Power Indices for Revealed Preference Tests*

James Andreoni
Department of Economics
University of California, San Diego

William T. Harbaugh
Department of Economics
University of Oregon

March 2006
Revised February 21, 2008

Abstract

Revealed preference tests are elegant nonparametric tools that ask whether individual or aggregate data conform to economic models of optimizing behavior. In designing a test using revealed preference, however, one faces a vexing tension between goodness-of-fit and power. If the test finds violations, then one must ask if the test was too demanding—is there an acceptable tolerance for goodness-of-fit? On the other hand, if no violations are found, one must demonstrate that the test was demanding enough—is the test sufficiently powerful? This paper provides a counter-weight to the many papers on goodness-of-fit by discussing indices of the power of revealed preference tests. Where possible, we attempt to unify the two approaches. We present four new indices and discuss their relative merits.

*We are grateful to Oleg Balashov, David Bjerk, Joseph Guse, and Grigory Kosenok for excellent research assistance, and to Ian Crawford, Melissa Famulari, Shachar Kariv, Justin McCrary, Gautam Tripathi, and Hal Varian for helpful comments. We also acknowledge the financial support of the National Science Foundation.

1 Introduction

One of the most elegant tools for testing theories of optimizing behavior is revealed preference. Given a vector of prices p_t and choices x_t at time t , we know that the bundle x_t is preferred to another bundle x if x was affordable when x_t was chosen, $p_t x_t \geq p_t x$. Relying on transitivity of preferences, one can string together chains of these inequalities to rank bundles, even those that were never directly compared by the consumer, and bound possible indifference curves that could have generated this data. Of course, if these chains of inequalities cannot all be mutually satisfied, then the choice data fail to conform with a model of utility maximization. Hence, revealed preference is both a descriptive and a diagnostic tool.[1]

The clarity and elegance of revealed preference was presented in a remarkable series of papers by Hal Varian (1982, 1983, 1984, 1985), which built on earlier work by Afriat (1967, 1972), Houthakker (1950) and, of course, Samuelson (1938). The application of these ideas has varied widely, including analysis of aggregate consumption data, individual data in repeated cross sections, and controlled laboratory experiments.

As a diagnostic tool, revealed preference tests ask whether all of the data satisfy the inequalities of revealed preference. To present these formally, we begin with a few definitions:

Definition: Directly Revealed Preferred: x_t is directly revealed preferred to x if $p_t x_t \geq p_t x$, and is *strictly* directly revealed preferred if $p_t x_t > p_t x$.

Definition: Revealed Preferred: x_t is revealed preferred to x if there is a chain of directly revealed preferred bundles linking x_t to x.

The revealed preference relation is the transitive closure of direct revealed preference. The building blocks of a revealed preference test are then the strong and weak axioms:

Definition: Weak Axiom of Revealed Preference (WARP): If x_t is directly re-

[1] Note that the same notions can be applied to optimizing by firms, as Varian (1984) demonstrates. For brevity, we will confine our discussion to consumer theory, but it all can be applied to producer theory as well.

vealed preferred to x, then x is not directly revealed preferred to x_t.

Definition: Strong Axiom of Revealed Preference (SARP): If x_t is revealed preferred to x, then x is not revealed preferred to x_t.

The most general and powerful notion of a revealed preference test is Varian's (1982) Generalized Axiom.

Definition: Generalized Axiom of Revealed Preference (GARP): If x_t is revealed preferred to x, then x is not strictly directly revealed preferred to x_t.

If the data are consistent with GARP, then there exists a utility function that could have generated the data. That is, the data conform with a theory of optimizing behavior. A failure to satisfy GARP, on the other hand, rejects the optimizing model.

There are two obvious issues with applying revealed preference tests to data. First is that the test is extremely sharp— a single violation of GARP results in a rejection of the model. One can naturally ask whether there is some tolerance that should be applied to the data to account for errors in either measurement or choice that can allow some "minor" violations to be accepted within the theory. This is the notion of goodness of fit of the model.

The other issue arises when the data fail to reject GARP. In particular, if the optimizing model is not in fact the correct model, would the revealed preference test applied be sensitive enough to detect it? This is a question of the power of the revealed preference test.

There have been several important attempts in the literature to formalize approaches to goodness-of-fit, most notably Varian (1990, 1991). By contrast, there have been few formal attempts to develop measures of the power of revealed preference tests. This paper is about developing indices of power.

The next section will review some of the ways revealed preference tests have been applied in the economics literature. Section 3 will describe existing notions of power indices. Sections 4 to 7 will present four new indices of power: Afriat Power Index, Optimal Placement

Index, Jittering, and Bootstrapping. Section 8 will apply these to experimental data, while section 9 will see how well the various indices correlate with each other. Section 10 is a conclusion.

2 Background

There is a venerable literature using revealed preference axioms to build new and better price indices. Manser and McDonald (1988) examined 27 years of aggregate consumption data. They note that if one can assume preferences are homothetic, then one can improve the power of GARP tests and narrow the bias in constructing exact price indices. The reason is that, under homotheticity, expansion paths are always rays through the origin. Hence, one can construct new budgets as paralell shifts of old budgets, project choices onto them, then use this "expanded" data to more closely measure the indifference curve through a reference budget. Using 101 commodities, Manser and McDonald found consistency with GARP and with homothetic preferences.

Famulari (1995) applied GARP analysis to a series of cross-sections of the Consumer Expenditure Survey (CEX) from 1982–1985. Famulari was interested in testing the common preferences assumption. She used both the time and regional variation in price to generate shifts in budgets. To increase the power of the test, she compared "households" of similar income creating 43 "groupings"of representative households based on income and other demographic characteristics. Of the 43 groupings, she found 42 of them satisfied GARP.

Blundell, Browning and Crawford (2003) note that GARP tests may actually be quite weak when applied to annual data. As incomes expand over time and relative prices are somewhat stable, there are few of the intersections across budgets that one needs to test the theory. They state, "There is also a concern that revealed preference tests are inherently lacking in power (as compared with parametric tests) and will fail to reject 'too often.' " Because revealed preference tests, and in particular GARP, put the mildest restrictions on behavior they can be seen as so flexible as to allow too many observations to pass the test.

Blundell et al. suggest one possible avenue is to combine parametric and non-parametric techniques and "consider flexible parametric models over regions where the nonparametric tests do not fail." However, they warn, "one of our concerns about currently used parametric models is that they may be too inflexible."

They applied their ideas to a series of cross sections (1974–1993) of the British Family Expenditure Survey, which is similar the CEX. They formed consumption into 22 composite goods and used semi-parametric kernel estimation to calculate expansion paths. Constructing the optimally powerful test, and using the expansion paths to project choices onto that test, they showed that GARP is not violated for long intervals of the survey. They went on to use this result to present far tighter bounds on cost-of-living indices than have been previously provided. Even by sharpening GARP, they still found the data largely fail to reject the optimizing model.

A parallel literature has developed around controlled laboratory experiments. Controlled experiments present both an opportunity and a challenge. The opportunity comes in being able to precisely control and measure income, prices and choices. The challenge is to control the situation enough that the experiment does not create artificially rational or spuriously "irrational" behavior.[2]

An important early study is by Battalio et al. (1973). The subjects were 38 female patients at the Central Islip State Hospital, a psychiatric hospital. This hospital had a functioning "token economy " where the patients earned tokens that could be traded for goods at a hospital store. The market had been functioning for several years when Battalio, with cooperation of the hospital, experimented with weekly changes in prices. He aggregated the commodities into 3 goods and measured weekly consumption over 7 weeks, periodically changing prices up or down. He found that half the subjects had revealed

[2]Suppose, for instance, that the goods purchased in a lab are storable and that there is a secondary market. Hoarding the goods that can be resold at the greatest profit could be an optimal strategy. Relying solely on profit maximization could generate artifically few violations of GARP. On the other hand, we could observe violations of GARP resulting in failures of profit maximization, or mixtures of profit and utility maximization. A more clean experiment provides goods that cannot be hoarded or resold afterward.

preference violations, though most of these could potentially be explained by data entry errors. Cox (1997) re-analyzed this data, explicitly including leisure as a good. Of 38 subjects, he found 24 had no violations, 8 had only 1 or 2 violations, and 37 subjects passed the 0.90 tolerance for Afriat Efficiency, which we discuss in detail below.

Sippel (1997) provided a much more challenging test with 10 budget sets over 8 commodities, all of which had to be consumed over the course of the experiment. Over two similar experiments involving 42 subjects, he founds 24 of them (57%) had violations of GARP. However, over half of these violators had only one violation, all but 4 had Afriat Efficiency above 0.95, and only 2 had Afriat Efficiency below 0.90. Nonetheless, Sippel argues that his study weakens confidence in the neoclassical model of choice.

