PRECOGNITIVE REMOTE PERCEPTION III:
Complete Binary Data Base
with Analytical Refinements

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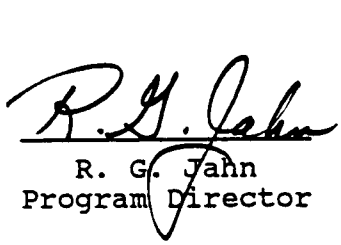

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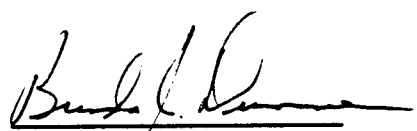

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ABSTRACT

Within the constellation of activities comprising the Princeton Engineering Anomalies Research Laboratory, a program addressing precognitive remote perception (PRP) experiments and analytical methodology provides important indicators of the basic nature of the consciousness-related phenomena under study. As the project has evolved, the binary scoring techniques used to quantify the PRP results have been refined to preclude a hierarchy of possible strategic or computational artifacts, thereby permitting more discriminating assessment of the experimental data, the design of more effective experiments, and the formulation of more appropriate theoretical models.

In this report are presented a complete update of the PRP data, descriptions of the analytical refinements, and a summary of the salient results. In brief, the PRP protocol continues to prove a viable means for achievement of anomalous information acquisition about remote physical targets by a broad range of volunteer participants. The full data base consists of 411 trials, 336 of which meet the criteria for formal data, generated by 48 individuals over a period of approximately ten years. Effects are found to compound incrementally over a large number of experiments, rather than being dominated by a few outstanding efforts or a few exceptional participants. The yield is statistically insensitive to the mode of target selection, to the number of percipients addressing a given target, and, over the

ranges tested, to the spatial separation of the percipient from the target and even to the temporal separation of the perception effort from the time of target visitation. Overall results are unlikely by chance to the order of 10^{-10}.

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"I can fly, or I can run
Quickly to the green earth's end,
Where the bow'd welkin slow doth bend,
And from thence can soar as soon
To the corners of the Moon."

John Milton
("Comus," IV)

I. Introduction

Evidence that human consciousness can access information spatially and temporally remote from its physical locus persists throughout an immense body of anecdotal lore extending from the primordial to the contemporary. Whether as a shaman of a primitive hunting tribe entering into communication with animal spirits to locate a source of food, a ruler of a Greek city-state consulting the Delphic oracle to determine the outcome of an imminent battle, or a modern corporate executive attempting through meditation to anticipate economic fluctuations, the human spirit instinctively endeavors to transcend space and time in its interactions with its environment. Although most of our mechanistic science officially decries such extrasensory processes, credible individuals continue to report "anomalous" experiences wherein knowledge has somehow been acquired concerning remote events, or events that have not yet taken place.

Efforts to comprehend the nature and implications of such phenomena also have a long historical trail that parallels the evolution of scholarly thought and social priority. The earliest philosophers approached the subject via a complementarity of mystical and pragmatic principles. The theological emphasis of

the Middle Ages conditioned scholars of that time to presume divine or demonic agencies for such anomalous information acquisition. In the 18th century, a systematic study of such topics, sponsored by the Roman Church, concluded that the grace of prophecy, "whereby a man may know. . . . future, past, or distant or hidden present things, or the secrets of hearts, or inward thoughts" 1) was not necessarily a divine or demonic miracle, but could have natural origins; 2) was more likely to occur to illiterate persons than to trained minds busy with "internal passions or external occupations"; 3) occurred more often in sleep than in waking; 4) was often indistinguishable from personal thoughts; 5) frequently took symbolic forms; and 6) could occur in heathens, children, women, animals, and fish, as well as in "holy people."(1) Late 19th century empiricism lent itself to methodical accumulation and documentation of spontaneous reports of such anomalies that could be examined for commonalities and patterns.(2) The "miracles" of wireless communication in the first half of the 20th century prompted association of the phenomena with prevailing electromagnetic theories.(3) In the modern scientific era, studies have characteristically focused on quantification and statistical assessment of experimental data acquired under tightly controlled research protocols.

The purpose of this report is to provide an update and summary of an experimental program of this last genre, conducted over the past decade as part of the Princeton Engineering Anomalies Research (PEAR) project. Based on a laboratory

protocol first called "remote viewing" by SRI physicists Harold Puthoff and Russell Targ,[4] the PEAR efforts have been directed toward the acquisition of a comprehensive data base and the development of analytical judging techniques for rendering free-response verbal descriptions of geographical targets into formats more amenable to quantitative analysis. The term "remote perception" is now preferred, to avoid any implication that the process is solely visual. Indeed, the evidence suggests that sounds, smells, and texture are also frequently perceived, along with more impressionistic aspects of a scene's ambience, such as age, symbolic representation, and emotional content. Since most of the PEAR perceptions are generated before the target scene is designated, the protocol is called "precognitive remote perception," or PRP.

Section II reviews the basic experimental procedure, protocol variations, and general analytical methodology, much of which has been described in more detail in earlier reports.[5-7] A number of more recent analytical excursions addressing a variety of methodological and interpretive issues are described in Section III. Section IV summarizes the results of the full data base and its various indicative subsets, and Section V presents a discussion of the formal and anecdotal observations, along with their implications for future studies of this kind. A summary of the major findings is given in Section VI. Details of more than 100 additional trials that have been added to the previously reported data base are provided in Appendix A-II, and additional appendices provide individual trial scores and further

technical details.

II. Experimental Design

A. Protocol

The basic experimental procedure for all our remote perception studies requires one participant, the "percipient," to attempt to describe an unknown geographical location where a second participant, the "agent," is, has been, or will be situated at a specified time. The percipient's impressions of the target are recorded in a stream-of-consciousness, free-response mode, optionally including drawings or sketches. These descriptions are usually handwritten, although some of the early trials were tape-recorded.

Percipients are free to determine their own perception strategies, and no systematic record has been maintained on the various subjective approaches employed for the task, nor on any psychological or physiological characteristics. All participants are untrained, uncompensated volunteers, none of whom claims exceptional abilities in this regard. Although no explicit tactical instructions are given, an attitude of playfulness is encouraged and emphasis is placed on the experience as a learning process, rather than on the achievement, per se. Percipients usually select the time and place most convenient for them to generate their descriptions, and no experimenters or monitors are present during the perception period, although precautions are taken to ensure that perceptions are recorded and filed before percipients have any sensory access to target

information. Descriptive styles vary widely from one participant to another, ranging from cryptic, sharply defined statements on one extreme, to lengthy impressionistic meanderings on the other. (Examples of typical perception transcripts are available in References 7-10.)

The agents, who are always known to the percipients, situate themselves at the target sites at the prescribed times and immerse themselves in the scenes for about 15 minutes. They then record their impressions verbally, sometimes supplemented by sketches and, whenever possible, by one or more photographs to corroborate their descriptions. As with the percipients, agents are free to employ their own subjective strategies and do not undergo any formal training. They are only encouraged to attempt in some way to "share" their target experiences with the percipients. A total of 38 percipients and 21 agents have contributed to the PEAR data base; 11 have served in both capacities, bringing the total number of participants to 48.

After recording their free-response descriptions, both percipient and agent encode these in the form of binary (yes/no) responses to identical lists of 30 descriptor questions that provide the basis for subsequent analytical assessment of the data acquired. These questions range from factual details, such as whether the scene is indoors or outdoors, or whether people or animals are present, to more impressionistic aspects, such as whether the scene is confined or expansive, or whether there is significant sound or motion. The complete list of descriptors, along with a sample response check sheet, is provided in

Appendix B.

This basic experimental design accommodates a number of variations which have been deliberately explored to greater or lesser extent as predicated by the empirical results, and which serve to index the data presented in this report:

1) Several alternative methods have been used for analysis of the concurrence between the percipient and agent responses.

2) Results of trials encoded ab initio by the percipients and agents themselves have been compared with those encoded ex post facto by a group of independent judges.

3) Trials have been classified as formal, questionable, or exploratory, depending on their conformity to pre-set criteria.

4) Two methods of target designation have been used: a) the target is randomly assigned from a blind pool of potential targets ("instructed" mode), or b) the target is freely chosen by the agent ("volitional" mode).

5) The number of percipients addressing a given target/agent situation has been varied.

6) The spatial separation between the percipient and the target has been varied from less than 1 mile to more than 6000 miles;

7) The temporal separation between the perception effort and the time of target visitation by the agent

has been varied from zero to plus or minus more than 120 hours.

Each of these options is discussed in the following sections.

B. Scoring Methods

Earlier attempts to quantify remote perception or other forms of free-response experimental data invoked a variety of human judging procedures. Many of these involved taking the percipient or judges to all of the target sites in the relevant pool, or, as in the case of our own early experiments, providing judges with randomized photographs and agent-generated verbal descriptions of the locations and asking them to rank order these scenes against the perceptions. Any such human judging process, however, presents a number of generic problems, such as vagaries in the judges' capabilities, subjective biases, and even possible anomalous functioning on the part of the judges themselves. Perhaps the most important concern is the statistical inefficiency of the ranking approach, whereby a given perception is reduced to a single datum, ordinal at best, in a small series of experimental trials.(11)

To alleviate some of these difficulties, more analytical scoring procedures have been developed to replace the subjective human judging process with a standardized form of information quantification that allows the signal-to-noise ratio of individual perceptions to be evaluated. The heart of the method is the establishment of a code, or alphabet, of 30 binary descriptive queries that are addressed to each target and

perception, responses to which produce two strings of 30 bits that serve as the basis for numerical evaluation of the given trial. These responses are then entered into computer files as binary digits and subjected to several scoring recipes. (At one point a ternary response option was explored to provide participants with an "unsure" alternative to the stark binary decision, but the added computational complexity was not justified by its modest advantages.[6])

A large number of binary scoring methods have been investigated over the course of the program, from a simple counting of the number of correct descriptor responses, to a variety of more sophisticated versions that weight the value of correct descriptor responses in accordance with their a priori likelihood of occurrence in the prevailing target pool. (These a priori descriptor probabilities will subsequently be referred to as "alphas," or α_i, where "i" denotes the ordinal number of the descriptor.) The total response score is then normalized in terms of the chance expectation or in terms of the highest possible score -- the score that would be achieved if all target and perception responses for an individual trial were identical.

For each scoring method, every perception in the data base is scored against every target and these scores are arrayed in a square matrix, the diagonal of which comprises all of the matched trial scores. The off-diagonal elements, or deliberately mismatched scores, can be assembled as a frequency distribution of empirical "chance" results to serve as a reference for calculating the statistical merit of the matched

scores. Despite detectable skew and kurtosis, these mismatch distributions have sufficiently Gaussian characteristics to allow simple parametric statistical tests. Since the mismatch scores reflect the same descriptor correlations inherent in the matched scores, statistical artifact from this source is largely precluded.

From the many scoring methods explored, five were selected for processing the data reported in Ref. 7. All were binary in nature, and all employed the same set of α_i derived from the more than 200 targets in the data base at that time. Each of these five methods, described in detail in Refs. 5-7, has certain advantages and weaknesses, and all admittedly sacrifice much of the subjective or symbolic impressions that might be detected by a human judge, since the forced "yes" or "no" responses are limited in their ability to capture the holistic ambience or overall context of a scene. Restriction of the extracted information to the 30 specified descriptors clearly excludes from the evaluation process many other potentially relevant details, such as texture, specific colors, or numerical features not covered by these questions. To some degree, these shortcomings may be gauged by submitting participants' free-response descriptions to human judge evaluation as a complementary method of assessment. Indeed, several of the experimental series were so judged and, while a few individual trials were found to suffer from the analytical judging and others to gain some advantage, the overall results were reassuringly similar.[5,8] This is also true of the disparities

among the five different scoring methods; across the full database or major subsets, all produce quite comparable bottom line results. This consistency of yield between the subjective human judging and the various analytical techniques confirms the efficacy of the latter in preserving the essence of the scenes, while providing more incisive tools for detailed examination of their structure and more thorough correlation of results with experimental parameters.

Given the essential indistinguishability of results calculated by the various scoring recipes, for the sake of brevity only one method was employed for the several explorations to be discussed here ("Method B" in previous reports). This method was chosen because it is the most conservative of the five, and because it treats positive and negative descriptor responses in a symmetrical and intrinsically normalized fashion. Briefly, this process weights each "yes" response by $1/\alpha$ and each "no" response by $1/(1 - \alpha)$, and divides the sum of all correct perception responses by the sum of all target descriptors similarly weighted, i.e. by the highest possible score for that trial (Appendix C-II).

C. Ex Post Facto vs. Ab Initio Encoding

The descriptor list mentioned above evolved through numerous refinements and explorations, many of which have been reported in earlier Technical Notes[5-7]. The first 68 trials in the data base, which had been generated prior to any of these analytical judging methods and had been subjectively evaluated by a number

of independent human judges, were used as a guide for selection of the first set of descriptor questions. This iterative process employed five individuals, each of whom independently reviewed, in random order, the verbal perceptions and encoded them via binary responses to an initial set of 30 descriptor questions. Agents' verbal descriptions, supplemented by target photographs, were then encoded in a similar fashion, with the encoders remaining blind to the correct matches. Cases of disagreement were discussed and, if necessary, resolved by majority vote. On the basis of these initial encodings, those descriptors whose frequency of occurrence was excessive or negligible, those which were too highly correlated with each other, and those which proved too ambiguous or otherwise ineffective, were revised or replaced and the data re-encoded by a similar process, until an optimal set of questions was established. This final descriptor set, reproduced in Appendix B-I, was confirmed by factor analysis to be well balanced in scoring effectiveness and reasonably independent, despite the inevitable correlations to be expected in attempting to quantify free-response descriptions of this type.[7,12]

The 68 trials originally encoded in this iterative fashion, 59 of which otherwise met the criteria for formal classification, have subsequently been labelled "ex post facto," and serve as a comparison for all later participant-encoded data, termed "ab initio." The latter group comprises 343 trials, 277 of them formal and, as will be seen, shows important distinctions from its predecessor.

D. Data Classification

Six criteria were established early in the experimental program for classification of all remote perception trials into three separate categories:

1. Formal trials are defined as those which follow the standard protocol or its variations described above, and which meet all of the following conditions:

a. The agent and percipient are known to each other.

b. The date and time of the target visitation are specified in advance.

c. The agent is present at the target within 15 minutes of the specified time and is consciously committed to fulfilling that experimental role at that time.

d. The percipient delivers a comprehensible verbal description and -- except for the ex post facto encoded trials -- a completed descriptor response form to the laboratory before there has been any opportunity to obtain feedback about the target.

e. Both agent and percipient have adequate familiarity with the experimental protocol and with the application and interpretation of the descriptor questions.

f. Substantiating target information, such as photographs, drawings, or written descriptions, is

available to corroborate the agent's descriptor responses.

2. Questionable trials are defined as any which meet criteria a-d, but fail to meet criterion e or f; or, for any reason, are vulnerable to sensory cueing.

3. Exploratory trials, so designated before implementation, are those which deliberately employ some non-standard protocol, such as unspecified or purposely altered target times.

Any trial not pre-specified as exploratory that fails to meet criteria a-d is defined as invalid and is not included in the data base. This includes practice or demonstration trials conducted for purposes of familiarizing participants with protocol and descriptors, or for entertainment of visitors.

The full PRP data base consists of 411 trials, 336 of which meet the criteria for formal classification. All of these, including the 21 questionable and 54 exploratory trials are described in detail either in Ref. 7 or in Appendix A-II of this report.

E. Target Designation

Beyond the Formal/Questionable/Exploratory distinctions, a secondary protocol parameter of considerable importance is the method of target selection. In the "Instructed" version of the protocol, the target is determined by random selection, without replacement, from a pool of potential targets prepared in advance by individuals not otherwise involved in the experiment. For example, the target pool designated "Princeton" consisted of 100

locations within a 30-minute drive from the University campus. Each target was specified on a 3x5 index card and sealed in an opaque envelope, along with instructions to the agent for reaching the site. The shuffled envelopes, randomly numbered, were kept in the office of an Assistant Dean of the Engineering School. Before each trial, a sequence of electronically generated random integers converted to a two-digit number designated the target envelope given to the agent, who opened it after leaving the building and proceeded to the assigned location. A similar procedure was followed for all instructed trials carried out elsewhere, albeit with smaller target pools.

The "Volitional" mode of target selection is typically employed when the agent is traveling on an itinerary unknown to the percipient, or is in an area where no prepared pool exists. At a prearranged local time, the agent selects and visits an accessible site. Agents are advised to avoid typical "postcard" scenes or targets that might be logically associated with any knowledge of their general whereabouts. (In almost all cases, the percipient is aware only that the agent will be "somewhere in Europe," "in the continental U.S.," or simply "out of town.")

The data base encompassed by this report consists of 128 instructed trials, of which 125 qualify as formal, and 283 volitional trials, of which 211 qualify as formal.

F. Single vs. Multiple Percipients

The standard PRP protocol involves a single percipient attempting to describe the location of a single agent, but early

in the experimental program a variation was introduced wherein two or more percipients addressed the same target. In all but two of the multiple-percipient trials the percipients were aware that others were addressing the same targets, although they did not always know their identities. As in the single-percipient trials, the agents and percipients invariably knew each other. In all cases the multiple percipients were spatially separated from each other and, in most cases, attempted their perceptions at different times. The number of percipients addressing a specific target ranged from two to seven, and each perception was scored as a separate trial against its appropriate target. A total of 120 such trials were conducted, all of them formal, involving 36 different targets, compared to 291 trials conducted with single percipients, 216 of which met the formal criteria.

A few exploratory trials involved more than one agent, and one group of formal trials (Series 15a,b in Ref. 7) also employed two agents. In all cases only one prespecified set of target encodings was included in the data base; the second agent's encodings were used only for informal comparison.

G. Spatial and Temporal Separations

Of central importance in attempting to identify mechanisms for the information acquisition observed in experiments of this class are the dependencies of the results on the distance separating the percipient and the target, and on the time interval between the perception effort and the period of target visitation by the agent. In an attempt to assess these

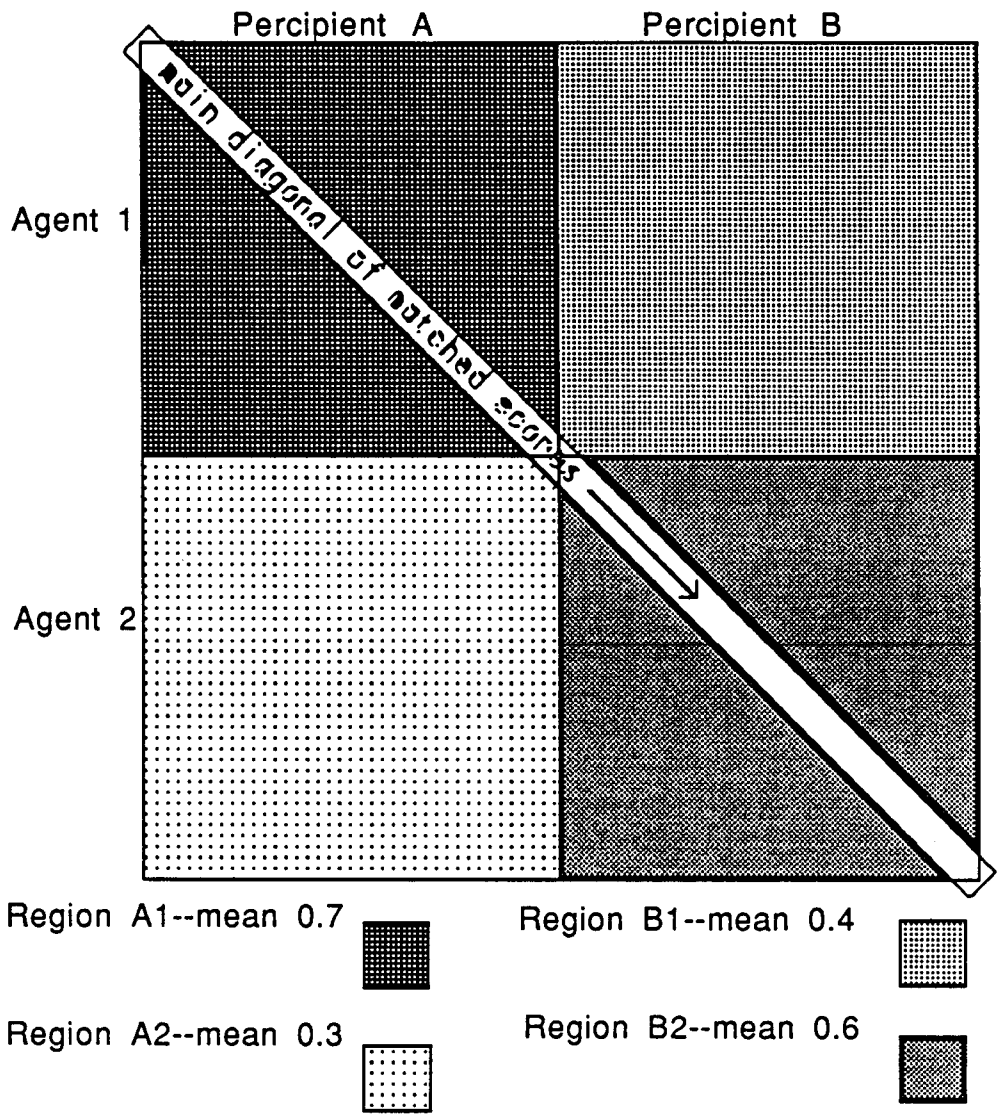
parameters systematically, our experiments have addressed targets ranging from proximate to global distances from the percipient, and have involved temporal separations up to several days, both plus and minus.

