is like platinum but unlike tungsten, which has a low density of d-like states there.

The appearance potential experiment probed about the same depth into the sample, but it probed the width of the unoccupied d-band states above E_F, shedding little light on the density of states at E_F. Platinum, with its almost filled d bands, displays only a narrow unoccupied bandwidth above E_F. Tungsten carbide was shown to have a broad unoccupied d band (or more likely, hybrid tungsten $5d$, carbon $2p$ bands) above E_F—broader, in fact, than the unoccupied d band of tungsten. In this sense tungsten carbide is unlike platinum. But being unlike in having a different unoccupied bandwidth is not inconsistent with being alike in having a high density of d states at E_F. Differences between tungsten carbide and platinum in such factors as crystal structure and electrons per atom inevitably break down any rigid band relation between unoccupied bandwidth and density of d states at E_F. A bulk property that we believe is important for strong catalytic activity is a high density of d states at E_F and, in this essential factor, tungsten carbide does appear to be like platinum.

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5. Supported by the Atomic Energy Commission. R.E.W. is also a consultant with the National Bureau of Standards.

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Analysis of Human Chronic Pain

Timmermans and Sternbach (*1*) essay to investigate the interrelations of personality variables and clinical measures of pain. The multivariate technique which best illuminates this sort of hypothesis is canonical correlation analysis (*2*), which, beginning with two clusters of measures and their matrix of intercorrelations, extracts maximally correlated pairs of subscales linear in the clusters separately. The subscales may be interpreted as either general factors (summations of diverse indicators) or specific "types" (systematic contrasts among the measures of a cluster). In the present instance, the two clusters are the pain variables and the personality measures. The computed canonical pairing of personality and pain scales would provide optimal evidence (as far as linear correlation-based computation can be evidence) for the influence of one upon the other, which the authors seek to estimate.

Unfortunately, the authors chose instead to perform a factor analysis using all of their variables together. This technique cannot express the formal distinction, crucial to the investigation, between the measure clusters and, as a result, there are several flaws in the data analysis as published.

1) The finding on which the authors rest—"a significant proportion of the variance is contributed by variables comprising a factor of *interpersonal alienation and manipulativeness*"—is null in the context of their goals. Whether or not there is information about type or intensity of pain to be gleaned from all the other measures, factor analysis is not searching it out; what it finds has no direct relevance to the establishment of such a relation. In the present case, factor 1 does not load on any variables of the pain cluster, so its estimation is useless for the clinical treatment of pain; similarly, factor 2, the pain factor, does not load on any of the personality variables—those variables have already been forcibly assigned to factor 1—or, in other words, is not at all predictable from the personality cluster. This mutual irrelevance is a specific goal of the rotation routine the authors chose to use, which pursues simple structure at the expense of just those intercluster correlations we are looking for—those which a canonical analysis would specifically display. It might happen that the canonical analysis would, in fact, extract just these two factors as its first pair, but we have no way of knowing.

2) Factor 2, the pain factor, is really only one item with two contrasts arbitrarily tossed in. For, with any score with which ratio is highly positively correlated, such as the second factor score, pain estimate minus ratio will necessarily correlate negatively if only the estimate and ratio are not too strongly associated in the population. But ratio loads high on this factor mostly because clinical is high on this factor and ratio has clinical for numerator. Without such redundancy factor 2 would not have emerged at all: with its effective latent root cut by two-thirds, it would have appeared at the end of the analysis as the unique variance it really is. This is clearly a flaw in the factor analysis. A different choice of redundancies, involving the estimate more, would quite change the content of factor 2. In a canonical analysis, however, it is strength of association with personality measures—not intracluster redundancy—that provides the assortment of pain variables into patterns of loadings. The resulting scales are resistant to this sort of confounding.

