ogy alone gives us no understanding of the different forms of society, the varying content of ideation, or the resulting problems. The principles governing the behavior of the language-using, concept-forming human species include biological principles but are by no means limited to them.

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27 June 1955

Stunkard alludes to the "welfare state" in very broad and damaging statements. In contrast to the closely knit and well-defined biological descriptions, he not once in the article gives one definition, one economic, or sociological description of this "welfare state." I would think that if one wishes to make an analogy between two things, one would define and describe these two things in as definitive a way as one's knowledge enables him to do.

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5 July 1955

I find somewhat disturbing Stunkard's reasoning in his interesting essay. He described the physical changes in the structures of animals that have changed from a free-living to a fixed, communal or parasitic condition. These changes he characterizes as regressions and argues from there that this will be the fate of man if he also should follow the same "slippery path."

This application of a heterogenous collection of zoological facts to human society is clearly indicated by the title. Certainly *freedom, bondage,* and *welfare state* are terms that can be applied to animals only by the broadest poetic license. Initially Stunkard draws a parallel between the free state of an animal in which it independently secures its food and fights in various ways for the privilege of reproduction and the human concept of freedom.

These two concepts however are by no means the same thing. No champion of human freedom ever intended the word *freedom* to mean that man should revert to a savage state. Instead freedom has always meant, in human terms, the right of a man to live and believe as he wishes so long as those wishes do not deprive his neighbor of his inherent freedom—in other words, not only respect for his own rights, but a deep and equally binding respect for the dignity of others. This is our classical concept of freedom, and cannot be applied to the hunting and reproductive habits of wild animals.

Stunkard furthermore implies that certain anatomic changes resulting from adaptation to a static living habit are symptoms of "degradation" of the animal. Is it Stunkard's opinion that the species of Gephyrea he mentions are any the less happy and contented because metamerism is lost? that *Echiuris* leads an unfulfilled existence because in the adult several pairs of mesoblastic somites are lost? that the flatworms mourn forever the loss of their cilia? or that fleas and lice are degraded and shamed by the absence of wings?

The obvious fault in this reasoning is that Stunkard uses the word *regression* not only in a scientific sense, meaning a return down the evolutionary path, but also in a moral and human sense, meaning falling into evil or shameful ways. Regression in evolutionary terms cannot be judged morally, since moral issues are not involved. It can be regarded only as successful or unsuccessful.

Freedom is a product of our human society, as is bondage and the welfare state. The sources and structure of freedom will not be found in an investigation of the sexual or feeding habits of the annelids. Nor can a "welfare state" be even slightly understood by studying the bee.

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22 June 1955

On Brain to Body Ratios and the Evolution of Intelligence

In his paper on the brain to body weight ratios of mammals [*Science* **121**, 447 (1955)], H. J. Jerison uses the data from my monograph [*Ann. N.Y. Acad. Sci.* **46**, 933 (1947)] as the basis of his discussion, but he develops his theme solely, I believe, out of the well-known Dubois formula, $E = kP^{\beta}$ (obviously identical with the Huxleyan "allometric" formula, $y = ax^{\beta}$). He concludes: "Deviation from the expected brain weight in the primates can be accounted for by assuming a special evolution of the brain in the direction of the development of additional cerebral tissue, the weight of which is independent of the body weight."

There is not space to document the fact that others besides myself have found the Dubois formula shaky ground. I do not believe that anyone today would advance it on such slim technical premises as Dubois did. Let me simply point out that (i) over decades quite a few investigators have accepted the formula without critical examination and have developed consequences from it that are no safer than their foundation; (ii) the formula simply fails to fit more than a small middle range of mammalian data, leaving data beyond either end—extensive arrays—uncovered. (This is obvious even to the eye in Jerison's adaptation of my figure.) (iii) Whatever the shortcomings of my monograph are (and it has them), it finds that all extant mammalian data—including data on primates and man himself—can be subsumed under one common mathematical formula or pattern. In other words—any notion that man is aberrant among mammals simply disappears—insofar as my formulation has any validity.

I believe that the principle of Occam's razor demands that we first explore for mathematical formulations that do not require such speculative bolsterings as that just quoted. And apart from logical economy, the notion that primates peculiarly develop "additional cerebral tissue, the weight of which is independent of the body weight," seems to me an unseizable form of theoretical biology.

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12 May 1955

E. W. Count's criticisms should be understood in terms of the basic aims of my paper. I was most concerned with developing an anatomical measure that could serve as an independent criterion for comparing species in terms of intelligence, and I sought to develop the measure from simple assumptions about the evolution of the brain. The relationship between brain weight and body weight was chosen, not because it is necessarily fundamental, but because the "index of cephalization" developed from this relationship had been the only measure at all related to our guesses about the relative intelligence of contemporary mammals (*1*). As I have shown in my paper (*2*), inconsistencies in that index limit its usefulness, and a reanalysis of the problem was necessary.

In attempting this reanalysis I accepted as a first premise that, regardless of intelligence level, a larger body would necessarily require a larger brain, because more tissue would have to be controlled. The problem was then to decide on a function relating brain weight to body weight. One of Count's main criticisms is of my choice of the "allometric size function" for this relationship. He would have preferred, I assume, that his function

$$\log E = k_1 + k_2 \log P - k_3 (\log P)^2$$

be used (E is brain weight, P, body weight, and k_i, constants). Although I was familiar with Count's equation, I chose to use the simpler and more conventional allometry formula, because, in spite of Count's suggestion that this formula does not fit the data adequately, a statistical, rather than an intuitive, test of goodness of fit indicates that the allo-

metry formula is acceptable (*3*). Incidentally, it should be noted that the use of the allometry formula does not commit one to Dubois' rationale. As I stated in my paper, ". . . it provides a satisfactory *empirical* description of brain weight and body weight relationships for the mammals as a class."