Two other studies allowed subjects to buy storable goods. Mattei (2000) used 20 budgets with 8 goods (mostly school supplies), and conducted three different experiments. The subjects were either 20 undergraduates, 100 graduate students, or 320 readers of a consumer affairs magazine. Mattei found from 25% to 44% of subjects had violations of GARP. Applying Afriat Efficiency of 0.95, the number fell to fewer than 4% in all studies. Fevrier and Visser (2004) used five budgets of 6 goods, which were all different varieties of orange juice. People first tasted the juices, rated their quality, and then were given an option to buy some of the juice as a reward for being in the experiment. They were given 5 different price options, where the prices adjusted in response to the quality ratings. They found that 30% of subjects were inconsistent with GARP, and 15% had Afriat Efficiency below 0.95.

A study by Harbaugh, Krause and Berry (2001) tested the rationality of children by offering them 11 budgets of chips and juice boxes. They found second-graders to be less rational than sixth-graders or college students, but that six-graders and college students were equally rational and had few violations of GARP.

Andreoni and Miller (2002) asked whether a rational model of altruistic behavior can explain subjects' generosity in a Dictator game. By endowing subjects with tokens that were redeemable for different values by two subjects, they generated different budgets of own-

and other-payoff. Using 8 budgets (or 11 in one condition), they tested whether a model of convex altruistic preferences can predict the data and found that over 90% of subjects were consistent with GARP. We discuss this data more in sections 8 and 9.

These studies illustrate the delicate tension between power and goodness-of-fit. If we design a test with many prices and many commodities, we present an immensely complicated task that many humans are sure to fail, perhaps because of the failure of economic theory or because of confusion or fatigue brought on by the task itself. On the other hand, if the test fails to find any violations of GARP we are left with lingering doubts that we designed a good test that could have uncovered the model's weakness.

To give us confidence that a successful test should be believed, we need informative measures of power. The definition of the power of a test is the probability of rejecting the null hypothesis when it is false. To state the power clearly, therefore, requires that one specify an alternative hypothesis. This is where indices of power can rise or fall. What is an informative alternative?

The next section will review two power indices that have been used in the literature. We then begin presenting new indices in the following sections.

3 Prior Power Indices

Next we describe the power indices of Bronars (1987) and Famulari (1995).

3.1 Bronars' Power Index

Stephen Bronars (1987) developed the first and most lasting index for the power of revealed preference tests. He specified an alternative hypothesis based on Becker's (1962) notion that individual choices are made at random. That is, individual choices are probabilistic and are uniformly distributed on the budget set. With this alternative, one can calculate the probability that a random set of choices will violate GARP. Perhaps more sensibly, one can conduct a series of Monte Carlo experiments on the budgets under the alternative hypothesis

and calculate the probabilities of GARP violations. Then the power of a particular GARP test is the chance that random choices will violate GARP. Bronars call this Method 1.[3]

Bronars also considered two modifications of Method 1. His Method 2 first derives random budget shares in which the expected share is $1/n$, where n is the number of goods. Method 3 finds random budget shares in which the randomness is centered on actual budget shares. Method 1, however, has come to dominate the literature.

An advantage of Bronars' approach is that it is both natural and simple. A disadvantage is that the alternative hypothesis is perhaps too naive. Suppose, for instance, the budgets offered did not intersect near the points where individuals are actually choosing. Then if preferences do not conform to utility maximization, the test would be unlikely to discover it. This is true even if Bronars' analysis shows that randomly made choices provide a high likelihood of violations. It would seem preferable to take account of the choices actually made when constructing the alternative hyposisis to use in forming an index.

3.2 Famulari's Power Index

Famulari (1995) offered a natural variant of Bronars' Index.[4] Consider a person who was observed with n price vectors $(p_1, ..., p_n)$, made choices $(x_1, ..., x_n)$, and thus had expenditures $(p_1x_1, ..., p_nx_n)$. The alternative hypothesis she suggests is that individuals randomly assigned their choices to the set of prices. Thus, randomly reorder the prices and rename them $(q_1, ..., q_n)$. Then evaluate the expenditures $(q_1x_1, ..., q_nx_n)$ for violations of GARP. Considering all possible orderings of the price vector, one can calculate the expected number of violations of GARP under the alternative hypothesis as the power of the GARP test.

This method has a distinct advantage over Bronars' Index in that it takes account of the set of choices actually made. Famulari's Index will, for instance, show zero power for someone

[3] One should also note the paper by Aizcorbe (1991) that argued that using Bronars' method to search for WARP violations in all pairs of observations may misstate power in that violations over pairs are not independent (comparing bundle a vs. b is not independent of the comparison of b vs. c). She then suggests a lower bound estimate of power based on independent sets of comparisons.

[4] Although Famulari did not formally present her idea as a power index, we do so here. Note, a similar approach was taken by Cox (1997) in his CPower measure, which was discovered independently.

who always spends his whole budget on only one good, but higher power for someone who always chooses interior solutions. Bronars' Index, by contrast, would show identical power for both. However, constructing this index requires considering some expenditure levels that were not in the data, and not all would be feasible to the person observed. Hence, a natural variant of Famulari's idea would be to randomly assign the budget shares actually chosen to the various price vectors offered. Note that this method is best suited to the case in which each person was observed to make choices under many distinct price vectors.

What follows next is a series of four different approaches to power indices. The first two methods, the Afriat Power Index and the Optimal Placement Index, do not specify alternative hypotheses and thus do not allow a statistical measure of power. They are better thought of as indices that will be correlated with power. Their advantage is their simplicity and intuitive appeal. The next two methods, Jittering and Boostrapping, will specify alternatives and calculate power based on this. All four indices derive their measure from the preferences exhibited by the individuals studied. Next we turn to presenting the four new indices.

4 The Afriat Power Index

Although the index proposed in this section was not suggested by Afriat, it seems natural to give it his name, for reasons that will become clear.

Varian (1990, 1991), building on Afriat (1967, 1972), constructed an index to describe the severity of a violation of revealed preference. To do so, Varian first defines a variant of the directly revealed preferred relation, $R^d(e)$, this way: $x_j R^d(e) x$ iff $ep_j x_j \geq p_j x$, where $0 \leq e \leq 1$. It follows to define $R(e)$ as the transitive closure of $R^d(e)$. Varian defines a version of GARP, which we call L-GARP(e) ("L" for lower), as

Definition: *L-GARP(e):* If $x_j R(e) x_k$, then $ep_k x_k \leq p_k x_j$, for $e \leq 1$.

Afriat's Critical Cost Efficiency Index, or the Afriat Efficiency Index for short, is the

largest value of $e \leq 1$, say e^*, such that there are no violations of L-GARP(e). If $e^* = 1$ then there are no violations of GARP in the original data, but for $e^* < 1$ there are violations. The value of e is illustrated in Figure 1. In each frame there is a violation of GARP and the dashed line shows how much a budget will have to be "relaxed" in order to generate no violations of L-GARP(e). The choices on the left are thought to be more severe violations of revealed preference than those on the right, and thus get a smaller value for the Afriat Efficiency Index. Before conducting analysis the researchers can set some critical level of e^*, say $\overline{e}$, such that they would consider any $e^* \geq \overline{e}$ a small or tolerable violation of GARP. Varian (1991), for instance, suggests a value of $\overline{e} = 0.95$.

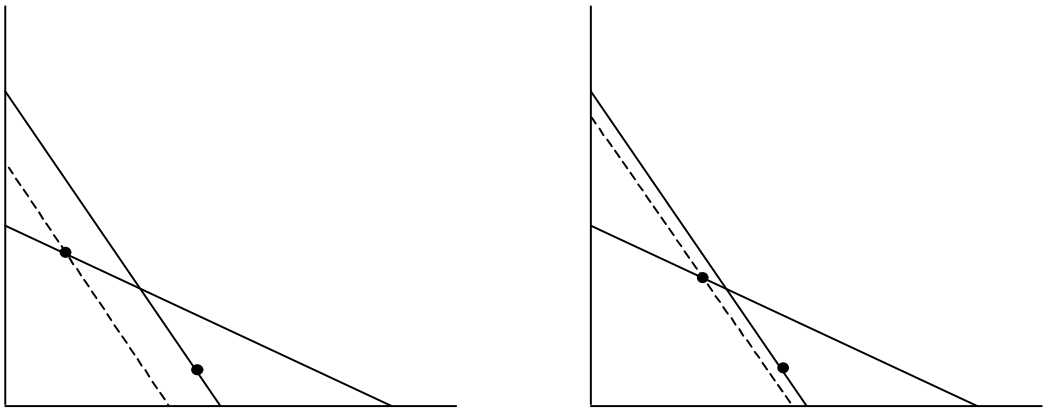


FIGURE 1

We can apply similar intuition to generate an index of power. Suppose a set of choices *does not* violate GARP. If the budget constraints cross near the area that subjects are actually choosing, then we can think of that set of budgets as being more diagnostic than a different set in which the choices are far from the intersections. For instance, Figure 2 shows two budgets without violations of revealed preference. However, the frame on the right gives us more confidence that the person choosing these goods satisfies utility maximization. If there were a violation or rationality, we would be more likely to uncover it in the right panel since even a small change in choices would have been enough to violate GARP. In the frame on the left, by contrast, there would have to be much larger violations of rationality before we could uncover them with this test.

