Vienna, 1968); *Sterile-Male Technique for Eradication or Control of Harmful Insects* (International Atomic Energy Agency, Vienna, 1969); *Sterile-Male Technique for Control of Fruit Flies* (International Atomic Energy Agency, Vienna, 1970).
43. L. F. Steiner, E. J. Harris, N. L. Mitchell, M. A. Fujimati, L. D. Christenson, *J. Econ. Entomol.* **58**, 519 (1965).
44. D. J. Nadel and G. Guerrieri, in *Sterile-Male Technique for Eradication or Control of Harmful Insects* (International Atomic Energy Agency, Vienna, 1969), p. 97.
45. L. F. Steiner, W. G. Hart, E. J. Harris, R. T. Cunningham, K. Ohinata, D. C. Kamakahi, *J. Econ. Entomol.* **63**, 131 (1970).
46. R. S. Patterson, D. E. Werdhaas, H. R. Ford, C. S. Lofgren, *Science* **168**, 1368 (1970).
47. A. R. Whiting, in *Strahlenwirkung und Milieu*, H. Fritz-Niggli, Ed. (Verlag Urban und Schwarzenberg, Munich and Berlin, 1962), p. 183; L. E. LaChance, in *Genetics of Insect Vectors of Disease*, J. W. Wright and R. Pal, Eds. (Elsevier, New York, 1967), p. 617; G. C. LaBrecque and C. N. Smith, Eds., *Principles of Insect Chemosterilization* (Appleton-Century-Crofts, New York, 1968); V. Landa and S. Matolín, in *Sterility Principle for Insect Control or Eradication* (International Atomic Energy Agency, Vienna, 1971), p. 173.
48. A. B. Borkovéc, *Environ. Mutagen Soc. Newsletter* **3** (1970).
49. R. C. von Borstel, in *Advances in Insect Population Control by the Sterile-Male Technique*, G. C. LaBrecque and J. C. Keller, Eds. (International Atomic Energy Agency, Vienna, 1965), p. 71; W. Klassen, J. F. Norland, R. W. Briggs, *J. Econ. Entomol.* **62**, 1204 (1969); D. S. Grosch, in *Sterility Principle for Insect Control or Eradication* (International Atomic Energy Agency, Vienna, 1971), p. 217.
50. H. S. Ducoff, A. P. Vaughan, J. L. Crossland, *J. Econ. Entomol.* **64**, 541 (1971).
51. R. C. von Borstel, in *Isotopes and Radiation in Entomology* (International Atomic Energy Agency, Vienna, 1968), p. 331.
52. E. A. Löbbecke and R. C. von Borstel, *Genetics* **47**, 853 (1962).
53. B. Wallace, personal communication.
54. E. Horber, *Radioisotopes Applied to Insects of Agricultural Importance* (International Atomic Energy Agency, Vienna, 1963), p. 313; *Waldhygiene* **6**, 161 (1966); in *Isotopes and Radiation in Entomology* (International Atomic Energy Agency, Vienna, 1968), p. 259.
55. C. F. Curtis, in *Advances in Reproductive Physiology*, M. W. H. Bishop, Ed. (Academic Press, New York, 1971), vol. 5, p. 119.
56. M. D. Proverbs and J. R. Newton, *Can. Entomol.* **94**, 1162 (1962).
57. M. Husseiny and H. F. Madsen, *Hilgardia* **36**, 113 (1964).
58. R. Cogburn, W. Tilton, W. E. Burkholder, *J. Econ. Entomol.* **59**, 682 (1966).
59. D. W. Walker and V. Quintana, *ibid.* **61**, 318 (1968).
60. D. T. North and C. G. Holt, in *Isotopes and Radiation in Entomology* (International Atomic Energy Agency, Vienna, 1968), p. 391.
61. E. F. Knipling, *BioScience* **20**, 465 (1970).
62. A. Serebrovsky, *Zool. Zh.* **19**, 618 (1940).
63. S. Wright, *Amer. Natur.* **75**, 513 (1941).
64. C. F. Curtis and A. S. Robinson, *Genetics* **69**, 97 (1971).
65. L. E. LaChance, J. G. Riemann, D. E. Hopkins, *ibid.* **49**, 959 (1964); D. E. Wagoner, C. A. Nickel, O. A. Johnson, *J. Hered.* **60**, 301 (1969).
66. C. Curtis, in *Sterility Principle for Insect Control or Eradication* (International Atomic Energy Agency, Vienna, 1971), p. 425.
