PLATO'S r

A NEW FORMULA BASED ON OLD PRINCIPLES

Tadeusz Grygier, University of Toronto

Philosophy and mathematics were always closely related, from Euclid and Pythagoras ("all things are numbers"), through Plato's theory of ideas (all eternal and real things are either ideas or numbers), to Whitehead and Russell's Principia Mathematica (27), and to Leibnitz and the Warsaw School of mathematical logic of Lukasiewicz, Lesniewski and Tarski (29,30).

And yet this relationship is often forgotten in modern science and technology, especially among statisticians, albeit to forget one's relatives is neither gracious nor - if I may be cynical - practical. Maintaining such contact may bring ample rewards: in families through the inheritance of money, in science through the borrowing of old, and the creation of new, ideas. As will be demonstrated, some of the old ideas can lead to most unexpected practical consequences.

The purpose of this paper is to present an extremely simple formula for product-moment correlations which finds its justification in Plato's theory of ideas. As with platonic love, Plato's r is more practical than at first appears: both save a great deal of trouble and labour, and help to avoid costly errors, which might otherwise occur despite precautions and technical equipment. Plato's r requires no equipment and, even if the "Procrustean Table" (Appendix A) is lost or unsuitable, a new one can be easily constructed once its principle is clearly perceived - and accepted.

According to Plato's theory, most clearly stated in the allegory of the cave at the beginning of Book VII of The Republic (19), we can never perceive reality itself but only its shadow. We are like prisoners in a cave who "lie from their childhood, their legs and necks in chains, so that they stay where they are and look only in front of them". A fire is burning behind them and they can see nothing of themselves or of each other "except the shadows thrown by the fire on the wall of the cave" (19, p.207). Plato anticipated, centuries before Freud, the mechanism of projection not only as a distortion of reality but as the very essence of perception.

As I have discussed elsewhere (11), I believe the point made by Plato to be as valid now, in the light of scientific methodology and of new evidence in social psychology, as it was in Ancient Greece. The scientist never attempts to perceive the total reality around him.* As Wojciechowski says, he "consciously chooses as his formal object a particular aspect of material reality and purposely ignores other aspects which he deems, and rightly so, unfit for his methods and means of investigation. Thus his point of departure, far from being a universal one, is carefully selected and limited" (33, p.30). If he wants to compare his results with those of other scientists he is careful to adopt the same methodological framework; then his conclusions, while never attaining universality, are not entirely individual and subjective. The restriction of his conceptual apparatus is deliberate (cf. Ajdukiewicz, 1, p.186), while a layman's is unorganized and unconscious. He is never faced with a heap of factual material and with the task of encompassing it by a theory; on the contrary, he has to hunt for a conceptual apparatus, "for it is this alone which can give rise to empirical sentences" (ibid.). And there is no conceptual apparatus without the neurological one.*

The layman's picture of reality is as structured as that of the scientist, but the structure is more complex and is not explicitly stated; it is based on the physical and physiological state of the organism, on the past history of its adaptation and on the expectations built on previous experience. The scientist, as much as the layman, is imprisoned by his organism and experience, but

*He often perceives nothing but a single aspect of the complex reality around him and treats it as if it represented the ultimum knowledge. Philosophy often claims to be more universal than science, but what the philosopher chooses as representing all nature also depends on his outlook. A materialist declares that mind is nothing but matter, while a phenomenalist sees matter as mere clusters of sensations, which are psychological phenomena. A Marxist regards morality and character as the outcome of economic processes, while an ethical skeptic sees good and evil as mere projections of likes and dislikes. For a fuller list of "pan-scientific radicals" and their views see Feigl (9).

*As Caws says (7, p.14): "All our knowledge has to be expressed in conceptual terms; we can know nothing intelligently about what is external except as it is mediated to us by the neurological apparatus which originally informs us of its existence. We have no sure way of telling whether the logic, which exhibits itself in every department of enquiry, has its root in that unconscious faculty of man which is interposed between what is delivered to his senses and what is received cognitively, or whether it is, in fact, an ontological characteristic of nature. Whatever may be the truth of this matter, one thing is certain: it is inescapable. It is certainly a condition of our thought, whether as a characteristic of our minds or as a characteristic of a world of which our minds are part, and consequently it is to be found equally in the systems invented by us and in those presented to us."