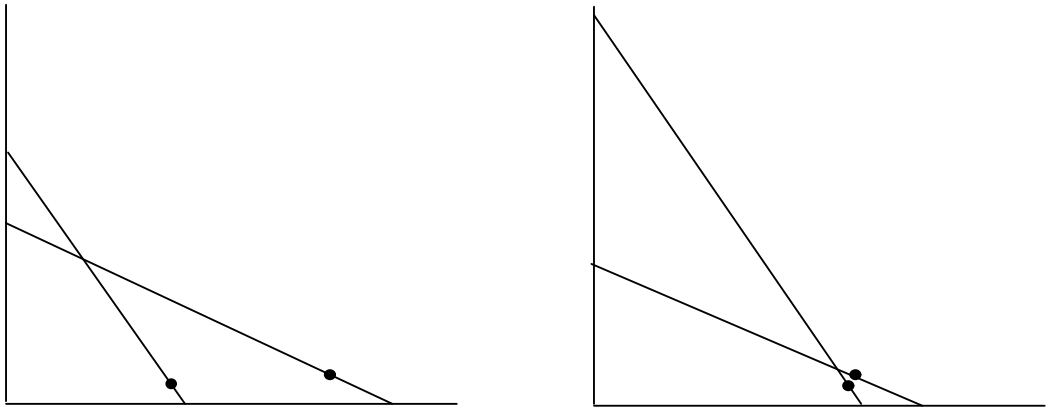


FIGURE 2

To capture the intuition behind Figure 2, define a concept $\widetilde{R}^d(g)$ as $x_j\widetilde{R}^d(g)x_k$ iff $gp_jx_j \geq p_jx_k$, where $g \geq 1$. Thus, if $g = 1$ we have the standard notion of directly revealed preferred. Then let $\widetilde{R}(g)$ be the transitive closure of $\widetilde{R}^d(g)$. Given this, we can define a new concept H-GARP ("H" for higher) as

Definition: *H-GARP(g):* If $x_j\widetilde{R}(g)x_k$, then $gp_kx_k \leq p_kx_j$, for $g \geq 1$.

Using this inverted notion of the Afriat Efficiency Index, we can define the Afriat Power Index as the *smallest* value of $g \geq 1$, say g^*, such that there is *at least one* violation of H-GARP(g). If $g^* = 1$ there is a violation of GARP in the original data. If $g^* > 1$ there are no violations of GARP in the data, but if g^* is close to 1 the choices are near where the budget constraints intersect. An example of the Afriat Power Index is shown in Figure 3. The choices on the left are less informative about rationality than those on the right, and the Afriat Power Index is closer to 1 in the panel on the right. Hence, while the Afriat Efficiency Index told us how much we need to "relax" the budgets to avoid violations, the Afriat Power Index tells us how much we need to "expand" budgets in order to generate violations. Note that an Afriat Power Index will always be finite as long as there is at least one pair of choices that can be ranked by revealed preference.

Notice what happens if a single choice is made at the point where two budgets intersect. Suppose first that there is no violation of GARP. Then the smallest shift in one budget constraint will create a violation, in which case $g^* = 1 + \varepsilon$, where ε is infinitely small. For

ease of discussion, we will refer to this as a case of $g^* = 1$. By contrast, suppose this point is involved in a violation of GARP. Then $e^* = 1 - \varepsilon$ can remove the violations, thus for convenience we use $e^* = 1$.

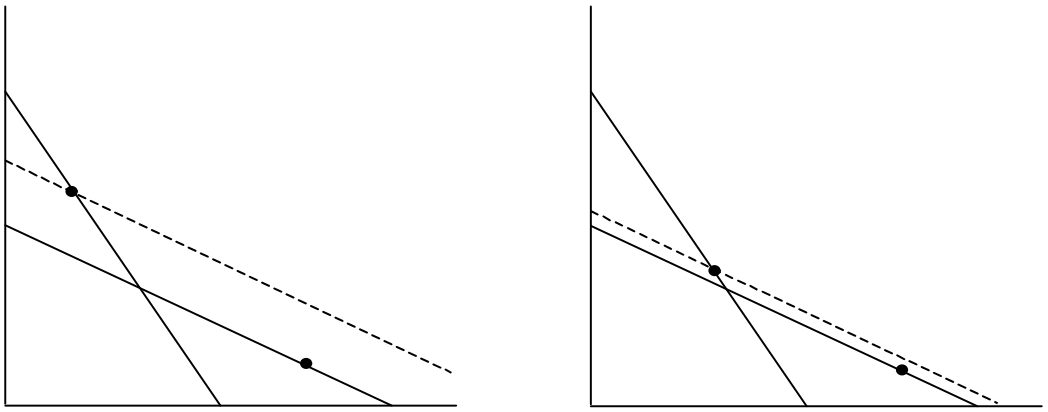


FIGURE 3

When can we say that the g^* found from the Afriat Power Index is "too big" and thus has too little power? One obvious approach is to switch our perspectives. If under the Afriat Efficiency Index we were willing to accept any $e^* \geq \bar{e}$ as an acceptably small violation of GARP, then any $g^* \leq 2 - \bar{e}$ should also be an acceptably powerful test of GARP.

4.1 Combining the Indices: The Afriat Confidence Index

Assign an individual i a number $A_i = e_i^* g_i^*$, where e_i^* is from the Afriat Efficiency Index and g_i^* is from the Afriat Power Index.[5] Call A_i person i's *Afriat Confidence Index.* If $A_i < 1$ the person has at least one violation of GARP and this number can be interpreted as indexing the severity of the violation. If $A_i > 1$ then the person has no violations of GARP, and the number can index the stringency of the GARP test. An $A_i = 1$ corresponds to the most ideal data—the person could not have been given a sharper or more successful test of revealed preference.

[5] Alternatively, we could derive A_i from a unified framework. Define $R_A^d(a)$ as $x_j R_A^d(a) x_k$ iff $a p_j x_j \geq p_j x_k$, for some $a > 0$, and let R_A be the transitive closure of R_A^d.
Define *A-GARP(a):* If $x_j R_A(a) x_k$, then $a p_k x_k \leq p_k x_j$, for $a \geq 0$.
Then let $a_i^* = inf\{a$: there exits a single violation of *A-GARP(a)*, or at which the smallest change in a would remove all violations of *A-GARP(a)*$\}$. Then $A_i = a_i^*$.

Notice that applying the Afriat logic to both the failures and successes of GARP tests gives us some bounds on our test. By selecting an $\overline{e}$ prior to analysis we gain a "confidence interval" on A_i, that is $\overline{e} \leq A_i \leq 2 - \overline{e}$. An A_i in this interval can be seen as a successful test of GARP.

4.2 Strengths and Weaknesses of the Afriat Confidence Index

A distinct advantage of the Afriat Confidence Index is that it makes a great deal of sense to combine the Afriat Efficiency Index with the Afriat Power Index. The Afriat Efficiency Index, despite its weakness, is still the primary index applied to violations. With a few changes in one's computer programming code for constructing the Efficiency Index, it is trivial to construct the Afriat Power Index and hence the Afriat Confidence Index.[6]

Nonetheless, the original Afriat Efficiency Index has long be seen as an imperfect measure. For instance, it is defined for only the *worst* violation, and does not give credit to an individual who may otherwise have large numbers of perfectly rational choices. In other words, it is not very forgiving of a single error. By the same token, it can potentially mask the troubling nature of a large number of small errors.

Similarly, the Afriat Power Index is also imperfect. It will score well if there is a single pair of budget constraints which cross near the choices, even if all other budget constraints cross far from the choices. Moreover, A_i will be 1 if a single choice falls on two budgets (violating WARP but not necessarily GARP), which may give a misleading impression of the power of the test overall. Importantly, this includes the case of corner solutions that occur on two budgets, even though such budgets have no chance of violating GARP. This should give designers of experiments reason to avoid budgets that intersect at corners.

[6] Varian (1991) constructs an improvement on the Afriat index that finds the *minimal* perturbation necessary to remove cycles in the data. This number will, in general, be closer to 1 than the Afriat index when there are violations of GARP. When there are no violations of GARP, one can also invert Varian's approach as we have done above with the Afriat Index. However, since we are finding the value of g such that an ε increase would result in a violation, we are automatically finding the shortest cycle. Thus, inverting Varian's goodness-of-fit approach would result in the same number as the Inverse Afriat Index above.

5 The Optimal Placement Index

Consider the choices a on budget A and b on budget B in the left panel of Figure 4. Here there are no violations of revealed preference. Start with choice a. If *ex post* we were to design the placement of a second budget that would have the maximum power to test whether a is a rational choice on A, we would obviously choose a budget that would intersect A at point a. Hence, rather than choose constraint B, we would choose C. How much better is C than B at testing rationality? As seen on the right panel of Figure 4, on budget B there is a fraction d/D of choices available that would violate WARP, while on constraint C there is a fraction e/E of available choices that would violate WARP. Hence, we can construct an index

$$\theta_{ab} = \frac{d/D}{e/E} = \frac{d}{e}\frac{E}{D}$$

to indicate the relative power of the test against choice a.