III. Analytical Refinements

Since publication of the 1983 PRP report[7], a number of further refinements of the analytical judging scheme have been considered and explored to varying extents. Some have been incorporated into the standard analysis; some have been discarded as unproductive; others are still being pursued. In this Section we review a few of the incorporated improvements, modifications, and instructive results that have devolved from these explorations.

A. A Priori Probabilities, Empirical Chance Distributions, and Possible Encoding Artifacts

A crucial component of the basic analytical scoring technique is the array of a priori descriptor probabilities, α_i, that represent the empirical likelihoods that the various questions will be answered "yes" by an agent. These α_i were originally calculated from their frequency of occurrence in the pool of approximately 200 target descriptions on file at the time of the previous analysis.[7,8] Examination of the current assortment of 327 targets used for the 411 trials reported here raised some questions about the generality of any single, universal set of descriptor probabilities for all of the various



PRP Simplified Example Matrix

Figure 1

data subsets of interest. In particular, the empirical estimates of the α_i were found to vary from some target subsets to others, often to statistically significant degrees (Appendix C-I). Given the central role of the α_i in all the scoring methods, some assessment of the sensitivity of the scores to such α_i variations seemed warranted.

Coupled to this issue is the process whereby scores obtained by any method are compared with some chance background distribution to determine the statistical likelihood that any correspondence between agent and percipient reports is due to chance. As described in Section II.B, our usual strategy has been to generate an empirical chance distribution consisting of all the deliberately mismatched targets and perceptions in the score matrix. The question to be addressed is whether vagaries in the specification of particular subset α_i's can feed through the matched scores and the empirical chance distribution into the statistical treatment to produce artificially inflated, or deflated, results. The subsets of most evident concern are those of particular percipient/agent pairs, although any of the protocol variation subsets might also be suspect, as well as those distinguished by geographical region or seasonal differences.

This potential problem can be illustrated via the hypothetical score matrix shown in Fig. 1. Assume for simplicity that the entire database is the work of just two percipient/agent pairs, A/1 and B/2, and that no anomalous transfer of information prevails in any of their data, so that the matched and mismatched

scores in each quadrant are statistically indistinguishable. If the pair A/1 happens to share a closely similar encoding style, e.g. a tendency to respond affirmatively to ambiguous features, or particular preferences for certain descriptors, their α_i patterns may tend to resemble each other more regularly than chance expectation, causing all of their scores, both actual trials and mismatches, to be higher than the unbiased expectation. In the example, this pair is arbitrarily assigned a mean score of 0.7. A similar situation is postulated between the pair B/2, to whose scores is assigned a mean of 0.6. However, if the permuted pairs, B/1 and A/2, (who do not actually perform trials together) happen to have <u>negatively</u> correlated α_i's to roughly the same degree, this would produce a comparably lowered distribution of mismatch scores in the remaining two quadrants of the matrix, which are assigned means of 0.4 and 0.3, respectively. In this hypothetical case, the scores of the four local regions of the matrix compound to an overall empirical chance mean very close to 0.5. In contrast, the combined mean of the matched scores of A/1 and B/2 along the full diagonal is 0.65, indicating a strong -- but spurious -- positive effect that would carry through any subsequent statistical analysis. It should be noted that this type of artifact need not be an enhancement; if A/1 and B/2 had contrasting, rather than corresponding α_i's, and A/2 and B/1 had positively correlated α_i's, the diagonal scores would have been artificially low.

While this example has been presented in terms of similarities or dissimilarities in individual encoding styles,

distorted response frequencies could also arise from other sources. For example, the date of the trial could suggest to the percipients higher likelihood of, say, snow on the ground or green vegetation. Such information might, albeit unconsciously, influence percipient responses so that a similar diagram could be drawn with "all summer trials" in one box and "all winter trials" in another. Likewise, ex post facto/ab initio, instructed/volitional, or single/multiple percipient dichotomies could be posed, or groupings of trials where the targets were drawn from the same geographical regions. (Daytime/nighttime contrasts could be another concern, but all trials in this data base were conducted during daylight hours.) These comparisons are discussed more fully in Appendix C-I.

The actual situation is considerably more complex than the simple illustration of Fig. 1, since all of the major data subsets involve many pairs of percipients and agents, only a small portion of whose permutations actually contribute to the matched target scores on the matrix diagonal, while the greater portion of the off-diagonal scores are constituted from percipients and agents who, like A/2 and B/1 in the illustration, do not actually perform experiments together. Furthermore, these various off-diagonal sub-groups are of various sizes and, of course, unknown correlation. For such complicated score arrays, the generic vulnerability still arises from the possibility that the percipient/agent descriptor correlations are not randomly distributed, but that, for whatever reason, those pairs actually performing experiments have more closely correlated response

preferences than those not paired in the experiments. Then, as in the illustration, the diagonal scores could be artificially inflated with respect to the total off-diagonal distribution.

Fortunately, despite the fact that the α_i of various percipient/agent subsets do indeed differ, sometimes quite significantly, a straightforward procedure for precluding artifacts from this source can be devised. Note that in Fig. 1, in the two quadrants A1 and B2, the mean scores along the main diagonals of matched scores are statistically indistinguishable from those in their immediately surrounding quadrant blocks, so that if the matched scores are referred to their <u>local</u> mismatch blocks, the score inflations compensate one another, and no statistical advantage obtains. Calculations with more elaborate, artificially constructed pseudo-trials confirm this compensation effect (Appendix D). Thus, any suspicion that local response preferences may be producing a spurious effect in real data sets may be checked simply by calculating each subset of matched scores independently, using its appropriate local α_i, and referring the matched scores to their <u>own</u> mismatch distributions.

Further details of this argument and verification of this strategic conclusion are presented in Appendix C-II. The results of the various data groupings mentioned above, including the individual percipient/agent pairs, each calculated as an independent subset, are described in Section IV below.

B. Local Subset Calculations

For the reasons just developed, it seems advisable to calculate the statistical results of each data subset using the α_i and the mismatch distribution appropriate to that subset. Such calculations reveal an unexpected, but important compensatory effect that proves to be nearly universal: namely, when the empirical chance distributions for the various subsets are calculated using their local a priori probabilities, the respective distribution parameters are statistically indistinguishable from each other, and from those of the full target distribution.

The single exception to this rule is the multiple percipient data, which account for 36 of the 327 targets and 120 of the 411 trials, all of them formal. An inevitable ambiguity arises in defining the proper α_i for these trials, i.e. whether each target should be counted only once, or whether it should be repeated for each perception addressing it. In either treatment, the multiple percipient data produce a mismatch distribution significantly different from that of the data base as a whole. However, if only one perception is included for each of the targets, it is again found that the local mismatch distributions for all the different subsets, each calculated with its own local α_i, are statistically equivalent.

The distribution statistics for each of the major target subsets are summarized in Table A. (In this table, "Multiple Group 1" includes only the first perception for each of the 36 multiple percipient targets, and "Multiple Group 2" uses the

Table A

Mismatch Distribution Parameters[13] for Various Subsets of the PRP Data Base

Subset*	No. Mismatch Scores**	Mean	Std. Dev.	Std. Error	Skew	Kur-tosis
All Targets (Universal)	106,602	.5025	.1216	.00037	.132	-.223
Formal Targets	63,252	.5026	.1209	.00048	.107	-.226
Multiple Group 1	1,260	.5009	.1235	.00348	.004	-.024
Multiple Group 2	1,260	.4982	.1192	.00336	.049	-.216
All Multiple Trials	14,280	.5002	.1200	.00100	.045	-.123
Instructed Targets	13,110	.5033	.1238	.00108	.048	-.268
Volitional Targets	44,732	.5030	.1229	.00058	.150	-.230
Ab Initio Encoding	79,806	.5021	.1223	.00043	.137	-.237
Ex Post Facto Encoding	1,892	.5039	.1229	.00283	.118	-.250
Chicago Targets	552	.5025	.1354	.00576	-.022	-.558
Princeton Targets	11,130	.5026	.1228	.00116	.093	-.324
Targets Elsewhere	38,612	.5030	.1234	.00063	.158	-.226
Winter Targets	9,312	.5023	.1209	.00125	.122	-.343
Summer Targets	52,670	.5026	.1220	.00053	.117	-.214

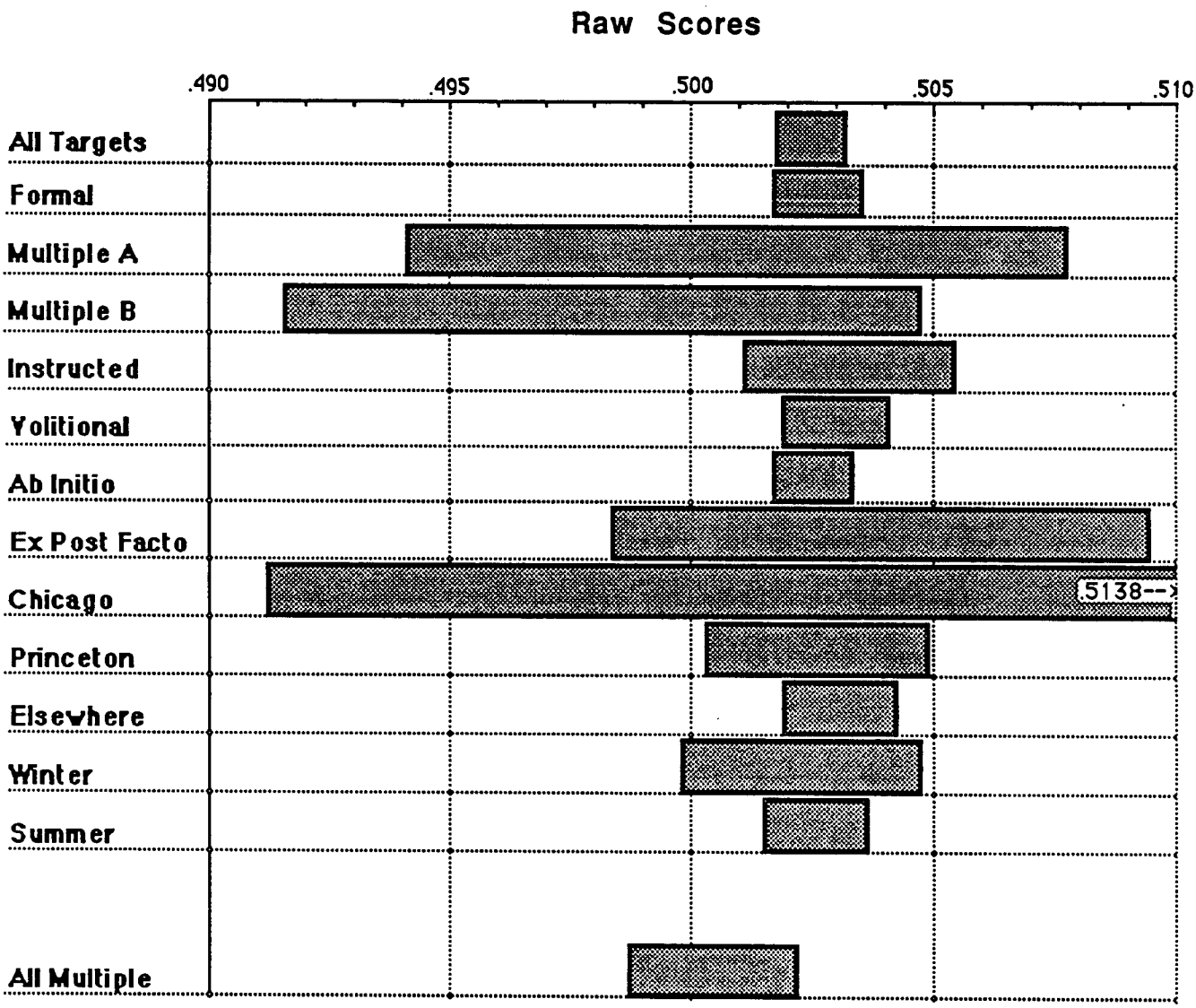
*All subsets, except Formal Targets, include Questionable and Exploratory trials as well as Formal data, since the total Formal and Non-Formal subset mismatch distributions are indistinguishable.

**The mismatch scores comprise the $(N^2 - N)$ off-diagonal elements of the square matrix of N targets and N perceptions.

second perception per target. The "All Multiple Trials" subset includes all perceptions, with targets repeated as necessary, and is calculated with α_i's that reflect these repetitions.)

Figure 2 shows the 95% confidence intervals for the empirical chance means for each of the subsets listed in Table A. These are computed as the means of the mismatch distributions ± the standard errors multiplied by 1.960 (the 2-tailed 5% z-score), and provide a conservative indication of the accuracy of the mean estimates. Although the means of the larger subsets are more precisely estimated than those of the smaller distributions, all of the mean values are seen to be quite close. Even the mean of the multiple data set, here represented separately at the bottom of the graph, differs from that of the full target distribution by only .0023, a difference that is significant only because of the very large N's involved. All of this suggests a major simplification in the statistical scoring of subsets: namely, that a <u>universal</u> mismatch distribution, constructed from all target α_i, and using only one perception per target, is indeed appropriate as an empirical chance reference for calculating the statistical merit of any matched score subset, <u>provided that those matched scores are computed from their own</u> α_i, since the results will be statistically equivalent to those obtained by comparison against the proper local chance distributions.

Two caveats must attend this simplification. The first excludes the multiple percipient subset for the reasons already mentioned. Note, however, that since the local empirical chance



95% Confidence Intervals for Means of
the PRP Subset Mismatch Distributions

Figure 2

distribution for the multiple percipient trials actually has a lower mean than that of the universal distribution, multiple percipient trials compared with their own subset distribution would actually appear more significant than when compared with the universal distribution. Thus, using the universal distribution for comparison with this subset errs on the side of conservatism.

The second caveat pertains to minimum subset data base size. The fortunate correspondence of chance distributions may not apply reliably to very small data sets, where fluctuations in the α_i due to small N may be significant. In individual agent/percipient pair comparisons, for example, the number of trials in a subset may be so small that both the local α_i calculation and the local mismatch distribution parameters become vulnerable to substantial statistical uncertainty. Since variance also tends to be poorly estimated in such small data sets, the comparison of matched scores with the local mismatch distribution seems likely to be less reliable than the comparison with the universal set. In these cases we have performed the calculations both ways and compared the results.

C. Analysis of Data

Since the parameters of the universal chance distribution can be most accurately estimated from the largest possible data set, it is derived from the full set of all 327 targets, using the first perception for each target, thus providing 106,602 off-diagonal components (the first line in Table A). This chance

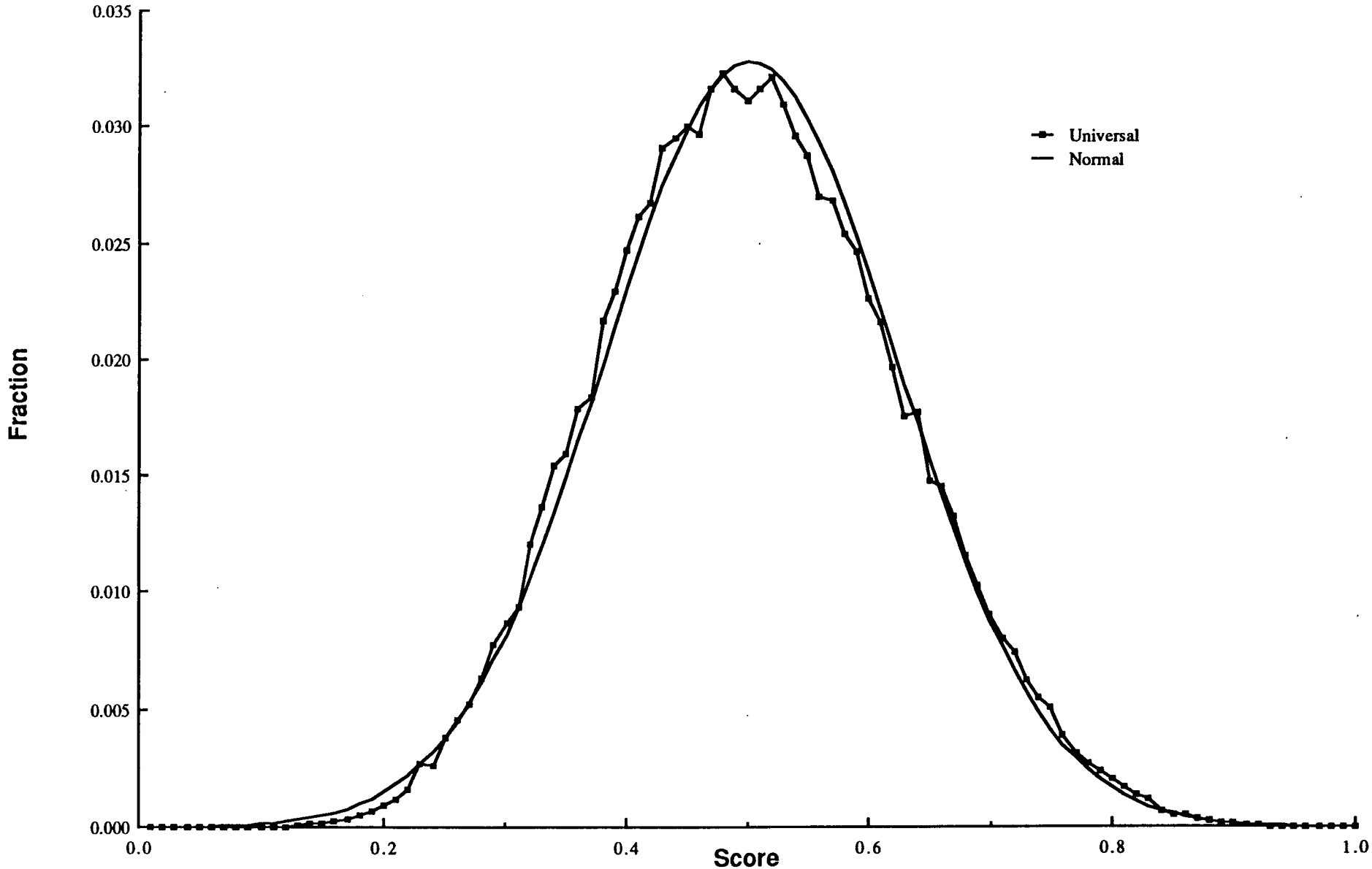


Figure 3: PRP Universal Mismatch Distribution Compared with Normal Distribution

distribution is shown in Fig. 3, overlaid on a normal distribution of the same mean, variance, and total area. Note that this distribution, like most of the subset distributions listed in Table A, entails some positive skew (.132) and negative kurtosis (-.223), both of which, given the very large N's involved, are statistically significant. Thus, some assessment is required of the extent to which the distorted shape of this distribution affects the calculation of parametric statistics based on a normal distribution.

