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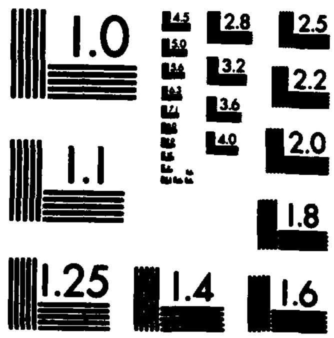

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Steps Toward an Empirical Evaluation of Robust Regression Applied to Reaction-Time Data[1]

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Abstract

Current statistical theory provides little useful guidance about how to reduce the sensitivity of analyses of reaction-time data to aberrant observations and violations of statistical assumptions, either because robust methods, which are very inviting, are insufficiently understood, or because aspects of the data are not characterized fully enough to permit devising theoretically justifiable analyses. In this report we suggest an empirical approach, in which one applies the same criteria to the problem of selecting a statistical method as those that one uses to select among alternative experimental procedures. We define six such criteria, and then describe five tests based on a set of about 36,000 observations in which we compare ordinary least-squares multiple regression as a way of characterizing the data with Huber's robust iteratively reweighted least-squares method. Results favor the robust method.

1. Introduction

During the past two decades there has been a vast growth in the use of reaction-time methods in psychological research, largely because psychologists have recognized that the analysis of reaction-time data can lead to powerful inferences about the structure of mental processes. However, the problem of contamination of RT distributions by aberrant observations is one that has not been solved either experimentally or analytically, and as inferences become more subtle, even small distortions in the data take on added importance.

To our knowledge the properties that have been established for all of the existing statistical methods of outlier elimination depend on assumptions that are either known to be false or are difficult to validate in most reaction-time data. (We assume that subjective methods of data trimming, although used, should be regarded as temporary expedients only, and we ignore those fortunate situations in which secondary concomitant observations are available to serve as a basis for data exclusion.) One commonly required property, for example, is that the form of the distribution is the same for the observations in each cell of an experimental design; that is, distributions associated with different cells differ at most in scale and location parameters. *A priori* this is unlikely to be true, given the factors whose levels typically vary from cell to cell and what we know about their effects on RT, and empirically the typical sample sizes per cell needed to validate it are difficult to achieve. Other unpalatable assumptions that are sometimes required include symmetry and sometimes normality of either the underlying distribution or the contamination distribution; both of these are likely to be false for reaction-time data, which are typically skewed toward large values.

Methods that trim data or weight them differentially have often been shown to lead to parameter estimates that are more efficient (smaller mean squared error) than others, but these estimates may be biased in small samples to an extent that varies in unknown ways across levels of experimental factors, or across "conditions". Moreover, bias is especially troublesome relative to inefficiency when precise quantitative models are being tested, or when quantitative aspects of the data, such as additivity of effects in factorial designs or linearity of the effects of quantitative factors, are of interest. The importance of additivity and linearity also argues against the use of nonlinear transformations of reaction times, which is sometimes proposed as a way to increase the likelihood that the data satisfy assumptions such as those mentioned above and to reduce the effects of skewness and contamination.[2]

In their book on applied regression analysis, Draper and Smith (1981, p. 344) express a conservative and idealistic view about the use of robust regression: "We believe use of robust regression methods is inadvisable at the present time, unless rules for deciding *which* robust method to use in *which* circumstances have been formulated and proved effective. If the model (which includes the assumptions about the error distribution) is wrong, the appropriate action to take is to change the model and use maximum likelihood estimation on the new model, *not* to change the method of estimation."

The principal difficulty with the approach advocated by Draper and Smith is that the effort needed in the context of each experimental problem to collect and analyze the data required to carry it out is considerable, and unlikely to be attempted by any but the most patient and courageous scientists.

The experimentalist who uses reaction-time measures, then, is in a quandary, concerned on the one hand that contamination and other assumption failures might influence conclusions based on conventional (ordinary least squares) methods (which are well-understood theoretically), but afraid on the other hand that the techniques available to limit the effects of such contamination (which are less well understood theoretically) may introduce other difficulties.

