The Rich Domain of Uncertainty: Comment

by

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ABSTRACT

Abdellaoui, Baillon, Placido and Wakker [2011] conclude that different probability weighting functions are used when subjects face risky processes with known probabilities and uncertain processes with subjective processes. They call this "source dependence," where the notion of a source is relatively easy to identify in the context of an artefactual laboratory experiment, and hence provides the tightest test of this proposition. Unfortunately, their conclusions are an artefact of estimation procedures that do not worry about sampling errors. These procedures have become ingrained in experimental economics more generally, and need to be examined carefully. In this case, they make a huge difference to the inferences one draws. Undertaking a maximum likelihood evaluation of their data, allowing for sampling errors, there is no evidence for source dependence.

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Abdellaoui, Baillon, Placido and Wakker [2011] (ABPW) conclude that different probability weighting functions are used when subjects face risky processes with known probabilities and uncertain processes with subjective processes. They call this "source dependence," where the notion of a source is relatively easy to identify in the context of an artefactual laboratory experiment, and hence provides the tightest test of this proposition. Unfortunately, their conclusions are an artefact of estimation procedures that do not worry about sampling errors.[1] These procedures have become ingrained in experimental economics more generally, and need to be examined carefully. In this case, they make a huge difference to the inferences one draws.

Consider the simple two-urn Ellsberg design, the centrepiece of their analysis. The known urn, K, has some objective distribution of balls with 5 colors. Design an experiment to elicit certainty equivalents for a number of these urns, where the probabilities are generated objectively and vary from urn to urn. Assume the subject believes that.[2] The unknown urn, U, has some mix of balls of the same colors. Define some lotteries from the U urn, such as "you get $100 if blue comes out, otherwise $0 if any other color comes out" or "you get $100 if blue or red comes out, otherwise $0 if any other color comes out." Then elicit certainty-equivalents for these bets.

Now write out some models to describe behavior. For the K urn, which we call risk, and restricting to two prizes, X and x, for X>x, we have

$$w_K(p)\ u_K(X) + [\ 1 - w_K(p)\ ]\ u_K(x) \tag{1}$$

for some objective probability p of the bet being true and the subject earning X. We then assume

[1] These estimation procedures are defended by Wakker [2010; Appendix A], so this is not just an inadvertent slip.

[2] If there is even the slightest concern by the subject that the experimenter might be manipulating the unknown urn strategically to reduce payouts, the Ellsberg paradox is explained: see Kadane [1992] and Schneeweis [1973]. This is why one should never rely on computer-generated realizations of random processes in behavioral research if at all possible. The experiment in ABPW was conducted entirely on a computer.

some specific functional forms for the probability weighting functions and utility functions, and estimate those parameters. For the U urn, which we call uncertainty, we propose

$$w_U(\pi)\ u_U(X) + [\ 1 - w_U(\pi)\ ]\ u_U(x) \tag{2}$$

for some subjective probability π of the bet being true and the subject earning X. So in the general models shown here the probability weighting function *and* the utility function are source-dependent. This is the model that ABPW propose: source dependence in both utility and probability weighting functions, which seems reasonable to test.

On the basis of *a priori* reasoning, some have suggested instead that we only have source-dependence in the probability weighting function, so we would have

$$w_K(p)\ u(X) + [\ 1 - w_K(p)\ ]\ u(x) \tag{1′}$$

$$w_U(\pi)\ u(X) + [\ 1 - w_U(\pi)\ ]\ u(x) \tag{2′}$$

Of course this is a testable restriction of the general model to $u_K(z) = u_U(z)$ for $z \in \{X, x\}$. There is an obvious, symmetric special case in which we only have source-dependence in the utility function:

$$w(p)\ u_K(X) + [\ 1 - w(p)\ ]\ u_K(x) \tag{1″}$$

$$w(\pi)\ u_U(X) + [\ 1 - w(\pi)\ ]\ u_U(x) \tag{2″}$$

Again this is a testable restriction of the general model to $w_K(p) = w_U(\pi)$ for $p=\pi$. Indeed, it is the alternative hypothesis offered by Vernon Smith [1969] in a comment on Ellsberg.