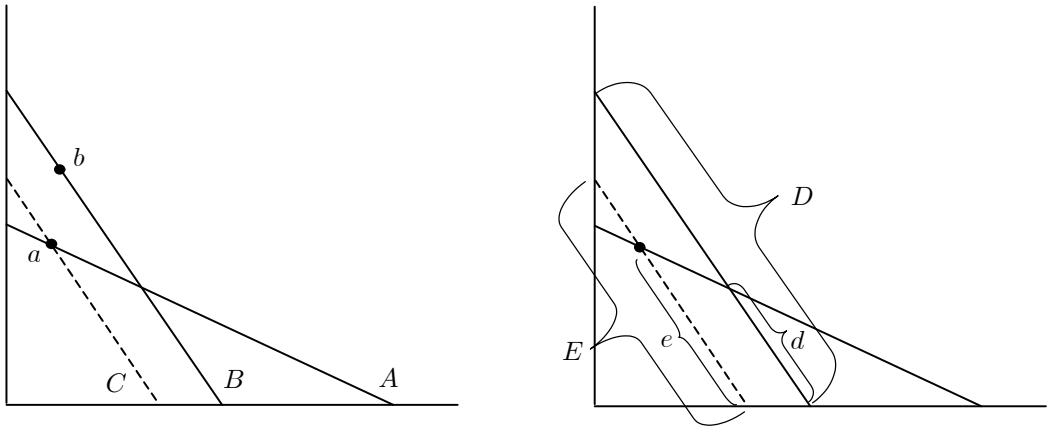


FIGURE 4

How about choice b on budget B in Figure 4? Here the budget A has no ability to find a violation of WARP, conditional on the observation of b. In this sense, the test has no power to show that b was chosen irrationally. Hence, we can say $\theta_{ba} = 0$. We can then state the power index for this particular pair of budgets as the maximum θ, that is $\theta^* = \max\{\theta_{ab}, \theta_{ba}\}$. We can call θ^* the *Optimal Placement Index.*

When there are more than two budgets and more than two goods, the idea gets a bit more interesting. In this context, θ_{ij} refers to the relative area of a budget surface, not simply budget lines. We must also account for multiple budget crossings. For every budget i, calculate the value of θ_{ij} for all $j \neq i$ other budgets (if a budget j does not cross i then $\theta_{ij} = 0$). Then for budget i define $\theta_i^* = \max\{\theta_{i1}, \theta_{i2}, ..., \theta_{i,i-1}, \theta_{i,i+1}, ..., \theta_n\}$. This θ_i^* is the maximum power attained over budget i. We can construct a power index by creating a metric over the θ_i^*'s. There are two natural metrics to use: the maximum, $\theta^* = \max\{\theta_1^*, \theta_2^*, ..., \theta_n^*\}$, and the average, $\theta^* = (1/n)\sum_{i=1}^n \theta_i^*$. We can call these the max-of-max's and the ave-of-max's aggregations of $\theta_{ij.}$ The higher the value of θ^* the greater the power of the revealed preference test.

An alternative way to define the index would be to begin by averaging across the other budgets, defining $\theta_i^{**} = \text{average}\{\theta_{i1}, \theta_{i2}, ..., \theta_{i,i-1}, \theta_{i,i+1}, ..., \theta_n\}$. We then again have two options for the overall power, $\theta^{**} = \max\{\theta_1^{**}, \theta_2^{**}, ..., \theta_n^{**}\}$ and $\theta^{**} = (1/n)\sum_{i=1}^n \theta_i^{**}$. We can call these the max-of-ave's and the ave-of-ave's aggregations of $\theta_{ij.}$ We will explore all four of these possible aggregations in Section 8 below. We will see that the max-of-max's is the least informative, but the other three are highly correlated.

Note that if a choice lies on two budget constraints, this approach will assign a value to that point of $\theta_i^* = 1$. If, by chance, all choices lie at points of intersection of two or more budgets then the power index will be $\theta^* = 1$. It is also possible for the index to take on a value of zero, even when budgets cross. Suppose, for instance, that choices are like those in Figure 3 above. Then $\theta^* = 0$ since, *ex post*, neither budget offers an opportunity to show the choice on the other budget is a violation of WARP. In fact, the index can move discontinuously from 0 to 1. For instance, if in Figure 4 the point a is "slid" down budget A, θ_{ab} rises to 1 at the intersection with budget B, but falls to 0 when a crosses the intersection. However, as with the Afriat Power Index, the Optimal Placement Index will always show power if at least one pair of choices can be ranked by revealed preference.

This method is similar to the "Sequential Maximum Power" technique of Blundell, Browning and Crawford (2003). Their analysis was aimed at data with sufficient observations to (nonparametrically) estimate expansion paths. For a given choice, the researcher can find the optimally placed budget, as we use the term above, and then use the expansion path to project a choice onto this optimal budget. In this way one can construct the optimal (most powerful) test of GARP using only the optimally placed budgets. If, as they suggest, estimating expansion paths is possible, then applying their test will give an Optimal Placement Index of 1. However, if there is insufficient data to estimate expansion paths then their Maximum Power test will be impossible. In this case, the power index presented here gives us a measure of how close the budgets in the GARP test come to meeting the Blundell, Browning and Crawford ideal.

5.1 Strengths and Weaknesses of the Optimal Placement Index

A shortcoming of this metric is that it is only operable for WARP, not GARP. When there are only two goods, WARP is both necessary and sufficient for the existence of a well behaved (strictly convex) preference, so the metric above would suffice. However, for more than two goods, WARP ceases to be sufficient. While it could in principle be generalized to GARP, the process would be difficult and tedious, with dubious net benefit. Moreover, with only two goods it is impossible to have a violation of GARP without also having a violation of WARP, although this is not the case with more goods. As a result, this power test is more demanding of the budgets than GARP would require, which will mean that the true power of the test is likely to be higher than this index might imply.

A second shortcoming of this index is that it only specifies the optimal placement of budget constraints for predetermined slopes.[7] In this index the slopes of budgets are not chosen, so neither is the index truly optimal.

Third, there is no natural threshold to indicate when a test has high power, short of $\theta = 1$. At best, therefore, this index can indicate relative degrees of power.

[7] This is also true of the Blundell, Browning and Crawford (2003) analysis.

Finally, there is an unstated assumption that the budget providing the greatest proportion of itself exposed to a violation of revealed preference is also the most likely to find such a violation. If all goods are normal goods, then this conclusion follows naturally from Proposition 1 of Blundell, Browning and Crawford (2003). Hence, normality is required for the optimally placed budgets to maximize power.

6 Jittering

Implicit in the prior two methods is that behavior is measured without error. Next suppose that there may be an element of randomness to either measurement or behavior. This measure of power is based on comparing the variation in choices observed to the degree of randomness necessary to generate violations of GARP.

To motivate this approach, suppose a person was offered the five budget constraints pictured in Figure 5, and all of the choices involved equal quantities of both goods, as in the left panel of the figure. These choices do not violate GARP and are consistent with preferences that have a kink at the 45-degree line, as would Leontief preferences. If we were to posit a sixth budget we would likely predict that, again, the choice would be on the 45-degree line. Hence, there appears to be very little randomness to these choices. By the same token, adding only the slightest shift in choices along the budget constraint (in the right direction) would result in a GARP violation. Hence, we would like to conclude that this is a very strong test of rationality—the data shows a great deal of regularity and predictability, and only the slightest perturbation would result in violations of GARP.

Compare this to the data shown in the right panel of Figure 5. Here the data look as though they are consistent with a perfect substitutes utility function. However, despite the relative degree of predictability of the data, there would have to be very big perturbations added to the data in order to generate violations of revealed preference. Hence, relative to the left panel, the right panel is a less powerful test of rationality.

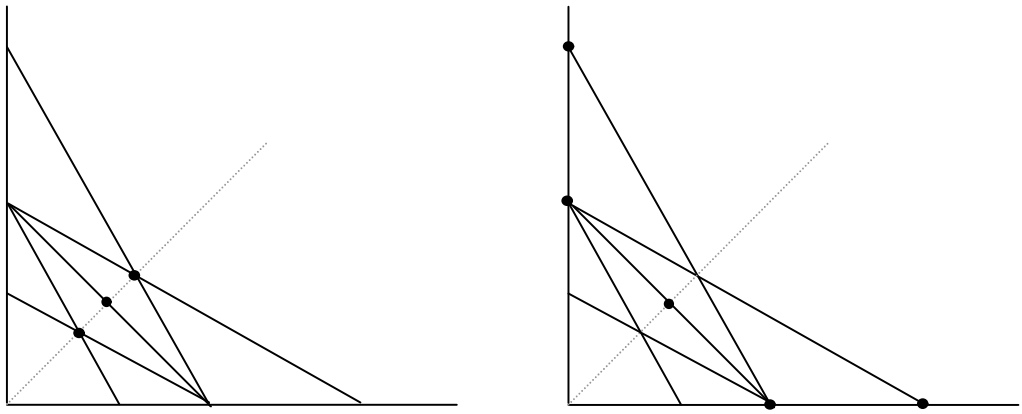


FIGURE 5

To formalize these intuitions, we first need to define the concept of errors. Restrict all budget constraints to be linear. Represent each choice as a point x_k along the budget line k of length ℓ_k. That is, $x_k \in [0, \ell_k]$.[8] Suppose the true choice is z_k but that the researcher observes $x_k = z_k + \varepsilon_k$, where the ε_k are independently and identically distributed according to some function $f(0, \sigma^2)$.[9]

Next we need to construct two measures. First is a measure of how much error we need to add to the data in order to generate a predetermined severity of GARP violations. Second is a measure of the amount of variance or error occurring naturally in the data. By comparing the variation we need to add to the naturally occurring variation, we can get an index of how tightly the model has been tested.