2. An Empirical Approach

One alternative to choosing a method arbitrarily and also to engaging in the theoretical analysis advocated by Draper and Smith is the empirical approach that we consider in the present report. It involves choosing a data analysis method in the same way as we choose an experimental procedure. Roughly speaking, we propose that a legitimate acceptance criterion for either an

2. Additivity of factor effects is important in relation to the idea that some mental processes are organized in stages that can be selectively influenced by experimental factors; linearity arises if a mental process includes a sequence of operations whose number reflects the number of task components to be accomplished, and whose mean duration is independent of this number. An example of additivity that pervades many reaction-time studies involves the individual differences in mean RT in experiments in which sensory, perceptual, or mnemonic factors are manipulated; a substantial part of this variation appears to be associated with decision or motor processes that are not systematically influenced by these factors, processes that may occur after the completion of those operations that such factors do influence. Insofar as such effects when measured in units of physical time are indeed additive, such that subject effects can be "removed" as in the analysis of variance, any nonlinear transformation would render them non-removable, and they would instead enter into the effects of interest, biasing the estimated mean values of such effects, and making them appear more subject to individual differences than they "really" are.

experimental procedure or an analysis method is whether the results it produces tend to be orderly and to make sense.

Whether a particular description or characterization of a set of data "makes sense" can probably be decided only years after the data have been collected; "making sense" here means being consistent with related findings and relevant theory. Orderliness, however, seems to be a quality that we can judge reasonably well without using a distant vantage point. For this purpose, any measure of order applied to the data description produced by a particular method must, of course, be known not to have been *forced* on the data by the method. Some examples should help to clarify our notion of order:

2.1 Invariance of the estimated effects of one factor (Factor 2) over levels of another (Factor 1)

Suppose that separate analyses are conducted for each level of an experimental factor (Factor 1). Then insofar as the estimated effect of a second factor (Factor 2) is closer to being identical in each of these analyses (i.e., the effects of the two factors are additive), and there is no reason to expect an interaction, that analysis method is to be preferred. The measure of similarity (or divergence) of the effects of Factor 2 across levels of Factor 1 should probably include an adjustment for difference in mean effect size (e.g., the mean effect of Factor 2 over levels of Factor 1) across methods. There are two senses in which the estimated effects of Factor 2 might vary in similarity across levels of Factor 1: they might differ systematically, or nonsystematically. In both cases the argument is based on a principle of parsimony, or the assumption that "nature" or "truth" is simple. Results indicating less nonsystematic variation are therefore closer to the "truth," as are results that indicate invariance rather than systematic effects.

2.2 Minimum variability over individual subjects

Suppose that a separate analysis is used for each subject's data, and we derive a measure of the effect of some experimental factor on performance. Suppose further that the analysis imposes no constraint on the size of this effect. Then the results are more orderly insofar as the variation across subjects in the size of the effect is smaller. (As the measure of variability it may be appropriate to use a coefficient of variation — i.e., dispersion as a proportion of mean effect size — to compensate for any differences in scale associated with different methods.) Note that this criterion can be regarded as an instance of the first, where Factor 1 now represents a "random effect" associated with subjects. The argument here depends on the idea that the measured variation reflects a combination of "true" variability across subjects plus "nontrue" variability that derives from measurement error or other sources of "random" influences. Since the analysis places no constraint on the estimated size of the effect, and separate analyses are performed for separate subjects, a method that produces less variation in effect size must be less sensitive to these other sources, and must thus give values closer to the "truth".

2.3 Minimum residual variability

More generally, any indication that a method produces results that are less sensitive to random variation (relative to true variation) should cause us to favor that method.

2.4 Linearity of the effects of quantitative factors

Given that the methods under consideration do not force linearity on the effect of a quantitative factor, the method that produces the best linearity is to be preferred. The argument is again based on a principle of parsimony, as in the case of the first criterion, together with the idea that except for a null effect of a factor, the simplest effect is a linear one. Because the form of a functional relationship depends on the scales of measurement of both the factor and the response, this criterion can only be applied if there is some basis for choosing a measurement scale in each case. Note that linearity can be regarded as another instance of invariance, or additivity: the effect of a quantitative factor is linear insofar as the effect of a fixed-size increment in the factor is invariant across different initial levels.