the layman, unlike the scientist, is not conscious of his lack of freedom from perceptual restrictions. Experimental psychology has shown how much projection there is in any perception, especially if the stimulus itself is relatively unstructured (such as an ink blot). Social psychology has confirmed that prejudice, which feeds on projection, also increases with ambiguity of the stimulus. The lesson that we can draw from both Plato and modern psychology is that, if we are to be prisoners, we should face this fact and have proper bars, so that our data will be neatly arranged for efficient manipulation. Instead of treating our data with reverence as if they were pieces of unattainable reality and then using statistics which could, only approximately, do them justice, we can devise the simplest possible formulae and then impose an order on our observations, so that they would fit our formulae exactly. Not only can this be done; it has been done, and the evidence so far shows that validity (i.e. approximation of assessment to the reality inferred from other observations) is not lost but probably increased. For this reason Q-sorts have, generally, a prearranged distribution which they impose on the sorter.

The formula I suggest is yet another step in the direction of simplicity, speed and accuracy.

The purpose for which the new formula was devised was, originally, very limited. In an investigation in Ontario training schools we wanted to correlate a number of sociometric scores with each other and with behaviour ratings by the staff. The sociometric scores reflected each boy's number of friends (this was the Preference score), his number of enemies (Rejection score), and two derivatives of Preference and Rejection scores, namely Acceptance score (calculated as the difference between the number of preferences and the number of rejections) and Emotional Response score (being the sum of preferences and rejections). Since raw sociometric scores depended on the number of subjects in the group - the larger the group the larger was the possible number of friends and enemies alike - we needed a prearranged distribution which would allow comparisons of relationships from group to group. We also wanted to impose a uniform quantitive frame onto the observers, in this case the staff of the training schools. Finally, we wanted to simplify and speed up the calculations of the correlation coefficient, so that they could be carried out by students with a non-mathematical background (in social work) with a minimum of error and at maximum speed.

First we had to arrange a distribution of data suitable for the calculation of product-moment correlation coefficients. This implied a quasi-normal distribution. The distribution had to be such that it could be used easily by untrained raters; therefore it had to fit their average frame of reference. Since we usually perceive objects as possessing a given quality in a range from very marked through marked and medium to markedly and conspicuously absent or weak, a five-point scale distribution was adopted. This is usual, although seven-point and even nine-point distributions are quite common, especially in Q-sorts. In the distribution we adopted the mean has a value of 3. Values 2, 3 and 4 have class intervals of 1 standard deviation. Values 1 and 5 lie outside these limits. This type of distribution is more satisfactory to the rater than that in which the middle value ranges from one standard deviation below to one standard deviation above the mean observation. Forced normal distributions for groups of varying sizes, from 12 to 100, are arranged in the "Procrustean Table to Stretch Data on". The last column of the Table will be explained later.

For a seven-point scale, values 2, 3, 4, 5 and 6 would have a class interval of two-thirds of the standard deviation; values 1 and 7 would fall outside these limits and the mean would be 4.0. The frequency distribution of values would be such that 5% of the total population would have a rating, score or other measure valued at one, 11% at two, 21% at three, 26% at four, 21% again at five, 11% at six and 5% at seven. The principle is exactly the same, whatever the number of classes, provided it is an odd number.

A standard distribution in which the mean is always the same and always a whole number and all deviations from the mean are also whole numbers allows the calculation of standard deviations and of cross-products of actual deviations from the mean to be simplified. This is usually done, but the new formula goes one step further than is customary: standard deviation is dispensed with altogether. With a mean of 3 and the distributions as shown in the Procrustean Table, the usual formula for product-moment correlations can be reduced to the sum of cross-products of deviations from 3 divided by the sum of all deviations squared. Moreover, the sum of deviations squared is constant for all groups of a given size. It can therefore become a part of the Procrustean Table instead of being calculated from the data; hence the last column of the Procrustean Table.

The derivation of the new formula is very simple. The usual formula for product-moment correlations runs like this:

$$r = \frac{\sum(X-\bar{X})(Y-\bar{Y})}{N\sigma_x\sigma_y}$$

In our case both means are 3 and both sigmas identical; therefore we have:

$$r = \frac{\sum(X-3)(Y-3)}{N\sigma^2}$$

Since sigma square is the sum of deviations squared divided by N, what remains in the denominator is the sum of deviations squared. The formula thus becomes:

$$r = \frac{\sum(X-3)(Y-3)}{\sum(X-3)^2}$$

The numerator can be calculated in one's head and the denominator read off the Procrustean Table.