We can get the first measure by adding random noise to the observed data. Let $\tilde{\varepsilon}_k$ be draws from the distribution $f(0, \tilde{\sigma}^2)$ for a specified value of $\tilde{\sigma}$. Let $\tilde{z}_k = x_k + \tilde{\varepsilon}_k$. Then for a given set of draws $\tilde{\varepsilon}$ we can test the "jittered" data, $\tilde{z}$, for GARP violations.[10] For each $\tilde{\sigma}$

[8] The generalization to budget planes is straightforward. We can think of vector x_k as a point on the plane with vector ε_k drawn from a multivariate distribution. It is important to note that if ther are m goods in the budget, then both x_k and ε_k are of dimension $m - 1$.

[9] The only other case we know of that used this method to get a sense of power was Manser and McDonald (1988). They "repeatedly multiplied all quantities consumed by i.i.d. lognormal psuedo random numbers with unit expectations." They progressively increased the variance of the lognormal random numbers and measured the violations of GARP over 100 simulations. They found standard deviations of 0.10 to 0.20 were needed to get significant violations of GARP, which they interpreted as strong power.

[10] Note that the terms "jitter," "jittered data," and "jitter statistics" are not original to this paper, but are well-accepted statistical terms. Jittering is commonly used in engineering, for instance.

we repeat this exercise for say 10,000 iterations. We can then search for the value of $\tilde{\sigma}$ such that, say, 5% of the jittered experiments find at least one GARP violation or have an Afriat Efficiency Index of $e^* \leq 0.95$ (or some other "critical" value). We can think of this critical value as that which we would use to reject the rationality of the data when there are errors. Call the value of $\tilde{\sigma}$ that meets the criterion σ^*. This σ^* gives us an indication of how close the chosen budgets came to finding a violation of rationality—the closer σ^* is to zero, the sharper the test of rationality.[11]

How do we use σ^* to ask whether the original data provide a powerful test of GARP? A sensible way is to test whether the noise added to create the jittered data, $\tilde{\varepsilon}$, is significantly bigger than the noise naturally occurring in the data, ε. A test with low significance will have high power.

Consider a test of the null hypothesis that $\tilde{\varepsilon}$ and ε both have the same variance, σ^2. For each individual in the sample, consider the statistic

$$\phi = \frac{\sum(x_k - \tilde{z}_k)^2/\sigma^2}{\sum(x_k - z_k)^2/\sigma^2} \approx \frac{\sigma^{*2}}{\sigma^2}.$$

Under the null hypothesis ϕ is characterized by the F distribution. If there are m goods on each of n budgets, then this F–test has $n(m-1)$ degrees of freedom in both the numerator and denominator.[12] Let s be the significance level of ϕ. Then one can consider the confidence of the test to be $1-s$. For instance, if $\sigma^* \approx 0$, then confidence in the test approaches 1.

How can we specify the level of natural variance σ? This question is reminiscent of that encountered by Varian (1985) in his goodness-of-fit analysis, and the answers are thus similar. One option is to find a parametric estimate of a utility function and let the standard error of the regression stand for σ. This, obviously, dilutes the value of non-parametric analysis with parametric analysis. Moreover, there often may be too few observations from a single

[11] Note that this method even works to find power when there are violations of GARP, but just relatively few. We may still want to think of performing jittering to see how much noise we need to add to bring violations up to some critical value.

[12] Recall that we are thinking of x as a point on a budget plane. Thus there are only $m-1$ independent values in the vector x, and $m-1$ elements in ε. Note also that the vector notation implies that $\sum(x_k - z_k)^2 = \sum_{k=1}^{n}\sum_{i=1}^{m-1}(x_{ki} - z_{ki})^2$.

agent to estimate such a function. We would be left to postulate σ from some other ad hoc means. Alternatively, we could derive the level of natural variance in the data needed to justify a given level of confidence. For instance, suppose we would conclude that the power of the test is insufficient if $\phi > C$. Then a $\phi \leq C$ would meet the desired level of confidence. Let $\overline{\sigma} = \sigma^* C^{-1/2}$. Then any $\sigma \geq \overline{\sigma}$ would be enough natural variance to satisfy the desired confidence, and we could appeal to intuitions about whether $\overline{\sigma}$ is "small." For example, suppose we gave a subject 8 budgets of two goods each, found no violations of GARP, and determined $\sigma^* = 0.2$ would generate a 5% chance of a violation. Suppose we would conclude that the GARP test is of weak power if ϕ revealed a difference between the variance of $\tilde{\varepsilon}$ and ε at, say, the $\alpha \leq 0.05$ significance level. From a $F(8,8)$-Distribution table we find $C = 3.44$, and solving we find $\overline{\sigma} = 0.11$. This implies that if the natural error in the data is from a normal distribution of mean zero with standard error of $\sigma \geq 0.11$, then we would not reject the test as lacking in power. If this value of $\overline{\sigma}$ seems reasonable given the circumstances, then the researcher can be comfortable with the power of the test.

Applying this to the example given in Figure 5 above, we could easily conclude that in both cases the level of natural variance is small since the data are so well organized and conform to an easily estimated utility function. However, in the panel on the left, the tiniest σ will generate violations of GARP ($\sigma^* \approx 0$ in this case) and the revealed preference test is passed with confidence approaching 1. By contrast, the right panel passes but with very low confidence. Here σ is again close to zero, while σ^* is going to be above 0.25, making ϕ extremely high and the required natural variance to be unnaturally large.

6.1 Strengths and Weaknesses of Jittering

The F–test above, as with Varian's (1985) chi-squared test of goodness-of-fit, has some distinct costs and benefits. The main strength of the approach is the ability to specify a statistical level of confidence for a stated σ. The main weakness is, obviously, having to state a σ and specify the probability distribution function as normal. Varian answers this by arguing that conceding a normal distribution is a small sacrifice compared to a full-blown

parametric estimation of utility. Moreover, having to specify a σ is tempered by being able to state a needed $\overline{\sigma}$ threshold for variance in the data. If $\overline{\sigma}$ is a number that all would agree is small given the nature of the data, then arguments over σ may be avoided.

7 Bootstrapping Indices for Panel Data

This technique was introduced in Andreoni and Miller (2002) and Harbaugh, Krause, and Berry (2001). We include it here for completeness.

When there are several measures on a series of subjects, one can ask the question of the power of the test in a new way. In particular, one can ask whether the organization put on the data by the subjects themselves—by matching individuals with choices—is superior to another method that would have randomly assigned choices to individuals from the universe of choices actually made.

For simplicity, consider an example of two experimental subjects given the same two budgets. Suppose the data are like that shown in Figure 6. Here there are no violations of revealed preference. Suppose that, on each budget, we were to pool the choices made by the subjects and then create new synthetic subjects by randomly drawing from the universe of choices actually made. That is, we use bootstrapping techniques to generate a measure of power. In the example of Figure 6, $x_1 \in \{a, d\}$, and $x_2 \in \{b, c\}$. Then there would be a 25% chance that the synthetic subject would be assigned choices a and c, hence violating GARP, which is the maximum likelihood possible with two budgets and no initial violations of revealed preferences.

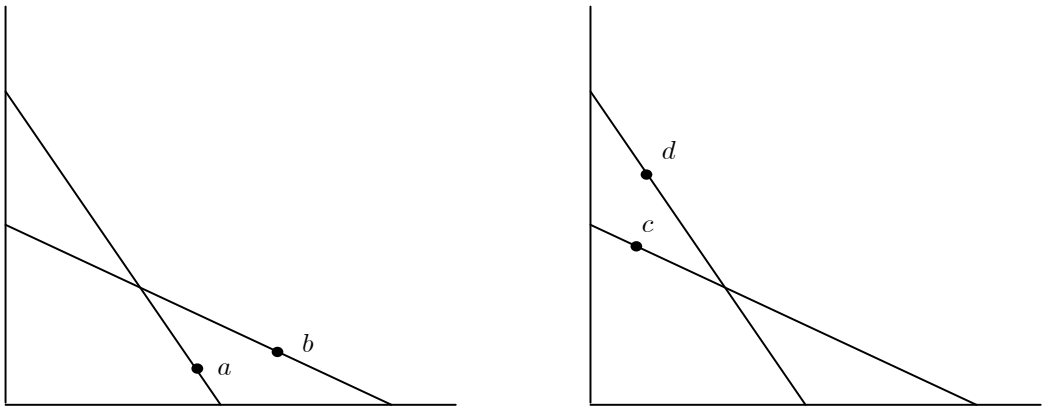


FIGURE 6

Compare these choices to those in Figure 7. Here there would be no chance that we could create a synthetic subject that would violate GARP. In this sense, the test has more power if the study generates data like that in Figure 6 rather than Figure 7.