3) When one uses a priori certain functional combinations of variables, the results of a factor analysis can be quite misleading. In particular, the correlation between alternate versions of a construct, and thus their assignment to one factor or another, is strongly dependent on mathematical details of form. For instance, ratio and difference represent essentially the same concept: ratio is the antilogarithm of the difference of the logarithms of the contrasting clinical and maximum pain indicators. Yet ratio loads almost wholly on factor 2, while difference loads mainly on factor 3. One contrast between the two types of pain tolerance seems to be part of the pain factor, while an intellectually identical alternate version of the contrast is not. A canonical analysis strategy helps us avoid this paradox. Both difference and ratio are particular special contrasts among the pain indicators. Analysis without any transforms would suggest, by inspection of secondary factors, a selection of linear contrasts which are optimally informative. To determine the influence of the various possible ratios, we would switch contexts and look for similar contrasts among the logarithms of the pain indicators. Ratio and difference are contrasts belonging in different analyses; a single factor analysis only confuses them.

There is a canonical analysis routine

in the very program package that Timmermans and Sternbach used for the factoring. Its invocation would have accomplished specifically what the authors meant to do; the purposes of oblique rotation, which they inadvertently selected, could not be more inimical to the goals of the study.

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2. Canonical analysis is not often mentioned in texts of elementary applied multivariate analysis. It is described rather more formally in the standard technical multivariate surveys [for instance, D. Morrison, *Multivariate Statistical Analysis* (McGraw-Hill, New York, 1967), pp. 207–220 and references therein].
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2 July 1974

The valuable contribution of Timmermans and Sternbach's (*1*) study lies in (i) the use of quantifiable indices of clinical pain by means of psychophysical matching techniques; (ii) the application of a sophisticated statistical method, that is, factor analysis, to isolate pain-specific factors; and (iii) the demonstration that subjective and objective pain estimates and pain tolerance load on different factors, suggesting that they are basically different components of the human pain response. While matching clinical with experimentally induced pain (*2*) and factor analysis of pain responses (*3*) have been used previously, Timmermans and Sternbach's combination of both is novel. However, the authors have singularly failed to emphasize these valid points and instead have focused on potentially spurious personality correlates of chronic pain, leading them to some unwarranted conclusions.

1) Timmermans and Sternbach's interpretation of their first factor as an "interpersonal alienation and manipulativeness" factor and their conclusion that it is characteristic of patients with chronic pain are invalid. Neither their experimental design, lacking control groups, nor their actual data warrant their conclusions. The factor analysis indicated that the Minnesota Multiphasic Personality Inventory (MMPI) psychotic and psychopathic scales were related and comprised the first factor, while all pain variables had essentially zero loadings on this factor. Thus, these personality characteristics seem to be unrelated to pain. This would support my finding (*4*) that there are no significant correlations between pain and MMPI factor scores. Furthermore, the patients' average means on the four psychotic-psychopathic MMPI scales were all essentially within normal limits, that is, the patients' means were similar to those of the standardization healthy norm group. In addition, the authors used no control group and, thus, even if one could describe chronic pain patients as showing alienation it might also be characteristic of other disease or disability groups. Actually, all the first factor shows is that several MMPI personality scales are interrelated and that this cluster of correlations is the most prominent in the study. While Timmermans and Sternbach's description of personality characteristics of chronic pain patients may reflect clinical experience and insight, it is not a justifiable interpretation of their results.

2) It is questionable to use several forms of the same construct as separate variables in a factor analysis, that is, difference (maximum pain tolerance minus clinical pain level) and ratio (clinical pain level over maximum pain tolerance $\times$ 100) are alternate versions of the same parameter. Only one should have been used. Furthermore, the application of a derived variable (that is, dependent upon other variables) in a factor analysis can be criticized if the other variables upon which it is dependent are also included in the same analysis. Fortunately, the authors' use of a derived variable together with directly measured variables can be defended because in a previous factor analytic study (*3*) a derived variable, the pain sensitivity range (PSR), which is the difference between pain tolerance and pain threshold, was employed successfully together with the other two variables upon which it is based. In that study it was shown that the PSR's from five different pain-induction methods loaded highly on a specific pain endurance factor, while the other two variables loaded highly on another factor, thus supporting empirical observations that PSR measures components different from pain threshold and pain tolerance.

