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SELECTIVE HUMAN VIRUS AND THE ETHICS OF RESEARCH REGULATION

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Two positions may be formulated on the question of whether government or citizens' committees should have the power to restrict scientific research. The first position holds that we should deal with the problem on an issue-by-issue basis. If the potential risks of a proposed research undertaking outweigh its potential benefits, either the public sector or the scientific community itself ought to curtail the research. Those who take such a position usually do not answer questions of research regulation in general terms.

The second position endorses a blanket solution: any limitations on pure scientific research constitute an infringement of a fundamental right, the scientist's "right to know." Therefore, there should be no regulation of pure scientific research. The scientific community itself may be expected to limit hazardous work. Those who support this position often compare the right to know with traditionally recognized civil liberties such as free speech.

Concern over particular hazardous experiments has for the most part fueled the debate between proponents of free and controlled science. Especially prominent has been the stormy dispute over recombinant DNA experiments. Modifications of the N.I.H. guidelines for biological and physical containment of potentially hazardous organisms and materials relieved the concern of many over risks and benefits of the research. Philosophical discussion of the propriety of curbing a scientists's right to know continues, however. We still have come to no consensus as to whether nonscientists should have the power to curtail scientific research.

This paper reconsiders this question in the light of an objection Hans Jonas has made to unregulated research. Jonas' argument and certain considerations as to the social nature of the modern scientific enterprise appear to refute the libertarian claim that any regulation of research constitutes an invasion of civil liberties. An example from the field of virology serves to broaden the objection.

JONAS' ARGUMENT

Jonas attacks the claim that the scientist's right to know is a civil liberty and may even constitute a right prior to freedom of speech. In order that freedom of scientific inquiry be a civil liberty, Jonas argues, it would have to be completely nonpractical (in the sense of having no necessary practical consequences). But, he writes, the entire field has grown irreversibly out of its age of innocence: ". . . not only have the boundaries between theory and practice become blurred, but the two are now fused in the very heart of science itself, so . . . the ancient alibi of pure theory and with it the moral immunity it provided no longer hold." In support of this claim, Jonas notes that every branch of science has its technological applications; that the award of research funds is often contingent upon the apparent feasibility and desirability of realizing such applications; that modern science, even in its purest form, requires high technology; and that biological research, in particular, has an immediate effect on the biosphere every time it alters an organism in a laboratory. Of course, the right to know does not necessarily imply the right to disseminate knowledge. The alternative, however, may be unattractive to some: a case of "keepers" of sensitive scientific information.

William May recently put forward an argument like that advanced by Jonas.(1) The right to know, May argues, does not entail the right to create life, and where knowing requires creating, the research must come under public scrutiny before it can proceed.

May's and Jonas' arguments for research regulation hinge on the products of inquiry, rather than on conceptions of the nature of research per se. One argument of the latter order received its classic statement by T. S. Kuhn.(2) Science, Kuhn suggests, may normally consist not so much in the objective pursuit of truth as in the articulation (whether by verification, amendment, or expansion) of accepted theories; and theories may well replace one another not through the unmistakable demands of truth but because some allow more articulation or problem-solving than others. If one accepts the thesis that scientific progress is more a question of survival in a Darwinian battle among competing theories than a gradual discovery of truth, the right to know deteriorates from the right to discovery to the mere right to tinker with existing paradigms. The right to know is less compelling, given the latter interpretation.

One might strengthen the relativist's case as follows. If we believe that theories are merely ways of organizing our possible experiences into a coherent whole, we must alter an accepted view of the role of the theorist or pure scientist. He no longer appears as the seeker after truth whose discoveries motivate the rest of the scientific community. His role, on the relativist view, is rather to keep the scientific community's concepts in order. This function is crucial: if our concepts are not well organized in a deductive framework, we shall not be able to detect inconsistencies within our structure of beliefs. But on this view, it does not make sense to divide science into complementary portions which are ineluctably practical or incorruptibly nonpractical. Theory depends entirely on experiment, in this view, just as it informs the range of possible experiments. To the extent that it determines what questions we can reasonably ask in the laboratory, it must recognize experimental results as its practical consequences. It is incoherent to divide pure from applied science. Thus pure science cannot claim an innocence that technology must forgo.