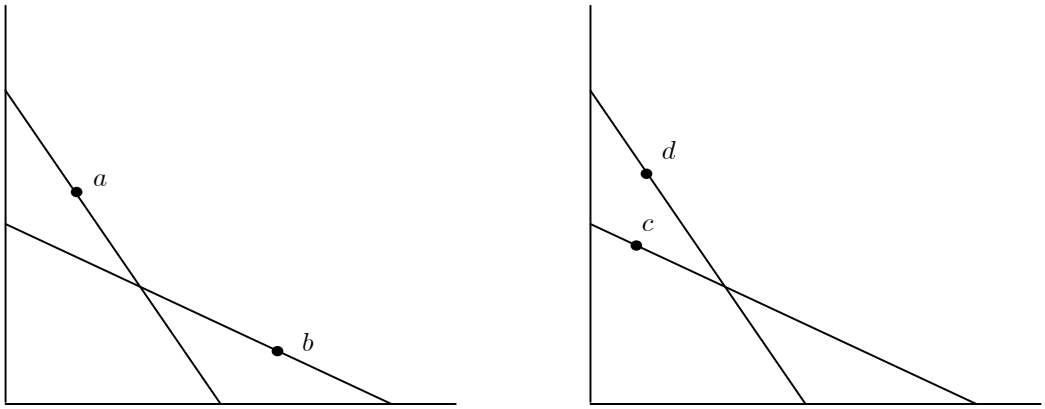


FIGURE 7

Note that this technique can report either greater or lesser power than a simple Bronars method of randomly assigned choices along budgets. For instance, in the budgets shown in Figures 6 and 7 a Bronars (Monte Carlo) test would show only about 12% of the cases finding violations, whereas the bootstrapping test will get exactly 25% violations (Figure 6) or 0% violations (Figure 7).

We can now specify as the alternative hypothesis that the full sample of choices on each budget is the population and that choices along a budget were chosen from this set at

random, with replacement. With this alternative, the probability of violations among the synthetic subjects is the power of the test.

7.1 Strengths and Weaknesses of Bootstrapping Methods

The main strength of this panel bootstrapping technique as compared to, say, Bronars' method, is that by treating the sample as the universe it reveals how well the test was suited to the population studied, and whether the test was indeed successful in generating enough variation in choices to make violations of GARP a credible possibility. This power index is particularly well suited to experiments with large numbers of subjects. Like the Bronars method, however, the alternative hypothesis specified is still likely to ascribe too much randomness to each subject, especially in very heterogeneous populations of experimental subjects.

8 Application to Experimental Data

In this section the indices described above will be applied to an experimental data set. The data employed are described in detail in Andreoni and Miller (2002) and Andreoni and Vesterlund (2001). Briefly, the experiment was designed to explore individual preferences for altruism by asking subjects to make a series of choices in a Dictator game, under varying incomes and costs of giving money to another subject. In particular, subjects made eight choices by filling in the blanks in statements like this: "Divide M tokens: Hold ____ at X points, and Pass ____ at Y points (the Hold and Pass amounts must sum to M)," where the parameters M, X, and Y were varied across decisions. The subject making the choice would receive the "Hold" amount times X, and another subject would receive the "Pass" amount times Y. All points were worth \$0.10.

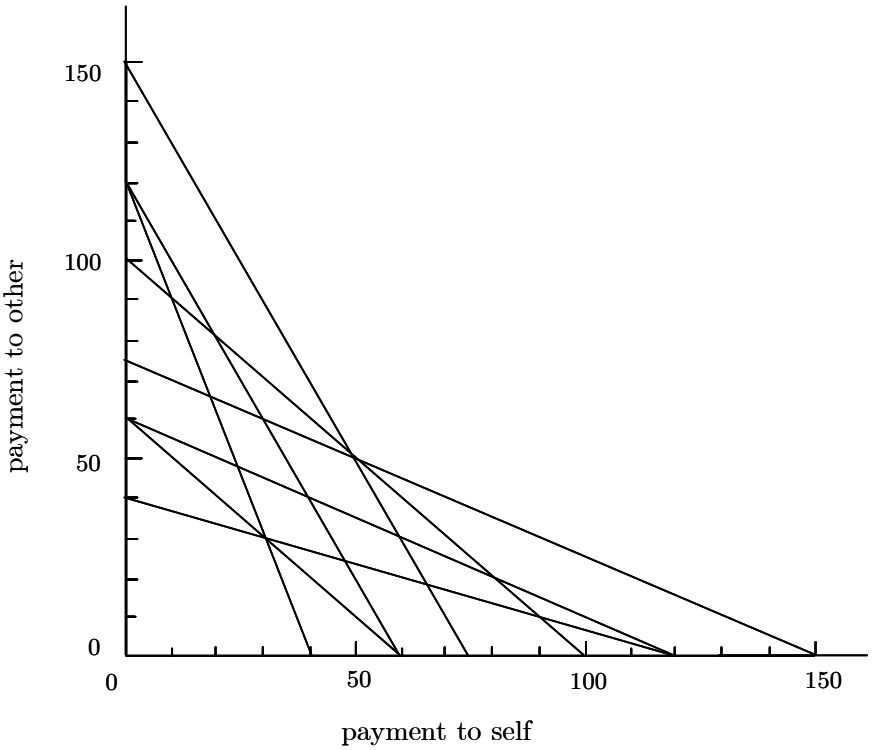


FIGURE 8: ANDREONI AND MILLER (2002) BUDGETS

Let π_s be payoff to self, and π_o be payoff to other. The hypothesis is that individuals have well-behaved preferences $U_s = U(\pi_s, \pi_o)$.The experimental parameters imply a budget constraint for any choice of

$$\frac{1}{X}\pi_s + \frac{1}{Y}\pi_o = M.$$

The parameters chosen provided the budgets shown in Figure 8. As can be seen, the pie to be divided ranged from \$4 to \$15 and the relative prices ranged from 3 to 1/3. After subjects made all 8 choices, one choice was selected at random by the experimenter and carried out.

Data was collected on 142 subjects and each subject's choices were tested for violations of GARP.[13] The result was that 13 of the subjects (9.1%) had violations of GARP. Applying the Afriat Efficiency Index, only 3 of these were found to be large violations (as we show below). This is a rather striking failure to contradict the neoclassical model of preferences, but leaves open the question of how discriminating the GARP test was at uncovering potential

[13] Andreoni and Miller (2002) report data on 176 subjects, but their session 5 is set aside here for brevity.

violations.[14] We consider the indices provided above, starting with the Bronars Index.

Bronars Index. Taking one million random draws from a uniform distribution on each of the eight budget sets, we found 0.78 of all Monte Carlo experiments resulted in at least one violation of GARP. Under this alternative hypothesis, Bronars' Method 1 has a modest degree of power. His Method 2, with random budget shares, fares worse, with a power of only 0.63. Method 3 is still worse, with a power of only 0.48. This illustrates the sensitivity of the power test to the alternative hypothesis.

Afriat Confidence Index. Table 1 shows the frequency of Afriat Confidence Indices, A_i, for all 142 subjects. The top of the table shows the 13 subjects who violated GARP at least once, and the bottom shows the 129 who had no violations. Nine of the 13 violators had Afriat Confidence Indices of 1, indicating that the smallest change in choices would remove all violations of GARP. One subject had an Afriat Confidence Index of 0.98, which is within the threshold setting of 0.95. Three of the 13 had severe violations beyond this threshold.

How about the 129 subjects who showed no violations? More than two-thirds of these (71%) had Afriat Confidence Indices of 1, indicating that the GARP test could not have been sharper. If we apply the same criterion for "high power" that we do to "small violation" then 107 (83%) of the non-violators have $A_i \leq 1.05$. Then the "confidence interval" of Afriat Confidence Indices such that $0.95 \leq A_i \leq 1.05$ includes 117 subjects. In sum, this means that 82.4% of subjects were given stringent tests of GARP and passed, 2.1% of subjects had significant violations of GARP ($A_i < 0.95$) and 15.5% were given GARP tests that were not sufficiently diagnostic ($1.05 < A_i$). Using the most stringent confidence interval, $A_i = 1$, we find 73.2% of subjects passed the strongest possible test.

[14] Andreoni and Miller (2002) reported both the Bronars Method 1 power index and the panel index. We repeat them here for completeness.