67. H. Laven, E. Jost, H. Meyer, R. Selinger, *ibid.*, p. 415; K. S. Rai and P. T. McDonald, *ibid.*, p. 437.
68. J. C. Carter, M. F. Lyon, R. J. S. Phillips, *J. Genet.* **53**, 154 (1955).
69. R. C. von Borstel, unpublished data.
70. M. J. Whitten, in *Concepts of Pest Management*, F. Guthrie and R. Rabb, Eds. (North Carolina State Univ. Press, Raleigh, 1970).
71. C. F. Curtis and W. G. Hill, *Theor. Population Biol.* **2**, 71 (1971).
72. E. F. Knipling, *J. Econ. Entomol.* **53**, 415 (1960).
73. W. Klassen and J. F. Creech, *U.S. Dep. Agric. Misc. Publ. No. 1182* (1971).
74. R. S. Edgar, G. H. Denhardt, R. H. Epstein, *Genetics* **49**, 635 (1964).
75. P. W. Whiting, *ibid.* **17**, 1 (1932).
76. D. T. Suzuki, *Science* **170**, 695 (1970).
77. M. Tarasoff and D. T. Suzuki, *Develop. Biol.* **23**, 492 (1970).
78. D. T. Suzuki, D. Procunier, J. Cameron, J. Holden, *12th Proc. Int. Congr. Genet.* **1**, 144 (1968); D. T. Suzuki and D. Procunier, *Proc. Nat. Acad. Sci. U.S.A.* **62**, 369 (1969).
79. D. L. Hartl, personal communication.
80. L. Sandler and E. Novitski, *Amer. Natur.* **91**, 105 (1957); S. Zimmering, L. Sandler, B. Nicoletti, *Annu. Rev. Genet.* **4**, 409 (1970).
81. D. H. Hartl, Y. Hiraizumi, J. F. Crow, *Proc. Nat. Acad. Sci. U.S.A.* **58**, 2240 (1967).
82. See Suzuki (*76*) for dominant temperature-sensitive lethal mutants, and D. L. Hartl [*Genetics* **66**, 147 (1970)] and L. Sandler, Y. Hiraizumi, and I. Sandler [*ibid.* **44**, 233 (1959)] for meiotic drive mutations which are lethal when homozygous.
83. W. A. Hickey and G. B. Craig, Jr., *Can. J. Genet. Cytol.* **8**, 260 (1966); L. C. Dunn, *Acta Genet. Statist. Med.* **4**, 139 (1953); M. M. Rhoades, *Genetics* **27**, 395 (1942).
84. C. Curtis, *Nature* **218**, 368 (1968).
85. M. J. Whitten, *Science* **171**, 682 (1971).
86. I. E. Rasmussen, *Drosophila Inform. Serv.* **34**, 53 (1960).
87. D. L. Hartl, *Can. J. Genet. Cytol.* **12**, 594 (1970).
88. ———, *Proc. Nat. Acad. Sci. U.S.A.* **63**, 782 (1969).
89. A. A. Berryman, *Can. Entomol.* **99**, 8 (1967).
90. T. P. Bogyo, A. A. Berryman, T. A. Sweeney, *Application of Induced Sterility for Control of Lepidopterous Populations* (International Atomic Energy Agency, Vienna, 1971), p. 27.
91. K. Kojima, in *Sterility Principle for Insect Control or Eradication* (International Atomic Energy Agency, Vienna, 1971), p. 447.
92. D. W. Walker, V. Quintana, J. Torres, *J. Econ. Entomol.* **64**, 660 (1971).
93. R. Milani, in *Sterility Principle for Insect Control or Eradication* (International Atomic Energy Agency, Vienna, 1971), p. 381; D. E. Wagoner, *Nature* **223**, 187 (1969); I. C. McDonald, *Science* **172**, 489 (1971).
94. H. Laven, *Nature* **216**, 383 (1967); *Mosquito News* **29**, 70 (1969).
95. V. A. Zaslavskii, *Zh. Obshch. Biol.* **28**, 3 (1967).
96. R. H. Smith, *Genetics* **60**, 227 (1968).
97. J. E. Foster and R. L. Gallun, *Ann. Entomol. Soc. Amer.* **65**, 750 (1972).
98. R. D. Bird, *Ecology of the Aspen Parkland of Western Canada in Relation to Land Use* (Research Branch, Canada Department of Agriculture, Publ. No. 1066, Ottawa, 1961).
99. E. F. Knipling, *Bull. Entomol. Soc. Amer.* **12**, 7 (1966).
100. Oak Ridge National Laboratory is operated by Union Carbide Corporation for the U.S. Atomic Energy Commission. This article is dedicated to Dr. Ken-ichi Kojima in memory of his contribution to the study of genetic control of insect populations (*91*).