2.5 Similarity of small-sample and large-sample characterisations of data

Suppose that data are partitioned, by subject for example, or by level of a treatment factor, and we perform separate analyses of the data subsets as well as analysis of their union. Then that method is better whose characterizations of the subsets are more similar to its characterization of the union. Because this property will generally be associated with the characterizations of the subsets being similar to *each other*, it can be regarded as another instance of the first.

2.6 Clarity of choice among models

We often attempt to use our data to select among alternative theories. In the present context a theory is tested by representing it as a regression model, fitting it to the data, and measuring goodness of fit. The theory that corresponds to the best-fitting model is then selected. A method is good insofar as this test leads to the same conclusion for different data sets.

In the sections that follow we discuss the background for tests using the first, third, fourth, and sixth of the above criteria that we have applied in a comparison of a particular robust regression method with ordinary least squares.

3. Source of the data used for these tests: Experiments on the transformation of short-term visual memory assessed by retrieval-time measurement

The data to which we applied the comparative analysis were generated in an experiment in which we measured the time to name a specified digit in a horizontal row of from 2 to 6 briefly-displayed digits. The target was specified by means of a "marker" stimulus that denoted its spatial location; In different blocks of trials we presented the marker at one of six different times relative to the onset of the array display, ranging from 350 msec before to 650 msec after the onset, and denoted -350 msec, . . . , +650 msec. In some blocks of trials the marker was visual: a 50-msec display of a two vertical line-segments, one above and one below the target's location. In other trial blocks the marker was tactile: a 50-msec vibration applied to one of six fingers; subjects had learned the correspondence between six fingertips and the six possible positions that defined the display area and that could be occupied by digits. Combination of the six marker delays and two marker modalities defined twelve conditions, in each of which we could examine the effect of array size on mean reaction-time. Each of six subjects had 14 hours of practice, followed by 22 hours of testing; the result was a data set for each condition and each subject that contained about 500 observations. Each subject provided us with twelve such data sets; the experiment therefore produced 72 such data sets, which we treated separately in our regression analyses.

At the start of a trial, subjects fixated in the center of the six-position display area. To avoid confounding the principal experimental factor — the number of displayed elements (*array size*) — with their separation, we placed elements in *contiguous* locations. To reduce (but not eliminate) the confounding of array size with retinal eccentricity of the possible target elements (and hence of the possible marker locations), we placed the arrays at all possible positions within the display area: An array of size s could be placed in any of $7 - s$ such positions.

In assessing the effect of array size (our principal goal) one has to consider a number of other factors that are also known to influence performance. The reasons they must be considered explicitly are twofold: In some cases we select their levels randomly because the experiment is too small to permit complete balancing; in other cases an orthogonal design is impossible and there is some degree of inherent confounding. Indeed, it is for these reasons that an explicit multiple regression method is necessary, rather than a more standard analysis suitable for multiway tables. [3]

The other main factors are as follows: First, there is the *serial position* of the target element within the array. Because the number of serial positions changes from one array size to another, and because we cannot specify a correspondence between positions in arrays that differ in size, the serial position effect is regarded as a separate effect for each array size. Second, there is the position of the target (and hence of the marker) within the display area: its *absolute position*. Our findings have forced our model of the absolute position effect to be somewhat complicated: the effect seems to be systematically smaller for targets that are the leftmost or rightmost elements of an array — end elements — than for interior elements. This generates a complicated interaction between serial position and absolute position, which is embedded in the multiple regression model that we fit to the data. The third main factor is the *identity of the target element* (one of the ten digits, in the present experiment) that must be identified and named. Some target elements are associated with longer reaction times than others, possibly because of identification-time differences, or differences in naming latency given the identity, or differences in measurement delay of our speech-onset detector possibly related to the initial sound of the spoken name.