These models can be estimated using data generated from the "Ellsberg experiment" of ABPW. In this experiment each subject was asked to state certainty-equivalents for 32 bets based on the K urn, and 32 bets based on the U urn, generating 64 observations per subject. They propose a power utility function defined over prizes z normalized to lie between 0 and 1

$$u(z) = z^{\rho} \tag{3}$$

where the parameter ρ is allowed to take on different values depending on the source K or U. So if

S is defined to be a binary variable such that S=1 when the U process was used and S=0 when the K process was used, one estimates ρ_K and ρ_U in

$$\rho = \rho_K + \rho_U S \tag{4}$$

and then there is an obvious hypothesis test that $\rho_U = 0$ in order to test for source independence with respect to the utility function.

The probability weighting function is due to Prelec [1998], and exhibits considerable flexibility:

$$w(p) = \exp\{-\beta(-\ln p^{\alpha}\}, \tag{5}$$

where w(p) is defined for $0<p<1$, and $\beta>0$ and $0<\alpha<1$ for choices from the K process. The same function $w(\pi)$ can be defined for the choices from the U process. It is similarly possible to estimate linear functions of the structural parameters α and β to test for source-independence:

$$\alpha = \alpha_K + \alpha_U S \tag{6}$$

$$\beta = \beta_K + \beta_U S \tag{7}$$

The obvious hypothesis test for source independence in probability weighting is that $\alpha_U=0$ *and* $\beta_U=0$.

The experimental data of ABPW can be used to estimate these structural parameters and undertake the hypothesis tests for source independence. Each of 66 subjects was presented with 32 tasks in which they were asked to indicate "switch points" between a bet on some outcome from drawing a ball from the urn and a certain amount of money. Half of the bets were based on draws from the K urn, and half from bets based on the U urn. The certainty-equivalents were ordered increments between 0€ and 25€, using 50 rows in a multiple price list elicitation. The end-result for each subjective lottery is a certain amount of money which is evaluated as being just less valuable than the lottery, and a certain amount of money which is evaluating as being just more valuable than the lottery. The switch point is enforced for the subject, and involves an increment of 0.5€.[3] Thus we have 64 binary lottery comparisons for each subject over 32 tasks.[4]

Each subject was told that one of the 32 tasks would be selected for payment, thereby incentivizing them to respond truthfully. Of course, this popular incentive mechanism presumes the validity of the (mixture) independence axiom, which is of course considered suspect in axiomatic derivations of the RDU model.[5] In effect, empirical RDU models that use this incentive mechanism require the analyst to entertain "bipolar" attitudes toward the independence axiom. They are pessimistic about it when the subject makes evaluations of each constituent lottery in a binary choice pair, but optimistic about it when the subject evaluates the overall compound lottery of payoffs across 32 tasks.

These binary comparisons can be used to generate maximum likelihood estimates of the structural parameters. Each comparison involves the "left" lottery

$$RDU_L = w_K(p)\ u_K(X) + [\ 1 - w_K(p)\ ]\ u_K(x)$$

or

$$RDU_L = w_U(\pi)\ u_U(X) + [\ 1 - w_U(\pi)\ ]\ u_U(x),$$

and the "right" lottery

$$RDU_R = u_K(Z)$$

for the certain amount Z.[6] The latent index

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[3] The use of an enforced switch points of this kind is studied in detail in Andersen, Harrison, Lau and Rutström [2006], and is referred to as the "sequential multiple price list" procedure.

[4] Each subject actually made 3,200 choices, since each of the 50 rows involved a binary choice. However, the choices either side of the switch point are correlated by design in the "sequential multiple price list" procedure, and contain no extra information. The results are qualitatively the same if one does include all choices, including the implied ones.

[5] The reference here to the *mixture* independence axiom follows Segal [1988][1990].

[6] An alternative specification would use $RDU_R = u_K(Z)$ for the certain amount when comparing to the risky lottery based on the K urn, and $RDU_R = u_U(Z)$ for the certain amount when comparing to the risky

$$\nabla RDU = RDU_R - RDU_L \tag{8}$$

can then be calculated. This latent index, based on latent preferences, is then linked to observed choices using a standard cumulative normal distribution function $\Phi(\nabla RDU)$. This "probit" function takes any argument between $\pm\infty$ and transforms it into a number between 0 and 1 using an increasing function. Thus we have the probit link function,

$$\text{prob(choose lottery R)} = \Phi(\nabla EU) \tag{9}$$

In addition, we assume a behavioral error specification, due originally to Fechner and popularized by Hey and Orme [1994]. This error specification posits the latent index