Evaluation of Infant Intelligence

Infant intelligence scores—true or false?

Michael Lewis and Harry McGurk

The late Sir Cyril Burt once remarked of intelligence, "Of all our mental qualities, it is the most far-reaching; fortunately it can be measured with accuracy and ease" (*1*, p. 28). Although much progress has been made in the field of psychometrics since Burt's statement, his early confidence has hardly been justified with respect to the measurement of intelligence during the early stages of human development. In common with many others, Burt espoused the view that intelligence is a finite potential with which the individual is endowed at conception; the manifestations of this intelligence increase at a stable rate during the growth process, but intelligence is subject neither to qualitative change nor to environmental influence. "It is inherited, or at least innate, not due to teaching or training; it is intellectual, not emotional or moral, and remains uninfluenced by industry or zeal" (*1*, p. 29).

It is a sine qua non of this view that measures of intelligence have high predictive validity from one age to another. Such validity is singularly lacking in every scale used to assess intelligence during early infancy. For example, Bayley (*2*), employing an early version of her infant development scales, reported correlations between scores at 1, 2, and 3 months and scores at 18 to 36 months which ranged between −.04 and .09. Recently, Bayley (*3*, p. 1174) has concluded, "The findings of these early studies of mental growth of infants have been repeated sufficiently often so that it is now well established that test scores earned in the first year or two have relatively little predictive validity." Stott and Ball (*4*) and Thomas (*5*), after extensive

Table 1. Mean and standard deviation (S.D.) of Mental Development Index scores.

Age (months)	Mean	S.D.
3	101.64	14.9
6	110.05	20.6
9	109.45	13.3
12	113.40	11.6
18	113.63	17.8
24	126.42	18.9

reviews of a wide variety of infant intelligence scales, arrived at essentially similar conclusions.

Despite these acknowledged limitations, infant intelligence scales are widely used in clinical situations, in the belief that, although lacking in predictive validity, these scales are valuable in assessing the overall health and developmental status of babies at the particular time of testing, relative to other babies of the same age. This use of infant intelligence scales is justified only if, in interpreting the resultant scores, the scores are regarded solely as measures of present performance and not as indices of future potential. What this performance may mean is questionable, since it is possible that "superior" performance may be indicative of poor performance later. For example, Bayley shows a correlation of −.30 between males' earlier test behavior and their IQ (intelligence quotient) at ages 16 to 18 (*6*). Infant intelligence scales are invalid as measures of future potential; the necessity for caution in this respect cannot be overstressed.

Intelligence test scores are frequently used as a criterion in evaluating the efficiency of infant intervention, or enrichment, programs. Typically, a sample of subjects from some specified population is exposed to a program of stimulation and interaction beyond the normal experience of that population. At various points in the program, intelligence test scores are obtained and compared with the scores of a control sample. If the scores of the former are higher than those of the latter, the program is evaluated positively; if not, it is evaluated negatively. Two assumptions underlie such procedures—one explicit, the other implicit. Explicitly, it is assumed that, while the limits of intellectual achievement may be genetically determined, mental development is strongly influenced by environmental factors. This view enjoys considerable support, but it is not the focus of our interest here. Implicitly, it is assumed that infant intelligence is a general, unitary capacity and that mental development can be enhanced by enriching the infant's experience in a few specific areas. Similarly, it is assumed that infant intelligence scales can reflect any improvement in competence that results from a specific enrichment experience. Data collected in the course of our longitudinal study of infant affective and cognitive development during the first 24 months of life allow us to consider whether there is any justification for the implicit assumptions.