In other experiments using the present paradigm the effect of array size on mean reaction-time has been closely approximated by a linear function, and this has been the case for all the delays studied. The aspect of our findings that we regard of most importance is the change in the parameters of this linear function as the marker is delayed: When the marker shortly precedes or is simultaneous with the array, the slope of the linear function is very close to zero: i.e., there is essentially no effect of array size. As the marker is delayed, the slope grows systematically, reaching what appears to be an asymptote at a delay of about a second; most of the change has occurred within about two thirds of a second. Together with results from other paradigms that we have used, these findings indicate to us the existence of a rapid and dramatic transformation of the internal representation of the visual display, such that the initial representation manifests a property of direct access by spatial location, and that this property is eliminated as the transformation proceeds.

For a synopsis of earlier experiments using a visual marker, as well as the present experiment in which tactile and visual markers are compared, see Sternberg, Knoll, & Turock, 1985. For a detailed account of findings from the

3. Note, however, that robust alternatives to the analysis of orthogonal experiments are also of great interest, and could be assessed in ways similar to those exemplified in the present report.

earlier experiments, as well as a summary of results from three other paradigms, also aimed at investigating the first second of visual memory, see Turock, 1985, and Sternberg, Knoll, & Turock, 1986.

One aim of our analyses, then, is to permit an assessment of linearity and, given linearity, to characterize the obtained linear functions by intercept and slope parameters.

4. The Regression Model

The regression model can be expressed as follows, where $\overline{RT}$ denotes mean reaction-time:

$$\overline{RT} = \mu + \alpha_s + \beta_{si} + \gamma_p + \delta_d.$$

The $\{\alpha_s\}$, which represent the array-size effect, are defined for s = 2, 3, 4, 5, and 6. They are constrained to sum to zero, and thus reflect 4 degrees of freedom (*df*). The $\{\beta_{si}\}$, which represent the serial-position effect, are defined for each s, and for i = 1, 2, . . , s; for each s-value they are constrained to sum to zero, and thus reflect 15 *df*. The $\{\gamma_p\}$, represent the two assumed absolute-position effects, one for end elements, defined for $p = 1, 2, 3, 4, 5$, and 6, and constrained to sum to zero, and the other for interior elements, defined for $p = 2', 3', 4'$, and $5'$, and constrained separately to sum to zero. The absolute-position effects therefore reflect 8 *df*. Finally, the $\{\delta_d\}$ represent the element-identity effect and are defined for $d = 0, 1, \ldots, 8$, and 9, and constrained to sum to zero, thus reflecting 9 *df*. The number of degrees of freedom in the model being fitted to the data is therefore 39.

5. The two regression methods

We applied each regression method to each of the 72 data sets discussed above. The vector of observations in each data set contained about 500 entries, while the parameter vector contained 39. Both regression methods are available as functions within the S language (Becker & Chambers, 1984). The first method was ordinary least squares regression, called **reg** within S. The second method was robust regression, or iteratively reweighted least squares, and called **rreg** within S. We used the variant of robust regression that embodied the Huber weighting function with constant 1.345. For readers unfamiliar with this method we provide a brief description. (For details see Coleman, Holland, Kaden, Klema, & Peters, 1980.)

Suppose we have a set of parameter estimates; the first such set might be obtained by ordinary least-squares regression; later sets are obtained by iteration. Let r_k be the set of residuals obtained by fitting the model with a particular set of parameter estimates to our data set (of approximately 500 observations). Define u as a scale parameter given by the median absolute residual divided by 0.6745. Now define a weight, w_i, as follows:

$$\text{if } |r_k| \leq 1.345u, \text{ then } w_k = 1;$$

$$\text{if } |r_k| > 1.345u, \text{ then } w_k = \left\{\frac{1.345u}{|r_k|}\right\}^{1/2}.$$

Thus, all observations contribute to the solution, and observations that differ from the model prediction based on the last set of parameter estimates by no more than 1.345 estimated scale units are given unit weight, whereas observations that differ by more than that amount are increasingly downweighted The iterated set of parameter estimates is now obtained by minimizing the sum of squared *weighted* residuals, where each residual r_k is weighted by w_k. The iteration continues until a criterion of convergence is met.