$$\nabla RDU = (RDU_R - RDU_L)/\mu \tag{8$'$}$$

instead of (8). An important, recent contribution to the characterization of behavioral errors is the "contextual error" specification proposed by Wilcox [2011]. It is designed to allow robust inferences about the primitive "more stochastically risk averse than," and avoids many potential specification problems. It posits the latent index

$$\nabla RDU = ((RDU_R - RDU_L)/\nu)/\mu \tag{8$''$}$$

instead of (3′), where ν is a new, normalizing term for each lottery pair L and R. The normalizing term ν is defined as the maximum utility over all prizes in this lottery pair minus the minimum utility over all prizes in this lottery pair. The value of ν varies, in principle, from lottery choice to lottery choice: hence it is said to be "contextual." For the Fechner specification, dividing by ν ensures that the *normalized* RDU difference $[(RDU_R - RDU_L)/\nu]$ remains in the unit interval.

Thus the likelihood of the observed responses, conditional on these specifications being true, depends on the estimates of ρ, α, β and μ given the above statistical specification and the

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lottery based on the U urn. In effect, this specification assumes that the source-dependence is "contextual" and defined by the choice context. Table A1 collates comparable results to those reported in Table 1 using this specification, and it makes no difference to the qualitative conclusions.

observed choices. The conditional log-likelihood is then

$$\ln L(\rho, \alpha, \beta, \mu; y, S) = \sum_i [(\ln \Phi(\nabla RDU) \times \mathbf{I}(y_i = 1)) + (\ln (1-\Phi(\nabla RDU)) \times \mathbf{I}(y_i = -1))] \quad (9)$$

where $\mathbf{I}(\cdot)$ is the indicator function, $y_i = 1(-1)$ denotes the choice of the Option R (L) lottery in choice task i, and S is the binary variable defined earlier to denote the U source.

Table 1 reports hypothesis tests and selected estimates from maximum likelihood estimation of this model.[7] Column 1 shows the ID number of the subject, columns 2 through 6 report *p*-values of hypothesis tests of source dependence, and columns 8 and 9 report the point estimate and standard error for the α_U parameter of the probability weighting function. As often happens with estimation at the level of the individual, numerical instability arises for some subjects: in the present case 13 of the 66 subjects were dropped due to the inability to estimate the model.

The *p*-values indicate striking evidence for source *independence*, and are sorted using the values on column 6. Those *p*-values less than 0.1, implying rejection of the null hypothesis of source independence, are shaded: there are very few shaded cells. Column 2 shows the *p*-values on the hypothesis test for the utility function, and the lowest three *p*-values are 0.14, 0.17 and 0.18 for subjects 18, 8 and 29, respectively. Columns 3 and 4 report *p*-values for each of the parameters of the probability weighting function, and column 5 and 6 report joint hypothesis tests. Only 4 subjects violate source independence with respect to the α and β parameters.

Columns 8 and 9 report the point estimates of the α_U parameter to illustrate a concern with the manner in which ABPW draw inferences from these data. For each subject they calculate the values of the parameters in two steps. First, they use non-linear least squares for ρ, $w_K(0.5)$ and $w_U(0.5)$ using choice tasks where one can *a priori* assume the value of $p = \pi = 0.5$. Thus they do not estimate the parameters α and β in this step, but directly estimate the decision weights. Second,

[7] Data and computer code to replicate these results is available at http://cear.gsu.edu/gwh/.

conditional on the point estimate for ρ, they calculate the values of α and β that minimize a quadratic distance metric using choice tasks for which one can *a priori* assume the value of p or π to be ⅛, ¼, ⅜, ⅝, ¾ or ⅞.

One immediate concern with this approach is that sample errors in the estimation of ρ in the first step are assumed away in the second step, likely resulting in an understatement of sample errors in the estimation of α and β. The fact that sample errors are not reported in the first step does not mean that they are zero. Indeed, it is common for all statistical packages to have non-linear least squares procedures with several ways of calculating standard errors. Another concern with this approach is that the estimates of $w_K(0.5)$ and $w_U(0.5)$ in the first step appear to play no role in constraining the estimates of α and β in the second step: for any values of α and β there is an implied value of $w_K(0.5)$ and $w_U(0.5)$, and these procedures do not respect that connection, which is a matter of theoretical consistency.