Our study involved a sample of approximately 20 infants who were tested at regular intervals during their first 24 months (*7*). There were approximately equal numbers of males and females, and the sample was heterogeneous with respect to social class, although it was slightly skewed toward the upper-middle classes. The mental scale of the Bayley Scales of Infant Development was administered at 3, 6, 9, 12, 18, and 24 months, as was the object permanence scale from Escalona and Corman's (*8*) Scales of Sensori-motor Development. In addition, at 24 months, infants were given language comprehension and production tasks. These tasks were based on items selected from the Peabody Picture Vocabulary Test. For the comprehension task, standard Peabody instructions were followed, although a restricted number of items was employed. For the production task, subjects were shown individual pictures adopted from the Peabody test and asked, "What is this called?" or "Can you tell me what this is?" Seventeen comprehension and 17 production items were administered to each subject.

Table 1 presents the mean ($\bar{X}$) Bayley Mental Development Index (MDI) for each age, together with standard deviations (S.D.). It will be noted that at all ages the mean scores for the MDI are consistently higher than the mean scores for Bayley's standardization sample ($\bar{X} = 100$; S.D. $= 16$). We believe these differences to be a reflection of the relatively high socioeconomic composition of our sample.

Table 2 presents mean scores and standard deviations for the object permanence scale of Sensori-motor Development. The scale is constructed to reflect the infant's acquisition of the concept of objects (*9*); such acquisition is evidenced in our sample by the regular increase in mean score from one age to the next. Mean scores and standard deviations for the language production and comprehension tasks at the age of 24 months are $\bar{X} = 11.53$, S.D. $= 4.66$ and $\bar{X} = 11.79$, S.D. $= 4.43$, respectively.

Table 2. Mean and standard deviation (S.D.) for object permanence scale of Escalona and Corman Scales of Sensori-Motor Development.

Age (months)	Mean	S.D.
3	1.10	0.77
6	5.10	1.65
9	8.45	1.90
12	11.80	2.31
18	14.90	1.77
24	15.95	1.39

Intercorrelations among the MDI scores at different ages and among the scores on the object permanence scale at different ages are presented in Table 3. As can be seen, of the 30 correlations shown, only four are significant beyond the $P < .05$ level. For the MDI scores, correlations between 3- and 9-month-olds and between 6- and 24-month-olds reached significance, although in each case the correlation (.45 and .54, respectively) is relatively low and accounts for less than 30 percent of the variance—relatively useless for predictive purposes. All other MDI correlations are low. Moreover, the data fail to reveal either simplex or other patterns of correlation; for example, MDI scores of 3-month-olds predict neither their scores at 6 months nor their scores at 24 months (indeed, in the latter instance, the correlation is negative). These findings apply across all ages tested (*10*).

Correlations among scores on the object permanence scale are correspondingly low. Again, only two of them, between 3- and 12-month-olds and between 3- and 18-month-olds, reach significance. Each of these correlations accounts for less than 25 percent of the variance. As with the MDI scores, there is no clear pattern of interrelation in the infant's performance on a sensorimotor function. To further stress the lack of interrelation, we would point out that other work (*11*) has indicated little or no interrelation over a variety of sensorimotor scales at any particular age. Thus, although for our sample there is an increase in the

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Table 3. Interage correlations for the Mental Development Index (upper right) and the object permanence scale (lower left).

Age (months)	Age (months) 3	6	9	12	18	24
3		.20	.45*	.06	−.01	−.25
6	−.10		.08	.34	.37	.54*
9	−.10	.00		.40	.13	.00
12	.48*	.16	.31		.29	.26
18	.46*	.07	−.13	.32		.36
24	.05	.39	−.07	.08	.05	

* $P < .05$.

mean score from one age to another, there is no indication that successful performance at the simpler level is predictive of an infant's ability to succeed on the more complex items when he is older.

Correlations between MDI and object permanence at each age and between language development at 24 months and MDI and object permanence at each age are presented in Table 4. The results reveal an interesting pattern of development among the intercorrelations. First, the MDI scales are most closely related to the object permanence scales in the first 6 months of infancy, while the MDI scales are most closely related to language at 18 and 24 months. This result makes good sense, since the early items from the MDI are more closely related to sensorimotor functions, while the later MDI items are more closely related to language. Finally, there was no significant relation between the object permanence scale and language ability at the age of 24 months. In fact, there are some rather high negative correlations at 9 months.

A number of general conclusions are justified on the basis of these data. Concerning the lack of predictive validity in infant intelligence scales, there is little to add; as is the case with so many other longitudinal studies, our results indicate that there is no reliable relation between successive measures of infant intelligence during the first 24 months of life. A similar picture emerges with respect to the measure of sensorimotor development—the object permanence scale—employed in our study. Although there was a regular increase in mean scores on this scale from one age to the next, and although the majority of subjects showed a steady rise in scores over the 24-month period (*8*), high scores at an early age were not predictive of high scores later.