6. First test: Extent of invariance across probe delays in the effect of target identity

The values of the parameters $\{\delta_d\}$ represent the effect of target identity, and do in fact differ reliably from digit to digit. To convey the magnitude of this effect, we determined the mean value of each of the ten parameter estimates over the six delays for each subject and each modality, and determined the range of the resulting ten means. We then determined the mean of this range over the six subjects for each modality, with the results shown separately in Table 1 for ordinary and robust regression.

Table 1
Range over Target Elements of Mean Target-Element Parameter δ_d.
(Mean taken over six delays.)

Modality:	Visual		Tactile	
Regression:	Ordinary	Robust	Ordinary	Robust
Subject:				
1	59.1	62.7	69.4	69.9
2	47.4	41.2	65.7	54.2
3	65.7	59.7	78.8	80.9
4	37.7	34.5	79.0	74.6
5	63.5	67.0	74.4	75.8
6	53.6	56.1	55.1	55.0
Mean Range:	54.5	53.5	70.4	68.4

The range of target-element parameters over target elements is clearly affected to a negligible extent, if at all, by the choice of regression method. This means that variability estimates can be compared across methods without being adjusted for scale differences. Note, however, that there is a clear difference between ranges for the two modalities. We believe that this reflects greater unreliability of the data, and therefore of estimates based on those data, for tactile than visual probes; evidence that favors this interpretation will be presented below.

Now we proceed to the test of invariance of digit effects over probe delays. Such invariance is, of course, an example of a criterion of the first type discussed in Section 2, and we need to consider why we might either expect or not expect an interaction. During the time interval between the probe and detection of the subject's response there seem to be at least four processes that are plausible loci of target-identity effects. Recall that the identity of the target specifies the (correct) spoken response (its name). The first plausible locus is in the process of deriving the identity of the target from its internal representation. The second is in the process of "computing" and preparing the vocalization of the name from the derived identity. The third is initiating the vocalization. The fourth locus is external to the subject, but indubitably a contributor to the measured reaction time: the process in our hardware and software of detecting the onset of speech. Because speech-onset detectors are imperfect, responding with different speeds to different initial sounds, this process shares the property of the other three of being a potential locus of a target-identity effect.

Once the identity has been derived there appears to be no basis for any interaction between marker delay and target identity: we would expect any target identity effects in the second, third, and fourth of the processes just mentioned to be invariant across delays.

We now turn to the first of these hypothesized processes. Here there would seem to be a possible source of an interaction between marker delay and target identity, as well as of a marker-identity effect. The state of the representation from which the identity is derived presumably depends on marker delay. Suppose that R_1, a "raw" visual representation is transformed during the delay into R_2, perhaps a more abstract representation and one from which it takes less time to derive the identity. The transformation thus facilitates the process of deriving the identity. It is possible that the magnitude of this facilitation would be different for different targets; if so, the target-identity effect should change with — that is, interact with — probe delay.

This argument does not lead to a strong expectation of an interaction because the existence of a target-identity effect does not *require* that any of it be localized in this first process, which is the only plausible locus among the four processes mentioned for an interaction. Furthermore, as argued in Section 2, if such an interaction existed, there is no reason why it should be systematically obscured by the analysis since the regression method was applied independently to data from different marker delays. Hence if one method produces a closer approximation to invariance than another, it is to be preferred.

To assess the extent of invariance over probe delays in the parameter estimates provided by each regression method we determined, for each subject and modality, and for each of the ten target elements (digits), d, the variance over the six delays of the estimate $\hat{\delta}_d$. We then averaged these variances over target elements; results are displayed in Table 2. The range analysis (see Table 1) indicates that there is no need to scale the variances differentially for the two regression methods.

Table 2
Variance of target-element parameters over six probe delays ($msec^2$).
(Quantities shown are mean variances, where mean is taken
over target elements.)