An example, taken from actual data in one of the training schools in Ontario, is reproduced in Appendix B. At the bottom of the sheet is given the formula for calculating Plato's $\underline{r}$. As can be seen, the number of possibilities for cross-products of deviations is very limited indeed. Whenever the score is 3 (which, of course, often happens), there is no deviation. Thus, whenever one of the pair of scores is 3 (which happens even more often), the cross-product is 0. When both scores are 2 or 4, the cross-product is 1; when one of them is 2 and the other 4, the cross-product is -1. The maximum value for a cross-product is 4 (for both scores of 1 or both of 5) or -4 (for one score of 1 and one of 5). The sum of cross-products can, therefore, be kept in mind as one is perusing the data. Logarithmic tables and desk calculators are unnecessary. In the example given no aids at all are needed, but usually a slide-rule is handy. Since no square roots are involved and only one division (in the final instance), the new formula not only saves time and reduces human errors, but also reduces the errors inherent in the process of division and of taking square roots of any but perfect squares. It also saves time. With the help of the Procrustean Table a person of average ability in arithmetic can calculate the correlation coefficient given in the example within some 20 seconds. A less convenient distribution of data, where it is necessary to use a slide rule, would add another 10 seconds. The last two columns given in the example need not be written down; the original values (in our example, the sociometric scores) suffice, and the rest can be calculated in one's head. The formula suggested can be applied to all five-point scales approximating a normal distribution; it can be easily modified for other scales.

The objection which could be raised against the method adopted here is essentially the same as that frequently used against all statistical procedures: that they distort reality. But what is reality? Russell implies that we cannot know it without inference. As he says, "real" things are not just those that cause sensations; they also "have correlations of the sort that constitute physical objects". Consequently, "A thing is said to be 'real' or to 'occur' when it fits into a context of such correlations" (24, p.185). My contention is similar: the distortion of reality begins at the level of observation, not statistical manipulation. What we handle in statistical procedures are not pieces of reality made into numerical abstractions, but pieces of observation already abstracted. Moreover, as Wojciechowski points out, both objective and subjective factors contribute to the production of number measures. "The cognitive structure, with its definite mode of knowing, sets its own demands, which are conditions <u>sine qua non</u> of knowability and intelligibility of number measures" (34,p.98). Evidence from psychology shows that the cognitive structure sets its demands and causes perceptions to be merely abstractions, irrespective of the use of number measures. Words, which we use as a vehicle of thought as well as a means of communication, are also abstractions; we are more used to words than to numbers, but the latter are more amenable to manipulation.

Patently true of the social sciences, this is also true of physics. Again, Russell supports this claim: "All that physics gives us is certain equations giving abstract properties of their changes. But as to what it is that changes and what it changes from and to - as to this, physics is silent" (26, p.224).

Since Locke and Nicod observation has been the basis for arriving at truth through induction. Nicod regarded simple enumeration as sufficient basis for inductive reasoning (13). Russell (25, p.58) and Whitehead (31, p.5) suggest that we should start from induction, proceed to deduction and then again to induction. Most scientists would regard their mode of operation as both inductive and deductive, and with this view I agree, although I regard the very distinction between inductive and deductive reasoning as somewhat arbitrary and misleading.

Both induction and deduction are merely aspects of ordering observations and concepts. The rules of deduction have been said to provide a regulative mechanism which enforces a consistent language and enables us to express in one form precisely what we have already said in another (Feigl, 9, p.18). The appearance of novelty in deductive inference is only psychological, as it provides sudden insights into the implications of the original set of premises (<u>ibid</u>.).

According to Popper (21), induction is no better; it has no place in scientific reasoning. Indeed, as was well known to Hume, induction by simple enumeration is never a valid form of argument. All statements reached by induction can only be treated as hypotheses which, in the words of Popper, are "tentative for ever". It is only in terms of probability - and therefore of statistics - that hypotheses can be proved or disproved and, as Sellars (28; <u>cf</u>. also Grygier, 10) says, induction is vindicated.