TABLE 1
AFRIAT CONFIDENCE INDICES, A_i

	A_i	Frequency	Percent
Violation of GARP:	0.83	1	0.70
	0.92	2	1.41
	0.98	1	0.70
	1.00	9	6.34
No Violation:	1.00	95	66.90
	1.01	2	1.41
	1.02	1	0.70
	1.03	3	2.11
	1.04	6	4.23
	1.07	4	2.82
	1.08	10	7.04
	1.10	1	0.70
	1.13	2	1.41
	1.14	1	0.70
	1.15	1	0.70
	1.17	3	2.11
Total:		142	100.00

Optimal Placement Index. There were four ways proposed to report this index. The first two begin by finding the maximum measure for each budget, with 1 being the ideal, and then either find the maximum across budgets (Max of Max's) or the average across budgets (Ave of Max's). The next two begin by averaging the power provided by all other budgets, and then find the maximum across budgets (Max of Ave's) or the average across budgets (Ave of Ave's).

Table 2 shows the results from the Optimal Placement Index. The Max of Max's measure reveals that 95% of subjects were, at some point in the study, given a most powerful test at least one time. The lowest Max of Max's index, $\theta = 0.75$, indicates that the most powerful test this subject faced was 75% as powerful as the most powerful test available. The obvious problem with the Max of Max's measure is that it only identifies the best test faced by the subject. The remaining three columns indicate that this is hiding a great deal of

heterogeneity across subjects in the power they faced. Here we are confronted directly with the fact that this index does not give us a specific criterion for high or low power. Compare the Max of Max's to the Ave of Ave's, for instance. One gives the impression of very high power, but the other of very low power, yet they are simply different ways of aggregating the same power measure.

TABLE 2
OPTIMAL PLACEMENT INDEX

	Max of Max's		Ave of Max's		Max of Ave's		Ave of Ave's	
Range*	Freq.	%	Freq.	%	Freq.	%	Freq.	%
95–100	135	95.1	24	16.9				
90–95			1	0.7				
85–90	3	2.1	4	2.8				
80–85	1	0.7	3	2.1				
75–80	3	2.1	8	5.6				
70–75			6	4.2				
65–70			7	4.9				
60–65			13	9.2				
55–60			2	1.4				
50–55			50	35.2				
45–50			5	3.5	57	40.1		
40–45			8	5.6	1	0.7		
35–40			6	4.2				
30–35			2	1.4	44	31.0	24	16.9
25–30			2	1.4	27	19.0	2	1.4
20–25					10	7.0	13	9.2
15–20			1	0.7			24	16.9
10–15					3	2.1	66	46.5
5–10							11	7.7
0–5							2	1.4
Total	142	100	142	100	142	100	142	100

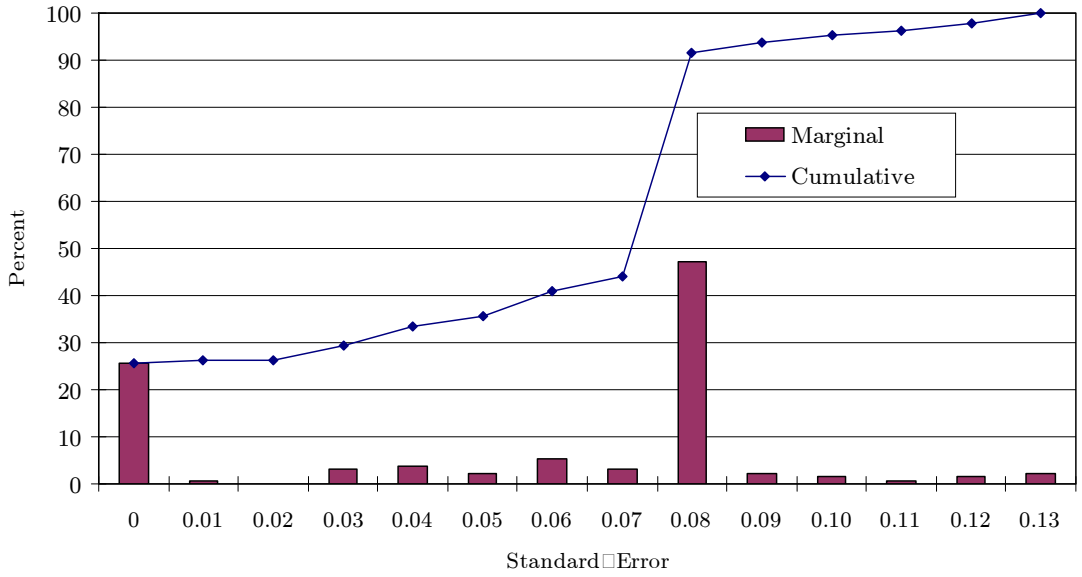
*Each range category includes the upper but not the lower element.

Jittering. There are two obvious ways of specifying the error distribution to jitter the data. First is to define the standard error in proportion to the length of the budget line ($\tilde{\sigma}_i = \tilde{\sigma}\ell_i$), which we call relative errors. The second is to let the distribution be the same for all budgets regardless of length ($\tilde{\sigma}_i = \tilde{\sigma}$), which we call absolute errors. In both cases we

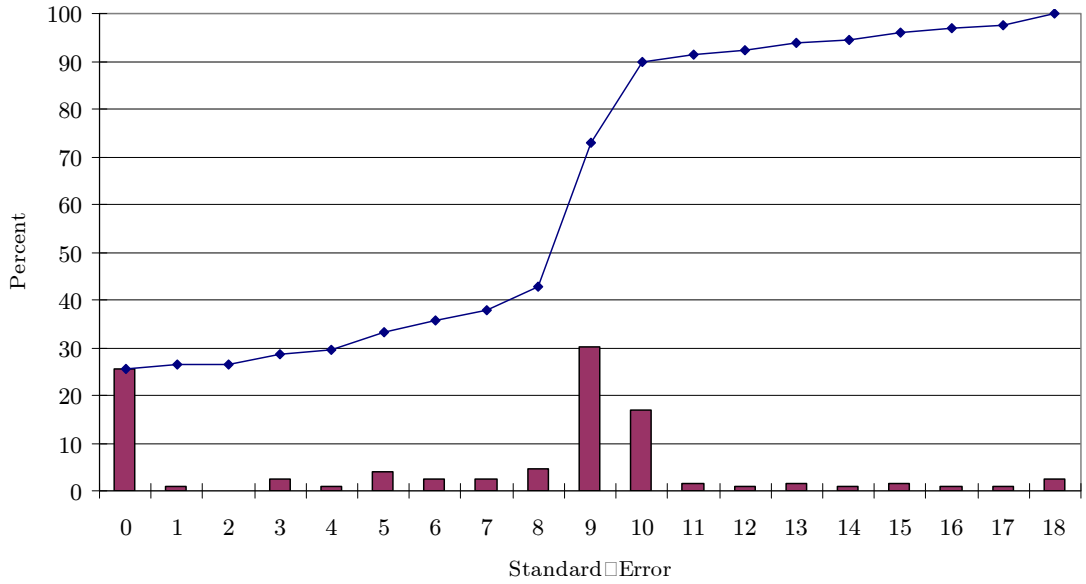
use a truncated normal distribution.[15] After we add the jitters we can then find the critical amount of natural variance in the data, $\overline{\sigma}$, such that the jittered data would fail the F-test at the 95% confidence interval. If the natural error in the data is above $\overline{\sigma}$, then we can believe the test had power of at least 0.95.

Figure 9 shows the values of $\overline{\sigma}$ for all 129 subjects who had no violations of GARP. The bars are the marginal density and the lines are the cumulative density. Panel a shows that, under relative errors, if the natural error in the data exceeds $\sigma_i = 0.08\ell_i$, then 90% of the subjects would have been given significantly powerful tests of GARP. Panel b shows that, under absolute error, a similar degree of power holds if the natural error exceeds $\sigma = 10$. This leads naturally to the question, how much natural error exists in the data? Looking at the data, one sees immediately that one source of natural error is rounding. Perhaps for cognitive ease, subjects have an overwhelming tendency to choose numbers divisible by 10. This is true for both the hold and pass amounts. In fact over 85% of all choices had both the hold and pass values divisible by 10. Another 11% were divisible by 5, but not 10. Only 4% of choices made were not divisible by either 10 or 5.

[15] We also considered censored errors, where the ends of the budget constraints absorbed the extra variance. The results are similar and, for brevity, are not presented.



a. Critical $\overline{\sigma}$ for Relative Error ($\tilde{\sigma}_i = \tilde{\sigma}\ell_i$).



b. Critical $\overline{\sigma}$ for Absolute Errors ($\tilde{\sigma}_i = \tilde{\sigma}$)

FIGURE 9: CRITICAL VALUES OF NATURAL ERROR FOR 95% CONFIDENCE ON JITTERED DATA.

Suppose we assume subjects restrict choices to those where both hold and pass amounts are divisible by 10, and that "rational rounding" would choose the point that yields the highest utility.[16] This means that the maximum error would be at least 5, assuming convex preferences. To be conservative, therefore, assume a uniform distribution of absolute rounding errors between 0 and 5, and thus an expected absolute error of 2.5 tokens. We can calculate what this means for ε_i. Under the assumption of relative errors,[17] this implies $E\ |\varepsilon_i| = 0.43$. For absolute errors this implies of $E\ |\varepsilon_i| = 5.7$. It is easy to show that our assumptions imply the standard error[18] of $\sigma \approx 1.15E|\varepsilon_i|$. For the assumption of relative errors, this means $\sigma_i = 0.049$, while for absolute errors it means $\sigma_i = 6.53$. As a result, rounding errors alone would provide enough natural variance in the data to make at least 38% of our GARP tests have sufficient power. If we were to believe that there is some other independent variation in the data (either from measurement, reporting or learning) that is roughly equal to noise from rounding, so the expected absolute error was about 5 tokens on each budget, then $\sigma_i \approx 0.1$ for relative and $\sigma_i \approx 13$ for absolute errors. If this were the case, then about 95% of the GARP tests would have sufficient power.