If we regard free scientific inquiry as an activity similar in nature to free speech, however, no argument can completely legitimate its regulation. First, there will remain disputes as to the severity of the risks which are to be accepted. But the embarrassing question also arises whether Jonas would open projects in pure mathematics to the scrutiny of vigilante committees. If the proponent of controlled science answers "No," he must distinguish between those fields of study that have grown out of their "age of innocence" and those that have not. Such a distinction is bound to be arbitrary, unless he can produce an acceptable criterion that demarcates, for once and for all, the boundaries of the practical and the nonpractical.

Before describing a criterion that may satisfy this requirement, I note that the last objection misses the point. Even if we do not grant Jonas his principal claim that technology and pure science have become indistinguishable, many examples (like certain aspects of human virology) show how difficult it may be to make the distinction in specific situations. We need not go so far as to claim that there is no distinction; but we must respect how difficult it has become for us to draw that distinction. The point is not that Jonas' principles force him to condone the careful regulation of work on dissertations in pure mathematics; rather, it is that we can no longer be sure what is pure mathematics and what is applied science.

SOCIAL NATURE OF MODERN SCIENCE

Another reason science has grown out of its age of innocence is that it has become an increasingly social undertaking. In the common conception, a lone, lofty-minded researcher stands staring out the window of his isolated, moon-lit laboratory. But this image of scientific inquiry is truer to the stage than the laboratory, for without communication, the individual's knowledge is of no consequence and, I would argue, therefore meaningless. It may be true, for example, that great strides in human thought were taken between 600 and 900 A.D. But these thoughts received no lasting expression; they died with the thinkers who passed unheard out of history in the isolated monasteries of early medieval Europe.

This point may seem mistaken. Surely all that matters, it will be objected, is what science we get done. Whether one man's work reaches others is a separate issue that has no bearing on the meaning or value of research. I deny this because it is precisely the response of a scientific community that endows any piece of research with significance. An isolated set of data, unlinked with the concerns of any modern research community, is no different from computer garble.

This may not have been true when modern science was in its making--when, so to speak, every man was his own research community. But today's science is essentially social. It is best modeled by the coral reef, or by termite colonies, in which the whole is so much more than the sum of its parts that isolated individuals (or isolated bits of information) lose their significance. That is, significance attaches only to the whole, not to the parts. Most modern scientists, I believe, define their work with respect to the goals of a research community. This in itself, if correct, would give the lie to any attempt to divorce the significance of scientific work from its place in a larger body of scientific thinking. This is why I claim scientific inquiry must have a social dimension to be meaningful.

Perhaps we may distinguish mathematics from science on the grounds that the former is much more an individual's profession. Mathematical results do not beg to be fit into general schemas and existing theories the way physical results do. A result in number theory need never echo in topology; but every change in physics affects chemistry. If we see science as an attempt to organize knowledge into a coherent whole, we may, on this view, consider mathematics an analysis of the implications of our concepts. As science is more concerned with synthesizing its findings, communication between its practitioners becomes more important, even essential, to the status of their results as scientific research.

If we grant that scientific inquiry may be regarded as meaningful only if the results have been communicated to other scientists, we have grounds for distinguishing the rights relevant to scientific inquiry from basic civil liberties. Basic civil liberties concern the individual agent. The right to know, however, involves all society, since it protects the scientist's right to investigate a field and especially to disseminate his findings. Thus exercise of the right to know effectively forces information on some part of society. This information, moreover, is of a special sort. It is unlike pornography, for example, because there are circumstances under which one cannot afford to ignore it. So the right to know, given the social requirements for scientific inquiry to be meaningful, literally thrusts the results of our science upon us.

This may have been well and good in the period Jonas calls science's age of innocence, but it is no longer. The example from human virology will suggest that certain lines of research may lead to the possibility of producing group-specific human viruses. Information of this kind concerns people even before technology has taken steps to develop the capability of exploiting its implications. The knowledge that a group with a particular genetic trait, and only that group, is susceptible to a virulent strain of virus, would seem to alter that group's perception of its role in society. Other examples can be given of scientific findings of which no one would want to have knowledge. So the forced exposure to investigative results poses problems at least in the life sciences. The right to know, ostensibly a liberty affecting only individuals, makes society a captive audience. The right to know could force socially disruptive findings on us. Jonas is right to point out the emergence of science from its age of innocence. The new circumstances in which science finds itself have radically changed its role; it is spoiled by success.