Direct comparisons of the parametric probabilities associated with particular z-scores based on the normal distribution, with non-parametric probabilities computed by integration of the empirical distribution, indicate that the effect of the non-normality is statistically inconsequential. For any given trial, probabilities calculated by these two methods typically differ by less than 1% in the z-score, a difference that propagates through the various composite z-score calculations to comparably minuscule differences in the overall probabilities. The difference in significance of individual trials is similarly inconsequential: of the 327 trials whose mismatches were used to construct the universal distribution, a total of 43 (13.2%) have z-scores > 1.645 (one-tailed 5% cutoff criterion) by parametric calculation, while 41 (12.5%) are in the top 5% tail of the nonparametric distribution.

To summarize this aspect of the data analysis, it now appears that the calculation of subset scores on the basis of their local α_i, and comparison of these against a universal

empirical chance distribution to determine their statistical merit, is less vulnerable to encoding artifacts than the earlier methods that applied a generalized α_i to all subsets of the data. The possibility of local biases in sections of the data base producing spurious effects can be even more strictly precluded by comparing the data in any given subset with its own local mismatch distribution, although when N is sufficiently large these local distributions turn out to be statistically indistinguishable from each other or from the universal distribution. This feature is further demonstrated in Figure 4, which compares three different evaluations of a group of 120 trials randomly selected from the formal data. In this frequency histogram, the grey bars show the results for this "subset" calculated with its local α_i and referenced to the universal chance distribution (mean z-score = 0.833, standard deviation = 1.053). The hatched bars show the same group of trials calculated with local α_i and referenced to the local mismatch distribution (mean z-score = 0.829, standard deviation = 1.012). The white bars indicate the results calculated by the original method described in Ref. 7, employing the generalized α_i and the empirical chance background distribution in use at that time (mean z-score = 0.779, standard deviation = 1.035). The three distributions are statistically indistinguishable.

For the following presentation of results, the first method--scores calculated with local subset α_i referenced to the universal chance distribution--will be applied throughout. In subsets with very small N's, the magnitude of possible

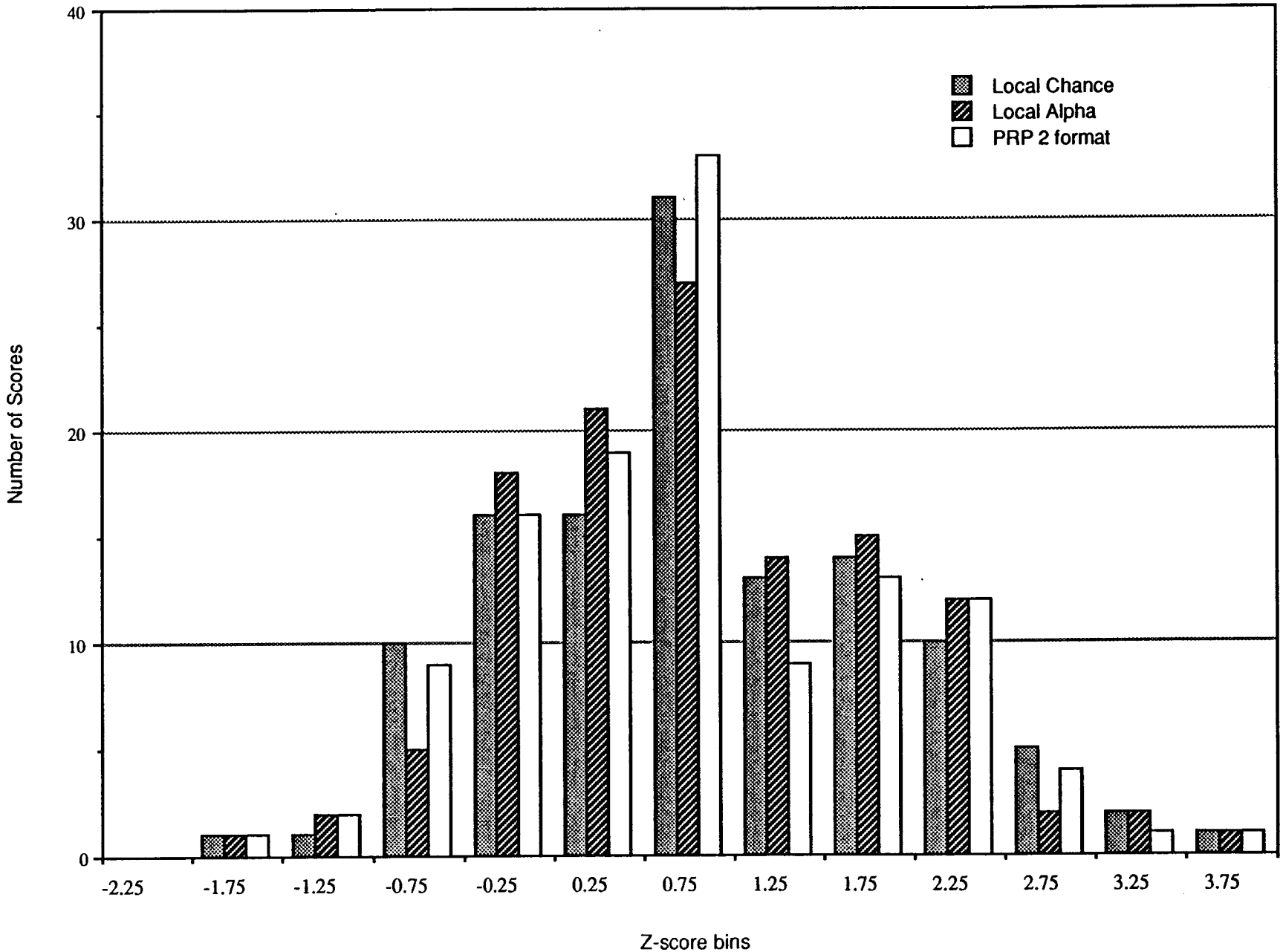


Figure 4: Three Methods of Scoring an Arbitrary Subset of 120 PRP Trials

statistical fluctuations make it advisable to carry out comparison calculations using the local mismatch distributions as well.

IV. Results

A. Characteristics of the Full Data Base

The results of the 336 formal PRP trials are highly significant, with a composite z-score of 6.355 ($p = 10^{-10}$). Even when the 75 non-formal trials are included in the calculation, the overall yield for all 411 trials remains well beyond chance expectation ($z = 5.647$). Details of the full PRP data base and its various subsets are summarized in Table B. In addition to those subsets representing planned variations of the protocol, e.g. the secondary variables ex post facto vs. ab initio encoding, instructed vs. volitional assignment of targets, and single vs. multiple percipients, ad hoc subdivisions of the data base by seasonal and regional groupings are also included. The table displays for each independently calculated data group the number of trials, the mean score, the effect size with its associated confidence interval (here computed on the more conservative 99% basis to emphasize the consistency of the yield), the standard deviation of the z-score distribution, and the composite z-score with its associated 1-tailed probability against chance. The last two columns give the number of trials in each subset having $z > 1.645$ (numbers in parentheses indicate $z < -1.645$), and the percentage of scores having a probability against chance > .50. Each group is scored with the local α_i

Table B

PRP Data Summary

Subset*	No. Trials	Mean Score	Effect Size	99% Conf. Intervals	SD of z	Composite z-score	Prob. (1-tailed)	# Trials p < .05	% Trials p < .5
All Trials	411	.5364	.279	± .135	1.060	5.647	8×10^{-9}	47 (12)	58.6
Formal Trials	336	.5447	.347	± .152	1.083	6.355	1×10^{-10}	44 (8)	61.9
Non-Formal Trials	75	.4969	-.046	± .278	.910	-.399	.655	3 (4)	44.0
Ab Initio	277	.5345	.263	± .161	1.033	4.378	6×10^{-6}	31 (5)	59.2
Ex Post Facto	59	.5942	.754	± .417	1.203	5.792	3×10^{-9}	14 (2)	74.6
Single Percipient	216	.5489	.382	± .194	1.098	5.614	1×10^{-8}	34 (6)	59.7
Multiple Percipients	120	.5404	.312	± .251	1.049	3.416	3×10^{-4}	12 (3)	63.3
Instructed Targets	125	.5653	.516	± .267	1.140	5.771	4×10^{-9}	23 (5)	64.8
Volitional Targets	211	.5322	.244	± .191	1.066	3.549	2×10^{-4}	25 (3)	59.7
Summer Trials	244	.5466	.363	± .183	1.099	5.663	7×10^{-9}	35 (5)	65.2
Winter Trials	92	.5407	.315	± .286	1.043	3.017	1×10^{-3}	13 (2)	56.5
Chicago Targets	31	.6189	.957	± .587	1.189	5.330	5×10^{-8}	10 (1)	80.6
Princeton Targets	106	.5504	.394	± .286	1.110	4.060	2×10^{-5}	14 (3)	62.3
Targets Elsewhere	199	.5267	.199	± .194	1.051	2.810	2×10^{-3}	20 (3)	58.3

*Except for All Trials and Non-Formal Trials, all subsets are computed using formal trials only and all are calculated with reference to the universal chance distribution of mismatched scores with N = 106,602, mean = .5025, S.D. = .1216.

associated with that subset, and except for the groups labelled "All Trials" and "Non-Formal Trials," the various subsets consist of formal trials only.

The effect size presented in this table is simply the mean z-score of all the trials in the subset, and thus is a measure of how much, on the average, the trial scores deviate from chance expectation as defined by the universal distribution of mismatch scores. The standard deviation (S.D.) of the z-score refers to the set of trial z-scores, and would be expected by chance to be 1; it is also numerically equal to the ratio of the S.D. of the matched score distribution to the S.D. of the empirical chance distribution. The composite z-score column provides a measure of the statistical significance of the entire subset, calculated by multiplying the effect size by $\sqrt{N}$.

B. Formal vs. Non-Formal

It is clear from the summary of Table B that the formal data display a strong anomalous yield that permeates throughout all of their various subsets, while the assembly of non-formal trials constitutes a distribution indistinguishable from chance. It should be re-emphasized, however, that the designation of "formal" or "non-formal" data is made solely on the basis of protocol, as described in Section II-E. The non-formal data are included in this report as a separate group for three reasons: to identify the protocol excursions that have been attempted; to allow comparisons of the yields from the formal and non-formal

experiments; and to preclude any concerns regarding data selection or suppression.

The non-formal group may be further divided into smaller independent subsets, each calculated with its own local α_i. For example, the 21 questionable trials that failed to meet the formal protocol criteria produce an overall effect size of -.064 with a composite z-score of -0.292. The 54 exploratory trials yield an effect size of -.084, z = -0.616. In 38 of these, the target time was intentionally left unspecified (effect = -.077, z = -0.475); in 10, the agent was unspecified to the percipient (effect = -.349, z = -1.104); and in four, the agent deliberately altered the target visitation time without the percipient's knowledge (effect = .502, z = 1.005). The remaining trial addressed a non-physical target, (the agent's visual imagery,) and had a normalized score of .596, z = 0.766.

The shape of the distribution of all 336 formal trial scores does not differ statistically from a normal Gaussian (Fig. 5). As in the universal chance distribution, there is a certain degree of positive skew (0.167) and negative kurtosis (-0.380), but these are not significant for this smaller number of trials. Figure 6 compares the Gaussian fit to the formal data with that of the chance distribution, drawn to the same scale, along with a curve indicating the difference between the two. In addition to the strongly significant shift of the mean of the data distribution (z = 6.355), there is a marginally significant increase in the distribution variance as well (F = 1.173, p = .016).

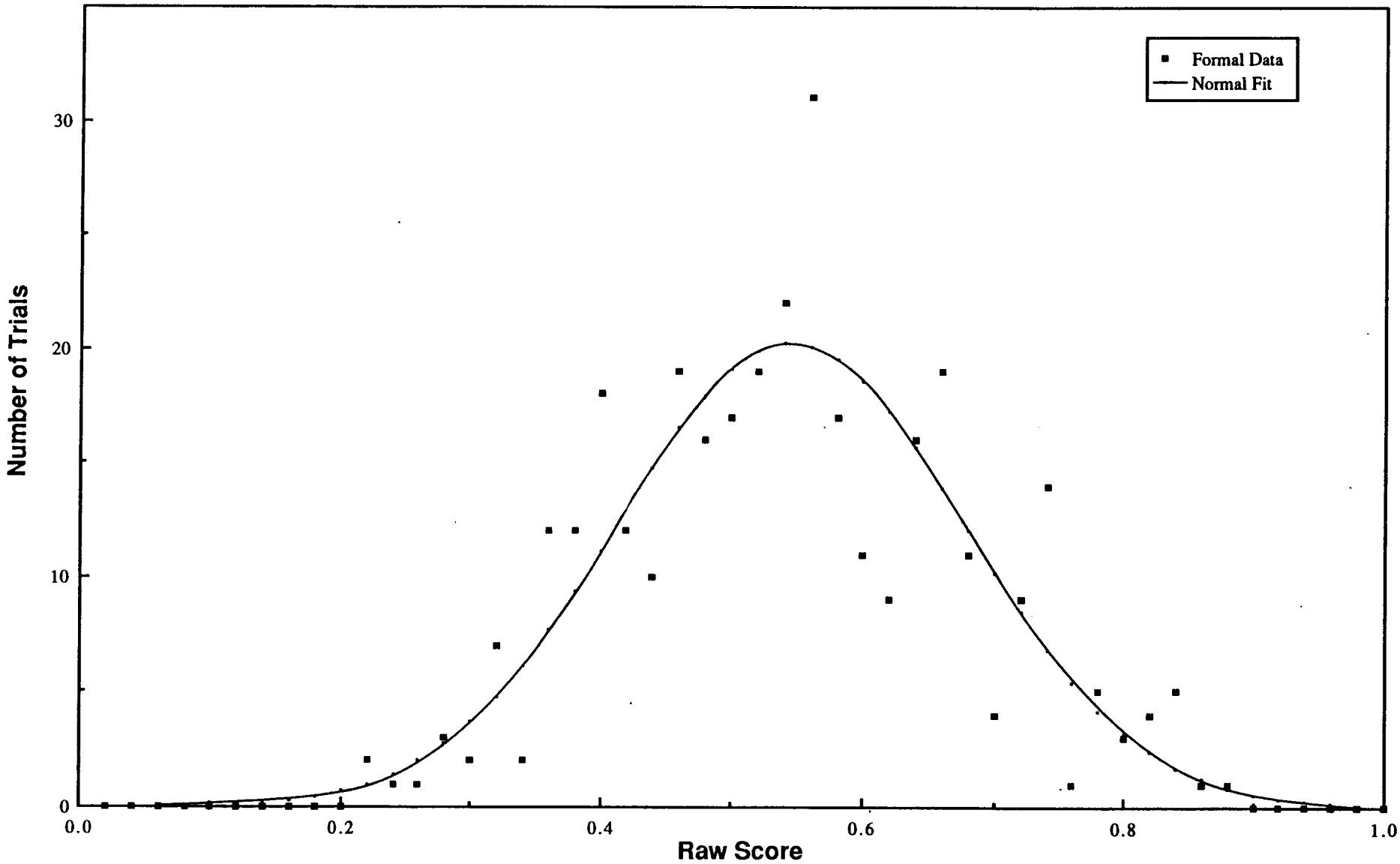


Figure 5: Formal PRP Data and Best Normal Approximation

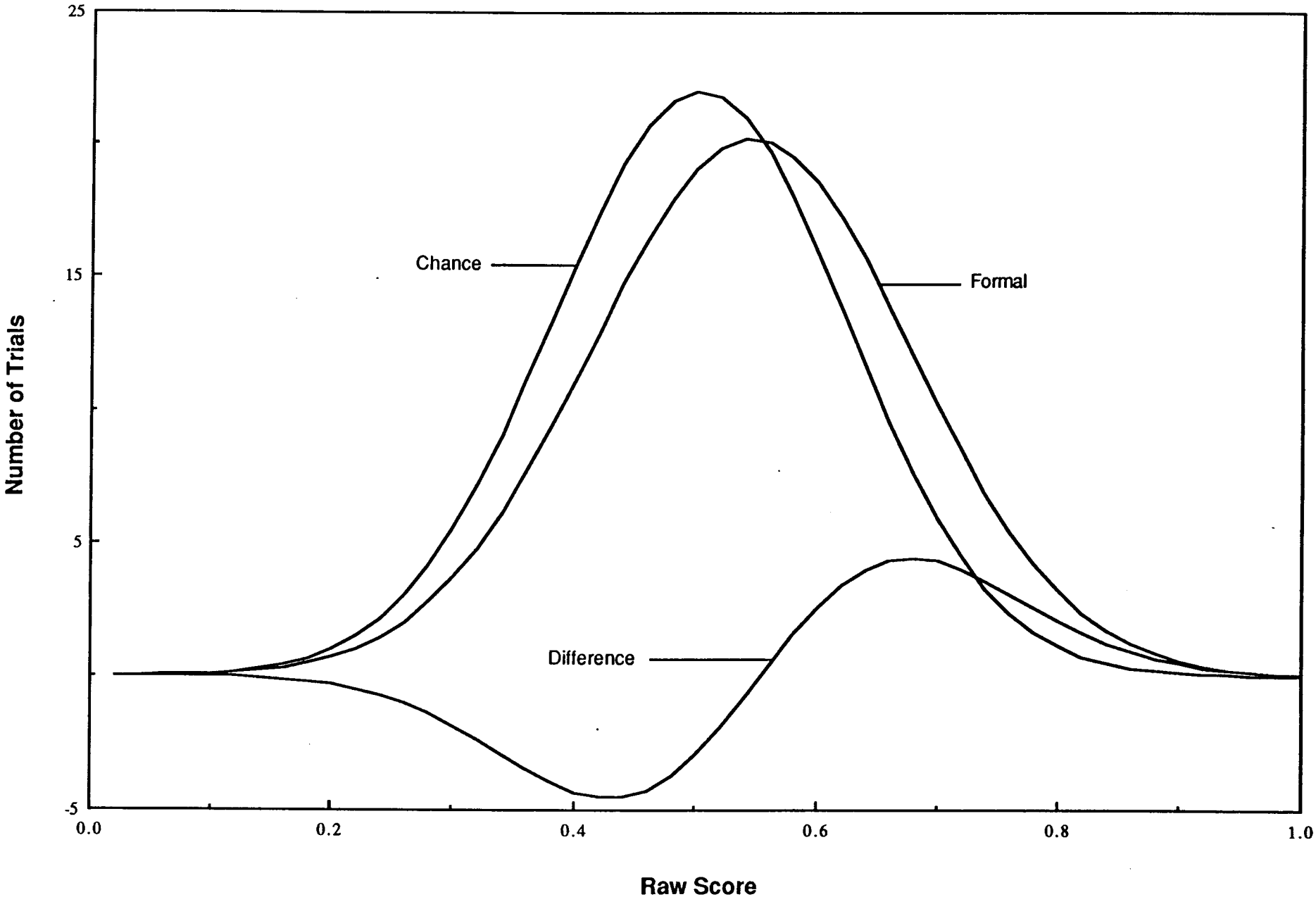


Figure 6: Normal Fits to Formal PRP Data and Chance, with Difference

Another informative way to display the data is to plot in chronological order the cumulative deviation of the scores from chance expectation (Fig. 7). From this representation it can be seen that, except for the small number of ex post facto trials at the start, the formal data compound in a stochastically linear fashion to a highly significant terminal probability, confirming that the principal source of the overall anomaly is a systematic accumulation of marginal extra-chance achievement, rather than a small number of extraordinary trials superimposed on an otherwise chance distribution. This behavior is consistent with results of the various human/machine experiments conducted in our laboratory and has important implications for any attempts to model such anomalous phenomena.[14]

C. Ab Initio vs. Ex Post Facto

Beyond the disparity between the formal and non-formal data, the other striking distinction in the results displayed in Table B is that between the yields of the ex post facto trials and those encoded ab initio. This can also be seen in Fig. 7, where the larger positive slope at the beginning of the cumulative deviation trace is directly attributable to the 59 formal ex post facto trials described in Section II.D. A 2x2x2 factorial analysis of variance covering the three binary distinctions of ab initio vs. ex post facto encoding, single vs. multiple percipients, and volitional vs. instructed target designation indicates that virtually all of the variance is attributable to the ex post facto vs. ab initio factor