These problems are compounded when inferences are drawn solely on the vector of point estimates of some parameter, with no regard for possible sample errors. For example, ABPW conclude that utility is linear because the *median* values of the *point estimates* of ρ for the K and U processes are not statistically different from 1 using a sign test. As it happens, this conclusion is generally correct, but for a very different reason: the point estimates of ρ have very large sample errors. So the statistical result arises because of poor estimates, and does not arise because subjects fail to exhibit diminishing marginal utility.[8] The median point estimate from our maximum likelihood specification is 1.003, but the median standard error is 0.67 (and significantly different from zero, if one is can use this metric descriptively). This finding, of course, makes one particularly

[8] Of course, the latter claim may be true: with large sample errors one simply cannot say. The claim that utility functions are linear in RDU models flies in the face of a wide range of data collected from laboratory experiments on the matter, surveyed in Harrison and Rutström [2008].

concerned about the assumption that the standard error of ρ=0 when making inferences about the parameters of the probability weighting, α and β.[9]

The key finding from ABPW is that there is source dependence in the probability weighting function. They first examine the median of the *point estimates* of α and β, noting that in their estimations $(\alpha_K+\alpha_U) < \alpha_K < 1$ and $(\beta_K+\beta_U) \approx \beta_U$ for these median values and our notation.[10] Of course, without any sense of the precision of these estimates of median values, they have no inferential value whatsoever. In fact, columns 3, 4 and 5 confirm that these inferences from the median point estimates are generally invalid.

ABPW next examine the values of two indices that are intended to convey the "insensitivity" and "pessimism" of the probability weighting function. These are derived from ordinary least squares approximations of the probability weighting function evaluated at the point estimates of α and β. They conclude that these two indices are significantly different for the K and U processes, but of course this is an approximation based solely on point estimates. The same hypothesis test can be undertaken directly, and more powerfully, by examining the α and β parameters themselves for each process and individual. Table 1 does precisely that, recognizing the standard errors in these estimates, and the conclusion is apparent.

The need to pay attention to the precision of estimates is so important, in terms of the way in which empirical inference in behavioral economics seems to be progressing, as to warrant an alternative demonstration. Consider the α parameter. Table 1 lists the point estimate and standard

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[9] ABPW also use inferences based on the experiment with objective probabilities to constrain their experimental design with subjective probabilities, specifically the inference that there was no source dependence in utility. Although we agree with that inference, for rather different statistical reasons, it is perilous to build experimental designs in the domain of subjective probability that rely on such maintained behavioral assumptions that are inferred from the domain of objective probability.

[10] In which the subscript U denotes the deviation from the estimate with the subscript K.

error of the α_U coefficient for each subject, where $\alpha_U = 0$ is consistent with source independence with respect to this parameter. If one tests whether the point estimates of α_U in column 7 of Table are significantly different from 0, one would conclude that they are. A two-sided sign test has a *p*-value of 0.027, and a two-sided *t*-test has a *p*-value of 0.0001. But these tests completely ignore the imprecision of these estimates, shown in column 8. If one just looks at the standard errors, it is apparent that these point estimates are not precisely estimated. The *p*-values in column 3 do formally and properly what these sign tests and *t*-tests do incorrectly, and the difference in conclusions is dramatic.

Of course, any failure to reject a null hypothesis could be an artefact of sample sizes being too small. True, but that provides no rationale for ignoring sample errors. Indeed, this point is so obvious that it leads one to question any statistical methodology that could claim to draw inferences from samples that were arbitrarily small. What if the sample of ABPW had been 6 instead of 66? They could still have drawn their conclusions, based on looking at the median values of individual estimates, despite the patent imprecision that N=6 should alert anyone to. The use of a sample of N=66 just confers this method with an illusion of statistical, large-sample validity.

ABPW (p. 704) note that

> We also analyzed our data using probabilistic choice-error theories and econometric maximum likelihood estimations. The results [...] all agree with the results reported here. All estimations of utilities and weighting functions were done at the individual level.

In fact, the *maximum likelihood* estimation referred to here was only undertaken with the pooled sample, and not at the level of the individual. The individual "estimations" were only undertaken with the least squares and quadratic distance procedures.

In conclusion, the evidence for source dependence is missing. This does not mean that the behavioral phenomenon is missing. Indeed, it is intuitively plausible once one moves to the domain of subjective probabilities, or where objective probabilities are presumed to arise from some inferential process.[11] But we should not mistake our intuition for the evidence, as comforting as that might be.