However, <u>pure</u> induction can never be vindicated, not even in probabilistic terms, because it never occurs in practice. The inductive-deductive process does not begin at the point of a scientific experiment or a philosophical discussion. All concept formation - which, as Vigotski (12) and Luria (16) have demonstrated, can be unconscious and entirely non-verbal - shows a mixture of analytic and synthetic processes. Experiments on perception and remembering, for instance by F. C. Bartlett in England (3,4), show the remarkable degree to which our perceptions are affected by previous experience (induction) and the conceptual frame of reference (which surely must imply deduction). More recently Quine has advanced the view that the dichotomy between synthetic and analytic statements is purely conceptual (22), while Wojciechowski (34) implies that the difference between sense data and measurements is also conceptual, and to oppose one way of knowing to the other is to oversimplify the issue. So, even if we agree that all ideas originate in the senses, we must also admit that everything our senses tell us is affected by our ideas. As Polanyi says (20), we learn to perceive only as we develop concepts about the things we experience.

An additional argument comes from the theory of relativity. If we accept its premises, all objects and events in space-time have to be determined in relation to other objects, including the observer. The observer is a part of the total system of relations; he can never be excluded.* Thus he adds to what Polanyi (20) calls "the unaccountable element in science" (the act of personal judgment in the scientific process), which is also subjective and also unavoidable. According to Polanyi every scientific process involves judgment and every act of judgment involves a personal decision; it is not subject to any rules as we cannot have rules to prescribe how judgment is to be applied.

This paper is not attempting to solve a problem which, in principle, is not capable of solution. Subjectivity will always be with us. But we can introduce some rules which can reduce the subjective element in the recording of observations, some order which, even if arbitrarily imposed, can be constant from case to case. Statistical tables, graphs, coefficients of correlation, measures of statistical significance, all help in the task of scientific understanding. As Wojciechowski says, counting is the act of knowing through the medium of appropriate numbers (34, p.90). Anything which facilitates counting makes a contribution to knowledge. Anything which helps to order scientific data makes a similar contribution.

A forced distribution, normal or otherwise, imposes an order on the observations to be made and recorded by the observer, an order which makes him quantitatively comparable to other observers, or to himself at different times. It is akin to the method of hypothesis in scientific enquiry in that it creates a directed process with rational supervision, instead of simple induction, in my view impossible in any case. A hypothesis, according to Barker (2), still makes use of induction, but directs observations towards a workable conclusion. A forced distribution has a similar function.

Any observation, whether expressed in terms of numbers or of statements of quality, can have only an historical significance unless it is reliable. If it refers to a reality which can be assumed to remain basically the same, irrespective of the lapse of time or a shift in the point of observation, it should remain constant, both qualitatively and quantitatively. It is well known that training increases the ability to make observations, especially systematic, reliable observations. Trained anthropologists, psychiatrists and psychologists are known to observe with greater precision. There is sufficient evidence to assume that people trained in the same manner tend to make and record their observations with greater consistency. In that sense their observations tend to be more reliable and, consequently, have more chance of being valid. By training I mean the enforcement - be it through didactic methods, discussions, or conscious and unconscious imitation - of a set of rules.

An important difference between, say, rules of social work, psychiatric or other clinical observation and the instruction to adopt a forced distribution of data is that statistical rules are simpler, more explicit and more precisely defined.* Both sets of rules result in increased reliability of the observations, especially if we define reliability in terms of correlations between the observations made of the same assumed piece of reality by different observers. If different observers adopt entirely different frames of reference it becomes impossible to learn anything about the reality that they are assumed to observe.

If John Smith tells us that he found only 1 in 20 people to be good we remain no better informed about the goodness of people whom John Smith had observed, let alone about the goodness of people in general; but we have learned something about John Smith and the criterion of goodness he employs. If for "goodness" we substitute crime or delinquency the situation will remain the same; if John Smith tells us that 1 in 20 people is criminal or delinquent we can see something of his quantitative perception of the world around him, but we are no better informed about the world.

A most recent example of variation in quantitative frame of reference was given at the 5th International Congress of Criminology in Montreal. Paul F. C. Mueller (18) examined 250 first admissions to a correctional institution and employed two actuarial methods and the clinical judgments of five correctional counsellors to predict parole outcomes. He found that, while actuarial predictions were normally distributed, the distribution of clinical judgments varied from counsellor to counsellor. Many counsellors were unduly pessimistic in their estimates: they ascribed to the offenders a lower probability of success than

*Sir Cyril Burt (6, 235-237) regards tables of measurement, correlations, factor-saturations and the like as comprising a series of mutually equivalent matrices which enumerate "only relations between qualities and not the amounts of those qualities by themselves: just as the coordinates of space and time can only state the position of a star in regard to some other object or observer, and never its absolute position in the universe".