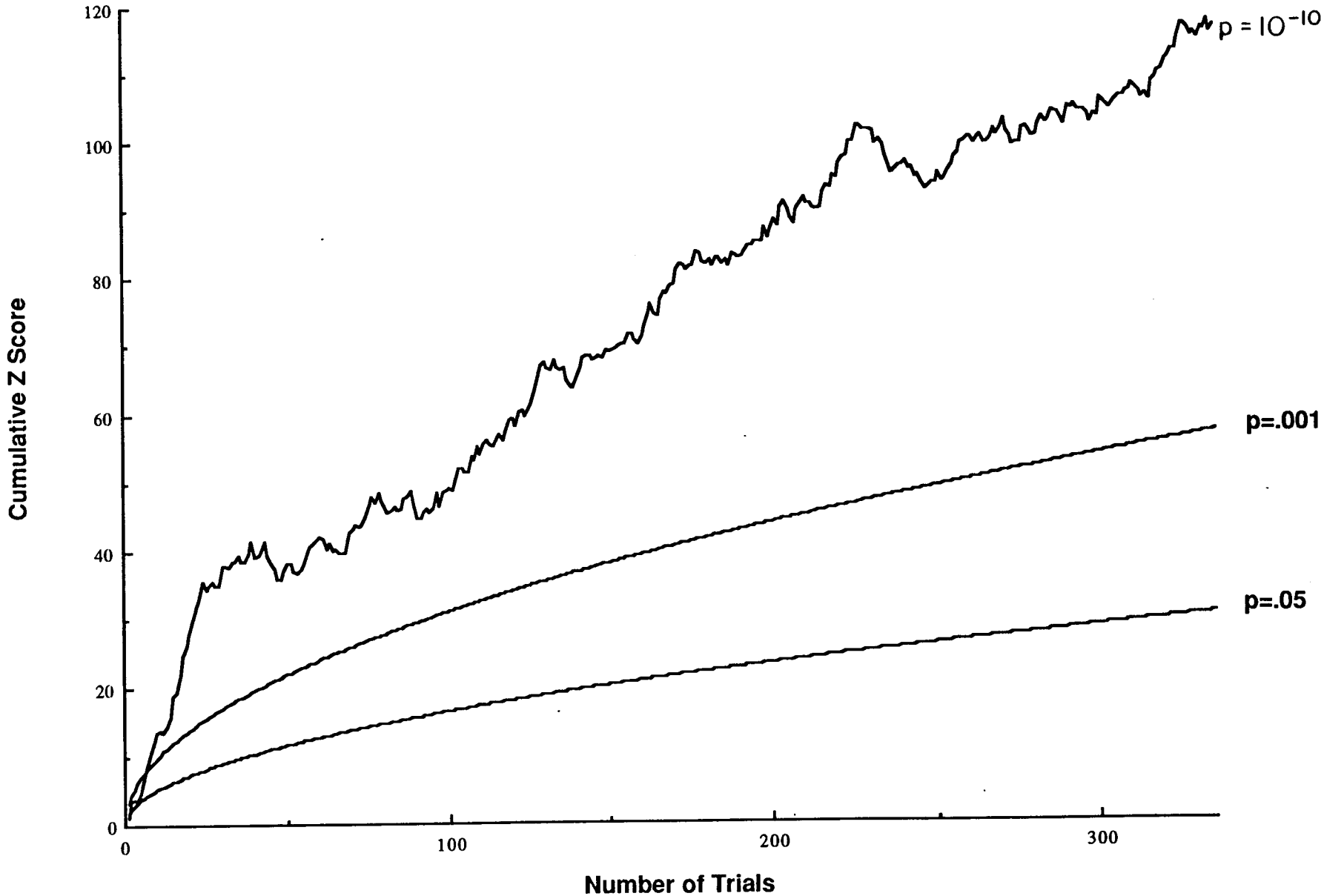


Figure 7: Cumulative Deviation of 336 Formal PRP Trials

($F = 8.866$, $p = .003$). By direct t-test, these two subsets also are significantly distinct ($t = 2.914$).

A number of features of these two data sets that could possibly account for the higher yield of the earlier data will be discussed in more detail in Section V. The more immediate concern, however, is to determine to what extent the yields of the full data base and its various subsets may be artificially inflated by the inclusion of the ex post facto trials. One example of such confounding influence is evident as a disproportionately high effect size in the "Chicago" regional subset of Table B, all 31 trials of which were encoded ex post facto.

The most direct way to preclude any possibility of spurious enhancement of the overall results, or of any of its subsets, from this source is simply to exclude the ex post facto data. The remaining body of 277 ab initio trials, constituting over 82% of the data base, remains highly significant ($z = 4.378$), and retains sufficient population for independent evaluation of its various subsets. The cumulative deviation trace of these ab initio trials (Fig. 8) displays a virtually linear accumulation of marginal effects, albeit of a more modest slope than that of the first 59 ex post facto trials of Fig. 7. Alternatively, when the individual ab initio trial z-scores are plotted in chronological order, the best fit line obtained from a least squares regression analysis entails only one significant coefficient -- a constant mean shift -- again implying a regularity of yield across the entire subset, with no apparent

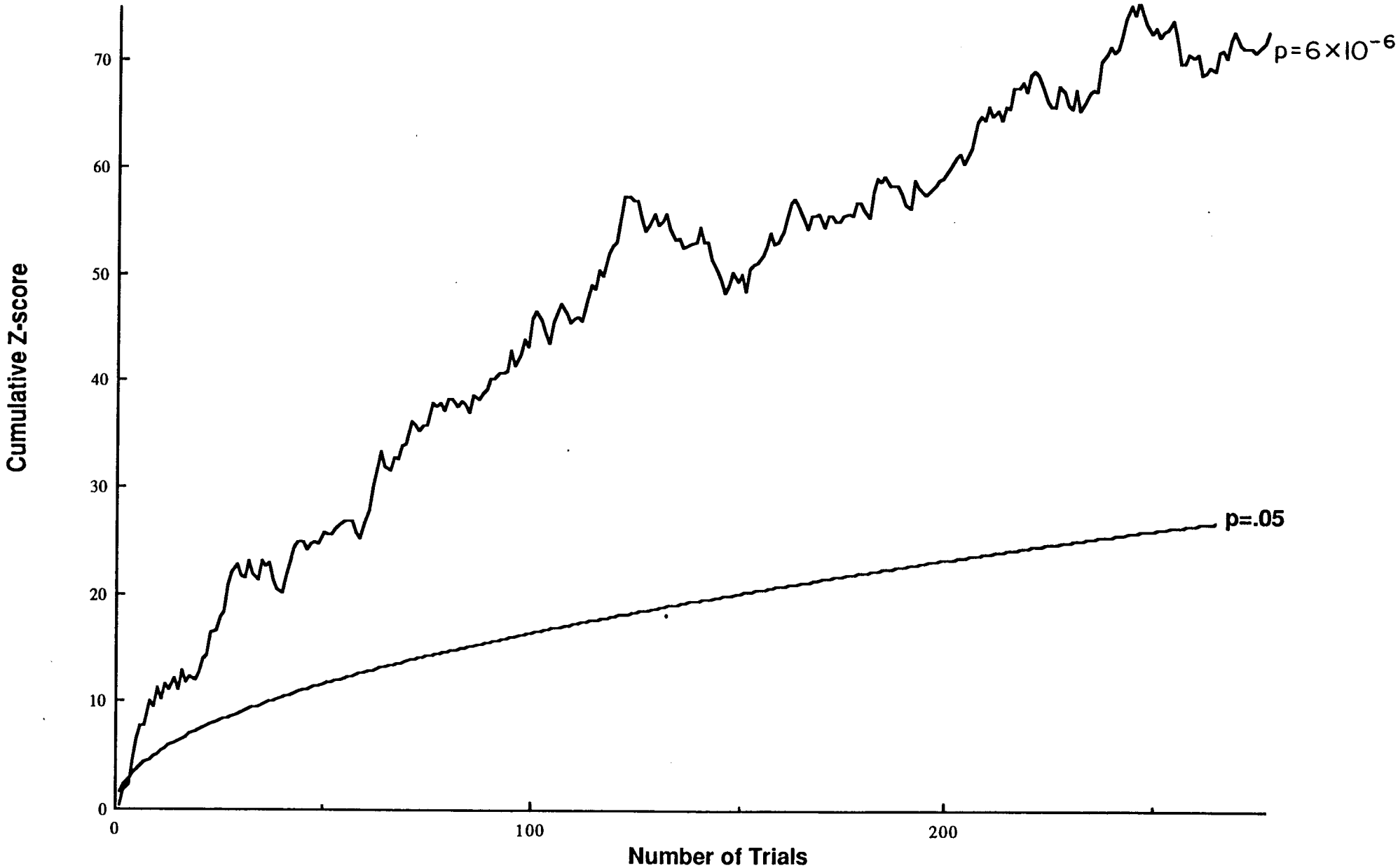


Figure 8: Cumulative Deviation of 277 Formal, Ab Initio PRP trials

decline or learning effects.

Table C summarizes the statistical results of the total ab initio data base and its various subsets, and Fig. 9 displays the 99% confidence intervals for each group. Analysis of variance now confirms that none of the secondary variables or the interactions among them contribute significantly to the overall effect. It might be worth noting, however, that the effect size of the ab initio instructed subset is slightly larger than that of the volitional group, a feature relevant to the possible encoding biases discussed in Section III.A, in the sense that one might anticipate a somewhat higher yield for trials in which agents select their own target sites. Contrary to this expectation, the volitional protocol appears to impose some slight, albeit insignificant, disadvantage.

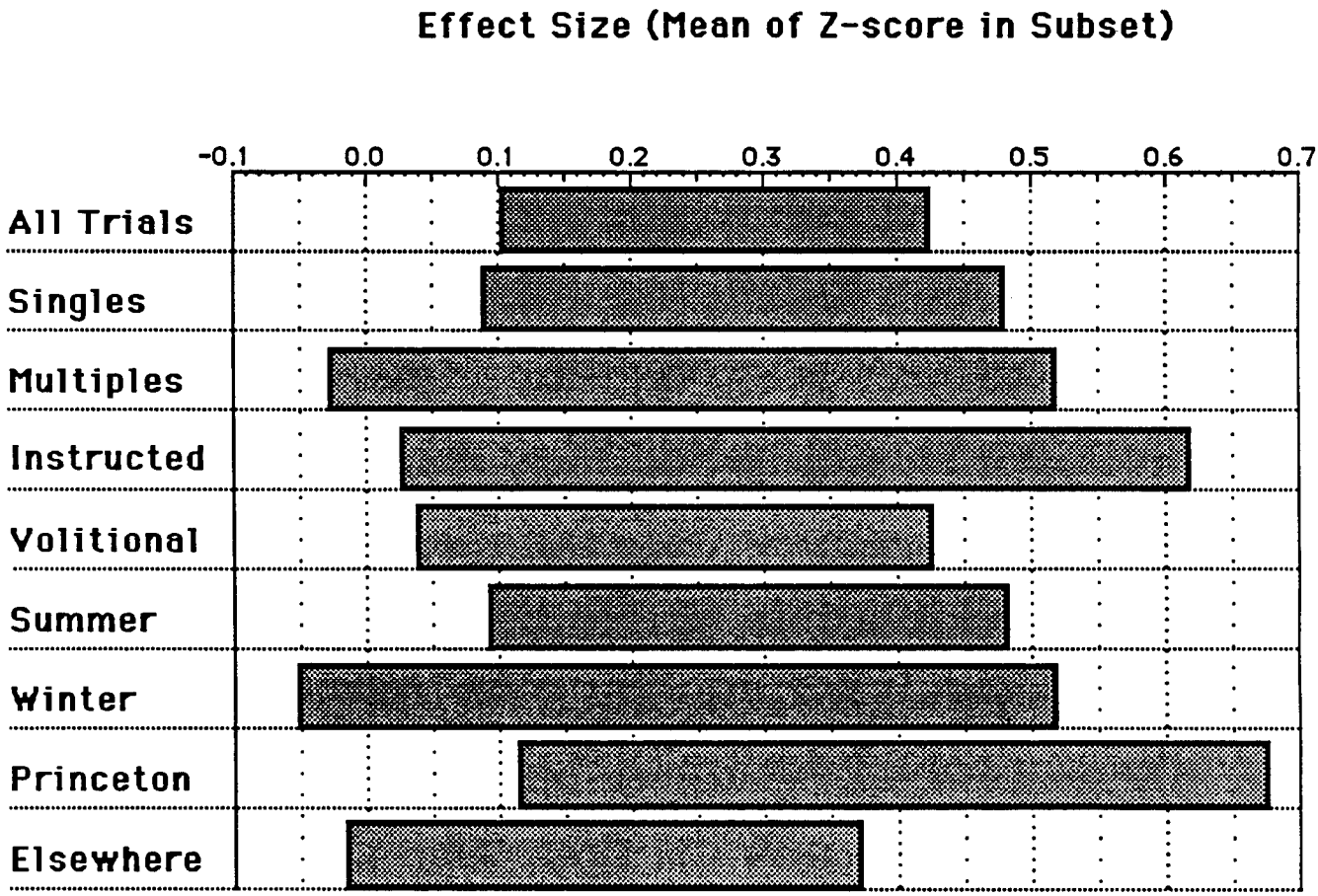
D. Agent/Percipient Pairs and Individual Contributions

As mentioned earlier, specific agent/percipient subsets also need to be examined relative to their own local α_i and mismatch distributions for evidence of possible encoding artifacts. The mean effect sizes and corresponding composite z-scores for each pair with five or more trials have been thus calculated and, because of the poor estimates of variance in the mismatch distributions of data sets with small N, two additional comparison calculations have also been made. Table D gives the composite z-scores by all three methods for 29 pair subsets, constituting 274 of the formal trials, and Fig. 10 displays these as cumulative deviation traces plotted in the same sequence as

Table C

Ab Initio Data Summary

Subset	No. Trials	Mean Score	Effect Size	99% Conf. Intervals	SD of z	Composite z-score	Prob. (1-tail)	# Trials $p < .05$	% Trials $p < .5$
All	277	.5345	.263	$\pm$ .161	1.033	4.378	6×10^{-6}	31(5)	59.2
Single	194	.5370	.284	$\pm$ .197	1.063	3.949	4×10^{-5}	24(6)	56.2
Multiple	83	.5321	.243	$\pm$ .275	0.974	2.215	.013	5(1)	63.9
Instructed	94	.5416	.322	$\pm$ .296	1.115	3.122	9×10^{-4}	11(5)	60.5
Volitional	183	.5308	.233	$\pm$ .194	1.020	3.148	8×10^{-4}	21(1)	59.6
Summer	195	.5374	.287	+ .195	1.058	4.013	3×10^{-5}	24(4)	61.5
Winter	82	.5308	.233	$\pm$ .285	1.002	2.107	.018	7(2)	56.1
Princeton	106	.5504	.394	$\pm$ .281	1.125	4.060	2×10^{-5}	14(4)	62.3
Elsewhere	171	.5243	.180	$\pm$ .197	1.000	2.348	.009	16(1)	58.5



PRP Ab Initio Scores: 99% Confidence Intervals for Subset Effect Sizes

Figure 9

Table D

z-Scores for Individual Agent/Percipient Pairs

			Composite z-Scores		
Agent	Percipient	N	Local α	Local Dist.	Non-local
10	1	6	1.705	1.587	1.944
10	7	10	1.252	1.193	0.789
10	11	5	0.542	0.329	0.549
10	13	6	1.399	1.925	0.776
10	14	17	0.672	0.590	0.925
10	23	5	1.366	2.600	0.821
10	25	5	0.314	0.112	-0.812
10	35	7	3.652	3.635	3.550
10	41	12	0.305	0.324	0.046
10	57	24	1.141	1.152	2.075
10	64	9	0.759	1.223	0.272
10	68	7	0.763	0.526	0.814
10	70	14	0.908	1.000	0.657
10	81	5	-0.019	-0.784	-0.845
14	10	19	-0.208	-0.240	-0.115
17	10	6	5.182	4.613	5.175
25	10	9	0.189	-0.264	0.765
41	10	11	1.754	1.682	0.765
41	14	8	1.633	1.713	1.108
41	36	7	0.506	-0.420	0.798
41	47	16	0.879	0.404	1.627
41	69	8	0.667	1.006	0.692
41	80	5	0.867	0.201	0.783
57	10	5	2.623	2.926	2.049
69	41	17	0.603	0.540	0.392
71	25	6	1.062	0.234	1.472
81	47	12	1.198	0.266	1.896
82	10	7	-0.623	-1.061	-1.122
94	10	6	1.271	0.815	1.214
Total Accumulation:		274	5.516	4.723	5.215

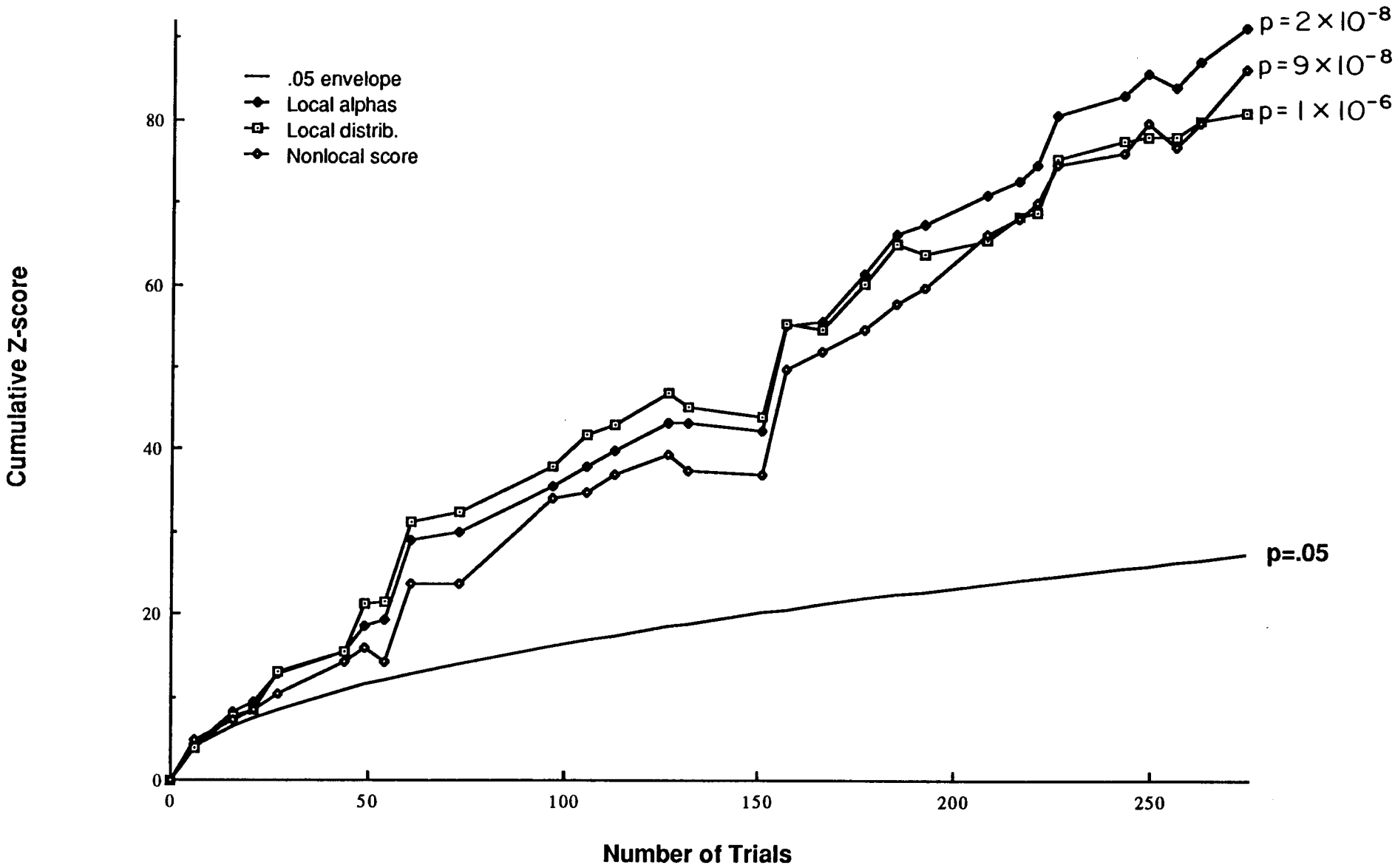


Figure 10: PRP Cumulative Deviations for 29 Agent/Percipient Pairs

the table. (The remaining 62 trials in the formal data base were performed by pairs with less than five trials each, and could not be individually calculated with any confidence.)

