possible that the replication of Swenson’s methodology in such a control group would have found comparable acceleration of job growth between the two periods in the firms sorted into the SSFF-incentivized group. If it did, that would suggest that the enactment of the SSFF in Swenson’s five studied states was not responsible for the speed-up in the rate of job growth.

Tables 6-10 of Swenson’s report make clear that one fundamental difference between the groups of multistate, allegedly SSFF-incentivized firms and in-state, allegedly non-incentivized firms, is that the multistate firms are considerably larger. For all five states in all years, the average number of

employees in the SSFF-incentivized firms is in the hundreds, while for the non-incentivized firms it is — at most — in the teens. Leaving aside all the other potential problems with the data and the sorting methods, perhaps all that Swenson has demonstrated is that the rate of job growth throughout the United States speeded up for large firms everywhere more than it did for small firms in the 2006-2008 period as compared to the 2003-2005 period. Indeed, a very cursory glance at nationwide data suggest that this could be the case, and if this was happening nationwide, it probably has nothing to do with tax changes in just five states.<sup>28</sup> In sum, even if Swenson had been able to carry out his study flawlessly on its own terms, its failure to include the appropriate control group would have substantially undermined his conclusion that SSFF incentivizes job creation.<sup>29</sup>

## Errors

The Swenson report contains several inaccurate factual assertions and numerous errors in summarizing the data upon which it relies. The errors include:

- Tables 6-10 of the report present summary data on the sales and employment levels for each of the five states over the five-year interval examined; data are presented separately for the SSFF-affected firms and for all firms in the state. Numerous cells in the tables (located in the columns presenting maximum values) are identical, even though the firms comprising each state's economic base are of course completely different. It appears that some data were accidentally cut and pasted from one state table to others.
- The 2006 sales of the largest multistate firm in Georgia are reported in Table 6 as "\$8,252,050,8000." The 2006 sales of the largest firm in Louisiana are presented as \$82,520,508,000; the 2006 sales of the largest *multistate* firm in Louisiana are presented as \$8,252,050,800. It seems likely that only one of these numbers is correct.
- In all five tables, Swenson reports the change in employment and sales for all firms in the state between 2006 and 2008 but inaccurately labels the change as that for the 2005-2008 interval in the case of employment and for 2007-2008 in the case of sales. (The description of the methodology in the body of the report seems to suggest that the interval examined was indeed 2006 to 2008, and the values reported generally appear to correspond to that interval as well.)
- In Table 6 for Georgia, Swenson reports the number of business locations throughout the United States owned by the largest firm in the state and the largest SSFF-affected firm in the state. The corresponding values have been omitted for the other four states in the study

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<sup>28</sup> A calculation of U.S. Census data tabulated by the Small Business Administration show that job growth slowed down at establishments with fewer than 500 employees in the 2006-07 period, relative to 2003-05, while it accelerated at establishments with more than 500 employees. Specifically, smaller firms' average annual rate of job growth fell from an annual rate of growth of 1.0 percent to -0.6 percent, while larger firms' rate of growth rose from 1.5 percent to 1.7 percent. U.S. Small Business Administration, "Private Firms, Establishments, Employment, Annual Payroll and Receipts by Firm Size, 1988-2007," downloaded from <http://www.sba.gov/advo/research/data.html>.

<sup>29</sup> Swenson notes at several points in his paper that financial constraints prevented him from obtaining more than five state's worth of NETS data. Other data and methodological issues aside, his study would have been more reliable had he chosen three of the five states that were in his study at random and compared their job growth acceleration with that of two states that did not enact SSFF during this period, also chosen at random.

without explanation.

- Numerous simple percentage changes in values are miscalculated throughout Tables 6-10. For example, the percentage changes in employment of multistate firms in the 2003-5 and 2006-8 intervals in Table 6 are reported as -.5 percent and +1.2 percent, when the data in the table indicate they should have been -.65 percent and +.83 percent, respectively.
- In Table 6 for Georgia, in three cells that are labeled as reporting percentage values, decimal values (prior to conversion to percentages) are presented.
- The body of the report says that there were 3,109 observations (examples) of multistate firms in the regression for Georgia, but Table 6 says there were 2,410. The body of the report says that sales declined .7 percent for the SSFF-affected firms in Louisiana in the two years prior to the adoption of SSFF, but in the table it is labeled as an increase.
- There is a key to the tables presenting the results of the regressions (Tables 11-15) indicating the degree of statistical significance of the coefficients of the variables — a standard practice in such research. But none of the coefficients is labeled in accordance with the key in any of the tables, leading the reader to wonder whether this was simply an error or an indication that none of the values was, in fact, statistically significant. For four of the five regressions for the sales variable the overall statistical significance of the result is provided, but it is omitted for the fifth regression. Again, the reader can't determine if this was an error or an indication that the overall regression result was in fact not statistically significant.
- The report describes the data-set as “an annual time-series of information on over 36.5 million U.S. establishments from January 1990 to January 2008” that “reflects the economic activity of the previous years (1989-2008).” But if January 2008 was the last month in the data-set, it reflects the activity of 1990-2007.
- Swenson states that “2006 is omitted from the analysis since it assumes a phase-in of SSF effects which may take up to a year.” But if he is calculating the rate of job growth between January 2003 and January 2005 and comparing that to the rate of job growth between January 2006 and January 2008, he is effectively omitting job growth occurring in 2005 from the analysis, not 2006.
- The report categorizes multistate retail chains as not being affected by the SSFF due to the fact that “SSF does not apply to these industries in most of the five states examined.” In fact, the SSFF applies to retailers in *all five* states. Louisiana is the only one of the five states in which the SSFF does not apply to all corporations, but even in Louisiana it still applies to retailers.<sup>30</sup>