Count's other criticisms seem to arise from his feeling that his "mathematical formulation" adequately accounts for mammalian data, and that I introduced unnecessary assumptions in my analysis. A brief reply to this point is impossible. I can state, first, that Count's analysis depends on the introduction of a second-order equation in log P and three constants, k_1, k_2, and k_3, without any attempt to justify the form of the equation or to suggest the biological significance of the constants. Occam's razor demands that "entities must not be multiplied beyond necessity" (*4*), and the terms of an equation are entities whether or not they are assigned physical referents. The fact that Count's "mathematical formulation" was not given a biological rationale is not an argument in its favor in terms of criteria of parsimony. A more detailed critique of Count's formulation has been presented by Sholl (*5*).

As for my assumptions and resulting analysis, the allometry formula, although it is presented in my paper as an empirical equation, can, in its general form be derived from simple assumptions. Dubois' error was less in his rationale for using the general equation than in his technique for determining a value for the exponent. A complete presentation of the argument would be out of place here, but Sholl's paper (*5*) covers some of the necessary ground.

My assumption that part of the brain weight is a function of intelligence and is evolved independently of the evolution of body weight is another way of stating the rather common notion that a given level of intelligence for a species is related to a given amount of brain. (I did not raise the problem of individual differences within a species.) At no point did I suggest that the primates are unique in developing this part of the brain weight. I specifically assigned the development to the mammals as a class and used the contemporary opossum as a species that represents the hypothesized primitive mammalian condition in which the entire brain weight is related to the body weight by the allometry formula. One of the advantages of my approach was, in fact, that the resulting equation presents a "common mathematical pattern" for mammals that made sense of primate data. Furthermore, human data were also subsumed under this pattern. And finally, the order of intelligence derived for macaques, baboons, and the orang on the basis of delayed reaction tests (*6*) follows the same order as that derived in my paper. These considerations show, I think, that my biological speculations were adequately seizable.

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References and Notes

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19 July 1955

Seasoning for the Calendar

The World Calendar would be assured of adoption if the United States would favor it. The postponement of its approval gives us a chance to include season balance in our new calendar.

Why does the winter solstice occur about 10 days before the end of the calendar year? The early Latins could start each 304-day calendar year with the same annual natural phenomenon, because they did not count winter days. After January and February were added to the old 10-month calendar, the 355-day Numa Calendar rapidly got out of phase with the natural year, because every day was counted. In the time of Julius Caesar it was a quarter of a year ahead. Intending to restore the year's ending to the winter solstice, Caesar added 90 days to 46 B.C., making it a season adjustment year 445 days long, which ran 5 days past the winter solstice. He never got around to correcting the error. In 45 B.C. he established a calendar of 365¼ days. By the 16th century A.D. the Julian Calendar was ending 20 days after the winter solstice, owing to its simple leap-year rule. Pope Gregory took 10 numbers out of October in 1582, making a partial adjustment year of 355 days, leaving 31 December still 10 days past the winter solstice. Gregory's refinement of the leap-year rule causes our present civil year to equal almost exactly the true length of the solar year and immobilizes the year's end at a meaningless time 10 days after the solstice.

If having the new calendar begin the day on or after the meaningful moment of some annual phenomenon would enhance its chances of approval, we should promote the idea. If Caesar and Gregory could declare adjustment years during times when changes were rare, why cannot we, who are experiencing many changes, declare another adjustment year and complete the return of 31 December to the solstice?

While we continue under the Gregorian Calendar with its date numbers progressing through the days of the week, we can choose for season adjustment a year in which a skip of 10 numbers will place 31 December of that same year on a Saturday; 1957 will be such a year; 1963 will be another such year. In 1957 (or 1963) 12 October followed by 23 October will put 31 December on a Saturday. Under the stabilized World Calendar *every* year will begin on a Sunday, and we can never omit 10 numbers without either forcing 1 January away from Sunday or breaking the cycle of the days of the week.

The persons who are the most influential in sponsoring the World Calendar fail to see the difference between absolute time and a man-made instrument for keeping track of time. They ask, "When and where will these lost days be reinstated?" Our reply can be, "When and where did we reinstate the days 'lost' by Gregory?" By deleting 10 calendar numbers during only one specified year, we shall not be deprived of days or bring the solstice any sooner; we shall merely leave out number labels on a time chart and give the calendar a different reading for the day the solstice arrives. If one's income for the 355-day adjustment year will be less, one's grocery bill will be less, also. A bank loan due 15 November 1957 will be due 25 November instead.

In revising our calendar, must we let a 2000-year old error and a 400-year old "fixation" prevent us from matching the calendar quarters with the four seasons? *Before* we adopt a static calendar, let us first synchronize our civil year with the solar year. Worldsday, the intercalated, unnumbered day between calendars, can be Solstice Day to boot. The first day of each quarter can be the first day of a season.

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1 August 1955

Every great advance in science has issued from a new audacity of imagination.—JOHN DEWEY.