In both the table and the graph, "Local α" refers to scores calculated with the α_i of the subset with statistical reference to the universal chance distribution; "Local Distribution" scores are calculated with local α_i and referred to the local mismatch distribution; and "Non-local" scores are calculated with the global α_i derived from all targets in the full data base, and are referenced to the universal distribution.

With a few exceptions, which can largely be attributed to the smaller N's, both the table and the graph show a reassuring degree of agreement among these three potentially disparate scoring strategies, especially in the overall composite results. Once again, virtually linear cumulative deviation trends can be seen which result from an accumulation of small yields from most of the participant pairs, rather than being attributable to only a few outstanding combinations. The comparable scale of overall effect in the individual pair data bases scored by these three different methods is further evidence against encoding artifact as any significant component of the PRP anomaly.

This point is buttressed by Table E and Fig. 11, which show the rank-ordered effect sizes for each of the 28 percipients who contributed more than one trial to the data base, and Table F, which does the same for each of the 15 agents. Despite the apparent non-normality of the respective distributions of effect size, most of which can be attributed to the ex post facto/ab

Table E

Individual Percipient Contributions in Order of Effect Size

(Local α_i/Universal Chance Calculations)

Percipient**	No. of Trials	Effect Size	Composite z-score	SD of z
8*	2	1.758	2.486	0.556
96*	3	1.460	2.529	0.974
35*	7	1.380	3.652	0.777
3*	3	1.364	2.362	1.068
2	2	.980	1.386	0.888
9	2	.757	1.071	0.667
1*	6	.696	1.704	1.075
23	5	.611	1.366	0.645
13	6	.571	1.399	0.789
4	3	.460	1.199	2.078
10*	77	.400	3.507	1.159
7	10	.396	1.252	1.842
80	5	.388	0.867	1.268
14*	28	.348	1.842	1.248
70	15	.307	1.188	0.912
68	7	.289	0.763	1.036
25	11	.263	0.871	1.216
88	3	.256	0.443	1.498
55	4	.251	0.502	0.589
11	5	.242	0.542	0.710
69	8	.236	0.667	1.488
47	28	.234	1.236	1.142
57	25	.214	1.070	0.874
36	7	.191	0.506	0.760
64	11	.178	0.589	1.149
41	33	.142	0.817	0.866
94	5	-.006	-0.013	0.549
81	5	-.008	-0.019	0.595
All	326	.366	6.602	

*Individual data bases statistically significant at the 5% level.

**Includes only percipients who participated in more than one trial.

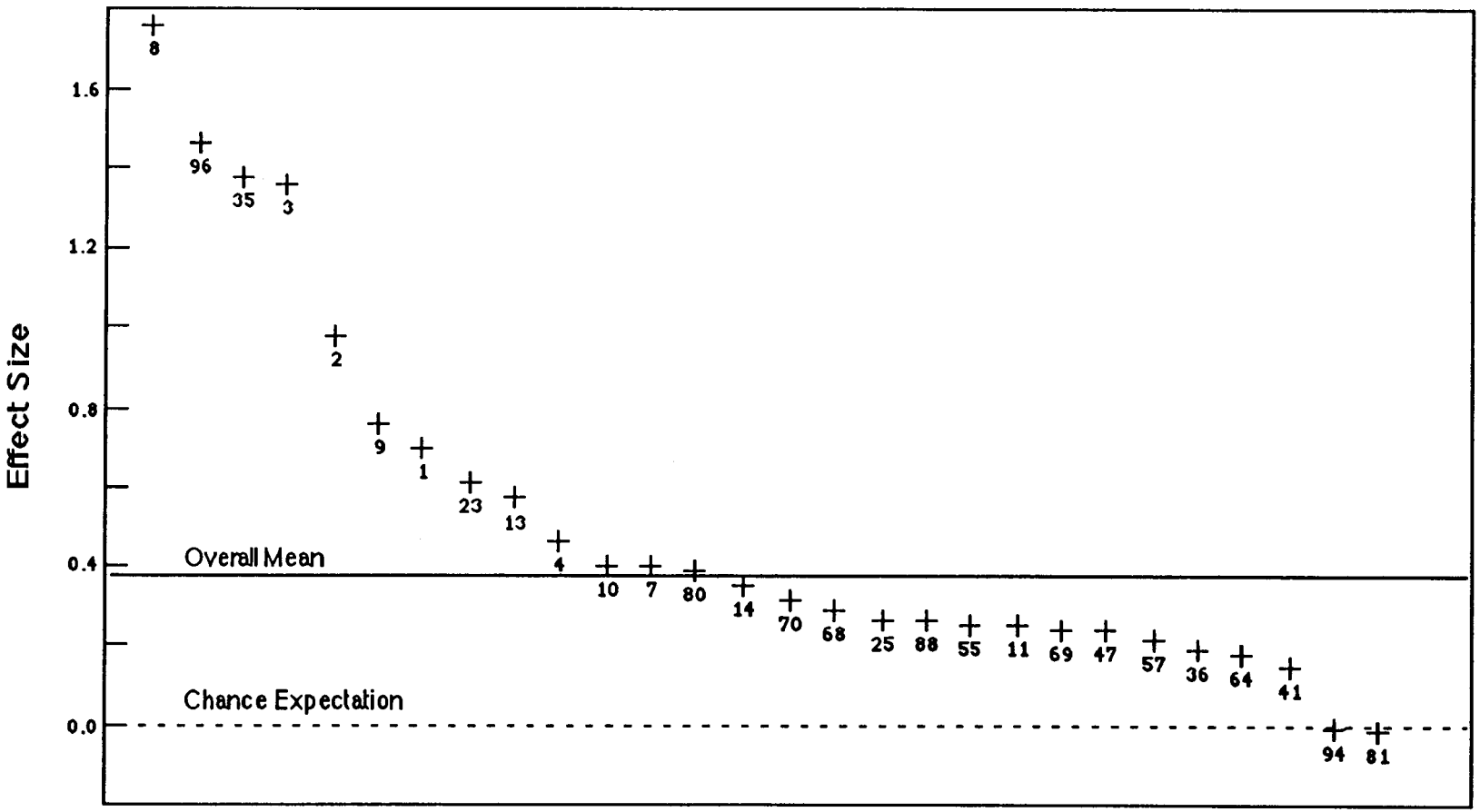


Figure 11: PRP Individual Effect Sizes, Labeled by Percipient Number

Table F

Individual Agent Contributions in Order of Effect Size

(Local α_i/Universal Chance Calculations)

Agent**	No. of Trials	Effect Size	Composite z-score	SD of z
17*	6	2.115	5.182	0.919
87*	3	1.254	2.171	1.023
83*	3	1.222	2.117	0.454
57*	5	1.173	2.862	1.173
72	4	.578	1.157	0.738
94	6	.519	1.271	0.997
71	6	.433	1.061	1.262
10*	167	.389	5.021	1.058
41*	59	.373	2.862	1.179
81	12	.346	1.199	1.260
80	8	.198	0.559	1.017
25	9	.063	0.188	1.269
69	21	.029	0.131	0.890
14	19	-.048	-0.208	1.110
82	7	-.235	-0.623	0.819
All	335	.374	6.842	

*Individual data bases statistically significant at the 5% level.

**Includes only agents who participated in more than one trial.

initio disparity, the positive contribution to the overall results from the vast majority of participants is again evident, with some 25% of the percipients and 40% of the agents producing statistically significant results. (Alternatively, 21% of the percipient/agent pairs achieve significance.) Except for two percipients and two agents, all other participants generated net positive effects, and three of these four exceptions produced positive results when functioning in the complementary role. (The fourth, agent 82, never performed as a percipient.)

To factor out the influence of individual data base size on the overall yield, only the first trials from each of the 38 percipients contributing to the formal data base were calculated as an independent subset and cumulated, as shown in Fig. 12. Even with the small size of this group of trials, the trace displays the same linear tendency as the full data base, reaching a terminal composite z-score of 3.890. Again it is clear that no one individual, or group of individuals, is dominating the observed results.

E. Spatial and Temporal Dependencies

Among the major findings of our earlier studies was the apparent independence of the yield on the spatial or temporal distance between the percipient and the target. These parameters may be re-assessed by the revised scoring process for the larger data base now in hand. Figures 13 and 14 are scatterplots of the 336 formal individual trial z-scores as functions of distance and time, respectively, with best fit and chance expectation lines

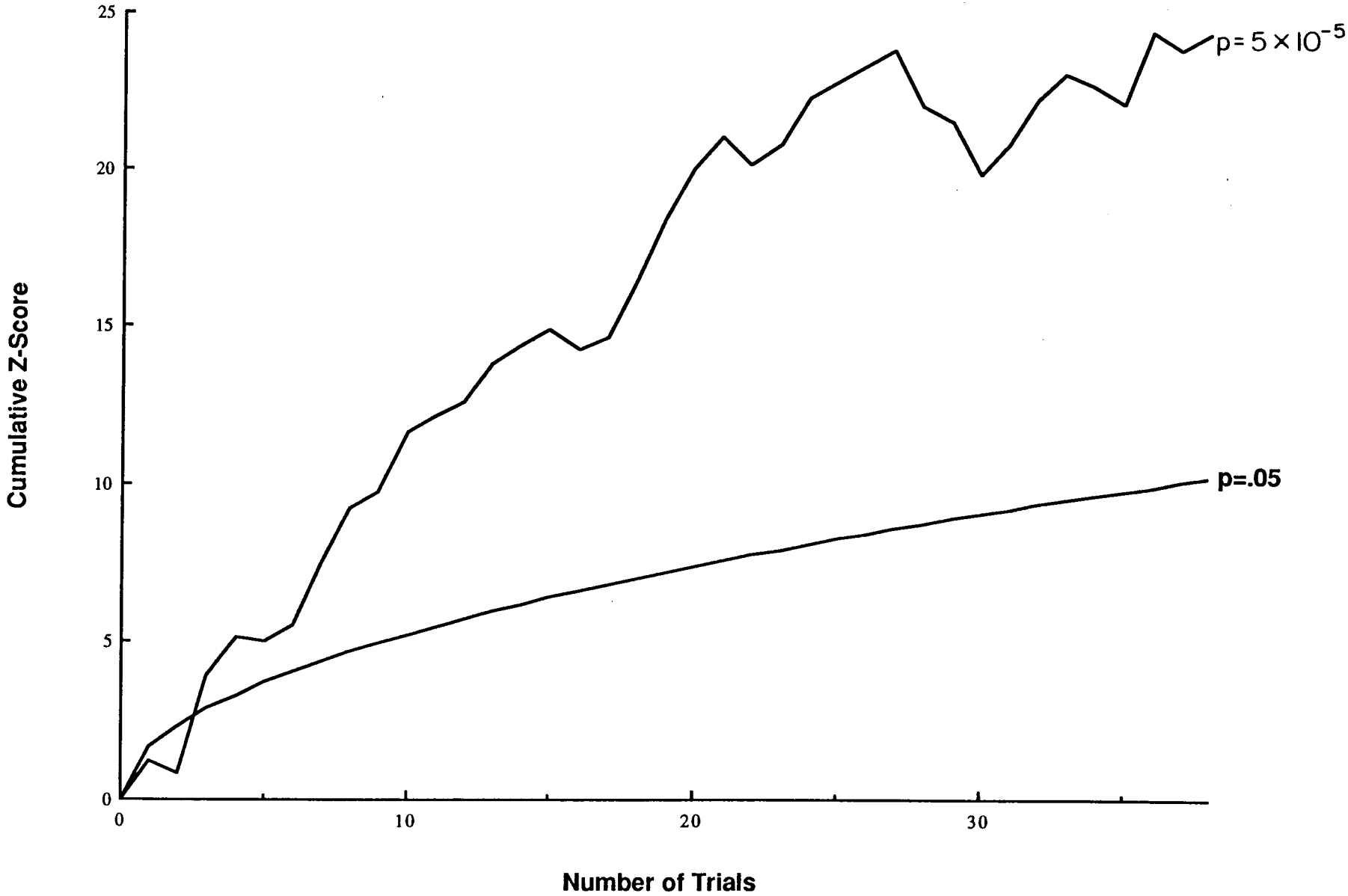


Figure 12: PRP Cumulative Deviation, 38 Percipients, First Trial Only

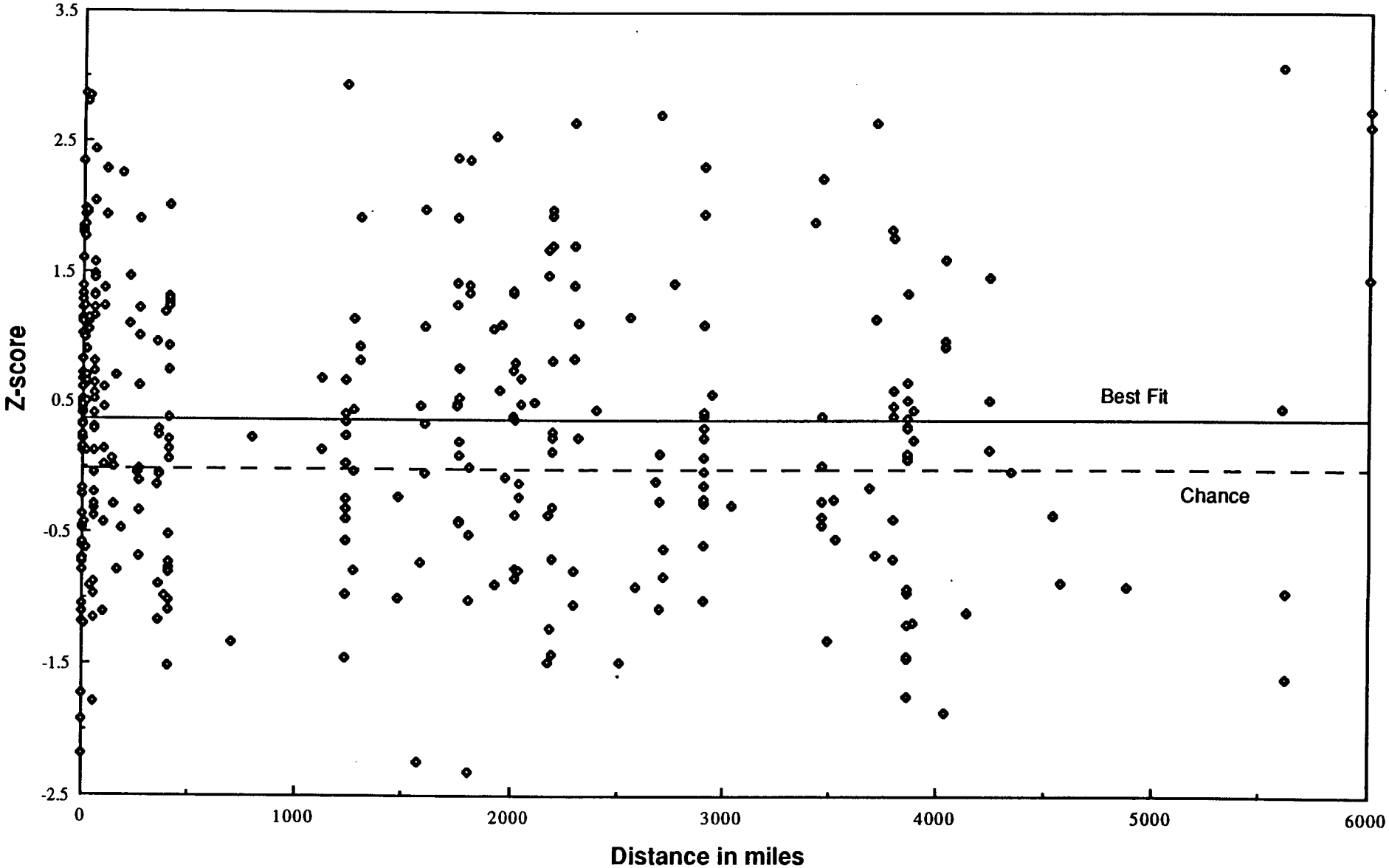


Figure 13: 336 Formal PRP Trials as a Function of Distance

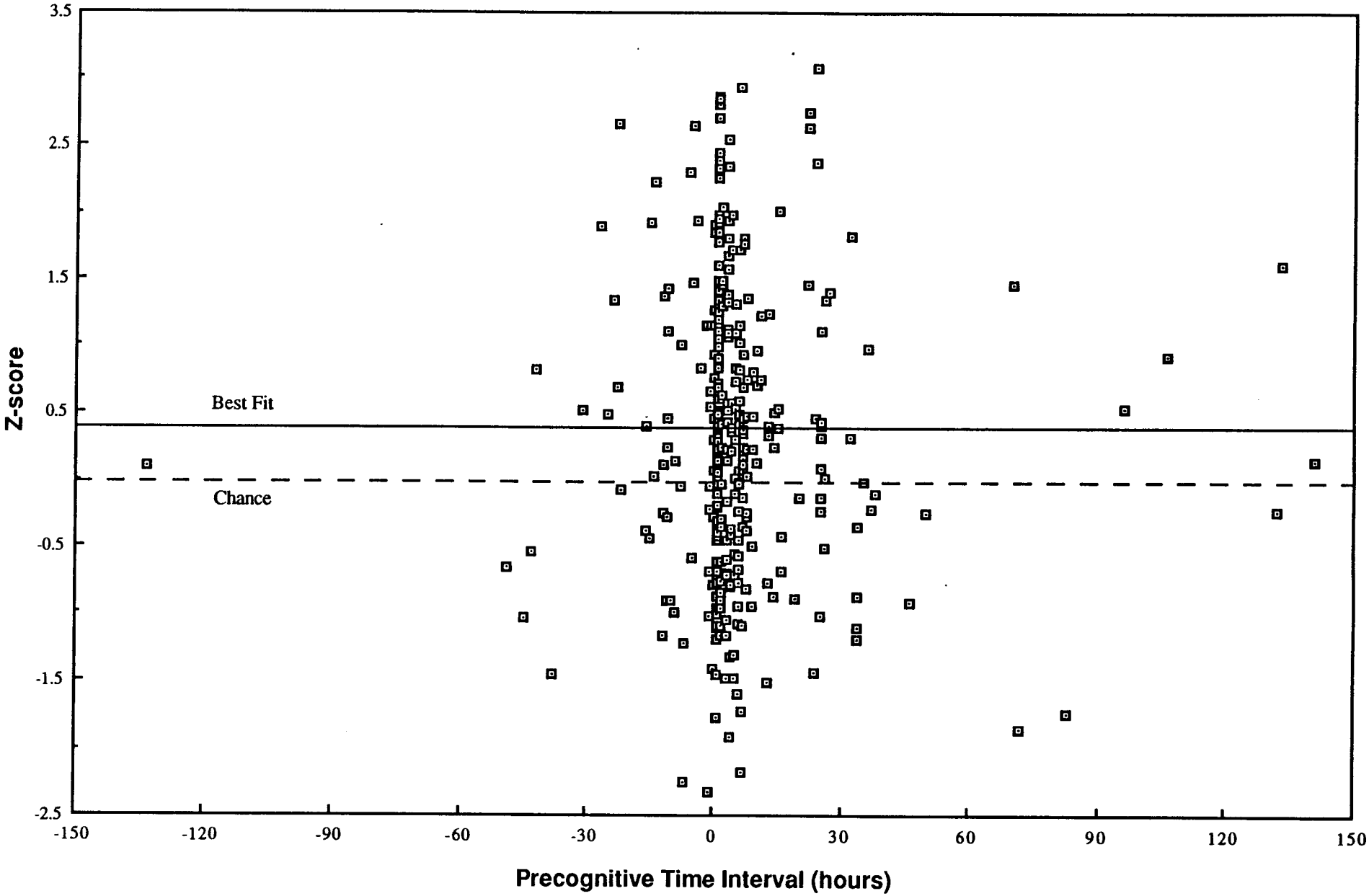


Figure 14: 336 Formal PRP Trials as a Function of Time

superimposed. Multiple regression analyses again indicate that only the constant terms -- the overall mean shifts -- are statistically significant, with the linear and higher order terms statistically indistinguishable from zero. Thus, simple zero-order fits are appropriate for representing these relationships. In other words, within the ranges of this data base there are no significant correlations with either distance or time.

This issue may also be approached by calculating independent subsets of the more extreme spatially and temporally remote trials and comparing them with those less distant. Table G displays the results for such independently calculated trial subsets with spatial separations of more than, and less than, 1000 miles between target and percipient. Numbers in parentheses indicate the results when only ab initio data are considered. In neither case are the differences significant [t = 1.235 (t = 0.055)].