What separates freedom of inquiry from freedom of speech, then, is that freedom of inquiry requires dissemination of knowledge which, by virtue of its nature and the possibilities it suggests, can affect society deeply. Freedom of speech occasions such a possibility only when knowledge is disseminated with a special urgency, as when someone yells "fire" in a crowded theater. Speech of the latter kind does not normally fall under the rights of the individual--one cannot yell "fire" at will--and scientific inquiry is to be distinguished from ordinary communication in the same way. Freedom of scientific inquiry, or the right to know, does not protect an individual's right to voice protest or pursue personal satisfaction in ways that do not affect others in the same ways that the fundamental rights protect that ability. This is the wedge dividing free scientific inquiry from liberties such as free speech.

It seems, therefore, that there may be grounds for distinguishing free scientific inquiry from basic civil liberties. The libertarian objection to research regulation fails on this view. If there are reasons to regulate some pure scientific research, it remains to ask how this is to be done. In turn, this question gives rise to many practical issues. They lie outside the bounds of this discussion, which has aimed merely to emphasize the choice we face between making research contingent on interdisciplinary risk-benefit analyses and adopting a policy that leaves decisions as to what research is acceptable to individual scientists.

RISKS AND BENEFITS: THE EXAMPLE OF VIROLOGY

The characteristic peculiarities of virology make it particularly relevant to this discussion. One such peculiarity follows from the nature of viruses. Viruses are neither dead nor alive in that they are dependent on growing cells to reproduce. Experimentation with viruses therefore strictly fits neither the category of biology nor protein chemistry. This dual nature of virology has consequences for the kinds of biohazards virologists incur, as well as the ethics of their work.

Although viruses are not independently viable, their capacity to reproduce in living cells holds out a threat of widespread infection, the horror of which has not been forgotten in the six centuries since the great plagues of Europe. In addition, the fact that viruses mutate much more rapidly than bacteria makes it possible to maintain vast archives of mutant forms. Nevertheless, viruses are not wholly natural--their simplicity allows scientists to gather a great deal of knowledge about them and renders them all the more manipulable. Direct genetic engineering will probably become a reality first in the field of virology. Thus the inert characteristics of viruses make them tractable to chemists, while their organic characteristics make them a biohazard like runaway mutant bacteria of the species *Escherichia coli*.

The half-live nature of viruses is only one peculiarity of the subject of virology. Other peculiarities give rise to political and social problems. Of particular interest to virologists is the selectivity with which certain viruses will infect groups within a population. Not only does this phenomenon help geneticists understand microbiological events, but it also sheds light on certain problems in cancer research--why, for example, carcinomata develop in some people but do not in others who carry the same virus while remaining healthy.

The study of virology has undoubtedly resulted in what unquestionably are scientific advances. Other results may be less clear-cut in their significance. Thus, development of selective viral strains will produce a body of data on mutant or recombinant forms (plus a few naturally occurring varieties), as well as information on differential susceptibility to them within a population. Fortunately, humans are so heterogeneous that we run only a negligible risk of coming upon a virus which would infect along racial lines. But even though human heterogeneity may rule out the possibility of racial warfare using a selective human virus, there remains the problem of "races" we shall create in exploring differential susceptibility. Might not a political power, for example, combine the capability of delivering some group-specific virus with a battalion of immune troops? What are the potential political uses of immune and nonimmune groups?

More pressing, because of its immediacy, is the effect widespread availability of a group-specific (or "polymorph-specific") virus would have on social relations. This problem has already appeared with the discovery that approximately 0.5 percent of the American population carries hepatitis B virus and can spread the disease under some conditions. Should legislation prohibit carriers from occupying positions in hospitals or civil service? Should they be quarantined? The possibility that a man-made virus could give rise to comparable risks in a different political setting (one, for example, which does not prefer immunization of a healthy majority to quarantine of an ill minority) is frightening. These problems are fortunately not with us right now. But why, as Robert Sinsheimer puts it, must we be "hostages to fortune"?

Knowledge of the type envisioned, fantastical as it seems, is not far removed from our present understanding of viruses. It is well documented that RNA phages (or parasitic predators) of Caulobacter, Pseudomonas, and E. coli attack selectively on the basis of the bacterial pili, or protein hairs protruding from the cells, which are determined in their conformation entirely by the organisms' genes.[3] Therefore, it seems not unlikely that there may be genetic determinants of susceptibility to human viruses. In fact, hepatitis B virus, evidently responsible for viral hepatitis, will depend for its selectivity on the previous host or hosts in which it has reproduced. Referring to that virus in his 1976 Nobel lecture, Baruch S. Blumberg wrote:

> Much of the coat (and possibly other portions of the virus) could be produced by the genes of the host. . . . If this is true, then the antigenic makeup of the virus