Modality:	Visual		Tactile	
Regression:	Ordinary	Robust	Ordinary	Robust
Subject:				
1	314	154	248	187
2	403	292	591	344
3	330	278	751	474
4	437	180	990	390
5	231	153	757	312
6	432	246	727	279
Mean Variance:	358	217	677	331

Note first that in all of the twelve comparisons, of which there are two per subject, robust regression provides a reduction in variance, such that the mean variances over subjects reveal a 39% reduction for visual markers, and a 51% reduction for tactile markers. The difference between regression methods is statistically significant ($p < .01$) for both marker modalities. This finding argues strongly in favor of the robust method.

7. Second test: Extent of invariance across probe modalities in the effect of target identity

As discussed above, we can conceive of a possible basis for a systematic effect of the *delay* of the probe on target-element parameters. On the other hand, we can think of no reason to expect differences between target digits to depend on the *modality* of the probe. Indeed, the remarkable similarity in the effect of delay on the slope of the function relating mean reaction-time to array size is consistent with the idea that once the critical location has been ascertained on the basis of information provided by the probe, the remaining processes, leading up to identifying and orally naming the element in that critical location, are the same, regardless of the modality by which the location information was conveyed. It follows that we expect to find invariance of the target-element parameters over modalities, and that insofar as a method reveals greater invariance, it is to be preferred.

To assess the extent of the invariance over modalities we proceeded as follows: For each target element and each delay we determined the variance of the (two) target-element parameters over modalities, by calculating half of their squared difference. For each subject and each regression type we then averaged this quantity over the six delays and ten target elements. The results are displayed in Table 3.

Table 3
Variance of target-element parameters over two modalities ($msec^2$). (Quantities shown are mean variances, where mean is taken over target elements and delays.)

Regression:	Ordinary	Robust
Subject:		
1	247.8	122.8
2	540.1	364.7
3	720.1	436.5
4	930.9	343.7
5	417.6	166.9
6	573.6	224.7
Mean Variance:	571.7	276.6

In all six comparisons, robust regression provides a reduction in variance, with a mean ratio of 0.49 ± 0.05. Like the finding in the section above, this additional application of the first criterion discussed in Section 2 argues strongly in favor of the robust method.

We have thus found that robust regression produces estimates of target-element parameters that are more invariant over both the delay and modality of the probe than does ordinary least squares regression. One possible "truth" that the robust method therefore brings us closer to is *full invariance* of these parameters relative to delay and modality: in that case any apparent failure of invariance would reflect nothing more than the influence of sampling error and contamination in the basic reaction-time data on the estimates of target-element parameters. A hint in this direction is the approximate equality of the variances displayed in Tables 2 and 3, within regression type.

To test the idea of full invariance we subjected the target-element parameters derived from each of the two regressions to an analysis of variance, with the factors *Subject, Modality, Delay,* and *Target Element*. To avoid effects on the analysis of the linear dependence among target-element parameters induced by the constraint that requires them to sum to one, we analyzed only the first nine of the ten parameters.

Values of F-statistics from analysis of the parameters derived from ordinary (robust) regression are as follows: In the first three F-tests, we evaluated a main effect by comparing it to its interaction with subjects. We obtained, for target element, $F(8, 40) = 9.53$ (10.61); for delay, $F(5, 25) = 0.90$ (1.33); and for modality, $F(1, 5) = 1.18$ (0.08). For the fourth F-test we compared the subject by target-element interaction to the four-way interaction, which we take as a residual mean square; the result is $F(5, 200) = 3.54$ (6.87). Finally, the residual mean square for target-element parameters from ordinary (robust) regression is 543.4 (264.1).

These results generate two clear implications. First, the values of F for delay and modality confirm the conjecture of full invariance: the observed variation of parameters over delay and modality is small enough relative to the residual to be entirely due to random variation. This conclusion strengthens the inferences above that favor the method producing greater invariance. Second, the amount of this random variation is halved by moving from ordinary to robust regression, as measured by the residual mean square.[4] This result means that the third criterion of Section 2 also favors the robust method.