Table G

Independent Subsets by Distance

	No. Trials	Effect Size	Composite z-score
d>1000 miles	180(155)	.272(.265)	3.652(3.301)
d<1000 miles	156(122)	.420(.258)	5.244(2.852)

A similar exercise can be performed for trial subsets where the time between perception and target visitation was equal to or greater than ±12 hours and for those less than ±12 hours. These results are displayed in Table H. In both cases, effect sizes in the two groups are again statistically indistinguishable [t = 0.314 (t = 0.544)].

Table H

Independent Subsets by Time

	No. Trials	Effect Size	Composite z-score
t<± 12 hrs.	263(206)	.360(.288)	5.845(4.133)
t≥± 12 hrs.	73(71)	.313(.210)	2.678(1.770)

V. Anecdotal Indications

A major implicit goal of all of the PRP research reported here and previously has been to identify experimental and analytical strategies for increasing the yield and replicability of the phenomenon. Each of the protocol subdivisions discussed above was posed in the hope of distinguishing particular parameters that were more propitious for this purpose. Yet, of these, only the ex post facto encoding has been found to show any substantial favorable impact on the yield and, unfortunately, our ability to identify the fundamental cause of this is confounded by the number and nature of the procedural differences.

First, since all of the ex post facto trials preceded the ab initio group, one might simply postulate a "decline effect" between the two sets. However, the regularity of yield of the latter group alone over several years of experimentation would suggest rejection of this hypothesis.

Second, most of the ex post facto perceptions were tape recorded, as percipients verbalized their impressions in a free-association style over the full ten to fifteen minute period of the trial. As a result, the majority of these transcripts contain considerably more descriptive material than the hand-

written versions typical of the later ab initio trials. Once the descriptor questions were in use, although percipients still were urged to allow their imagery free rein, it was inevitable that they would tend to focus their awareness to varying degrees on those features specifically addressed by the questions, thereby constraining the more free-flowing, diffuse scanning style apparent in many of the earlier transcripts.

Before concluding that the use of descriptor questions per se is an inhibiting factor, however, it should also be recalled that in the development of the analytical judging methodology the choice of descriptor questions was strongly influenced by the contents of these same early trials. Hence, it should not be surprising to find that the ex post facto trials gain some advantage when quantified by these criteria.

Finally, it may be worth noting that the ab initio data were generated for the primary purpose of testing and refining the established analytical scoring methods, whereas the ex post facto trials were empirical attempts to replicate earlier studies and to explore the limits of the phenomenon itself. The experimenters' and participants' goals and attitudes, being more oriented toward analytical concerns in the later ab initio experiments, may thus possibly have had an inhibitory influence on the more aesthetic dimensions of the process, consistent with some consciousness "uncertainty" relationship like that suggested in our theoretical model.(10,14)

Again, the subjective nature of these speculations precludes firm identification of cause and effect in the ex post facto/ab

initio scoring disparity without much further systematic experimentation. Yet these, like other anecdotal observations derived in the course of our more than ten years of PRP experimentation, may have some value for the impressionistic insights they can provide. As another example, in a number of the exploratory trials, although the date was pre-arranged, the time of target visitation was intentionally left unspecified; in a few others, the agent deliberately altered the time without the percipient's knowledge. All told, 42 trials were conducted where the percipient had no knowledge of the actual time of the agent's visit. In Series 11 (Ref. 7 and Appendix A-I), which consisted of 20 time-unspecified trials, the percipient mistakenly began the experiment on the day after it was officially scheduled to start, and as a result the first perception actually addressed the second target, and so on. In the formal analysis this series yielded a net negative score. However, if the first target is eliminated and the matches are displaced by one, thereby comparing the perceptions with the target sequence the percipient actually was addressing, the remaining 19 "trials" yield a mean effect size of .276, which is quite comparable to the formal ab initio experimental value of .263. Combining these displaced "trials" with the other 22 time-unspecified trials, and calculating the entire group of 41 as an independent subset, produces an effect size which is also competitive at .245. Clearly, such post hoc manipulations cannot be offered as valid data, but they do suggest that exact specification of the time of target visitation may not be an essential component of the

experimental protocol.

Other potentially informative anecdotes include occasional reports by agents of their attention being drawn to features outside their intended circumscription of the target and their later discovery that these components had been accurately described by the percipient while the designated target was ignored. In a few cases, percipients appear to produce accurate descriptions of agents' whereabouts at other times on the day of the trial, which bear no resemblance to the formal target. In several of these instances the agent later reported having been in a heightened emotional state, or totally absorbed in the activity of the "perceived" period. As yet another example, in two void attempts where the agent forgot the assigned task and no target was identified, the percipients complained of being unable to obtain any impressions -- one actually suggested that the agent "must have forgotten."

Such reports, if confirmed by experiment, would suggest that the agent, or more accurately, the agent's attentional state, may serve as a reference "signal" with which the percipient's consciousness may establish a "resonance," and thereby a channel for sharing information. The concept of such information sharing, independent of physical time and distance, might be less problematical from a theoretical standpoint than what at present tends to be interpreted as an information "transmission."

The next phase of our PRP program will attempt to address some of these informal observations more systematically in the framework of formal studies. As a first step, we are currently

developing a quaternary scoring method called "Feature Importance Discriminating Option" (FIDO), that will relieve the constraints of the forced choice binary response mode by permitting participants the options of distinguishing the relative importance of any descriptive feature, and "passing" on any that are too ambiguous for clear response. Percipients will also be encouraged to generate more extensive free-response descriptions before turning to the descriptor questions, and emphasis will be placed on obtaining larger data bases from given percipients and percipient/agent pairs so that comparisons of individual patterns of achievement can better be assessed. Formal manipulation of variables such as specified vs. unspecified time of target visitation and physical targets vs. agents' visual imagery are also under consideration.

VI. Summary

The additional body of experimental data and the various analyses described in this report concur with our earlier results in indicating highly significant and replicable anomalous information acquisition under the PRP protocols employed. Specifically, we continue to find:

1. The quantitative analytical scoring techniques employed are effective in capturing much of the informational content of the free-response PRP process, with results statistically comparable with those obtained by impressionistic human judging techniques.

2. The overall results of the 336 formal trials in the data base are highly significant compared to chance, regardless of the particular scoring recipes employed.

3. The data compound steadily to increasingly significant terminal probabilities, indicating that the overall effect is the result of statistically consistent, albeit marginal, contributions across the entire data base, and is not dominated by a few highly successful trials.

4. The results are insensitive to the mode of target designation, whether randomly assigned or volitionally selected by the agent.

5. The number of percipients addressing a given target has no significant effect on the success of the experiment.

6. The yield is insensitive to the physical distance separating the percipient from the agent/target, up to several thousand miles.

7. The yield is insensitive to temporal separation between perception and target visitation, up to differences of plus or minus several days.

8. The earliest trials, which provided the basis for developing the set of descriptor questions underlying the analytical judging methodology, score significantly higher than those trials encoded ab initio by participants at the time of the trial.

Nonetheless, the ab initio data alone are highly significant when evaluated independently.

9. The possibility that the statistical yield of the matched target scores might be artificially inflated by correlations in the encoding tendencies of the participants can effectively be precluded by localized treatment of the descriptor probabilities and the empirical chance distributions.

10. Examination of the results from each participant and each percipient/agent pair contributing to the data base indicates that the capacity for producing the PRP phenomenon is widespread among these non-gifted, untrained volunteers, and that no particular individuals or pairs dominate the results.

Precognitive remote perception, in the analytical format practiced here, thus stands as a valid and instructive component of our overall anomalies research program. As noted above and elsewhere,[10] PRP phenomena provide complementary indications to those offered by our human/machine experiments on the basic nature of the interaction of consciousness with physical systems and processes, and thereby on the even grander problem of the role of consciousness in the establishment of experiential reality. Taken in this complementarity, these two genre of experiments continue in an iterative dialogue with our proposed theoretical model[10,14], helping us to converge on more accurate interpretations of the empirical results, more effective new

experiments, and more complete understanding of the fundamental processes of the mind and its world.

APPENDICES

Appendix A

I. Individual Trial Scores

The following tables present the normalized scores and associated z-scores for each of the 411 individual trials in the full data base. Data from each of the four major subsets--formal *ab initio*, formal *ex post facto*, exploratory, and questionable -- are calculated independently, using the appropriate local α_i and referenced to the universal chance distribution described in the body of this report (N = 106,602; mean = .5025; standard deviation = .1216). Each trial is indexed by series and trial number, consistent with the format followed in Ref. 7, and the individual trial scores and z-scores as calculated by Method B in that earlier analysis (Appendix C-II) are provided for purposes of comparison. Asterisks denote trials significant by the 1-tailed .05 criterion ($z > 1.645$), and asterisks in parentheses (*) indicate negative z-scores in excess of -1.645. The data labelled Series 99 are those identified as Misc. Trials in the earlier report; Series 999 consists of six additional miscellaneous trials, one of which was exploratory. Readers are referred to Ref. 7 for details of the specific protocol parameters for Series 1 - 39, and to part II of this Appendix for details of the later series. These tables confirm the findings discussed in the text, that the revised scoring method yields only trivial changes from the earlier format and that the overall interpretation of the results remains unaltered.

Table A.1

Individual Trial Scores

Formal Ab Initio Data

Series No.	Trial No.	Revised Method Score	Revised Method z-Score	Old Method B Score	Old Method B z-Score
5	1	0.551	0.399	0.545	0.335
5	2	0.455	-0.389	0.468	-0.305
5	3	0.327	-1.443	0.332	-1.426
5	4	0.384	-0.970	0.402	-0.846
5	5	0.430	-0.593	0.416	-0.728
5	6	0.513	0.084	0.500	-0.038
6	1	0.590	0.720	0.564	0.494
6	2	0.542	0.327	0.536	0.259
6	3	0.474	-0.237	0.480	-0.205
6	4	0.858	2.920 *	0.854	2.901 *
7	1	0.581	0.643	0.587	0.688
7	2	0.584	0.670	0.574	0.577
7	3	0.441	-0.508	0.447	-0.474
7	4	0.551	0.399	0.549	0.367
7	5	0.655	1.252	0.660	1.288
7	6	0.693	1.569	0.721	1.794 *
7	7	0.648	1.193	0.673	1.400
7	8	0.405	-0.803	0.436	-0.564
7	9	0.663	1.318	0.663	1.312
7	10	0.364	-1.140	0.353	-1.253
9	1	0.364	-1.136	0.361	-1.185
9	2	0.405	-0.802	0.393	-0.921
9	3	0.558	0.459	0.544	0.327
9	4	0.409	-0.768	0.412	-0.762
9	5	0.557	0.448	0.541	0.306
9	6	0.530	0.223	0.518	0.117
9	7	0.603	0.827	0.611	0.886
9	8	0.290	-1.748(*)	0.294	-1.744(*)
9	9	0.233	-2.216(*)	0.256	-2.057(*)
9	10	0.485	-0.148	0.460	-0.369
9	11	0.627	1.024	0.636	1.094
9	12	0.444	-0.482	0.447	-0.478
9	13	0.558	0.455	0.559	0.456
9	14	0.259	-1.998(*)	0.285	-1.818(*)
9	15	0.522	0.164	0.535	0.257
9	16	0.570	0.557	0.569	0.538
9	17	0.457	-0.376	0.436	-0.562
9	18	0.719	1.780 *	0.711	1.713 *
9	19	0.524	0.180	0.531	0.223
9	20	0.427	-0.622	0.448	-0.473

Table A.1--contd.

Series No.	Trial No.	Revised Method Score	Revised Method z-Score	Old Method B Score	Old Method B z-Score
10	1	0.555	0.435	0.551	0.391
10	2	0.667	1.351	0.702	1.641
10	3	0.557	0.445	0.561	0.469
10	4	0.795	2.408 *	0.790	2.364 *
10	5	0.738	1.937 *	0.728	1.858 *
10	6	0.655	1.257	0.661	1.301
13	1	0.497	-0.046	0.493	-0.094
13	2	0.788	2.345 *	0.804	2.481 *
13	3	0.420	-0.676	0.461	-0.359
13	4	0.735	1.909 *	0.730	1.868 *
13	5	0.373	-1.068	0.384	-0.993
15	1	0.671	1.386	0.669	1.367
15	2	0.424	-0.647	0.431	-0.607
15	3	0.635	1.089	0.634	1.074
15	4	0.382	-0.988	0.391	-0.942
15	5	0.725	1.827 *	0.717	1.760 *
15	6	0.362	-1.154	0.351	-1.273
15	7	0.564	0.505	0.554	0.410
15	8	0.471	-0.258	0.479	-0.209
15	9	0.584	0.670	0.591	0.722
15	10	0.660	1.296	0.675	1.415
15	11	0.535	0.267	0.552	0.398
15	12	0.758	2.103 *	0.736	1.917 *
16	1	0.520	0.143	0.532	0.230
16	2	0.656	1.266	0.636	1.094
16	3	0.575	0.597	0.585	0.671
16	4	0.802	2.465 *	0.782	2.304 *
16	5	0.648	1.193	0.650	1.210
16	6	0.584	0.670	0.597	0.771
16	7	0.384	-0.971	0.379	-1.040
16	8	0.483	-0.160	0.478	-0.222
16	9	0.685	1.498	0.682	1.475
16	10	0.364	-1.137	0.364	-1.164
16	11	0.419	-0.685	0.417	-0.723
16	12	0.717	1.762 *	0.732	1.888 *
17	1	0.456	-0.380	0.476	-0.238
17	2	0.548	0.376	0.540	0.297
17	3	0.288	-1.761(*)	0.289	-1.785(*)
17	4	0.397	-0.864	0.397	-0.891
17	5	0.462	-0.329	0.482	-0.185
17	6	0.696	1.591	0.700	1.620
17	7	0.665	1.338	0.675	1.418
17	8	0.674	1.413	0.683	1.483
17	9	0.555	0.429	0.559	0.451
17	10	0.508	0.043	0.496	-0.068

Table A.1--contd.

Series No.	Trial No.	Revised Method Score	Revised Method z-Score		Old Method B Score	Old Method B z-Score	
18	1	0.408	-0.779		0.419	-0.708	
18	2	0.567	0.527		0.581	0.632	
18	3	0.529	0.214		0.534	0.250	
18	4	0.476	-0.218		0.463	-0.341	
19	1	0.637	1.107		0.643	1.146	
19	2	0.478	-0.200		0.468	-0.298	
19	3	0.517	0.121		0.496	-0.070	
19	4	0.553	0.417		0.573	0.566	
19	5	0.544	0.343		0.544	0.325	
19	6	0.559	0.465		0.567	0.523	
19	7	0.491	-0.091		0.500	-0.038	
21	1	0.509	0.050		0.513	0.070	
21	2	0.369	-1.100		0.389	-0.958	
21	3	0.433	-0.571		0.423	-0.685	
21	4	0.667	1.351		0.643	1.148	
21	5	0.668	1.357		0.687	1.515	
21	6	0.781	2.290	*	0.794	2.403	*
22	1	0.706	1.675	*	0.712	1.719	*
22	2	0.688	1.526		0.690	1.537	
22	3	0.319	-1.505		0.317	-1.548	
22	4	0.462	-0.336		0.438	-0.552	
23	1	0.656	1.264		0.635	1.083	
23	2	0.490	-0.105		0.510	0.048	
23	3	0.641	1.136		0.623	0.985	
23	4	0.539	0.301		0.574	0.579	
23	5	0.746	2.002	*	0.739	1.947	*
26	1	0.470	-0.269		0.486	-0.151	
26	2	0.441	-0.504		0.458	-0.386	
26	3	0.551	0.398		0.541	0.301	
26	4	0.502	-0.001		0.501	-0.030	
26	5	0.772	2.212	*	0.769	2.194	*
26	6	0.456	-0.384		0.445	-0.493	
27	1	0.553	0.419		0.567	0.516	
27	2	0.409	-0.771		0.410	-0.782	
27	3	0.643	1.153		0.650	1.206	
27	4	0.502	0.006		0.502	-0.016	
28	1	0.414	-0.727		0.419	-0.708	
28	3	0.565	0.514		0.528	0.196	
28	4	0.450	-0.431		0.466	-0.316	
29	2	0.415	-0.721		0.442	-0.516	
29	3	0.708	1.689	*	0.715	1.743	*
29	4	0.453	-0.408		0.457	-0.395	
29	5	0.558	0.460		0.567	0.523	
29	6	0.555	0.432		0.535	0.252	
30	1	0.627	1.021		0.614	0.906	
30	3	0.503	0.002		0.507	0.019	
30	4	0.577	0.616		0.578	0.607	
30	5	0.499	-0.032		0.469	-0.296	

Table A.1--contd.

Series No.	Trial No.	Revised Method Score	Revised Method z-Score	Old Method B Score	Old Method B z-Score
31	1	0.523	0.167	0.545	0.340
31	2	0.732	1.889 *	0.735	1.914 *
31	3	0.329	-1.425	0.337	-1.383
31	4	0.633	1.076	0.635	1.080
32	1	0.677	1.433	0.698	1.602
32	2	0.411	-0.749	0.409	-0.788
32	3	0.824	2.643 *	0.844	2.817 *
32	4	0.603	0.830	0.611	0.887
32	5	0.384	-0.971	0.360	-1.193
33	1	0.351	-1.244	0.346	-1.310
33	4	0.396	-0.873	0.416	-0.734
34	1	0.737	1.931 *	0.740	1.950 *
34	2	0.605	0.846	0.591	0.722
34	3	0.613	0.905	0.638	1.104
35	1	0.392	-0.912	0.412	-0.764
35	2	0.409	-0.772	0.422	-0.680
35	3	0.534	0.257	0.536	0.266
36	1	0.537	0.286	0.526	0.177
36	2	0.463	-0.323	0.483	-0.173
36	3	0.740	1.955 *	0.731	1.883 *
37	1	0.653	1.236	0.664	1.327
37	3	0.462	-0.336	0.478	-0.217
37	4	0.718	1.771 *	0.736	1.917 *
38	1	0.442	-0.494	0.458	-0.380
38	2	0.773	2.221 *	0.774	2.238 *
38	3	0.575	0.596	0.574	0.578
38	5	0.557	0.451	0.574	0.579
39	1	0.743	1.975 *	0.739	1.944 *
39	2	0.777	2.260 *	0.722	2.221 *
39	3	0.500	-0.024	0.506	0.016
40	1	0.461	-0.340	0.461	-0.360
40	3	0.502	-0.001	0.473	-0.260
40	4	0.293	-1.725(*)	0.279	-1.864(*)
40	5	0.365	-1.134	0.343	-1.339
40	6	0.572	0.570	0.568	0.527
40	7	0.630	1.047	0.613	0.901
40	8	0.365	-1.127	0.373	-1.085
41	1	0.554	0.420	0.547	0.356
41	2	0.581	0.647	0.613	0.905
41	3	0.327	-1.442	0.325	-1.482
41	4	0.386	-0.961	0.408	-0.802
41	5	0.512	0.077	0.518	0.112
41	6	0.390	-0.927	0.385	-0.990
41	7	0.538	0.295	0.571	0.555
41	8	0.515	0.101	0.519	0.120
41	9	0.539	0.302	0.541	0.308
41	10	0.664	1.326	0.668	1.358
41	11	0.321	-1.493	0.321	-1.519

Table A.1--contd.