3) It is statistically dubious to apply factor analysis to variables that already belong to different sets or categories with within-category intercorrelations. The authors used three categories—pain responses, MMPI scales, and the Health Index. Either the variables in each of these sets should have been analyzed separately by means of factor analysis and the obtained factor scores compared between categories, or, alternately, a canonical correlation analysis would have been appropriate.

Finally, these critical comments are made with the hope that Timmermans and Sternbach might be able to reexamine and possibly reanalyze their data, which in turn may allow a reevaluation of their conclusions.

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15 July 1974

Timmermans and Sternbach (*1*) presented an analysis of pain and personality test data gathered from patients with chronic pain syndromes. They isolated a factor, labeled "interpersonal alienation and manipulativeness," that, they asserted, described the tendency of the patients to use their chronic pain in "pain games."

This conclusion is unwarranted because the extraction of a factor from an intercorrelation matrix indicates how the variables covary and not the typical tendencies of the subjects. Factor analysis and, indeed, any correlational analysis of data from a single homogeneous group will obscure the ways that group differs from other groups. Timmermans and Sternbach's factors thus revealed little about the unique characteristics of patients with chronic pain. A similar analysis of the measures they used may approximate the factors they found regardless of the people tested.

Swenson *et al.* (*2*) have published the intercorrelations of the Minnesota Multiphasic Personality Inventory (MMPI) scales based on norms gathered from 50,000 patients at the Mayo Clinic. Some of these patients were experiencing pain but few were experi-

Table 1. Comparison of the Timmermans and Sternbach factors having large MMPI loadings with those from an analysis of MMPI data gathered from general hospital patients. The Mf (male) key was used with the general hospital population, and the direction of factor 2 was reflected.

Chronic pain patients from Timmermans and Sternbach				MMPI scale	General hospital patients			
Factor					Factor			
1	4	6	7		1	2	3	4
.13	.03	−.18	.80	Hs	.42	.06	−.41	.30
.17	−.53	−.21	.40	D	.71	−.04	−.35	.11
.21	.01	.01	.78	Hy	−.12	−.06	−.90	.04
.70	.09	−.08	.22	Pd	.16	.69	−.19	−.24
.05	.03	−.77	.06	Mf	−.01	−.01	.05	−.92
.75	−.15	.08	.15	Pa	.08	.48	−.38	−.23
.48	−.04	−.15	.32	Pt	.78	.38	.05	.09
.69	−.21	−.06	.33	Sc	.62	.56	.03	.03
.58	.47	−.30	−.18	Ma	−.17	.91	.19	.21
.03	−.81	.08	−.08	Si	.95	.23	.18	−.13

encing chronic pain and, as in most hospitals, most were not likely to be experiencing any pain at all. I analyzed the intercorrelation matrix presented by Swenson *et al.*, using the same package of computer programs Timmermans and Sternbach employed [see (*3*)] and selecting the same options they selected. Specifically, the principal components method with unities in the diagonal followed by an oblique rotation with the amount of acceptable obliqueness being controlled by the program's default value (zero) was chosen. [It should be noted that the distinguishing characteristics of factor analysis in contrast to principal components analysis is the substitution of estimates of communalities in the diagonals in place of unities (*4*). Thus, Timmermans and Sternbach performed a principal components analysis of their data, not a factor analysis as they reported. As a matter of fact, usage of the principal components method is seldom suggested, and the manual accompanying the program used for the original and the present analyses urges researchers to select a different method of analysis.]