* Thus a sample is generally defined as "a conveniently small portion drawn from a batch to judge the quality of the batch in a certain predesignated respect" (2, p.474). This description is similar to that of a symptom of a disease, but the rules of statistics are more precise and explicit than the rules of medicine.

On the use of oversimplified theories in science, particularly in the "exact" sciences, which make extensive use of mathematical analysis, see Williams (32). He regards simplification as one of the major techniques of science and shows the advantages of a simple "theory of games" in dealing with serious and complex problems.

could be justified on the basis of past experience. Others were so conservative that they predicted a 50% or slightly below chance of success for the majority of the offenders they assessed. Each counsellor saw the offenders, quantitatively, in his own unique way. His observations seemed to have a closer relation to himself as the observer than to the men he was supposed to describe. It is likely that an imposition of the same numerical framework, i.e. a forced distribution, would have increased the interpreter's reliability to the point that at least some validity of clinical judgment would become possible, though by no means certain.*

The only harm done would have been to the observer's self-image. The more we project ourselves onto the reality around us, like Plato's prisoners in the cave, the more we feel that we know intimately what we are trying to observe. What we see then is close to our hearts and therefore regarded as real.** But, as Russell remarks, it is safer to assume that reality exists if it can be consistently perceived by more than one observer (26, p.225). Purely subjective observations are more likely to be mere projections of ourselves and self-deceptions. Complete objectivity implies independence from observer effects or, as Bass (5) puts it, zero variance due to the examiner; this can never be achieved, but we can, at least, eliminate one source of this variance and make the rest more explicit.

An imposition of an objective and explicit frame reduces the observer's chance to project onto all others his own ideas, implicit but nevertheless quantitative, about the world around him.***

* Lack of numerical framework in clinical predictions may be partly responsible for the fact that in a well-known study by Meehl (17) the actuarial method showed superiority over clinical judgments.

** By contrast, if we try to be objective we lose sight of our aims. In psychological measurement, as Loevinger says, "the more one objectifies the nature of the universe from which the sample of items is to be drawn, the less likely is the universe to represent exactly the trait which the investigator wishes to measure" (15, p.655). Any experienced clinician will confirm the wisdom of the above observation; but the consequence of his attitude is that the more he sees what he wishes to see the more inclined he is to accept it as reality.

*** The danger of adopting a frame which is not explicit enough is illustrated by Eddington's (8, p.202) well-known example of the ichthyologist who casts his net into the water, examines his catch in the usual scientific manner, and concludes that all sea creatures are at least two inches long; his conclusion may remain "tentative for ever", but it will never be disproved unless he stops fishing and examines his net - which will never bring up anything that it is not adapted to catch. Unfortunately, a framework may be as visible as a fishing net, but its consequences are often hidden.

A forced distribution is bound, by its very nature, to reduce the amount of projection on the part of the observer. It thus serves a function similar to that claimed for psychoanalysis: it allows new insights through reducing self-deception.

Perhaps the greatest value of all statistical analysis lies in its ability to alert the scientist whenever his notions are clearly incompatible with systematic observations. But the secondary gain is by no means negligible: the science of statistics has contributed to the ability of the scientist, especially of the social scientist, to observe in a more systematic and explicit manner. Our Procrustean Table is harsh, like an iron bed; but it is at least explicit, and its frame is as standard as that of a bed in a modern hospital. Subjective judgments are as cruel to reality as Procrustes was to the travellers he used to rob; and they add another threat, that of the unknown.

The travellers caught by Procrustes at least knew where they were. Subjects assessed by purely subjective judgments are just as distorted, with limbs stretched out or chopped off at the whim of the investigator; and, what is more, they are subject, as in Kafka's The Trial (14), to unknown punishments for unknown misdemeanours. If we cannot avoid the shadow of Procrustes in science, let us at least reduce the sinister, hidden threats of Kafka. Then we can achieve what Anatol Rapoport (23, p.3) calls "the deeper aspect of the freedom of science: the freedom from inner rather than externally imposed constraints".

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APPENDIX A.