Series No.	Trial No.	Revised Method Score	Revised Method z-Score		Old Method B Score	Old Method B z-Score	
41	12	0.517	0.120		0.522	0.147	
41	13	0.306	-1.618		0.317	-1.548	
41	14	0.378	-1.026		0.391	-0.943	
41	15	0.394	-0.894		0.404	-0.832	
41	16	0.359	-1.183		0.351	-1.267	
41	17	0.582	0.652		0.554	0.412	
42	1	0.650	1.213		0.647	1.181	
42	2	0.398	-0.857		0.392	-0.934	
42	3	0.590	0.717		0.593	0.738	
42	4	0.315	-1.545		0.342	-1.349	
42	5	0.750	2.038	*	0.749	2.029	*
42	6	0.541	0.318		0.549	0.371	
42	7	0.531	0.232		0.527	0.187	
42	8	0.589	0.712		0.605	0.832	
42	9	0.613	0.911		0.631	1.054	
42	10	0.656	1.261		0.673	1.402	
42	11	0.365	-1.133		0.390	-0.948	
42	12	0.515	0.106		0.504	0.001	
42	13	0.640	1.131		0.678	1.442	
42	14	0.665	1.336		0.650	1.211	
42	15	0.664	1.329		0.658	1.270	
42	16	0.535	0.266		0.524	0.161	
42	17	0.415	-0.717		0.418	-0.716	
42	18	0.358	-1.188		0.379	-1.035	
42	19	0.406	-0.797		0.408	-0.794	
42	20	0.650	1.210		0.666	1.342	
42	21	0.494	-0.069		0.486	-0.152	
42	22	0.523	0.172		0.520	0.123	
42	23	0.367	-1.117		0.356	-1.231	
42	24	0.612	0.899		0.623	0.981	
42	25	0.512	0.076		0.508	0.032	
42	26	0.438	-0.531		0.440	-0.532	
42	27	0.505	0.021		0.497	-0.058	
42	28	0.570	0.559		0.585	0.665	
42	29	0.511	0.073		0.502	-0.015	
42	30	0.491	-0.096		0.484	-0.165	
42	31	0.649	1.206		0.660	1.288	
42	32	0.513	0.083		0.217	0.540	
42	33	0.403	-0.818		0.411	-0.773	
43	1	0.412	-0.745		0.430	-0.612	
43	2	0.814	2.558	*	0.815	2.575	*
43	3	0.645	1.171		0.653	1.231	
44	1	0.474	-0.237		0.459	-0.378	
44	2	0.556	0.436		0.568	0.525	
44	3	0.405	-0.805		0.416	-0.730	
44	4	0.486	-0.134		0.515	0.088	
45	1	0.514	0.093		0.549	0.371	
45	2	0.406	-0.797		0.406	-0.816	

Table A.1--contd.

Series No.	Trial No.	Revised Method Score	Revised Method z-Score	Old Method B Score	Old Method B z-Score
45	3	0.374	-1.054	0.381	-1.021
46	1	0.474	-0.238	0.472	-0.271
46	2	0.826	2.664 *	0.840	2.784 *
46	3	0.418	-0.692	0.440	-0.534
47	1	0.455	-0.388	0.455	-0.406
47	2	0.445	-0.471	0.473	-0.261
47	3	0.587	0.697	0.610	0.877
47	4	0.535	0.267	0.544	0.331
47	5	0.569	0.546	0.583	0.655
47	6	0.515	0.103	0.517	0.107
48	1	0.565	0.515	0.572	0.563
48	2	0.585	0.682	0.566	0.509
48	3	0.592	0.740	0.603	0.817
48	4	0.546	0.356	0.557	0.439
48	5	0.398	-0.856	0.379	-1.035
48	6	0.582	0.650	0.600	0.795
48	7	0.604	0.838	0.595	0.752
48	8	0.807	2.506 *	0.819	2.609 *
48	9	0.550	0.389	0.520	0.127
48	10	0.469	-0.273	0.463	-0.343
48	11	0.659	1.288	0.677	1.433
48	12	0.394	-0.892	0.412	-0.762
48	13	0.551	0.402	0.551	0.385
48	14	0.408	-0.774	0.408	-0.801
48	15	0.651	1.225	0.659	1.285
49	1	0.480	-0.182	0.492	-0.100
49	2	0.729	1.866 *	0.708	1.689 *
49	3	0.517	0.116	0.539	0.291
49	4	0.565	0.511	0.557	0.435
49	5	0.391	-0.915	0.418	-0.712
49	6	0.687	1.519	0.676	1.424
50	1	0.727	1.848 *	0.718	1.770 *
50	2	0.444	-0.478	0.432	-0.600
50	3	0.344	-1.299	0.340	-1.360
51	1	0.473	-0.244	0.487	-0.144
51	2	0.703	1.645 *	0.706	1.673 *
51	3	0.273	-1.887(*)	0.293	-1.748(*)
51	4	0.612	0.899	0.608	0.863
99	1	0.683	1.483	0.674	1.408
99	2	0.605	0.845	0.606	0.842
99	3	0.368	-1.107	0.373	-1.084
99	4	0.457	-0.375	0.447	-0.473
99	5	0.492	-0.085	0.498	-0.050
999	7	0.501	-0.014	0.512	0.062
999	8	0.474	-0.233	0.456	-0.400
999	9	0.557	0.450	0.559	0.452
999	10	0.541	0.313	0.515	0.093
999	11	0.636	1.099	0.633	1.067

Table A.2

Individual Trial Scores

Formal Ex Post Facto Data

Series No.	Trial No.	Revised Method Score	Revised Method z-Score	Old Method B Score	Old Method B z-Score
1	1	0.661	1.302	0.635	1.080
1	2	0.616	0.934	0.571	0.552
1	3	0.741	1.958 *	0.769	2.197 *
1	4	0.601	0.807	0.636	1.094
1	5	0.460	-0.352	0.458	-0.381
1	6	0.730	1.874 *	0.719	1.783 *
1	7	0.518	0.123	0.544	0.326
1	8	0.460	-0.347	0.492	-0.104
1	9	0.626	1.012	0.625	0.998
1	10	0.475	-0.228	0.429	-0.625
1	11	0.596	0.770	0.565	0.505
1	12	0.742	1.967 *	0.727	1.844 *
1	13	0.832	2.708 *	0.861	2.952 *
1	14	0.845	2.814 *	0.850	2.862 *
1	15	0.769	2.192 *	0.747	2.015 *
1	16	0.829	2.684 *	0.831	2.714 *
1	17	0.625	1.008	0.631	1.050
1	18	0.752	2.054 *	0.783	2.311 *
1	19	0.554	0.422	0.324	0.161
1	20	0.564	0.506	0.538	0.282
1	21	0.689	1.531	0.741	1.961 *
1	22	0.598	0.786	0.586	0.679
1	23	0.521	0.156	0.589	0.705
1	24	0.624	0.996	0.577	0.600
1	25	0.651	1.219	0.670	1.370
1	26	0.527	0.198	0.514	0.078
1	27	0.570	0.558	0.559	0.456
1	28	0.737	1.926 *	0.725	1.828 *
1	29	0.645	1.175	0.638	1.107
1	30	0.367	-1.114	0.370	-1.116
1	31	0.634	1.078	0.628	1.024
2	1	0.879	3.099 *	0.884	3.145 *
2	2	0.560	0.471	0.552	0.392
2	3	0.838	2.758 *	0.839	2.774 *
2	4	0.823	2.639 *	0.816	2.583 *
2	5	0.702	1.641	0.664	1.323
3	1	0.496	-0.053	0.477	-0.226
3	2	0.382	-0.994	0.376	-1.065
3	3	0.193	-2.547 (*)	0.216	-2.388 (*)
3	4	0.233	-2.214 (*)	0.230	-2.274 (*)
3	5	0.476	-0.222	0.494	-0.085
3	6	0.418	-0.697	0.367	-1.135
3	7	0.551	0.400	0.579	0.620
3	8	0.537	0.280	0.535	0.257

Table A.2--contd.

Series No.	Trial No.	Revised Method Score	Revised Method z-Score		Old Method B Score	Old Method B z-Score	
3	9	0.563	0.495		0.527	0.185	
3	10	0.418	-0.692		0.397	-0.890	
3	11	0.655	1.250		0.651	1.214	
3	12	0.565	0.517		0.527	0.182	
3	13	0.672	1.395		0.632	1.058	
3	14	0.495	-0.063		0.545	0.339	
3	15	0.428	-0.616		0.402	-0.845	
3	16	0.674	1.410		0.670	1.366	
3	17	0.469	-0.275		0.482	-0.184	
3	18	0.628	1.028		0.621	0.964	
3	19	0.743	1.982	*	0.731	1.878	*
3	20	0.707	1.683	*	0.716	1.757	*
3	21	0.362	-1.158		0.337	-1.385	
3	22	0.506	0.032		0.489	-0.127	
3	23	0.530	0.224		0.540	0.296	

Table A.3

Individual Trial Scores

Exploratory Data

Series No.	Trial No.	Revised Method Score	Revised Method z-Score	Old Method B Score	Old Method B z-Score
11	1	0.403	-0.815	0.470	-0.288
11	2	0.646	1.177	0.661	1.298
11	3	0.463	-0.327	0.441	-0.523
11	4	0.325	-1.459	0.282	-1.844(*)
11	5	0.438	-0.530	0.461	-0.361
11	6	0.447	-0.453	0.440	-0.537
11	7	0.412	-0.742	0.384	-1.000
11	8	0.580	0.638	0.610	0.880
11	9	0.504	0.014	0.524	0.166
11	10	0.558	0.457	0.571	0.552
11	11	0.417	-0.703	0.469	-0.290
11	12	0.569	0.548	0.618	0.942
11	13	0.468	-0.283	0.478	-0.217
11	14	0.489	-0.109	0.467	-0.306
11	15	0.388	-0.942	0.443	-0.508
11	16	0.497	-0.044	0.506	0.013
11	17	0.487	-0.130	0.482	-0.184
11	18	0.440	-0.517	0.475	-0.244
11	19	0.684	1.496	0.715	1.743 *
11	20	0.433	-0.572	0.496	-0.070
12	1	0.239	-2.165(*)	0.238	-2.203(*)
12	2	0.230	-2.239(*)	0.257	-2.048(*)
12	3	0.549	0.383	0.493	-0.097
12	4	0.558	0.459	0.624	0.993
12	5	0.515	0.100	0.526	0.179
12	6	0.501	-0.016	0.568	0.528
12	7	0.449	-0.438	0.510	0.051
12	8	0.477	-0.211	0.458	-0.390
12	9	0.396	-0.876	0.440	-0.540
12	10	0.432	-0.576	0.455	-0.408
20	1	0.504	0.012	0.469	-0.295
20	2	0.579	0.632	0.602	0.814
20	3	0.638	1.117	0.646	1.177
20	4	0.506	0.028	0.523	0.151
20	5	0.496	-0.056	0.465	-0.323
20	6	0.507	0.036	0.492	-0.101
20	7	0.608	0.866	0.577	0.603
20	8	0.297	-1.691(*)	0.313	-1.588
24	1	0.360	-1.175	0.397	-0.887
24	2	0.400	-0.840	0.388	-0.963
24	3	0.489	-0.111	0.464	-0.331
24	4	0.640	1.129	0.600	0.794
24	5	0.509	0.051	0.530	0.206

Table A.3--contd.

Series No.	Trial No.	Revised Method Score	Revised Method z-Score	Old Method B Score	Old Method B z-Score
25	1	0.498	-0.037	0.487	-0.140
25	2	0.442	-0.498	0.436	-0.565
25	3	0.746	1.999 *	0.773	2.229 *
25	4	0.428	-0.612	0.428	-0.631
25	5	0.560	0.472	0.572	0.557
30	6	0.596	0.766	0.618	0.943
33	2	0.592	0.739	0.620	0.956
37	2	0.575	0.600	0.623	0.983
38	4	0.592	0.733	0.540	0.290
40	2	0.432	-0.582	0.428	-0.634
999	6	0.596	0.770	0.580	0.627

Table A.4

Individual Trial Scores

Questionable Data

Series No.	Trial No.	Revised Method Score	Revised Method z-Score	Old Method B Score	Old Method B z-Score
4	1	0.385	-0.962	0.436	-0.566
4	2	0.441	-0.505	0.437	-0.557
4	3	0.226	-2.270(*)	0.296	-1.730(*)
4	4	0.580	0.639	0.517	0.107
4	5	0.710	1.706 *	0.717	1.767 *
4	6	0.536	0.273	0.478	-0.221
4	7	0.565	0.513	0.584	0.658
4	8	0.659	1.290	0.651	1.213
4	9	0.607	0.858	0.612	0.889
8	1	0.398	-0.856	0.387	-0.978
8	2	0.405	-0.802	0.350	-1.275
8	3	0.469	-0.275	0.478	-0.215
8	4	0.324	-1.467	0.327	-1.469
14	1	0.459	-0.359	0.467	-0.310
14	2	0.543	0.330	0.502	-0.020
14	3	0.394	-0.892	0.454	-0.419
14	4	0.473	-0.242	0.548	0.360
14	5	0.396	-0.875	0.362	-1.182
14	6	0.391	-0.920	0.416	-0.733
30	2	0.607	0.861	0.541	0.306
33	3	0.821	2.618 *	0.852	2.881 *

Appendix A--contd.

II. Individual Trial Specifications (Series 40-51, 999)

The following table presents details of the individual trials conducted since July 1983 that were not included in the tables of Ref. 7. Each trial is indexed by Series and Trial number, corresponding to those listed in Appendix A-I. The column labelled Protocol indicates whether the trial was formal (F), exploratory (X), or questionable (Q); whether the target selection was instructed (I) or volitional (V); and whether there was a single percipient addressing the target (S) or multiple percipients (M). The agent and percipient numbers are indicated, along with the percipient's general geographical location (U.S. unless otherwise noted), and the next two columns give the spatial and temporal separations between percipient and target (approximate distances are given in miles; times in hours and minutes, with retrocognitive trials indicated by a minus sign.) The last column identifies the location of the target.

Table A-II

Series No.	Trial No.	Protocol	Agent	Prcpt.	Percipient Location	Distance	Time	Target Location
40	1	F V S	82	10	Charlottesville, VA	4540	34:00	Santa Margherita, Italy
40	2	X V S	82	10	Princeton, NJ	4050	37:45	San Remo, Italy
40	3	F V S	82	10	Princeton, NJ	4340	34:45	Garda Lake, Italy
40	4	F V S	82	10	Princeton, NJ	3860	83:15	Niendorf, W. Germany
40	5	F V S	82	10	Princeton, NJ	3860	34:00	Gothmund, W. Germany
40	6	F V S	82	10	Princeton, NJ	3860	15:05	Niendorf, W. Germany
40	7	F V S	82	10	Lewes, DE	4040	35:30	San Remo, Italy
40	8	F V S	82	10	Toronto, Canada	4140	34:00	San Remo, Italy
41	1	F V M	41	14	Princeton, NJ	3860	13:20	Zeltingen, W. Germany
41	2	F V M	41	14	Princeton, NJ	3860	-0:30	Uerzig, W. Germany
41	3	F V M	41	14	Princeton, NJ	3860	23:45	Wittlich, W. Germany
41	4	F V M	41	14	Princeton, NJ	3860	9:00	Moselkern, W. Germany
41	5	F V M	41	14	Princeton, NJ	3860	-12:00	Kroev, W. Germany
41	6	F V M	41	10	Princeton, NJ	3860	45:45	Zeltingen, W. Germany
41	7	F V M	41	10	Princeton, NJ	3860	13:10	Uerzig, W. Germany
41	8	F V M	41	10	Princeton, NJ	3860	6:15	Wittlich, W. Germany
41	9	F V M	41	10	Princeton, NJ	3860	31:30	Moselkern, W. Germany
41	10	F V M	41	10	Princeton, NJ	3860	-23:45	Kroev, W. Germany
41	11	F V M	41	64	Princeton, NJ	3860	-38:00	Zeltingen, W. Germany
41	12	F V M	41	64	Princeton, NJ	3860	-133:00	Uerzig, W. Germany
41	13	F V M	41	69	Albuquerque, NM	5620	6:00	Zeltingen, W. Germany
41	14	F V M	41	69	Albuquerque, NM	5620	6:05	Wittlich, W. Germany
41	15	F V M	41	57	Rochester, NY	4880	-9:45	Zeltingen, W. Germany
41	16	F V M	41	80	Woodstock, NY	3890	1:30	Kroev, W. Germany
41	17	F V M	41	70	New York, NY	3800	6:00	Uerzig, W. Germany
42	1	F V M	10	68	Princeton, NJ	100	13:00	Harrisburg, PA
42	2	F V M	10	68	Princeton, NJ	360	13:55	Hamilton, Ontario, Canada
42	3	F V M	10	68	Princeton, NJ	410	11:25	Stratford, Ontario, Canada
42	4	F V M	10	68	Princeton, NJ	410	13:10	Stratford, Ontario, Canada
42	5	F V M	10	68	Princeton, NJ	410	14:30	Stratford, Ontario, Canada
42	6	F V M	10	68	Princeton, NJ	410	14:30	Stratford, Ontario, Canada
42	7	F V M	10	68	Princeton, NJ	360	14:00	Toronto, Ontario, Canada
42	8	F V M	10	57	Bronxville, NY	160	10:01	Harrisburg, PA
42	9	F V M	10	57	Bronxville, NY	350	9:30	Hamilton, Ontario, Canada
42	10	F V M	10	57	Princeton, NJ	410	0:15	Stratford, Ontario, Canada
42	11	F V M	10	57	New York, NY	410	1:00	Stratford, Ontario, Canada
42	12	F V M	10	57	Carteret, NJ	410	0:50	Stratford, Ontario, Canada
42	13	F V M	10	57	Bronxville, NY	390	1:00	Stratford, Ontario, Canada

Table A-II - contd.

Series No.	Trial No.	Protocol	Agent	Prcpt.	Percipient Location	Distance	Time	Target Location
42	13	F V M	10	57	Bronxville, NY	390	1:00	Stratford, Ontario, Canada
42	14	F V M	10	57	Carteret, NJ	410	0:45	Toronto, Ontario, Canada
42	15	F V M	10	64	Princeton, NJ	100	-11:30	Harrisburg, PA
42	16	F V M	10	64	Princeton, NJ	360	0:27	Hamilton, Ontario, Canada
42	17	F V M	10	64	Princeton, NJ	410	2:30	Stratford, Ontario, Canada
42	18	F V M	10	64	Princeton, NJ	410	2:00	Stratford, Ontario, Canada
42	19	F V M	10	64	Princeton, NJ	410	0:30	Stratford, Ontario, Canada
42	20	F V M	10	64	Princeton, NJ	410	2:00	Stratford, Ontario, Canada
42	21	F V M	10	64	Princeton, NJ	360	-1:00	Toronto, Ontario, Canada
42	22	F V M	10	41	Princeton, NJ	100	-9:10	Harrisburg, PA
42	23	F V M	10	41	Princeton, NJ	360	-11:30	Hamilton, Ontario, Canada
42	24	F V M	10	41	Princeton, NJ	410	0:20	Stratford, Ontario, Canada
42	25	F V M	10	41	Princeton, NJ	410	0:00	Stratford, Ontario, Canada
42	26	F V M	10	41	Princeton, NJ	410	9:00	Stratford, Ontario, Canada
42	27	F V M	10	41	Princeton, NJ	360	-8:15	Toronto, Ontario, Canada
42	28	F V M	10	55	Princeton, NJ	100	1:30	Harrisburg, PA
42	29	F V M	10	70	New York, NY	140	6:45	Harrisburg, PA
42	30	F V M	10	70	New York, NY	350	6:30	Hamilton, Ontario, Canada
42	31	F V M	10	70	New York, NY	410	10:45	Stratford, Ontario, Canada
42	32	F V M	10	70	New York, NY	410	6:30	Stratford, Ontario, Canada
42	33	F V M	10	70	New York, NY	410	3:30	Stratford, Ontario, Canada
43	1	F V S	87	14	Princeton, NJ	3710	-49:00	Brussels, Belgium
43	2	F V S	87	14	Princeton, NJ	3710	-4:30	Brussels, Belgium
43	3	F V S	87	14	Princeton, NJ	3710	-1:30	Brussels, Belgium
44	1	F V S	10	57	Gosier, Guadelupe	2040	37:05	Princeton, NJ
44	2	F V S	10	57	Gosier, Guadelupe	2080	14:00	New York, NY
44	3	F V S	10	57	Gosier, Guadelupe	2040	13:03	Princeton, NJ
44	4	F V S	10	57	Gosier, Guadelupe	2040	37:30	Princeton, NJ
45	1	F V S	69	41	Princeton, NJ	2700	10:00	Berkeley, CA
45	2	F V S	69	41	Princeton, NJ	2720	7:30	San Francisco, CA
45	3	F V S	69	41	Princeton, NJ	2700	5:30	Berkeley, CA
46	1	F I S	41	69	Berkeley, CA	2700	49:30	Princeton, NJ
46	2	F I S	41	69	Berkeley, CA	2700	0:35	Princeton, NJ
46	3	F I S	41	69	San Francisco, CA	2720	2:15	Princeton, NJ
47	1	F V M	69	41	Princeton, NJ	1760	1:20	Albuquerque, NM
47	2	F V M	69	94	Princeton, NJ	1760	15:45	Albuquerque, NM
47	3	F V M	69	10	Princeton, NJ	1760	0:15	Albuquerque, NM

Table A-II - contd.