8. Third test: Deviation from linearity of the array-size effect

As mentioned in Section 3, we have found the relation of $\overline{RT}$ to array size for data averaged over groups of subjects to be remarkably well-approximated by a linear function at all marker delays, with a slope that is close to zero for negative or zero delays and that increases rapidly with positive marker delays. (Data from several experiments that support this generalization can be found in Sternberg, Knoll, & Turock, 1985.)

In applying the regression analyses under consideration to our data we wished to assess the goodness of fit of such a linear function, so we deliberately did not impose a linear constraint on the parameters $\{\alpha_s\}$ that represent the effect of array size. We are thus able to compare the linearity of the estimates of these parameters derived by means of the two regression methods. Such a

4. This reduction in noise is equivalent to doubling the size of the experiment which, for data collection alone, would have added about $7000 to the cost of the project.

comparison bears on two of the criteria discussed in Section 2: the fifth (methods are preferred for which small-sample and large-sample characterizations of data are similar), and the fourth (methods are preferred that produce better linearity).

We performed the comparison as follows: Each method provided a parameter set $\{\alpha_s\}$ for each of the 72 sets of data. We fitted a line to each set (by ordinary least squares regression), and extracted for each parameter set the residual mean square after fitting the line. The square root of this quantity is the *residual standard error (RSE)*. For each subject and regression method we determined the mean of the resulting RSE's over the six delays. The resulting measures of deviation from linearity are shown in Table 4 for each subject and marker modality, and for each type of regression.

Table 4
Measure of deviation from linearity of the array-size effect (*RSE* in *msec*), averaged over six probe delays.

Modality:	Visual		Tactile	
Regression:	Ordinary	Robust	Ordinary	Robust
Subject:				
1	7.33	9.83	12.17	10.83
2	10.33	10.50	21.00	16.17
3	11.83	9.83	21.83	19.17
4	12.50	10.83	22.67	13.17
5	9.17	7.17	16.67	14.33
6	7.50	9.67	22.17	11.67
Mean:	9.78	9.64	19.42	14.22

While there is virtually no difference between the means for visual markers, the mean RSE for tactile markers (considerably greater) is reduced for every subject as we move from ordinary to robust regression, with a mean reduction of 27%, and is brought closer to the RSE for visual markers. (Note, however, that even the results from robust regression differ significantly between modalities, with a mean difference of 4.59 ± 1.35 *msec*, so that although the robust method comes closer to the desired invariance, it is not achieved.) An analysis of variance applied to the mean RSE's showed that regression type, modality, and their interaction were all significant ($p < .05$); the effect of delay was also significant, but none of its interactions were.

Although the advantage of robust over ordinary regression in this comparison of array-size parameters is less dramatic than in the two comparisions based on target-element parameters, it is nonetheless present, providing us with independent but consistent conclusions.

9. Fourth test: Extent of invariance of the array-size effect across probe modalities

The mean array-size effect is estimated by the slope of a line fitted to the $\{\alpha_s\}$ parameters. Because the magnitude of this effect changes dramatically with delay, and because we have no principle to guide us in aligning the delays for probes in different modalities, it is hazardous to compare the effects of array size across modalities. Nonetheless, in the mean data it seems clear that by assuming that physically equal delays for the two probe modalities correspond psychologically we achieve remarkably equal slopes. (See Figure 12 in Sternberg, Knoll, & Turock, 1985.) Thus it seemed reasonable to expect invariance of the array-size effect, averaged over the six delays, across probe modalities, and thus to ask which type of regression produced a closer approximation to such invariance. Results of an analysis designed to provide an answer to this question are displayed in Table 5.

Table 5
Comparison of Array-Size Effect Across Modalities.
Quantities shown are slopes, B (in *msec/element*), of lines fitted to the $\{\alpha_s\}$ for visual (B_V) and tactile (B_T) probes (in *msec/element*) and their absolute differences, $|B_V - B_T|$.