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<sup>30</sup> The Louisiana code provides that “For taxable periods beginning on or after January 1, 2006. . . the Louisiana apportionment percent of any taxpayer whose net apportionable income is derived primarily from the business of manufacturing or merchandising shall be computed by means of a single ratio consisting of the [sales factor].” It further provides that “The term ‘business of manufacturing or merchandising’ shall only include a taxpayer whose net apportionable income is derived primarily from the manufacture, production, *or sale of tangible personal property.*” [Emphasis added.] Author Mazerov confirmed orally with Louisiana Department of Revenue staff on September 28, 2010, that the SSFF applies to multistate retailers.

- Swenson asserts that “no major, across-industry tax law or rate changes occurred in these states during the time period examined.” In fact, New York enacted a major structural change in its corporate income tax known as “combined reporting” in April 2007; the change affected virtually all industries in the state.

Most of these errors may indicate nothing more than insufficient fact-checking and proofreading. Nonetheless, the lack of attention to detail should, by itself, raise serious questions about the accuracy of the overall conclusions of the report, since — as discussed above at length— the report provides no documentation whatsoever of how the regression results were translated into the calculation of job creation estimates for any of the five states or for California.

### **General Lack of Transparency in Presenting Results**

Whether or not to allow the SSFF to go into effect next year is a critical public policy choice for California and its citizens. The state is facing a budget gap of approximately \$18 billion in the current fiscal year; implementation of the SSFF contributes \$160 million to that gap and will cost the state approximately \$800 million annually by FY16.<sup>31</sup> Both the Governor’s Department of Finance and the Legislative Analyst’s Office project significant fiscal gaps for the foreseeable future. How those gaps are closed will have profound consequences for the well-being of millions of the state’s residents.

Given the seriousness of the state’s fiscal problems, a responsible analyst choosing to inject a complex statistical study into the debate would go out of his or her way to explain the report and its findings in language that elected officials and members of the news media can grasp and in sufficient detail for other experts in econometrics to evaluate. The Swenson study completely fails in that regard; its author is essentially demanding that his academic credentials substitute for even minimal transparency in presenting his research. For example:

- The report makes no attempt whatsoever to interpret the results of the regression analyses for policymakers, for example, to explain that “the value of X for the regression coefficient for a particular variable in the equation implies, in common language, Y.”
- The description of the fundamental methodology underlying the report is extremely cursory and leaves unanswered many questions that are obvious to a more knowledgeable reader. For example, did the author remove governmental and non-profit organizations from the data before running the regressions? What did he do with firms that came into or went out of existence during the time period under examination? Why do his regression analyses in Tables 11-15 use only on the order of one-quarter to one-third of the data points that Tables 6-10 list as being available?<sup>32</sup>
- In deriving and presenting the key equation used in the regression analysis, Professor Swenson

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<sup>31</sup> Franchise Tax Board, “Impact of 2009 Budget Legislation,” December 4, 2009.

<sup>32</sup> Author Mazerov sent two e-mail messages and left two voice-mail messages with Professor Swenson indicating that he wished to ask several questions concerning the methodology. Professor Swenson did not respond.

does not even bother to identify several of the variables.

- As noted above, Swenson does not report key indicators of statistical significance of any of his variables – a standard feature of an analysis of this type.<sup>33</sup> These variables — firm characteristics — are crucial to his analysis; he hypothesizes that they indicate whether SSFF is likely to be beneficial to a firm and induce it to expand.
- Given that the central issue in the methodology is determining whether the rate of sales and employment growth during the two periods is different for firms affected by the SSFF and those non-affected, it is curious that in presenting summary statistics in Tables 6-10, the author chose not to present the statistics for the two groups of firms. Instead, he presented one set of data for SSFF-affected firms and another set of data for SSFF-affected firms and non-SSFF-affected firms combined. The latter set of data is presented in such a way that a reader cannot easily derive the data for the non-SSFF-affected firms nor be certain of the accuracy of any attempt to do so.

## **Conclusion**

From the tiniest details of basic proofreading and fact-checking to its fundamental methodological approach, the Swenson report is substantially flawed. Until these issues are addressed and its anomalous data and results are explained in terms that policymakers can reasonably be expected to understand, it simply should be given no credence in evaluating the potential impact on California employment and revenues of the repeal of the single sales factor formula by Proposition 24.

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<sup>33</sup> Under each of the tables presenting his statistical results, Professor Swenson indicates that data enabling the reader to compute measures of statistical significance are available upon request: “For standard errors under OLS, robust estimation methods, and weighted least squares, contact the author.” Author Mazerov indicated in each of the four messages he sent to Professor Swenson that he wished to receive these data. Professor Swenson did not provide them.