Only at the earlier ages was there any significant association between object permanence and MDI scores, and we attribute this to the fact that, for these ages, both scales measure sensorimotor abilities. After the age of 9 months, none of the correlations between the two scales was significant. There was no association between the early MDI scores and the scores on the language tests at 24 months; however, there were significant correlations between the MDI scores at 18 and 24 months and the language scores at 24 months. There was no association whatever between scores on the object permanence scale and scores on the language tests. For 24-month-olds, of course, the Bayley test has a considerable verbal loading, whereas the object permanence scale has none.

All in all, these findings cast serious doubt on the notion that the concept of general intelligence is applicable to the period of infancy. We have found no evidence to support the view that intelligence is a capacity which unfolds at a steady rate throughout the process of development and which increases only quantitatively from one age to the next. Rather, our data tend to support the view, advanced by Bayley (*3*), that at each stage of infant development intelligence comprises a set of relatively discrete abilities, or factors. During the early period of development, according to Bayley, these clusters of abilities are relatively age- or stage-specific; therefore there is no necessary continuity between intelligence as defined at one stage of development and as defined at another. Our data, as well as other information (*11*), indicate that, even with respect to sensorimotor functions, there is a lack of continuity.

Our data also cast doubt on the notion that scores on infant intelligence scales can be generalized beyond the particular set of abilities, or factors, sampled by the items administered at the time of testing. Thus, an infant who showed dramatic gains in tasks involving sensorimotor function would not necessarily manifest such gains on tasks involving verbal skills.

The implications of these conclusions for evaluation of infant intervention programs seem clear. Simply stated, infant intelligence scales are unsuitable instruments for assessing the effects of specific intervention procedures. This is true primarily because infant intelligence is not a general, unitary trait, but is, rather, a composite of skills and abilities that are not necessarily covariant. Such a view of intelligence is by no means new (*12*), but it is one that appears to require constant restating in order to counteract a tendency to reify simple, single measures of infant intelligence.

Frequently, evaluations of infant intervention programs have been confused because of a failure both to specify clearly the particular set of skills which the program seeks to emphasize and to develop specific criteria for tests of those skills. Consider an intervention procedure that is primarily intended to influence sensorimotor intelligence—for example, the development of object permanence. An appropriate curriculum might involve training subjects in a variety of peek-a-boo and hide-and-seek tasks. It is clear from our data and from the arguments presented above that a standard infant

Table 4. Correlations of the three measures of intellectual skills (MDI is the Mental Development Index).

	Age (months) 3	6	9	12	18	24
Correlations of MDI with object permanence	.24	.60*	.16	.09	.23	.02
Correlations of MDI with language at 24 months						
Comprehension	−.19	.40	.10	.22	.42	.49†
Production	−.24	.14	.04	.21	.57*	.48†
Correlations of object permanence with language at 24 months						
Comprehension	.13	.39	−.28	.17	.38	.21
Production	.21	.26	−.34	−.23	−.15	.31

* $P < .01$. † $P < .05$.

intelligence scale would be the *wrong* instrument to use in assessing the efficiency of such a program and, further, that the use of such an instrument is likely to lead to erroneous conclusions about the program's efficiency. Even more serious is the possibility that, by using the wrong instrument of evaluation in a large number of programs, one would erroneously conclude that intervention in general is ineffective in improving intellectual ability, thereby supporting the view that environment is ineffective in modifying intelligence. There are few who would suggest that schoolchildren should be administered a standard intelligence test after, say, a course in geography. Yet, such a procedure would be analogous to using an intelligence test to measure the success of attempts to teach the object concept to infants. Clearly, the success of a geography course is best assessed by tests of geographical knowledge and understanding; by the same token, the success of a program stressing sensorimotor skills is best assessed by specific tests of sensorimotor ability. In both cases, there may be some instances of improvement in intelligence test scores, but such improvement has to be regarded as fortuitous.

It cannot be emphasized too strongly that the success of specific intervention programs must be assessed according to specific criteria related to the content of the program. By focusing attention upon the criteria for evaluating programs, the necessity for careful specification of the program's goals will be emphasized. As argued above, the failure to specify goals has been a contributing factor in the confusion over means of evaluating intervention programs.