[11] For example, by the application of Bayes Rule or the reduction of compound lotteries.

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Table 1: Maximum Likelihood Estimates

Subject	*p*-value on tests of source-dependence					Point Estimate of α_U	Standard Error on Estimate of α_U
	ρ_U	α_U	β_U	α_U and β_U	ρ_U, α_U and β_U		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
53	0.59	0.01	0	0	0	-0.494	0.19
57	0.99	0.07	0.01	0	0	-0.301	0.17
29	0.18	0.03	0	0	0.01	-0.182	0.08
6	0.46	0.39	0	0.02	0.04	-0.232	0.27
71	0.92	0.48	0.39	0.38	0.22	-0.428	0.61
8	0.17	0.4	0.1	0.22	0.22	-0.413	0.49
50	0.37	0.23	0.61	0.23	0.23	-0.68	0.57
61	0.69	0.88	0.16	0.33	0.28	-0.069	0.44
18	0.14	0.44	0.19	0.27	0.36	0.223	0.29
7	0.64	0.27	0.15	0.3	0.5	0.43	0.39
45	0.53	0.54	0.37	0.61	0.51	-0.509	0.84
46	0.66	0.23	0.25	0.38	0.52	-0.54	0.45
55	0.43	0.9	0.18	0.41	0.56	0.067	0.51
34	0.83	0.61	0.8	0.38	0.58	0.387	0.75
5	0.95	0.86	0.3	0.58	0.63	0.059	0.33
10	0.61	0.62	0.5	0.75	0.69	0.222	0.45
3	0.55	0.3	0.54	0.49	0.7	-0.459	0.44
59	0.89	0.9	0.4	0.68	0.73	0.095	0.75
20	0.51	0.39	0.46	0.65	0.73	-0.615	0.71
40	0.62	0.87	0.28	0.54	0.74	0.073	0.46
48	0.56	0.53	0.52	0.53	0.74	-0.232	0.37
66	0.83	0.98	0.32	0.55	0.75	-0.006	0.26
38	0.41	0.65	0.33	0.62	0.75	-0.309	0.68
26	0.49	0.68	0.36	0.64	0.75	-0.279	0.68
60	0.69	0.55	0.34	0.6	0.77	-0.361	0.6
27	0.65	0.93	0.37	0.6	0.78	0.065	0.71
16	0.6	0.93	0.36	0.62	0.81	0.033	0.4
11	0.88	0.92	0.51	0.78	0.81	-0.053	0.51
24	0.65	0.9	0.62	0.86	0.87	0.126	1
39	0.42	0.95	0.86	0.98	0.88	0.048	0.72
17	0.54	0.91	0.88	0.99	0.9	-0.153	1.36
23	0.86	0.58	0.93	0.85	0.9	-0.561	1
54	0.66	0.73	0.91	0.79	0.92	-0.333	0.96
31	1	0.96	0.64	0.9	0.93	-0.095	1.9
28	0.89	0.91	0.53	0.82	0.93	0.034	0.31
2	0.86	0.52	0.76	0.81	0.93	-0.292	0.45
64	0.59	0.85	0.8	0.95	0.93	-0.271	1.41

33	0.8	0.78	0.77	0.91	0.94	-0.329	1.18
15	0.8	0.96	0.55	0.83	0.94	0.033	0.75
44	0.62	0.97	0.88	0.98	0.94	-0.03	0.88
80	0.77	0.69	0.82	0.87	0.96	-0.442	1.11
42	0.82	0.85	0.85	0.9	0.98	-0.41	2.12
62	0.8	0.91	0.84	0.98	0.98	-0.331	2.9
30	0.87	0.96	0.7	0.93	0.99	0.155	2.8
43	0.98	0.99	0.74	0.94	0.99	0.021	1.65
9	0.89	0.82	0.87	0.94	0.99	-0.403	1.79
47	0.97	0.93	0.77	0.96	0.99	0.151	1.63
13	0.89	0.91	0.82	0.96	0.99	-0.083	0.7
106	1	0.91	0.88	0.98	1	-0.393	3.49
35	0.9	0.94	0.92	0.99	1	-0.097	1.22
49	1	0.98	0.87	0.99	1	-0.093	3.93
19	0.96	0.91	0.91	0.98	1	0.38	3.19
41	0.88	0.95	0.96	1	1	-0.142	2.24