Table 1 contains the loadings found by Timmermans and Sternbach for the factors defined primarily by MMPI measures and those I found. The chronic pain patients' factor 1 defined by psychopathic deviate (Pd), paranoia (Pa), psychasthenia (Pt), schizophrenia (Sc), and hypomania (Ma) (*5*), and called "interpersonal alienation and manipulativeness" by Timmermans and Sternbach, is closely paralleled by the factor 2 from the general hospital sample. The same five MMPI scales define the two factors. If an orthogonal rotation is used instead of the oblique rotation (*6*), if three instead of four factors are retained for rotation, or if the female key for Mf is used instead of the male key, a factor defined by Pd, Pa, Pt, Sc, and Ma is always found. Thus, the conclusion drawn by Timmermans and Sternbach that "rehabilitation must be directed" to "social integration and self-control" as well as to attempts to reduce pain could have also been drawn from data obtained from a general hospital population. Of course, the existence of this factor does not mean the majority of either the general hospital patients or the chronic pain patients is extreme on this dimension. Rather it means that general hospital patients vary on this dimension just as the patients with chronic pain do. An analysis that may detect characteristics specific to chronic pain patients would involve contrasting such individuals with an appropriate sample of people who are not experiencing such pain.

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5. The other MMPI scales are hypochondriasis (Hy), depression (D), hysteria (Hy), masculinity-femininity (Mf), and social introversion (SI).
6. Timmermans and Sternbach did not explain why they chose an oblique rotation instead of an orthogonal rotation in an exploratory study. Since the factors are correlated after an oblique rotation, it is more difficult to gauge the relative importance of the rotated factors. The percentages of variance accounted for by the unrotated factors do not apply to the rotated factors as Timmermans and Sternbach imply [see p. 218 in (*3*)].

9 September 1974

There seems to be little agreement about the most appropriate method for factoring pain and personality data. We used the principal components method and subsequent oblique rotation of the data to avoid having to make possibly unwarranted assumptions. We wished to use defined (as opposed to inferred) factors, which would give the best combination of the variables, accounting for more variance in the data than any other linear combination of the variables. The oblique rotation made no restrictions concerning correlation between the extracted factors, for we were unwilling to put this requirement on the data at this early stage. Bookstein and Wolff are persuasive in their support of a canonical correlation analysis, and we now plan to use this method on a larger sample, but note that canonical factoring is "relatively new and the merits . . . are still the subject of some debate" (*1*).

Our use of both derived and direct pain measures was deliberate and was based on the successful factor analysis by Wolff (*2*) that we cited, in which both types of measures were included and a pain endurance factor emerged. Wolff's present argument supports our use of these measures and answers Bookstein's criticism.

Posavac and Wolff seem to think that a "control" group would have tested whether our findings were unique to patients with chronic pain, but they fail to explain how to obtain clinical pain measures from patients who are not experiencing pain. The only possible control would be a group experiencing acute pain, and it is known that patients with acute pain have MMPI (Minnesota Multiphasic Personality Inventory) profiles quite different from those with chronic pain (*3*). The need to obtain both pain and personality measures was important since behavior during pain and personality measures have been shown to be related (*4*), and there was no a priori reason to suspect that they would appear on independent factors.

We did not present our findings as unique, and it is entirely possible that other disabled or invalid groups might yield similar data, but this does not invalidate the results; descriptive statistics do not require controls. Fortunately, however, Posavac provides us with a comparison between MMPI scores from both patients with pain and other medical patients. Note that his factor 1 is a manifest subjective distress factor, with very high loadings on depression (D), anxiety (Pt), confused thinking (Sc), and introversion (Si). This factor is *not* to be found in our patients with chronic pain and suggests a striking difference between the two groups. As further support for differences between the patient groups, we refer Posavac to a paper (*5*) (of which Swenson is a coauthor) which shows clear MMPI differences between pain patients and other medical patients. Merskey and Spear (*6*) review numerous similar findings.

Wolff objects to our interpretation of factor 1. We quite agree with him that this factor is independent of the pain factor, and pointed this out. We also emphasized that the average scores on the scales comprising this factor were within normal limits; were they not, they would have suggested an interpretation of a psychoticism factor. However, the average scores were not "similar to those of the standardization healthy norm group." They are a standard deviation (on the average) above the mean, reflecting a distinct (albeit subclinical) difference from the healthy norm group. This supports our labeling and interpreting the cluster of scales on factor 1 as an "interpersonal alienation and manipulativeness" factor. In addition, we described this finding as supporting clinical descriptions of pain behavior, rather than vice versa (*7*).