PROCRUSTEAN TABLE

To Stretch Data On

N	f(X=1)	f(X=2)	f(X=3)	f(X=4)	f(X=5)	$E(X-3)^2$
12	1	3	4	3	1	14
13	1	3	5	3	1	14
14	1	3	6	3	1	14
15	1	4	5	4	1	16
16	1	4	6	4	1	16
17	1	4	7	4	1	16
18	1	4	8	4	1	16
19	1	5	7	5	1	18
20	1	5	8	5	1	18
21	1	5	9	5	1	18
22	1	5	10	5	1	18
23	2	5	9	5	2	26
24	2	6	8	6	2	28
25	2	6	9	6	2	28
26	2	6	10	6	2	28
27	2	6	11	6	2	28
28	2	7	10	7	2	30
29	2	7	11	7	2	30
30	2	7	12	7	2	30
31	2	7	13	7	2	30
32	2	8	12	8	2	32
33	2	8	13	8	2	32
34	2	8	14	8	2	32
35	2	8	15	8	2	32
36	2	9	14	9	2	34
37	2	9	15	9	2	34
38	2	9	16	9	2	34
39	3	9	15	9	3	42
40	3	9	16	9	3	42
41	3	10	15	10	3	44
42	3	10	16	10	3	44
43	3	10	17	10	3	44
44	3	11	16	11	3	46
45	3	11	17	11	3	46
46	3	11	18	11	3	46
47	3	11	19	11	3	46
48	3	12	18	12	3	48
49	3	12	19	12	3	48
50	3	12	20	12	3	48
51	3	12	21	12	3	48
52	3	13	20	13	3	50
53	3	12	21	13	3	50
54	4	13	20	13	4	58
55	4	13	21	13	4	58

N	f(X=1)	f(X=2)	f(X=3)	f(X=4)	f(X=5)	$E(X-3)^2$
56	4	13	22	13	4	58
57	4	14	21	14	4	60
58	4	14	22	14	4	60
59	4	15	21	15	4	62
60	4	15	22	15	4	62
61	4	15	23	15	4	62
62	4	15	24	15	4	62
63	4	15	25	15	4	62
64	4	15	26	15	4	62
65	4	16	25	16	4	64
66	4	16	26	16	4	64
67	4	16	27	16	4	64
68	5	16	26	16	5	72
69	5	16	27	16	5	72
70	5	17	26	17	5	74
71	5	17	27	17	5	74
72	5	17	28	17	5	74
73	5	17	29	17	5	74
74	5	18	28	18	5	76
75	5	18	29	18	5	76
76	5	18	30	18	5	76
77	5	19	29	19	5	78
78	5	19	30	19	5	78
79	5	19	31	19	5	78
80	5	19	32	19	5	78
81	5	20	31	20	5	80
82	5	20	32	20	5	80
83	5	20	33	20	5	80
84	6	20	32	20	6	88
85	6	20	33	20	6	88
86	6	21	32	21	6	90
87	6	21	33	21	6	90
88	6	21	34	21	6	90
89	6	21	35	21	6	90
90	6	22	34	22	6	92
91	6	22	35	22	6	92
92	6	22	36	22	6	92
93	6	22	37	22	6	92
94	6	23	36	23	6	94
95	6	23	37	23	6	94
96	6	23	38	23	6	94
97	7	23	37	23	7	102
98	7	23	38	23	7	102
99	7	24	37	24	7	104
100	7	24	38	24	7	104

APPENDIX B.

EXAMPLE

Showing the calculation of Plato's correlation coefficient between Preference (P) and Rejection (R) sociometric scores in a group of 19 boys in an Ontario Training School

Subject	P score	R score	(P-3)(R-3)	Σ(R-3)(R-3)
A	2	2	1	1
B	3	2	0	1
C	4	4	1	2
D	3	1	0	2
E	4	3	0	2
F	3	3	0	2
G	4	3	0	2
H	3	4	0	2
I	5	5	4	6
J	4	4	1	7
K	3	3	0	7
L	1	2	2	9
M	2	3	0	9
N	2	2	1	10
O	2	3	0	10
P	3	4	0	10
R	3	2	0	10
S	4	3	0	10
T	2	4	-1	9

$$r = \frac{\Sigma(X-3)(Y-3)}{\Sigma(X-3)^2} = \frac{9}{18} = .50$$