Series No.	Trial No.	Protocol	Agent	Prcpt.	Percipient Location	Distance	Time	Target Location
47	4	F V M	69	41	Princeton, NJ	1760	4:00	Albuquerque, NM
47	5	F V M	69	94	Princeton, NJ	1760	-1:25	Albuquerque, NM
47	6	F V M	69	10	Princeton, NJ	1760	7:15	Albuquerque, NM
48	1	F V M	10	57	New Haven, CT	2110	-30:50	Grand Canyon, AZ
48	2	F V M	10	57	New Haven, CT	1940	0:40	Red Lake, UT
48	3	F V M	10	57	New Haven, CT	2050	-23:00	Hopi, AZ
48	4	F V M	10	94	Princeton, NJ	2020	7:15	Grand Canyon, AZ
48	5	F V M	10	94	Princeton, NJ	1930	19:00	Red Lake, UT
48	6	F V M	10	94	Princeton, NJ	2010	7:30	Hopi, AZ
48	7	F V M	10	96	Princeton, NJ	2020	-42:00	Grand Canyon, AZ
48	8	F V M	10	96	Princeton, NJ	1930	3:00	Red Lake, UT
48	9	F V M	10	96	Princeton, NJ	2010	3:30	Hopi, AZ
48	10	F V M	10	41	Princeton, NJ	2020	1:55	Grand Canyon, AZ
48	11	F V M	10	41	Princeton, NJ	2010	8:15	Hopi, AZ
48	12	F V M	10	70	Princeton, NJ	2020	2:00	Grand Canyon, AZ
48	13	F V M	10	70	Princeton, NJ	2010	1:00	Hopi, AZ
48	14	F V M	10	64	Princeton, NJ	2020	2:00	Grand Canyon, AZ
48	15	F V M	10	64	Princeton, NJ	2010	1:00	Hopi, AZ
49	1	F V S	94	10	Princeton, NJ	3680	20:00	Paris, France
49	2	F V S	94	10	Princeton, NJ	3790	32:00	Muenster, W. Germany
49	3	F V S	94	10	Princeton, NJ	4240	140:30	Riga, Latvia
49	4	F V S	94	10	Princeton, NJ	4240	95:30	Riga, Latvia
49	5	F V S	94	10	Pompano Beach, FL	4580	33:45	Riga, Latvia
49	6	F V S	94	10	Princeton, NJ	4240	69:30	Riga, Latvia
50	1	F V S	10	88	Glastonbury, U.K.	3420	-26:30	Princeton, NJ
50	2	F V S	10	88	Glastonbury, U.K.	3530	-42:30	Waterloo, Ontario, Canada
50	3	F V S	10	88	Glastonbury, U.K.	3490	4:30	Binghamton, NY
51	1	F V S	80	10	Princeton, NJ	3520	131:45	London, U.K.
51	2	F V S	80	10	Princeton, NJ	4040	132:30	Milan, Italy
51	3	F V S	80	10	Princeton, NJ	4040	71:30	Milan, Italy
51	4	F V S	80	10	Princeton, NJ	4040	106:00	Milan, Italy
999	1	F V S	80	41	Princeton, NJ	150	5:20	Woodstock, NY
999	2	F V S	80	41	Princeton, NJ	150	0:25	Woodstock, NY
999	3	F V S	80	41	Rachtig, W. Germany	3890	7:00	Woodstock, NY
999	4	F V S	80	41	Rachtig, W. Germany	3890	7:30	Woodstock, NY
999	5	F V S	69	41	Princeton, NJ	1950	2:30	Shungopoui, AZ
999	6	X V S	10	55	Princeton, NJ	260	2:45	Charlottesviile, VA

Appendix B

I. Descriptor Questions

1. Is any significant part of the perceived scene indoors?
2. Is the scene predominantly dark, e.g. poorly lighted indoors, nighttime outside, etc. (not simply dark colors, etc.)?
3. Does any significant part of the scene involve perception of height, or depth, e.g. looking up at a tower, tall building, mountain, vaulted ceiling, unusually tall trees, etc., or down into a valley, or down from any elevated position?
4. From the agent's perspective, is the scene well-bounded, e.g. interior of a room, a stadium, a courtyard, etc.?
5. Is any significant part of the scene oppressively confined?
6. Is any significant part of the scene hectic, chaotic, congested, or cluttered?
7. Is the scene predominantly colorful, characterized by a profusion of color, or are there outstanding brightly colored objects prominent, e.g. flowers, stained-glass windows, etc. (not normally blue sky, green grass, usual building colors, etc.)?
8. Are any signs, billboards, posters, or pictorial representations prominent in the scene?
9. Is there any significant movement or motion integral to the scene, e.g. a stream of moving vehicles, walking or running people, blowing objects, etc.?

10. Is there any explicit and significant sound, e.g. auto horn, voices, bird calls, surf noises, etc.?
11. Are any people or figures of people significant in the scene, other than the agent or those implicit in buildings, vehicles, etc.?
12. Are any animals, birds, fish, major insects, or figures of these significant in the scene?
13. Does a single major object or structure dominate the scene?
14. Is the central focus of the scene predominantly natural, i.e. not man-made?
15. Is the immediately surrounding environment of the scene predominantly natural, i.e. not man-made?
16. Are any monuments, sculptures, or major ornaments prominent in the scene?
17. Are explicit geometric shapes, e.g. triangles, circles, or portions of circles (such as arches), spheres or portions of spheres, etc. (but excluding normal rectangular buildings, doors, windows, etc.) significant in the scene?
18. Are there any posts, poles or similar thin objects, e.g. columns, lamp posts, smokestacks, etc. (excluding trees)?
19. Are doors, gates or entrances significant in the scene (excluding vehicles)?
20. Are windows or glass significant in the scene (excluding vehicles)?
21. Are any fences, gates, railings, dividers or scaffolding prominent in the scene?
22. Are steps or stairs prominent (excluding curbs)?

23. Is there regular repetition of some objects or shape, e.g. lot full of cars, marina with boats, a row of arches, etc.?
24. Are there any planes, boats, or trains, or figures thereof apparent in the scene, moving or stationary?
25. Is there any other major equipment in the scene, e.g. tractors, carts, gasoline pumps, etc.?
26. Are there any autos, buses, trucks, bikes, or motorcycles, or figures thereof prominent in the scene, moving or stationary (excluding agent's car)?
27. Does grass, moss, or similar ground cover compose a significant portion of the surface?
28. Does any central part of the scene contain a road, street, path, bridge, tunnel, railroad tracks, or hallway?
29. Is water a significant part of the scene?
30. Are trees, bushes, or major potted plants apparent in the scene?

II. Sample Descriptor Check-Sheet

Signature _____

Date and Time _____

Location _____

		Yes	No	Comments	Emph.*	Unsure*
1	Indoors					
2	Dark					
3	Height					
4	Bounded					
5	Confined					
6	Hectic					
7	Color					
8	Signs					
9	Motion					
10	Sound					
11	People					
12	Animals					
13	Single Object					
14	Natural focus					
15	Natural Environment					
16	Monuments					
17	Shapes					
18	Poles					
19	Doors					
20	Glass/Windows					
21	Fences					
22	Stairs					
23	Same					
24	Planes					
25	Equipment					
26	Vehicles					
27	Grass					
28	Roads					
29	Water					
30	Trees					

*Used for exploratory purposes only.

Appendix C

I. α_i Variations and Significance Tests

The α_i values represent the frequency of occurrence of each descriptor element in the target data base of a given subset and as such provide an empirical indicator of the likelihood of that question receiving an affirmative response. In this sense, the α_i values are estimates of the binomial probabilities in the given finite samples.

For a descriptor of binomial probability p, the number of actual occurrences in N targets is normally distributed with mean Np and standard deviation $\sqrt{[Np(1 - p)]}$. The measured estimate of the probability, then, is also normally distributed with mean p and standard deviation $\sqrt{[p(1 - p)/N]}$. Thus, for two samples, N_1 and N_2, drawn from the same distribution, the difference in the observed ratios is the difference between two normally distributed quantities, and therefore is itself a normally distributed quantity, with mean zero and standard deviation $\sqrt{\frac{p(1-p)}{N_1} + \frac{p(1-p)}{N_2}}$. If the underlying probability is not known, the best estimate of p in each case is the observed frequency in the sample. If p_1 and p_2 are the observed ratios in samples 1 and 2, then the quantity $z = (p_1-p_2)/\sqrt{\frac{p_1(1-p_1)}{N_1} + \frac{p_2(1-p_2)}{N_2}}$ is, under the null hypothesis of equal probability in both samples, a standard normal deviate. Therefore, this quantity may be used as a z-score to determine the likelihood that two observed samples are in fact drawn from the same distribution.

Each α set involves 30 such binomial samples, one for each

descriptor. Comparing the respective α_i between two data sets will result in 30 separate z-scores, and the question of whether the α_i are significantly different between the subsets becomes, in essence, the question of whether the set of 30 z-scores for the descriptors follows a normal distribution of mean zero. The table below indicates how the sets of 30 α_i differ across certain formal data subset pairs. The first column gives the largest absolute z-score in the set of 30; the second gives the number of z-scores of the 30 that exceed an absolute magnitude of 1.96 (2-tailed 5% criterion); and the third indicates the standard deviation of the set of 30 z-scores.

Subset Comparison	Largest $\|z\|$	No. $\|z\|$ > 1.96*	Std.Dev.**
Instructed vs Volitional	6.537	15	2.775
Instr. vs Vol. (ab initio only)	4.757	13	2.210
Ab Initio vs Ex Post Facto	3.560	12	1.610
Winter vs Summer	2.362	2	1.137
Princeton vs Elsewhere	5.000	10	2.076

*Expected number is 1.5.

**Expectation is 1.000; 5% bounds are 0.786 and 1.21.

All of these comparisons, with the exception of Winter vs Summer, clearly indicate differences in the α_i sets far in excess of any credible random fluctuation.

To ascertain that these differences are solely attributable to the α_i rather than to some other source of computational artifact, within-subset α_i comparisons were made between the

first and second half of the full formal instructed data set, and between arbitrary subsets of the full formal data base (constructed by randomly assigning trials to two groups of equal N) with the following results:

Within-Subset Grouping	Largest |z|	No. |z| > 1.96	Std.Dev.
Instr. 1st half vs 2nd half	2.642	2	1.172
All formal, random sort	1.769	0	0.716

From such calculations it seems clear that interior computational artifacts are not the source of the scoring distortion discussed in Section III.A, and that attention must be focused on the effects of α_i variations, per se. As described in the text, and further developed below, these possibilities can easily be precluded by calculating scores with the local α_i appropriate to the subset in question.

II. Assessment of Descriptor Frequency Artifacts

As described in Section II-B, the scoring algorithm employed in the various analyses reported in this paper, called "Method B" in earlier reports,[5-7] has at its core the assignment of an a priori probability, α_i, for each descriptor question. For each question answered "yes" by both agent and percipient a score of $1/\alpha_i$ is awarded, and for each question answered "no" by both agent and percipient a score of $1/(1 - \alpha_i)$ is awarded. No score is awarded for descriptors where agent and percipient disagree. The sum of the descriptor scores for all 30 questions is normalized by dividing it by the "perfect" score that would be

achieved if all descriptors were in agreement, yielding the normalized score for the trial in question. This same procedure is followed for calculating the mismatched scores constituting the empirical chance distribution that serves as a statistical reference.

In the original methodology, the α_i employed were a set of generalized target probabilities (GTPA) derived empirically from the agents' responses to the first 214 targets -- those in hand at the time the technique was developed. By using these generalized α_i, rather than the widely varying α_i associated with each small series, it became possible to standardize the scoring algorithm and permit calculation of a universal mismatch distribution of sufficiently large N to allow statistical evaluation of each individual trial or group of trials. As the size of the data base increased, these generalized α_i were periodically recalculated and, since the α_i and the recalculated scores were found to be reasonably consistent with the earlier analyses, the original GTPA α_i continued to be used for the sake of consistency. Hence, in the analyses reported in Ref. 7 the value of each individual descriptor α_i did not take into account the empirical variability among the local subset α_i discussed in Section III.A and Appendix C-I, and the possibility of artifacts arising from this source was not pursued.

To minimize subscripts, it will be assumed in the following discussion that one descriptor at a time is being considered. Let "A" denote the probability that an agent answers the question "yes," "P" the probability of a "yes" answer by a percipient,

and α the probability parameter used in the scoring. If, as in the actual experiments, α is calculated empirically from the actual target responses in the data set under consideration, then $A = \alpha$, by definition.

The average contribution of the descriptor to the mismatch score numerator is $(1/\alpha)AP + (1/[1 - \alpha])(1 - A)(1 - P)$. If $\alpha = A$, the expected average of this contribution becomes 1, regardless of the value of P. Similarly, the average contribution to the mismatch normalizing denominator is $(1/\alpha)A + (1/[1 - \alpha])(1 - A)$, which is 2 when $\alpha = A$. Thus, the average mismatch score computed from all the α_i for the local subset must be very close to 1/2. (It can differ from this value only due to the complicated way in which descriptors interact, which arises both from correlations between related descriptors and from the fact that all descriptors contribute to the normalizing denominator. In fact, despite these complicating factors, the experimental chance distribution has a mean score of .5025, which is within 0.5% of the theoretical value.)

The impact of a local bias ($A \neq \alpha$) in a data subset can be roughly estimated by restricting the variation to a single descriptor, assuming the other 29 to be correctly represented. The mean score for a mismatch trial in this data set will then be $(29 + N)/(58 + D)$, where N and D are the numerator and denominator contributions from the single descriptor under consideration. This mean score will be increased if $N/D > 1/2$, and reduced if $N/D < 1/2$. The formulas for N and D can be simplified as follows:

$$N = (1/\alpha)\,A\,P + (1/[1-\alpha])\,(1-A)\,(1-P) = [A\,(P-\alpha) + \alpha\,(1-P)] \,/\, (\alpha-\alpha^2)$$
$$= 1 + \frac{A-\alpha}{\alpha-\alpha^2}\,(P-\alpha)$$

$$D = (1/\alpha)\,A + (1/[1-\alpha])\,(1-A) = [\alpha + A\,(1-2\alpha)] \,/\, (\alpha-\alpha^2)$$
$$= [2 + \frac{A-\alpha}{\alpha-\alpha^2}\,(1-2\alpha)]$$

whence it follows that the direction of distortion of the mean score depends both on whether the agent's positive response frequency A is larger or smaller than α, and on whether the percipient's response frequency P is larger or smaller than 0.5:

IF:	$A>\alpha$		$A<\alpha$	
AND:	$P>0.5$	$P<0.5$	$P>0.5$	$P<0.5$
MEAN SCORE IS:	Increased	Reduced	Reduced	Increased

None of the foregoing, of course, takes into account any possible anomalous correlation in the matched scores, but only the base probability that a descriptor will be answered in a particular way. Therefore, it applies properly to the empirical chance distributions, whose values are necessarily determined by the relative frequencies of yes and no answers to the various descriptors. Moreover, this expected shift appears only if $A \neq \alpha$, or when calculating groups of data with subsets that have distinct α_i (see Fig. 1 and the associated discussion in Section III.A). The quantitative impact of this effect is assessed more fully in Appendix D.

The variability of the descriptor response frequencies is thus seen to constitute an important component of the analytical technique employed in these experiments, one that is addressed simply by calculating each data subset with its own relevant α_i.

Appendix D

Calculations with Pseudo-data

As one form of control on the scoring procedure, groups of pseudo-trials, constructed using computer-generated random binary sequences of 30 bits, were subjected to the standard computational recipes. Two types of groupings of 100 "trials" each were generated: one employing a uniform probability of .5 for each bit, and the other using the actual agent and percipient descriptor response frequencies of the Instructed data subset, chosen because of its large effect size and because its α_i differ most from the universal set. This process was repeated 15 times for the uniform α_i set and 30 times for the empirical α_i set, and each of these 45 groups of 100 was scored against its own local mismatch distribution.

The following table summarizes the results of these calculations, where each entry represents the average of all 15 or 30 repetitions of the calculations. The uncertainty indicated is the corresponding 1-sigma statistical error. The "Maximum Score" column reports the largest (absolute magnitude) composite z-score attained by any of the 15 or 30 groups of 100 pseudo-trials, together with the probability that such a large score would be obtained by chance in a distribution of 15 or 30 standard normal variates, as appropriate.

	Uniform $\alpha_i = .5$	Empirical α_i
Mismatch scores		
Mean	.49998 ± .00003	.50445 ± .00017
S. D.	.09182 ± .00016	.10660 ± .00043
Matched scores		
Mean score	.50241 ± .00240	.50517 ± .00192
Mean z-score	.02648 ± .02613	.00674 ± .01800
S. D. of z	.98767 ± .01478	.96942 ± .00969
Composite z	.26478 ± .26130	.06737 ± .18000
Max. composite z (prob.)	2.390 (.119)	2.274 (.293)

While the uniform and empirical α_i's produce slightly different mismatch distributions, the individual z-scores are found to be more or less normally distributed (the standard deviation of the trial z-scores is consistently reduced by approximately .03 in the set using the empirical α_i). The mean scores, and more importantly, the composite z-scores, (calculated by $\left(\sum_{i=1}^{N} z_i\right) \Big/ \sqrt{N}$, where N = 100 in this case), which are the ultimate statistical figures of merit, show no significant deviations from zero. The standard deviations as well as the means of the sets of composite z-scores are consistent with sampling from a normal distribution of mean zero and standard deviation one. This exercise confirms the efficacy of the scoring procedure and statistical methodology in the sense that despite the different α_i employed, the matched and mismatched score distributions are invariably well behaved and statistically indistinguishable.

A similar random data simulation was employed to examine the effects of α_i-related encoding artifact in the calculation of matched scores of local subsets. Since the formal Instructed and

Volitional subsets displayed the most extreme α_i differences (Appendix C-I), these groups would appear to be potentially most vulnerable. For this reason, the N's and α_i's of the respective agent and percipient response frequencies of these two subsets were used to construct 125 artificial "instructed trials" (I) and 211 artificial "volitional trials" (V). Both groups were scored independently with the appropriate local α_i, and also as a single independent group of 336 "trials" (I+V) computed with its own α_i, with the following results:

	Mismatch		Matched		
Group (N)	Mean Score	Std. Dev.	Mean z-score	S.D. of z	Composite z-Score
I(125)	.5049	.1076	.1365	.7609	1.526
V(211)	.5032	.0935	-.0010	.9975	-0.014
I+V(336)	.5032	.0963	.1521	.9242	2.788

Although the I and V subsets show no significant differences between their matched and mismatched distributions, the apparently "significant" z-score of 2.788 for the I+V group contrasts strongly with the combined z-score of 0.920 obtained for the I and V groups scored independently,* providing a clear example of the kind of spurious effect that can result solely from differences in subset α_i's, akin to the hypothetical example raised in Section III-A and discussed in Appendix C-II. When these simulated data are addressed as independent subsets the artifact disappears, thus reaffirming the effectiveness of the

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* $$z_{1+2} = \frac{(z_1\sqrt{N_1}) + (z_2\sqrt{N_2})}{\sqrt{N_1 + N_2}}$$

procedure described in Section III. In contrast, the effect observed in the actual combined experimental data is repeated throughout its various independently calculated subsets, and hence cannot be attributed to encoding artifact.

An unanticipated, but potentially informative observation emerges from examination of these artificially constructed "data" and their comparison with the experimental results. As one progresses from the most random of the mismatch distributions, that constructed with the uniform α_i of .5, through that of the simulated data with empirical α_i, to the universal mismatch distribution of experimental data, the distribution variances progressively increase by an increment of approximately .015 in each case (SD = .092, .107, .122, respectively), differences that are highly significant given the large N's. While we cannot account for the apparent regularity of this increase, it can be attributed to the non-uniformity of the α_i in the second group, and to the non-independence of the α_i in the experimental mismatches (the inevitable correlations among various descriptors in actual target encodings). It may be recalled, however, that a yet further broadening of variance (S.D. = .129) is observed when the actual matched score distribution is compared with the universal reference, in this case by an increment of about .007. A similar phenomenon has been observed in our human/machine experiments as well, when experimental distributions of series scores are compared with their relevant baselines.[9,10] Clearly, this effect merits further study.

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