Finally, Bookstein's comment that "factor 1 does not load on any variables of the pain cluster, so its estimation is useless for the clinical treatment of pain" represents a common confusion that we were very careful to avoid. We state that factor 1 has clear implications for the rehabilitation of patients with pain, not for treating pain (*8*).

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20 November 1974

Plutonium-244 Fission Tracks: An Alternative Explanation for Excess Tracks in Lunar Whitlockites

Recently Hutcheon and Price (*1*) reported on the presence of ^{244}Pu fission tracks in lunar breccia 14321. The mineral phase investigated for its fossil track record was a large *shocked* whitlockite crystal; the fission track excess attributed to the spontaneous fission of ^{244}Pu was of the order of 30 percent. This finding seems to be supported by the observation that in situ-produced fission xenon of ^{244}Pu was present in the same rock (*2*). However, as the ^{244}Pu fission track excess is rather small, it could be due to several factors which have not been considered by Hutcheon and Price. (i) Using the spontaneous fission decay constant 8.46×10^{-17} year^{-1} (*3*) rather than 6.85×10^{-17} year^{-1} (*4*) for the calculation of the ^{238}U fission track contribution within 3.95×10^9 years would lower the apparent track excess by about 20 percent. (ii) Lunar whitlockites are known to be highly enriched in rare-earth elements (*5*). Therefore, because of the high thermal neutron absorption cross section of gadolinium, the uranium content of the whitlockite measured with a given thermal neutron flux in the reactor could have been underestimated by Hutcheon and Price, which in fact would also lower an apparent track excess. For instance, the presence of 1 percent gadolinium in a 500-μm crystal would lower the uranium content apparently by about 20 percent. (iii) A misinterpretation of dislocations as fission tracks would also explain an apparent track excess. As we have evidence for the existence of dislocations in whitlockites of breccia 14321, we will concentrate our discussion only on this subject.

One polished section of this rock, previously studied for the cosmic-ray track record (*6*), was surveyed for whitlockites. By a process of alternate polishing (in steps of 10 μm) and etching (for 30 seconds at 22°C in 0.25 percent HNO_3 solution) ten whitlockites ranging in size between 6 and 30 μm were found. The densities of etch pits determined by optical and scanning electron microscopy (SEM) on replicas were found to range between 0.18×10^7 and 34×10^7/cm^2. When the etch-pit densities are low, the alignments of many pits can be observed. Two crystals having etch-pit densities of 0.18×10^7 and 16×10^7/cm^2, respectively, were large enough ($\sim$ 25 by 15 μm and 30 by 18 μm) to be studied in more detail. After the first etching they were irradiated with ^{252}Cf fission fragments in order to superimpose on them track densities of 2×10^7 and 50×10^7/cm^2, respectively. Then the crystals were etched for 30 seconds (same conditions), and we observed that their etch-pit densities remained the same as before (Figs. 1 and 2). In other words, the ^{252}Cf fission tracks were not revealed. In the crystal with the low etch-pit density the fission tracks were visible in the SEM after an etching time of 50 seconds; however, the heavily corroded crystal surface made their observation difficult. In the crystal with the high etch-pit density the surface was completely destroyed after etching for 50 seconds and, therefore, fission tracks were not visible.

In another attempt to verify the nature of the etch pits in the whitlockite grains of breccia 14321 we erased a possible fossil fission track record by maintaining the whitlockite sample at a temperature of 500°C for 1 hour. At this temperature fission tracks are completely erased in chondritic whitlockites. In the annealed chip we found ten more whitlockites (between 5 and 15 μm) by applying the same procedure as before. In these crystals the etch-pit densities ranged between 0.4×10^7 and 15×10^7/cm^2, that is, in about the same interval as that found in the unannealed sample. The crystal with