The nature and structure of infant intelligence is a complex and, as yet, unsolved problem. In our search for social relevance, we must not be misled into thinking that the worth of our efforts can be determined solely by the magnitude of infants' scores on intelligence tests of demonstrably limited generality.

References and Notes

1. C. Burt, E. Jones, E. Miller, W. Moodie, *How the Mind Works* (Appleton-Century-Crofts, New York, 1934).
2. N. Bayley, *Genet. Psychol. Monogr.* **14**, 1 (1933).
3. ———, in *Carmichael's Manual of Child Psychology*, P. H. Mussen, Ed. (Wiley, New York, 1970), pp. 1163–1209.
4. L. H. Stott and R. S. Ball, *Infant and Preschool Mental Tests: Review and Evaluation* (Monographs of the Society for Research in Child Development No. 30, serial No. 3, Univ. of Chicago Press, Chicago, 1965).
5. H. Thomas, *Merrill-Palmer Quart. Behav. Develop.* **16** (No. 2), 179 (1970).
6. N. Bayley, *Amer. Psychol.* **10**, 805 (1955).
7. Not all infants were able to complete all test. The smallest *N* for which data are reported here is 19. Data from an additional 120 infants, seen cross-sectionally at 3, 6, 9, 12, 18, and 24 months (20 infants at each age), were essentially similar to those reported here.
8. S. K. Escalona and H. H. Corman, "The validation of Piaget's hypotheses concerning the development of sensori-motor intelligence: methodological issues," paper presented at a meeting of the Society for Research in Child Development, New York, 1967.
9. J. Piaget, *The Origins of Intelligence in Children*, M. Cook, Transl. (International Universities Press, New York, 1952).
10. These results cannot be attributed to low instrument reliability. Bayley has reported split-half reliabilities ranging from .81 to .92, although she too fails to obtain any stability across these early ages [*Manual of Bayley Scales of Infant Development* (Psychological Corporation, New York, 1969)].
11. W. King and B. Seegmiller, "Cognitive development from 14–22 months of age in black, male, first-born infants assessed by the Bayley and Hunt-Uzgiris scales," paper presented at a meeting of the Society for Research in Child Development, Minneapolis, 1971.
12. J. P. Guilford, *Amer. Psychol.* **14**, 469 (1959).

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NEWS AND COMMENT

Grant Termination: Scientist Wins $16 Million for Loss of Support

John Julian Wild is a scientist who, by his own account, has fallen on hard times more than once during his career. One of those times was the winter of 1964, when a grant he had from the National Institutes of Health (NIH) was terminated because his sponsor, the Minnesota Foundation, withdrew its support. Wild's grant, which had been approved for 4 years and a half million dollars, came to an end after only 18 months. Eventually, Wild sued the foundation, its parent organization —the Amherest H. Wilder Foundation —and Frank M. Rarig, Jr., for $48 million. (Rarig was an administrator of both foundations.) Late last month, at the conclusion of a 6-week trial in a district court in Minneapolis, a jury awarded Wild a whopping $16 million in compensatory and punitive damages. The defendants will appeal.

Now Wild is suing the Department of Health, Education, and Welfare (HEW), asking that it release information about himself that he believes to be in HEW files. The two cases have set official nerves on edge. Nobody yet seems to know just what the Wild cases may mean as far as the possibility of future suits by other investigators is concerned. The question of whether this is a unique situation or one that may prompt similar actions simply cannot be answered now.

Wild discussed his career as a scientist and some of his attitudes about research in a lengthy telephone interview with *Science*.

A naturalized citizen who is British by birth, Wild, a physician, came to the United States shortly after World War II to work in the department of surgery at the University of Minnesota, where, he said, he was supported by a 2-year fellowship from the U.S. Public Health Service. "Then, afterwards, there was nothing further for me there [at the department of surgery]," he recalled, adding that, although Owen Wangensteen, who was chairman of surgery, tried, he "couldn't find me any more money."

John Julian Wild "fell on hard times." He says that he did not want to return to London to face the state of chaos that research was in in postwar England. Nor was he sympathetic to Britain's introduction of socialized medicine. So he looked for resources around Minneapolis, where he was, and still is, living.

In London during the war, Wild became interested in the bowel. He tells of seeing vast numbers of patients whose bowels were paralyzed by hemorrhaging caused by the effects of bomb blasts. The condition can be lethal. "I solved the problem," Wild said, "by developing a tube to relieve bowel distension." It was at this time