> would be, at least in part, a consequence of the antigenic characteristics of the host from whence it came. . . . This view . . . introduces an interesting element into the epidemiology of infectious agents in which not only the host and virus are factors, but also the previous host or hosts of the agent.(4)

Thus the antigenic or virulent specificity of hepatitis B virus may be manipulated by collecting samples from infected hosts. The virus then serves to carry human proteins to other hosts; if the proteins are similar to those of the next victim, there will be little immunological response, allowing the virus to propagate. Hepatitis B can be lethal in some populations but has a less dramatic effect in others. Blumberg reported that ". . . [in a Greek village] if either parent was a carrier of Hepatitis B surface antigen there were significantly more male offspring than in other matings."(5) Here we have another potential demographic weapon: "cultured" hepatitis B virus might be released to reduce the female/male ratio in a susceptible population and thus reduce the numbers of the succeeding generation. The fact that this possibility may be remote may comfort us, but the example should nevertheless alert us to the dangers involved in further research on these organisms.

A less clearly selective (though more widespread) virus is Epstein-Barr, which is thought to cause infectious mononucleosis and possibly Burkitt's lymphoma (a cancer). As Albert Kaplan wrote:

> It is now established beyond reasonable doubt that EB virus is the causative agent of the heterophil-positive,

> classical form of infectious mononucleosis, a disease with predilection for adolescents and young adults in the higher socio-economic groups.[6]

Burkitt's lymphoma occurs primarily in New Guinea and Africa. After mentioning the possibility that different viruses cause the two diseases, with EB virus being a mere passenger in the case of Burkitt's lymphoma, Kaplan suggested that "Another alternative is that the same virus is associated with the benign and malignant diseases, and the difference depends on genetic and/or environmental cofactors."[7] Thus its malignancy may have genetic determinants. More important, EB virus is present in most human lymphocyte cell lines and in fact may be responsible for the peculiar growth of lymphocytes in successful laboratory cultures.[8] Because they are easy to grow, lymphocytes are widely used in laboratory work on animal cells; in fact, they are available commercially. Thus we appear, in effect, to be marketing EB virus on a large scale. As a consequence, "laboratory workers who do not have antibodies to the virus may risk getting infectious mononucleosis, which can be a devastating disease."[9]

This brings up the problems created by industrial virology, a matter which complicates and renders more pressing the dangers inherent in virological research. The problems fall into two categories: inadvertent and intentional production of virus. As in the case of Epstein-Barr virus, production of lines of animal cells may entail inadvertent production of viruses which may behave differently under laboratory conditions than in the body. Some two hundred American corporations and five hundred companies worldwide are deliberately producing natural and unnatural microorganisms. A supporter of these practices declared enthusiastically that:

"Recent advances in the genetics and regulatory biology of industrial microorganisms can indeed be made more efficient and economical by genetic manipulation of microbial strains in use at present."[10] The production of viruses, however, may be particularly dangerous: "The consequences of a biological spill during large-scale production of tumor viruses can be of monumental proportions."[11] This writer pointed out the dangers that may attend use of a standard item of laboratory equipment, such as a centrifuge:

> The typical centrifuge is little short of a mechanical nightmare when viewed from the standpoint of containment and decontamination. Besides the basic mechanism for spinning a rotor, it consists of a maze of parts and accouterments, many of which are either inaccessible or would be damaged by decontamination procedures.[12]

Given our current economic system, availability of viral organisms cannot be separated from knowledge of them, or from risks attendant on industrial propagation of them. Availability of selective human viruses will be an indirect consequence of virological research.

Virology bears a detailed evaluation of potential risks because its benefits have already been well documented. Scientists have repeatedly looked to viruses for knowledge of biochemical processes, and it is on virology that biochemistry has turned. Perhaps virology bears the key to cancer. Yet virological research may incur risks of widely disparate kinds. The only certain conclusion we may draw is that risk-benefit analyses of virological research will become enormously complex. This complexity is part of what Jonas means by the sudden maturation of science.

If science becomes capable of producing information as compelling as some of the findings in selective human virology projected in this paper, it may force a change in our attitude to the relation between the individual and society. Science is a monolithic social undertaking. Its effect may be to socialize man to an extent unprecedented in the western world. We may reach a point where society decides selectively to stop its ears, and this may necessitate the delineation of acceptable fields of research. That is to say, we may reach a point where society opts occasionally for ignorance, or redefines wisdom.

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