Bootstrapping. Since the experimental data set here is actually a panel, we can use the panel techniques to measure the power of the test. On each of the eight budgets there are 142 observations. We conducted one million Monte Carlo experiments where we took one draw from the 142 observations on each budget to form a synthetic subject. Of the million synthetic subjects drawn, 76.6% had at least one violation of GARP. This is very close to the power found from Bronars' Method 1 which assumed purely random draws from a uniform distribution of choices. However, the power from the Panel approach far exceeds the power from Bronars' Methods 2 and 3.

[16] This need not hold at the corners of the budget set where, for instance, holding all 75 tokens might be optimal.

[17] Budgets range in length from 85 to 167. The average length of a budget constraint is 135.

[18] Assume $-a \leq \varepsilon_i \leq a$. Then by symmetry $E|\varepsilon_i| = \int_{-a}^{a} |\varepsilon_i| \frac{1}{2a} d\varepsilon = \int_0^a \varepsilon_i \frac{1}{a} d\varepsilon = a/2$. Also, $\sigma_i^2 = E(\varepsilon_i^2) = a^2/3$. Combining these gives $\sigma^2 = 4E|\varepsilon_i|^2/3$.

9 Correlations Across Indices

Since the indices presented above are attempting to measure the same thing, we also calculated the correlations across the various measures. Table 3 shows the correlations across subjects. This presents the correlations for only the case that GARP was not violated. The reason is that the Afriat Confidence Index is not monotonic, hence correlations using all data would tend to dampen correlations. Nonetheless, all correlations are nearly identical when all observations are used.

The first thing to notice form Table 3 is that the two most blunt indices, the Afriat Confidence Index and the Max of Max's, are very weakly correlated with all other measures. By contrast, the remaining five indices are extremely highly correlated. Perhaps most comforting is that Jittering and the Optimal Placement measures (other than Max of Max's) are very highly correlated, with correlations of 0.74–0.84. In particular, this gives confidence that Jittering—the most parametric but most statistically precise index—is measuring the same thing that the Optimal Placement Index—the most intuitive but statistically imprecise index—is measuring.

Table 3
Absolute Correlations Across Power Indices,
Only When GARP is Not Violated

	Afriat	Optimal Placement Index				Jittering
	Confidence Index	Max of Max's	Ave of Max's	Max of Ave's	Ave of Ave's	$\overline{\sigma}$ relative
Afriat Confidence	1					
OPI* Max of Max's	0.221	1				
OPI Ave of Max's	0.194	0.337	1			
OPI Max of Ave's	0.168	0.295	0.763	1		
OPI Ave of Ave's	0.284	0.242	0.950	0.811	1	
Jittering $\overline{\sigma}$ relative	0.132	0.103	0.831	0.739	0.835	1
Jittering $\overline{\sigma}$ absolute	0.202	0.112	0.802	0.747	0.831	0.990

* OPI stands for Optimal Placement Index

10 Discussion and Conclusion

The objective of this paper was to formally present, analyze, and compare four new approaches to measuring the power of revealed preference tests. The most straightforward approach inverts the well-known Afriat Efficiency Index into the Afriat Power Index. This power index, however, suffers from being a blunt instrument in the same way the efficiency index is an imprecise measure of goodness of fit. Nonetheless, it seems both natural and simple to report the Afriat Power Index alongside the Afriat Efficiency Index in any study, as the Afriat Confidence Index.

The most intuitive index proposed is the Optimal Placement Index. By allowing "20-20 hindsight" to the economist, it asks how well the experimental instrument performed relative to the best possible instrument that could have been dynamically generated after each choice. The intuitive appeal of this metric is tempered by the fact that it does not produce clear guidance on power or a natural threshold to appeal to as "high power." In short, the approach is "too non-parametric."

The Jittering method concedes this by adding a modicum of structure. If, for instance, we assume errors are normally distributed, then we can use an F-test to conclude whether the revealed preference test was sufficiently "close" to finding a violation of rationality if it were present. This test is the least transparent of those presented, and far less transparent than the Optimal Placement Index. However, when all the tests were applied to experimental data, the correlation between Jittering and the Optimal Placement Index was extremely high. This indicates that both the transparent and the opaque techniques are measuring the same thing, and suggests little is compromised—and potentially much gained—by assuming a parametric model of errors.

The final metric presented, Bootstrapping Power, finds the power of the revealed preference test over a full sample, not an individual subject, when there are repeated measures. It asks whether the organization put on the observations by the subjects is superior to that of randomly reassigning and remixing subjects' choices. This has an advantage over prior

measures by relying on the actual sample of choices to generate the alternative hypothesis, rather than imposing, for instance, a uniform distribution across the entire budget set.

In sum, whether using survey or experimental data, the tension between goodness-of-fit and power is clear in revealed preference tests. In this paper we hope to have provided some guidance to researchers to both design and analyze tests that maximize our ability to make the correct inferences about economic models of maximizing behavior.

References

Afriat, Sidney (1967): "The Construction of a Utility Function From Expenditure Data," *International Economic Review*, 8, 67-77.

Afriat, Sidney (1972): "Efficiency Estimates of Production Functions," *International Economic Review*, 13, 568–598.

Aizcorbe, Ana M. (1991): "A Lower Bound for the Power of Nonparametric Tests," *Journal of Business and Economic Statistics,* 9, 463–467.

Andreoni, James and John H. Miller (2002): "Giving According to GARP: An Experimental Test of the Consistency of Preferences for Altruism," *Econometrica*, 70 (2), 737–753.

Andreoni, James and Lise Vesterlund (2001): "Which is the Fair Sex? Gender Differences in Altruism," *Quarterly Journal of Economics*, 116, 293–312.

Battalio, Raymond C., John H. Kagel, Robin C. Winkler, Edwin B. Fisher, Robert L. Basmann, and Leonard Krasner. "A Test Of Consumer Demand Theory Using Observations Of Individual Consumer Purchases," *Western Economic Journal*, 11(4), 411–28.

Becker, Gary S. (1962): "Irrational Behavior in Economic Theory," *Journal of Political Economy,* 70, 1–13.

Blundell, Richard W., Martin Browning, and Ian A. Crawford (2003): "Nonparametric Engel Curves and Revealed Preference," *Econometrica*, 71, 208–240.

Bronars, Stephen G. (1987): "The Power of Nonparametric Tests of Preference Maximization," *Econometrica*, 55 (3), 693–698.

Cox, James C. (1997): "On Testing the Utility Hypothesis," *The Economic Journal*, 107, 1054–1078.

Famulari, Melissa (1995): "A Household-Based, Nonparametric Test of Demand Theory," *Review of Economics and Statistics,* 77, 372–382.

Février, Philippe and Michael Visser (2004): "A Study of Consumer Behavior Using Laboratory Data," *Experimental Economics*, 7, 93–114.

Harbaugh, William T., Kate Krause, and Tim Berry (2001): "GARP for Kids: On the Development of Rational Choice Behavior," *American Economic Review*, 91, 1539–1545.

Houthakker, Hendrik (1950): "Revealed Preference and the Utility Function," *Econometrica,* 17, 159–174.

Manser, Marilyn E. and Richard J. McDonald (1988): "An Analysis of Substitution Bias in Measuring Inflation, 1959-1985," *Econometrica*, 56, 909–930.

Mattei, Aurelio (2000): "Full-Scale Real Tests of Consumer Behavior Using Expenditure Data," *Journal of Economic Behavior and Organization*, 43, 487–497.

Samuelson, Paul A. (1938): "A Note on the Pure Theory of Consumer Behavior,: *Econometrica,* 5, 61–71.

Sippel, Reinhard (1997): "An Experiment on the Pure Theory of Consumer's Behavior," *The Economic Journal*, 107, 1431–1444.

Varian, Hal R. (1982): "The Nonparametric Approach to Demand Analysis," *Econometrica*, 50, 945-973.

Varian, Hal R. (1983): "Nonparametric Test of Models of Consumer Behavior," *Review of Economic Studies*, 50, 99–110.

Varian, Hal R. (1984): "The Nonparametric Approach to Production Analysis," *Econometrica,* 52, 579–597.

Varian, Hal R. (1985): "Non-Parametric Analysis of Optimizing Behavior with Measurement Error," *Journal of Econometrics,* 30, 445–458.

Varian, Hal R. (1990): "Goodness-of-Fit in Optimizing Models," *Journal of Econometrics*, 46, 125–140.

Varian, Hal R. (1991): "Goodness of Fit for Revealed Preference Tests." University of Michigan CREST Working Paper Number 13.