Regression:	Ordinary			Robust		
Measure:	B_V	B_T	$\lvert B_V - B_T \rvert$	B_V	B_T	$\lvert B_V - B_T \rvert$
Subject:						
1	19.4	11.3	8.1	16.4	11.3	5.1
2	19.0	20.2	1.0	17.4	18.3	0.9
3	21.1	22.8	1.7	19.4	21.6	2.2
4	22.9	13.7	9.2	19.5	15.7	3.8
5	19.4	21.3	1.9	16.9	18.2	1.3
6	26.1	22.8	3.3	22.6	19.4	3.2
Mean	21.3	18.7	4.20	18.7	17.4	2.75

For five of the six subjects the robust method produces greater invariance across modalities of the mean array-size effect over delays, by this measure. The difference (1.45 ± 0.93 *msec/element*) is not statistically significant, however, so this test is inconclusive.

10. Fifth test: Clarity of model comparisons

To compare ordinary and robust regression with respect to the clarity of comparisons of goodness of fit of alternative models, we fitted three regression models, competitive with the model described in Section 4 (which we denote *Model 1*), to each of our 72 sets of data, using both regression methods. The exact structure of the three models is not important for the present report, so we provide only rough descriptions. The three competing models differ from Model 1 in the structure of their serial-position and absolute-position effects.

Model 2. Two separate absolute-position effects are contained in Model 1, one for end elements, and the other for interior elements. In Model 2 no distinction is made between these two types of element: The same absolute-position effect is assumed to apply to both. As a result there are fewer free parameters.

Model 3. For any array-size in Model 1 the assumed serial-position effect is fitted in such a way that the position of the array in the display area is irrelevant. Thus the leftmost element in an array of three elements that is placed as far to the *left* in the six-element display area as possible is regarded as having the same serial position as the leftmost element in an array that is placed as far to the *right* as possible. In Model 3, separate sets of parameters are fitted to arrays of different eccentricities and to centered arrays, and, moreover, serial position is assigned symmetrically for arrays in different positions, such that the leftmost element in an array on the left, for example, is regarded as having the same serial position as the rightmost element in an array on the right. This model has more free parameters than Model 1.

Model 4. In this Model we use the simple absolute-position structure of Model 2 and the complex serial-position structure of Model 3.

For each of the 72 data sets and each regression type, each of the three competing models was compared to Model 1, using two methods, as follows: In the first method, we computed root mean square residuals (RMSR) for each model and then determined the number of data sets favoring the most favored model in each pair. The second method was the same, with the RMSR replaced by the (more robust) median absolute residual (MAR). Results are displayed in Table 6, where the model whose number is shown in boldface is the favored one.

Table 6
Number of 72 data sets favoring the favored model.

Statistic:	RMSR		MAR	
Regression:	Ordinary	Robust	Ordinary	Robust
Models:				
1,2	72	67	47	48
1,3	61	59	51	51
1,4	49	46	48	46

In this table, higher numbers indicate clearer choices between models. Of the comparisons using the RMSR, all three favor ordinary over robust regression, but with a difference that is relatively small. Of the three comparisons using the MAR, robust and ordinary regression do about equally well. More microscopic analysis of the data, in which we considered these comparisons separately for different delays and modalities, added nothing to the impression conveyed by Table 6: For these data and models the regression methods do not differ substantially in relation to the criterion of clarity of model comparisons.

11. Summary and Conclusion

In this report we have advanced a notion about how alternative *statistical* procedures might be compared in the absence of adequate characterization of the data in terms of requirements of the procedures and/or adequate understanding of the procedures. Our proposal is that the criteria normally used to select among alternative *experimental* procedures be applied in this situation. These criteria are often only implicit, however, and may well depend on the field of study (and quite possibly the particular investigator); hence in Section 2 we defined and discussed six such criteria. We then described a domain of data with respect to which we have thought about this issue, and introduced the multiple regression model that we have been applying to these data. We described the two methods of fitting the multiple-regression model that we have been using, one being ordinary least-squares regression, the other Huber's "robust" iteratively reweighted least-squares method. Finally we reported our findings from five tests in which we applied the criteria defined earlier in a comparison of the two methods: In two of the tests they did not differ substantially, while three tests favored the robust method.

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