Table A1: Maximum Likelihood Estimates Under Alternative Specification

Subject	*p*-value on tests of source-dependence: ρ_U	α_U	β_U	α_U and β_U	ρ_U, α_U and β_U	Point Estimate of α_U	Standard Error on Estimate of α_U
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
53	0.63	0.01	0	0	0	-0.506	0.18
57	0.62	0.06	0.21	0.04	0	-0.305	0.16
6	0.21	0.34	0.92	0.62	0.01	-0.21	0.22
29	0.47	0.05	0.74	0.11	0.03	-0.186	0.09
71	0.66	0.44	0.61	0.5	0.12	-0.447	0.57
59	0.03	0.79	0.22	0.13	0.14	0.069	0.26
61	0.78	0.88	0.49	0.68	0.22	-0.077	0.49
7	0.87	0.29	0.57	0.57	0.51	0.452	0.43
45	0.99	0.56	0.67	0.6	0.61	-0.665	1.14
34	0.45	0.66	0.52	0.75	0.62	0.25	0.57
5	0.79	0.85	0.71	0.93	0.62	0.06	0.32
3	0.47	0.24	0.6	0.51	0.63	-0.507	0.44
46	0.97	0.27	0.68	0.52	0.66	-0.606	0.55
8	0.48	0.56	0.75	0.7	0.67	-0.394	0.67
24	0.41	0.84	0.28	0.5	0.68	0.164	0.81
40	0.55	0.93	0.89	0.98	0.71	0.041	0.46
47	0.31	0.64	0.53	0.82	0.73	-0.431	0.91
11	0.58	0.99	0.8	0.97	0.76	-0.006	0.41
55	0.87	0.85	0.71	0.93	0.77	0.154	0.8
66	0.96	0.97	0.72	0.94	0.78	-0.008	0.26
16	0.57	0.93	0.8	0.97	0.8	0.031	0.38
27	0.99	0.87	0.64	0.82	0.81	0.156	0.95
10	0.98	0.72	0.68	0.9	0.83	0.176	0.49
60	1	0.5	0.73	0.79	0.86	-0.423	0.63
39	0.42	0.79	0.42	0.72	0.88	0.175	0.67
48	0.97	0.65	0.86	0.86	0.88	-0.202	0.45
23	0.84	0.54	0.9	0.79	0.89	-0.726	1.19
15	0.66	0.92	0.89	0.99	0.9	-0.08	0.75
28	0.73	0.99	0.97	1	0.91	0.004	0.32
20	0.85	0.48	0.96	0.77	0.91	-0.617	0.87
26	0.81	0.68	0.99	0.9	0.92	-0.347	0.83
18	0.86	0.78	0.99	0.96	0.92	0.116	0.41
33	0.76	0.8	0.66	0.84	0.93	-0.284	1.14
44	0.57	0.96	0.53	0.81	0.93	-0.04	0.74
17	0.67	0.98	0.58	0.82	0.94	-0.05	1.68
2	0.97	0.54	0.81	0.83	0.94	-0.283	0.46
38	0.94	0.68	0.87	0.92	0.95	-0.413	0.99

80	0.72	0.65	0.77	0.89	0.95	-0.487	1.08
50	0.82	0.71	0.74	0.92	0.97	-0.407	1.11
106	0.73	0.97	0.69	0.92	0.98	-0.146	3.38
42	0.92	0.87	0.99	0.98	0.98	-0.422	2.5
64	0.72	0.93	0.75	0.94	0.99	0.18	2.15
43	0.98	1	0.87	0.98	0.99	0.009	1.6
54	0.81	0.79	0.8	0.95	0.99	-0.369	1.39
62	0.95	0.9	0.82	0.96	0.99	-0.404	3.16
9	0.96	0.84	0.99	0.98	0.99	-0.388	1.92
30	0.94	0.98	0.85	0.98	0.99	-0.086	2.76
13	0.94	0.92	1	0.99	1	-0.081	0.82
35	0.89	0.93	0.85	0.98	1	-0.104	1.26
49	1	0.98	0.92	0.99	1	-0.089	3.92
19	0.94	0.92	0.98	1	1	0.287	3.01
41	0.93	1	0.94	1	1	0.009	2.38